[THE ACADEMY.]

PROFESSOR POGGENDORFF.

By WALTER FLIGHT.

By the death of Professor Poggendorff, of Berlin, the world has lost a man whose name is deservedly famous in every land where science is pursued.

Johann Christian Poggendorff, a native of Hamburg, was born on December 29, 1796, and lived to the advanced age of eighty-one years. He was a son of a successful merchant of that city. Having in his earliest years no inclination to adopt his father's calling, and feeling a desire to pursue science, he devoted himself to the study of pharmacy. After some time, however, he relinquished this line of action, and in 1820 entered the University of Berlin as a student. In the succeeding year his first scientific contribution appeared, it being a paper published in Oken's journal *Isis*, and entitled: "Physisch-chemische Untersuchugen zur näheren Kenntniss des Magnetismus der Volta'schen Säule." This was followed in 1826 by "Ein Vorschlag zum Messen der magnetischen Abweichung," when he devised the instrument to which Gauss, at a later date, gave the name of the magnetometer (see C. F. Gauss, *Intensitas vis magneticae terrestris ad mensuram absolutam revocata*, 1832). In the year 1834 he was called to the Chair of Physics in the University of Berlin.

Poggendorff's researches led him into varied fields of work, but in none with so much success as in that of physics, and in his later years his attention was almost entirely devoted to the study of voltaic electricity. Among his investigations may be mentioned the inquiry into the quantitative determination of electro-motive force, the devising methods for the estimation of the maximum strength of two voltaic currents, an examination of the phenomena of galvanic polarization, the construction of a commutator—so happily referred to in Prof. Scheerer's poem on the jubilee day (*see infra*)—the determination of the resistance of liquids to the passage of electricity, the development of heat by electric currents, diamagnetic polarity, and the devising of new means of intensifying induction currents. In his earlier years he also examined the boiling-points of saturated solutions, and some years later wrote a memoir on the determination of the density of vapors. He critically examined the instruments employed for the estimation of the intensity of light, and investigated interference phenomena. Even meteorological questions were not beyond his range, and we find him discussing the fluctuations of the barometer, rain, parhelia, and star showers. Among the chemical questions which he attacked were the preparation of bromine from salt-springs, new modes of preparing formic acid, the preparation of sodium bicarbonate, the nature of the compounds of aluminium, and the existence of the hydrides of silver, and of other metals. And, lastly, in the branch of mineralogy Poggendorff examined the composition of the felspars and other mineral species allied to them, the nature of graphite, etc.

In addition to these numerous and important investigations, Prof. Poggendorff's labors in the field of literature were of vast extent. We find him, in 1837, allied with Liebig in editing the classical *Handwörterbuch der reinen und angewandten Chemie*. His connection with this publication ceased after the issue of the first volume. In 1853 he published his *Lebenslinien zur Geschichte der exacten Wissenschaften*, a forerunner and sketch of the great work which came ten years later, the invaluable *Biographisch-literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften* in two volumes. Now that fourteen years have elapsed since its publication, a supplementary volume is urgently required, although the want may to some extent be supplied when the Royal Society issue the additional volumes of their "Catalogue of Scientific papers (1800–1863)."

In the spring of 1824, soon after the death of Prof. Gilbert, of Leipzig, who had edited the *Annalen der Physik* bearing his name since 1799, Barth, the publisher of that famous serial, learned that Poggendorff had matured a plan for producing a journal devoted to physics and chemistry. To increase the already numerous channels for scientific publication by the foundation of a new serial appeared injurious alike to science and to private interests, and negotiations between editor and publisher resulted in the merging of Gilbert's *Annalen* in the new venture, of which Poggendorff held the management with such signal success for more than half a century. Mitscherlich and Heinrich Rose, among chemists, Erman and Seebeck, among physicists, as well as Fr. Hofmann, Von Buch, and A. von Humboldt, gave it their warmest support; Berzelius, Arfvedson, and Bonsdorff, promised to send the results of their labors to the *Annalen*; and through Humboldt's aid and co-operation of the *savans* of Paris, through Gustave Rose's that of the best scientific workers in London and Edinburgh, was secured. The *Annalen der Physik und Chemie*, as the new serial was named, while mainly devoted to the publication of researches in the branches of science referred to in the title was, according to an announcement in the first part, to deal also with such allied subjects as meteorology and what is now called physiography, and while pure mathematics was not considered to come within the area of the editor's labors, that branch of study would yet find a place in the *Annalen*, in so far as it tended to illustrate chemistry and physics. Some notion of the completeness with which during the five decades the works of the leaders of science have been recorded in the *Annalen* may be gained by an inspection of the following short list of the number of papers of some of the more distinguished contributors:

Berzelius	112	Ramelsberg	177
Brewster	67	Vom Rath	71
Dove	104	Riess	84
Faraday	76	Gustav Rose	100
Haidinger	90	Heinrich Rose	193
Heintz	60	Scheerer	57
Liebig	56	R. Schneider	50
Magnus	65	Schönbein	88
Poggendorff	152	Wöhler	65

The translation of Regnault's memoirs alone occupies 696 pages, and the notices of the researches of Faraday cover 1,617 pages. After Poggendorff had filled the editorial chair for half a century, and 150 volumes of the *Annalen*, as well as some supplementary volumes, had appeared, more than sixty of his friends determined that the time had come to do honor to such vast labor and such unflagging care by themselves contributing to and editing a special jubilee volume of the journal, which appeared in February, 1774; it bears on the title page the words: "Jubelband dem Herausgeber J. C. Poggendorff zur Feier fünfzigjährigen Wirkens gewidmet," and contains an excellent likeness of their revered chief. He lived to direct the publication of but a few more volumes, and must have died about the time that the first part of the newly projected *Beiblätter zu den Annalen*, to which we referred some weeks ago, passed through the press.

A COMBAT WITH AN INFECTIVE ATMOSPHERE.*

By JOHN TYNDALL, F.R.S., Professor of Natural Philosophy.

A YEAR ago I had the honor of bringing before the members of the Royal Institution some account of an investigation in which an attempt was made to show that the power of atmospheric air to develop life in organic infusions—infusions, for instance, extracted from meat or vegetables—and its powers to scatter light went hand in hand. I then endeavored to show you that atmospheric air, when left to itself, exercised a power of self-purification; that the dust and floating matter that we ordinarily see in it disappeared when the air was left perfectly tranquil; and that when the air had thus purified itself, the power of scattering light and the power of generating life disappeared together. For the sake of reminding you of this matter, we will now cause a beam of the lamp to pass through the air. You see the track of the beam vividly in the air. You know that the visibility of the track is not due to the air itself. If the floating matter were removed from the air, you would not be able to track the beam through the room at all. You see the track in consequence of the floating dust suspended in the air. If the air be inclosed in a place free from agitation the dust subsides, and then, as I endeavored to show you a year ago, the air possesses no power of generating life in organic infusions. The nature of the argument is this: You see the dust as plainly as if it were placed upon your hand and you could feel it with your fingers. You found that the dust, when it sowed itself in organic infusions, produced a definite crop in those infusions; and you are equally justified in inferring that the crop thus produced is due to the germs in the dust, as a gardener would be in believing that a certain crop is produced from the seeds which he sows. I say that the inference that his crop is the product of the seeds that he sows is not more certain than the inference that those crops produced in the organic infusions are due to the seeds contained in them.

You know the method that we resorted to for the purpose of enabling us to get rid of this dust. The object was to allow the air to purify itself, and it was done in this way: I have here the first chamber that was used in these experiments. You see at the bottom a series of test tubes entering the chamber: they are air-tight, and they open into it. There are windows at the sides, and here is a pipette through which the liquids can be introduced. Behind we have a door which opens upon its hinges. Now, imagine this perfectly closed; imagine it abandoned entirely to itself, left perfectly quiet. In a few days the floating dust of the air contained in the chamber entirely disappears; it has removed itself by its own subsidence; and then when you send a beam of light, such as we have here through these windows, you see no track of the beam within the chamber. When the air is in this condition, you pour through this pipette infusions of beef, mutton, or vegetables into these tubes, and allow them to be acted upon by the air. Last year, between fifty and sixty of these chambers were constructed, and the invariable result was that these infusions never putrefied, never showed any change, were perfectly sweet months after they were placed there, as long as the air had this floating matter removed. You had nothing to do but to open the back door and allow the dust-laden air to enter the chamber to cause these infusions to fall into a state of putrefaction, and swarm with microscopic life, in three days after opening the door. I have a smaller chamber here—for we use chambers of different sizes—and it will enable you to understand our exact process. You see here the stand on which the chamber rests. There are two bent tubes that communicate with the outer atmosphere, for I wish to have a free communication between the air outside and the air within. You see the pipette through which the tube is filled. When the infusion is poured in, you place it in an oil-bath contained in a copper vessel, such as we have here, in which you boil it for five minutes. Now, that boiling for five minutes was found capable of sterilizing every germ contained in the infusions placed in these chambers. This year our experiments began by a continuation of those that we made last year. In order to enable you to judge of the severity of the results obtained last year, I have here five cases belonging to the experiments then made. You will see that the infusions are vastly concentrated because of their slow evaporation. The quantity of liquid is reduced to one-fifth of its primitive volume, but this one-fifth is as clear as rock crystal; whereas, the tubes exposed to the ordinary air outside fell long ago into utter putrefaction. They became turbid and covered with scum; and when you examine these infusions to ascertain the cause of that turbidity, you find it to be produced by swarms of small active organisms.

This year our inquiries began in the month of September. But we will pass over these inquiries for the moment and go to those of October. On October 29th, two members of the Royal Institution collected a quantity of fungi in Heathfield Park, Sussex. These were brought to London on the 30th. They were placed for three hours in warm water, and whatever juices they possessed were thus extracted from them. They were placed in chambers and digested separately. There were three kinds of fungi; we will call them red, yellow and black. Now, I confess that, thinking I had secured a perfect freedom from any invasion of those contaminating organisms that produce putrefaction, I expected that we should find that these infusions of fungus would maintain themselves perfectly clear. To my surprise, in three days the whole of them broke down; they became turbid, and covered by a peculiar fatty, deeply indented, corrugated scum. Well, that was a result not expected, but I pursued the matter further. I got another supply of fungi. Even in this first experiment, I had adopted care at least as great as that which I adopted last year, and which led to a perfect immunity from the invasion of putrefaction. With the fresh supply of fungi, I operated with still more scrupulous care. The infusions were placed as before in three chambers. In one of these the infusion remained perfectly pellucid; there was no trace of any organism to be seen. In each of the other chambers one of the three tubes gave way. Each chamber contained three tubes; so that out of nine tubes containing an infusion of fungus seven proved to be intact, entirely uninvaded. Therefore, whatever argument or presumption was raised by the first chamber in regard to the idea that life was spontaneously generated in it, was entirely destroyed by the deportment of the other chambers. Seven out of the nine remaining intact was sufficient to show that it was some defect in the experiment that caused the first chambers to give way so utterly. I continued the experiments, and inasmuch as fungi disappeared on the approach of winter, other substances were chosen. I took cucumber and beetroot, having special theoretical reasons for doing so, and prepared infusions of them with the aid of my excellent assistant, Mr. Cotterell. We placed these in our chambers as before, boiled them for five minutes, and abandoned them for what I supposed to be moteless air within. Again, to my surprise, an infusion of beetroot in one chamber, and an infusion of cu cumber in another, broke down. All the tubes became turbid and covered with this peculiar fatty scum. Other chambers were then tried. I had begun to suspect that we were operating in contaminated atmosphere; that my infusions were in the midst of a pestilence which it was hardly possible to avoid. The consequence was, that I withdrew the preparation of the infusions from the laboratory downstairs, and I went to one of the highest rooms in the Royal Institution, had the infusions prepared there, and introduced into the cases, which were afterwards boiled in the laboratory below. There were a great number of these cases. The substances chosen were cucumber, beetroot, turnip, and parsnip. Great care was taken to have the infusions properly prepared, and to have them rendered as clear as possible. To give you an idea of the care taken, I may mention that the infusions of turnip and beetroot were passed through twenty-four layers of filtering paper, and were thereby rendered clear; that the infusion of cucumber was passed through one hundred and twenty layers of filtering paper, and thereby rendered clear; and that the infusion of parsnip was passed through three hundred layers of filtering paper, and it was still opalescent. The suspended particles were so small that the filtered paper had no power whatever to arrest them, and the finest microscope ever made would have proved powerless to exhibit the individual particles that produced this opalescence. Notwithstanding all this care, the chambers containing these infusions in three days became filled with bacterial life. They were turbid, covered with scum, and showed all evidences of putrefaction. This was on November 20th. On November 25th, we went upstairs and prepared another chamber, or a series of chambers. When the tubes containing the infusions were placed in the oil-bath, the liquids within the tubes opening into the case of course boiled, steam was discharged into the case, the air of the case being thereby rendered warm. It was found that on the cessation of the ebullition, although the pipette was immediately plugged with cotton-wool, and the bent tubes also plugged with cotton-wool, still, in consequence of the contraction of the air within, there was a considerable indraught. Last year, we found invariably that the interposition of the cotton-wool entirely sifted this entering air so as to arrest any germs of seed that it might contain. I thought, however, in this case, that the germs might be carried in by the suction when the air of the chamber contracted. In the former case, we operated after having filled the chamber with the infusion, and boiled it in the laboratory; in this case, we took the additional precaution of boiling the infusion upstairs, and taking care that it was properly plugged with cotton-wool. But here, again, notwithstanding this augmented care, the infusion utterly gave way, and showed those evidences of life that had distracted me previously. When I say distracted, it is not meant that I was in the least degree daunted or perplexed about it. I knew perfectly well that the matter would be probed by and by.

On November 27th, a new chamber was constructed containing cucumber and turnip. Particular care was taken with the stopping of the pipette, and also the bent tubes opening into the atmosphere. In one instance, about this time, it was noticed that the infusions in the tubes within the chamber opening into the moteless air, or at least what I supposed to be the moteless air, fell more rapidly into a state of putrefaction, became more rapidly covered with scum, than the tubes exposed in the air outside. When the tubes containing precisely the same infusion were exposed to the air outside they were perfectly clear, while those within were turbid and covered with scum. This brought to my mind an experiment made the previous year with trays placed one above the other. It was found that, when two trays were placed one above the other, although the upper tray had the whole air of the room for its germs to deposit themselves, the under tray was always in advance of the upper in the development of life. The reason was simply this: The air in the under tray was less agitated, and this floating matter had time slowly to sink in the infusions. There was no other solution possible than that, by some means or other, the germs had insinuated themselves into my chamber, and that these germs, sinking slowly through the unagitated air of the chamber, were able to produce the effect within, in advance of the effect produced upon the openly exposed tubes without. On November 27th, I had a similar case, and also on November 30th, and on December 1st. The chambers were prepared and filled with all care, and yet the infusions broke down, became turbid, and were covered with scum. I then had a number of tubes filled with infusions, and sealed them hermetically. They were exposed in an oil-bath, and heated for a quarter of an hour to a temperature of 230° F., for I wanted to see whether these effects were due to any germs of life in the infusions themselves. This superheated cucumber infusion was introduced into the chamber, and it was found that the superheating of the infusion did not even retard the development of life. In two days, every tube of the chamber was swarming with bacteria. I then passed on to another system of experiment pursued last year; that is, the exposure of the infusions to air calcined by passing a voltaic current through platinum wire, so as to raise the wire to a state of incandescence. Such arrangements are here. We have underneath this shade two wires, and stretching from wire to wire we have a spiral of platinum. Passing a voltaic current through the spiral, it was found last year that five minutes of incandescence were sufficient entirely to sterilize and destroy all germs contained in this air, and to protect the infusions underneath from all contamination; the time of incandescence was doubled this year. The wire was raised as close to the point of fusion as possible; still, notwithstanding all this additional care, the infusions one and all gave way. I thought that there might be some defect in the construction of the apparatus.

Here, you see, is an old broken apparatus containing infusions that have remained perfectly good since last year; but great pains were taken in having the apparatus of the most improved form. Still, notwithstanding all my efforts, the infusions broke down and became swarming with life. My attention was now very keenly arrested, and on December 1st I scrutinized more closely than ever I had done previously the entry of the infusions through the pipette tube into the tubes opening into the chamber, and I noticed, at all events, a danger of minute air-bubbles being carried down along with the descending infusion. That caused me to adopt another mode of experiment; but, previously to this, I fell back upon some of the infusions found so easy to sterilize the previous

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year. I operated upon beef, mutton, pork, and herring infusions, and found that even such infusions, which with ordinary care were completely sterilized last year, and are preserved to the present hour intact like the others, all gave way.

How, then, are we to look at these things? Here are results totally different from those that we obtained last year. You may ask me, perhaps, "Why do you not loyally bow to the logic of facts and accept the conclusion to which those experiments apparently so clearly point? Why do you not regard them as a demonstration of the doctrine of spontaneous generation? Is there any other way of accounting for it than by a reference to this doctrine?" You may ask whether I was held back by prejudice from accepting this conclusion; whether I was held back by a love of consistency or by the fear of being turned into ridicule and sneered at by those whom I ventured to oppose on a former occasion. Ladies and gentlemen, there is a title which I believe, as the generations pass, will, if the owners of the title are true to themselves, become more and more a title of honor—that is, the title of a man of science; and of that title I should be utterly unworthy were I not prepared to trample all influences and motives such as those mentioned under foot, and were I not ready, did I conceive myself to be in error in what was brought before you last year, to avow here frankly and fully in your presence that error. I should be unworthy the title of a scientific man if my spirit had not been brought into this state of discipline as to be able to make such an avowal. Why, then, do I not accept those results as proving the doctrine of spontaneous generation? The celebrated argument of Hume comes into play here. When I looked into all my antecedent experience, and into the experience of other men for whom I have the greatest esteem as investigators, it was more easy for me to believe the error of my manipulation, to believe that I had adopted defective modes of experiment, than to believe that all this antecedent experience was untrue. It was my own work that was thus brought to the bar of judgment, and my conclusion was, that I was far more likely to be in error than that the great amount of evidence already brought to bear upon the subject should be invalid and futile. Hence, instead of jumping to the conclusion that these were cases of spontaneous generation, I simply redoubled my efforts to exclude every possible cause of external contamination. This was done by means of doing away with the pipette altogether, and using what we call a separation funnel. Here you have a chamber with a pipette entering. This pipette tube has not a bulb or mouth such as you have here; it is simply closed by a tube of india rubber, and that again is closed by pinchcock. Now, here we have an infusion of hay. At present, this stop-cock stops it. I turn it on, it goes down; I turn it off, and this liquid column is now held by atmospheric pressure. This was introduced into the india rubber tube, the india rubber tube being first filled with the infusion, so that no bubble of air could get in. When the separation funnel was placed thus, and the cock was turned on, the liquid was introduced into the chamber without an associated air-bubble. Mr. Cotterell will show you the result of this severe experiment. Here is an infusion of cucumber, the most refractory of all infusions that I have dealt with. It was prepared on December 8th, 1876, so that it is between six and seven weeks old. Two days were sufficient to break down this infusion when contamination attacked it; but, by this more severe experiment, it is enabled to maintain itself as clear as crystal, although it has been there for six or seven weeks. You will see by the light behind that it is, as I have described it, perfectly clear. You will observe that the infusion is diminished by evaporation, but it is as clear as distilled water, and there it remains as the result of this severe experiment.

Let us now ask how it is that these curious results that I have brought before you were possible; how is it that the results of this year differ so much from those obtained previously. The investigation of this point is worthy of your gravest attention. I am now called back to the experiments with which this inquiry this year began. As already stated, it was begun in September, and, leaving out the earlier experiments, I passed on to October 30th. I have now to bring your attention back to the earlier experiments performed in the laboratory. They were suggested by the ingenious investigations of Dr. William Roberts, of Manchester, and by the subsequent investigation of a man to whom we are indebted more than to any other for the knowledge we possess of the different species of those small organisms that we call bacteria; I refer to Professor Cohn, of Breslau. Let me say that I entertain the very highest opinion of the intelligence and ability with which Dr. Roberts has carried out these experiments, they are in the highest degree creditable to him. This is the experiment to which I refer. Some chopped hay is put into a little can; it is raised to a temperature of 100 deg. to 120 deg.; it is kept for three hours, then poured off and filtered. Last year, we found that hay thus treated was sterilized by five minutes' boiling. I mean that, when it is exposed to the air that has this floating matter removed from it, it never shows any sign of microscopic life. Now, if you examine this natural hay-infusion with litmus paper, you will find that it turns the litmus paper red, showing that it is an acid infusion. Dr. Roberts found that acid infusions could be easily sterilized. He took a vessel with an open neck at the top and filled it two-thirds full with the infusion he wanted to operate upon; he then stuffed the neck with cotton-wool, and sealed it hermetically with a spirit-lamp above the plug of cotton-wool; he then placed it in a vessel containing cold water, and he gradually raised the water to a state of ebullition and maintained the boiling temperature for any required time. In that way he avoided all commotion, all evaporation, all ebullition in the infusion. After he had placed the tube in this condition in the water, and subjected it to a boiling temperature for any required time, he took it out and simply filed it across the neck and broke it off. Here you have the infusion practically exposed to the atmosphere. The plug intervenes to prevent the entrance of dust, and still allows an interchange between the air of the bulb and the air outside. When Dr. Roberts took this acid infusion and neutralized it by the addition of caustic potash, he found it to possess the most extraordinary power of resistance to heat; he found that, in some cases, it required more than two hours to reduce this infusion to sterility; he also found that, in a particular case, it actually required no less than three hours' boiling to produce this effect. This was very different indeed from the results that I had obtained last year. I made many experiments with hay-infusion, and in every case we sterilized it by five minutes' boiling. I was led to take up the subject this year through the emphatic manner in which Professor Cohn corroborated the results of Dr. Roberts. I operated sometimes with tubes like those of Dr. Roberts, and sometimes with those which I call Cohn's tubes. These are formed by heating a certain portion of a test-tube and drawing it out so as to leave an open funnel above, a bulb below, and a narrow tube between both. These are Cohn's tubes. His method was this: He placed the tubes, as they are placed here, in boiling water, and when they had been subjected to a boiling temperature for a sufficient time he simply lifted them out. He found a certain amount of water condensed upon the neck of the bulb; he waited one or two minutes until that evaporated, and then quietly plugged his tube with cotton-wool, and he thought that this was perfect immunity against the entrance of contamination; and Professor Cohn is very emphatic in saying that there is no thought of contamination from without in pursuing this method of experiment. I operated upon a great variety of hay-infusions, and after a time, by pursuing with the most scrupulous exactness the method laid down by Dr. Roberts and Professor Cohn, it was possible for me, by practice, now to corroborate and now to contradict them. It is perfectly useless to bring forward before public assemblies merely opposing assertions, so that I did not really content myself with falling back upon the results I obtained last year, but tried to get some knowledge as to whence the differences arose which showed themselves between me and these distinguished men. Here are tubes of alkalized hay, some of them subject to a boiling temperature, not for three hours, but for ten minutes, and they are perfectly brilliant; there is not the slightest evidence of life in them; they have been entirely sterilized by an exposure to a boiling temperature of ten minutes. If I illuminate them, you will find that these infusions are perfectly brilliant; there is no turbidity that gives any sign of the production of animalcular life. These tubes have remained there for three months perfectly intact, uninvaded by those organisms which were invariably found both by Dr. Roberts and by Professor Cohn. Again, we turn to another series of tubes, and find that every one of them has given way. Thus I went on ringing the changes, until, as I have said, it was in my power, by pursuing with undeviating fidelity the mode of experiment laid down by Dr. Roberts and Professor Cohn, to get at one time a contradiction and at another time a corroboration of their results.

And what was the meaning of these irreconcilable contradictions? The meaning was this: when we came to analyze these various infusions, we found that those that were sterilized by a boiling of from five to ten minutes were invariably infusions of hay mown in the year 1876, whereas the others were infusions of hay mown in 1875, or some previous year. The most refractory hay-infusion that I have ever found was in the case of some Colchester hay five years old. Now, what do these experiments point to? The answer may be in part gathered from an observation described in the volume of the *Comptes Rendus* for 1863 by one of the greatest supporters of the so-called doctrine of spontaneous generation. A description is there given of an experiment that was made by the wool-staplers of Elbœuf. They were accustomed to receive fleeces from Brazil which were very dirty, and had, amongst other things, certain seeds entangled in them. These fleeces were boiled at Elbœuf sometimes for four hours; and the seeds were afterwards sown by some of these expert fellows that had to deal with the fleeces, and were found capable of germination. The thing was taken up by Pouchet. He gathered these seeds, exposed them to the temperature of boiling water for four hours, and then examined them closely; and he found (and I recently made an experiment which showed the some thing to be true with regard to dried and undried peas) that the great majority of the seeds were swollen and disorganized, while the others were scarcely changed; they were so indurated and perhaps altered in the surface as to prevent the liquid from wetting them. At all events, a number of them appeared to be quite unchanged. He separated these two classes of seeds and sowed them side by side in the same kind of earth. The swollen seeds were all destroyed; there was no germination; but in the case of the others there was copious germination. Here, then, you have these seeds proved to be capable, by virtue of their dryness and induration, of resisting the temperature of boiling water for four hours. There is not the slightest doubt that, if time permitted, I could heap up evidence of this fact, that the wonderful sterility of this old hay is due to the induration and desiccation of the germs associated with it. Here you have three tubes containing cucumber infusion of crystalline clearness; they have been simply subjected to a boiling temperature for ten minutes; they have been completely sterilized, and they are as clear as when the infusions were first introduced into the tubes. On the other hand, here are tubes that have been subjected to a boiling temperature for five hours and a half showing a swarming development of life. What is the reason of this difference? The reason depends entirely upon the method of experiment. When Dr. Roberts filled his bulbs, he simply poured in his infusion, plugged his tube, sealed it, and subjected it to a boiling temperature. Not only did the liquid contain germs, there was a quantity of air above the liquid, and the germs were diffused in the air. Germs thus diffused in the air are very differently circumstanced from germs diffused in a liquid: they can withstand for hours a boiling temperature; whereas that selfsame temperature, brought to bear upon germs immersed in liquid, destroys them in a few minutes. And why do these tubes differ? The reason is to be sought entirely in the method of filling the tubes containing the clear infusions. Take one of Dr. Roberts' bulbs. You see that the top is united to a T-piece with a collar of india rubber. This comes down and ends in the neck of the bulb. Here is an air-pump, and here is the end of the T-piece surrounded by a tube of india rubber, and here is a pinchcock to close that tube of india rubber. If you open the pinchcock and work the air-pump with which this end is connected, it is completely exhausted. You may allow it to be filled with air; you may then open the pinchcock; the air will enter through the cotton-wool, and will fill the bulb. In this way you get the bulb filled, not with common air, but with filtered air. This process is carried on three or four times, so as to make sure that the common air has been displaced by the filtered air. We will suppose that I detach the tube from the air-pump and other precautions taken. At present you see the bulb is empty. Taking an infusion of hay, I put the end of the T-piece into the infusion to be introduced into the bulb. The bulb is dipped into hot water; the air expands, and it is driven out. Simply introducing our bulb into cold water, the air shrinks, and by atmospheric pressure the liquid is driven into the bulb. Again we drive the air out, and, by a few operations of this kind, we find that we can charge our bulb with a very great degree of accuracy. You can see the liquid in the bulb at the present time. In this way we charge a bulb which has had its common air and floating matter removed with our infusion. When it is charged, it is very carefully removed, and great precautions are taken so as to prevent any indraught of air. For instance, it is always removed from the cold water, so that, when it is lifted up into the air of the laboratory, a slight expansion shall take place, so that the motion of the air shall be from within outwards, instead of from without inwards. In that way we can, by careful manipulation, obtain bulbs devoid of this floating matter. These are the bulbs you now see before you showing this beautifully pellucid infusion.

Were this a biological investigation, and not a physical one, I should feel myself out of my element in dealing with it. I leave the determination of the species of bacteria to others far more competent than I am. I can see these organisms and wonder at them when I see them through the microscope; but I have no ability or knowledge to classify them and divide them into species, genera, etc. But these are purely physical experiments, and it is only by such severe experiments that this question can be freed from the haze and confusion in which it has been hitherto involved. Even the celebrated Professor Cohn—I say it with the greatest regard and respect for him—appears to have no adequate notion of the care necessary to be taken in experiments of this kind. To lift a tube out of the boiling liquid, and allow it to remain quietly in the air, the entry of the air taking place from without inwards, and then, after one or two minutes' exposure, to plug it with cotton-wool and say that no contamination can reach it, is in my opinion a great mistake. He could not, but by the merest accident, get an infusion free from contamination by operating in this way. I have here tubes prepared according to his method. Here are some melon-tubes all putrid, all gone into a state of fermentation. I ask you to compare those with some other melon-tubes that I have operated upon in a different way and that are as clear as crystal. The others are all gone, simply through a defect in the mode of manipulation.

The defeats that I at first described to you were due entirely to the contaminated atmosphere in which we worked. It ought to be noted that, in the earlier experiments in this inquiry, the results were always in accordance with those brought before you last year. By degrees, however, masses of hay were introduced into the laboratory—old hay and new hay from various places; and they ended by rendering the atmosphere so virulently infective that everything was contaminated by the germs set afloat. It resembled the case of a surgical ward of a hospital, where gangrene and putrefaction have attained such a predominance that the surgeon has in despair to shut up his ward and abandon it to disinfection. Desiring to free myself from this pestilential atmosphere, I wrote to my friend, the President of the Royal Society, Dr. Hooker, and I found that he was able to furnish me with a means of getting away from it. In Kew Gardens, there is a beautiful new laboratory, erected by the munificence of that most intelligent supporter of science, Mr. Thomas Phillips Jodrell. He, at his own expense, has had this beautiful laboratory built—being designed, I believe, by Dr. Thiselton Dyer. It is one of the neatest things I have ever seen, and it is to me a great gratification that the first experiments made in that laboratory were those to which I have now to refer. I broke away from the contaminated air of the Royal Institution. It is very well for you that I can tell you, that all the germs referred to are perfectly innocuous to human beings, for I have no doubt the air of this room is contaminated with them. A series of chambers was made—not of wood, for I wanted to get rid even of that, but of tin—and I would not allow Mr. Cotterell to carry those chambers into the Royal Institution at all. They were carried from the tinman's where they were made to the laboratory at Kew. There, with the greatest care, the tubes were treated first with carbolic acid and then washed with water, and then with caustic potash, to get rid of all traces of carbolic acid, and finally drenched with distilled water. Carbolic acid, as you know, is a deadly foe to these germs. In this way I hoped that every contamination that might be adhering to the tubes would be destroyed, and that, having got clear of an infected atmosphere, we might get the same results as we invariably obtained last year. The temperature was raised to between 80 deg. and 90 deg., and once a little above 90 deg., so that the warmth was all that could be desired for the development of those organisms. It gives me the deepest gratification to find that what was foreseen has occurred, and that this very day these chambers have come back from Kew perfectly intact. They comprise the most refractory substances that I had experimented upon here. It was almost impossible to save a cucumber; I never did succeed in saving a melon-infusion from contamination, and from this so-called spontaneous generation. But here, when the air had been allowed to deposit all its motes, and when we were withdrawn from an infected atmosphere, as I have said, the chambers were returned with their infusions as clear as crystal. Mr. Cotterell will show you some of them. You will see that one of these is muddy and turbid, and it has a deposit at the bottom. These are all dead bacteria, and the muddiness is due to swarming bacterial life. Here you have two infusions perfectly clear. Why did the other tube give way? When we came to examine it, a little pin-hole was found at the bottom of the chamber, and through that pin-hole the germs got in. Here is a melon-infusion; and, in order to show you what would have occurred if the infusions had not been protected from the floating dust of the atmosphere, we have hung beside this case the two tubes that have been exposed to the common air and have fallen into a state of utter rottenness. In this way, from the Jodrell Laboratory at Kew, we have had these cases returned with their infusions perfectly intact. Even in our infected atmosphere, when we subject our infusions to experimental conditions sufficiently stringent, we are able entirely to shut out contamination, and to show that spontaneous generation never occurs. When we get clear of our atmosphere altogether, this is a matter of perfect ease and facility; and we find in Kew Gardens that nature runs her normal course.

COURSE OF THE SAP IN PLANTS.

AT the last meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Andrew Murray read a paper combatting the theory of a descending current of sap at any period or under any circumstances. He maintained that absolutely no proof whatever has hitherto been adduced of a descent of sap. Nor would he admit of an assimilating process in the leaves and a transference of food thus prepared to where growth is taking place, or where, under certain conditions, growth would take place. His view he believes to be supported by the results of experiments conducted by Mr. Herbert Spencer (Linnean Society's Transactions, vol. xxv), and since repeated and extended by Prof. W. R. McNab. It is essentially this, that the ascending sap deposits the wood as it rises, and the surplus water returns to the atmosphere through the leaves. Mr. Murray concluded with an appeal for a re-investigation of the subject.—*Academy.*

HOME-MADE CORALS.—The manufacture of napoline, an imitation of coral, is being carried on by a Connecticut firm. It is, it is said, made from cheese or curds. The curd is separated from the water by chemicals and drying, subjected to a 40-tons pressure, and cut into the shapes of flowers, etc., being capable of receiving any color.