edition of his work on the Sun, on an enlarged scale. He has quoted so largely from Mr. Lockyer's 'Solar Physics' that an intended translation of this work is abandoned for the present."

I have the honour to inform you that the complete original of my second edition has been in the hands of M. Gauthier for more than a month, so far as that part which may have something in common with Mr. Lockyer's work is concerned, and that I had not seen Mr. Lockyer's work until a fortnight ago, when I bought it from M. Loescher here in Rome. Mr. Lockyer of course is quoted, but only from his original memoirs, and not from his new publication, nor in such a manner that his publication will render my work useless.

Rome, March 23

P. R. SECCHI

[The following explanation has been sent us by the Paris correspondent who furnished us with the note referred to by Father Secchi :—

"I was told by his (P. Secchi's) editor himself, when I spoke to him about publishing a French edition of Mr. Lockyer's 'Solar Physics,' the substance of what I have written to you. I think that the note I have written is a recommendation of Father Secchi's work; but not so his statement that he did not possess 'Solar Physics' until it was too late to use it. There is nothing whatever dishonourable in quotation."—ED.]

Spontaneous Generation Experiments

SINCE October 1870 I have, as opportunity offered and other work permitted, made a series of experiments bearing on the question of spontaneous generation. They seem to me to tell so plain a story that I am anxious to relate it.

The thoughts which led to the experiments were briefly these :—

The occasional or even frequent presence of living growths in fluids after they have been exposed to a temperature of 212° F. and are contained in closed tubes or flasks is rather an indication of the imperfection of a method than the proof of a theory; for under like circumstances living organisms ought either always or never to develop; the conditions being uniform, the results should be uniform.

When the tubes are closed at a blow-pipe flame after boiling, steam cannot be escaping from the aperture at the time of actual closure, and it is conceivable that in the momentary collapse of the contents which then occurs some atmospheric air containing organic matter may pass into the tube and invalidate the experiment.

The contained air, if any there be after the sealing of the tubes, must be vastly rarefied, and the ordinary atmospheric conditions, other than purity, which are essential, must be absent or greatly modified.

I attempted to devise an experiment which would be free from these possible sources of error; one in which the atmospheric pressure should be normal, in which the physical structure of the air should be unaltered, and in which there should be no chance of organic contamination after heating. Further it seemed a good thing to be able to show at the same time and in the same apparatus two distinct specimens of the boiled fluid, the one exposed only to cleaned air, the other exposed also to common air; and also to use a fluid which would indicate to the naked eye by change of colour, or of clearness, or of consistence, the time at which living growths made their appearance.

The latter condition was secured by using a fluid (for the idea of which I am indebted to Mr. Heisch's experiments on water-impurities) composed of 10 cc. of urine, 1 gramme of white sugar, and 90 cc. of distilled water. This when boiled and filtered is a clear transparent liquid, which becomes milky on the occurrence of organic growth during fermentation in thirty to forty hours, according to the heat to which it is exposed.

The other conditions were effected by using a glass tube of the shape of the capital letter M, with curved bends instead of the angles; a tube which may be described as having four straight legs joined to each other by two loops on the upper side and one on the lower; the first leg closed and the last leg open and short.

This tube, so bent, was made very hot, so as to expel as much air as possible from it; the open end was then plunged into the boiled and filtered urine-sugar fluid, and such a quantity allowed to flow in on the cooling of the tube as left the first, second, and third legs about half full when the tube was held upright. The tube was again heated to the boiling of the contained fluid in order to expel as much air as possible by the generation of

steam. It was then allowed slowly to cool, so that the first leg was about one-third filled with fluid; and such an amount was left in the lower loop as would rise in the second and third legs to about the same extent as the tube cooled (and the cooling was designedly prolonged); air passed through the fluid in the lower loop to fill the space in the first upper loop, between the two masses of fluid, left vacant by the condensation of steam.

The tube was then hung up, away from direct sunlight, and exposed to the ordinary changes of temperature of my study.

If I have been able to describe intelligibly this very simple matter, it will be seen that I had here two portions of the same fluid separated from each other; both having been heated to the same temperature and both exposed to atmospheric air.

The conditions were precisely similar with one exception; intentional and crucial. The air in the first upper loop, to which air only the fluid in the first leg was exposed, had passed through and been washed by the fluid in the lower loop; and the fluid of this loop was on one side exposed to the washed air and on the other side to the ordinary atmosphere.

In experiments with this apparatus the phenomena were, in eight cases, as follows :—On the second or third day the fluid in the loop was milky, and the fluid in the first leg was bright. At the end of a week, a month, four months, indeed as long as the tube was kept, the one continued clear, the other was turbid. At the expiration of a time, varying in different experiments from four days to four months, I tilted over the least drop of the turbid fluid in the loop into the clear fluid in the first leg, when at once the milkiness began, and in a day the whole of the leg fluid was turbid also.

In many cases I examined the two fluids, clear and turbid, with a twelfth-inch object-glass, and found Bacteria in the turbid fluid; nothing in the clear fluid.

Twice I left (once unintentionally, once intentionally) so little fluid in the loop that, there being a small aperture, it did not fulfil its purpose as a filter and a valve, and in both cases the two masses of fluid became turbid at the same time.

In six other experiments I used urine; in four instances the loop fluid showed symptoms of putrefaction, and became turbid in four or five days, but the leg fluid remained clear. On the closure of the experiment, at varying periods from a week to four months, the bright urine appeared, on microscopic examination, to contain no organic growth, but underwent putrefaction as ordinary urine when exposed to the air.

In the two other experiments both urines putrefied at the same time. In one case I hastened the cooling by cold; in the other I left very little fluid in the loop.

In four experiments I used Dr. Charlton Bastian's turnip-cheese fluid. In all cases the solution was milky when made; twice it was filtered and twice unfiltered, and in all cases, when examined by the microscope after the lapse of some days or weeks, the fluid in both leg and loop contained organic growth.

The experiments on urine and urine-sugar fluid show, in my view, both positively and negatively, that there is something in the ordinary air which is a necessary condition of the origin of organic growth in these liquids.

Positively this position is demonstrated when, after six months, the fluid in contact with unwashed air is seen to be full of organic growth, and the fluid in contact with washed air is still unchanged.

Negatively it is supported when both fluids are seen to grow turbid at the same time from imperfect washing of the air, by reason of too rapid cooling or too scanty a supply of fluid for the washing.

The experiments with Dr. Bastian's turnip-cheese fluid were for some time a puzzle to me, and made me fear that there was an undetected fallacy in my other experiments. But now it is clear that the contradiction is only apparent. Dr. Burdon Sanderson has shown that this fluid contains within itself the elements of organic growth which are not destroyed at 212° F., the temperature at which my experiments were necessarily conducted.

I am anxious not to press these experiments unduly, but they seem to me to range themselves unequivocally in opposition to the theory of spontaneous generation; although they touch no great extent of the subject.

That the something in the ordinary air necessary for the origin of organic growth in the fluids used is a gaseous impurity of the air is supported by no fact of which I am aware; but whether it be living organised germ or dead unorganised matter, these experiments do not explain or attempt to explain.

LEONARD W. SEDGWICK