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Leedham-Green, Charles.

Publication/Creation

London : Simpkin, Marshall, Hamilton, Kent & co., 1904.

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OF THE HANDS :
A BACTERIOLOGICAL INQUIRY.

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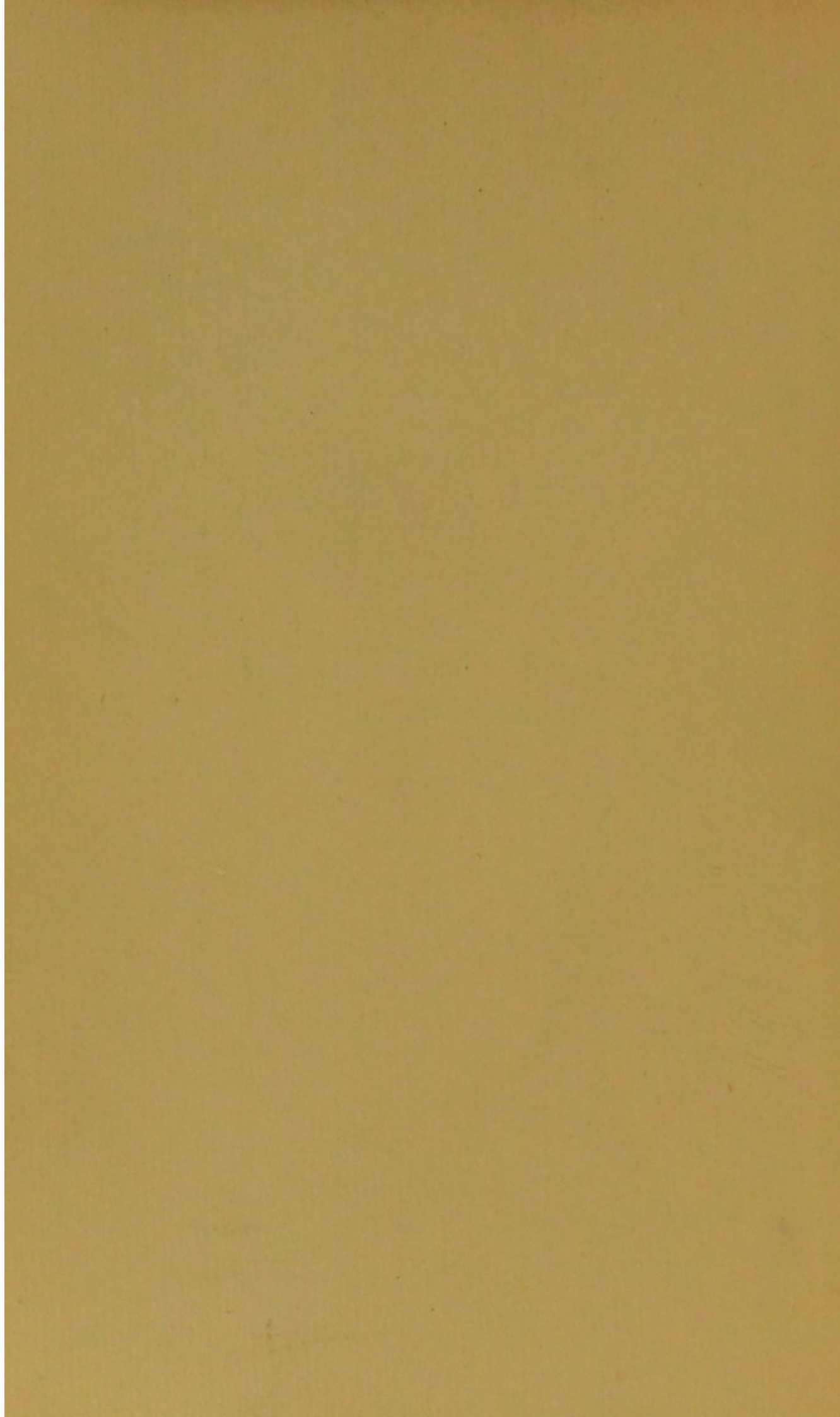
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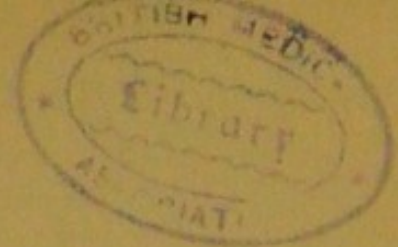


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ON THE STERILISATION OF THE HANDS

A BACTERIOLOGICAL INQUIRY INTO
THE RELATIVE VALUE OF VARIOUS
AGENTS USED IN THE DISINFECTION
OF THE HANDS.

By

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LONDON :

SIMPKIN, MARSHALL, HAMILTON, KENT & CO., LTD.

BIRMINGHAM :

CORNISH BROTHERS, LTD.

1904.

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ON THE STERILISATION OF THE HANDS.

INTRODUCTION.

THE following series of experiments grew out of an endeavour to determine, for personal guidance, the relative value of certain methods advocated for rendering a surgeon's hands sterile. There was at the time no thought of writing a formal treatise; but, as one investigation led to another, and as interesting and difficult problems presented themselves for solution, I was tempted to publish an account of my work as a slight contribution to the mass of literature that had already appeared on this subject.

In 1896 I wrote an account of a series of experiments on the sterilisation of the hands by means of alcohol (*Deutsch. Med. Woch.*, 1896), and a little later in the same year of a further series to determine the merit of various other agents used in cleansing the skin (*Brit. Med. Journal*, October, 1896).

As this subject has, of late years, been discussed almost solely in German papers, English writers having strangely ignored it (see bibliography at the end of this paper), I have ventured to describe its gradual development in some detail. During the past decade considerable attention has been given to the question of sterilising the skin, with the result that certain facts have been definitely established which require no further confirmation. At the same time, it would be a mistake to assume, as has been done, that the whole matter has been settled. There are yet numerous important problems which await solution, and a keen strife still rages over the subject.

METHOD OF INVESTIGATION.

Almost every experimentalist uses a different method of testing the sterility of the hands. This is one of many reasons which make a comparison of the results of different investigators almost an impossibility. In some cases the test applied is too simple and not sufficiently thorough, as when Schleich* uses a platinum loop to stroke the dried hand; whereas in others it is too severe, extending even to the excision of portions of the skin. We desire a test which shall give a fair indication of the extent to which the hands at a surgical operation are infected after any process of sterilisation has been attempted. The test should present an idea of the number of microbes that would be liable to be detached during an operation. In deciding whether the hands may be rendered sterile or not, we do not require a stronger test than this. When, however, we turn to consider the relative efficiency of the various methods of sterilisation proposed, more stringent means may with advantage be employed.

Personally I have almost invariably proceeded as follows:—After the process of cleansing the hands has been completed, they have been finally rinsed in warm sterilised water, and then, *whilst still wet*, a sterile slip of thin ivory, having broad angular ends and roughened surfaces, has been rubbed by the operator over both his hands, and its points have been passed under his nails. This slip has then been dropped into a tube containing culture medium. This method is similar to the one suggested by Fürbringer,† except that he used sharpened pieces of hard wood instead of ivory. The

* *Neue Methode der Wundheilung*. Berlin, 1899.

† *Untersuchungen und Vorschriften über die Desinfektion der Hände des Arztes*. Wiesbaden, 1886.

latter is, in my opinion, decidedly preferable, as the ivory slips are more easily sterilised, and the broad angular end is vastly superior to a sharp point for scraping the nail-fold. It is important that the hands be tested whilst *still wet*, for, as we shall see, the epithelial cells and microbes are by no means so readily detached from a dry hand as from a wet one. For the culture medium I have used agar-agar in tubes. Since my object has been rather to ascertain whether the hands were free from micro-organisms than to count their number, I have preferred tubes to Petri's plates; for, as every bacteriologist knows, the use of the latter involves considerable risk of accidental infection—a risk which, for my purpose, it was desirable to avoid. The culture tubes were repeatedly sterilised and tested in the incubator before being used, and heated again prior to inoculation, in order to liquefy the agar-agar. They were then cooled down to a temperature slightly above that at which the agar solidifies, and the inoculated ivory slips were dropped into them. The tubes were then gently agitated, in order to detach and distribute through the medium any microbes that might be adhering to the slips. They were then placed in a slanting position until the agar was solidified. For six days they were incubated at body-temperature, after which the colonies of micro-organisms were counted by means of a low magnifying glass. Experience has taught me that it is advisable to re-count them after a lapse of several weeks. For, although under ordinary circumstances all micro-organisms will have developed within the six days, this is not always the case, *if the bacteria have been in contact with a powerful antiseptic*. Where such contact has occurred, the micro-organisms are often inhibited in their growth for a considerable time, and are only recognisable

as colonies a fortnight or more after incubation. Moreover, the production of the pigment, characteristic of many microbes, is often very slow. In re-counting the colonies at the end of several weeks, any new growth, which might have appeared on the surface of the agar, was ignored as being possibly due to accidental infection. Care has also to be taken to avoid another source of error in the estimation of the number of colonies which develop in the agar tube, namely, the subsequent infection of the surface of the agar by the condensation-water which forms at the bottom of the tube. If the agar has been inoculated whilst liquid, the condensation-water will also probably contain some of the microbes. These microbes are, of course, placed under circumstances very favourable to their rapid development, and, if the infected condensation-fluid be allowed to run over the sloped surface of the agar, a greatly exaggerated idea will be formed of the number of micro-organisms originally introduced into the tube. I mention this point merely to show that I was alive to these dangers, and therefore carefully preserved the tubes in a vertical position, and practically ignored surface growth. The method described above readily permits of counting the colonies up to the number of sixty or one hundred, and thus the degree of sterility of the hands effected by a given cleansing process can be determined.

It is convenient to classify the results of the various methods under three heads:—

- (1) Those tubes which have less than twenty colonies, which may be considered as *practically sterile*;
- (2) Those containing more than twenty and less than eighty colonies, which may be described as being *moderately infected*;

- (3) Where more than eighty colonies appear, the tubes must be considered *severely infected*.

In classifying the tubes thus, I have this time followed the practice of Schæffer; and I agree with him in thinking it better not to distinguish between absolutely sterile and nearly so, but to group them under one head.

The mode adopted for testing the sterility of the hands is most important in any experiments relating to their disinfection. There is no method which can give us a *true* indication of the number of living micro-organisms that are to be found on the hands—nothing short of removing the skin from both hands would accomplish this. All that can be done is to gain an approximate idea of the number of bacteria that can be detached. And, as the test should as nearly as possible approach the conditions met with in a surgical operation, both hands must be treated, as they both may come in contact with the wound. We must be able to say not only whether the hands are sterile or not, but also, approximately, the number of microbes which have been removed. The use of a fluid medium, therefore, like broth, is to be deprecated, as it can give no indication of the number and variety of the micro-organisms; and similarly gelatine should not be used, as it does not admit of incubation at body-temperature. The dipping of the finger tips in fluid or solid media (Kümmel*), and the stroking of the dry hand with a platinum loop (Schleich), are examples of inadequate tests, which have led to the adoption by their advocates of erroneous, and one might also say ludicrous, conclusions. Hands which are literally teeming with micro-organisms may appear quite sterile when subjected to either of these

* *Deutsch. Med. Woch.* 1886.

tests. But certain experimentalists have gone to the opposite extreme, and have imposed conditions which have no analogy in surgery: such as forcibly to scrape off the superficial layers of the epidermis with a sharp spoon (Petruschky, Gottstein-Mikulicz, Bunge), or the excision of small portions of the skin, and the incubation of them in liquid media (Lauenstein).

Haegler, in his admirable series of experiments,* made use of short lengths of sterile ligature silk, with which he rubbed the surface of the skin and cleaned the nail-furrow. This is a very fair test, and approaches the conditions met with in surgery; but, in my own experience, I have not found it so efficient as the sterile slips of ivory or wood. It is not easy to bring the thread into contact with the whole of the hand. Moreover, it has the great disadvantage of holding fast any epithelial cells and micro-organisms which it may have removed from the skin, and thus, when the liquid agar is agitated, the microbes are not distributed throughout the medium, but grow along the silk thread, making it difficult to determine the number of organisms originally present.

Krönig and Blumberg† have made use of a totally different method. Believing the number of germs found on the surgeon's hands to be a matter of indifference, provided the *pathogenic* ones are destroyed, they resorted to experiments upon animals. They proceeded in the following way. They first inoculated the hands with a pathogenic micro-organism (*micrococcus tetragenus*). The hands were then thoroughly cleansed, and afterwards scraped, and white mice were inoculated with the scrapings. If

* *Händereinigung*. Basel, 1900.

† *Beiträge zur Handedesinfektion*. Leipzig, 1900.

the mice lived, it was accepted as a proof of the efficiency of the process of sterilisation. If, on the contrary, the mice died, and a *post mortem* examination revealed the presence of the micrococcus tetragenus, it was regarded as sufficient evidence of the inefficiency of the process.

So far as I know, Krönig and Blumberg are the only experimentalists who have adopted the inoculation of animals as a test.* Their work deserves close attention, but at the same time it must be borne in mind that experiments by inoculation of the lower animals are fraught with exceptional difficulties, and the results must, therefore, be received with considerable caution; for erroneous and paradoxical results are not infrequently obtained. Our knowledge of the whole subject of the virulence of micro-organisms and of tissue resistance is so rudimentary, that it is necessary to exercise the utmost caution in drawing conclusions even from the simplest experiments. I do not wish to disparage the work of Krönig and Blumberg in the slightest degree, but merely to utter a word of warning that, in cases where the results obtained by inoculating animals contradict those of the test-tube, it does not necessarily follow that the latter are in fault.

There is considerable difference of opinion as to the advisability of testing the hands in their normal condition (*i.e.*, not specially infected), or after they have been inoculated with a pure culture of some special micro-organism. There are advantages and disadvantages in each case. Obviously all surgeons are much more deeply interested in ascertaining whether the hand can be rendered sterile in its ordinary state than in learning whether this can be done after artificial inoculation. The former is essentially of

* Fäth u. Meissl, *Archiv. f. Gynäkol.*—Bd. 72., 1904—have recently published the results of some experiments upon similar lines.

much more practical importance than the latter. Nevertheless, the hand which has been artificially infected offers certain distinct advantages in testing the efficiency of the sterilising processes. (a) *It gives greater uniformity to the test.* The number and the variety of microbes adhering to our hands vary very greatly from day to day. (b) *It enables us to regulate the stringency of the test at will,* by the employment of a microbe which is more or less resistant. This is especially useful for comparing the relative values of two or more methods of sterilisation. (c) And lastly, *it does away with the possibility of accidental infection from microbes floating in the air.* This last point is, as we shall see, a very important one.

When, after prolonged washing, the hands have been proved to be infected, it is always open to the critic to suggest that the colonies which have appeared in the culture medium have sprung from microbes floating in the air, having settled on the damp sterilised hands, and that, therefore, the infected tube is no evidence that the method of sterilisation has been at fault. There can be no doubt that this is possible, for, while in surgery we are accustomed to ignore the possibility of air-infection, in bacteriology we have constantly to reckon with it. It is undoubtedly true that the air, even in our most aseptic operating theatres, does contain certain microbes floating in it, and, though the number of them is very small, yet it is sufficient to raise the question of "air infection."

Several recent investigators (Paul and Sarwey*) have endeavoured to obviate the possible risk by conducting the sterilisation process in a so-called "aseptic box"—a large metal case having small openings in its sides which admit

* *Münch. Med. Woch.*, No. 37, 1901.

of the hands being thrust through them. Thus the entire processes of washing and inoculating the nutrient medium are conducted inside the box, which has been previously thoroughly sterilised by steam. By some such device the risk of air-borne infection is reduced to a minimum.

Personally, I have not availed myself of the sterilised cabinet, but have employed the still more certain method of excluding air-infection—that of testing hands which have been artificially infected with a pure culture of a readily recognisable micro-organism. This is, in my opinion, the only reliable way to exclude the possibility of air-infection. For this purpose I have, for my experiments, selected four micro-organisms: *bacillus pyocyaneus*, *bacillus prodigiosus*, *sarcinæ aurum*, and the potato bacillus. The first was chosen as being a fair example of a mildly pathogenic, non-spore-bearing germ, readily recognisable, and of but moderate resistance. The *bacillus prodigiosus* was soon abandoned, as its resistance to anti-septics was found to be very much below that of the ordinary pathogenic cocci, thus rendering it unsuitable as a test object. Recently I have made frequent use of one of the yellow *sarcinæ*, and have found it a very suitable microbe, easily recognisable by its marked pigment. On being tested with regard to its power of resistance to anti-septics, it was found to be almost exactly that of the *staphylococcus aureus*. When, in order to decide between the relative merits of two or more sterilising methods, it became necessary to make use of a more stringent test, the potato bacillus was employed, it being a non-pathogenic micro-organism, which readily produces spores of a most resistant nature.

In artificial infection I proceeded in the following way. The contents of a three-days'-old broth culture of the microbe were poured into the hollow of the hands of the person about to be experimented on, and he was instructed to rub the fluid well into the hands until they were quite dry. The process of sterilisation was then gone through. It has been urged that such marked infection has no parallel in ordinary surgical work, and imposes too severe a task on any method of cleansing. In reply to this criticism, it must, of course, be granted that in practice the surgeon's hands are not brought into contact with massive pure cultures. But, on the other hand, the test by artificial inoculation is by no means either so severe or so unnatural as might at first sight appear. The difficulty of removing or destroying microbes on the skin lies far less in respect of their numbers than of their situation and surroundings. Are they embedded in fat, or albumen? Are they deep in the follicles, or only on the surface of the skin? What is their power of resistance to chemicals?

The microbes present on the hands vary greatly both as regards number and kind. For the most part they are to be regarded as of purely accidental occurrence, picked up with the dust and dirt of daily life. Provided that the skin be in its normal state, and free from inflammation, there is no reason to believe that the microbes develop upon it, as the conditions favourable to active bacterial life are absent. The number and variety of micro-organisms present upon the hands of any person must be largely dependent on the nature of his occupation. The position of the microbes on the skin is also of interest. We may expect to find them wherever fine molecular dirt can penetrate, but not deeper than that. Thus we meet with

them, not only on the surface of the cuticle, but between the cells of the superficial layers of the epidermis, and in the entrance to the sweat-glands, sebaceous ducts, and hair follicles. Haegler,* who has studied this point with great care, tells us that they do not penetrate deeply anywhere, either between the cells or in the sweat and sebaceous ducts. And he was unable to cause them to do so, even by very forcible rubbing in of pure cultures. It must be noted that the microbes are not lying free on the skin, but are imbedded in the grease and albuminous matter found there. This, as we shall see, has a very marked influence upon the difficulty with which they are removed or destroyed. So much so that, although we should, *à priori*, expect that hands which had not been especially infected would be much more easily sterilised than those which had been severely infected with pure culture, the reverse is often found to be the case. For frequently it is easier to remove the microbes, with which the hands have been artificially infected, than those which have been picked up in the ordinary day's work. This is explained by the fact that the microbes of artificial infection are unprotected by layers of fat and albumen. I say "frequently" advisedly, because the power of resistance possessed by the special microbe has to be taken into consideration. Where very resistant bacteria, such as the potato bacillus, are employed to infect the hands, the difficulty of rendering them sterile may be materially increased.

In making my experiments, I soon observed how much more easily some hands were freed from bacteria than others. And a short experience enabled me from inspection pretty accurately to predict whether few or many

* *Op. cit.*, page 23.

colonies would develop from the testing. Those which had a smooth, soft skin and short, well-kept nails were cleansed with far greater ease than those where the skin was rough or covered with coarse hairs and large sebaceous follicles. The degree of ease with which the hands are cleansed varies somewhat according to the time of the year. This is, doubtless, due to the fact that anything which causes the skin to become rough, such as an east wind, greatly increases the difficulty of removing the microbes. This is an important point and one to which we shall have occasion to refer again. Another point, to which Haegler* has drawn attention, is the difficulty, if not impossibility, of removing bacteria from apparently insignificant cuts in the cutis.

During the following experiments I tested the hands of about thirty persons, chiefly colleagues and resident surgeons. By applying the test to a number of persons I obtained, as I thought, a more accurate idea of the *practical* value of the various cleansing processes than I could have done had I confined it, as many investigators do, to one pair of hands. Those who experiment solely upon their own hands, as a rule, apparently obtain better results than those who, like myself, extend their operations to a larger number, for increased experience brings increased skill even in cleansing the hands. Hence the difficulties and probable sources of error, revealed by repeated bacteriological tests, are more likely to be avoided. Moreover, it is inconvenient to restrict the test to one person, for, after each cleansing process, an interval of some days must elapse before it can be repeated. Otherwise, the hands suffer and become rough or excematous, and if antiseptics

* *Op. cit.*, page 35.

be employed other errors may arise. And it is to be remembered that, as the hands of different persons vary greatly in their character, a method which may be suitable to one person is not necessarily so to another. The importance of this observation has not received the attention which it merits. The hands of different individuals differ markedly in their tolerance of antiseptics and other chemical agents. It is quite common to find persons who are unable to use (say) perchloride of mercury, on account of its irritating action on their skin, although they find no inconvenience attending the use of carbolic solutions, and *vice versa*.

Apart from the extreme cases where the employment of a given chemical calls forth an acute dermatitis and so prohibits its use altogether, it will generally be found that one disinfecting agent is better tolerated, that is to say, produces less roughening of the skin, than any other. *Any method which causes the hands of an individual to become rough and cracked stands at once condemned for habitual use by him, no matter how efficient the method may have proved to be on the hands of other persons.* The personal factor in the disinfection of the hands must not be overlooked.

For these reasons I have tested as large a number of persons as possible who, like myself, were engaged in surgical practice. In all cases they have been interested in the results of their work, and have earnestly endeavoured thoroughly to cleanse the hands. For their willing help I gratefully tender my sincere thanks. The experiments were, for the most part, conducted in the operating theatre at the Queen's Hospital, with the exception of those in which the hands were inoculated with pathogenic bacteria, which were carried out either in the University Bacteriological Department or in my private laboratory.

It is perhaps hardly necessary to state that every possible care was taken to prevent accidental infection. Everything that came into contact with the operator's hands was subjected to at least ten minutes' boiling. Water, soap, brushes, loofah (with the exception of the alcohol and volatile antiseptics) were boiled from ten to fifteen minutes in covered enamelled iron hand-bowls, which had themselves previously been boiled in soda solution. The ivory slips, too, were sterilised by repeated and prolonged boiling. Control experiments were always made to test the ivory slips, brushes, water, soap, etc., which in every case proved perfectly sterile.

MECHANICAL CLEANSING OF THE HANDS.

The object of my first enquiry was to learn the value of a simple mechanical cleansing of the hands. By mechanical cleansing I mean those measures by which we seek merely to remove the microbes, without any attempt being made to destroy them. Since the microbes are incorporated with the dirt and grease of the skin, and are situated not only on its surface, but also in all its folds and between the superficial cells of the epidermis, also in the openings of sweat ducts and in the sebaceous follicles, it is necessary to adopt means which have the power to penetrate the covering of fat, and to loosen and detach the superficial cells of the epidermis. The more effectually this is done, the greater is the number of micro-organisms that are removed.

The cleansing effect of washing the hands with soap and water is, of course, due to the power the soap has of liquefying and emulsifying the greasy matter of the skin. The larger the amount of free alkali contained in the soap, the greater will be its power to emulsify and remove the fat. Most of the cheap hard soaps contain an excess of free alkali, more than sufficient for this purpose. Indeed, in some cases the excess is so great as to have a decidedly caustic action on a delicate skin. Any soap which is irritating to the hands is unfit for use in our experiments, as nothing can compensate for any roughness of the skin produced by its use. This detergent action of the soap is greatly assisted by the use of very hot water, and by mechanical rubbing and scrubbing of the skin. Hot water,

in addition to its macerating power on the epithelial cells, causes an increased flow of the sweat, which helps to remove the dirt from the openings of the ducts.

SIMPLE WASHING WITH SOAP AND HOT WATER, WITH USE
OF NAIL BRUSH AND NAIL CLEANER.

In all cases I observed the following method: First, the hands received a preliminary wash with soap, under a gentle stream of hot water. After the gross dirt had thus been removed, the nails were carefully cut and cleaned with a nail cleaner. A vigorous washing and scrubbing in a hot boiled solution of common yellow soap then followed for an allotted time. Several times during the process the hot soap solution was replaced by a freshly sterilised one, and another nail brush was supplied. The temperature of the soap solution when given to the operator was 50° C.—generally a little too hot for the immersion of the hands. Both nail brush and loofah were used in order thoroughly to scrub the entire surface, and special attention was paid to the nail-fold. When the scrubbing had lasted for the proper period it was followed by a rinsing in hot sterile water. An ivory slip was then taken from boiling water by means of sterilised forceps and given to the operator with all possible precaution. He was directed to rub his hands with the roughened surfaces of it, and to pass the point under each finger-nail. The slip was then dropped into a tube of liquefied agar-agar, which was placed in a slanting position until the medium had solidified. It was then put into the incubator. Twenty such experiments were made, the time spent in washing varying from five to thirty minutes. *In every case the hands proved to be strongly infected, for each tube contained innumerable colonies.* A similar series of twenty

experiments was then made, substituting soft soap for the hard yellow soap. No improvement could be detected in the results, but the hands suffered slightly from the prolonged scrubbing, owing to the caustic alkaline properties of the soap.

These two series of experiments made it sufficiently clear that no amount of scrubbing with soap and hot water will materially cleanse the hands from bacteria.

WASHING WITH SOAP AND WATER, WITH THE USE
OF SAND OR MARBLE-DUST.

I then repeated these experiments, substituting for the nail brush and loofah fine sterile *sea-sand* (as advocated by Sanger*), or medium-sized *marble-dust* (Wittkowski†) to scrub with. They were used in the following manner:—After the preliminary wash to remove the gross dirt, a handful of sterile sand was taken up in one hand and a large lump of sterile soft soap in the other, and with the addition of a little hot water the hands were vigorously rubbed with this mixture. The rubbing was continued for the allotted time, with frequent changes of water. The soap was then washed off in sterile water, and the hands were tested in the usual manner. One might naturally suppose that a prolonged and vigorous rubbing of the hands with sand or marble-dust would unpleasantly scratch and roughen the skin, but the reverse is the case, for it is left delightfully smooth and soft. Unfortunately, when bacteriologically examined, *the hands in every instance proved to be strongly infected*, and I was unable to detect any appreciable improvement upon the former

* *Asepsis in der Gynakologie und Geburtshilfe*, Leipzig, 1893.

† *Therap. Monatschr.*, July, 1894.

method. The statement made by Sanger* and others, that by this means the hands can be rendered sterile, is quite incorrect.

WASHING WITH SCHLEICH'S SOAP.

The unsatisfactory results of my experiments with sea-sand and marble-dust did not predispose me in favour of Schleich's marble-dust soap,† but I was led by curiosity to try it. This is a soap prepared according to an exact formula, in which beeswax, stearine, marble-dust, and ammonia are added to clear yellow soap. In testing it I was careful strictly to observe all Schleich's directions for using the soap.‡ But, instead of finding (as he did) that "the hands invariably proved sterile, even after being intentionally infected with pus," *in no single case did I succeed in anything like sterilising even the normal hand.* The results, indeed, were not better than those obtained by scrubbing with common soap and hot water. The great difference between Schleich's assertion and my results may be readily explained by the difference in the mode of testing the hands. Schleich's method was to stroke the dried cleansed hand with a sterile platinum loop, and then to inoculate gelatine plates with the platinum wire. This test is most inadequate and of no practical value. The use of gelatine plates prevents the necessary incubation of the medium at the body-temperature, while the stroking with the smooth platinum loop is unlikely to detach micro-organisms from the surface of the hand, *especially if the hand be dry when tested.*

The relative difficulty with which microbes are detached from a dry skin, as compared with a wet one, is

* *Op. cit.*, page 11.

† *New Methoden der Wundheilung*, Berlin, 1899.

‡ *Op. cit.*, page 126.

a very important point. Many erroneous conclusions have been drawn from not observing it. When making the experiments described above, I was startled, and at first greatly perplexed, on finding that not only was it impossible to render the hands sterile by prolonged scrubbing, *but that apparently the more one scrubbed, the more infected did the hands become.*

Having satisfied myself that it was a practical impossibility adequately to cleanse the hands by simply washing them with soap, I was still wishful to learn its value in removing the microbes. For this purpose I tested the hands, both *before* and *after* washing, and also several of the bowls of soap solution in which they had been washed. I was surprised to find that in almost every case *the hands were apparently far less infected before than after the most prolonged washing.* That this was not really the case was obvious, because all the waters in which washing or rinsing had taken place teemed with microbes which had been removed from the hands. That their increase was not due to accidental infection from the air was soon proved by a few experiments on artificially infected hands. Whence, then, the reason of the increase in the number of microbes? Not from the cleansing agents, for they were sterile beyond doubt. It was clear that the increase merely arose from the bacteria coming off the skin more readily. The prolonged maceration in the hot water and alkaline soap had loosened and detached the surface epithelium, so that when the hands were tested the ivory slip brought away a greater amount of debris. *Before washing, the dry smooth skin does not easily give up the microbes on its surface.*

Therefore, though the mechanical removal of dirt by washing is probably an essential part of all sterilising methods, it must be remembered (1) That the number of microbes left on the hands is always very considerable; and (2) That the skin has been so altered as greatly to facilitate their detachment.

This loosening of the surface epithelium is an important fact, and one to which we shall have occasion to refer again. Haegler noticed that if, after the hands had been washed for a considerable time, they were then rubbed with a dry, rough, sterile cloth, a large quantity of this loosened skin came away in flakes, and that hands so treated showed a decided improvement, in their bacterial condition, over those where this rubbing was omitted. This assertion I can heartily endorse, with the reservation that, although there is a marked improvement, yet the hands are still far from being sterile.

Considerable discussion has taken place as to the advisability of making use of nail brushes. When it was found that the brushes in ordinary use were loaded with micro-organisms, there was a natural outcry against the danger thus incurred, and numerous substitutes were suggested, such as sand, marble-dust, loofah, and fine wood shavings. None of these is, however, so convenient as the nail brush, nor so efficient, at least so far as the hands are concerned. For all fail to cleanse the nail-fold as effectually as the nail brush does. Their chief utility lies in cleansing the skin of the patient. A general impression prevails that the skin of the hands is much more difficult to sterilise than that of other parts of the body. This is only true in so far as the nail-fold places special mechanical difficulties in the way of removing the dirt. On the other

hand, it must be borne in mind that the hands will stand prolonged and vigorous scrubbing, which would seriously abrade the skin elsewhere. It is in such cases that sand and loofah are valuable, for they may be used where the hard fibres of the nail brush could not be tolerated. In another place I shall have to mention some experiments made in cleansing the skin of other parts of the body. The results showed that, as a rule, the hands were more easily rendered sterile than other parts.

The difficulty of sterilising the nail brushes has been greatly exaggerated. They can be boiled in soda solution for five minutes or more, and thus sterilised with certainty, however strongly infected they may be. This I have repeatedly proved by experiment, and have, therefore, no hesitation in stating that the contrary assertion* is based on an error in bacteriological technique. In practice it is hardly worth while treating an infected brush, seeing that a new one can be purchased for one penny; and no reliance can be placed upon the power of antiseptic lotions to sterilise the brushes.

In giving an account of these experiments I have several times emphasised the fact that sterile hot soap and water, in which the hands were washed, was frequently renewed during the process. To have omitted to do this would have been a grave error, for each of the waters, more especially the earlier ones, teemed with microbes. Even that in which the hands were finally rinsed never failed to show the presence of numerous organisms.

* Schenk and Zaufal, *Munch Med. Woch.*, 1900.

CONCLUSIONS RESPECTING THE MECHANICAL CLEANSING
OF THE HANDS.

1. That even after the most prolonged (even to half an hour) and energetic washing of the hands in soap and water, it is not possible materially to diminish the number of microbes on them.
 2. The same conclusion holds good whether sea-sand, marble-dust, or Schleich's soap be employed.
 3. There is no advantage to be gained by unduly prolonging this washing process, as the hands *never become sterile*, and, owing to the loosening of the epidermis, generally appear more infected after than before the washing.
 4. Neither is there any advantage offered by the use of soft soap, or soap containing an excess of free alkali. Any increase in detergent action is more than counterbalanced by its irritating effect on the skin.
 5. The water should be used as hot as it can be borne, and it should be frequently renewed. After washing, the hands may, with advantage, be rubbed with a dry, rough, sterile cloth to assist in the removal of the superficial cells of the epidermis.
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THE VALUE OF TURPENTINE AND OTHER FAT-SOLVENTS.

Turpentine.

A short series of experiments was then made to ascertain the value of turpentine as a cleansing agent. These were conducted in a similar manner to those just described. The hands were well scrubbed with soap and hot water, and from time to time rubbed and brushed with pure spirits of turpentine. Bearing in mind the strong cleansing properties of turpentine, on account of its power to dissolve fats, I hoped for a considerable improvement on my former efforts. But I was unable to detect any alteration for the better, as the following table will show.

Experiments		Time allotted to washing.		RESULT.
1	..	7 minutes	..	Countless colonies of staphylococcus and others.
2	..	8	..	"
3	..	10	..	"
4	..	12	..	"
5	..	15	..	"
6	..	18	..	"

In every one of six experiments, in which the time of washing was varied from seven to eighteen minutes, *countless colonies of staphylococcus and other micro-organisms were present.*

Benzoline and Xylol.

Similar experiments were made with the substitution of benzoline or xylol for the turpentine; but the results were uniformly bad.

THE VALUE OF ANTISEPTICS IN AQUEOUS SOLUTIONS.

The wonderful improvement in the results of operations which followed the epoch-making discoveries of Pasteur and Lister was attributed almost entirely to the use of antiseptics, without regard being had to the credit due to a more universal adoption of drainage of the wound, and to the greater attention paid to cleanliness and hygienic measures generally. The extraordinary power of immediately sterilising anything with which they came in contact was supposed to belong to dilute solutions of antiseptics. For instance, momentarily to immerse the hands in a solution of carbolic acid or perchloride of mercury was thought to be enough to sterilise them beyond all shadow of doubt. This exaggerated view of the power of chemical antiseptics was largely based on certain crude test-tube experiments on pure cultures of micro-organisms. From which experiments it was stated that even dilute solutions of carbolic acid or of perchloride were capable of killing all the pyogenic bacteria within the space of a few seconds.

The testing of the power of any given antiseptic to destroy micro-organisms, whether on the hands or elsewhere, is by no means the simple matter it is often assumed to be, and, as a rule, that power is greatly overestimated. One of the main difficulties to be contended with is that a certain quantity of the antiseptic is of necessity introduced into the culture-medium. The amount, it is true, is very small; but the experiments of Koch, Geppert,* and many others have shown that the introduction of even an infinitesimal quantity of an antiseptic into

* *Berlin klin. Woch.*, 1889, No. 36 and 37; 1890, Nos. 11, 12, 13.

a nutrient medium may be sufficient to prevent the *development* of the bacteria in it. Although a relatively strong solution of an antiseptic is needed to *kill* most micro-organisms, and more especially to destroy their spores, yet an infinitesimal quantity of it, when introduced into the culture medium, may prevent or retard their growth, though it fails to kill them. For instance, Koch found that the addition of one part of perchloride of mercury in 1,600,000th of peptone broth was sufficient to hinder the development of anthrax spores and bacilli. Haegler found that an addition of one part of perchloride of mercury to 200,000 of bouillon prevented the growth of staphylococci.

It, therefore, by no means follows that a culture is really sterile because it appears to be so. The organisms may be undestroyed, or their growth may be retarded; but they may be living and ready to develop in more favourable circumstances. Before we can accurately estimate the value of an antiseptic, we must be able either entirely to remove all trace of it from the test-object, or else absolutely to neutralise its action. Unfortunately, we are not able to do either of these with any degree of certainty except in the case of some of the antiseptics. With others, such as the important phenol group, we have no better means of neutralising their action than the very imperfect one of rinsing the test-object in sterile water. The mercuric salts are the most readily neutralised, for, after treating them with a dilute solution of ammonium sulphide, an inert sulphide of mercury is formed. In the following experiments I have principally made use of the mercuric antiseptics, as being at once the most powerful of the chemical antiseptics and those which are most readily neutralised.

It has been contended by some that this power of antiseptics to prevent the growth of micro-organisms has been greatly over-estimated, and may with impunity be ignored. Lockwood found that broth, to which had been added 1 in 5,000 of sublimate, allowed of a luxuriant bacterial growth. Fürbringer also quotes a somewhat similar experiment in support of this contention.

Such statements in no way refute that of Geppert in which he maintains that a minute trace of an antiseptic in a culture medium may be sufficient to prevent or retard the development of bacteria, *especially of those which have previously been weakened by the action of an antiseptic*. And my own experiments, described below, strongly support this view. In a large number of these experiments I have tested the hands both before and after the neutralisation of the antiseptic, and, although in many cases where the hands were infected, colonies have developed on the tube where the antiseptic has not been neutralised, yet a greater number appeared and a freer growth took place in the tube where the antiseptic had been neutralised. And, moreover, in a not inconsiderable number of cases, it was only in the neutralised tube that the colonies developed at all. This cannot be explained by supposing that the growth in the neutralised tube was due to impurities in the neutralising solution. For not only was care taken to sterilise the solution, but the same phenomenon was observed when, by the appearance of the special microbe used to artificially infect the hands, all possibility of an accidental infection was excluded.

All conclusions drawn from experiments with antiseptics, in which the precaution of neutralising the action of the antiseptics has not been taken, are valueless. And

we shall not be wrong if we regard the results of all culture experiments, even where the antiseptic has been neutralised, as invariably over-estimating the bactericidal power of the chemical. Our neutralisation of the antiseptic is, at the best, but an imperfect performance; and, as I have mentioned previously, many of the colonies are so inhibited from growth by the action of the chemical as to develop only after the lapse of some weeks.

Before passing on to examine this class of experiments, in relation to the hands in detail, it may not be out of place to consider for a moment the power of destroying bacteria possessed by antiseptics in general. I was anxious to ascertain this, that I might have some idea of the likelihood of any given antiseptic rendering the hands sterile. Although there is no lack of published experiments on this question, I preferred to obtain the information first hand. The primary point which I desired to settle was the time taken by the antiseptics in common use to destroy the ordinary pus-producing cocci. To determine this I proceeded as follows: I soaked small pieces of sterile ligature silk in a forty-eight hours' broth culture, or in a watery suspension of an agar culture of the staphylococcus aureus, bacillus pyocyaneus, or other micro-organism. The pieces of silk were then removed from the culture and carefully dried in a warm chamber, precautions being taken to prevent their accidental inoculation. When the threads were dry, they were dropped into a flask containing a sterile solution of the antiseptic about to be tested; after remaining in it for a definite period, they were gently washed in sterile water to remove the antiseptic, and then placed in a small flask of broth, or tube of liquid agar, in which they were incubated for several days. When the mercuric salts were tested, the silk threads were treated with a 10 per cent.

solution of ammonium sulphide before being placed in the flask of broth in which they were incubated.

I first tested watery solutions of perchloride and of bin-iodide of mercury (1 in 1000) upon silk threads inoculated with the bacillus pyocyaneus, and with the following result:—

(1) *Perchloride of Mercury* (1 in 1000).—After five minutes' immersion in the antiseptic, the bacillus pyocyaneus developed in the broth medium, and after ten minutes, and again after fifteen minutes; while after twenty minutes' immersion in the antiseptic the broth remained sterile. This experiment was repeated with slight variations; but in no case was the bacillus killed under fifteen minutes' immersion, and sometimes not until after thirty minutes.

The experiment was repeated with staphylococcus aureus, with precisely the same results.*

The experiments were then repeated with the bin-iodide of mercury solution (1 in 1000).

(2) *Bin-Iodide of Mercury and Bacillus Pyocyaneus.*

After 5 minutes, Bacilli pyocyaneus develop.

" 10	"	"	"	"
" 15	"	"	"	"
" 20	"	Sterile.		

(3) *Bin-Iodide of Mercury and Staphylococcus Aureus.*

After 5 minutes, Staphylococci develop.

" 10	"	"	"
" 15	"	"	"

Similar attempts were then made with a watery solution of carbolic acid (1 in 40) and with one of lysol (2 per

* Opitz tested the influence of sublimate (1 in 1,000) on staphylococcus, and found that, after even half-an-hour's action by the sublimate, colonies of staphylococcus grew in the medium.

cent.). These experiments were made without taking any further means to remove the carbolic acid or the lysol from the silk threads than that of freely rinsing them in sterile water. This method was very imperfect, and left the threads smelling distinctly of the antiseptic. The results were as follows:—

(4) *Carbolic Lotion* (1 in 40) *with Bacillus Pyocyaneus*.

After 5 minutes, *Bacilli pyocyaneus* develop.

"	10	"	"	"	"
"	15	"	"	"	"

(5) *Carbolic Lotion* (1 in 40) *with Staphylococcus Aureus*.

After 5 minutes, *Staphylococci* develop.

"	10	"	Sterile.
"	15	"	"

(6) Similar experiments with lysol (2 per cent.) gave practically the same result as the other antiseptics, viz.: that within a period of ten minutes neither was the *staphylococcus aureus*, nor the *bacillus pyocyaneus*, with any certainty, destroyed.

Thinking that the uniformly bad results possibly arose from the microbe being in some way protected by the fibre of the silk from the action of the antiseptic, I adopted a method suggested by Epstein,* and frayed out the silk threads before inoculating them. On another occasion I gave up the use of silk threads in favour of large glass beads. These, after being sterilised, were immersed for a short time in a watery suspension of an agar growth of the microbe which was about to be tested, and then slowly dried in a warm chamber. They thus became coated with a fine film of micro-organisms. Neither of these modifications materially affected the results of the experiments,

* *Zeitschr. f. Hyg.*, Bd. 24, 1897.

although a slightly greater proportion of the culture tubes remained sterile—a fact which, it seemed to me, was due rather to the detachment of the microbes from the test object than to the greater exposure of the germs to the action of the antiseptic.

Here it may be well to sound a note of warning lest anyone should, by chance, be tempted to draw deductions as to the relative powers of the antiseptics used in the experiments just related.

Although the same conditions, as far as possible, prevailed in each of these experiments, certain discrepancies of necessity arose, and it soon became apparent that the slightest variation had far-reaching effects. No two different cultures of a micro-organism seemed to have quite the same susceptibility to an antiseptic. The age of the growth, the medium on which it had been grown, the temperature at which it had been kept, all influenced its powers of resistance, as also did the thickness of the film deposited on the test objects, and the temperature at which the test objects were dried. These apparently small points so greatly affected the results that I regard it as useless to compare one set of experiments with another, unless they are conducted under precisely similar conditions.

The experiments, however, show clearly, I think, that neither the staphylococcus aureus nor the bacillus pyocyaneus (common microbes of suppuration) is readily affected by the antiseptics in general use, and that there can be no certainty of destroying them within a reasonable time, unless the antiseptic exceeds the strength at which it can be tolerated by the skin. But it must be noted that the microbes in these experiments were purposely so placed as to be fully exposed to the action of the antiseptic; but

this is a condition not fulfilled in the case of microbes on the hands. For in the latter case, as we have seen, the bacteria are mixed with the dirt and debris, and so protected from the action of any antiseptic by the albuminous and fatty covering. It scarcely requires any experiments to demonstrate the fact that microbes, when embedded in albumen or fatty matter, are far less influenced by watery antiseptic solutions than when freed from such protection.

As a matter of fact, I found, as Haegler and others have done, that if the infected test objects were dipped into oil, or other fatty matter, before being exposed to the antiseptic, they might remain in the chemical for hours, even days, together, without the slightest effect following. Having seen how slight was the influence which these antiseptics had on the micro-organisms in these test-tube experiments, I did not anticipate any great improvement in the results when testing them on the hands. I, however, proceeded as follows:—

EXPERIMENTS UPON THE NORMAL HANDS.

(1) *Perchloride of Mercury*.—After the preliminary washing and trimming of the nails, as in the previous experiments, the hands were vigorously scrubbed with soap and hot water for several minutes; then the soap was thoroughly rinsed off in several waters. The hands were then rubbed and scrubbed with a watery solution of perchloride of mercury, again rinsed in sterile water, and tested by means of two ivory slips. One of these slips was immediately dropped into an agar tube, the other was first neutralised with a 10 per cent. solution of ammonium sulphide.*

* In my earlier experiments I neutralised the mercury by immersing the hands in the sulphide of ammonium solution. This proved a better method, but, as it stained the hands an inky black, which resisted all efforts to wash it off, I found it unpleasant to use myself, and utterly failed to induce my colleagues to try it a second time!

Time allotted to washing with soap and water.		Time allotted to antiseptic (1-1000).		RESULT.
5 minutes	..	3 minutes	..	Strongly infected.
8 "	..	4 "	..	Moderately "
				(about 50 colonies.)
10 "	..	5 "	..	Strongly infected.
10 "	..	6 "	..	" "
10 "	..	8 "	..	" "

(Hands very rough and sore after the washing.)

(2) *Bin-iodide of mercury* was then substituted for the perchloride of mercury; otherwise the test remained the same.

Time allotted to washing.		Time allotted to Bin-iodide (1-1,000).		RESULT.
5 minutes	..	5 minutes	..	All strongly infected.
8 "	..	5 "	..	" "
10 "	..	6 "	..	" "
10 "	..	6 "	..	" "

(3) I then repeated the experiment, using a *watery solution of carbolic acid* (1-20) in place of the mercury. Instead of being neutralised with sulphide of ammonium, the hands were soaked in sterile water for four minutes, to remove as much of the carbolic as possible.

Time allotted to washing.		Time allotted to Carbolic (1-20).		RESULT.
5 minutes	..	5 minutes	..	Strongly infected.
10 "	..	5 "	..	Moderately "
15 "	..	5 "	..	" "

(4) *Lysol* (2 per cent.) was then tested in a similar manner.

Time allotted to washing.		Time allotted to Antiseptic		RESULT.
5 minutes	..	5 minutes	..	Strongly infected.
10 "	..	6 "	..	" "
10 "	..	8 "	..	Moderately "
				(60 colonies.)
5 "	..	6 "	..	About 50 colonies.

As this antiseptic is largely employed as a sterilising agent for the hands, I paid more attention to it than to some of the others, and repeated the above experiments upon hands which had been artificially infected with *sarcinæ* and other microbes. The results were uniformly bad, and left no doubt that this antiseptic, like those already mentioned, is practically useless, for the purpose of sterilising the hands, when used in the way just described.

It was surprising to hear at the recent discussion on "Asepsis and Antisepsis" (Brit. Med. Asso., Oxford, 1904) how much reliance is still placed on the power of aqueous solutions of antiseptics to disinfect the hands, notwithstanding the numerous experiments which demonstrate their inefficiency. True, few of the speakers save Sir W. Macewen gave any proof of the efficiency of the antiseptics except the inadmissible reference to "excellent clinical results."

The statement of Sir W. Macewen that numerous bacteriological tests on his own and his assistants' hands had shown that prolonged washing with simple soap and hot water was sufficient in the majority of cases to sterilise them, and that after two minutes immersion in a 1-20 solution of carbolic acid, the hands were invariably found to be sterile, is directly contrary to the experience of almost all recent investigators (Paul and Sarwey, Haegler, Schaeffer, &c.). Until details are given of the method adopted in testing the hands in Sir W. Macewen's clinic, to say where the discrepancy lies is impossible.

THE VALUE OF PERMANGANATE OF POTASH.

I next turned my attention to *Permanganate of Potash*, which was, I think, first advocated, as an agent for sterilising the hands, by Kelly.* This antiseptic has been used in various ways, but the following method is suggested by Kelly, and is the one largely resorted to in this country and in America.

After the hands have been well washed with soap and water, they are soaked in a hot saturated solution of permanganate of potash. This has the effect of staining the skin a deep mahogany brown. They are then rubbed with a hot saturated solution of oxalic acid, which quickly removes the brown stain. The method is agreeable to use, for, though the strong oxalic acid solution produces a prickling sensation, especially on the skin of the forearm, yet the process neither roughens nor injures the skin in any way, and may be repeatedly used on the same day. It also possesses the not immaterial virtue of enabling the surgeon readily to control the disinfection of his assistant's hands. According to Kelly, who tested the method bacteriologically, the hands may easily be sterilised by this process, and he judged the method to be far superior to all others in use, not even excepting the spirit-sublimate method of Fürbringer.†

In testing the potassium permanganate method, the experiments were made on hands artificially infected with

* *American Journal of Obstetrics and Diseases of Women*, 1894.

† Although Kelly obtained "innumerable colonies," when testing the hands after Fürbringer's method, his results with his own method were remarkably good, viz.: Out of 50 experiments, 44 proved sterile; the remaining six yielded respectively 80, 20, 10, 9, 5 and 4 colonies.

the yellow *sarcinae*. This was done because I was desirous of comparing this procedure with others, such as Fürbringer's, which I knew to be of considerable value.

After a vigorous scrub with soap and frequently renewed hot sterile water, the hands were immersed and scrubbed in a freshly prepared hot saturated solution of permanganate of potash. After five minutes of this treatment, the hands, being now stained a deep brown, were transferred to a hot saturated solution of oxalic acid, with which they were scrubbed for another five minutes. The effect of this was to remove the brown stain, and to leave the hands a natural pink colour. After the oxalic acid had been removed by freely rinsing in hot sterile water, the hands were tested in the usual manner.

Subsequent laboratory experiments, to be described later, caused me slightly to modify this procedure. I found that the hands absorbed and retained a considerable quantity of the oxalic acid, notwithstanding the rinsing in the hot water. And unless the acid were neutralised, a small quantity was introduced into the medium by the test object, which adversely affected the development of any bacteria in the tube. To obviate this possible error, sterilised lime-water was substituted for the hot water, with a view to converting the free oxalic acid on the hands into an insoluble and inert oxalate of lime.

EXPERIMENTS UPON HANDS ARTIFICIALLY INFECTED WITH SARCINÆ.

(1) *With Permanganate of Potash and Oxalic Acid.*

No. of Experiment.	Time allotted to washing with soap, &c.	Hot saturated solution of potassium permanganate.	Hot saturated solution of oxalic acid.	RESULT.
1 ..	5 minutes	5 minutes	5 minutes	Very strongly infected with sarcinæ and others.
2 ..	5 "	5 "	5 "	Six small colonies of sarcinæ.
3 ..	5 "	5 "	5 "	Very strongly infected with sarcinæ and others.
4 ..	5 "	5 "	5 "	Sterile.
5 ..	10 "	6 "	5 "	Over 100 colonies.
6 ..	7 "	5 "	5 "	16 colonies.
7 ..	5 "	5 "	5 "	100 small colonies of sarcinæ and others.
8 ..	5 "	5 "	5 "	Two small colonies.

Thus out of eight experiments no less than half the number were very strongly infected. I was, accordingly, compelled to regard this method as quite inadequate, and as greatly inferior (as will be seen later) to that of Fürbringer.

It is not possible in this paper for me to attempt to show in each case, in which the results of my experiments differ markedly from those of other investigators, how this difference has arisen. I have already indicated, in my remarks on testing the hands in general, how many of the discrepancies may be accounted for.

In the case of Kelly's experiments, it is to be noticed that he largely made use of gelatine roll tubes, thereby excluding the requisite incubation at body-temperature;

and, secondly, although he was careful, in Fürbringer's experiment, to neutralise the sublimate, he took no steps to do the same for the oxalic acid. Lest it be thought that the discrepancy might be due to the fact that artificially infected hands were made use of by me, I may say that these experiments were repeated on those which were non-infected, and were equally unsatisfactory.

On theoretical grounds it would, indeed, be surprising if the use of permanganate of potash, as advocated by Kelly, proved successful in sterilising the hands. It stands far below the mercuric salts as a bactericidal agent. It can claim no special power of penetrating the epithelium, nor is the chemical action, which causes the decolourisation of the permanganate by the oxalic acid, one likely to have any marked influence in the sterilisation. To estimate its power as a destroyer of bacteria, I made the following test-tube experiments :—

Small portions of medium-sized, sterilised ligature silk were soaked for half an hour in fresh broth culture of staphylococcus aureus. These threads were then dried and immersed in a hot saturated solution of permanganate of potash. After a stated time they were removed from the permanganate and placed in a hot saturated solution of oxalic acid, until they were completely decolourised. They were then neutralised in sterile lime-water, washed in sterile water, and incubated in broth. The result was as follows :

Experiments with permanganate of potash and oxalic acid upon silk threads infected with staphylococcus aureus.

After 5 minutes' immersion, considerable growth.

"	10	"	"	"	"
"	15	"	"	"	"
"	20	"	"	"	"

"SUBLAMIN" (MERCURIC-SULPHATE-ETHYLENEDIAMINE).

The next antiseptic tested was one which has been recently introduced and strongly advocated by Krönig and Blumberg* as being far superior to all other agents in the treatment of the surgeon's hands. This antiseptic is made by the well-known firm of manufacturing chemists, E. Schering, of Berlin, and has appeared in the market under the name of "Sublamin." It is a preparation containing mercuric sulphate and ethylenediamine in chemical combination. The presence of the ethylenediamine is said entirely to do away with the irritating action to the skin, and to secure a greater penetrative power for the metallic disinfectant, for by its means the coagulation of the albuminous substances of the skin, usually produced by metallic agents, is prevented. Schering prepares two very similar combinations of mercury—one the mercuric-sulphate-ethylenediamine, and the other the mercuric-citrate-ethylenediamine. They are both, according to Blumberg, equal in disinfecting power, and there seems little to choose between them, save that the sulphate can more easily be prepared in the tablet form. That known by the trade name of "Sublamin" is the sulphate combination, and it is the one with which I made my experiments.

Krönig and Blumberg formed their high opinion of the disinfecting properties of mercuric-citrate-ethylenediamine from the results of a series of carefully conducted experiments upon white mice. Their method of procedure was briefly as follows. The hands of the experimentalist

* *Beiträge zur Händedesinfektion*, Leipzig, 1900; and again *Münchener Medicinischer Wochenschrift*, 1900, No. 29 and 30.

were artificially infected with a fresh culture of the micrococcus tetragenus (a micro-organism which is pathogenic to mice). They were then scrubbed with soap and hot water for five minutes, and afterwards brushed with solution (1 to 3 in 1000) of mercuric-citrate-ethylenediamine. The antiseptic was then removed by rinsing in water, and a rubbing with sterile bouillon and marble-dust followed. A small quantity of the marble-dust was then removed from the hands and inoculated into a mouse.

Similar experiments were made substituting perchloride of mercury (1 in 1000) for the citrate. The result of the experiment was that when the citrate had been used the mice did not die of tetragenus infection, but a number of them did when perchloride of mercury had taken its place. Unfortunately, the value of the experiments was very greatly impaired by the fact that the mice were seriously affected by the mercury, which was unavoidably introduced with the marble-dust, and a large proportion of them, about one half, died of mercury-poisoning.

Fueth* repeated these experiments upon guinea-pigs, contrasting the "sublamin" with spirit-soap. The animals which had been inoculated with scrapings from hands disinfected with spirit-soap died of tetragenus infection, but those which had been treated with "sublamin" showed no sign of tetragenus poisoning. Unfortunately, the experiments were very few in number; only four animals were employed in testing each antiseptic, and one of the four which received the scrapings from the "sublamin" hands did not recover from the narcosis.

I proceeded to test the efficacy of "sublamin" as follows:—After the hands had been artificially infected

* *Centralblatt für Gynäkologie*, September, 1902.

with sarcinæ, they were scrubbed with soap and hot water for a minimum of ten minutes (the nail brush, soap, and water being frequently renewed), then they were rubbed and brushed with a hot solution of "sublamin" (2 to 3 in 1000), rinsed in hot water, and tested in the usual way. The results were as follows:—

Experiments with "Sublamin" on hands infected with Sarcinæ.

No. of Experiment.	Time allotted to washing with soap and water.	Time allotted to scrubbing with sublamin.	RESULT.
1 ..	10 minutes	5 minutes	More than 100 colonies of sarcinæ and others.
2 ..	10 "	5 "	" " "
3 ..	10 "	5 "	Marked infection, sarcinæ and others.
4 ..	10 "	5 "	More than 100 colonies.
5 ..	10 "	5 "	Almost sterile.
6 ..	10 "	5 "	25 large colonies of sarcinæ.
7 ..	10 "	5 "	Marked infection.
8 ..	10 "	5 "	100 small colonies of sarcinæ and others.
9 ..	10 "	8 "	Marked infection with sarcinæ.
10 ..	10 "	8 "	" "

These exceedingly bad results made me curious to learn what bactericidal power this new antiseptic possessed when placed in the most favourable conditions. I therefore made the following experiments:—

Medium-sized silk threads were infected with a fresh broth culture of staphylococcus aureus, sarcinæ, and with bacillus pyocyaneus respectively. The threads were then dried and immersed in a warm solution of "sublamin"

(3 in 1000). Afterwards the mercury was neutralised in a (10 per cent.) solution of sulphide of ammonia; the threads were then washed in sterile water and cultivated in broth.

Staphylococcus aureus with "Sublamin" (3 in 1000)

Time allotted.			RESULT.			
5 minutes	..		Broth tube infected with staphylococcus.			
10	"	..	"	"	"	"
15	"	..	"	"	"	"

Sarcinæ with "Sublamin" (3 in 1000).

Time allotted.			RESULT.			
5 minutes	..		Broth tube infected with sarcinæ.			
10	"	..	"	"	"	"
15	"	..	"	"	"	"

Pyocyaneus bacillus with "Sublamin" (3 in 1000).

Time allotted.			RESULT.			
5 minutes	..		Broth tube infected with pyocyaneus.			
8	"	..	"	"	"	"
10	"	..	"	"	"	"
12	"	..	"	"	"	"
15	"	..	"	"	"	"
20	"	..	"	"	"	"
25	"	..	"	"	"	"
34	"	..	"	"	"	"

It must not be inferred from these experiments that the pyocyaneus proved more resistant to the antiseptic than the staphylococcus or sarcinæ. The explanation lies in the fact that a greater number of tests were made with the pyocyaneus bacillus.

These experiments were repeated with slight variations, but the result was the same. The bactericidal power of this antiseptic, judging from the few experiments made, is considerably below that of perchloride of mercury; and, as a sterilising agent when applied to the hands, it is of

little value, and cannot compare in efficiency with the alcohol-sublimate method of Fürbringer, which it was introduced to supplant.

It is, however, to be noted that Krönig and Blumberg lay less stress upon its germ-destroying property than upon its power to prevent the development of the bacteria within the body-tissues.

"If we can so impregnate the skin with an antiseptic that, on inoculation of susceptible animals with portions of the skin, the antiseptic continues to exercise an inhibitory action sufficient to prevent the development of the bacteria, we have attained adequate sterilisation.

"As we attach especial importance to this inhibitory action, we do not need to select the metallic salts which in pure solutions are the strongest disinfectants, but we may try others, which have perhaps the property of penetrating the skin more deeply, as well as possessing other practical advantages."*

Krönig and Blumberg and Fueth therefore claim as the result of their experiments upon animals that, whatever be the sterilising power of "sublamin," it does prevent the development of the bacteria within the body tissues.

It is, of course, quite impossible, as the result of experiments of an entirely different character, to deny this assertion, much as I may doubt it. Every bacteriologist knows, only too well, the difficulties and the liability to error which surround animal experiments, and, until more numerous experiments less open to objection have been made, I should hesitate to accept the results of a few made upon animals, when they directly clash with those made on culture-media, and I do not think the contention of Krönig and Blumberg can be sustained.

* *Beiträge zur Händedesinfektion*, S 28, Leipzig, 1900.

It would certainly be remarkable if the small quantity of antiseptic which is introduced with the test object should be sufficient to prevent the development of bacteria within living tissues, when it is insufficient to prevent their development within the culture-tube.

Since the above was written, Schæffer has published (*Monatsschr f. Geburtsh. u. Gyn.*, March, 1904) an account of several experiments upon animals, which prove that "sublamin" by no means possesses the power of preventing the development of bacteria in the body which Krönig, Blumberg, and others have claimed for it. In this respect, as in its bactericidal properties, "Sublamin" stands far below both alcohol and perchloride of mercury.

ANTISEPTIC SOAPS.

A natural desire to simplify and shorten the process of hand-disinfection has led to many suggestions that the antiseptic should either be combined with the soap or used at the same time. It is stated that in doing so the efficiency of many antiseptics is increased, through a gain in penetrating power. It is, however, equally open to question whether the combination will not hinder the action of one another—the antiseptic impairing the cleansing properties of the soap, and the soap at the same time reducing the chemical activity of the antiseptic. It is obvious, for instance, that the combination of an antiseptic like perchloride of mercury with soap would at once neutralise the disinfecting power of the sublimate by converting it into inert calomel.

I contented myself with examining two of the most promising of the antiseptic soaps, viz., bin-iodide of mercury soap, as suggested by McClintock, and one containing lysol. They were tested both as regards their power of disinfecting the hands and their action upon infected threads and glass beads. A strong solution of yellow soap was made with boiling water, and a sufficient quantity of bin-iodide of mercury was then added to bring it to a strength of 1 in 1000. A similar soap solution was made, to which lysol was added to the amount of 2 per cent. The hands were scrubbed with these soaps for five, ten, and fifteen minutes, with frequent changes of bowls, brushes, and soap solution. After rinsing in water, and, in the case of the bin-iodide, neutralising with ammonium sulphide, the hands were tested in the usual manner. The result showed no improvement on the previous test of these antiseptics, and in the case of the infected silk and beads it went to show that the efficiency of the antiseptic was diminished, not increased, by the admixture of soap.

ALCOHOLIC DISINFECTION.

To Professor Fürbringer belongs the credit of first drawing attention to the value of alcohol, as an aid to the disinfection of the skin. As a result of his experiments he found that, unless the fatty secretion of the skin was first removed, it was useless to attempt to destroy the germs by means of antiseptics, as the micro-organisms, protected by a thick layer of fat and epithelial débris, never came into actual contact with them. He did not regard alcohol in any way as an antiseptic in itself, but used it merely as a solvent of fatty matter in order that the antiseptic might act directly on the organisms.

Landsberg,* as a result of his experiments, concluded that the apparent sterility of the hands treated with alcohol was not a true one, the micro-organisms not being destroyed, but merely rendered more difficult of removal through the hardening of the epidermis.

Some few years later Reinicke† followed up Fürbringer's investigations, and aroused considerable interest in the whole question of sterilisation of the skin. As the result of numerous experiments he affirmed that, not only was alcohol essential to any method employed for sterilising the hands, but that it alone was practically sufficient to ensure the complete removal of all micro-organisms. In proof of this he quoted a large number of experiments made upon his own hands, after they had been artificially infected with a very resistant spore-bearing bacillus—the

* *Deutsch. Med. Woch.*, 1888 and 1889.

† *Centralblatt f. Gynäk.* Bd. 2, 1894.

potato bacillus. Reinicke found that, when his hands were thus infected, the ordinary methods of disinfection (soap and water alone, or followed by the use of sublimate, carbolic acid, chlorine water, or lysol) were quite ineffective, and that even Fürbringer's method failed. But he affirmed that he was able readily to succeed by the use of alcohol alone. His best results were obtained after washing the hands with soap and water for five minutes, then scrubbing them from three to five minutes with 90 per cent. alcohol. He also affirmed that by rubbing his hands with alcohol alone, without any previous washing, he could within five minutes render them quite sterile. This assertion naturally attracted considerable attention, and was quickly followed by other papers on the same subject. Krönig,* from a few experiments made on the skin of a fresh cadaver into which anthrax spores had been rubbed, came to the conclusion that the skin could not be thoroughly disinfected by means of alcohol. His experiments, however, were so few and so open to objection that little reliance can be placed upon them.

These papers were shortly succeeded by a series of articles by Ahlfeld, who soon began to be regarded as the special champion of the hot-water-alcohol method. Of sixty experiments made by him, in which the normal hands were rubbed from one to three minutes with 96 per cent. alcohol, only four proved infected. In a second paper on this subject, Ahlfeld gives further details of his experiments with alcohol. Not only does he claim that by its use can the normal hand be certainly sterilised, but also hands that have been directly infected with stinking pus or other foetid discharges. He advises that the hands

* *Centralblatt f. Gynäkology*, 1894. No. 52.

should be scrubbed for five minutes with hot soap and water, and then rubbed with a flannel dipped in 96 per cent. alcohol for another five minutes. In a third paper he narrates experiments which would seem to show that alcohol is not merely a mechanical means of removing dirt, but is actually destructive of organisms. With unabated zeal Ahlfeld has continued year after year to test the alcohol-disinfected hands of his assistants, students, and midwifery pupils, and has published statistics of hundreds of such experiments. He confidently asserts that, not only are the results of this method of disinfection far superior to those of any other, but also that by its means we are able actually to sterilise the hands with ease and certainty.

In 1896 I published an account of a series of experiments, which I had made with a view to testing the assertions of Reinicke and Ahlfeld. I found that although alcohol was a most valuable, if not the most valuable, agent we possess in the cleansing of the hands, yet it did not accomplish all or nearly all that Reinicke, Ahlfeld, and others claimed for it. Of twelve experiments made upon the normal hands, where strong spirit was used for from three to five minutes, after a prolonged washing in hot water, half the number of cases proved to be severely infected. And when the hands had been intentionally infected with pus or pure cultures of microbes, the results were worse.

This disparaging view of the power of alcohol readily and effectually to sterilise the hands called forth many protests on the part of its supporters, and further experiments were quoted to show how efficient the method was; and my failure to obtain like results was attributed to accidental infection or failure in technique.

Reinicke, Ahlfeld, Fürbringer, and Poten, all stoutly defend the power of spirit to sterilise the hands. But there have been several important articles published recently which show that the disinfection by alcohol is open to considerable question (*see* Krönig, Gottstein, Blumberg, Paul and Sarwey, Haegler, Baumm, Bunge, and Schaeffer).

I was therefore glad of an opportunity to test the alcohol method again, at greater length, and in the light of greater experience.

EXPERIMENTS WITH ALCOHOL UPON THE HANDS.

These were carried out in a similar manner to those already described. The hands were first washed in several waters to remove the gross dirt, then scrubbed with sterile hot water and soap, and in the middle of the process a sterile nail cleaner was used. The hands were rinsed in sterile water to remove the soap, and then rubbed with the alcohol. In these experiments I used either absolute alcohol or methylated spirit, that is, 96 per cent. This would, however, be slightly diluted by the moisture on the hands from the previous rinsing. In addition to a nail brush, I have also used a piece of sterilised loofah or lint with the spirit. After using the spirit, the hands were soaked in warm, sterile, normal saline solution, in order to remove as much of the alcohol as possible, and to imitate, to some extent, the maceration by the body fluids of the epithelium of the surgeon's hands during an operation.

(1) *Experiments with Spirit upon Normal Hands.*

No. of Experiment.	Time spent with soap and water.				Time spent with spirit.	RESULT.
1 ..	5 minutes		Spirit Meth.		3 minutes	Numerous colonies.
2 ..	5 "		Abs. Alc.		5 "	" "
3 ..	8 "		" "		4 "	Several hundred small white colonies.
4 ..	5 "		" "		5 "	Countless white colonies.
5 ..	7 "		" "		4 "	Marked infection.
6 ..	12 "		Spirit Meth.		4 "	One colony.
7 ..	8 "		" "		4 "	Countless colonies.
8 ..	6 "		" "		3 "	Ten colonies.
9 ..	9 "		" "		4 "	Numerous small colonies.
10 ..	5 "		Abs. Alc.		5 "	Sterile.
11 ..	5 "		" "		5 "	Three colonies.
12 ..	5 "		" "		5 "	Sterile.
13 ..	5 "		" "		5 "	Thirty colonies of albus and aureus.
14 ..	5 "		Spirit Meth.		4 "	Many colonies.
15 ..	6 "		" "		5 "	Sterile.
16 ..	5 "		" "		4 "	One colony.
17 ..	5 "		" "		5 "	Sterile.
18 ..	5 "		" "		5 "	Six colonies : staphylococci.
19 ..	5 "		" "		5 "	Many colonies of staphylococci.
20 ..	5 "		" "		5 "	Sterile.
21 ..	5 "		" "		3 "	"
22 ..	5 "		" "		5 "	"
23 ..	5 "		" "		5 "	"
24 ..	3 "		" "		5 "	One colony.
25 ..	3 "		" "		4 "	Sterile.

Thus out of twenty-five experiments, in sixteen cases (64 per cent.) the hands proved sterile or nearly so (under twenty colonies). In four cases they were moderately infected (more than twenty and less than eighty colonies), and in five cases badly infected (more than eighty colonies).

(2) *Experiments with Spirit upon Hands Infected with Bacillus Pyocyaneus.*

No. of Experiment.		Time spent with soap and water.		Time spent with spirit.	RESULT.
1	...	5 minutes	Spirit Meth.	3 minutes	Over a hundred distinct small colonies, bac. : pyocyaneus.
2	...	10 "	" "	3 "	Countless distinct colonies, many pyocyaneus-
3	...	5 "	Abs. Alc.	5 "	Numerous colonies, some pyocyaneus.
4	...	15 "	" "	4 "	Countless colonies, some pyocyaneus.
5	...	3 "	Spirit Meth.	5 "	Countless colonies.
6	...	7 "	" "	5 "	Numerous small colonies
7	...	8 "	" "	5 "	Countless colonies.
8	...	5 "	Abs. Alc.	5 "	One hundred colonies of pyocyaneus.
9	...	8 "	Spirit Meth.	8 "	Sterile.
10	...	5 "	Abs. Alc.	5 "	Two colonies.
11	...	3 "	Spirit Meth.	5 "	Countless colonies of pyocyaneus.
12	...	5 "	" "	5 "	Sterile.
13	...	5 "	" "	5 "	"
14	...	5 "	" "	5 "	Slight infection.
15	...	5 "	" "	5 "	Sterile.

Thus out of fifteen experiments, six were sterile or nearly so, and nine were badly infected.

(3) *Experiments with Spirit upon Hands Infected with Sarcinæ.*

No. of Experiment.		Time spent with soap and water.		Time spent with spirit.	RESULT.
1	...	5 minutes	Spirit 80%	3 minutes	Six small colonies.
2	...	5 "	" "	3 "	Slight infection.
3	...	3 "	Spirit Meth.	3 "	" "
4	...	5 "	" "	3 "	Thirty colonies.
5	...	8 "	" "	3 "	Four colonies.
6	...	7 "	" "	4 "	Marked infection.
7	...	7 "	" "	4 "	" "
8	...	5 "	" "	4 "	Twenty-five colonies, sarcinæ and others.

Thus in eight experiments, four were sterile or nearly so, two were moderately infected, and two markedly so.

(4) *Experiments with Spirit upon Hands Infected with the Potato Bacillus.*

No. of Experiment.	Time spent with soap and water.		Time spent with spirit.		RESULT.
1 ...	5 minutes	Spirit Meth.	5 minutes	Dense growth.	
2 ...	5 "	" "	5 "	Seven colonies.	
3 ...	5 "	Abs. Alc.	5 "	Numerous colonies.	
4 ...	8 "	" "	5 "	Several colonies.	
5 ...	5 "	Spirit Meth.	5 "	Countless colonies.	
6 ...	5 "	" "	5 "	" "	
7 ...	5 "	" "	5 "	" "	
8 ...	5 "	" "	5 "	" "	
9 ...	6 "	" "	5 "	Many colonies.	
10 ...	5 "	" "	5 "	Countless colonies.	
11 ...	5 "	" "	5 "	Twelve colonies.	

Thus of eleven experiments, two were sterile or nearly so, one was moderately infected, and eight showed marked infection.

These statistics show a considerable improvement upon those which I published some years ago. Though much of that improvement is more apparent than real, for formerly I included, under the term "infected," cases where even only one colony developed, instead of regarding, as I have done in these statistics, all tubes in which less than twenty colonies developed as "sterile."

The power which spirit possesses for cleansing the hands is certainly remarkable, far surpassing that of all the other methods so far tested. Although in a not inconsiderable proportion of the cases just stated it did not render the hands sterile, yet it never failed, even when used for a very brief period, to materially diminish the number of microbes. The longer the spirit was used, the better

was the result obtained. The same could not be said of the preliminary washing with soap and water. It did not seem to make any appreciable difference whether five or fifteen minutes were spent on this part of the process.

There is, however, a limit to the time during which the spirit can be used. For if it be prolonged beyond five minutes, the hands suffer severely. They become rough and unpleasant to the touch, and even eczematous. It is impossible to render such roughened hands sterile.

I have already commented upon the desirability, nay, the necessity, of the hands being kept in a good cosmetic condition. All methods of disinfection tend to roughen and impair the surface of the skin to a greater or less degree, but by the exercise of care and the free use of glycerine, lanoline, and other emollients, the hands can and ought to be kept in an exemplary state.

In order to obtain the complete benefit of the spirit method, it is necessary to employ the alcohol for the full five minutes, and, as Ahlfeld has rightly insisted, the whole procedure must be carried out with vigour and intelligence. It was interesting to notice the difference in the ease or difficulty with which the hands of different persons were sterilised: and this quite apart from the energy and intelligence with which the cleansing was performed. Several of the experimentalists seemed absolutely unable to render their hands anything approaching a state of sterility. Others did so with ease and almost certainty. Energy, determination, and experience, though they counted for much, did not influence the result so much as the character of the hands and nails. Hairy hands with coarse skin, broken or ill-kept nails, greatly increase the difficulty of sterilisation.

It must be owned that my figures (64 per cent. of sterility) are somewhat discouraging, and fall far short of the claims put forward by Ahlfeld for this process. Had I restricted my test to one or two pairs of selected hands, as has been the custom of most experimentalists, I have no doubt that I should have obtained a considerable improvement in my figures. But I was anxious to learn for my own guidance, not so much the utmost possibilities of this method as the frequency with which I might expect my assistants (house-surgeons and nurses) to succeed in rendering their hands sterile.

From my experiments I am forced to conclude that, though the spirit be employed for not less than five minutes, and the hands be in an exemplary cosmetic condition, yet a perfect sterility cannot be obtained, and, in a considerable proportion of cases, the hands are still surgically infective. When they have been artificially infected either with pure culture of a resistant microbe, or with purulent fluids, the likelihood of their being rendered sterile is greatly diminished.

As an example of the curious blunders that can be made in this work, we may note that Reinicke,* although working with the extraordinarily resistant spores of the potato bacillus, was able readily to sterilise his hands, after a short application of the alcohol. Nevertheless, he admits that an immersion of three-quarters of an hour in alcohol was insufficient to kill the spores of the same bacillus.

* *Centralblatt f. Gynäkology.* Bd. 2, 1894.

SPIRIT SOAP.

The desire to shorten the hot-water-alcohol disinfection method of Ahlfeld led Mikulicz* to try the effect of combining the soap with the alcohol. And he was surprised to find that, by using spirit soap (*Pharmacopœa Germanica*) in place of Ahlfeld's method, he not only was able materially to shorten the time spent in cleansing the hands, but he also obtained better results. Hanel† agrees with Mikulicz, that sterilisation can easily be obtained by spirit soap.

The advantages claimed for this method are a considerable shortening of the process—a preliminary wash with soap and water being unnecessary—and the saving of the skin from the irritation of the plain alcohol. The method, moreover, is so simple and convenient that it has been widely adopted. To estimate its advantages I tested it side by side with the hot-water-alcohol method. The hands of two experimentalists were inoculated with a fresh culture of *sarcinæ*, which was rubbed on till the hands were dry. One of these persons then cleansed his hands with spirit soap, the other by means of the hot-water-alcohol method. At the conclusion of the cleansing, the hands of both were rinsed in sterile water and tested in the usual way. The spirit soap was made according to the German *pharmacopœia*, and contained 50 per cent. of alcohol. The hands were well rubbed with it for a period of from eight to ten minutes. From time to time during the process fresh portions of the spirit soap, with sterile brushes and lint, were supplied. The second experimentalist, after

* *Deutsche Med. Wochenschr.*, 1899. No. 24.

† *Beiträge zur klinischen Chirurgie*, 1900. Bd. 26.

washing with soap and water for five minutes, used methylated spirit for four minutes. The result of the test showed that the spirit soap is *far inferior* to the hot-water-alcohol method, for, out of eight experiments made with the spirit soap, *all the cases proved to be strongly infected with sarcinæ*; whilst with the hot-water-alcohol method five were sterile, one moderately infected, and two markedly so. The experiments were repeated with other preparations of spirit soap, but with very similar results. *In every case the hot-water-alcohol method proved to be vastly superior to the spirit soap.*

I cannot, therefore, agree with Paul and Sarwey* in their statement that, although spirit soap does not render the hands sterile, it is at least as efficacious as the hot-water-alcohol method. And I agree with Haegler and Schaeffer in their conclusions as to the inferiority of spirit soap.

Before I was aware of the disadvantage of combining the spirit with soap, it had occurred to me that an antiseptic might with advantage be added to the spirit soap. For this purpose I selected bin-iodide of mercury, and added it to the amount of 1 in 1000 to the spirit soap. With this antiseptic soap I made six experiments upon sarcinæ-infected hands, with the very unsatisfactory result that all save two proved to be severely infected.

* *Münch. Med. Woch.*, 1900. No. 28.

FÜRBRINGER'S METHOD.

(Hot Water, Alcohol, and Sublimate.)

It has already been noted that the introduction of alcohol, as an aid to the disinfection of the hands, springs from Fürbringer's early experiments, published in 1888. Attributing the poor results which he obtained, after the use of watery antiseptics, to the fact that these antiseptics were unable, on account of the investment of grease, to come into direct contact with the micro-organisms of the skin, he found that, if this fatty covering were first removed by washing with alcohol, the results showed marked improvement. So much so, indeed, that he felt justified in stating that, were the hands treated on the lines laid down by him, they could be sterilised with almost absolute certainty, no matter how strongly they might have been infected. The following is his method:—

1. The nails are carefully pared and cleaned.
2. The hands scrubbed for one minute with soap and hot water.
3. The hands scrubbed for one minute in spirit (not under 80 per cent.).
4. Before the spirit has evaporated, washed for one minute in a solution (2 in 1000) of perchloride of mercury.

On the value of Fürbringer's method (as in the case of the hot-water and alcohol procedure of Ahlfeld), a keen controversy has raged for many years. Landsberg (1888), Preindlsberger (1891), Henke (1893), found Fürbringer's method unsatisfactory. Reinicke (1894) also, after treating his hands according to Fürbringer's method, had very unsatisfactory results, numerous colonies developing in

every case. His results were better if the time spent in using the alcohol was extended from three to five minutes, *and better still if the washing with the chemical anti-septic was entirely omitted*—a curious statement, confirmed by both Ahlfeld (1895) and Mikulicz (1898).

In 1896 I compared Fürbringer's method with several others, and, although it proved superior, the results were far from satisfactory, *e.g.*, out of eleven experiments upon normal hands, twice they proved sterile, four times almost so, and five times badly infected.

Paul and Sarwey (1901), from a few, but very exact, experiments, obtained "extraordinarily unfavourable results."* Krönig and Blumberg also report unfavourably. They found that, after the hands had been inoculated with tetragenus microbes, and then sterilised according to Fürbringer's method, mice died when inoculated with scrapings from them.

On the other hand, numerous reports testified to the efficiency of the method. Amongst them we must specially mention the experiments published by Haegler (1900), who, after extending Fürbringer's method to five minutes for each act, out of eighty-four experiments on the normal hands had a sterile result of 81 per cent., and out of fifty-six experiments on his own hands a sterility of 89 per cent. Danielsohn and Hess† report results better even than the above.

My tests were made both on normal hands and hands artificially infected. After prolonged scrubbing with soap and water, the hands were well rinsed in hot sterile water, and whilst still wet vigorously brushed with the spirit. In these experiments I principally used methylated spirit (96

* *Münch. Med. Wochenschr.*, 1901. Page 1453.

† *Deutsche Med. Wochenschr.*, No. 37. 1902.

per cent.), the moisture on the hands and the nail brush, however, slightly diluting this. After using the spirit, the hands were brushed with a hot sterilised solution of the antiseptic—generally perchloride of mercury—and, before testing them, were well rinsed in sterilised warm water, and the sublimate neutralised in the usual manner.

In the few cases in which I used carbolic lotion, I had to content myself with freely rinsing my hands in several waters.

Experiments with Fürbringer's Method upon Normal Hands.

No. of Experiment.	Soap and Water.	Spirit.	Time.	Antiseptic.	RESULT.
1 ...	5 min.	Abs. Alc.	5 min.	Sublimate 1—5000	One hundred colonies Staphylococcus and Pyocyaneus.
2 ...	5 "	" "	4 "	Sublimate 1—2000	Six small colonies.
3 ...	7 "	" "	5 "	Sublimate 1—3000 5 min.	Sixty small colonies.
4 ...	12 "	Sp. Meth.	4 "	Sublimate 1—2000 3 min.	Numerous colonies of Staphylococcus and Pyocyaneus.
5 ...	8 "	" "	4 "	Sublimate 1—2000 3 min.	Numerous colonies.
6 ...	7 "	Alc. 2 E.	3 "	Lot : carbol 1—20 3 min.	Sterile.
7 ...	6 "	Sp. Meth.	3 "	Sublimate 1—2000 3 min.	Sterile.
8 ...	9 "	" "	4 "	Sublimate 1—1000 2 min.	One colony.
9 ...	5 "	Abs. Alc.	5 "	Sublimate 1—1000 1 min.	Sterile.
10 ...	5 "	" "	5 "	Sublimate 1—1000 3 min.	Sterile.
11 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	Four small colonies.
12 ...	5 "	Sp. Meth.	4 "	Sublimate 1—2000 3 min.	Sterile.
13 ...	5 "	" "	5 "	Sublimate 1—1000 3 min.	Sterile.
14 ...	5 "	" "	5 "	Sublimate 1—1000 3 min.	Sterile.
15 ...	5 "	" "	5 "	Sublimate 1—1000 3 min.	Sterile.
16 ...	5 "	" "	5 "	Lot : carbol 1—20 3 min.	Sterile.
17 ...	5 "	" "	3 "	Sublimate 1—1000 2 min.	Sterile.
18 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	Sterile.
19 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	Sterile.

Thus out of nineteen experiments upon the normal hands, fifteen proved sterile or nearly so, one moderately infected, and three severely infected.

*Experiments upon Hands Infected with Bacillus
Pyocyaneus.*

No. of Experi- ment.	Soap and Water.	Spirit.	Time.	Antiseptic.	RESULT.
1 ...	10 min.	Sp. Meth.	5 min.	Lot Ac. Carbol 1—20 5 min.	Numerous colonies of Bacillus Pyocyaneus and others.
2 ...	10 "	Abs. Alc.	3 "	Sublimate 1—2000 3 min.	Numerous colonies of Pyocyaneus and Staphylococcus.
3 ...	5 "	" "	5 "	Sublimate 1—2000 5 min.	Thirty small colonies.
4 ...	15 "	" "	4 "	Sublimate 1—2000 3 min.	Numerous colonies of Bacillus Pyocyaneus and others.
5 ...	7 "	" "	4 "	Sublimate 1—2000 5 min.	Ten small colonies.
6 ...	5 "	" "	3 "	Sublimate 1—2000 3 min.	Nine small colonies.
7 ...	8 "	" "	3 "	Sublimate 1—2000 3 min.	Sterile.
8 ...	7 "	Sp. Meth.	5 "	Sublimate 1—2000 3 min.	Four small colonies.
9 ...	8 "	" "	5 "	Sublimate 1—1000 3 min.	Sterile.
10 ...	8 "	" "	8 "	Sublimate 1—1000 3 min.	Sterile.
11 ...	5 "	Sp. Abs.	9 "	Sublimate 1—1000 2 min.	Sterile.
12 ...	3 "	Sp. Meth.	5 "	Sublimate 1—1000 2 min.	Ten small colonies Pyocyaneus.
13 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	Sterile.
14 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	Sterile.
15 ...	5 "	" "	5 "	Sublimate 1—2000 2 min.	Moderately infected.
16 ...	5 "	" "	4 "	Sublimate 1—2000 1 min.	Sterile.

Thus out of sixteen experiments upon hands artificially infected with bacillus pyocyaneus, eleven proved sterile or nearly so, two moderately infected, and three severely infected.

Experiments upon Hands Infected with Sarcinæ.

No. of Experiment.	Soap and Water.	Spirit.	Time.	Antiseptic.	RESULT.
1 ...	5 min.	Sp. Meth. 80%	3 min.	Sublimate 1—2000 3 min.	Slight infection.
2 ...	5 "	" "	3 "	Sublimate 1—2000 3 min.	Thirty small colonies of Sarcinæ.
3 ...	5 "	" "	3 "	Sublimate 1—2000 3 min.	Two small colonies
4 ...	5 "	" "	3 "	Sublimate 1—2000 2 min.	Sterile.
5 ...	8 "	" "	4 "	Sublimate 1—2000 3 min.	Thirty small colonies.
6 ...	7 "	" "	4 "	Sublimate 1—2000 3 min.	Sixteen small colonies.
7 ...	7 "	" "	4 "	Sublimate 1—2000 3 min.	About thirty colonies, many Sarcinæ
8 ...	5 "	" "	4 "	Sublimate 1—2000 3 min.	About fifty small colonies, many Sarcinæ.

Thus out of eight experiments with sarcinæ, three proved sterile or nearly so, and five moderately infected.

Experiments upon Hands Infected with Potato Bacillus.

No. of Experiment.	Soap and Water.	Spirit.	Time.	Antiseptic.	RESULT.
1 ...	5 min.	Sp. Meth.	6 min.	Sublimate 1—1000 2 min.	Sterile.
2 ...	5 "	Abs. Alc.	5 "	Sublimate 1—1000 2 min.	Numerous colonies.
3 ...	8 "	" "	5 "	Lot. carbol 4% 2 min.	Several large colonies
4 ...	5 "	Sp. Meth.	5 "	Sublimate 2—1000 2 min.	Marked infection.
5 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	" "
6 ...	3 "	" "	5 "	Sublimate 1—1000 2 min.	" "
7 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	Numerous colonies.
8 ...	5 "	" "	5 "	Sublimate 1—1000 2 min.	" "
9 ...	6 "	" "	5 "	Sublimate 1—1000 2 min.	Marked infection.
10 ...	5 "	" "	5 "	Sublimate 1—2000 2 min.	Slightly infected.
11 ...	5 "	" "	5 "	Sublimate 1—2000 3 min.	Moderate infection.

Thus out of eleven experiments with potato bacillus, one proved sterile, two moderately infected, and eight severely infected.

As regards the normal hands, these results are very similar to those obtained by Haegler, when testing a number of them, and represent, I think, very fairly the degree of sterilisation that can be hoped for under ordinary circumstances. If the experiments were confined to one pair of suitable hands, with the time given to the spirit extended, I have no doubt that a slightly greater degree of sterility could be obtained; though I regard Haegler's 89 per cent. sterility, as do also Paul and Sarwey, as representing practically the utmost of which the method, under the most favourable circumstances, is capable.

The experiments with artificially-infected hands prove that the poor results obtained with the normal hands were not due to micro-organisms in the air settling on the hands during the long testing process. They further show, I think, that the numerous statements, to the effect that the hands, *no matter how strongly infected*, can readily be sterilised by this method, are based on a grave error, the difficulty of sterilising the hands increasing in direct ratio to the resistance of the micro-organisms.

It is not surprising to find that it is practically impossible to sterilise hands which have been directly infected with a spore-bearing microbe of such great resistance as the potato bacillus.

Thinking that the test of scraping under each finger nail with a finely serrated piece of ivory was, perhaps, too severe for practical conclusions, I modified the experiment by simply rubbing the slip briskly over the skin of the hands, avoiding the nails altogether. As it has been

affirmed that the skin of the hands is sterilised with comparative ease, all the difficulty lying in the nail-folds, I anticipated very different results from those I had previously obtained. In this I was disappointed, for, after the skin had been directly infected, it was exceedingly difficult to free it from microbes; indeed, I did not succeed in a single instance in doing so after infection with the potato bacillus.

Experiments on the Sterilisation of the Skin.

I.

No. of Experiment.	Time Spent in Washing.	Remarks.	RESULT.
1 ...	5 minutes	Infected with potato bacillus.	Numerous colonies of potato bacillus.
2 ...	5 "	" "	" "
3 ...	5 "	" "	Ten large colonies.
4 ...	5 "	Infected with bacillus pyocyaneus.	A few small colonies of bacillus pyocyaneus.
5 ...	5 "	" "	Numerous colonies of bacillus pyocyaneus.

II.

No. of Experiment.	Time Spent in Washing.	Time Spent with Spirit.	Remarks.	RESULT.
1 ...	5 min.	5 min.	Infected with potato bacillus.	Numerous colonies.
2 ...	5 "	4 "	" "	" "
3 ...	5 "	5 "	Infected with bacillus pyocyaneus and potato bacillus.	Numerous colonies of bacillus pyocyaneus and potato bacillus.
4 ...	5 "	5 "	Infected with bacillus pyocyaneus.	Sterile.
5 ...	5 "	5 "	" "	Numerous colonies.

III.

No. of Experiment.	Time Spent in Washing.	Time Spent with Spirit.	Time Spent in Sublimate (1—1000).	Remarks.	RESULT.
1 ...	5 min.	4 min.	2 min.	Infected with bacillus pyocyaneus	One small colony.
2 ...	5 "	5 "	2 "	" "	Sterile.
3 ...	5 "	5 "	2 "	Infected with potato bacillus.	Numerous colonies.
4 ...	5 "	5 "	2 "	Infected with potato bacillus and bacillus pyocyaneus.	Numerous colonies of potato bacillus, but none of bacillus pyocyaneus.

A comparison of the relative difficulty of sterilising the skin of the hands (the nails excepted) and the skin of other parts of the body shows that the skin of the hands is the more readily sterilised. This, probably, is owing to the fact that it allows much greater force to be used in the mechanical cleansing.

Although the results of Fürbringer's method leave much to be desired, none other has in my experience been equally successful; the only one approaching it at all, in efficiency, being that of Ahlfeld. Judging, however, from my statistics, and still more from the appearance of the culture tubes, Fürbringer's method appears decidedly superior to the hot-water and alcohol process; for in almost every case where the hands were tested after the use of the alcohol, and again after the antiseptic, the former proved less infected than the latter. It is possible that this superiority is due in some measure to imperfect neutralisation of the antiseptic.

I therefore found no support for the curious statement made by Reinicke, and confirmed by Ahlfeld and Mikulicz,

to the effect that hands treated with sublimate after the alcohol disinfection proved to be *less* frequently sterile than when treated with alcohol alone.*

Haegler tries to explain this paradoxical result by assuming that the washing in the sublimate removes the alcohol, which in this way lessens its hardening effect upon the epidermis, thus allowing the microbes to be more readily removed. My experiments, however, lead to the conclusion that this strange result must have been due to an error in technique—possibly an accidental infection during the neutralisation by the sulphide of ammonia.

In spite of the superiority of Fürbringer's method over that of Ahlfeld, two facts must not be lost sight of. One is that the antiseptic is incapable of destroying many of the pathogenic microbes, within the short time it is used, even under the most favourable conditions; and the other is that our neutralisation of the antiseptic is at best but a crude and imperfect one; so that a minute amount of the antiseptic may still be introduced into the culture tube, sufficient to inhibit the development of some of the bacteria.

It is certain that the two first stages of Fürbringer's method are the most important, but, on the other hand, we shall probably be wrong if we conclude that the sublimate solution plays no part in the sterilisation of the hand, be it only a subordinate one.

* Simple alcohol disinfection gave according to Mikulicz 59—78% sterility, and a similar treatment plus sublimate only 47%.

THE PART PLAYED BY ALCOHOL IN THE CLEANSING AND DISINFECTION OF HANDS.

That alcohol possesses extraordinary power of affecting the micro-organisms of the hands is agreed upon by almost every experimenter; but at the same time there is a marked divergence of opinion as to the manner in which it acts. It was regarded at first as essentially a *mechanical cleanser*, its property of dissolving fat enabling it at once to remove the grease and dirt from the skin, and so allowing the subsequent antiseptic to come into direct contact with the microbes. Although alcohol does possess, to a certain degree, this property of mechanical cleansing, its value (even when absolute alcohol is used) as a fat-removing body has been greatly exaggerated, it really having very slight power in that direction; and it is not to be compared with such substances as ether, benzoline, and xylol. Yet these excellent fat-removing fluids are almost useless as agents for the sterilisation of the skin (see *Braatz. Münsch. Med. Wochenschr.*, No. 29, 1900). Nor is alcohol very efficient simply as a cleanser; for, as Haegler points out, if the hands be rubbed with a watery suspension of Indian ink till dry, it is almost impossible by means of alcohol to remove the ink grains, though by simple scrubbing with soap and water this may easily be effected. We must therefore look to some other explanation of its value.

There has been much discussion of recent years as to whether alcohol possesses definite bactericidal powers or not. According to Koch's classical experiments upon anthrax spores, it has little or no such property; for, even after four months' immersion in absolute alcohol, the spores

were not killed. These experiments, however, have since been repeated and modified in many ways by Ahlfeld, Epstein, Minervini, and others, and they show that evidently alcohol possesses a greater germicidal power than Koch was led to conclude. There is now a general consensus of opinion that *absolute* alcohol has little or no bactericidal power, but that dilute spirits do possess a certain power, varying with the degree of dilution. Epstein* found that the most powerful proportion was 50 per cent. In 80 per cent. alcohol, neither was the bacillus pyocyaneus, prodigiosus, nor staphylococcus killed after ten minutes' immersion; though in 50 per cent. they were all either destroyed or their growth was hindered. Minervini† agrees that absolute alcohol has very slight bactericidal power, the staphylococcus aureus not being destroyed within three days. His best results were obtained with 70 per cent. alcohol, but even then none of the microbes experimented upon were killed within ten minutes. Haegler got his best results with 65 per cent. alcohol, and found that this strength was capable of killing the staphylococcus aureus in two minutes, though after five minutes in 96 per cent. the coccus was still living.‡

I also made a few experiments on this point, as I was wishful to learn the resistance of my cultures of sarcinæ, pyocyaneus, etc., to alcohol; accordingly I proceeded to repeat the experiments which I had made with the antiseptics, using silk threads and glass beads. My best results were obtained with a 70 per cent. spirit, when the non-spore-bearing organisms were generally destroyed

* Zeitschr. f. Hygiene. Bd. 24, 1897.

† Zeitschr. f. Hygiene. Bd. 29, 1898.

‡ See also Sazwedel and Elsner, *Berlin klin. Woch.*, No. 23, 1900, and Schaeffer, page 65.

within five minutes, sometimes within two minutes. This would seem to show that alcohol of 70 per cent. has a greater germicidal power than solution of sublimate (1—1000), bin-iodide of mercury (1—1000), "sublamin" (3—1000), or than a saturated solution of permanganate of potash. In order to determine the accuracy of this, I made another long series of experiments, in which I tested the comparative sterilising powers of alcohol, perchloride of mercury, and bin-iodide of mercury. My mode of procedure was as follows :—A sloped agar tube was inoculated with the test micro-organisms, and incubated for two or three days. The luxuriant growth on the surface of the agar was then removed and suspended in a small quantity of sterile water. In this watery suspension of the micro-organism, small portions of sterile silk thread or glass beads were immersed, then removed, and carefully dried in a warm chamber. These infected test objects were then dropped into small flasks containing 70 per cent. alcohol, perchloride of mercury (1—1000), and bin-iodide of mercury (1—1000) respectively. After a definite time the test-object was removed from the antiseptic and dropped into a second flask containing plain sterile water, or 10 per cent. ammonium sulphide, according to the antiseptic from which the test-object had been removed. After the lapse of half an hour the test-object was placed in liquefied agar and incubated for several days. The results of these experiments showed that dilute alcohol has a much greater bactericidal power than it has been usually credited with. In all the experiments—and they were very numerous—in which the silk threads or glass beads were inoculated with non-spore-bearing micro-organisms, 70 per cent. alcohol proved to be much more powerful than a watery solution (1—1000) of either perchloride or bin-iodide of

mercury. *The infected threads which were immersed in 70 per cent. alcohol were almost invariably sterilised within five minutes; whilst those placed in the mercury solutions were never sterilised within the half-hour.* The micro-organisms tested in this way were the staphylococcus aureus, bacillus pyocyaneus, and sarcinæ.

Similar experiments were then made with test-objects infected with the spores of the anthrax bacillus. Here the results were very different. The alcohol seemed to be powerless to affect these resistant spores, for even after the lapse of a week (the longest period tested), the spores were uninjured. The mercury solutions in this case proved to be the stronger sterilising agent, for all the spores were generally killed within twenty-four hours, though in no instance within six hours.

When I remarked that with a 70 per cent. alcohol I found the staphylococcus and other non-spore-bearing micro-organisms to be destroyed within five minutes, it was with the reservation that this occurred when the test-objects were coated with the thinnest possible layer of microbes. But when, for instance, the silk threads were soaked for twenty-four hours in the broth culture, or the glass beads (previously coated with an emulsion of micro-organisms) were dried in the incubator for some hours, or when by any other means the bacteria were made less accessible or more resistant, then the time taken by the spirit to sterilise the test-objects was always greatly prolonged. Therefore, whilst it must be admitted that dilute alcohol possesses a distinct bactericidal power, and, provided it come into *direct contact* with the microbes, is able to destroy the pyogenic cocci within a few minutes, yet it has not the

power of destroying the micro-organisms as they are situated on our hands with any certainty within a reasonable time.

The efficiency of the alcohol in the sterilisation of the hand does not therefore depend primarily upon its bactericidal powers. The best proof of the truth of this statement lies in the fact that, if the spirit in which the hands have been cleansed be filtered, living micro-organisms may generally be cultivated from the sediment, even where the tested hands have proved sterile.

Schaeffer draws attention to the power which alcohol possesses of being able to *detach the surface epithelium which has been previously macerated and loosened by the energetic washing with soap and hot water*; and it is to this property that he attributes the chief worth of the alcohol. In support of this he quotes the following experiment:—After a vigorous wash with soap and hot water for ten minutes, the hands were carefully rinsed in flowing hot water, and then brushed for another five minutes in alcohol. The spirit was then poured into a tall conical glass, and allowed to stand for some hours. At the bottom of the vessel a white flocculent sediment formed, over 1 cm. in height, which on microscopic examination was seen to consist of a number of scales of epithelium and fat drops. The alcohol was then filtered, and the deposit dried and weighed, when it scaled 0.2 gr. The experiment was then repeated, but, instead of a prolonged scrub in hot water, the hands were simply washed for a short time in cold water, with the result that the sediment of fat and epithelium was scarcely a third of the weight of the deposit in the former test.

Krönig* early drew attention to the *power which alcohol*, especially in the more concentrated forms, *possesses of hardening and fixing the epithelial cells so that they adhere more firmly to the skin*. The good results in Reinicke's experiments he attributed to this property, and denounced the sterilisation by alcohol as an "apparent sterilisation," the microbes being neither removed nor destroyed, but merely rendered less easily detached.

Numerous writers on the subject confirm this power of alcohol to harden and contract the skin, for, as Haegler remarks, if the hand, after it has been treated with alcohol, be examined under a magnifying glass, it will be noticed that the effect of the spirit has been to contract the skin, causing the natural folds and furrows to become narrower, deeper, and more numerous; whilst the openings of the sweat-ducts have retracted. The effect of this is to render it more difficult to rub off the bacteria, seeing that these places where the microbes lurk—the folds, furrows, and accidental cuts—are rendered less accessible by reason of this shrinkage of the skin.

This astringent action has been the cause of much confusion in estimating the real value of alcohol in the disinfection of the hands; for if they are tested, as is often the case, immediately after treatment with alcohol, a very high percentage of sterility will be obtained, notwithstanding previous infection with resistant microbes, the dry, hard, spirit-polished skin not readily giving up either its epithelial scales or its microbes. This is obviously an insufficient test; for what we want to ascertain is, whether the hands, when wet and macerated with the warm,

* *Centralblatt f. Gynäkol.*, 1894. No. 52.

albuminous fluids of the body—the conditions met with in surgical practice—are sterile or at least will not prove infective.

In my own experiments I have sought to imitate the macerating action of the blood and serous fluids, by soaking and gently rubbing the two hands together in either a warm saline solution or albuminous fluid, from five to ten minutes, afterwards testing them when still wet. Whilst soaking in the warm water, the alcohol absorbed by the skin is slowly given up, and the harsh, grating sensation felt after treatment with the spirit, when rubbing the hands together, gradually disappears. Haegler points out that when the hands are soaked in an albuminous fluid rather than in plain water, the spirit is more rapidly given up.

Now, whilst it is unfair to test the hands direct from the alcohol, it is equally so to extend the macerating action of the subsequent saline, or albuminous, solution to an inordinate length, *i.e.*, beyond the conditions met with in surgery.

As the result of my experiments I find that, when the hands are soaked in either solution, the hardening effect of the alcohol upon the epithelium is too rapidly lost to warrant the surgeon relying upon its action, save for short operations. Not only did the hands in a relatively large proportion of cases prove infected (see experiments with alcohol), but the fluid in which they were soaked also contained numerous living microbes—an experience in accordance with that of Haegler,* and Paul and Sarwey.†

* Haegler proved that out of experiments in which he treated the hands with spirit, 82% were sterile (soap and hot water 5 minutes, spirit 5 minutes, rinsed in sterile water for 5 minutes). But he found also that, if he

Alcohol possesses yet another property, which materially enhances its value. Partly by reason of its aptitude to dissolve fats, still more by its water-extracting power, it admirably *prepares the way for the subsequent use of watery antiseptics*. Skin which has been first treated with spirit not only allows the antiseptic solution to come more directly into contact with its surfaces, and to penetrate more deeply into its ducts, but the cells themselves take up the mercury more readily. This fact is forcibly impressed upon every experimentalist who uses sulphide of ammonia with which to neutralise the sublimate on his hands.

If, after a thorough washing with soap and water, the hands be bathed in strong sublimate solution, a considerable amount of the mercury is taken up by the epithelial cells, forming an insoluble compound. Then, if, after well rinsing, the hands be now plunged into a solution of sulphide ammonia, the skin and nails will be faintly stained a blackish brown, due to the formation of sulphide of mercury. Should they have been treated with alcohol between the washing with soap and the immersion in the sublimate, the discolouration will be much more marked, for, instead of having a slightly dusky hue, they will appear almost inky black, the effect of the alcohol having been greatly to increase the absorption of the mercury. The dark stain resists all attempts to remove it

soaked the hands in warm sterile serum for 10 minutes, not only was the surface of the skin always infected, but there were large numbers of microbes in the serum.

† Paul and Sarwey (*Münch Med. Wochenschr.*, December, 1899) find that the hot-water-alcohol method so affects the hands that they give up relatively few microbes when tested with the wooden slip, but that after soaking them in water it is always possible to remove a larger or smaller number of bacteria.

by washing, and persists for several days, until, in fact, the surface epithelium has been thrown off. The tenacity with which the skin retains the sublimate is remarkable, for the hands will show this discolouration, when treated with sulphide of ammonia, days, even weeks, after having been in contact with the mercuric salts. It would be an error, however, to suppose that the mercury penetrates to any great depth of the skin, as it does not extend beyond the superficial layers of the epidermis. It is easy by rubbing with pumice stone to remove the discolouration produced by the ammonia sulphide, leaving the pink skin beneath, which is not stained by further application of the sulphide.

Haegler states that not only does alcohol materially assist the penetration of the sublimate, but it also helps the mercury to penetrate the bacterium; and that if threads inoculated with a resistant micro-organism—such as anthrax spores—be treated with alcohol prior to the sublimate solution, a decided increase in the bactericidal power of the mercury ensues. My own experiments do not confirm this statement. The power which alcohol possesses in such a marked degree, of facilitating the penetration of the mercury, is not dependent upon its power of dissolving fat. For ether, which is a far better solvent, has not this property, as is readily shown by the ether-treated hand becoming much less discoloured by the ammonium sulphide than that treated with alcohol. Several experiments were made, substituting ether for alcohol in the methods of Ahlfeld and Fürbringer; the results were not nearly so good as when alcohol was used. This is doubtless due to the fact that ether does not mix well with water; consequently, to obtain the full benefit

from its use, the hands before the application must be free from water, and again after its use, and also before the watery antiseptic is applied. This I attempted to do as follows:—

Soap and Water	...	...	4 minutes.
Spirit	...	...	2 "
Ether	...	...	1 "
Spirit	...	...	2 "
Sublimate (1—1000)	...	...	2 "

I was, however, unable to detect an improvement on Fürbringer's method in the results of this one. It has occurred to many that Fürbringer's method might be considerably shortened, and possibly rendered more efficacious, by the simple expedient of combining the sublimate with the alcohol. But, unfortunately, it has been demonstrated by Koch, and confirmed by Epstein,* Minervini, and many others, that *strong* alcoholic solutions of chemical antiseptics act far less powerfully than simple aqueous ones; indeed, solutions of perchloride of mercury, or of carbolic acid in absolute alcohol, are practically devoid of any bactericidal action. Apparently the strong spirit, by rapidly hardening the surface of the cells, prevents the entrance of the antiseptics.

I was, however, wishful to learn whether the addition of a *small* percentage of alcohol to a watery solution of perchloride of mercury increased or diminished its germicidal power. On this point I made a prolonged series of experiments. These were conducted on the lines laid down on page 67. Test-objects were infected

*Epstein found that antiseptics in a 50% alcoholic solution were more powerful than if dissolved either in absolute alcohol or in simple water; and Haegler noticed that the addition of a small quantity of alcohol (10—20%) increased the germicidal power of an aqueous antiseptic solution.

with non-spore-bearing microbes—staphylococcus, bac. pyocyaneus, or sarcinæ—and, after being dried, were dropped into small flasks containing perchloride of mercury to the strength of 1-1000, in absolute, or 30, 40, 50, 60, 70, 80, 90 per cent. alcohol. After the test-objects had been removed from the antiseptic solution, they were treated with ammonium sulphide in the usual manner. A very few experiments showed that when the sublimate was dissolved in absolute alcohol it had practically no antiseptic value. *It was also found that, with the exception of absolute and very high percentages of alcohol, the spirituous solutions of the sublimate were markedly superior to the watery solutions in sterilising power.* The efficiency increased in direct ratio to the percentage of alcohol in the solution up to about 70 per cent., when any further increase in the proportion of the alcohol caused a reduction in the sterilising power of the antiseptic. It required many experiments to determine the exact percentage of alcohol necessary to obtain the best results, but finally it seemed that when the sublimate was dissolved in 70 per cent. alcohol its sterilising power was at its greatest. The following table shows the result of one series of many experiments made to determine this point:—

EXPERIMENTS WITH THREADS INFECTED WITH THE STAPHYLOCOCCUS.

1-1000 PERCHLORIDE OF MERCURY.

No. of minutes of immersion.	Aqueous Solution.	30% Alcohol.	40% Alcohol.	50% Alcohol.	60% Alcohol.	70% Alcohol.	Absolute Alcohol.
1 min. ..	Infected.	Infected.	Infected.	Infected.	Infected.	Infected.	Infected.
2 " ..	"	"	"	"	"	"	"
3 " ..	"	"	"	Slight growth.	Slight growth.	Sterile.	"
10 " ..	"	"	"	"	Sterile.	"	"
15 " ..	"	"	Sterile.	Sterile.	"	"	"
20 " ..	"	Sterile.	"	"	"	"	"
25 " ..	"	"	"	"	"	"	"
30 " ..	"	"	"	"	"	"	"

As 70 per cent. of alcohol proved most powerful when the plain alcohols were tested (see page 67), it now became necessary to determine whether the addition of the perchloride of mercury increased the sterilising power of the alcohol at all. It was found that when 70 per cent. simple alcohol was tested side by side with 70 per cent. sublimate-alcohol, with test-objects infected with staphylococcus, bac. pyocyaneus, or sarcinæ, no decided superiority of one over the other could be detected. They both sterilised the threads and beads in from two to five minutes; but, on the other hand, when they were tested with anthrax spores, the sublimate-alcohol proved the more powerful. In this case the simple spirit (70 per cent.) seemed almost powerless to affect the resistant spores, for even after a week's immersion they readily developed. When, however, the 70 per cent. spirit contained sublimate to the strength of 1-1000, the spores ceased to grow after from twelve to twenty-four hours' treatment.

The sterilisation of the hands by Fürbringer's method we have seen to be comprised of three different parts:—

- (1) The removal of the microbes by the mechanical cleansing power of the soap and the alcohol;
- (2) The hardening action of the alcohol on the epidermis, so that the micro-organisms are less easily detached; and
- (3) The destruction of the organisms by the germicidal action of the spirit and of the sublimate.

We have reason to believe that the second is the most important, and that the third is the least so.

Now although, as we have seen, the scrubbing with the sublimate solution plays a part in the sterilisation of the hand (its omission giving rise to inferior results), it is

obvious that the use of it lessens the hardening effects of the alcohol; the watery antiseptic solution extracting the spirit, and once more softening and loosening the surface of the epithelium. We are, so to speak, building up with one hand and pulling down with the other. If, therefore, we can replace the aqueous solution of sublimate with a still more efficient alcoholic one, we shall avoid this macerating action, and may look for a material improvement in our results. With a view to testing this, I compared the results of Fürbringer's method with those of the following modification of it:—

Soap and Water	...	...	...	...	...	5 min.
Methylated Spirit	...	...	...	...	...	2 "
Weak alcoholic solution of perchloride of mercury	2	"				

The results of these tests showed that the substitution of an alcoholic antiseptic for the watery one in Fürbringer's method was followed by a slight improvement in the efficiency of the process. But the above statement does not, I think, adequately indicate the advantage of substituting an alcoholic antiseptic for an aqueous one. The chief advantage lies, as I have already mentioned, in that the hardening action of the plain alcohol upon the skin has not been reduced by a subsequent soaking and scrubbing in the watery antiseptic. If, after the use of the sublimate-alcohol, the hands are rubbed dry with sterilised gauze or towel, the surface of the dried skin becomes hard and polished, so that the epithelial scales are detached with difficulty. This being so, one was naturally led to consider whether it is necessary, or advisable, to use both plain alcohol and sublimate-alcohol in the cleansing process. Would not the method be equally efficient if simply the 70 per cent. sublimate-alcohol were used after the preliminary wash in hot water? Though such a modification

simplifies the process, and gives equally satisfactory results, it has certain disadvantages. Sublimate-alcohol has a more powerful action on the skin than the plain spirit, and if its use be prolonged for more than two or three minutes, the hands may suffer; and that amount of time is hardly sufficient to obtain the maximum results by the cleansing process. It is rather an advantage than otherwise that the cleansing method adopted requires the use of two forms of alcohol. It prevents the undesirable habit of attempting to cleanse the hands in one bowl of fluid.

Before I was aware of the value of the astringent action of the alcohol on the epidermis, I endeavoured to get better results by adding a small percentage of glycerine to the spirit and also to the aqueous antiseptics, hoping thereby to obtain greater power of penetration for these agents, and consequently an increased efficiency. My expectations were not realised, the results being worse, not better. This I now attribute to the fact that the glycerine would lessen the hardening action of the alcohol, so that the epidermis would become more easily detached.

ON THE DURATION OF THE STERILITY OF A DISINFECTED HAND.

Springing from the question as to whether it is possible, or not, to sterilise the hands, is another and scarcely less important question, viz.: How long does the sterility, which has been obtained with so much trouble, last? Can we rely upon the hands which have once been sterilised remaining so for a reasonable period, say for the length of an operation; or do they, after a relatively short time, again become infective?

Attention was first drawn to this point by Doederlein, at the German Surgical Congress in 1898. He had been "startled" to find that sterilised cotton gloves, which had been worn over sterilised hands during an aseptic operation, contained numerous microbes at the conclusion of it. Doederlein's statement naturally attracted considerable attention, for it was obvious that, if micro-organisms were found in the operation gloves, they would not be absent from the uncovered hand of the operator. To be able to sterilise the hands would avail us little if organisms were again to be present within a few minutes.

The experiments of Doederlein were followed by those of Gottstein and Blumberg, Haegler, Schaeffer, and others; and Doederlein's assertion, that a sterilised hand lost its sterility during the course of an aseptic operation, was confirmed. But considerable difference of opinion existed, and still remains, as to the origin of this secondary infection. Assuming that all instruments, mops, cloths, etc., with which the surgeon's hands may come in contact, are sterilised beyond all cavil, there are three sources from which this secondary infection of the hands may spring—

(1) the skin of the patient, (2) the air, (3) the deeper parts of the skin of the surgeon's hands, the ducts, &c.). The first of these is guarded against in practice by the careful covering of the skin around the wound by sterilised dry cloths, and is, of course, excluded in the laboratory experiments. Doederlein believed that the wet cotton gloves "caught" the microbes floating in the air, and that these microbes then rapidly developed in the moist gloves. There is no doubt that any microbes floating in the air, and coming into contact with the wet surface of the hand, would adhere to it; but there is no reason to believe that an appreciable development of them could take place within the relatively short duration of an operation. The conditions are unfavourable for any rapid bacterial growth. Doederlein's experiments, it is now acknowledged, were vitiated by a serious error in technique, which left it open to question whether the hands were really sterile before the gloves were put on.

Haegler modified Doederlein's experiments in the following manner:—After carefully sterilising the hands, a rubber glove was drawn over the right hand, and over that a cotton glove; the left hand was simply covered with a cotton glove. An aseptic operation was then performed, and on its completion the cotton gloves were bacteriologically examined, when it was found that the glove from the right hand contained fewer micro-organisms than that from the left hand. He always found that if a sterilised hand was covered with a sterile rubber glove, the hand in a short time proved infected. Again, if after sterilising the hands (as proved by the thread method) they were rubbed and bathed in sterile ascitic fluid from ten to thirty minutes, the fluid was subsequently always found to contain large numbers of microbes.

Paul and Sarwey's experiments lead to the same conclusion, viz., that though the hands may appear sterile or nearly so immediately after treatment by Fürbringer's method, yet, after soaking them in warm water for a few minutes, it is always possible to remove a larger or smaller number of organisms from the surface of the hands.

On the other hand, Schaeffer* comes to an exactly opposite conclusion, and states that a well-disinfected hand, after the lapse of an hour or two, even when macerated in warm water, hardly gives off more bacteria than it does immediately after the sterilisation. The bad results of Haegler and others he attributes to air-borne infection.

In attempting to decide this point, I proceeded as follows. The hands were infected with *sarcinæ*, and, after an interval of from half an hour to an hour, were disinfected by a prolonged hot-water-alcohol or hot-water-alcohol-sublimate process. The hands were then rinsed in water for a minute or two, and afterwards tested with the ivory slip in the usual manner. The hands were next soaked and gently rubbed together in either a warm saline, or albuminous, solution for a period varying from ten to sixty minutes, when they were again tested. These tests gave me results directly opposed to Schaeffer's statement, for I found that the longer the hand was macerated, the more numerous were the micro-organisms removed by the ivory slip. *Hands which had proved sterile, or almost so, directly after the cleansing process, gradually became more infective as, under the imbibition of the water, the alcohol-hardened epithelium loosened.*

* I quote Schaeffer specially as being the most recent writer on this point.

ON THE VALUE OF OPERATION GLOVES.

As the difficulty of sterilising the hands began to be recognised, it was natural to seek some easy and certain way by which the risk of infecting the wound by the hand could be avoided. One of the earliest suggestions was that the operator and his assistants should wear some form of glove, which might be sterilised with absolute certainty, and, by preventing the contact of the hand with the wound, obviate all risk of infection. Two kinds of gloves were advocated, one made of smooth impermeable rubber (Zoege von Manteuffel, Friedrich), the other of ordinary woven silk or cotton (Halsted, Mikulicz, Perthes). The simple woven cotton gloves have the advantages of being cheap, pleasant to wear, and readily sterilised by steam. There is, however, a serious drawback to their general adoption. The protection afforded by these gloves is dependent upon their being dry; as soon as they are moistened by water, blood, or other fluid, they no longer afford any barrier against the micro-organisms passing from the hand to the wound. If a sterilised cotton glove be drawn over an artificially-infected hand, and the surface of the glove be tested by means of an ivory slip, or, better, by a silk thread rubbed over the glove, it will be found that the glove affords almost perfect protection from the infected hand, *so long as it remains dry*. But if a little sterile serum or water be poured on the glove, it no longer seems to afford the slightest hindrance to the passage of the microbes to the silk thread or ivory slip. Owing to this fact, the value of woven cotton, or of silk gloves, is greatly reduced, for it is hardly possible for the operator

to avoid getting his hands moistened with blood or other fluids. Mikulicz, who so strongly advocates their use, attempts to get over this difficulty by changing the gloves as often as they become wet. But this is attended by too great inconvenience to secure general adoption.

The smooth impermeable rubber gloves are, of course, free from this objection, and, as long as they are intact, afford a perfect protection. Their great disadvantage lies in the diminution they cause in the fine sense of touch. No matter how thin the glove be made, a marked loss of tactile sensation is noticed by the wearer. For many operations this is no great matter; but for others it more than out-balances the lessened risk of infection. Other minor disadvantages of the rubber gloves are their liability to be cut or pierced by needles or other sharp instruments, thus losing their efficiency, and their relatively high cost. Many of these objections have been removed by the introduction of the thin seamless gloves suggested by Friedrich. With these the loss of the sense of touch is very slight, and one soon gets accustomed to it, so that for probably the majority of operations they may be used without inconvenience. Their chief disadvantage lies in the ease with which they are torn.

There is great advantage in the use of gloves by the operator's assistants. However conscientious and experienced the assistants and nurses may be, they have not the same sense of responsibility to urge upon them the necessity for the adequate sterilisation of the hands; and the operator has often an uncomfortable feeling that his assistants' hands are not above suspicion. The use of sterilised rubber gloves has here great advantages. The

gloves for the assistants may be of stouter material, and should be of such a size that they can be readily slipped on and off.

Another use of the gloves is the protection they afford to the surgeon's hands from contact with infectious matter. We have seen that the number and variety of micro-organisms found on the hands depend largely upon the occupation of the individual. The hands take up microbes from almost everything they come in contact with. Seeing the great difficulty with which infected hands are rendered sterile, it is of the utmost importance that the surgeon should take care that his do not come in contact with septic matter. The need for this prophylaxis has not received the attention it deserves. It is true that for many years it has been recognised that the attendance of operating surgeons at *post mortem* examinations incurs a considerable risk of carrying infection to their patients; and even the laity knows that a doctor, who has recently attended a case of puerperal fever, should, for a time, abstain from midwifery practice. Yet how frequently does one see even an experienced surgeon unnecessarily handling an infective part—removing soiled dressings with the hand, opening abscesses, examining the rectum, vagina, or mouth, or touching granulating wounds—without the slightest precaution to prevent the infection of his hands. By a little thought most of these occasions of infection can be avoided. Septic wounds can be dressed as readily by means of forceps as by the hand; and impermeable gloves can be drawn on when septic parts are to be handled or operated upon. And even on those relatively rare occasions where thin rubber gloves cause too great a loss of tactile

sensation, the skin of the hand can still be protected to a considerable degree by means of a thin coating of wax or paraffin.

It has frequently been suggested that the liability of the hands to convey infection could be obviated by covering them with a thin layer of wax, varnish, or some other impervious body. Unfortunately, none of these suggestions has been found reliable, for the thin coating of wax, varnish, etc., is liable to crack and peel off under the operative manipulations.* But though these films cannot be relied upon to protect the wound from an infected hand, they may with advantage be employed to protect the hand from infection by the wound—more especially so when the manipulation is of short duration. For this purpose, I have found that a 20 per cent. solution of hard paraffin (melting point, 45°C.), in xylol, as suggested by Menge,† when rubbed on the skin, deposits a fine flexible layer of paraffin, and so protects the epidermis from infective material.

* With regard to the recent suggestion of Murphy (*Jour. of Amer. Assoc.* No. 12, 1904), that the hands be coated with a 4% solution of gutta-percha in benzine, the same objection holds good.

† *Munch. Med. Woch.*, No. 4, 1898.

TIME AS A FACTOR IN DIMINISHING THE INFECTIVITY OF THE HANDS.

Lastly we have to consider the value of time as a factor in diminishing the infectivity of the hands. Attention has already been drawn to the well-recognised rule: That the surgeon who has attended a puerperal fever case, or who has come into contact with virulent septic matter shall abstain for a time from attending other parturient women. This rule, which dates back to the epoch-making discoveries of Semmelweiss on the contagion of puerperal fever, is well recognised and appreciated by the laity; but it has been frequently attacked in recent years by various medical authorities as either futile or unnecessary. Those who have judged it to be *unnecessary* have usually done so under the erroneous impression that the hands can be readily and certainly sterilised, no matter how severely soever they may have been infected. A striking instance of this is to be found in a recent article by Dr. Horrocks, on Puerperal Sepsis (*Brit. Med. Journ.*, Feb., 1904, p. 353), where he expresses his opinion thus:—“And this leads me to the second question, namely: How soon after ceasing to attend upon a puerperal septic case is it possible to resume practice? The reply is, About fifteen to twenty minutes. In other words, just as long as it takes to get your hands aseptic.” Unfortunately, the method which Dr. Horrocks believes to be capable of rendering the hands aseptic, viz., “by scrubbing with soap and hot water, followed by scrubbing with a 2 per cent. solution of lysol, and finally immersed for not less than two minutes in a solution of perchloride of mercury (1 in 1000), or of formaline (also 1 in 1000),” has been repeatedly shown to be

incapable of even approximately sterilising them. Indeed, Dr. Horrocks himself naïvely states that, after the hands had been so treated, "the bacteriologist at Guy's Hospital has never been able to discover any streptococci, though he has always by scraping the nail been able to cultivate *non-pathogenic* staphylococci." (The italics are mine.)

A similar view is held by Dr. Galabin, in the last edition of his well-known text-book on Midwifery, published in April, 1904. In discussing the prophylaxis of puerperal fever (page 852), he evidently regards the clothes as more likely to carry infection than the hands of the surgeon, and only suggests the desirability of abstention from further midwifery practice in the case of a very contagious form of the disease; and even then he goes on to add: "My own belief is that antiseptic precautions carried out as described above are an adequate security." The antiseptic precautions referred to consist in the sterilisation of the clothes by steam, and the washing of the hands "in some effective antiseptic solution, such as perchloride of mercury (1 in 1000)."

We have seen that with the best methods of disinfection known, the task of cleansing a severely-infected hand is always one of great difficulty, and some uncertainty. And, remembering the extreme virulence of the puerperal fever organisms, and the enhanced susceptibility of the parturient woman, it seems, to say the least of it, unwise to omit the precaution of abstention, unless, as has been stated by some, it is useless. But what is the value of abstention?

When we examine into the reasons which have led to the adoption of this period of abstention, we find them meagre and somewhat conflicting. For this precaution has

been adopted rather from clinical experience than from bacteriological experiment. It is possible to suppose that the period of abstention could affect the virulent septic micro-organisms on the hands, in one or more of the following ways:—(1) By the death of the organism, (2) By the diminution of its numbers, or (3) By a reduction in the virulence of the microbe.

1.—The supposition that the septic microbes, when situated upon the hands, rapidly die from lack of moisture, or from other unfavourable surroundings, is without foundation. We have ample proof that the staphylococcus and other septic micro-organisms readily retain their vitality for many weeks, under conditions far less favourable to them than when they are adherent to the skin.

2.—That a diminution in the number of the microbes takes place during the period of abstention is no doubt true. The shedding of the epithelial scales, aided by repeated washings, would certainly get rid of large numbers of the micro-organisms, and so reduce the special infectivity of the hands.

3.—It is uncertain how far the virulence of the micro-organism is likely to be diminished during the period of abstention. Haegler, who experimented specially upon this point, does not think that this virulence is likely to be reduced at all during the few days of abstention. He made several experiments by allowing very virulent staphylococci to dry upon the skin of his thigh, and states that after six days he was unable to detect any diminution in the virulence of the cocci. He does not, however, mention the steps he took to estimate this—a matter of no small difficulty.

Personally, I do not think that Haegler's conclusion, that the micro-organisms are not likely to lose their virulence during the relatively short period of abstention, is warranted. It is at variance with my own experience in experiments with the diphtheria bacillus and the pneumococcus. It is also contrary to the general experience of bacteriologists, namely: That all pathogenic micro-organisms are highly sensitive to their environment; and that an almost inappreciable variation in it is sufficient, if not to destroy the organisms, at least materially to inhibit their growth, and to lower their vitality and virulence. I think we are therefore justified in concluding that the institution of a period of abstention in midwifery practice, after contact with specially virulent septic matter, is a reasonable and valuable ordinance, and one which it would be well to observe as far as practicable in general surgical work.

In fixing the period of abstention, regard should be had to the virulence of the infection, the nature of the proposed operative procedure, and the probable local and general susceptibility of the patient.

CONCLUSIONS.

It may be convenient to summarize the results of the preceding inquiry under the following conclusions:—

1.—That, even after the most prolonged and energetic washing of the hands in soap and hot water, it is not possible materially to diminish the number of microbes on them.

2.—The same conclusion holds good whether sea-sand, marble-dust, or Schleich's soap be employed.

3.—There is no advantage to be gained by unduly prolonging this washing process, as the hands never become sterile, and, owing to the loosening of the epidermis, generally appear more infected after than before the washing.

4.—Neither is there any advantage offered by the use of soft soap, or soap containing an excess of free alkali. Any increase in detergent action is more than counter-balanced by its irritating effect upon the skin.

5.—The water should be used as hot as it can be borne, and it should be frequently renewed. After washing, the hands may, with advantage, be rubbed with a dry, rough, sterile cloth to assist in the removal of the superficial cells of the epidermis.

6.—The use of turpentine, benzoline, or xylol during or after the washing with soap and hot water does not appreciably improve the results.

7.—The aqueous solutions of carbolic acid, lysol, perchloride or bin-iodide of mercury are practically powerless to affect the micro-organisms situated on the hands, and the use of these antiseptics after a thorough preliminary washing of the hands utterly fails to render them sterile.

8.—The use of a saturated solution of permanganate of potash, followed by the application of strong oxalic acid (Kelly's method), gives wholly inadequate results.

9.—The bactericidal power of the new antiseptic, "Sublamin," is considerably below that of perchloride of mercury, and, as a sterilising agent for the hands, is of little value, and cannot be compared in efficiency with the alcohol-sublimate method of Fürbringer, which it was introduced to supplant.

10.—The combination of an antiseptic like lysol or bin-iodide of mercury with a soap does not increase the power of the antiseptic, but rather tends to lower it. Such soaps are practically valueless for the cleansing of the hands.

11.—Alcohol possesses a remarkable power of sterilising the hands, far surpassing that of all other agents. In order to obtain the full benefit of the spirit method, it is necessary to employ the alcohol for from four to five minutes; and the whole procedure must be carried out with vigour and intelligence.

12.—Unless the hands are in an exemplary cosmetic condition, good results cannot be obtained by any method. A roughened or chapped hand does not admit of disinfection.

13.—Though the spirit be employed for not less than five minutes, and though the hands be in an exemplary cosmetic condition, yet a perfect sterility cannot be obtained, and, in a considerable proportion of cases, the hands are still surgically infective. When the hands have been recently infected with septic matter, the likelihood of their being rendered sterile is greatly diminished.

14.—Spirit soap is greatly inferior to plain alcohol as a cleansing agent; and the addition of bin-iodide of mercury or lysol to this soap does not materially increase its value.

15.—The power which alcohol possesses of sterilising the hands is principally due to its property of hardening and fixing the superficial cells of the epidermis; in addition to which it has a marked bactericidal action.

16.—Although absolute alcohol has little or no germicidal action, when diluted it does possess a certain power, varying according to the degree of dilution, 70 per cent. being the most powerful. In this strength it has a much greater bactericidal action upon the pyogenic cocci than has an aqueous solution of either bin-iodide or perchloride of mercury (1-1000).

17.—With the exception of absolute and very high percentages of alcohol, the spirituous solutions of antiseptics (perchloride of mercury) are markedly superior to the aqueous solutions in sterilising power. The efficiency increases in direct ratio to the percentage of alcohol up to about 70 per cent., when any further increase in the proportion causes a reduction in the sterilising power of the antiseptic.

18.—The watery antiseptic solution in Fürbringer's method may with advantage be replaced by 70 per cent. sublimate-alcohol. It is a more powerful bactericide, and it does not reduce the hardening of the epidermis by the previous alcohol.

19.—Of all the methods tested, the best results were obtained by the following modifications of Fürbringer's process :—

- (a) The hands were first scrubbed for five minutes with soap and very hot water (about 50 degrees C.), the water being frequently changed. The use of sterile sea-sand as an addition to the nail brush is an advantage.
- (b) Rubbed with methylated spirit for three minutes.
- (c) Scrubbed for a minute or two with 70 per cent. sublimate-alcohol (1 in 1000).
- (d) Finally rubbed until dry and polished with a sterile cloth.

This I believe to be the most efficient of all the methods at present suggested. But my remarks on the importance of the personal factor in the selection of the disinfecting process (see page 13) must not be overlooked.

20.—Hands proved to be practically sterile directly after the cleansing process gradually become more and more infective, as, under the imbibition of the water, the alcohol-hardened epithelium becomes loosened.

21.—As no method of cleansing the hands guarantees sterility, it is desirable that impermeable gloves be worn when the nature of the operation will permit, whether the wound be aseptic or septic. In the former case they protect the patient, in the latter the surgeon, from risk of infection.

22.—Where gloves cannot be tolerated on account of the consequent loss of tactile sensation, the epidermis of the hands may be protected to some extent from direct contact with septic matter by a thin layer of hard paraffin dissolved in xylol.

23.—A period of abstention from operative work, after contact with specially virulent septic matter, is desirable.

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