last sequence of figures is sufficiently curious, and is one that can easily be remembered.—C. V. B.

A Green Sun.

I RECOLLECT reading some years ago in NATURE an observation of Mr. Norman Lockyer, to the effect that he had seen the sun green through the steam escaping from the funnel of a boat on Lake Windermere. In May 1888, I spent a considerable time one day viewing the sun through steam escaping at various pressures from the boilers of a colliery in Monmouthshire. In no case could I, or the friends with me, succeed in seeing the sun of a green colour through the steam, although we viewed it in a very great variety of ways. All we saw was the usual orange or red coloration.

But, this month, I have been watching the sun through steam puffed out from locomotives, and have, on five or six occasions, seen a bluish-green coloration extending over the whole disk. But sometimes the sun appeared simply white, and sometimes it was coloured orange-red. I cannot exactly determine the circumstances which produce the bluish-green; but I have seen it best with freshly puffed steam which had not risen very far above the funnel.

If the vapour particles are assumed of fairly uniform size, the following may be a possible explanation. The rays, coming through particles of vapour (really water particles in suspension) may be retarded as compared with the rays passing between the particles; and, if this retardation is such as to delay the red light passing through the particles one half wave-length, as compared with the red light not so passing, the result would be the destruction of the red element in the white light, and the light left would then appear bluish-green. This suggestion I owe to the courtesy of Sir G. G. Stokes, who also communicated to me the following very interesting observation. When a jet of transparent steam is escaping from a tube, we know, from Mr. Shelford Bidwell's experiments, that the steam becomes visible vapour, if an electrified point is brought near the jet. Sir G. Stokes noticed that the permanent shadow of the vapour on a screen was orange; but that, for a fraction of a second after the commencement of the electrification, a faint greenish shadow, preceding the orange one, was frequently seen. Water globules, about one ten-thousandth of a centimetre in diameter, might produce the requisite retardation.

I shall be glad to hear of other observations of this bluish-green coloration. After the great eruption of Krakatão we know that the sun was seen coloured green and blue.

CHAS. T. WHITMELL.

18 Park Place, Cardiff, February 26.

Frozen Fish.

THAT fish suffer, when imprisoned under a covering of ice in comparatively shallow ponds or lakes, goes without saying. But do they necessarily die when inclosed for lengthened periods in solid ice? My own opinion is that the latter condition is far less injurious to them than the former. It is a question of importance, for it concerns the conditions under which fish probably exist in comparatively shallow waters in high latitudes.

In one of the "Arctic Voyages" (I am not able, at this moment, to give the reference, and rely upon memory) it is distinctly stated that fish (carp, I think), frozen so hard as to necessitate the use of an axe in order to divide them, revived when thawed before the cabin fire, and "jumped about," as is usual with fish out of water.

There are fish and fish. Has the severe winter of 1890–91 caused any important mortality; if so, to which in particular?

R. MCLACHLAN.

Lewisham, March 6.

Zittel's "Palæontology"—Reptiles.

IT has been pointed out to me that in my review of Prof. v. Zittel's "Palæontologie" (March 5, p. 420) I have omitted to mention that, although other writers have placed the Dolichosauria next to the Pythonomorpha, it is only in a paper recently read by Mr. G. A. Boulenger before the Zoological Society, but not yet published, that the one group was considered to be the ancestor of the other. R. L.

March 9.

THE CHEMICAL SOCIETY'S JUBILEE.

AT the meeting in celebration of the Jubilee of the Chemical Society, held in the theatre of the London University on Tuesday, February 24, 1891, the proceedings were opened by the following address from the President, Dr. W. J. Russell:—

We meet to-day to celebrate the fifty years' existence of our Society, a time, if measured by the progress which our science has made, equal to centuries of former ages, but which in years is so brief a space that we have, I am happy to say, with us to-day some of those who were present and who took an active part in the foundation of the Society, and I need hardly say with how much interest we shall listen to their reminiscences of the time and circumstances connected with the birth of our Society.

I would, by way of introduction, say a few words, first, with regard to our Society, and afterwards with regard to the state of chemistry in England when our Society was founded. We boast, and I believe rightly, that our Society holds the distinguished position of being the first which was formed solely for the study of chemistry. Chemistry and physics, twin sisters, had hitherto always dwelt together, and many were the societies, both in this country and abroad, devoted to their joint study and development.

In London there was the Royal Society, which had hitherto received the most important chemical papers; there was also the Society of Arts, which is 110 years, and the British Association, which is ten years, senior of our Society. In Manchester the Literary and Philosophical Society had been founded and actively at work since 1781; and we admit that our neighbours at Burlington House, the Astronomical, Antiquarian, Linnean, and Geological Societies, are all our seniors: they had a distinct individuality and literature of their own, which called them into existence some forty to eighty years before the commencement of our Society. Small private chemical societies, no doubt, existed: they are the natural forerunners of a large society, and become merged into it. The Chemical Section of the British Association, which is an ephemeral and peripatetic Chemical Society, had existed from the founding of that body. If we turn to other countries, we find that, much as our science had been cultivated on the Continent, it did not until later times engross a whole society to itself, the French Chemical Society not having been formed until 1857, and the now great Berlin Chemical Society not until 1868. Our interest, however, at the moment is rather in the growth of chemistry in this country than in what occurred elsewhere.

To-day we may learn how it came about that the first Chemical Society was established in England. I may, however, state that the reason for our meeting depends on the official record that on February 23, 1841, twenty-five gentlemen "interested in the prosecution of chemistry" met together at the Society of Arts to consider whether it be expedient to form a Chemical Society. Of the twenty-five who then met I am happy to say three are present—Sir W. Grove, Sir L. Playfair, and Mr. Heisch; and Mr. J. Cock is another of this band who is still alive but is not present.

These twenty-five gentlemen appear without dissent to have come to the conclusion that it *was* expedient to form a Chemical Society, and appointed a committee of fourteen to carry this resolution into effect. So expeditious were they in their work, that in little more than a month the first general meeting was held, and the provisional committee brought forward a report embodying a plan for the constitution and government of the Society, and this plan remains essentially the same, save in one point, to the present day. I refer to the formation of a museum of chemical specimens; this project was abandoned some years ago. It is worth recording that at this first general meeting Thomas Graham was elected President; Messrs. W. T. Brande, J. T. Cooper, J. F. Daniell, R. Phillips, Vice-Presidents; Mr. Arthur Aikin, Treasurer; Messrs. Robert Warington, E. F. Teschemacher, Secretaries; Council—Dr. T. Clarke, Rev. J. Cumming, Dr. C. Daubeny, Messrs. T. Everitt, T. Griffiths, W. R. Grove, H. Hennell, G. Lowe, W. H. Miller, W. H. Pepys, R. Porrett, Dr. G. O. Rees. Also that the Society then numbered seventy-seven members. We hail Sir W. Grove as being the most active member who is still among us in founding our Society, for he was a member of the first Council, was present at the first meeting, and was a member of the provisional committee. I must here add to the official record, for it does not tell us how these twenty-five gentlemen "interested in the prosecution of chemistry" were collected together at one time and place. Obviously some special force was required to build up this complicated molecule; that special force was embodied in and exercised by Robert Warington. By his activity and energy he brought about this meeting, and we can imagine how difficult and troublesome a work it probably was, how some of these gentlemen had to be instigated to action, others repressed, some convinced that the aim was desirable, others that it was feasible. But whatever the difficulties were, Mr. Warington succeeded, and to him we are indebted for the formation of our Society. Although he has passed away, he is ably represented here to-day by his son. The love for the Chemical Society has proved to be hereditary: Mr. Warington of to-day is a most active and valued member; is one of our Vice-Presidents: and, as our programme shows, is about to present to us records connected with the early history of our Society which are of great interest now and will become of increasing value as time goes on.

I turn now at once from these matters immediately connected with our Society to the consideration of what was being done in chemistry in this country fifty years ago. At that time public laboratories for the systematic teaching of chemistry did not exist in London. The number of real students of chemistry in this country was very small. They were looked upon by their friends as being eccentric young men, who probably would never do any good for themselves, and these few students found practical instruction in the private laboratories of some of the London teachers.

The practical teaching of chemistry appears to have been undertaken in Scotland much earlier than in England, for Dr. D. B. Reid held practical classes at the University of Edinburgh as early as 1832. Graham came to London from Glasgow in 1837, and until the opening of the Birkbeck Laboratory, in 1846, he had from time to time private students working in his laboratory. And so with the other teachers, who all had private or articled pupils. I doubt whether the pupils received much systematic instruction, but they gained an insight into laboratory work, saw how apparatus was put together, and how analyses were made. We have indeed to wait some years before public laboratories are established, for not till 1845 is the College of Chemistry opened, and this appears to have been really the first public laboratory in London, and its object, as stated by its founders, is "to establish a practical School of Chemistry in England." About the same time both University and King's College established laboratories. The Council of our Society recognized the importance of these occurrences, for in the Annual Report in 1847, they say, "although an event not immediately connected with the Society, the Council has much pleasure in commemorating the late successful establishment in London of chemical laboratories expressly designed to further the prosecution of original research. The new laboratories of the College of Chemistry, and of the two older Colleges of the London University, now offer facilities for practical instruction and research not surpassed we believe in any foreign school."

While speaking of laboratories in London, I should however mention that the Pharmaceutical Society established a laboratory especially if not exclusively for its own students as early as 1843.

It was not till several years later, till 1850 and 1851, that the medical schools in London established classes of practical chemistry.

If we consult the scientific journals of the time immediately preceding the formation of our Society, we find it was by no means a period of chemical activity in this country, but rather a dull time, given more to the study and slow development of the science than to discovery. Methods of analysis, both organic and inorganic, had been much improved, and the dominant idea was the determination of the empirical composition of bodies, and the preparation of new compounds, whose existence was predicted by a study of Dalton's "Atomic Theory." Graham, Kane, and Johnson of Durham were the leaders in scientific chemistry, and the authors of the most important chemical papers of the time. Graham had very lately published his notable paper on the constitution of salts, a paper which gained for him, some years after its publication, a Royal medal. Kane was an active worker and a bold theorist, and at this time his reputation was much increased by a paper on the chemical history of archil and litmus. Johnson was also a most active chemist. His contributions relate to many branches of the science, but especially to the chemical composition of minerals. In 1841, however, he is engaged on a long series of papers on the constitution of resins. He will probably be best known and remembered as an agricultural chemist. Faraday we can hardly claim as a chemist at this time, for he was then rapidly publishing his long series of experimental researches in electricity. While speaking of electricity I should state that it was in 1840 that Smee described his battery, and the Society of Arts awarded him a gold medal for it. An important branch of our science was, however, coming into existence, a branch which has found many and sucessful investigators in this country. I mean photography. It was in 1840 that Herschel published in the Philosophical Transactions his elaborate paper on the chemical action of the rays of the solar spectrum, a paper in which he recognizes a new prismatic colour beyond the violet, and chemical activity in the spectrum beyond the red, and besides discussing many other matters, establishes his previously discovered hyposulphite of soda as the best agent for the fixing of sun pictures. Fox-Talbot had previously given an account of photogenic drawing, and claims that as far back as 1835 he took pictures of his house by means of a camera and chloride of silver paper, but it is not till 1838 that the Secretary of the Royal Society extracts from him a clear account of the details of his process, and it is in 1841 that he is granted a patent for improvements in obtaining pictures or representations of objects. Again, in the following year Herschel publishes another paper of much importance. I can here only mention how actively this line of research was prosecuted by Robert Hunt, how many, ingenious, and interesting were the experiments he made, and how valuable was the account he afterwards gave of this subject in his "Researches on Light." Thus the work done in this branch of chemistry at the time of which I am speaking is certainly noteworthy, probably more so than in other branches of chemistry. In fact, of other advances in chemistry there is little to record, but I may mention that Clarke's process for determining the hardness of water also holds its jubilee this year, for it was in 1841 that a patent was granted to Dr. T. Clarke for a new mode of rendering certain waters less impure and less hard.

Not a single chemical paper appears in the Phil. Trans. for 1841, but there are two papers which were much discussed at this time, and although they were readily shown to be erroneous, still are interesting as indicating the chemical ideas of the day. One is by Robert Rigg, who is carrying on an experimental inquiry on fermentation, and is termed "Additional Experiments on the Formation of Alkaline and Earthy Bodies by Chemical Action when Carbonic Acid is present"; it is published in the Proceedings of the Royal Society. The other is a paper by Dr. S. M. Brown, entitled "The Conversion of Carbon into Silicon," and is published in the Transactions of the Royal Society of Edinburgh.

With regard to the first paper, Mr. Rigg believes that he has demonstrated that, when fermentation takes place, a great and direct increase in alkaline and earthy salts, viz. of potass, soda, and lime occurs, an increase varying from fifteen to nineteen times the original amount. Denham Smith, who has only very lately passed away, showed that the theory simply rested on inaccurate experiment.

The object of the other paper is to demonstrate that, on heating paracyanogen, nitrogen is given off, and a residue of silicon remains. Dr. Brett and Mr. Denham Smith controverted this, and, in a paper in the *Philosophical Magazine*, proved that the supposed silicon was simply carbon in a very incombustible state. So important an experiment was this alleged conversion of carbon into silicon considered to be at the time of its publication, that it attracted Liebig's attention, and in a letter to Dr. Playfair, which was communicated to the meeting of the British Association at Plymouth, in 1841, Liebig says he has repeated Dr. Brown's experiment on the production of silicon from paracyanogen, but has not been able to confirm one of his results.

As far as pure chemistry is concerned it was rather a time of repose. The beginning of the century had been a brilliant time for chemistry in England. Dalton had published his atomic theory; Davy had decomposed potash and soda, and had demonstrated that chlorine was an element; and Cavendish and Wollaston were then still at work. In fact the most important discoveries of that time were made in this country, but I fancy that during this later period a feeling grew up that the age of brilliant discoveries was over, and that, apart from the preparation of a few new compounds, the essential work of the time was analysis and the determination of the percentage composition of bodies. Still much quiet study of the science was going on, as is indicated by the considerable demand which existed for good text-books. Henry's, Turner's, Kane's, and Graham's "Chemistry"—all these, without mentioning others, went through numerous editions, and played a very important part in the spread of chemical knowledge in our country.

Another text-book, which is interesting as showing how little organic chemistry was studied in this country, is Dr. Thomas Thompson's work on "Vegetable Chemistry." Dr. Thompson states in his preface that the object of the book is to lay before the British public a pretty full view of the present state of the chemistry of vegetable bodies, and further, he says, "that the ultimate analyses he gives have, with very few exceptions, been made upon the Continent, and principally in Germany and France. British chemists have hardly entered on the investigation." Evidently then at this time organic chemistry had been but little studied in this country.

When our Society was founded, Thomas Graham was certainly the most distinguished chemist in England. He came to London in 1837 as professor of chemistry at University College, succeeding Edward Turner. The work he had already accomplished was of a high order, and he was now occupied in writing his book, which appeared in 1842.

The book was an admirable account of the chemistry of the time; it contained a well arranged and clearly written introduction, describing the principles and latest discoveries in those branches of physics which bear most directly on chemistry. There was also an able and succinct account, probably the best which had then appeared in this country, of organic chemistry; and with regard to physiological chemistry, he states in the preface that he gives a "condensed view of the new discoveries in this department, which now enters for the first time into a systematic work on chemistry."

There are, however, indications that a knowledge of the discoveries and discussions going on on the Continent only slowly reached this country. This is strongly insisted on in the *Phil. Mag.* of 1841, by Messrs. Francis and Croft, who state that "but little of what is done abroad, especially in Germany, seems to find its way into England, or at least until the lapse of some years." In proof of this statement they mention results lately published by Dr. Apjohn, Prof. Johnston, and Dr. Golding Bird, all of which had been known on the Continent some time previously. A valuable series of communications described as "Notes of the Labours of Continental Chemists," is afterwards communicated by these chemists to the *Phil. Mag.*, and continued for several years.

The visit of Liebig in 1837, when he attended the meeting of the British Association at Liverpool, must have given some stimulus to the study of organic chemistry in England, and we find that he undertook to report to the British Association on "Isomeric Bodies," and also on organic chemistry, and this great undertaking resulted in his two works, the one "Chemistry, in its application to Agriculture and Physiology," and the other, "Chemistry, in its applications to Physiology and Pathology." Both books were dedicated to the British Association, the first appearing in 1840, the second in 1842. It is very difficult for us now to realize the importance of these works, and properly to appreciate not only the large amount of new knowledge which they contained, but, what is of still greater importance, the novelty of treating such subjects in a truly scientific spirit. Gradually this treatment of the subjects became understood and appreciated, and people took a higher view of chemistry, and regarded it as a true science, and not merely as a study which might lead to useful results.

If then it be true that chemistry at this epoch was not rapidly progressing in this country, we naturally ask how it came about that our Society from its very foundation was so successful. The explanation is not difficult to find, nor doubtful, for we have only to turn from our own country to the Continent and learn what is happening there. Liebig is at Giessen, Wöhler at Göttingen, Bunsen at Marburg, Dumas, Laurent, Gerhardt, and a host of distinguished and active chemists in France, and at this time even Berzelius and Gay Lussac are alive. Liebig, with his wonderful energy and ability, was powerfully advocating the theory of compound radicles, and was extending in every direction our knowledge of organic chemistry, and inspiring all who came within the range of his influence with a love for investigation. Dumas, at the same time, both as a chemist and a finished advocate, was advancing his views on substitution and chemical types. Laurent, and afterwards Gerhardt, were with conspicuous ability showing how these theories were to be extended and modified so as to assume a form which has even with the lapse of time been but little altered. Thus on the Continent it was a time of wonderful activity; chemistry was every day becoming more of a true science, and the constitution as well as the composition of bodies was actively being discussed and investigated. This activity on the Continent took time to reach and really affect us here. The older chemists thought the new theories were visionary and unsound, the simple theories of their younger days were being swept away, and only slowly did they realize the meaning of the newer form of their science; but the wave of progress could not be stopped, and in this country we had been ripening for the change. Clearly the immediate cause of this sudden increase of chemical activity in England was Liebeg. His famous school had now been established for several years at Giessen, and if the older men in this country did not altogether put their trust in him, the younger men, breaking through all restraint, flocked from this country to his laboratory, there to become indoctrinated with his enthusiasm for the study of chemistry, and to learn how scientific investigation was to be carried on. At this epoch our Society was founded, and our Journal shows how successful Liebig's teaching was, how a new spirit was instilled into English chemistry, and how much valuable work his students did. Our Society gave them a ready means of publishing their discoveries, and a meeting-place for discussion and mutual interchange of ideas. Thus do I explain the success which from the first has attended on our Society; and having now led you to this point I stop, for my part was merely to speak the prologue, and I leave the story of the development of our Society in other hands.

INFECTIOUS DISEASES, THEIR NATURE, CAUSE, AND MODE OF SPREAD.[1]

II.

ONE of the earliest and most important discoveries was that made by Pasteur as to the possibility of attenuating in action an otherwise virulent microbe—that is to say, he succeeded in so growing the microbes, that when introduced into a suitable animal they caused only a mild and transitory illness, which attack, though mild, is nevertheless capable of making this animal resist a second virulent attack. Jenner, by inoculating vaccine, inoculated a mild or attenuated small-pox, and by so doing protected the individual against a virulent small-pox Pasteur succeeded in producing such an attenuated virus for two infectious diseases—chicken cholera and splenic apoplexy or anthrax; later on also for a third—swine erysipelas. For the first two he produced cultures grown under certain unfavourable conditions, which owing to these conditions lose their virulence, and when inoculated fail to produce the fatal disease, which they would produce if they were grown under normal conditions. What they produce is a transitory mild attack of the disease, but sufficient to protect the animal against a virulent form; thus in anthrax he showed that by growing the *Bacillus anthracis* at a temperature of 42°·5–43° C. for one week, the bacilli become slightly weakened in action; growing them for a fortnight at that temperature, they become still more weakened, so much so, that if this culture (*première vaccine*) be injected into sheep or cattle (animals very susceptible to anthrax) the effect produced is slight; then injecting the culture which has been growing only eight days at 42°·5, the effect is a little more pronounced, but not sufficient to endanger the life of the animal. Such an animal, however, may be regarded as having passed through a slight attack of anthrax, and as being now protected against a second attack, however virulent the material injected. In the case of swine erysipelas, Pasteur found that the microbe of this disease, transmitted through several rabbits successively, yields a material which is capable of producing in the pig a slight attack of swine erysipelas, sufficient to protect the animal against a second attack of the fatal form. Passing the anthrax virus from however virulent a source through the mouse, it becomes attenuated, and is then capable of producing in sheep only a mild form of disease protective against the fatal disorder. Attenuation of the microbes has been brought about outside the body by growing them under a variety of conditions somewhat unfavourable to the microbe.

Attenuation of the action of the anthrax microbes has been produced by adding to the cultures some slightly obnoxious material (*e.g.* mercuric bichloride 1 : 40,000), by which the growth is somewhat interfered with; or subjecting an otherwise virulent culture for a short time to higher degrees of temperature (anthrax to 56° C. for five minutes; fowl enteritis, twenty minutes, 55° C.); or exposing them for short periods to some obnoxious chemical substance (*e.g.* anthrax to carbolic acid, anthrax to bichloride of mercury 1 : 25,000 for twenty minutes); or the microbes are passed through, *i.e.* are grown in the body of certain species of animals, whereby the microbes become weakened as regards other species (swine erysipelas, anthrax, diphtheria, and tetanus); finally, some microbes become attenuated spontaneously, as it were, by growing them in successive generations outside the animal body, *e.g.* the pneumonia microbe, the erysipelas microbe, and others. However good the nutritive medium, these microbes gradually lose their virulence as cultivation is carried on from subculture to subculture; in diphtheria the culture which was virulent at first loses its virulence as the same culture becomes several weeks old.

All these facts are of considerable importance, inasmuch as they enable us to understand how, in epidemics, the virulence of the microbe gradually wears off and becomes ultimately *nil*, and because they indicate the ways of attenuating microbes for the object of protective inoculations.

Another important step in the study of Bacteria was this: it was shown that they have, besides their special morphological and cultural characters, definite chemical characters. Specific chemical characters (specific ferment actions) of Bacteria have been known for a long time through the earlier researches of Pasteur—*e.g.* the Bacteria causing the acetic acid fermentation of alcohol, the mucoid fermentation, *e.g.* when beer becomes ropy, the lactic acid fermentation of milk-sugar, when milk becomes spontaneously sour, &c.

Similarly, it has been shown that when animal or vegetable matter undergoes the change known as putrefaction or putrid decomposition, substances are produced which resemble alkaloids in many ways, and which, introduced into the circulation of man or animals, act poisonously, the degree of action depending, *cæteris paribus*, on the dose, *i.e.* the amount introduced. These alkaloids—called ptomaines of Selmi—have been carefully investigated and analyzed by Brieger; they are different in nature according to the organism that produces them, and according to the material in which this organism grows: neurin, cadaverin, cholin, &c., are the names given to these substances.

Recent research has shown that pathogenic Bacteria, *i.e.* those associated with, and constituting the cause of specific diseases, are capable of elaborating poisonous substances—toxalbumins or toxins, as they are called—not only in artificial culture media, but also within the human or animal body affected with the particular pathogenic microbe. Thus, in anthrax or splenic apoplexy, Hankin and Sidney Martin have shown this to be the case; in diphtheria (Fraenkel and Brieger), in tetanus (Kitisato), similar toxins have been demonstrated. We can already assert with certainty that a microbe that causes a particular disease causes the whole range of symptoms characterizing the disease by means of a particular poisonous substance or substances it elaborates in and from the tissues of the affected individual.

Another important fact ascertained about some of the toxic substances produced by the different pathogenic Bacteria was this: that if, after they are elaborated in an artificial culture fluid, and, by certain methods of filtration, separated from the Bacteria, and injected into a suitable animal, they are capable of producing the same

[1] Lecture delivered at the Royal Institution on Friday, February 20, 1891, by E. Klein, M.D., F.R.S., Lecturer on Physiology at St. Bartholomew's Hospital Medical School. Continued from p. 419.