nothing abnormal in the vessels or grey matter; the peripheral layer of white matter was scarcely tinted; there was overgrowth of connective tissue and atrophy of nerve-fibres. Below this, the grey cornua had lost their symmetry; in one section, the right posterior cornu was atrophied, while in several others, again, below this, the left anterior horn was shortened, and a portion of the central grey mass was encroached upon by a tract of lighter coloured tissue distinguishable to the naked eye, passing obliquely backwards. The central canal was replaced by proliferated nuclei, and appeared as if pushed towards the left. The vessels in its vicinity were irregular and shrunken, the perivascular spaces around them being dilated. The ganglion-cells were normal, while the white columns presented the same appearances above noted. In the upper cervical region, the sheath of pia mater was fibroid; the vessels were somewhat thickened; those running into the anterior fissure were surrounded by loose connective tissue and widely dilated and filled with blood-corpuscles; the central canal was obliterated; the vessels around it were thickened and shrunken, appearing in the grey cornua like fibroid nodules. The ganglion-cells of the anterior cornua were but little affected; the nervous strands of the posterior cornua were indistinct and atrophied. In the dorsal region, the same general changes were observed: the ganglion-cells of the anterior cornua were here more affected, being in some sections diminished in number, rounded, and finely granular, though deeply and evenly coloured by carmine. The nervous reticulum of the posterior cornua was indistinct and atrophied. Similar changes prevailed in the lumbar and sacral regions, the ganglion-cells being here relatively less affected than in the dorsal portion of the cord. The central canal throughout the entire column was replaced by proliferated nuclei, which formed a large ovoid mass transversely, and were scattered, sometimes widely so, in the grey commissure. The atrophic and degenerative changes in the white columns was equally diffused in the different regions, and sections of the spinal nerves running in the sheath surrounding the cord presented the same coarse and altered appearance.

Summary.—Moderate fibrosis of vessels, overgrowth of connective tissue, atrophy of nerve-fibres, partial degeneration of ganglion-cells of the anterior cornua, and of nervous reticulum of the posterior cornua, with obliteration of central canal and dilatation (especially in the cervical region) of perivascular spaces.

[To be continued.]

THE CHEMICAL THEORY OF CONTAGIUM COMPARED WITH THE CORPUSCULAR THEORY, WITH SPECIAL REFERENCE TO THE ACTION OF DISINFECTANTS.*

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THE subject which I have the honour to bring before you is one full of deep interest, involving as it does some of the most subtle and controverted questions of our day; and while I claim for it an importance second to none in the long roll of subjects connected with sanitary science, I beg your indulgence for a short time, while I bring some points perhaps hitherto untouched to your notice; and, with your assistance, I hope I shall be enabled to throw some light—however faint the glimmer may be—on our present knowledge of this most abstruse subject.

We shall regard *contagia* as substances which must come into actual contact with the organism, if they are to affect it. By *infection* will be understood the power of being propagated through the inhalation of air tainted by the perspiration or breath of the affected person; and by the term *disinfectant* is meant a destructive agent, that is, a substance which will prevent infectious diseases from spreading by destroying their specific poisons. I propose, then, firstly, to give a short *résumé* of the views held in relation to the spore-theory of disease; and, secondly, to see how far these theories bear close investigation, and the conclusions drawn from them.

Dr. Beale, in the year 1870, promulgated his views of the intimate nature of contagia, and submitted that the actual properties in vaccine lymph lay in transparent particles of extreme minuteness, which he termed bioplasm, born in and only growing in the body. These particles are living, but do not assume any special form; they are not microzymes or any vegetable life. "I am justified," he says, "in advancing the doctrine that the germs originate in man's organism, and

that they are descended from the normal bioplasm of the body. They consist of a peculiar kind of living matter, the smallest particles of which, when supplied with proper pabulum, will grow and multiply. They can, in fact, be only identified by their effects. The contagious virus is, then, living and growing matter, the particles not being directly descended but having resulted from the multiplication of particles from without, which are capable of growing and multiplying in the blood, while it is not improbable that a few of the contagious disease-germs actually acquire their virulent properties after they have left the organism in which they have become developed, and while they remain immersed in media containing the proper elements of their nutrition and further development. They are extremely minute, and can exist under many different conditions. In connection with this latter statement, it is worthy of remark that Dr. Beale asserts that, when once living matter has actually become ice, its life is for ever destroyed and it is incapable of being revived or revitalised. Now, lymph was frozen by means of solid carbonic acid and submitted to a temperature of 116 deg. below zero of Fahrenheit, and after this intense cold it was found to be unaffected. Under what conditions, then, can it retain its vitality?

The theory of Hallier, utilising the laws of alternate generation and of digenesis in the reproduction of certain cryptogamic vegetations, is an endeavour to establish the dictum that "all virulent agents are nothing more than an allotropic condition of various mucedines or moulds, which only arrive at their complete development without the body, though in it they multiply in the form of exceedingly fine granules designated micrococci". Every virulent malady has been, according to this idea, set down as depending upon the presence of a fungus, each epidemic owing its origin to a different fungus. His theory is plainly dependent on metamorphosis; otherwise it would not stand argument, and until further investigation throws more light upon the fungoid nature of contagium, I think we may fairly dismiss it without discussion. With reference to this, I may say in passing, that in no subject is there more diversity of opinion than exists among mycologists; and those are not few who deny all alternation of generation among fungi, on which alone Hallier's theory is founded.

Closely allied to Hallier's is the bacterioid origin of disease; and whether we regard these minute forms as mere particles of protoplasm, or as a series of steps in the autogenesis of a specific form; or agree with Cohn, who is inclined, from their resemblance to *alga crenothrix*, or class them with oscillatorians; or with Lister, who believes that he has demonstrated the origin of bacteria from a fungus, a species of *dematiu*; their presence is sufficient to urge us to persevere in further investigations, and to endeavour to learn something definite of the character and growth of these minute organisms. A modification of this theory is, that bacteria naturally harmless in themselves derive poisonous properties by feeding on morbid food, and thus become the carriers of specific contagium. The history of these minute forms has been studied with much care, and the observations of Pasteur are worthy of notice in passing. He is of opinion that some of the infusorial ferments, among which are the *bacteridia*, develop only in oxygen; while another class, namely, *vibrios*, undergo their transformations when the gas is absent. Different changes are brought about by different germs; the butyric acid fermentation is due to a vibrio, the alcoholic to a *torula*, etc.

The results of the most recent experiments on this subject that I am aware of are those recorded by Messrs. Downes and Blunt, on "The Influence of Light on the Development of Bacteria", that, in fact, it is inimical to their production, and that under favourable conditions it may prevent their development; while under less favourable conditions it may not prevent, but only retard. They further state that those conditions which tend to neutralise the action of light are the same which are known to favour processes of fermentation and putrefaction.

Passing on to more recent investigations, we gain fresh information from that distinguished veterinarian of the Lyons School, Dr. Chauveau, whose researches have been confirmed by Dr. Burdon Sanderson, and with extreme care have again been submitted to trial by Drs. Braidwood and Vacher. Dr. Chauveau tells us—and a study of his experiments will well repay perusal—that so far as vaccine lymph is concerned, the contagium consists of minute particles, suspended in the contagium liquid; in short, that it is the particles and not the soluble constituents of vaccine on which its activity depends. By dilution of the lymph, Dr. Chauveau was enabled to separate by subsidence minute particles called leucocytes, and he ascertained that the serum without them was just as virulent as when loaded with them. Thus he eliminated the presence of leucocytes as being the active germs, and after most painstaking investigations, he demonstrated that the elementary granules of the pure serum never subsided and passed

* Read in the Section of Public Health at the Annual Meeting of the British Medical Association in Manchester, August 1877.

through every filter; the separation of plasma from particles could only, therefore, be effected by diffusion. I need not follow the successive steps of his investigations, which are fully recorded elsewhere; and shall only quote his conclusions, which are briefly as follows.

I. The vaccinal serosity is not virulent, and the activity resides in the solid granule in all or only in part in these elementary organisms.

II. In diluted vaccine, the number of abortive punctures was always greater the weaker the solution; a result which he attributed to the particles of vaccine being separated, and only now and then finding their way to the point of the instrument used for inoculating. Messrs. Braidwood and Vacher tell us that we possess the strongest indirect proof that the contagium of the virus of vaccine consists of transparent particles, neither soluble in water nor in watery liquid, and not capable, without losing their properties, of assuming the form of vapours; that the density of these particles was somewhat greater than the liquid surrounding them, as indicated by their tendency to subside. Chauveau further ascertained that, in sheep-pox, water maintained in contact with the virus acquired no infective properties, which was directly the reverse with the water mixed with the virus, thus proving the particulate nature of the contagium.

In what manner are these particles produced? Do they spring from like bodies, or are they generated *de novo*? Are they animal or vegetable substances, or are they of the nature of ferments? How do they comport themselves under varying conditions. Such are the questions which perplex men of the most profound thought at the present time.

Again, we have the physical theory, of which Dr. Richardson is the exponent. He explains the non-vital nature of contagium by asserting that certain organic animal poisons exert their deleterious action by setting at liberty the oxygen of the blood in undue quantity, being themselves decomposed in the process.

In his paper on "The Experimental Study of Disinfectants," Dr. Baxter has given us much valuable information; still he appears to me to start his argument with premisses of a very questionable nature, when he tells us that the infective principle is neither soluble nor diffusible, and therefore neither can it be volatile. Assuming this, it follows, he states, that all distinction between contagion and infection must cease. The one is communicable by direct contact, the other at a distance, and may be transferred as a solid, as a liquid, or a gas. Again, we find Dr. Baxter experimenting on the virus of infective inflammation; but, may I ask, what proof have we that the lymph of infective inflammation is of the nature of germs? Is it not much more reasonable to suppose that it is simply fibrinous exudation, which after coagulation has acquired poisonous properties, and the physical and chemical characters ascribed to it by Dr. Baxter an after-change, not indeed by Dr. Baxter's own account of a very marked character, for "few or no rod-like bacteria were observed, and none of the organisms exhibited movements of translation".

Time will not permit me to refer to the interesting experiments of Tyndall or Bastian, the former holding the germ-theory, the latter what may be termed the chemical theory; that is, that as a result of fermentative change, gases are given off, and with these other soluble products mix imperceptibly with the changing and quickening mother liquid, in all parts of which certain insoluble products also make their appearance and reveal themselves to us as specks of protoplasm, that is, of living matter, and speedily assume the well known forms of one or other variety of bacteria. "These insoluble particles," says Dr. Bastian, "serve to bridge the gulf between dead and living matter, and thereby afford a long-sought-for illustration from chemical to so-called vital combinations."

It has been, I think, assumed by far the greater number of pathologists, and by Dr. Baxter in particular, that specific contagia, so far from being of an extremely subtle and delicate nature, really consist in solid particles, which in vaccine lymph, according to Dr. Burdon Sanderson, are the twenty-thousandth of an inch in diameter. But, may I ask, what proof have we that they cannot exist in any other form, or that the material of these very particles did not circulate in the blood of the living body in a dissolved state, and, as such, were capable of being thrown off by transpiration with other products? Assuming this, what becomes of Dr. Baxter's corollary that the contagium is not volatile? And to admit the possibility of this would to some extent account for the communicability of small-pox before the development of the eruption, which ought to be the fruiting time if, as many suppose, the particles in question be the minute spores of a specific fungus; and upon this theory the etiology of many diseases is explained with more than ordinary positiveness. The unstable character of the blood never seems to have been considered (and this is not alone confined to the blood, for so all organic liquids, when placed under a similar condition of rest),

nor the tendency such substances evince to separate into a coagulum or thicker portion and a thinner stratum. Some late investigations by Dr. Creighton are important with reference to this subject. The BRITISH MEDICAL JOURNAL alludes to them thus: "During the last year or two, a great deal has been heard in England about a discovery by Dr. Klein of certain vegetable organisms which constitute the actual contagious element in sheep-pox and typhoid fever, these diseases being supposed to be caused by the introduction of specific spores into the system of a healthy individual, the course of the disease corresponding with their development. This pathological doctrine has sustained a sudden and fatal shock in consequence of certain investigations by Dr. Creighton, which are described in the *Proceedings of the Royal Society*, vol. xxv, No. 172. Dr. Creighton has found that, when fresh tissues are put in solutions of chromic acid or alcohol, mucous and other albuminoid fluids coagulate in forms which simulate the appearance of the organisms which Dr. Klein believed he had discovered. Dr. Creighton remarks that, 'although Dr. Klein considered that he had before him in these preparations the various conditions of a fungus, to which he gave a specific name, and although he professed to find the various conditions of spore, mycelium, and fructification, occurring in their natural sequence, to correspond with the regular advance of the pathological process, there is no doubt that the circumstantial account rests on erroneous observation and on defective evidence, and that the appearances found in the skin of a sheep are none other than those resulting from the coagulation of albuminoid fluids under particular circumstances'."

To the theory of what may be termed solid particles one does not object; nay, there is much, very much, to commend such views, borne out as they are by experimental facts; but why limit contagium so far? Why can it exist in no other form? and why, in the writings quoted, should there be a tacit disavowal of all other forms in which it may exist? Carefully prepared diffusion has failed to rob those particles of their morbid power; and the same may be said of the altered fibrine of infective inflammation.

The fact is, we have still to find out what contagium is or really becomes in its active state. We know how difficult it is to dissolve coagulated albumen or fibrine; and is not diffusion in a test-tube very different from diffusion in the blood? At least, we have to deal with one of two things in discussing the germ or spore theory of disease; viz., A, the organisms themselves, including, of course, their developmental history; and B, the morbid products of their nutritive processes. The putrefactive odour is not due to the decomposition of animal or vegetable matter, as Cohn has proved, but is evolved by the active development of bacterium term on a solution of tartrate of ammonia, without a particle of organic matter. The organisms are, therefore, at least in the case just given, the sole producers of the principles yielding that odour. Now, although we may be unable to trace any specific form of disease to this source, such principles may be regarded as equivalent to those products of other organisms which are presumed to be the producers of specific disease. When once evolved, therefore, the morbid energy is resident in B, the products, rather than in A, the organisms themselves; and this may be absorbed though colloidal when in an extreme state of dilution, and develop diseases. And it is even possible to conceive that an agent capable of arresting the reproductive power of A may at the same time exert little influence on B, and *vice versa*.

Numerous experiments have shown that a certain relationship exists between the subjugation of bacteria and the correction of the offensive odours surrounding them. Thus chloralum destroyed bacteria in a beef-infusion, while the smell was but little affected. Condry's fluid checked the odour in a great part, while the bacteria preserved their activity. On the other hand, both were simultaneously subdued by chloride of lime.

The fact appears to be, that we attribute too much to the coincident appearance or occurrence of microscopic forms with certain diseases, and to ignore purely chemical causes, though involving organic matter. Take, for example, a poison-wound: if the bad consequences depended upon the introduction of such germs into the living body, one would say that more grave results would happen after sufficient time had elapsed to favour their further development; but this is notably not the fact, for we know that the virulence of the fluids is in proportion to their freshness, and therefore to their freedom from bacteria. It would be absurd to refer the host of organic poisons with which we are acquainted to germs of any kind. The moistened fang, too, of a rabid dog will carry death to many whom he attacks in his wanderings, yet we can detect no sensible alteration in the saliva to account for this. I need hardly allude to the experiment of Mr. Crooks, by which he obtained wonderful power over the cattle-plague by simply diffusing carbolic acid through the air, in one farm the disease altogether disap-

pearing. He tells us in his report "that sound animals have been kept within eight feet of sick beasts without taking the plague, when the air was kept constantly charged with carbolic acid". Again, in whatever condition such a chemically active substance may be introduced into our bodies, whether as a solid, a liquid, or a gas, one of two things must happen. If it be of the nature of spores, ordinary reproduction, such as the formation of mycelium and subsequent fruiting, is the consequence; or they must operate dissolved in our bodies. Now the former is exceedingly unlikely, in that they differ from true fungi in the absence of all tendency to bud or branch, their only known mode of multiplication being by transverse cleavage; and the question occurs, how far the change resulting from the latter may not be catalytic—resolving the fluids and solids it encounters into its own likeness or nature without being changed itself. The action of disinfectants on schizomycetes is generally admitted to afford an index of the disinfecting power of the agent; and, assuming that carbolic acid, for instance, has no effect on the solid particles, yet such may not be the case with gaseous forms. Of course, only in proportion to the strength of the vapour liberated is it of use. Contagium, liberated by transpiration and diffused in the atmosphere, may possibly be corrected by carbolic acid. We have our argument on the effect the acid has on solid particles in a liquid medium; but we have no experimental proof of any results of this on the gaseous form.

But enough. Time will not permit me to enlarge on this subject; and, in conclusion, I would most earnestly impress on all not to be led away with results which to many appear so conclusive, but to persist in their endeavours to rid the atmosphere of infective places of their chemical poisons, as well as the spore or germ—the offending particle, whatever it may be—regarding both as dangerous and injurious; calling in first Nature's great means, namely, diffusion, dilution, and oxidation; and supplementing these by aerial disinfectants, the action of which, although imperfect, is not to be condemned in the wholesale manner we have been accustomed of late to hear.

CLINICAL MEMORANDA.

NOTE OF A CASE OF PULMONARY PHTHISIS WITH LARGE VOMICA, IN WHICH THE CAVITY HAS BEEN TAPPED WITH GOOD RESULT.

IN this case, of which the interest is apparent, and in which the results thus far have been remarkably good, the history is not yet complete; but the importance of the case seems to justify a preliminary note of its clinical features.

The patient was a man aged 28, who had suffered from consumptive disease of both lungs for several years. During the last six months, a large cavity had formed in the base of the left lung, as well as some smaller ones in the upper lobe. The cough was most harassing, and often followed by vomiting; the expectoration profuse (about two pints a day) and exceedingly offensive, causing loss of appetite and diarrhoea not only to the patient, but to his nurse and attendants; and a high afternoon temperature was maintained for several months. Microscopical examination showed also that lung-disintegration was steadily proceeding. The diagnosis was a large and superficial basic cavity in a contracted and fibrotic lung, with pleura adherent in front. On June 19th, at my request, Mr. Erichsen, assisted by Mr. Marcus Beck, introduced a large-sized trocar and cannula between the seventh and eighth ribs, and successfully tapped the cavity. The operation was attended by considerable difficulties on account of the toughness of the fibrotic lung, and was followed by cutaneous emphysema and limited pneumothorax, the former subsiding in a few days. The relief was immediate. About two pints of foetid pus were evacuated, the expectoration was reduced to two or three ounces a day, all fevor ceased, and both pulse and temperature fell; and, though nearly a month has elapsed since the operation, the progress has been in every way satisfactory. The discharge is slight, and the cavity has been several times washed out with disinfecting fluids.

C. THEODORE WILLIAMS, M.D., F.R.C.P.

THERAPEUTIC MEMORANDA.

SALICYLIC SNUFF IN HAY-FEVER.

IN a case of hay-fever in which I have tried everything in previous years, I have this year found immense relief follow the use of snuff composed of pure salicylic acid. This was suggested to me by the fact that relief is often afforded by the topical application of solution of quinine; and, indeed, in this case I first combined quinine with the

salicylic acid; but the good result is now maintained much more agreeably by the salicylic acid alone, of which the patient snuffs about ten or fifteen grains daily. He speaks most highly of the magical effects of this "white powder". As this observation extends to only one case, it would be wrong to draw conclusions from it; but I am sure the suggestion will be acceptable to those who are baffled by this very obstinate complaint.

W. J. H. WOOD, Boston.

WARBURG'S TINCTURE.

IN a case of puerperal hyperpyrexia, recently under my care, the employment of Warburg's tincture, as advocated in the *British Medical Journal* by Dr. Playfair, and again, last month, by Dr. Burnley Walker, seemed to be followed by the happiest results. The patient was a healthy primipara, and the labour was normal; towards the close of the second stage, five grains of ergotine were injected into the gluteus maximus muscle. The day following delivery, no complaint was made by the patient, but on the second day the temperature was 104, and during the following eight days the case gave the greatest anxiety. There was no evidence of peritoneal inflammation; neither milk nor lochia were suppressed, and, but for occasional sharp pain in the pelvis and discomfort from the pyrexia, she made no complaint. For eight days, the temperature continued to range from 104 deg. to 105 deg., with the exception of one day, when a reduction to 102 deg. was effected by the use of large doses of quinine. On the morning of the eleventh day, the temperature was 104 deg. and the pulse 106. At 5 P.M., half an ounce of Warburg's tincture was given on an empty stomach, with the result that three hours afterwards the temperature was lower than I had ever found it—namely, 101.2 deg. and the pulse 100. On the following morning, however, matters were no better. I was disheartened to find the temperature up to 104 deg. and the pulse 120. As, however, relief had been given on the previous day by the use of the tincture, I resolved to try it again. I ordered a saline purgative, and after this had acted, about 6 P.M., I again gave half an ounce of the tincture. The following morning, all was changed for the better, the temperature was 98.4 deg. and the pulse 80, and she expressed herself to be greatly relieved. Her recovery was complete and uninterrupted.

ALEX. R. COLDSTREAM, M.B., L.R.C.S.ED., Leith.

TOXICOLOGICAL MEMORANDA.

POISONING BY RED PRECIPITATE.

ON the evening of June 22nd, a man came to the Cumberland Infirmary and asked me to see his wife, who lived close by, stating that she had accidentally swallowed some poison. On my arrival, the friends informed me that she had, about four hours previously, taken some beer out of a cup containing a quantity of red precipitate (the remainder of the powder in a paper was shown to me). The exact quantity she had swallowed I could not ascertain; but it appeared to have been a considerable amount.

The patient, a young married woman aged 20, was lying upon the bed in a state of stupor, with a weak, irregular, scarcely perceptible pulse; pupils dilated, but sensible to light; the surface of the body very cold and clammy to the touch; and a copious discharge running from the mouth. She had vomited just before I arrived, and I noticed particles of the red precipitate amongst the vomited matter. Her bowels were very loose, and she had great pain across the abdomen. Having washed out the stomach thoroughly with tepid water, I injected the white of eggs with some flour and water into the stomach. This was partly vomited, but sufficient was retained. After waiting a short time, her collapsed condition continuing, I injected brandy and strong coffee into the stomach. This had the desired effect of rallying her considerably; and I then left her, ordering brandy to be administered in small doses at frequent intervals.

I saw her again in two hours, when I found her pulse stronger and more regular, the surface of the body warmer, and altogether her condition improving. She now complained loudly of cramps in the lower limbs and severe pain in the abdomen. On inquiry, I found the bowels had been moved several times during my absence. I applied mustard plasters to the calves of both legs, which greatly relieved the cramps; and subsequently gave her a full dose of castor-oil with tincture of opium in it.

The following morning, the patient was better, although suffering much from pain in the epigastrium, with a constant desire to vomit. She was much salivated; and the diarrhoea continued, although the bowels were moved less frequently than at first. I prescribed a mix-