

## METHODS OF DISINFECTION.\*

## PART II.

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INTERCALARY.—I. SURFACES *v.* CUBIC CONTENT.—In the first part of this paper I gave reasons for discontinuing the use of sulphur fumigation as a method of practical disinfection. I also indicated some physical considerations which appeared to me important. The chief one I should like to emphasize is that the application of a disinfectant should be reckoned not primarily by cubic space, but by superficial area. The assumption at the basis of all our practice is that room-walls are retainers and possible distributors of infection. The rest of the room is air, and foul air can be changed for fresh. It follows that surfaces alone are to be considered. This needs emphasizing, for the long habit of regarding disinfection as fumigation has dominated all our ways of writing about the disinfection of rooms. If we could safely blow infected dust from floors, walls, and ceilings, we should not need to discuss the sterilizing of the dust retained. But this is precisely what constitutes the particular problem.

II. THE STANDARD OF DISINFECTION FOR NON-ISOLATED PATHOGENIC GERMS.—In the matter of tests, we are to be guided as much by practice in hospital as by practice in the laboratory. Few of the pathogenic organisms are exceptionally difficult to destroy; but the question arises, May we safely assume that what kills the more feebly resisting will also kill those as yet unisolated—of scarlet fever, for instance, or measles? Is there any good ground for supposing that any infection of the non-isolated group is more resistant than the bacillus of tubercle, or the spores of anthrax? Or must we, to be safe, use nothing less potent than that which will destroy *B. subtilis* and its spores? Practice is able to furnish at least a partial answer. Take the following diseases—scarlet fever, typhus fever, measles, and small-pox.

1. Such coccal organisms as have been found associated with scarlet fever have proved less resistant than *B. subtilis*. It is certain that many of the well-known disinfectants have been found sufficient to extirpate scarlet fever infection. It is possible to render a scarlet fever case, and *à fortiori* scarlet fever clothing, incapable

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of communicating fresh infection. If it be said that the infection loses in potency towards the end of the specific history of a case, the reply is—Does it? Experience of a “tailed” series of cases—one catching fire as the other goes out—will soon make discussion of that point superfluous. What kills tubercle may be safely held to kill scarlet fever.

2. The infection of typhus fever, again (even if we accept, provisionally, Drs. Porter and Barbour's diplococcus), is one of the most easily destroyed of any. If it is one of the most virulent infections, it is also one of the most vulnerable. Free exposure to the air robs the infection of its potency. Free exposure to air, however, is not possible where clothing is chronically filthy. I have found the ordinary processes—spraying with 2 per cent. formalin, steeping in izal ( $\frac{1}{2}$  per cent.), in corrosive sublimate solution, etc.—sufficient. There is nothing to indicate any exceptional resistance in the specific organism of this disease. On the contrary, everything points to its feebleness outside the conditions special to its nurture.

3. With measles it is not different. The organism seems to die a rapid natural death whenever the conditions for its spread are removed.

4. Small-pox receives, naturally, very detailed attention. It is difficult to estimate how much of its decrease is due to vaccination and how much to destruction of virus. But I have not had any experience to indicate that the small-pox virus is peculiarly resistant to the action of the ordinary disinfectants. It is true I never gave it much of a chance. In disinfecting a small, overcrowded, two-roomed house, I used 10 per cent. formalin; but that was not because I had any scientific ground for believing that 2 per cent. would not have been sufficient, probably it would have been; but with extreme infections, like those of small-pox and typhus fever, one wishes to err on the safest side.

I do not wish to “whittle away” the seriousness of the non-isolated organisms; but it is legitimate to infer that, if the diseases are destroyed by the known methods of disinfection, their specific organisms are as easy to kill as those already known to us. The standards applied to the known organisms are also applicable to the known diseases due to organisms that are unknown.

To resume now the “choice of a disinfectant”:

(d) *Corrosive Sublimate*.—This is the disinfectant used by the Municipal Disinfection Service of Paris for the sprayers. So far as I can gather, no toxic results have been reported; but when I adopted the spray for house-disinfection, I hesitated to run any

risk. My hesitation has been justified by at least one episode. Recently, for the disinfection of some small-pox wards, I gave the disinfectors some powders of corrosive sublimate; each powder contained 70 grains of the sublimate and 15 grains of ammonium chloride; one powder making, with 1 gallon of water, a solution of 1 in 1,000 (corrosive sublimate). The men found the spray extremely irritating; it induced vomiting in both; it affected the bronchi in one, who continued to cough for a night, and was occasionally affected to vomiting. The symptoms passed off completely in less than twenty-four hours. There was no sign of salivation afterwards. Possibly the presence of ammonium chloride may have had something to do with the toxic symptoms; but one experiment of the kind was enough. The men both preferred the formalin solution to the corrosive. I have seen similar symptoms when the same wards were *washed* with 1 in 1,000 solution of corrosive sublimate. Apparently, the amount absorbed by inhalation was enough to induce squeamishness, and in one or two cases persistent vomiting. These facts would not be enough to justify the non-use of corrosive sublimate for spraying; but they certainly ought to induce great care in its use, and, since other less noxious disinfectants are available, I do not feel urged to the use of corrosive sublimate, even with its admittedly high disinfective potential.

(e) *Chloride of Lime and Hypochlorites*.—In the literature of the lime chlorides and hypochlorites there seems to be some discrepancy of statement. Probably this is due to the confusion of commercial names. About the disinfective efficiency of hypochlorites of lime and soda there is no discrepancy. The active ingredient is chlorine; the efficacy of the disinfectant is in direct proportion to the amount of available chlorine, and the only point for practice is—which is the least objectionable form of applying the gas to the walls, or securing its generation there? Two illustrations are enough to demonstrate efficiency: (a) Dr. Sheridan Delépine found that “if a weak solution, made with about one part of the original chloride of lime and 100 parts of water, was applied to the walls or to any paper impregnated with tuberculous matter, thorough disinfection was obtained in a few hours. Disinfection was invariably obtained in that way, and, in fact, an exposure of a few minutes was generally sufficient, even with a weak solution like the one he had mentioned, when the layer of virulent sputum was not very thick.”\* Again, on experimenting with “hypochlorite of lime on various other germs than the tubercle bacillus,” Professor Delépine “found that

\* *Journ. of Sanitary Instit.*, vol. xviii., p. 403.

every germ tried was rapidly destroyed when brought into contact with a 10 per cent., or even 1 per cent. solution of chloride of lime. . . . Anthrax spores placed for a few minutes in such a weak solution as that he had just mentioned were killed.”\* (In these quotations the terms “chloride of lime” and “bleaching powder” are, I presume, synonymous, and both are synonymous with “chlorinated lime.” All three are names for a mixture containing chloride of calcium and hypochlorite of calcium, in varying proportions. It is a pity that one precise term cannot be agreed upon.) The results recorded in Dr. S. Rideal’s book, and in the Maine Report, are somewhat inconstant. The inconstancy is mainly due to the inconstancy in the composition of the liquor resulting from the dissolving of chloride of lime. The liquor seems to contain chlorine in a quantity varying with the previous exposure of the chlorinated lime. If this be exposed to light or to moisture the amount of available chlorine is reduced. Consequently, to be quite reliable, the chlorinated lime must be fresh, if not freshly made. This difficulty could be easily overcome. But there are other objections to the chlorinated lime liquor—it bleaches and tarnishes. Even these are not insuperable objections; in many varieties of house and ship they are not objections at all, but they certainly take away from the general simplicity and convenience of the disinfectant. I am thinking chiefly of walls and furniture. Where clothing, sponges, and the like are in question, one objection is serious: hypochlorite or chloride of lime renders, or tends to render, these articles brittle and ready to disintegrate. The same happens with hypochlorite of soda. No doubt this disastrous effect can be avoided when the disinfective solution is very dilute, or the time of exposure is short. But, in routine disinfection, these are very serious difficulties; for usually the articles to be disinfected are not very clean, and, therefore, the solutions should be stronger rather than weaker; and usually the removal from the steeping tub must be left to the discretion of the lieges, and, therefore, overtime is certain to be rather the rule than the exception. On one occasion, I left two good sponges for an hour in a recommended solution of hypochlorite of soda (perhaps, as it ought to be, the solution was stronger than normal). They were beautifully bleached; they smelt wholesomely of chlorine. But when I began to use them, they came to little pieces in my hand. I have not experimented with linen, cotton, or woollen articles; but from the experience of others, I gather that the same disintegration is liable to occur. In the disinfection of walls this is less an objection. Professor Delé-

† *Journ. of Sanitary Instit.*, vol. xviii., p. 404.

pine applied his liquids by a brush. He says that the damage to papers, etc., was less than one might expect. Possibly, but other things sufficiently nearly equal, that disinfectant is best that does no damage at all. He suggests that the damage was due to the mechanical action. In that case the spray would be at once a partial preventive of damage and an adequate disperser of the liquid. In brushing, as in spraying, the object is to get the liquid on to the organism. The mechanical action of brushing is more gross and less regular than the wind action of the sprayer, and, therefore, the spray method is preferable. The only point open to argument is: Is the mechanical effect of the spray sufficient to drive the liquid on to the organism? To this the reply is that what blows cobwebs away is enough to blow liquid on to the cobweb dust. The spray thoroughly wets, under considerable local pressure, the surfaces exposed to it. What the hair-brush does the spray-brush—for it is a brush of fine threads of air and liquid—will also do (see section iv. *infra*). (b) An illustration from the Maine Report: "The careful work of Nissen\* tended to confirm the value which Dr. Sternberg and the Committee of the American Public Health Association had ascribed to chloride of lime. Nissen's experiments were made with pure cultures of the bacillus of typhoid [*sic*], of cholera, of anthrax, and *Staphylococcus pyogenes aureus* and *Streptococcus erysipelatis*. The micro-organisms were in bouillon with 1 per cent. of peptone and 0.5 per cent. of common salt. Typhoid bacilli were destroyed with certainty in five minutes when the bouillon contained not less than 0.12 per cent. of chloride of lime, whether the mixture was filtered or not. Cholera bacilli were always killed in five minutes, and in most cases in one minute. . . . Anthrax bacilli, without spores, were destroyed in one minute with a 0.1 per cent. solution, and streptococcus just as quickly when the bouillon contained 0.2 per cent. of chloride of lime. Solutions of chloride of lime were found to lose their disinfectant power rapidly when used on anthrax spores. This was observed even in ten, fifteen, and thirty minutes after they were prepared. . . . Nissen says that chloride of lime may be added in the form of powder to dejections at the rate of 0.5 per cent. of the volume of the matter to be disinfected; or, taking into consideration the difference in the quality of the chloride of lime, 1 per cent. may be added (1 gramme to 100 cc.). The stool may be emptied in ten minutes after the addition of the chloride of lime."† (In the Maine Report chloride of lime is synonymous with hypochlorite of calcium.)

\* *Zeit. für Hygiene*, viii. 62, 1890.

† Maine Report, pp. 199, 200.

(f) *Lysol*.—This I take as the type of a viscid, but soluble, disinfectant. It is held to be superior to carbolic acid in disinfective power. For washing of wood-work, floors, beds, etc., it is eminently suitable, being at once a soap and a disinfectant. For such purposes it should be used in hot solutions of 1 to 2 per cent. For the purposes of spraying or showering it is more suitable than the emulsions, but less suitable than the clear liquids. With its other numerous uses we are not here concerned.

(g) *Carbolic Acid and Common Salt*.—A disinfectant so well accredited by so many various masters as carbolic acid hardly requires mention. But carbolic acid will not do everything, and to do anything it must be intelligently handled. With the anthrax spore test it fails, like most others; but the range of its efficiency is wide enough for most practical purposes. For disinfecting walls by washing or rubbing, it may be sufficient; but in the practicable solutions it is certainly less reliable than the hypochlorites, or corrosive sublimate, or formaldehyde. "In a paper on the molecular conditions of aqueous solutions of disinfectants, as regards their efficiency, Dr. Scheurlen states that 1 per cent. solutions of carbolic acid, or of the cresols, in water failed to destroy *Staphylococcus pyogenes aureus* in five minutes; but 1 per cent. solutions of carbolic acid, with 24 per cent. of common salt, or 1 per cent. cresol, with 12 or 13 per cent. of common salt, destroyed the same organisms in one minute. He found also that 1 per cent. carbolic acid with 12 and with 20 per cent. of common salt, and  $\frac{1}{2}$  per cent. as well as 1 per cent. of cresol, with 13 and again with 19 per cent. of common salt, destroyed anthrax spores in three days at latest, while these solutions without the addition of the salt had hardly the slightest effect upon the spores."\* These are striking facts. Surgical habits have, I suspect, had a good deal to do with the uncritical belief in the high disinfective potential of carbolic acid.

(h) *Formaldehyde*.—The disinfective efficiency of formaldehyde it is convenient to discuss under two heads—1. Efficiency as a free gas; 2. Efficiency as a solution in water (formalin) or alcohol (holzlin). The disinfective agent is always the gas, but for some reason or other the results of the free gas do not quite conform with the results of the gas in solution. The same was true of sulphurous anhydride and sulphurous acid. The precise physical and chemical reasons for this I need not speculate upon; what concerns us now is the fact of the discrepancy. A few selected

\* Maine Report, pp. 191, 192; *Archiv für Exper. Pathol. u. Phar.*, xxxvii. 74, 1896.

facts will determine whether, for our primary purpose of room disinfection, either the free gas or the dissolved can be relied upon.

1. *As a Free Gas.*—The gas has been put to very severe tests, and has come out on the whole well. Authorities are not unanimous, but very nearly so. The objective results recorded are certainly very strongly in its favour. Professor Sheridan Delépine names the following organisms as the "most convenient for testing gaseous disinfectants": *Bacillus coli communis*, *Bacillus pyocyaneus*, *Staphylococcus pyogenes aureus*, *Bacillus subtilis* (sporing), *Bacillus anthracis* (sporing), *Bacillus tuberculosis* in sputum, fresh or dry, horse manure with highly-resistant sporing bacilli. These are to be taken in young or old cultures, in emulsions, on threads, on agar, etc. He adds: "I think a disinfectant capable of killing the six first organisms exposed to it in various states and under various conditions is, for practical purposes, a reliable disinfectant. If, in addition, it is capable of destroying the spores usually found in horse manure, and which often can resist for half an hour the action of saturated steam at 100° C., the disinfectant may be considered as exceedingly powerful."\* How does formaldehyde stand these tests? Thus: "(1) With unimportant exceptions the *Bacillus coli communis*, *Bacillus pyocyaneus*, *Bacillus tuberculosis*, and *Staphylococcus pyogenes aureus* were killed whether in the dry or moist state, even when placed in deep narrow recesses (2 inches from the opening of tubes open only at one end, or protected by one to three layers of filter-paper, or embedded in a thick layer of sputum). (2) The spores of the *Bacillus anthracis* were killed in twelve experiments out of nineteen, made under conditions not favourable to the free access of the gas. The differences in the results seem to have been due more to certain states of the spores than to variability in the action of formic aldehyde. (3) The action on spores of the *Bacillus subtilis* was uncertain. The very highly-resisting spores of a horse-manure bacillus resembling the hay bacillus could not be killed by exposures of a practical duration. The only evidence of action was a distinct delay in their growth."† He adds, further, that "wood spirit of the proper quality and strength costs 10s. a gallon. . . . It is quite evident that formaldehyde is the best gaseous disinfectant we possess at the present time for objects which are liable to be damaged by damp, chlorine, dry and moist heat." Now as to quantities and time of exposure. One or two instances

\* "Some Experiments on the Disinfection of Rooms by Gaseous Formaldehyde," *Journal of State Medicine*, 1898.

† *Ibid.*, p. 15.

are enough. In three experiments with silk threads impregnated with thick emulsion of anthrax spores and kept dry in the dark for eighteen months, the duration of exposure was twenty-four hours; the amount of alcohol consumed was 5,900 m.; the distances of the objects from the lamp were respectively 9 feet, 6 feet, 10½ feet; the cultivating test media were respectively bouillon, agar, bouillon. The result was complete sterilization in each case. The control experiment gave abundant growth. In five experiments with partially-dried tuberculous sputum, the duration of exposure was twenty-four hours. The amount of alcohol consumed was 2,950 m.; the distances were as in the anthrax experiments. In every case the result was complete sterilization. For practice, the only point to decide is this: Can Professor Delépine's conditions be reproduced? This I shall discuss further on.

These striking experiments are in many ways confirmed by the work of Messrs. F. C. Robinson and B. L. Bryant in their "Bacteriological Report upon Formaldehyde."\* But their results are best discussed under methods of application. All the leading facts have been summarized by Dr. A. G. Young.† With two or three exceptions the authorities speak in the same sense as Professor Delépine.

2. *In Solution*—*Formalin (water), holzin (alcohol)*.—That the disinfective potential of formalin (that is, 35 to 40 per cent. solution of formaldehyde) stands very high is now beyond dispute. Blum, Vanderlinder and Burk, and Strehl are the only investigators whose results are not favourable. But Blum admits that "weak solutions suffice to prevent fermentation with gradual destruction of the bacteria."‡ The evidence on the other side is overwhelming. Mosso and Paoletti find "that 1 part in 20,000 slows the ammoniacal fermentation of urine, and that 1 in 4,000 inhibits it entirely. . . . Walter states that a solution of formalin, 1 in 10,000, prevents the growth of the bacilli of anthrax, cholera, typhoid, diphtheria, and of the *Staphylococcus pyogenes aureus*. . . . In 1 per cent. solution it kills a pure culture of almost any pathogenic micro-organism in thirty minutes. In 3 per cent. solution it kills anthrax bacillus in fifteen minutes, and all other pathogenic micro-organisms in one minute."\* Walter also found that faecal matter was almost instantaneously deodorized by a 1 per cent. solution, and sterilized in ten minutes with 10 per cent. solutions. Philip found that "with formalin anthrax bacilli were killed in

\* Maine Report, 1896-97, pp. 132-169.

† *Ibid.*, pp. 234-265.

‡ *Ibid.*, p. 235.

\* *Zeit. für Hygiene*, April, 1896, summarized in *Medical Annual*, 1897, p. 40.



fifteen minutes with a 10 per cent. solution, and anthrax spores capable of withstanding action of steam at 100° C. twelve minutes were destroyed in less than five hours with a 10 per cent. solution. With a 5 per cent. solution, diphtheria bacillus was destroyed in ten minutes, anthrax bacillus in fifteen minutes, *Staphylococcus aureus* in thirty minutes, and anthrax spores in five hours.”\* Even some that doubt the efficiency of the free gas in dust-disinfection acknowledge the efficiency of the solution. Thus, Professor Sims Woodhead: “From what one knew of its action in solution there could be no doubt that formic aldehyde or formalin was an exceedingly powerful disinfectant.”† Dr. Blaxall is reported to have sterilized milk by a solution of one part formalin in 500 milk. Probably some organisms are much more susceptible to this disinfectant than others. This would explain the slight discrepancies recorded. Then, in confirmation of its disinfective potential, formalin is a powerful deodorant. Anyone that doubts this may relieve his doubts by sprinkling a few drops of formalin in a lobby or room that smells of “cats.” The “cats” smell is almost as pervasive and persistent as musk, to which the odoriferous agent may be allied; yet, in a very short time, the odour yields before formalin. “Aronson says that formaldehyde combines readily with nearly all ill-smelling compounds, as hydrogen-sulphide, mercaptan, and ammonia, forming others that are non-offensive.”‡

These are only some striking selections from a vast mass of evidence. They are enough to justify the inference that if the pathogenic organisms are subjected for a sufficient time to a sufficiently strong solution of formalin, the result will be disinfection. The strength of solution is limited by considerations of practice. I have used for ships, and for some rooms, a 10 per cent. solution. But when this is sprayed the operators cannot long occupy the room. When, however, rooms or forecastles are small the operators can stand near the door; they can begin at the distal parts first, and retreat gradually. For routine purposes, I use a solution of formalin 1 part, and water 40 parts; that is, formalin 4 ounces, water 1 gallon. This, however, is qualified by one fact. To prevent the polymerization of formaldehyde in the original formalin, I add to each gallon bottle about 5 ounces of glycerine. This prevents any precipitate. It also results in making the solution dry more slowly. This means that the gas is longer retained in the solution; for, as can easily be shown, formaldehyde is very readily absorbed by moisture, and therefore, of course, is long retained by it. This

\* *Centr. für Bak.*, xvii. 499, 1895, in Maine Report, p. 236.

† *Journ. of San. Instit.*, vol. xviii., p. 420.

‡ Maine Report, p. 237.

secures longer exposure of the wall-dust to the action of the gas. The amount of glycerine, however, is not enough to make the sprayed walls damp for any considerable time. Since I began to use glycerinated formalin, I have had no complaint either of the precipitation of the formaldehyde or of the hyper-hygrosopic effects of the glycerine. A German observer recommends spraying with "glycero-formol"—the glycerine preventing polymerization.

Does formaldehyde penetrate? It is important to answer this question, but it is hardly relevant to my primary topic; for penetration rather concerns clothing, mattresses, etc., than surfaces of walls and furniture, and I have shown that steam methods are the best for clothing, etc. All the same, formaldehyde does penetrate more than any other gaseous disinfectant. The vacuum principle could easily be applied to increase the penetration. It has been applied by Dr. Kinyoun.

IV. METHODS OF APPLYING DISINFECTANTS TO SURFACES.—So much for the efficiency of these selected disinfectants. Many others, it is unnecessary to say, might be named, and these are in no way prejudiced by anything I have said. Like those I have selected, they must go by the scientific evidence they can produce, and many of them can produce much evidence. The matter I am mainly concerned with is, given an efficient disinfectant, how shall we apply it efficiently to the surfaces of a room and of its contents? There is a long distance between the exact conditions of experiment and the rough conditions realizable within the limits of possible practice. But the distance has to be bridged, and the following are four good methods of bridging it. Personally, I prefer the last of the four, because, in my experience, I have found it best fulfil the three conditions I have found of most importance in the evolution of the disinfective organ of the public health service. The three conditions are: simplicity, rapidity, and cheapness. Its efficiency I consider equal to any of the others.

1. *Disinfection by Free Gas.*—Here I consider only the methods of applying formaldehyde. The only other gas that deserves further discussion is chlorine, and that is best applied—not as a gas, but as a solution—by brush, shower, or spray.

In all forms of disinfection by a free gas the object is twofold: first, to disinfect the air—a useless labour; second, to disinfect the surfaces of room and furnishings. To secure the second object, it is essential to fill the cubic content of the room with the gas. To do this it is necessary to prevent the escape of the gas by blocking of outlets. Here is the great practical difficulty. To stop the outlets even of a fairly well-built modern room is a matter of patient

and detailed labour. The major outlets—windows and fire-places—can be dealt with easily; the innumerable minute outlets only with very great difficulty. In dilapidated or ill-constructed rooms the stoppage of outlets is frequently impossible, yet such are frequently the houses demanding most thorough disinfection. In Professor Delépine's experimental room the necessary conditions were secured. In the rooms described by Messrs. Robinson and Bryant, similar care was taken to prevent rapid dispersion of the gas. With formaldehyde, however, the rapidity of dispersion is less than with gases whose density is nearer the density of the air. Yet even with formaldehyde the stoppage of outlets is essential to the concentration necessary for disinfection. For my own part, I consider that the stopping of outlets—or inlets—is a matter of considerable skill; it is also a matter of considerable time; it is rarely a matter of simplicity. Doubtless it *can* be done; but the doing of it adds to the bother of the disinfection. But let it be distinctly understood that to attempt formaldehyde disinfection without the stoppage of outlets is to reduce it to the same level of farcical futility as sulphur-fumigation frequently—not, of course, always—falls to. Let us, however, assume the stopped outlets.

(a) *Evaporation of Formalin*—(a) *Sheet Evaporation*.—To hang in the room sheets soaked in formalin is a simple method, and probably may be made adequate. But there ensues a great waste of formalin, for there is left a polymeric residue. This may be re-utilized by exposure to heat, or reconverted into formaldehyde by hot water; but to the extent that it is polymerized, it is for the time inert. For the disinfection of small articles or delicate fabrics, however, the sheet method is good and useful—that is, the amount of formaldehyde communicated to the fabrics is sufficient for disinfection, but for room disinfection proper it may be discarded.

(β) *Heat Evaporation*.—To prevent polymerization during evaporation, several pieces of apparatus have been constructed. Trillat's is designed to evaporate, under pressure, a combination of calcium chloride and formaldehyde. In practice, the objections to his apparatus outweigh, in my opinion, its advantages. And there is evidence to prove that "the polymerization of formalin while undergoing distillation is largely a myth."\* "If rapidly distilled, polymerization rarely occurs."† Novy has designed a vessel for such rapid distillation. "A good Bunsen burner will distil five ounces of formalin, the amount necessary per 1,000 cubic feet, within ten or fifteen minutes. Should there be a tendency of the formalin to polymerize, it can be prevented by the addition of 5 or

\* Novy, Maine Report, p. 239; *Medical News*, lxxii., p. 641, 1898. † *Ibid.*

10 grains of borax."\* Definite results are not given, but they seem to have been good. Provided, therefore, the conditions are such as to insure the necessary concentration, the evaporation of formalin will give sufficient formaldehyde. The vaporization of "paraform" may be mentioned under this head. It is really a repolymerization of formaldehyde.

(γ) *Formogenic Apparatus*.—Formaldehyde may be directly generated from methylic alcohol. The apparatus used by Professor Delépine was the Formogène Richard. Others are, the Moffat generator and Bowdoin generator, and there are still others.

Of all generators tested by the American investigators, one thing was true: "Of all the forms of apparatus tested, none of them can be relied upon to sterilize free surfaces in five hours with the amount of material advocated by their different manufacturers. . . . None of them came up to the standard claimed for them."†

(δ) *Temperature, Dampness of Objects, etc.*—Temperature is of some consequence. The higher the temperature, the more rapid the disinfection. But the variation in efficacy is not great between 50° F. and 80° F. (room temperature). Dampness of walls is also of some importance. Professor Robinson "says that dampness is a disadvantage as it absorbs more or less of the gas and holds the odour in the rooms."‡ But "Novy found that infectious material is much more readily disinfected when it is moist. Even wet spore material is thoroughly disinfected by formaldehyde, whereas such material is not affected by sulphur. He found that the walls and floor of the room to be disinfected, and whatever articles are present (previously spread out as much as possible), should be sprayed with water before exposure to formaldehyde vapour. Owing to the great solubility of formaldehyde, large vessels of water should not be kept in the room to be disinfected. When water is thus kept in the room, scarcely any odour of formalin will remain at the end of twenty hours, whereas, in the absence of such water, the odour at the end of the time mentioned will be intolerable."§

A reasonable deduction from these physical peculiarities of formaldehyde is that the ends of the free-gas method are well attained when the gas is dissolved up by the moisture on the walls or objects. But this is only another way of saying that a solution of the gas applied to the walls is an efficient method of disinfection. If this is so, would it not be less roundabout to squirt, or shower, or brush, or spray the solution directly on? Is it not probable that, since the solution can be first graded to a more than sufficient

\* Novy, Maine Report, p. 239; *Medical News*, lxxii., p. 641, 1898.

† Maine Report, p. 153.

‡ *Ibid.*, p. 242.

§ *Ibid.*, p. 242.

strength, the direct application of it to the walls will be *à fortiori* more reliable than the application of water and gas separately? In fact, the results with the free gas are a most powerful confirmation of the methods where the solution is applied directly—that is, of the brush, or shower, or spray.

Before passing to these, I may note that the results obtained when the formogenic lamps were used to fill an air-tight bag containing infected clothing were excellent. Indeed, we here have a method that, for disinfection of clothing, might become almost a perfect substitute for steam. To secure penetration, it might be possible to arrange an application of the aspiration pump. Dr. Kinyoun has devised a metallic “disinfecting chamber, to which is attached a vacuum apparatus, a small boiler in which formaldehyde is generated by volatilization from a mixture of formalin and calcium chloride, and a second small boiler from which ammonia is turned into the disinfecting chamber by the evaporation of ammonia water.”\* These appliances are still in the experimental stage, but the non-spoiling of fabrics is a strong point in their favour.

2. *Disinfection by Moist Brushing or Rubbing.*—Of the efficacy of brushing or rubbing with disinfective solutions, there is no question. Professor Delépine’s results are decisive. In many towns no objection is taken to the method. But it is admitted that such a method involves the spoiling of wall-papers. It is also very laborious. Except for special cases, therefore, the method seems to me loaded with disadvantages. With any of the solutions I have discussed, brushing may be made effective; the mechanical action counts for something. But, after all, the essence of the matter is the wetting of infectious material with a sufficiently strong disinfective solution. This can be done by shower or spray with sufficient mechanical force to insure thorough wetting, yet without any destruction of the paper, except when the disinfectant has chemical effects on the colour. These effects, as I have already stated, are among the disadvantages of the hypochlorites. With solutions of formalin, as I have shown elsewhere, there is no destruction of paper and no interference with colours. In very soft papers, occasionally there may be some “running”; but after four years of formalin I have had not one complaint of destroyed papers.

3. *Disinfection by Jet or Shower.*—Here again the purpose is to wet thoroughly the surfaces to be disinfected. This, like spraying, can also be done without destruction of papers. With suitable appliances, it could be made simple, rapid, and cheap. There must

\* Maine Report, p. 239.

be a certain extravagance in the amount of liquid used, but this is no great objection. In jetting or showering with formalin there is the double advantage of a solution and a gas. The "messaging" due to the wetting of floors is, if kept short of "damages" point, an advantage; it compels the housekeeper, for the sake of comfort, to clean up. The same is true of the spray.

4. *Disinfection by Spray.*—(a) *General Results.*—In the *Sanitary Journal* for March, 1897, I gave some account of the spray method. My experience since then has confirmed me in the positions there maintained. In hospital disinfection I have used the formalin spray for over four years. In general disinfection I have used it for over three years. More than 2,000 houses or rooms have been sprayed. I have ceased to keep any record of the number of hospital wards disinfected. I may state generally that the sprayings included cases of typhus and enteric fevers, diphtheria, small-pox—modified, discrete, confluent, and hæmorrhagic—chicken-pox, measles, scarlet fever, erysipelas, cellulitis, and anthrax. Wards occupied for six months with scarlet fever have been sprayed and then used for measles. Wards occupied for months with measles have been sprayed and used for chicken-pox. Wards exposed for weeks to chicken-pox have been sprayed and used for measles. And so on. Diphtheria has been housed after scarlet fever, and scarlet fever after diphtheria; scarlet fever after whooping-cough, and whooping-cough after scarlet fever, the whole yielding a large variety of permutations and combinations. In houses second cases frequently occur within the incubation period; but, except in one or two cases where personal contact with a patient was proved, I have not had a recurrence of infection. In the hospital I have never had any cross-infection after spraying except once, and that was found to be due to an undiscovered case of scarlet fever still persisting after an outbreak due to a case of mixed diphtheria and scarlet fever infection. A considerable number of those double cases had been admitted, and one or two escaped observation, the evidences of diphtheria predominating. The result was that several diphtheria patients developed scarlet fever. One could not attribute the spread entirely to conveyance by nurses. Part of it was, in all probability, aerial. After a thorough disinfection, yet another case occurred, and this, as I have said, was due to an original case that had escaped observation. When this was removed and the ward again sprayed, no fresh case appeared. This outbreak showed that the patients in a ward may be rapidly infected; it also shows that isolation and disinfection are efficient to stamp out such infection. The amount due to the infected room it is impos-

sible to specify precisely; it is equally impossible to eliminate that amount altogether. More stress may be laid on the disinfection of houses and wards infected with typhus fever, small-pox, and measles. In no case have I found recurrence except within the incubation period. Further, I have noticed that after a scarlet fever pavilion has been some months in use, it begins to grow more or less unhealthy, even when kept as clean as hospital methods make possible, and that is *very* clean. One notices more swollen glands, more irritated eyes, more eczematous mouths and noses, more small abscesses, more slowly mending throats, and so on. In such circumstances I have shifted the whole camp, sprayed the pavilion, and in a few days reoccupied. The result has always been good. The known habits of microbic life in occupied rooms would lead us to expect nothing less. The dust on hospital walls is mainly wool from the beds. These, in a scarlet fever ward, must be regarded as the most infectious articles in the ward. Consequently, in such wards, the persistence of streptococcus or staphylococcus, in some form, is almost normal. These organisms must be kept in check. To them many of the minor ailments I have referred to are doubtless due. Under the formalin spray such ailments vanish. The pre-Listerian experiences of hospitalism are only too ready to repeat themselves; but the repetition is now inexcusable, for hospitals can be so constructed as to make cleansing and disinfection easy.

These large effects are among the best confirmations of the efficiency of any method. They are less definite in detail than the results of laboratory experiment; but they are equally impressive in the mass. I could add many other groups of facts; for example, an outbreak of measles in a scarlet fever ward (owing to an imported case), shifting of the camp, spraying, reoccupation without occurrence of fresh cases. Such isolated groups have little value by themselves. They must be taken as part of a sustained experience. When the facts are of a well-understood order, as when a surgical wound is nursed in an erysipelas ward, they fall in with the great body of experiences that form the scientific basis of aseptic surgery. More than once I have nursed a tracheotomy case in a ward sprayed after being recently occupied by an erysipelas case. Such exposures, however, have been, in my own experience, too few to found any negative proposition upon them. At the same time, every surgeon knows the susceptibility of open wounds in an erysipelas ward, and the treachery of erysipelas once it enters a surgical ward. On one occasion a surgical ward in a general hospital had somehow become infected; two or three fresh

cases occurred. The ward was fumigated with sulphur, and the usual measures taken with floors and beds, but cases continued to recur. It was then sprayed, and correspondingly radical measures were taken with beds, utensils, etc. No fresh cases occurred. Here the spraying was only a part of the general sterilization; but it was the greatest part so far as the ward itself was concerned.

These facts, and such as these—the changes rung on infected wards, the interchange of diseases carried out as nearly as possible on the principles of aseptic surgery, the vanishing of minor ward ailments, the sweetening of houses—have combined to confirm not only the masses of evidence in favour of the high potency of formalin as a disinfectant, but also the particular method of applying it to infected areas.

(b) *Some Practical Points.*—(a) *Time of Exposure.*—An important point to settle is—How long does any dust on a wall remain exposed to the disinfectant applied? If corrosive sublimate is the disinfectant used, one minute is sufficient to disinfect non-sporing germs, and five minutes, or thereby, to disinfect anthrax spores. If formalin is used, in 2 per cent. solution, I should say that at least five minutes' exposure ought to be secured; but, if possible, half an hour, or an hour, or longer. Now, with the spray, I have found as follows: *First*, A varnished wooden wall of a hospital was sprayed on a frosty day. The temperature of the ward was about 40° F. The drops of liquid remained quite visible, and on rubbing with the finger the surface was found quite moist at the end of four and a half hours. This was several times repeated. *Second*, In a hospital ward, with impervious walls, the wall remained moist for one and three-quarter hours, although the part was exposed to a mild sun shining through a window. In the shade the same wall remained moist for at least three hours. *Third*, I have made no exact observations on papered walls; but here the capillarity of the paper plays a part. The effect is the same as when a paper is soaked in a disinfectant. The rate of drying depends on the temperature and the amount of water-vapour. This, in a recently sprayed room, is necessarily great; evaporation is, consequently, slowed down, and, in every case, one may be reasonably certain that the time of exposure will not be less than half an hour to an hour. From rough observation, I am inclined to regard the time of drying as much longer. But other tests are available with formalin. For, so long as any gaseous smell remains, formalin is being given off from the walls. Now, I have sometimes found a strong smell of formalin in a sprayed room at the end of twenty-four hours. In the hospital first-named, when the temperature



was low, the disinfectors found that a ward sprayed on one afternoon was almost intolerable next forenoon—a period of eighteen hours, and this without any special effort to retain the gas. In warm rooms, where a fire is active, the smell, intolerable at first, vanishes rapidly, but it is usually powerful even at the end of four or five hours, unless special measures have been taken to ventilate. The persistence of the smell indicates that the formalin on the walls is still active. And, with papered or sized walls, the absorption of the liquid secures its better diffusion. Spraying becomes a soaking. I should add that, with formalin, the spray acts not only as a moistener, but also as a fumigator. It applies the dissolved gas in the first instance to the most important surfaces, but, thereafter, the gas is set free to penetrate the parts that the liquid may have missed. This cannot at any time be great, for a good sprayer throws a cloud of about 5 feet by 2 or 3 feet; this soon fills a room, and every globule gives up something to the atmosphere. Thus it seems to me that, while the advantages of forced application of a strong liquid are gained, the advantages of a subsequent fumigation by formaldehyde are not lost. The delicate mobility of the spray-cloud makes it applicable to any shape of room, press, closet, wardrobe, or other combination of the inconveniences that fill the ordinary “furnished” bedroom. This point I mention because it has been maintained that the spray can be relied on only if every part is “religiously” wetted. Every part ought to be wetted; but the wetting is not, with formalin, the whole case. On one occasion I took some friends into the ambulance shed at East Pilton Hospital. The odour of formalin was so strong that they had to withdraw. The ambulance had gone out an hour before; the driver had sprayed it, and the diffused formalin in the large well-ventilated shed was the remnant of the spraying.

(β) *Strength of Solutions.*—Of formalin, I use a solution of 1 in 40; that is, formalin 4 ounces, water 1 gallon. Recently, on account of the addition of glycerine, I have used a little over 4 ounces; in cases requiring special disinfection, 6, 7, 8, and 10 ounces to the gallon. On the basis of the facts already given, I consider that, with 1 in 30 to 40, there is ample margin for any reasonable variation in the time of exposure. For the other solutions the standard strengths would be—corrosive sublimate, 1 in 1,000; hypochlorite of calcium or soda, 1 per cent.; lysol, 2 per cent.; carbolic acid with salt, 5 per cent.

(γ) *Management of the Equifex Sprayers.*—I name these, because I am not familiar with the working of others. There are several little points of management that make all the difference between

satisfaction and dissatisfaction in the use of the spray. Perhaps I shall be well enough within my title if I detail one or two such points.

(1) *Length of Tubes.*—For convenience of working I prefer 12-foot tubes to either 8-foot or 6-foot tubes. Doubtless the labour is somewhat increased by the increased column of water; but the adaptivity of the sprayer is also increased. Where two men work the instrument—one as the pumper and the other as the disperser—12-foot tubes increase the convenience many fold. The longer the tubes the longer the pumper may remain in one spot, and the more regularly may the disperser work. Indeed, in small rooms the pumper barely needs to change his *locus* at all. The disperser, on the other hand, finds it easier to manipulate the nozzle freely, and to direct it under beds, into drawers, under mattresses, behind doors, behind wardrobes, and so on. Where the roof is over 8 feet high, it is a great convenience to have over 8 feet of tubing to work with. Further, the longer tube secures a more equable production of the spray-cloud. As the arteries, by their elasticity acting against a frontal resistance, convert a pulsating into a non-pulsating current, so the more diffused elasticity of the longer tubes maintains a steadier pressure of air and water. This can be readily tested. If, after the air-pressure is high, the air and water stopcocks are both closed, the spray still continues for a considerable number of seconds. With the shorter tubes, the current stops in a second or two. Once more, the longer the tube, the thicker their walls should be. The thinner tubes readily form “aneurisms” under the pressure. Probably such high pressure is never needed, but it is apt to occur in practice. For the two-men system, longer and thicker tubes increase the convenience; for the one-man system, shorter tubes diminish the labour.

(2) *Adjustments.*—With the larger equifex instruments, where the air-pump is the only pump, there is usually no difficulty in adjustment. I hesitate to detail the extremely elementary mechanical directions necessary for satisfactory sprayings; I should hesitate still more had I not found them sometimes beyond the skill of intelligent, but non-mechanic, workmen. To make the sprayer work properly, it is necessary to have the rubber tubes clean, unobstructed, and properly fastened; to have the air-pump well oiled both in the piston and piston-rod (I had an apparatus seriously spoiled for want of so seeming simple a precaution); to secure that the ball-valve is acting freely; to cleanse it with benzine if necessary; to see that the tube between air-pump and cylinder is free of grease or other obstruction (benzine being here

again useful); to test the patency of the air outlet and water outlet before fastening on the rubber tubes; to test separately the air-tube and water-tube after fastening; to see that the internal nipple of the air-tube is perfectly pervious (it is apt to be choked by shreds of rubber or dirt); to examine from time to time the collared retarder of the water-tube, and to see that it is pervious; to see that the stopcocks are tight; to wash out the cylinder frequently. In the larger apparatus I have found it best to turn on the water first; then slowly to turn on the air until the necessary fineness of spray is obtained; then with slow, steady thrusts to maintain such pressure as will give the particular best "note." Every sprayer has a pitch of its own when it is at its best. The men prefer to secure this note first, and to maintain it all through the operation. Again, if the spray tends to squirt, it is occasionally because some water has passed into the air-tube. Let this be pumped clear, and a little readjustment for higher or lower levels will prevent the squirting.

With the more portable apparatus, which has two pumps, one for sucking up the water, the other for forcing in the air, adjustment is more difficult. The secret of rapid adjustment lies in the feed-pipe. The stopcock on this should be turned off until the apparatus takes up just the amount necessary to keep a constant spray. The other stopcocks should be adjusted correspondingly. I have found that the bottom nipple valve is apt to freeze on exposure in cold weather. Care is needed in thawing. I should not think of noting these trifles did I not know that they *have* given rise to difficulties. I may add that the pumping labour of the more portable spray has been found distinctly less than of the heavier, and this without impairing the extent or quality of the spray-cloud. Frequent oiling is absolutely essential in the lighter apparatus. The apparatus should be pumped empty after a day's use.

As a criticism of both appliances one may say that the heavier is needlessly heavy and clumsy; the lighter suffers from the necessity of a detached vessel for the liquid. To meet the latter point, I am adjusting a light vessel to the outside of the cylinder. Vessel and cylinder can be strapped to the pumper's leg, and thus the stability of the whole sprayer is improved.

*Conclusion.*—In this discussion I have attempted to be at once critical and practical; to base my opinions and inferences only upon adequately demonstrated facts; to select instances relative to the limited problem of room-disinfection; and, finally, by a sifted correlation of practices, to educe a simpler practice.