centres? As evidence of this volitional effect, I have heard since that the girl, after the recovery of motor power, was on one occasion in the hospital-kitchen, and declared to the cook that she was about to vomit; and, on being informed by the cook that she could not do so there, she so far controlled herself that she made no further allusion to the matter and did not vomit.

This case illustrates the fact that sometimes the chameleon-like symptoms of hysteria will simulate and may be mistaken for diseases of serious import, such as disease of the hip or other joints; and it may be that the very influence on her mind on being told this contributed to increase the symptoms, while the subsequent assurance that nothing of this kind was wrong with her aided in the restoration of the arrested powers. The facts as to the actual effects of the metallic applications in this case are so far very meagre; the rapid recovery from the anæsthesia, and the permanence of this up to the present time, removing the opportunity of making further trials and experiments. Should there, however, be a recurrence of the symptoms, I shall pursue the matter, as the patient is likely to continue or again come under observation.

P.S.—October 21st, 1879, I have seen the girl to-day. She has had no recurrence of the hemianæsthesia since, nor any apparent loss of motor power; she, however, complains that the electric pains in the legs sometimes suddenly seize her, and that for short intervals the left foot becomes numb, she looks well but is stated to be nervous.

DISINFECTION BY ACIDS.

By JOHN DOUGALL, M.D., F.F.P.S. GLASGOW,

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[Concluded from page 728 of last number.]

ACIDS have an action on putrescible fluids which seems to me of much value in a sanitary sense. I have already referred to this in my paper on the Science of Disinfection,* and the numerous experiments by which it was discovered are detailed in my paper on Putrefiers and Antiseptics.† A few remarks on this point may not be out of place. If a clear, fresh, putrescible fluid be set aside for a short time, and then examined by the microscope, it will be found swarming with bacteria and their allies in active motion, fetid, and opaque. Its reaction is neutral; but these changes would ensue though it were made moderately alkaline or *very faintly* acid. If it be now exposed in a cool dark place for from nine to twelve months, and then examined, it will still be found swarming with life, cloudy, fetid, and containing albumen. In a few months later, putrefactive reduction is completed. The solution may be still cloudy, but is now almost free from fætor; a bacterium is only seen here and there; a faint trace of albumen may or may not be detected; and its specific gravity, at first perhaps about 1.6, is now probably about 1.1 or 1.2. Such are the chief physical phenomena of putrefaction. As regards its chemical aspects, the chief elements in animal matter are oxygen, hydrogen, carbon, nitrogen, phosphorus, and sulphur. The hydrogen unites with the oxygen to form water; with the nitrogen to form ammonia; with the sulphur, phosphorus, and carbon, to form sulphuretted, phosphoretted, and carburetted hydrogen gases. The oxygen also unites with the carbon to form carbonic anhydride. In course of time, the ammonia is oxidised into nitrous and nitric acids and water; the sulphuretted hydrogen into sulphurous and sulphuric acids and water; the phosphoretted hydrogen into phosphoric acid and water. These free acids then at once combine with any bases present to form their respective salts. Putrefaction is thus seen to be a process of reduction; and the greater the number of equivalents of elements forming the atoms of a compound, the more easily does such a compound decompose. This instability is increased in animal substances by their containing nitrogen, which has less tenacity in its affinities than any of the elements. The large quantity of water present also forms a most favourable medium for putrefaction, which only takes place at death, as then the chemical forces by which the living tissues act on vital organic compounds cease, and the elements of the original compounds unite to form bodies less complex, but more stable, as described above.

If to another portion of the same clear, fresh, putrescible fluid, a quantity of acid, mineral or vegetable, be added, so as to cause marked acidity, and the mixture be then placed under the same conditions as the former, then in a day or two it shows quite a new set of changes. One or more small tufts of mycelia are present, their silky filaments radiating from a tiny nucleus; also numerous free spores and double torulaceous cells. If the proper quantity of acid have been added, no bacteria are

present, and the fluid, instead of becoming hazy, becomes clearer and sparkling. Its reaction is of course distinctly acid, its odour somewhat mouldy, but not at all unpleasant; while, in about four months, it ceases to respond to albumen tests, indicating that decomposition is completed. This is fermentation. Again, if to a portion of a putrefying fluid a slight excess of acid be added, putrefaction is soon arrested, and the mixture, instead of being then fetid, has an odour like that of oil of spirea. And if to separate portions of a fresh and of a fermenting albuminous fluid, a larger quantity of acid be added than is sufficient to cause fermentation alone, then the fresh fluid neither putrefies nor ferments; fermentation is arrested in the fermenting fluid; and both mixtures remain in this antiseptic state for an indefinite time, and latterly cease to respond to albumen tests.

Now, these results seem to me strong proofs that putrefaction and fermentation, whatever be the chemical changes they produce on solutions of animal fluids, are distinct processes. There is abundant evidence that putrefying matter is very injurious to health, but there is little or none to show the effects of fermenting matter in this respect. It is obvious, however, that fermentation must be harmless as compared with putrescence. Putrid matter evolving noxious effluvia for nearly twelve months* must be more hurtful than the same matter when fermenting, it being always almost odourless and fully decomposed in about four months. Moreover, as already observed, acids added in excess both prevent and arrest putrefaction and fermentation; and this not by antiseptic but by destruction, as is shown by the animal solution ultimately ceasing to respond to the tests for albumen, without the physical signs of either putrefaction or fermentation having been present.

Convinced of the truth of these facts and deductions, I have now for a number of years exclusively practised them in every form of infectious disease, for which there has been ample opportunity both in my capacity as medical officer of health and in private practice. I have watched the effects with scrupulous care and an unprejudiced desire to learn whether, and if so how far, theory and laboratory experiments were confirmed by practice. My experience and observations have further convinced me that disinfection by acids is cheap, quick, easy, and certain: and I have frequently been struck with the summary manner in which an infectious disease has been thus arrested, under circumstances most favourable for its propagation.

About eight months ago, I attended a girl, aged 6, suffering from scarlatina. The family—besides the father, mother, and a servant girl—consisted of eight children, four older and three younger than my patient, none of whom had the disease. The house contained four small rooms and kitchen. The girl was kept in a room by herself; none of the family were allowed to see her; but during her illness, which lasted about a month, they remained in the house. The mother nursed the sick girl, at the same time mixing freely with the other children, four of whom had to stay from school, thus greatly increasing their risk of being infected; but as yet none of them have taken the disease.

About five months ago, I attended a male, aged about 18, son of a sanitary inspector. I found him very prostrate, and covered with a livid scarlatinal rash. Next day, he was greatly worse, and the case looked hopeless. The house consisted of two rooms and kitchen, and contained, besides the father and mother, four children, excluding my patient, whose ages ranged from six to sixteen, none of whom had the disease. Two days afterwards, a brother of the patient turned ill and was fevered. It was assumed that he also was sickening with scarlatina, and hence was put in a bed in the same room with the patient; but recovering in a couple of days, he was removed to a friend's house. Briefly, my patient, after a rather protracted illness, with profuse desquamation of the cuticle, quite recovered. The other children lived in the house, and as usual were kept from school. The mother nursed the patient, mixing freely with the rest of the family; and yet, up to the present, none of them have taken the disease.

About two years ago, I attended a girl, aged 12, ill of typhoid fever. The house consisted of four rooms and kitchen. Besides the father, mother, two servant girls, a sick nurse, and my patient, there were five young children staying in the house. The patient's bowels were very loose for about three weeks. She ultimately recovered; the children were kept at home during her illness; none in the house previously had, nor subsequently took, the disease.

About four years ago, I was hurriedly called to see a boy, about six years old. I found him in a state of collapse, and covered with a petechial scarlatinal rash; he died next day. The house consisted of

* Transactions of the Social Science Congress. Glasgow: 1874.

† Glasgow Medical Journal, November 1872, and February 1873.

* I have a bottleful of beef-juice prepared about five years ago, which is still quite putrid: its contents have, however, been kept from air and light. From this can be inferred the time which animal solids may lie festering in a choked or improperly flushed drain.

two rooms and kitchen; besides the father, mother, and my patient, other four children stayed in the house, none of whom were sent away, nor had the disease previously, nor were affected soon afterwards. About twelve months subsequently, however, the family still residing in the same house, and the mother having had another child, I was called to see a boy of the same family, aged about five years. He was covered with a bright scarlatinal rash, and ultimately had slight albuminuria during copious desquamation, but made a fair recovery; the domestic arrangements were the same as with the first case. None of the rest of the family were affected.

I could relate a great number of similar cases, but that is unnecessary, as it seems to me obvious, from the highly favourable conditions, surrounding those detailed, for the spread of infectious disease; that its arrestment must be greatly, if not solely, attributed to the means of disinfection employed. I do not, however, wish it to be understood that I have been so successful in all my cases of infectious disease, the subtlety, attenuation, and extremely obscure modes of operation of zymotic poisons being such as frequently to baffle most, if not all, attempts to modify or prevent their action. Again, it is next to impossible to get some people to bother themselves in carrying out systematic instructions as to disinfection; they do it either spasmodically or imperfectly. In these latter cases, I usually find the results much more unfavourable than where the instructions are faithfully attended to. On the other hand, so far as I can remember, every focus of infection under my care, the spread of which had been so suddenly arrested under conditions most favourable for its transmission as to impress it on the memory, was disinfected entirely to, and sometimes, I thought, beyond, my satisfaction. Moreover, every year of increased experience confirms me in these views.

For disinfecting, I advise the use of three acids—hydrochloric, sulphurous, and aromatic glacial acetic. The hydrochloric consists of one of the strong acid to twenty of water, and is used chiefly to disinfect typhoid excreta, and the bed and body clothes of persons with infectious disease. In typhoid cases, a large cupful of this fluid is put in the vessel before it is used. After being emptied down the water-closet, it is rinsed with another cupful, which is also at once put down the closet, the water running while this is being done. In using this fluid for clothes, these only require to be sprinkled with it, so as to sensibly damp them, allowing them to remain so for about four hours; but of course saturation for about an hour is preferable. With thick woollen clothes, such as blankets, however, an inconveniently large quantity of the acid fluid is needed for saturation. It is worth remembering, in such cases, that wool resists the action of acids far more than cotton or linen, and as this material is more likely, from its porous nature, to retain infection than the latter, it should be subjected longer to the action of the acid. Boiling water is afterwards poured over the acidified clothes, which are allowed to stand for an hour. They are then thoroughly rinsed with warm water till all the acid is got rid of, when soap with soda or potash may be used to wash them. If the acid be not removed, the soap will not form a lather. The solution of this acid may also be used for sprinkling on floors, pouring down sinks, etc.

The sulphurous acid is used exclusively for aerial disinfection, which is done morning and night during the patient's illness, not only in the patient's apartment, but in every room in the house. The process is simple; a piece of live coal is placed on a shovel and powdered sulphur sprinkled on it. The sulphurous fumes thus generated should be made to impregnate the entire atmosphere of the house, until it becomes somewhat irritable. If more fumes have been generated than can be tolerated, the excess is easily got rid of by opening the doors and windows, and by allowing what may be still forming to ascend the chimney. Fifteen minutes is sufficient to allow the acid to act each time it is used; but if the apartment be empty, it may be used some hours. Mattresses and carpets can be disinfected with sulphur fumes by suspending them spread out by the four corners with ropes, and allowing the acid to float thickly along the lower surface for ten minutes, then turning them upside down, and repeating the process. Bright metallic surfaces are darkened by sulphurous acid.

The aromatic glacial acetic acid, though more than a hundred times the price of the others, gives great satisfaction in the using, its well known grateful cooling odour forming a pleasant contrast to the repulsive emanations from the body of an infectious patient. It consists, besides the acid, of the oils of rosemary, neroli, cinnamon, cloves, bergamot, lavender, and rectified spirit. I frequently use this acid as an occasional aerial disinfectant, but never to the exclusion of sulphurous fumes. It is directed to be dropped on a hot shovel and volatilised through the house daily, which is the more effective way; or, where its price is a consideration, a little may be poured occasionally into a small vessel of hot water, placed over the gas, in the patient's room, by which means it becomes more gradually dissipated, producing at the same time

a constant pleasant odour in the apartment. I have had much satisfaction also in using it dilute—one drachm to one ounce of water—for sponging the body, by which two objects are attained—lowering of the temperature and disinfection of the skin. Almost without exception, patients like this process, and are usually much refreshed by it. In all scarlatinal cases, more especially during desquamation, I invariably direct this to be done twice daily. The practice, so much in vogue, of smearing the skin of a scarlatinal patient during desquamation with lard or warm suet, to prevent the morbid epithelial *débris* from scattering, seems to me highly calculated to injure the sufferer. By this means the mouths of the sebaceous and sudoriparous glands, the very channels through which, in all probability, the poison finds egress from the blood, are plastered up; so that, besides the unpleasantness it causes, it is an effectual way of sealing the infective matter in the body. It looks precisely analogous to hermetically closing every door, window, crack, and crevice in a room where a person is lying soaked with infection. On the other hand, by sponging the skin with the aromatic acid, the infectious scurf is permanently deprived of its dangerous properties, while, at the same time, the numerous orifices of the gland-ducts are cleared of their morbid secretions, thus giving free exit to the poison from the blood. Moreover, it is well known, and has been proved experimentally, that the evaporation and oxidation of most perfume-yielding oils, including those in aromatic acetic acid, generate ozone—a powerful disinfectant. Here, then, is another strong recommendation for sponging the body with the substance in question, as by this means an atmosphere of ozone may actually be made to evolve gradually and constantly from the patient's skin, floating under the bedclothes and enveloping every part of the body.

It has been urged, as a fatal objection to the use of acids in disinfecting enteric excreta, unsupported, however, by any experimental or practical data, that they corrode and destroy the metallic fittings of the water-closet. That this objection, applied to hydrochloric acid, is, for all practical purposes, quite groundless, I have proven hundreds of times; hence it seems to me entirely superfluous to give examples to confute it. One, however, may be cited. A few years since, during an epidemic of typhoid, I attended a young man ill, and who at last died, of the disease. His bowels were very loose, moving more or less about ten times in twenty-four hours for nearly three weeks. During this period about three-quarters of a gallon of strong hydrochloric acid, diluted in the proportion of one of acid to twenty of water, had been used to disinfect his excretions, all of which was put down the water-closet without affecting the fittings in the least perceptible degree.

In order, however, to obtain some exact results on this point, the following experiments were made with the metals used in closet-fittings. Pieces of brass, of copper, of lead, and of cast-iron, each measuring two inches in superficies, were weighed and immersed in two ounces of a liquid consisting of one part of hydrochloric acid in nineteen of water, for twenty-five hours. After this, they were washed in pure water and again weighed, when the weight of the brass, copper, and lead, was found unaltered, while the iron had lost thirteen grains, or 4.33 per cent. of its weight.

I submit that these results are a sufficient answer to those who aver that closet-fittings are injured by at least hydrochloric acid, the substance I prefer, as it is by far the cheapest of the mineral acids. In putting the acid fluid down the closet, the first metal it touches is the copper pan, containing usually several ounces of water, by which it is at once more diluted; then, when the handle is raised, there is a rush of water, causing a further great dilution; and the fluid is next carried into a cast-iron basin, thence through a leaden trap, into an iron or glazed sewer-pipe. The lever and other movable apparatus of the closet are of iron and brass; and they, being situated externally to the pan and basin, the acid fluid does not touch them at all. Now, as iron was the only metal affected of the four used in the experiment, hence it is the only one likely to be affected in the closet; but the iron pan is about a quarter of an inch thick and the soil-pipe about one-eighth, while the acid fluid as it passes over the iron basin is diluted to at least 1 in 100, and when it goes down the iron soil-pipe to at least 1 in 500; moreover, the time of contact is but a few seconds. Hence it is obvious, considering that the iron in the experiment was in contact with the strong acid fluid for twenty-five hours, that the action of this fluid on the iron of a water-closet must be extremely little. Even supposing the strong acid fluid were to act *per se* on the iron of a closet, say for ten seconds ten times daily during three weeks, as might be required in a bad case of typhoid, and as was in the case I mentioned; yet, on the data of the experiment with the iron, it would take nine hundred days, or about two years and a half, at this rate to dissolve six-and-a-half grains off each square inch of surface of the iron.

In conclusion, it may be stated, as a summary of the foregoing, that putrefaction may be prevented, neutralised, or arrested, or its odour

masked or neutralised, and yet any infective matter present, be, in all probability, unaffected, preserved, or only made dormant for a brief period, as exhaustive experiments with carbolic acid and infective fluids, more especially with vaccine, show; that to use purely antiseptic substances as antizymotics is highly paradoxical, preservation being practised and destruction anticipated; further, that the mineral acids are true disinfectants, operating by both physical and chemical destruction, and also by transformation, of organic molecules, as in fermentation; hence, they prevent and arrest putrefaction, they transform putrefaction into fermentation; they deodorise; and, moreover, they are highly antizymotic as regards vaccine, virus of glanders, virus of infective inflammation, and therefore, by inference, as regards all other infections.

ENLARGEMENT OF THE BRONCHIAL LYMPHATIC GLANDS WITH RELATION TO WHOOPING-COUGH.

By W. B. CHEADLE, M.D., F.R.C.P.,

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THE theory that compression or irritation of the vagus nerve by enlarged bronchial glands is the cause of the spasmodic cough in whooping-cough, advocated by Dr. Gueneau de Mussy and combated by Dr. Barlow, is no new one. Some quarter of a century ago, Dr. Ley, having observed the frequent association of laryngismus stridulus with swelling of the glands of the neck and chest, and the entanglement of the pneumogastric nerve or its branches amongst them, attributed the temporary closure of the glottis in this affection to pressure made by these enlarged glands upon the recurrent laryngeal nerve, or some part of the eighth pair. And again, later, finding a like condition of these structures in whooping-cough, he suggested that the enlargement of these glands from a specific poison, similar to that of the parotid gland in mumps, might after all be the essence of whooping-cough. These views were brought forward and favoured by Sir Thomas Watson in his well known lectures. The revival of the theory by Dr. Gueneau de Mussy gives it fresh prominence and importance.

Now, I think, most pathologists would agree upon two primary propositions connected with the question, viz.:

1. That the spasmodic cough and laryngeal spasm of whooping-cough are due to some morbid influence acting upon the vagus;
2. That enlarged bronchial glands are constantly found on *post mortem* examination in all fatal cases of the disease.

The single instance of well marked whooping-cough in which a *post mortem* examination shall show this last condition to be absent, required by Dr. Gueneau de Mussy to refute his theory, will, I imagine, be sought in vain.

The absence of such evidence against the hypothesis does not, however, establish its correctness.

The evidence of physical examination during life supports that obtained *post mortem*, and I believe that, in whooping-cough, enlarged bronchio-tracheal glands are always present. But it by no means follows that the two conditions stand in the relation of cause and effect. They are rather, I think, both effects of a common cause.

The difficulty in the way of what may be called the compression theory is precisely that put forward by Dr. Barlow and Dr. Archambault, and combated so ingeniously by Dr. Gueneau de Mussy; viz., the fact that a similar enlargement of the bronchio-tracheal glands is commonly met with in other pulmonary affections besides whooping-cough. And, even further, I think it may be stated as a general pathological law that, *whenever there is prolonged inflammation or irritation of the bronchial mucous membrane in children, enlargement of the bronchial glands certainly follows.* We see it in the *post mortem* room as the accompaniment of bronchopneumonia, of phthisis, of measles, of whooping-cough. Swelling of the bronchial glands, indeed, results from an inflamed bronchial mucous membrane, just as certainly as swelling of the cervical glands follows an inflamed scalp. This enlargement, then, being present in many disorders other than whooping-cough, without any spasmodic cough (as Dr. Gueneau de Mussy fully allows), if the tumefaction of the glands be the cause of the spasmodic cough in one case and not in another, it must be due to some special conditions present in the one instance and not in the other. It is in this respect that Dr. Barlow's case is so valuable. It clears the ground by showing positively that mere mechanical pressure is not the determining cause.

Dr. Gueneau de Mussy suggests that the determining conditions may be some qualification of pressure; as that the compression acts on certain points of the nervous cluster, or that it acts in certain special

degree, or that it produces a certain kind of stimulus which may depend upon the nature of the morbid process. It is difficult to see how such variations of pressure could produce the effect, and still more difficult to conceive how the nature of the morbid process could modify the influence of pressure by an enlarged gland; but, granting that such variations might afford a reasonable explanation of the phenomenon, they rest so far on pure hypothesis. Under ordinary circumstances, the act of coughing is excited by stimulus applied to the peripheral twigs of the pneumogastric, either those of the superior laryngeal in the larynx, or the pulmonary branches to the bronchi; or, in some cases, possibly by irritation of the gastric portion in the stomach; or again, the stimulus may be reflected, as, for instance, in cough caused by the act of laughing. Cough is set up in the same way in whooping-cough, by the stimulus of accumulating mucus in the larynx, trachea, or bronchi, or a blast of cold air, or a full meal, or a fit of laughter. It seems more reasonable to suppose that the peculiar spasmodic character of the cough is due to the special action of a special virus upon the terminal fibres, or to an irritable condition established by its influence on them or in the nerve-centres, rather than to the rough mechanical influence of gland-pressure. In the parallel case of the pressure of an aneurism or other tumour upon the recurrent nerve, we have spasmodic cough indeed, but not until the pressure has become extreme enough to produce paralysis of the vocal cord and persistent hoarseness and stridor of respiration. Then accumulating mucus in the narrowed glottis *does* produce violent cough. But in whooping-cough, there is no such evidence of persistent pressure. The only way in which it seems to me probable that enlarged bronchial glands can play any active part in the matter is, that the turgescence of the vessels and tissues of the neck, which results from the successive expiratory efforts, may so temporarily increase the pressure upon the inferior laryngeal, as to produce a momentary spasm of the glottis, and thus cause the characteristic whoop. But then closure of the glottis is part of the act of coughing, and the whoop would be accounted for by the spasm set up by special or extreme irritation being more persistent than that of ordinary cough. The occurrence of cough simulating whooping-cough is explicable in the same way, by excessive irritation and irritability of the nerve-fibres or centres involved. The recurrence of spasmodic cough, too, with each successive catarrh, months after the original attack of whooping-cough has ceased, is as easily explained by a persistent irritability of nerve-structures, as by a persistent tumefaction of bronchial glands. I agree with Dr. Gueneau de Mussy that these relapses of whooping-cough are not contagious; I have long been in the habit of allowing patients thus affected to mix freely with others, both in private practice and in hospital, and have never yet seen the disease communicated thereby.

A CASE OF PLEUROPNEUMONIA WITH EFFUSION TREATED BY THE ERGOT OF RYE: RAPID RECOVERY.

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UNTIL now, the ergot of rye has been employed principally as an ocytotic, sometimes as a hæmostatic, and in certain affections of the spine. It has recently been employed as a febrifuge in typhoid fever, and as a local application in ophthalmia; but, having read of its utility in pneumonia, I determined to try it in the following case.

On May 20th last, I was called to see a young married woman aged 28, suffering from symptoms of some pulmonary affection. On examination, I found she had all the signs of the commencement of pneumonia of the right lung, the left being unaffected. She had been ailing for about eleven days, and, thinking she had only a "cold", continued her occupations as usual. On the 19th, however, finding herself prostrated with intense fever, she was obliged to lay up, and sent for me the next day. She complained of great pain in the lower and back part of the chest, accompanied with slight oppression and a continual cough; and as she was in a state of great agitation, with a full soft pulse and dry skin, I gave her a mixture composed of bromide of potassium, liquor ammoniæ acetatis, digitalis, and morphia, in some syrup and water, of which she took a tablespoonful every two hours. This had the effect of quieting the patient, but the other symptoms had not much abated. At my visit on the morning of the 25th, I noticed that the sputa were mixed with blood, and signs of the supervention of pleurisy of the same side became evident, accompanied with considerable effusion of fluid. I then prescribed a combination of the liquid extract of ergot of Bonjean with liquor ammoniæ acetatis, which she was to take every two hours. On the morning of the 26th, the patient was more oppressed, the effusion considerably increased, extend-