[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 Untersuchungen 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 feldspars 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, Arfwedson, 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 unflinching care by themselves contributing to and editing a special jubilee volume of the journal, which appeared in February, 1874; it bears on the title page the words: "Jubiläum 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, 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 cucumber 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

* A lecture delivered at the Royal Institution, January 19, 1877. From the *British Medical Journal*.