of the vessel lower than its neck, the other, the efferent, reached to within one centimetre of the liquid, in order the better to remove the heavy stagnant gases. To each of the tubes was attached Liebig's bulbs filled with sulphuric acid. To the efferent was further attached

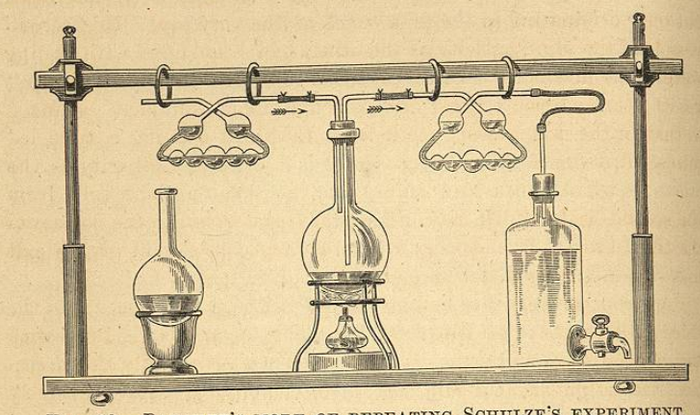

FIG. 59.—POUCHET'S MODE OF REPEATING SCHULZE'S EXPERIMENT. (FROM POUCHET).

a water aspirator, which was kept constantly acting (Fig. 59). The cork, &c., were all luted with copal. The fluid was then boiled for an hour. In twenty-six days spirilla, vibriones, and penicillium were present in the fluid.

In order, as he supposes, to have more rigorous conditions than those of Schulze, he introduces the following method of procedure:

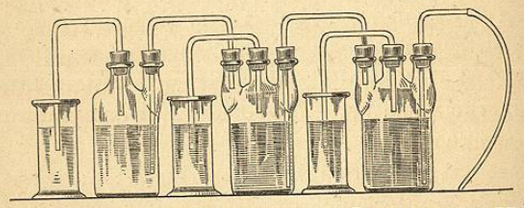

FIG. 60.—ANOTHER MODE ADOPTED BY POUCHET FOR TESTING SCHULZE'S VIEWS (FROM POUCHET).

A flask is used having the capacity of one litre, and having three necks. This is quite filled with concentrated sulphuric acid. The first neck contains a bent tube, which by one extremity communicates with an air pump, while the other extremity passes down to the

bottom of the acid; through the middle neck passes a siphon, which commences in the flask at the level of union of the upper and middle thirds, while its other end passes into an empty vessel. The third orifice contains a tube passing down to the bottom of a second vessel, but only originating in the first vessel at the very top. This second vessel, of the same capacity as the other, had been filled with boiling water; the first neck allowed the passage of the tube from the first vessel; through the central one passed a siphon similarly arranged to that in the first vessel: while from the third opening a tube led into a third vessel. This third vessel had the same capacity as the others, but had only two openings, the first receiving the tube from the second flask, which, however, did not pass down to the bottom of the third flask as in the other two cases; and the second giving exit to a siphon arranged as before (Fig. 60).

A strong decoction of boiling hay having been introduced into the third vessel so as to fill it exactly, the whole apparatus was luted with the greatest care, and thus the apparatus contained only this decoction, the sulphuric acid and the water which had been previously boiled. Air was then forced slowly into the sulphuric acid, and traversing the acid, it collected at the top, forcing the sulphuric acid through the siphon; this air then passed on through the second flask, passing through the water and forcing the water through the siphon; and from the second into the third vessel, forcing some of the decoction through the siphon; then the apparatus was abandoned to itself, and a quantity of the same decoction was placed outside for comparison. In twenty days the hay infusion, both in the apparatus and in the vessel outside, contained 'mucor' and bacteria.

A second experiment was done by introducing into the third flask 5 grammes of hay, which had been exposed to the action of steam for five minutes. The vessel was then filled with boiling water. A similar result was obtained.

Again, he introduced into the third flask filtered water, and 10 grammes of hay exposed for thirty minutes to a temperature of 200° C. Nevertheless, monads and vibriones were found.

A similar experiment to Schwann's was also performed, the air in the apparatus just described being passed over a heated tube instead of through sulphuric acid. Nevertheless, organisms (penicillium and vibriones) developed. Two flasks (instead of three) were used, the first being filled with boiling water (this flask having the three necks as usual, one receiving the heated air, one for the siphon, and the third leading into the second

flask), and the second containing boiled decoction of hay and having two necks. He states that he succeeded more often in obtaining organisms when filtered, unboiled water was substituted for the boiled water; and, as he has already proved, as he thinks, that water does not contain organisms, he considers it a matter of indifference whether the water has been boiled or not!

Similar experiments with *artificial air* yielded the same results.

A new objection is then brought forward, viz., that if the atmosphere be the universal disseminator of germs, the greater the mass of air in contact with a fluid, so much the greater ought to be the number of organisms which grow in that fluid. This he has not found to be the case, and he also points out that in flasks containing the same fluid, placed close to each other at the same time, different forms of organisms may develop, a fact which he does not consider capable of explanation on the theory of Panspermism.

Pouchet further examined *air dust* microscopically, and he admits that he finds spores of fungi and other microscopic organisms, but he does not think that they are ever present in sufficient quantities to explain the numbers which are found in an infusion, for he states that the bacteria themselves do not increase in number by fission (or but very slowly), but that on the contrary each is spontaneously generated.

To prove that organisms do not in the main come from the air he took five grammes of dust from the roof of the Rouen Cathedral, and placed it in 100 grammes of distilled water. Then in another similar vessel he placed 100 grammes of distilled water, containing five grammes of the dry stalks of China aster, previously exposed for two hours to a temperature of 200° C. These two vessels were placed under the same glass shade. Eight days later the first contained vibriones, monads in small numbers, and a few kolpodes. The second was full of monads and kolpodes.

During a later research Pouchet examined the dust which collected in the lungs and bones of birds, and he states that there can always be found evidences of the locality in which the birds lived. For instance, this dust in the case of forest

birds contains fragments of wood, leaves, starch, &c. He says that he never found spores of plants nor ova of microscopic animals nor encysted animalculæ. That he should not have found spores of fungi, which one would think are more numerous in the forest air than starch granules, is inconceivable, unless it is supposed that there is a special provision in the animal economy which prevents their entrance into the lungs, though permitting the admission of starch granules. The probability is that he did not recognise them.

Not only does Pouchet support the doctrine of the spontaneous generation of bacteria and the lowest forms of animal or vegetable life, but he is also prepared to accept the spontaneous origin of fleas, acari, and cysticerci!

If, now, we carefully examine these experiments, we shall see that they are full of the grossest blunders. Take, for instance, his experiments to show that organisms are not present in putrescible substances, in water or in air. The first experiment is simply absurd. Some seeds are charred and introduced into a vessel containing distilled water, and covered with a bell jar; organisms develop. What does this prove? Does it prove that organisms were not present and may not constantly be present in putrescible substances exposed to the air? No. It merely proves that this cannot be their only source; for here the water and the air were not heated or otherwise purified, and therefore the organisms might be easily communicated through them.

Again, to prove that water does not contain them, an artificial water is prepared, boiled for a quarter of an hour, placed in an unpurified vessel containing some hay, which had been previously heated and exposed to ordinary air. What does this show? Certainly not that ordinary water does not contain organisms. Even admitting that this specimen of water did not contain them, there was ample explanation of their presence from the fact that the fluid was put into an impure vessel, and that the air had access to it.

Nor is Pouchet more successful in his attempt to show that the air is not the vehicle.

In considering the question as to the presence of organisms or their spores in the air Pouchet puts forward the idea that if

organisms were present in the atmosphere in sufficient numbers the atmosphere would be totally obscured. But it has been asserted and shown by Pasteur and others (as we shall see later on) that organisms are by no means so numerous in the atmosphere as was formerly supposed, but that they are generally derived from dust which has settled or from water. Further, Tyndall has shown, by means of the beam of light, what numbers of minute particles fill the air around us, and up to a certain point, instead of obscuring it, really render the light visible.

In his repetition of Schulze's experiment it must be admitted that Pouchet has a stronger case, but even here the flask and the tubes were not purified, the quantity of fluid as compared with the size of the flask was very small, and there is always the possibility of a flaw in the cork or in the joinings of the various tubes. And further, this experiment loses its force when Pouchet admits that he does not always get organisms, and states, on the contrary, that, when a simple apparatus is employed, a negative result is obtained. Thus to quote his own words: 'Dans un appareil à simple rentrée d'air' (this consists of a flask having only one tube passing through its cork, to which tube Liebig's bulbs are attached, see Fig. 61), 'et dont les boules de Liebig contenaient de l'eau, on remplit le tiers du ballon de colle de farine légère: que l'on y tient quinze minutes en ébullition à l'aide d'une lampe. Celle-ci éteinte, l'air rentra dans l'appareil en traversant l'eau peu-à-peu. L'appareil fut abandonné deux mois à une température moyenne de 14 degrés, et pas la moindre moisissure ne se déclara à la surface de la colle durant tout ce temps.

'Au contraire un critérium, placé à côté et en contact avec l'atmosphère, avait au bout de cinq jours toute sa surface envahie par des champignons.

'Une expérience enterprise le même jour et dans les mêmes conditions, mais dans laquelle l'air est introduit dans l'appareil en traversant des boules de Liebig remplies d'acide sulfurique, donna absolument les mêmes résultats.'

Thus Pouchet showed that not only was Schulze's experiment successful, when performed with a simple apparatus, but he further demonstrated that it was not necessary that the air

should pass through sulphuric acid; if it were merely *washed in water* it was sufficient.

Again, 'Dans notre appareil à simple rentrée d'air, et dont les boules étaient remplies d'eau, on mit 175 grammes d'urine humaine et on l'y tint en ébullition pendant un quart d'heure. Ensuite l'air rentra en traversant l'eau et l'appareil fut abandonné sous l'influence d'une température moyenne de 12 degrés. Deux mois après, l'urine était encore parfaitement limpide et pas la moindre moisissure ne s'était déclarée à sa surface. Un critérium placé à côté, au bout de huit jours, était envahi par une abondante végétation cryptogamique.

'Une expérience est faite le même jour et absolument dans les mêmes circonstances, seulement l'air ne rentre dans l'appareil qu'en traversant des boules remplies d'acide sulfurique. Le résultat est absolument le même que dans l'expérience précédente: l'urine est intacte.'

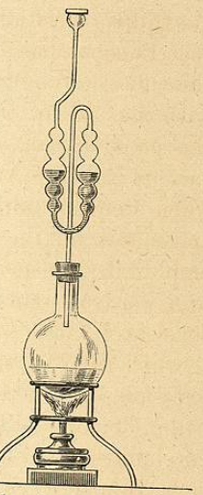

FIG. 61.—A SIMPLE MODE OF REPEATING SCHULZE'S EXPERIMENTS (FROM POUCHET).

Such are examples of Pouchet's general results with this simple apparatus, and, when he states that with the more complicated arrangement, even with much more prolonged boiling, he generally obtains cryptogamic vegetation, I do think that I am only drawing a fair inference when I suppose that there was something defective about his apparatus.

Nor is he more fortunate with Schwann's experiment. Referring to Pasteur's results—that when he (Pasteur) performed Schwann's experiment of having in contact with fermentescible substances only previously heated air, he obtained neither fermentation, nor yeast, nor infusoria—he says: 'L'air calciné a ici encore arrêté la fermentation et les produits organiques qui en dérivent; cet air est donc également impropre au développement de phénomènes chimiques, comme il l'est à celui des phénomènes vitaux. L'expérience de

Schwann, et celles qui ont été calquées sur elle, sont donc absolument insignifiantes.' Truly a strange interpretation of the facts, more especially as he had previously attempted to demonstrate that calcined air and air which had previously passed through sulphuric acid were equally incapable of preventing the development of organisms, and as in the 'more rigorous' conditions under which he performed the experiments he found that organisms always developed!

Looking more closely at these so-called 'more rigorous' conditions, we find that they contain a sufficient number of loopholes to explain the result.

What are we to think of the third experiment to show that the air is not the vehicle of these organisms? An impure vessel is taken, containing dust, and unboiled filtered water is introduced into it along with some heated hay. Is it any matter for surprise that organisms are found here, however pure the air? The dust in the vessel contained plenty of their spores, the various tubes passing into it contained dust and impure air, while the water itself was teeming with organisms or their spores. And similar objections may be urged against the first and most rigorous experiment. Boiling hay infusion is introduced into a vessel containing dust; no attempt is made to destroy the septic properties of this dust or to purify the air and the dust in the tubes leading to and from it. Air is now forced into the infusion, carrying along with it the organisms which Pouchet ought to have known were theoretically present in the air in the last connecting tube, as well as on the walls of that tube. This is the 'rigorous' experiment, which is sufficient 'to overturn the experiments of Schwann and Schulze.' It is needless to pursue the criticism of such methods.

The following experiment is stronger, and he considers it sufficient to upset those of Schwann and Schulze. A flask was introduced to the bottom of a vessel containing a decoction of barley, which had been kept boiling for six hours. The flask being completely filled with this fluid, was brought to the surface and corked, and then the circumference of the cork was surrounded by varnish. On the sixth day a deposit of yeast was seen, and the flask burst on the seventh.

Here an impure vessel, cork, &c., were used, and the heat

was not applied to them for a sufficient length of time. Further, the fluid cannot have been boiling when the vessel was introduced and corked, otherwise, if he had done it efficiently, the operator would have scalded his fingers. It is, moreover, noteworthy that Pouchet only got organisms in one experiment of this kind.

In his 'Micrographie Atmosphérique,' Pouchet falls into the great error of assuming that organisms do not subdivide quickly enough to account for their rapid appearance in infusions. Now it has since been made out by several observers that organisms do divide with sufficient rapidity, and of this I may mention two instances from Mr. Lister's work. In the course of observations on a form of micrococcus, which he terms 'Granuligera,' he found that in little more than an hour and a half they had trebled their numbers, a fact which he made out by observing the subdivision of a little group. And with regard to the *Bacterium lactis*, he found definitely by numeration that it doubled by fission in an hour, so that given one *Bacterium lactis* there would be in twenty-four hours no less than 8,388,608 bacteria; and other forms of organisms develop even more quickly.

Then, again, Pouchet objects that with free access of air, a *greater variety of forms* ought to be found, but it must be remembered that what may nourish one form may not be good for another, and that the products of the growth of the form which in the first instance was most vigorous and most numerous, may interfere seriously with the growth of other varieties; and in the experiment which he mentions it is quite evident that in the vessel to which the dust was added there is little or no nutriment compared with that contained in the stalks of the China aster. And lastly, when he places similar infusions, similarly treated, in flasks of the same size, under the same glass shade, and in similar conditions, and finds that the organisms which appear differ in form in the two flasks, he does not obtain, as he supposes, a proof of spontaneous generation, but the contrary. For on that theory the same infusion, in the same conditions, ought to give rise to the same species of organisms; and the occurrence of different forms can only be explained by supposing that different spores gained access to

the various infusions, a view quite in accordance with the theory of Panspermism.

Appearing shortly after Pouchet's work, and leading to diametrically opposite conclusions, were the researches of M. Pasteur, which have by many been considered as administering the death-blow to the theory of heterogeny.

Pasteur,[1] in his account in the 'Annales des Sciences naturelles,' begins by attempting to demonstrate the existence of spores in the atmosphere, a fact which Pouchet had previously admitted. Such attempts are, however, very unsatisfactory, partly from the difficulty of recognising what are and what are not spores, and also from the fact that if spores do exist they must be so excessively minute as to be in many cases invisible under the microscope. Professor Tyndall has shown, by means of the condensed beam of light, the existence of innumerable solid particles in fluids, in which but few could be detected by the microscope. Pasteur certainly demonstrated, and this is generally admitted, that spores of fungi do occur in the atmosphere. It is not necessary, however, to have this demonstration of the existence of spores, for the matter can be set at rest by experiment alone, and it is these experiments, and not the demonstration of the existence of spores of fungi in the atmosphere, which give the value to Pasteur's work.

Operating with an albuminous saccharine fluid, in the manner described below, Pasteur always succeeded in preventing the growth of organisms in that liquid, in presence of heated air. 'J'ai certainement eu l'occasion de répéter plus de cinquante fois l'expérience, et, dans aucun cas, cette liqueur, si altérable, n'a donné vestige de productions organisées en présence de l'air calciné.'

Into a flask with a capacity of 250 to 300 c.cm. were introduced 100 to 150 c.cm. of the saccharine albuminous fluid. The neck of this flask, which had been drawn out, was then connected with a platinum tube in which the air could be raised to a very high temperature. The fluid was boiled for two or three minutes, and then the calcined air allowed to enter.

Although the experiment succeeded in the case of the

[1] *Annales des Sciences naturelles*, série 4, t. xvi. 1861.

saccharine albuminous material, and some other fluids, it did not succeed with milk. Leaving milk out of consideration for a moment, it was shown that other fluids which, although previously boiled, when exposed to ordinary air, rapidly become the seat of development of organisms, remain barren when exposed to heated air. (It has been remarked by several authors, especially by Pouchet, that Pasteur was unable to succeed, in many cases, in repeating Schwann's experiment with calcined air. This is true: but the experiment which failed was the one where the flask is inverted over mercury, and the calcined air then introduced, and Pasteur has pointed out that it is from the mercury that the source of contamination is derived.) It remained to enquire further what happened when dust which had not been heated was introduced into the

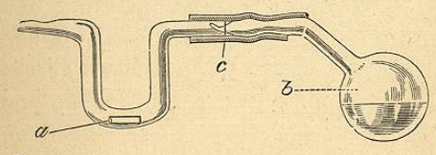

FIG. 62.—PASTEUR'S MODE OF INTRODUCING DUST INTO FLASK CONTAINING CALCINED AIR.

b, the flask containing the infusion sealed after being filled with heated air. *a*, piece of asbestos containing dust. When the tube in which this rests has been purified and filled with heated air, the neck of the flask *b* is broken at *c*, and the dust slipped in. The neck of the flask is again sealed. (From Pasteur.)

fluid in presence of heated air. This was done; the air coming in contact with the boiled fluid was, as in the former case, previously heated, and the fluid had remained barren for one or two months; then precautions being taken that no unheated air was introduced, unheated dust was put into the flask (Fig. 62), and, as a consequence of this, development of organisms rapidly occurred, and these organisms were of the same kind as those which appeared in fluids freely exposed to air. Thus Pouchet's objection to Schwann's experiment (viz., that heated air interfered with the growth of organisms) was completely upset.

Fresh urine behaves in exactly the same manner when treated in the two ways described.

In the case of *milk* Pasteur was, however, unsuccessful when the boiling temperature was maintained for only two or

three minutes (a length of time quite sufficient in the former cases); and this want of success he believes to be due to the fact that in this fluid organisms can resist the boiling temperature for a longer period than in other liquids. Two or three minutes of a temperature of 110° C., or a prolonged temperature of 100° C., were, however, sufficient to sterilise milk. This resisting power he considers to be due to the alkalinity of the fluid, for he finds the same difficulty if he renders his sugared yeast water alkaline by the addition of carbonate of lime. In the case of milk and of the latter fluid, thus purified and preserved unchanged for some weeks, the addition of the atmospheric dust, in the way described, produces the same results as in the fluids previously referred to.

But it is not necessary to use calcined air, ordinary air will be equally inert, if only it has had opportunity to deposit its dust before being admitted into the flask. This he showed to be the case by his well-known experiments with flasks with bent necks. Into a flask a suitable quantity of the fluid to be experimented on is introduced, the neck is then drawn out long and bent in various directions (Pasteur only figures one long curve) so as to present obstructions to the entrance of solid particles along with the air (see Fig. 3, p. 16). The fluid is then boiled for the requisite length of time, and, the lamp being removed, ordinary air is allowed to enter. Fluids may be prepared in this way, and preserved for any length of time desired. On the other hand, if the neck be straight, so as to allow the dust to fall readily into the flask, organisms appear, however long the neck. In the same way, if the neck be broken off short, so that there is no obstruction to the entrance of dust, organisms rapidly develop.

In the case of milk the boiling point must be raised to about 110° by heating the fluid under pressure, or the milk must be boiled for a long time.

That the cause of the growth of these organisms is not continuous, as would be the case were it some known or unknown force, but that it is the advent to the fluids of solid particles floating in the atmosphere, is likewise demonstrated by the following experiment. Flasks containing the saccharine yeast solution were prepared, and sealed during boiling. These

flasks being opened in different places, with certain precautions against the admission of dust from its neck, &c., air rushed in, carrying with it any suspended dust. The neck of the flask was again sealed, so as to prevent any further entrance of dust. It was thus found that the air in some places and under some conditions contained none of the particles which give rise to organisms, while, on the other hand, when they were present the organisms might be of very various kinds. One of the situations in which such flasks could be opened without any development of organisms, was in some cellars which had not been entered for a long time, and in which the dust had therefore settled. Precautions were of course taken that the operator neither introduced the particles himself nor stirred up the dust of the cellar.

I may quote one experiment: Le 14 août 1860 j'ai ouvert et renfermé dans les caves de l'Observatoire dix ballons contenant de l'eau de levûre de bière, et onze autres ballons de la même préparation dans la cour de l'établissement, à 50 centimètres du sol, par un vent léger. Tous ont été rapportés le même jour dans l'étuve de mon laboratoire, dont la température est de 25 à 30 degrés. J'ai conservé jusqu'à ce jour tous ces ballons. Un seul de ceux ouverts dans les caves renferme une production végétale. Les onze ballons ouverts dans la cour ont tous fourni des Infusoires ou des végétaux du genre de ceux que j'ai déjà décrits.'

But the greatest blow was given to the views of the heterogenists when Pasteur demonstrated that albuminoid materials are not necessary for the development of bacteria and fungi, but that they can be replaced by crystalline salts, such as phosphates and salts of ammonia. He prepared a fluid of the following composition, in which these organisms readily grew:—

Eau pure	100 grammes
Sucre candi	10 „
Tartrate d'ammoniaque	2 à 5 „
Cendres fondues de levûre de bière	4 „

'Si l'on sème dans cette liqueur, en présence de l'air calciné, les poussières qui existent en suspension dans l'air,