OBSERVATIONS

ON THE

OPTICAL DEPORTMENT OF THE ATMOSPHERE IN REFERENCE TO THE PHENOMENA OF PUTREFACTION AND INFECTION.

Abstract of part of a Paper read before the Royal Society, January 13th, 1876.

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I. INTRODUCTION.—The author alluded, in an introduction, to an inquiry on the decomposition of vapours, and the formation of actinic clouds, by light, whereby he was led to experiments on the floating matter of the air. He refers to the experiments of Schwann, Schroeder and Dusch, Schroeder himself, to those of the illustrious French chemist Pasteur, to the reasoning of Lister and its experimental demonstration regarding the filtering power of the lungs; from all of which he concluded, six years ago, that the power of developing life by the air and its power of scattering light would be found to go hand in hand. He thought that the simple expedient of examining by means of a beam of light, while the eye was kept sensitive by darkness, the character of the medium in which their experiments were conducted, could not fail to be useful to workers in this field. But the method had not been much turned to account, and this year he thought it worth while to devote some time to the more complete demonstration of its utility.

He also wished to free his mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of "spontaneous generation". Pasteur has pronounced it "a chimera", and expressed the undoubting conviction that, this being so, it is possible to remove parasitic diseases from the earth. To the medical profession, therefore, and through them to humanity at large, this question is one of the last importance. But the state of medical opinion regarding it is not satisfactory. In a recent number of the BRITISH MEDICAL JOURNAL, and in answer to the question, "In what way is contagium generated and communicated?" Messrs. Braidwood and Vacher reply that, notwithstanding "an almost incalculable amount of patient labour, the actual results obtained, especially as regards the manner of generation of contagium, have been most disappointing. Observers are even yet at variance whether these minute particles, whose discovery we have just noticed, and other disease germs, are always produced from like bodies previously existing, or whether they do not, under certain favourable conditions, spring into existence *de novo.*"

With a view to the possible diminution of the uncertainty thus described, he submitted without further preface to the Royal Society, and especially to those who study the etiology of disease, a description of the mode of procedure followed in this inquiry, and of the results to which it had led.

A number of chambers or cases were constructed, each with a glass front, its top, bottom, back, and sides being of wood. At the back is a little door, which opens and closes on hinges, while into the sides are inserted two panes of glass, facing each other. The top is perforated in the middle by a hole two inches in diameter, closed air-tight by a sheet of India-rubber. This sheet is pierced in the middle by a pin, and through the pin-hole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar, two inches in diameter, and one inch and a half high, surrounds the pipette, the space between both being packed with cotton-wool moistened by glycerine. Thus, the pipette, in moving up and down, is not only firmly clasped by the India-rubber, but it also passes through a stuffing-box of sticky cotton-wool. The width of the aperture closed by the India-rubber secures the free lateral play of the lower end of the pipette. Into two other smaller apertures in the top of the case are inserted, air-tight, the open ends of two narrow tubes, intended to connect the interior space with the atmosphere. The tubes are bent several times up and down, so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air.

The bottom of the box is pierced sometimes with a single row, sometimes with two rows of holes, in which are fixed, air-tight, large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

On the 10th of September, the first case of this description was closed. The passage of a concentrated beam across it through its two side windows then showed the air within it to be laden with floating matter. On the 13th, it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days of quiet sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air.

During ebullition, aqueous vapour rose from the liquid into the chamber, where it was for the most part condensed, the uncondensed portion escaping, at a low temperature, through the bent tubes at the top. Before the brine was removed, little stoppers of cotton-wool were inserted in the bent tubes, lest the entrance of the air into the cooling chamber should at first be forcible enough to carry motes along with it. As soon, however, as the ambient temperature was assumed by the air within the case, the cotton-wool stoppers were removed.

We have here the oxygen, nitrogen, carbonic acid, ammonia, aqueous vapour, and all the other gaseous matters which mingle more or less with the air of a great city. We have them, moreover, "untortured" by calcination, and unchanged even by filtration or manipulation of any kind. The question now before us is, Can air thus retaining all its gaseous mixtures, but self-cleansed from mechanically suspended matter, produce putrefaction? To this question, both the animal and vegetable worlds return a decided negative. Among vegetables, experiments have been with hay, turnips, tea, coffee, hops, repeated in various ways with both acid and alkaline infusions. Among animal substances are to be mentioned many experiments with urine; while beef, mutton, hare, rabbit, kidney, liver, fowl, pheasant, growse, haddock, sole, salmon, cod, turbot, mullet, herring, whiting, eel, oyster, have been all subjected to experiment.

The result is, that infusions of these substances exposed to the common air of the Royal Institution laboratory, maintained at a temperature of from 60 deg. to 70 deg. Fahr., all fell into putrefaction in the course of from two to four days. No matter where the infusions were placed, they were infallibly smitten in the end. The number of the tubes containing the infusions was multiplied till it reached six hundred, but not one of them escaped infection.

In no single instance, on the other hand, did the air which had been proved moteless by the searching beam show itself to possess the least power of producing bacterial life or the associated phenomena of putrefaction. The power of developing such life in atmospheric air and the power of scattering light are thus proved to be indissolubly united.

The sole condition necessary to cause these long dormant infusions to swarm with active life is the access of the floating matter of the air. After they have remained for four months as pellucid as distilled water, the opening of the back door of the protecting case, and the consequent admission of the mote-laden air, suffice in three days to render the infusion putrid and full of life.

That such life arises from mechanically suspended particles is thus reduced to ocular demonstration. Let us inquire a little more closely into the character of the particles which produce the life. Pour eau de Cologne into water; a white precipitate renders the liquid milky. Or, imitating Brücke, dissolve clean gum-mastic in alcohol, and drop it into water; the mastic is precipitated and milkiness produced. If the solution be very strong, the mastic separates in curds; but, by gradually diluting the alcoholic solution, we finally reach a point where the milkiness disappears, the liquid assuming by reflected light a bright cerulean hue. It is, in point of fact, the colour of the sky, and is due to a similar cause; namely, the scattering of light by particles, small in comparison to the size of the waves of light.

When this liquid is examined by the highest microscopic power, it seems as uniform as distilled water. The mastic particles, though innumerable, entirely elude the microscope. At right angles to a luminous beam passing among the particles, they discharge perfectly polarised light. The optical deportment of the floating matter of the air proves it to be composed in part of particles of this excessively minute character. When the track of a parallel beam in dusty air is looked at horizontally through a Nicol's prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a considerable portion of the light from the finer matter is extinguished. The coarser motes, on the other hand, flash out with greater force, because of the increased darkness of the space around them. It is among the finest ultra-microscopic particles that, the author shews, the matter potential as regards the development of bacterial life is to be sought.

But, though they are beyond the reach of the microscope, the existence of these particles, foreign to the atmosphere but floatin as certain as if they could be felt between the fingers, or seen by the naked eye. Suppose them to augment in magnitude until they come, not only within range of the microscope, but within range of the unaided senses. Let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand filled with nutritious earth, with which we mix our unknown particles; and that in forty-eight hours subsequently buds, and blades of well defined cresses and grasses, appear above the soil. Suppose the experiment, when repeated over and over again, to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the products of dead dust, of mineral particles; or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly consider the experiment with the flower-pot as clearing up our pre-existing ignorance; we should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been "spontaneously generated".

This reasoning applies word for word to the development of bacteria from that floating matter which the electric beam reveals in the air, and in the absence of which no bacterial life has been generated. There seems no flaw in this reasoning; and it is so simple as to render it unlikely that the notion of bacterial life developed from dead dust can ever gain currency among the members of a great scientific profession.

A novel mode of experiment has been here pursued, and it may be urged that the conditions laid down by other investigators in this field, which have led to different results, have not been strictly adhered to. To secure accuracy in relation to these differences, the latest words of a writer on this question, who has materially influenced medical thought both in this country and in America, are quoted. "We know", he says, "that boiled turnip or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with bacteria and vibriones in the course of from two to six days". Who the "we" are who possess the knowledge is not stated. The author is certainly not among the number, though he has sought anxiously for knowledge of the kind. He thus tests the statements in succession.

And, first, with regard to filtered air. A group of twelve large test-tubes were passed air-tight through a slab of wood coated with cement, in which, while hot, a heated "propagating glass", resembling a large bell-jar, was imbedded. The air within the jar was pumped out several times, air filtered through a plug of cotton-wool being permitted to supply its place. The test-tubes contained infusions of hay, turnip, beef, and mutton, three of each, twelve in all. They are as clear and cloudless at the present moment as they were upon the day of their introduction; while twelve similar tubes, prepared at the same time, in precisely the same way, and exposed to ordinary air, are clogged with mycelium, mould, and bacteria.

With regard to the calcined air, a similar propagating glass was caused to cover twelve other tubes filled with the same infusion. The "glass" was exhausted and carefully filled with air, which had passed through a red-hot platinum-tube, containing a roll of red-hot platinum gauze. Tested by the searching beam, the calcined air was found quite free from floating matter. Not a speck has invaded the limpidity of the infusions exposed to it, while the contents of twelve similar tubes, placed outside, have fallen into rottenness.

The experiments with calcined air took another form. Six years ago, it was found that, to render the laboratory air free from floating matter, it was only necessary to permit a platinum-wire heated to whiteness to act upon it for a sufficient time. Shades containing pear-juice, damson-juice, hay- and turnip-juice, and water of yeast, were freed from their floating matter in this way. The infusions were subsequently boiled, and permitted to remain in contact with the calcined air. They are quite clear to the present hour; while the same infusions, exposed to common air, became mouldy and rotten long ago.

It has been affirmed by other workers on this question, that turnip and hay infusions, rendered slightly alkaline, are particularly prone to exhibit the phenomena of spontaneous generation. This was not found in the present investigation to be the case. Many such infusions have been prepared, and they have continued for months without sensible alteration.

Finally, with regard to infusions wholly withdrawn from air, a group of test-tubes containing different infusions was boiled under a bell-jar filled with filtered air, and from which subsequently the air was removed as far as possible by a good air-pump. They are now as pellucid as they were at the time of their preparation more than two months ago, while the contents of a group of corresponding tubes exposed to the laboratory air have all fallen into rottenness.

There is another form of experiment on which great weight has been laid; that of hermetically sealed tubes. On the 6th of last April, a discussion on the "Germ-Theory of Disease" was opened before the Pathological Society of London. The meeting was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The following important summary of these was given by Dr. Bastian. "With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blow-pipe-flame; we may boil the fluid, seal the vessel during ebullition, and, keeping it in a warm place, may await the result, as I have often done. After a variable time, the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with bacteria and allied organisms."

Previously to reading this statement, the author had operated upon sixteen tubes of hay and turnip infusions, and upon twenty-one tubes of beef, mackerel, eel, oyster, oatmeal, malt, and potato, hermetically sealed while boiling, not by the blow-pipe, but by the far more handy spirit-lamp-flame. In no case was any appearance whatever of bacteria or allied organisms observed. The perusal of the discussion just referred to caused the author to turn again to muscle, liver, and kidney, with the view of varying and multiplying the evidence. Fowl, pheasant, snipe, partridge, plover, wild-duck, beef, mutton, heart, tongue, lungs, brains, sweetbread, tripe, the crystalline lens, vitreous humour, herring, haddock, mullet, cod-fish, sole, were all embraced in the experiments. There was neither mistake nor ambiguity about the result. One hundred and thirty-nine of the flasks operated on were exhibited, and not one of this cloud of witnesses offered the least countenance to the assertion that the liquid within flasks boiled and hermetically sealed swarm subsequently more or less plentifully with bacteria and allied organisms.

The evidence furnished by this mass of experiments that Dr. Bastian must have permitted errors either of preparation or observation to invade his work is, it is submitted, very strong. But to err is human; and, in an inquiry so difficult and fraught with such momentous issues, it is not error, but the persistence in error for dialectic ends by any of us, that is to be deprecated. The author shows by illustrations the risks of error run by himself. On October 21st, he opened the back-door of a case containing six test-tubes filled with an infusion of turnip, which had remained perfectly clear for three weeks, while three days sufficed to crowd six similar tubes exposed to mote-laden air with bacteria. With a small pipette, he took specimens from the pellucid tubes, and placed them under the microscope. One of them yielded a field of bacterial life monstrous in its copiousness. For a long time he tried vainly to detect any source of error, and was prepared to abandon the unvarying inference from all the other experiments, and to accept the result as a clear exception to what had previously appeared to be a general law. The cause of his perplexity was, however, finally traced to the tiniest speck of an infusion containing bacteria which had clung by capillary attraction to the point of one of his pipettes.

Again, three tubes containing infusion of turnip, hay, and mutton, were boiled on November 2nd under a bell-jar containing air so carefully filtered that the most searching examination by a concentrated beam failed to reveal a particle of floating matter. At the present time, every one of these tubes is thick with mycelium, and covered with mould. Here, surely, we have a case of spontaneous generation. Let us look to its history.

After the air has been expelled from a boiling liquid, it is difficult to continue the ebullition without "bumping". The liquid remains still for intervals, and then rises with sudden energy. It did so in the case now under consideration; and one of the tubes boiled over, the liquid overspreading the resinous surface in which the bell-jar was embedded. For three weeks the infusions had remained perfectly clear. At the end of this time, with a view of renewing the air of the bell-jar, it was exhausted, and refilled by fresh air which had passed through a plug of cotton-wool. As the air entered, attention was attracted by two small spots of penicillium resting on the liquid which had boiled over. It was at once remarked that the experiment was a dangerous one, as the entering air would probably detach some of the spores of the penicillium, and diffuse them in the bell-jar. This was, therefore, filled very slowly, so as to render the disturbance a minimum. Next day, however, a tuft of mycelium was observed at the bottom of one of the three tubes; namely, that containing the hay-infusion. It has by this time grown so as to fill a large portion of the tube. For nearly a month longer, the two tubes containing the turnip and mutton infusions maintained their transparency unimpaired. Late in December, the mutton-infusion, which was in dangerous proximity to the outer mould, showed a tuft upon its surface. The beef-infusion continued bright and clear for nearly a fortnight longer. The recent cold weather caused me to add a third gas-stove to the two which had previously warmed the room in which the experiments are conducted. The warmth of this stove played upon one side of the bell-jar, causing currents; and, on the day after the lighting of the stove, the beef-infusion gave birth to a tuft of mycelium. In this case, the small spots of penicillium might have readily escaped attention; and, had they done so, we should have had here three cases of "spontaneous generation" far more striking than many that have been adduced.

In further illustration of the dangers incurred in this field of inquiry, the excellent paper of Dr. Roberts on Biogenesis, in the *Philosophical Transactions* for 1874, is referred to. Dr. Roberts fills the bulb of an ordinary pipette up to about two-thirds of its capacity with the infusion to be examined. In the neck of the pipette, he places a plug of dry cotton-wool. He then hermetically seals the neck, and dips the bulb into boiling water or hot oil, where he permits it to remain the requisite time. Here we have no disturbance from ebullition, and no loss by evaporation. The bulb is removed from the hot water, and permitted to cool. The sealed end of the neck is then filed off, the cotton-wool alone interposing between the infusion and the atmosphere.

The arrangement is beautiful, but it has one weak point. Cotton-wool free from germs is not to be found, and the plug employed by Dr. Roberts infallibly contained them. In the gentle movement of the air to and fro as the temperature changed, or in any shock, jar, or motion to which the pipette might be subjected, we have certainly a cause sufficient to detach a germ now and then from the cotton-wool, which would fall into the infusion and produce its effect. Probably, also, condensation occurred at times in the neck of the pipette; the water of condensation carrying back from the cotton-wool the seeds of life. The fact of fertilisation being so rare as Dr. Roberts found it to be is a proof of the care with which his experiments were conducted. But he did find cases of fertilisation after prolonged exposure to the boiling temperature; and this caused him to come to the conclusion that, under certain rare conditions, spontaneous generation may occur. He also found that an alkalised hay-infusion was so difficult to sterilise that it was capable of withstanding the boiling temperature for hours without losing its power of generating life. The most careful experiments have been made with this infusion. Dr. Roberts is certainly correct in assigning to it superior nutritive power. But, in the present inquiry, five minutes' boiling sufficed to completely sterilise the liquid.

Summing up this portion of his inquiry, the author remarks that he will hardly be charged with any desire to limit the power and potency of matter. But, holding the notions he does, it is all the more incumbent on him to affirm that, as far as inquiry has hitherto penetrated, life has never been proved to appear independently of antecedent life.

Though the author had no reason to doubt the general diffusion of germs in the atmosphere, he thought it desirable to place the point beyond question. At Down, Mr. Darwin and Mr. Francis Darwin; at High Elms, Sir John Lubbock; at Sherwood, near Tunbridge Wells, Mr. Siemens; at Pembroke Lodge, Richmond Park, Mr. Rollo Russell; at Heathfield Park, Miss Hamilton; at Greenwich Hospital, Mr. Hirst; at Kew, Dr. Hooker; and at the Crystal Palace, Mr. Price, kindly took charge of infusions, everyone of which became charged with organisms. But to obtain more definite insight regarding the diffusion of atmospheric germs, a square wooden tray was penetrated with a hundred holes, into each of which was dropped a short test-tube. On October 23rd, thirty of these tubes were filled with an infusion of hay, thirty-five with an infusion of turnips, and thirty-five with an infusion of beef. The tubes, with their infusions, had been previously boiled, ten at a time, in an oil-bath. One hundred circles were marked on paper, so as to form a map of the tray, and every day the state of each tube was registered upon the corresponding circle. In the following description, the term "cloudy" is used to denote the first stage of turbidity, distinct but not strong. The term "muddy" is used to denote thick turbidity.

One tube of the hundred was first singled out and rendered muddy. It belonged to the beef group, and it was a whole day in advance of all the other tubes. The progress of putrefaction was first registered on the 26th of October. The map then taken may be thus described.

Hay.—Of the thirty specimens exposed, one had become "muddy" —the seventh in the middle row reckoning from the side of the tray nearest the stove. Six tubes remained perfectly clear between this muddy one and the stove, proving that differences of warmth may be overridden by other causes. Every one of the other tubes containing the hay-infusion showed spots of mould upon the clear liquid.

Turnip.—Four of the thirty-five tubes were very muddy, two of them being in the row next the stove, one four rows distant, and the remaining one seven rows away. Besides these, six tubes had become "clouded". There was no mould on any of the tubes.

Beef.—One tube of the thirty-five was quite muddy, in the seventh row from the stove. There were three cloudy tubes, while seven of them bore spots of mould.

As a general rule, organic infusions exposed to the air during the autumn remained for two days or more perfectly clear. Doubtless, from the first, germs fell into them, but they required time to be hatched. This period of clearness may be called the "period of latency", and, indeed, it exactly corresponds with what is understood by this term in medicine. Towards the end of the period of latency, the fall into a state of disease is comparatively sudden; the infusion passing from perfect clearness to cloudiness more or less dense in a few hours.

Thus the tube placed in Mr. Darwin's possession was clear at 8.30 A.M. on the 19th of October, and cloudy at 4.30 P.M. Seven hours, moreover, after the first record of our tray of tubes, a marked change had occurred. It may be thus described. Instead of one, eight of the tubes containing hay-infusion had fallen into uniform muddiness. Twenty of these had produced bacterial slime, which had fallen to the bottom, every tube containing the slime being covered by mould. Three tubes only remained clear, but with mould upon their surfaces. The muddy turnip-tubes had increased from four to ten; seven tubes were clouded, while eighteen of them remained clear, with here and there a speck of mould on the surface. Of the beef, six were cloudy, and one thickly muddy, while spots of mould had formed on the majority of the remaining tubes. Fifteen hours subsequent to this observation, viz., on the morning of the 27th of October, all the tubes containing hay-infusion were smitten, though in different degrees, some of them being much more turbid than others. Of the turnip-tubes, three only remained unsmitten, and two of these had mould upon their surfaces. Only one of the thirty-five beef-infusions remained intact. A change of occupancy, moreover, had occurred in the tube which first gave way. Its muddiness remained grey for a day and a half, then it changed to bright yellow green, and it maintained this colour to the end. On the 27th, every tube of the hundred was smitten, the majority with uniform turbidity; some, however, with mould above and slime below, the intermediate liquid being tolerably clear. The whole process bore a striking resemblance to the propagation of a plague among a population, the attacks being successive and of different degrees of virulence.

From the irregular manner in which the tubes are attacked, we may infer that, as regards quantity, the distribution of the germs in the air is not uniform. The singling out, moreover, of one tube of the hundred by the particular bacteria that develope a green pigment shows that, as regards quality, the distribution is not uniform. The same absence of uniformity was manifested in the struggle for existence between the bacteria and the penicillium. In some tubes, the former were triumphant; in other tubes of the same infusion, the latter were triumphant. It would seem also as if a want of uniformity as regards vital vigour prevailed. With the selfsame infusion, the motions of the bacteria in some tubes were exceedingly languid; while in other tubes the motions resembled a rain of projectiles, being so rapid and violent as to be followed with difficulty by the eye. Reflecting on the whole of this, the author concludes that the germs float through the atmosphere in groups or clouds, with spaces more sparsely filled between them. The touching of a nutritive fluid by a bacterial cloud would naturally have a different effect from the touching of it by the interspace between two clouds. But, as in the case of a mottled sky, the various portions of the landscape are successively visited by shade, so, in the long run, are the various tubes of our tray touched by the bacterial clouds, the final fertilisation or infection of them all being the consequence. The author connects these views with the experiments of Pasteur on the non-continuity of the cause of so-called spontaneous generation, and with other experiments of his own.*

The tray of tubes proved so helpful in enabling him to realise mentally the distribution of germs in the air, that on the 9th of November he exposed a second tray containing one hundred tubes filled with an infusion of mutton. On the morning of the 11th, six of the ten nearest the stove had given way to putrefaction. Three of the row most distant from the stove had yielded, while here and there over the tray particular tubes were singled out and smitten by the infection. Of the whole tray of one hundred tubes, twenty-seven were either muddy or

* In hospital practice, the opening of a wound during the passage of a bacterial cloud would have an effect different from the opening of it in the interspace between two clouds. Certain caprices in the behaviour of wounds may possibly be accounted for in this way.

cloudy on the 11th. Thus, doubtless, in a contagious atmosphere, are individuals successively struck down. On the 12th, all the tubes had given way; but the differences in their contents were extraordinary. All of them contained bacteria, some few, others in swarms. In some tubes they were slow and sickly in their motions, in some apparently dead, while in others they darted about with rampant vigour. These differences are to be referred to differences in the germinal matter, for the same infusion was presented everywhere to the air. Here also we have a picture of what occurs during an epidemic, the difference in number and energy of the bacterial swarms resembling the varying intensity of the disease. It becomes obvious from these experiments that of two individuals of the same population, exposed to a contagious atmosphere, the one may be severely, the other lightly attacked, though the two individuals may be as identical as regards susceptibility, as two samples of one and the same mutton infusion.

The author traces still further the parallelism of these actions with the progress of infectious disease. The *Times* of January 17th contained a remarkable letter on Typhoid Fever, signed "M.D.", in which occurs the following statement: "In one part of it (Edinburgh), congregated together and inhabited by the lowest of the population, there are, according to the Corporation return for 1874, no less than 14,319 houses or dwellings—many under one roof, on the 'flat' system—in which there are no house connections whatever with the street sewers, and, consequently no water-closets. To this day, therefore, all the excrementitious and other refuse of the inhabitants is collected in pails or pans, and remains in their midst, generally in a partitioned off corner of the living-room, until the next day, when it is taken down to the streets and emptied into Corporation-carts. Drunken and vicious though the population be, herded together like sheep, and with the filth collected and kept for twenty-four hours in their very midst, it is a remarkable fact that typhoid fever and diphtheria are simply unknown in these wretched hovels."

This case has its analogue in the following experiment, which is representative of a class. On November 30th, a quantity of animal refuse, embracing beef, fish, rabbit, hare, was placed in two large test-tubes opening into a protecting chamber containing six tubes. On December 13th, when the refuse was in a state of noisome putrefaction, infusions of whiting, turnip, beef and mutton, were placed in the other four tubes. They were boiled and abandoned to the action of the foul "sewer gas" emitted by their two putrid companions. On Christmas Day, these infusions were limpid. The end of the pipette was then dipped into one of the putrid tubes, and a quantity of matter comparable in smallness to the pock-lymph held on the point of a lancet, was transferred to the turnip. Its clearness was not sensibly affected at the time; but, on the 26th, it was turbid throughout. On the 27th, a speck from the infected turnip was transferred to the whiting; on the 28th, disease had taken entire possession of the whiting. To the present hour, the beef and mutton tubes remain as limpid as distilled water. Just as in the case of living men and women in Edinburgh, no amount of fetid gas had the power of propagating the plague as long as the organisms which constitute the true contagium did not gain access to the infusions.

The universal prevalence of the germinal matter of bacteria in water has been demonstrated with the utmost evidence by the experiments of Dr. Burdon Sanderson. But the germs in water are in a very different condition, as regards readiness for development, from those in air. In water they are thoroughly wetted, and ready, under the proper conditions, to pass rapidly into the finished organism. In air they are more or less desiccated, and require a period of preparation more or less long to bring them up to the starting-point of the water-germs. The rapidity of development in an infusion infected by either a speck of liquid containing bacteria or a drop of water is extraordinary. On January 4th, a thread of glass almost as fine as a hair was dipped into a cloudy turnip-infusion, and the tip only of the glass fibre was introduced into a large test-tube containing an infusion of red mullet; twelve hours subsequently, the perfectly pellucid liquid was cloudy throughout, and full of life. A second test-tube containing the same infusion was infected with a single drop of distilled water furnished by Messrs. Hopkin and Williams; twelve hours also sufficed to cloud the infusion thus treated. Precisely the same experiments were made with herring with the same result. At this season of the year several days' exposure to the air are needed to produce the same effect. On December 31st, a strong turnip-infusion was prepared by digesting in distilled water at a temperarure of 120 degs. Fahr. The infusion was divided between four large test-tubes, in one of which it was left unboiled, in another boiled for five minutes, and in the two remaining ones boiled, and, after cooling, infected with one drop of beef-infusion containing bacteria. In twenty-four hours, the unboiled tube and the two infected ones were cloudy; the unboiled tube being the most turbid of the three. The infusion here was peculiarly limpid after digestion; for turnip it was quite exceptional, and no amount of searching with the microscope could reveal in it at first the trace of a living bacterium; still germs were there which, suitably nourished, passed in a single day into bacterial swarms without number. Five days have not sufficed to produce an effect approximately equal to this in the boiled tube, which was uninfected but exposed to the common laboratory air.

There cannot, moreover, be a doubt that the germs in the air differ widely among themselves as regards *preparedness* for development. Some are fresh, others old; some are dry, others moist. Infected by such germs, the same infusion would require different lengths of time to develop bacterial life. This remark applies to and explains the different degrees of rapidity with which epidemic disease acts upon different people. In some, the hatching period, if it may be called such, is long, in some short, the differences depending upon the different degree of preparedness of the contagium.

The author refers with particular satisfaction to the untiring patience, the admirable experimental skill, the veracity in thought, word and deed, displayed throughout the inquiry by his assistant Mr. John Cottrell, who was zealously aided by his junior colleague Mr. Frank Valter.

OBSTETRIC MEMORANDA.

PERCHLORIDE OF IRON IN UTERINE HÆMORRHAGE.

UNDER the above heading, Dr. Joseph Pratt, in the JOURNAL of January 22nd, gives the particulars of a case of *post partum* hæmorrhage treated by the introduction into the uterus of a sponge saturated with tincture of perchloride of iron. Dr. Pratt states that he is not aware of anyone having used the tincture of iron in the same way. In vol. xi of the *Obstetrical Transactions*, and again in vol. xv and xvi, he will find that I practise a similar mode of treatment, and have done so for years. Had my communication which I sent to the JOURNAL during the late discussion been inserted, the members would have been rendered conversant with my "ready method" of arresting *post partum* hæmorrhage.

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EMPHYSEMA DURING LABOUR.

E. N., AGED 26, a healthy and well developed primipara, was, on January 11th, 1876, in the third stage of labour, which was protracted in consequence of the face of the child presenting towards the pubes of the mother. After some severe expulsion pains, unassociated with any advancement of the fœtus, and, about one hour before delivery, she complained that she could not see with her right eye. It was at once found that this eye was closed by an enlargement and puffiness of the lids and surrounding skin, which presented the usual characteristic, crackling on pressure, of so-called surgical emphysema. Shortly afterwards, it was noticed that this swelling and crackling extended around and about the neck, chest, abdomen, and back, as far down as on a level with the umbilicus, disfiguring and distorting the features and contour of the upper half of the body. Delivery was expedited by the application of the forceps, and the patient has made a recovery apparently undisturbed by this complication. On January 17th, the emphysema had disappeared everywhere except just about the upper part of the sternum. There has been since delivery no elevation of temperature, nor increase of circulation or respiration.

In seeking for a cause for this emphysema, I was first led (owing to a statement of the patient; viz., that she felt something "snap" in front of her throat, pointing to the crico-thyroid membrane) to think of the possible rupture of this membrane during a severe forcible expiratory effort; but further reflection, and the emphysema remaining about the top of the sternum as long or longer than anywhere else, has led me to suggest that the rupture of some marginal air-cells into the anterior mediastinum would be sufficient to permit a point of leakage, through which, owing to the extreme violence of the checked expirations, sufficient air escaped into the subcutaneous tissues to cause the symptoms described. I may add, however, that there are no evidences of coarse emphysema in our patient.

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