in a vertical straight line, the distance P C in Fig. 20 being the same as it was in Fig. 19; but from a well-known property of a circle, if H be any one of the holes pierced in the piece, the angle H P′ P is constant, thus the straight line H P′ is fixed in position, and H moves along it; similarly all the other holes move along in straight lines passing through the fixed pivot P′, and we get straight line motion distributed in all directions. This species of motion is called by Prof. Sylvester "tram-motion." It is worth noticing that the motion of the circular disc is the same as it would have been if the dotted circle on it rolled inside the large dotted circle; we have, in fact, White's parallel motion reproduced by linkwork. Of course, if we only require motion in one direction, we may cut away all the disc except a portion forming a bent arm containing C, P, and the point which moves in the required direction.

The double kite of Fig. 18 may be employed to form some other useful linkworks. It is often necessary to have, not a single point, but a whole piece moving so that all points on it move in straight lines. I may instance the slide rests in lathes, traversing tables, punches, drills, drawbridges, &c. The double kite enables us to produce linkworks having this property. In the linkwork of Fig. 21, the construction of which will be

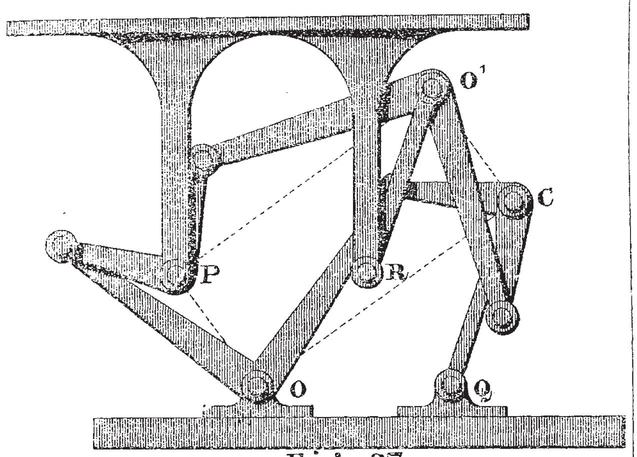

Fig 27.

at once appreciated if you understand the double kite, the horizontal link moves to and fro as if sliding in a fixed horizontal straight tube. This form would possibly be useful as a girder for a drawbridge.

In the linkwork of Fig. 22, which is another combination of two double kites, the vertical link moves so that all its points move in horizontal straight lines. There is a modification of this linkwork which will, I think, be found interesting. In the linkage in Fig. 23, which, if the thin links are removed, is a skeleton drawing of Fig. 22, let the dotted links be taken away and the thin ones be inserted; we then get a linkage which has the same property as that in Fig. 22, but it is seen in its new form to be the ordinary double parallel ruler with three added links. Fig. 24 is a figure of a double parallel rule made on this plan with a slight modification. If the bottom ruler be held horizontal the top moves vertically up and down the board, having no lateral movement.

While I am upon this sort of movement I may point out an apparatus exhibited in the Loan Collection by Prof. Tchebicheff which bears a strong likeness to a complicated camp-stool, the seat of which has horizontal motion. The motion is not strictly rectilinear; the apparatus being, as will be seen by observing that the thin line in the figure is of invariable length, and a link might therefore be put where it is, a combination of two of the parallel motions of Prof. Tchebicheff given in Fig. 4, with some links added to keep the seat parallel with the base. The variation of the upper plane, from a strictly horizontal movement is therefore double that of the tracer in the simple parallel motion.

Fig. 26 shows how a similar apparatus of much simpler construction employing the Tchebicheff approximate parallel motion can be made. The lengths of the links forming the parallel motion have been given before (Fig. 4). The distance between the pivots on the moving seat is half that between the fixed pivots, and the length of the remaining link is one-half that of the radial links.

An *exact* motion of the same description is shown in Fig. 27. O, C, O′, P are the four *foci* of the quadriplane shown in the figure in which the links are bent through a right angle, so that O C · O P is constant, and C O P a right angle. The focus O is pivoted to a fixed point, and C is made by means of the extra link Q C to move in a circle of which the radius Q C is equal to the pivot distance O Q. P consequently moves in a straight line parallel to O Q, the five moving pieces thus far described constituting the Sylvester-Kempe parallel motion. To this are added the moving seat and the remaining link R O′, the pivot distances of which, P R and R O′, are equal to O Q. The seat in consequence always remains parallel to Q O, and as P moves accurately in a horizontal straight line, every point on it will do so also. This apparatus might be used with advantage where a very smoothly-working traversing table is required.

(*To be continued.*)

SPONTANEOUS GENERATION[1]

THE investigation embodied in the memoir now submitted to the Society was opened in the summer of 1876 by a series of tentative experiments on turnip-infusions, to which were added varying quantities of bruised or pounded cheese. I was soon, however, drawn away from them to other experiments on infusions of hay. With this substance no difficulty was encountered in my first inquiry. Boiled for five minutes, and exposed to air purified spontaneously or freed from its floating matter by calcination or filtration, hay infusion, though employed in multiplied experiments at various times, never showed the least competence to kindle into life. After months of transparency, I have, in a great number of cases, inoculated this infusion with the smallest specks of animal and vegetable liquids containing *Bacteria*, and observed twenty-four hours afterwards, its colour lightened, and its mass rendered opaque by the multiplication of these organisms.

But in the autumn of 1876, the substance with which I had experimented so easily and successfully a year previously, appeared to have changed its nature. The infusions extracted from it bore in some cases not only five minutes' but fifteen minutes' boiling with impunity. But on changing the hay a different result was often obtained. Many of the infusions extracted from samples of hay purchased in the autumn of 1876, behaved exactly like those extracted from the hay of 1875, being completely sterilized by five minutes' boiling.

To solve these discrepancies, numerous and laborious experiments were executed with hay derived from different localities, and by this means in the earlier days of the inquiry, it was revealed that the infusions which manifested this previously unobserved resistance to sterilization were, one and all, extracted from old hay, while the readily sterilized infusions were extracted from new hay, the germs adhering to which had not been subjected to long-continued desiccation.

I then fell back upon infusions whose deportment had

[1] "Further Researches on the Deportment and Vital Resistance of Putrefactive and Infective Organisms, from a Physical Point of View." By John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution.—Abstract.

been previously familiar to me, and in the sterilization of which I had never experienced any difficulty. Fish, flesh, and vegetables were re-subjected to trial. Though the precautions taken to avoid contamination were far more stringent than those observed in my first inquiry, and though the interval of boiling was sometimes tripled in duration, these infusions, in almost every instance, broke down. Spontaneously purified air, filtered air, and calcined air,—calcined, I may add, with far greater severity than was found necessary a year previously,—failed, in almost all cases, to protect the infusions from putrefaction.

I had the most implicit confidence in the correctness of my earlier experiments; indeed, incorrectness would have led to consequences exactly opposite to those arrived at. Errors of manipulation would have filled my tubes and flasks with organisms instead of leaving them transparent and void of life. By the unsuccessful experiments above referred to a clear issue was therefore raised: Either the infusions of fish, flesh, and vegetable had become endowed in 1876 with an inherent generative energy which they did not possess in 1875, or some new contagium external to the infusions, and of a far more obstinate character than that of 1875, had been brought to bear upon them. The scientific mind will not halt in its decision between these two alternatives.

For my own part the gradual but irresistible interaction of thought and experiment rendered it at first probable, and at last certain that the atmosphere in which I worked had become so virulently infective as to render utterly impotent precautions against contamination, and modes of sterilization, which had been found uniformly successful in a less contagious air. I therefore removed from the laboratory, first to the top, and afterwards to the basement of the Royal Institution, but found that even here, in a multitude of cases, failure was predominant, if not uniform. This hard discipline of defeat was needed to render me acquainted with all the possibilities of infection involved in the construction of my chambers and the treatment of my infusions.

I finally resolved to break away from the Royal Institution, and to seek at a distance from it a less infective atmosphere. In Kew Gardens, thanks to our President, the requisite conditions were found. I chose for exposure in the Jodrell laboratory the special infusions which had proved most intractable in the laboratory of the Royal Institution. The result was that liquids which in Albemarle Street resisted two hundred minutes boiling, becoming fruitful afterwards, were utterly sterilised by five minutes' boiling at Kew.

A second clear issue is thus placed before the Royal Society:—Either the infusions had lost in Kew Gardens an inherent generative energy which they possessed in our laboratory, or the remarkable instances of life development, after long-continued boiling, observed in the laboratory are to be referred to the contagium of its air.

With a view to making nearer home experiments similar to those executed at Kew, I had a shed erected on the roof of the Royal Institution. In this shed infusions were prepared and introduced into new chambers of burnished tin, which had never been permitted to enter our laboratory. After their introduction the liquids were boiled for five minutes in an oil-bath.

The first experiment in this shed resulted in complete failure, the air of the shed proving to be sensibly as infective as the air of the laboratory.

Either of two causes, or both of them combined, might, from my point of view, have produced this result. First, a flue from the laboratory was in free communication with the atmosphere not far from the shed; secondly, and this was the real cause of the infection, my assistants in preparing the infusions, had freely passed from the laboratory to the shed. They had thus carried the contagium by a mode of transfer known to every physician.

The infected shed was disinfected; the infusions were again prepared, and care was taken, by the use of proper clothes, to avoid the former causes of contamination. The result was similar to that obtained at Kew, viz., organic liquids which in the laboratory withstood two hundred minutes' boiling, were rendered permanently barren by five minutes' boiling in the shed.

A third clear issue is thus placed before us, which I should hardly think of formulating before the Royal Society, were it not for the incredible confusion which apparently besets this subject in the public mind. A rod thirty feet in length would stretch from the infusions in the shed to the same infusions in the laboratory. At one end of this rod the infusions were sterilized by five minutes' boiling, at the other end they withstood two hundred minutes' boiling. As before, the choice rests between two inferences:—Either we infer that at one end of the rod animal and vegetable infusions possess a generative power, which at the other end they do not possess; or we are driven to the conclusion that at the one end of the rod we have infected, and at the other end disinfected air.

The second inference is that which will be accepted by the scientific mind. To what, then, is the inferred difference at the two ends of the rod to be ascribed? In one obvious particular the laboratory this year differed from that in which my first experiments were made. On its floor were various bundles of old and desiccated hay, from which, when stirred, clouds of fine dust ascended into the atmosphere. This dust proved to be both fruitful and in the highest degree resistant. Prior to the introduction of the hay which produced the dust, no difficulty as regards sterilization had ever been experienced; subsequent to its introduction my difficulties and defeats began.

In these and numerous other experiments a method was followed which had been substantially employed by Spallanzani and Needham; and more recently by Wyman and Roberts, the method having been greatly refined by the philosopher last named. The flasks containing the infusions were only partially filled, the portions unoccupied by the liquids being taken up with ordinary unfiltered air. Now as regards the death-point of contagia, we know that in air it is higher than in water, the self-same temperature being fatal in the latter and sensibly harmless in the former. Hence my doubt whether, in my recent experiments, the resistance of the contagium did not arise from the fact that it was surrounded, not by water but by air.

I changed the method, and made a long series of experiments with filtered air. They were almost as unsuccessful as those made with ordinary air.

One source of discomfort clung persistently to my mind throughout these experiments. I was by no means certain that the observed development of life was not due to germs entangled in the film of liquid adherent to the necks and higher interior surfaces of the bulbs. This film might have dried, and its germs, surrounded by air and vapour, instead of by water, might on this account have been able to withstand an ordeal to which they would have succumbed if submerged.

A plan was, therefore, resorted to by which the infusions were driven by atmospheric pressure through lateral channels issuing from the centres of the bulbs. As before, each bulb was filled with one-third of an atmosphere of filtered air, and afterwards heated nearly to redness. When fully charged, the infusion rose higher than the central orifice, and no portion of the internal surface was wetted save that against which the liquid permanently rested. The lateral channel was then closed with a lamp without an instant's contact being permitted to occur between any part of the infusion and the external air. It was thus rendered absolutely certain that the contagia exposed subsequently to the action of heat were to be sought, neither in the superjacent air nor on the interior surfaces of the flasks, but in the body of the infusions themselves.

By this method I tested in the first place the substance which, at an early stage of the inquiry, had excited my suspicion—without reference to which the discrepancy between the behaviour of infusions examined in the winter of 1875-76 and those examined in the winter of 1876-77 is inexplicable, but by reference to which the explanation of the observed discrepancy is complete—I mean the old hay which cumbered our laboratory floor.

Four hours' continuous boiling failed to sterilise bulbs charged with infusions of this old hay. In special cases, moreover, germs were found so indurated and resistant, that five, six, and in one case even eight hours' boiling failed to deprive them of life. All the difficulties encountered in this long and laborious inquiry were traced to the germs which exhibited the extraordinary powers of resistance here described. They introduced a plague into our atmosphere—the other infusions, like a smitten population, becoming the victims of a contagium foreign to themselves.[1]

It is a question of obvious interest to the scientific surgeon whether those powerfully resistant germs are amenable to the ordinary processes of disinfection. It is perfectly certain that they resist to an extraordinary extent the action of heat. They have been proved competent to cause infusions, both animal and vegetable, to putrefy. How would they behave in the wards of a hospital? There are, moreover, establishments devoted to the preserving of meats and vegetables. Do they ever experience inexplicable reverses. I think it certain that the mere shaking of a bunch of desiccated hay in the air of an establishment of this character might render the ordinary process of boiling for a few minutes utterly nugatory, thus possibly entailing serious loss. They have, as will subsequently appear, one great safeguard in the complete purgation of their sealed tins of air.

Keeping these germs and the phases through which they pass to reach the developed organism clearly in view, I have been able to sterilise the most obstinate infusions encountered in this inquiry by heating them for a minute fraction of the time above referred to as *insufficient* to sterilise them. The fully developed Bacterium is demonstrably killed by a temperature of 140° F. Fixing the mind's eye upon the germ during its passage from the hard and resistant to the plastic and sensitive state, it will appear in the highest degree probable that the plastic stage will be reached by different germs in different times. Some are more indurated than others and require a longer immersion to soften and germinate. For all known germs there exists a period of incubation during which they prepare themselves for emergence as the finished organisms which have been proved so sensitive to heat. If during this period, and well within it, the infusion be boiled for even the fraction of a minute, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, and before the least *sensible* change has occurred in the infusions, each successive heating will destroy the germs then softened and ready for destruction, until after a sufficient number of heatings the last living germ will disappear.

Guided by the principle here laid down, and applying the heat discontinuously, infusions have been sterilised by an aggregate period of heating, which, fifty times multiplied, would fail to sterilise them if applied continuously. Four minutes in the one case can accomplish what four hours fail to accomplish in the other.

If properly followed out the method of sterilisation here described is infallible. A temperature, moreover, far below the boiling point suffices for sterilisation.

Another mode of sterilisation equally certain, and perhaps still more remarkable, was forced upon me, so to speak, in the following way:—In a multitude of cases a thick and folded layer of fatty scum, made up of matted *Bacteria*, gathered upon the surfaces of the infusions, the liquid underneath becoming sometimes cloudy throughout, but frequently maintaining a transparency equal to that of distilled water. The living scum-layer, as Pasteur has shown in other cases, appeared to possess the power of completely intercepting the atmospheric oxygen, appropriating the gas and depriving the germs in the liquid underneath of an element necessary to their development. Above the scum, moreover, the interior surfaces of the bulbs used in my experiments were commonly moistened by the water of condensation. Into it the *Bacteria* sometimes rose, forming a kind of gauzy film to a height of an inch or more above the liquid. In fact, wherever air was to be found, the *Bacteria* followed it. It seemed a necessity of their existence. Hence the question, What will occur when the infusions are deprived of air?

I was by no means entitled to rest satisfied with an inference as an answer to this question; for Pasteur, in his masterly researches, has abundantly demonstrated that the process of alcoholic fermentation depends on the continuance of life without air—other organisms than *Torula* being also shown competent to live without oxygen. Experiment alone could determine the effect of exhaustion upon the particular organisms here under review. Air-pump vacua were first employed, and with a considerable measure of success. Life was demonstrably enfeebled in such vacua.

Sprengel pumps were afterwards used to remove more effectually both the air dissolved in the infusions and that diffused in the spaces above them. The periods of exhaustion varied from one to eight hours, and the results of the experiments may be thus summed up:—Could the air be completely removed from the infusions, there is every reason to believe that sterilisation *without boiling* would in most, if not in all cases, be the result. But, passing from probabilities to certainties, it is a proved fact, that in numerous cases unboiled infusions deprived of air by five or six hours' action of the Sprengel pump are reduced to permanent barrenness. In a great number of cases, moreover, where the unboiled infusion would have become cloudy, exposure to the boiling temperature for a single minute sufficed completely to destroy the life already on the point of being extinguished through defect of air. With a single exception, I am not sure that any infusion escaped sterilisation by five minutes' boiling after it had been deprived of air by the Sprengel pump. These five minutes accomplished what five hours often failed to accomplish in the presence of air.

The inertness of the germs in liquids deprived of air is not due to a mere *suspension* of their powers. They are *killed* by being deprived of oxygen. For when the air which has been removed by the Sprengel pump is, after some time, carefully restored to the infusion, unaccompanied by germs from without, there is no revival of life. By removing the air we stifle the life which the returning air is incompetent to restore.

[1] A hard and wiry hay from Guildford, which I have no reason to consider old, was found very difficult to sterilise.

AGRICULTURAL EXPERIMENTS AT WOBURN

In the autumn of 1875 Mr. C. Randell proposed to the Council of the Royal Agricultural Society that it be referred to the Chemical Committee to consider the propriety, and the manner, of instituting a series of experiments, to test the accuracy of the estimated value of manure obtained by the consumption of different articles of food, as given in Mr. Lawes' paper, in the Spring Number of the Journal of the Society.

As it was decided that experiments by practical farmers in different districts could not be relied on, the Duke of