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HAEHLER'S CLEANSING DISINFECTION & PROTECTION OF THE HANDS

TRANSLATED BY

HERON WATSON

With
Dr Watson's Compliments

The Cleansing, Disinfection, and Protection of the Hands


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CUIQUE·IN·SUA·ARTE·CREDENDUM

The Cleansing, Disinfection, & Protection of the Hands

An Experimental and Critical Study

By

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Translated with the special authority of the Author by

Charles Heron Watson

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Dedicated

WITH DEEPEST GRATITUDE

TO THE MEMORY OF

MY ESTEEMED TEACHER AND BENEFACTOR

THE LATE

PROFESSOR DR AUGUST SOCIN

Preface

THE subject of the preparation of the hands has, within recent years, been so widely discussed that some may consider it a work of supererogation once more to attempt to deal exhaustively with the subject. In my opinion, however, there are various aspects of the question which still remain in the dark, and I am confident that the thread of the whole subject has often been lost owing to the ill-advised attention which has been bestowed on some new method, or new medium; whilst the well-defined laws of Chemistry, Physics, and Physiology have been treated with scant respect because of experimental results which have been obtained with some one medium.

For the last twelve years I have particularly devoted my attention to work dealing with the infection and the treatment of wounds. This work I have carried out both in the laboratory, and on a large number of patients who were most generously placed at my disposal by the late Professor A. Socin—the then chief of the Surgical Klinik—and by the present Professor O. Hildebrand.

My reason for commencing with some account of the literature of the subjects with which I propose to deal in the course of this work—the preparation of the hands and all that is germane to it—is, that I am fully convinced of the importance to the future of operative

surgery of elucidating this question in its bearing on operative technique.

I completed the work in August 1899.[1] The cause of the delay in going to press arose—irrespective of further claims—from the fact that I was obliged to partly re-write the chapter on the situation of germs in the skin, on account of investigations with regard to boils and to the permeability of the skin in the case of bacteria. On account of the broad basis on which the work is constructed it was impossible to deal critically with publications which had appeared later, for otherwise the revision would have been unending. Thus these later publications could only be considered in notes, or in an index to the literature.

Although this work makes no pretence of having completely solved the question of the preparation of the hands in all its aspects, and although I cannot boast of having introduced any new "perfectly efficacious" medium, or method, with which to prepare the hands, still I trust that the Medical profession may find some interest and value in the results of an experimental review of the *entire* subject, such as are now offered for their inspection by one of their number.

CARL S. HAEGLER.

BASEL, *April* 1900.

[1] Much that it contains formed the basis of my communications to the Basel Medical Society on the 1st and 15th of December 1898.

Preface to the English Edition

FIVE years have now elapsed since I first published this work, and I then believed that the psychological moment had arrived to come to a definite conclusion with regard to the question of the preparation of the hands for operative purposes. I must have laboured under a delusion—at least, so it would appear—for the stream of literature dealing with the subject has by no means run dry. Everything, in fact, points to the contrary, for nearly as much literature on the subject has appeared in the course of these five years as had been published during all the previous years combined; and the quest still continues in search of an antiseptic capable of rendering the hands sterile!

On the one hand, such an astonishing literary production may be interpreted as a good omen. It certainly proves how thoroughly the Medical profession at the present day has been imbued with the conviction that the success of every surgical operation depends on the preparation of the hands. It also practically admits that the difficulties of such a preparation of the hands are universally recognised to their full extent, and that it is impossible to over-estimate this factor.

On the other hand, however, it is evident, from numerous recent works, that in this matter there is still a tendency to diverge from the strict logical path

of Chemistry, Physics, and Physiology, and that in many places there is yet even considerable uncertainty with regard to a clear understanding of *where* the germs are to be found on our hands.

The endeavour is still made to find a reliable means of disinfection, quite regardless of the fact that the non-resistant body-cells must fall the first victims to any chemical medium which is sufficiently powerful to incapacitate the germs so well sheltered by our skin, and that our hands may thus be ruined.

Since the appearance of the German edition of this book I have taken no controversial part in this question; for I stand firm on a basis composed of hundreds of experiments and experiences extending over a period of twelve years, and, principally, because I have had nothing to alter with regard to the views which I then expressed, although up to to-day I have continued to work at the subject.

When Mr Charles Heron Watson, whose personal acquaintance I was so fortunate as to make during his sojourn in Germany for the purpose of professional study, requested permission to translate my book into English, I gladly acquiesced in his proposal.

I felt it to be an honour that my views should receive public recognition in the land which claims Lister as a son, and which is thus the cradle of all our successful endeavours with regard to the treatment of wounds.

I was all the more emboldened to further his suggestion for this reason, that in England and in America less attention had been bestowed on this question generally—at least, so far as it is permissible to judge from the literature. The excellent volume published by Leedham-Green is the only one of note with which I am acquainted.

The translation is an accurate representation of the German edition. I had no reason to make any substantial alterations.

The main points at issue remain as before—

That we possess neither chemical nor mechanical means enabling us to rely with certainty on the practicability of always rendering the hands absolutely free from germs, either for the space of a moment or for a period of time requisite for the performance of an operation. That, in view of this, the expression "sterilisation of the hands" is meaninyless.

That the germs which are most difficult of access are those which are mainly to be found in the accidental irregularities of the surface of the skin, and in the minute and almost imperceptible wounds. That the first essential in the question of the preparation of the hands is thus Prophylaxis. That to avoid infecting the hands, to save them, and to preserve their condition, is the best antiseptic treatment they can receive. That the logical consequence of this conviction is as difficult as it is important.

Great attention has lately been bestowed on the question of disinfection by means of Alcohol, which Ahlfeld brought into notice, and for which he and his adherents loudly claim excellent results. Of the one hundred and forty-four works which have appeared since the publication of my book, the majority discuss the question with regard to the importance of Alcohol in this respect. That the discussion has often assumed an unpleasant and personal form is much to be deplored.

Investigations which I and others have carried out during the past year have shown me that it is as impossible to produce complete sterility of the skin with Alcohol as it is with any chemical or mechanical medium. Statistics of good clinical results in no way

bear out my conclusions in this respect. In the case of these results following surgical operations, other factors, in addition to the preparation of the hands, require to be considered. Surgeons, who have devoted special attention to their hands, have obtained good results when a mere perfunctory disinfection with Corrosive Sublimate was in vogue!

But the main question to be decided is whether or not we can actually sterilise our hands, and maintain them in this condition. The first point to be considered —*i.e.* the pathogenesis of the germs in question—is a minor one, since from the outset we can never know whether the germs on our hands are pathogenic or harmless. The only way in which to settle the question of primary importance is by means of conscientious experimental investigations, which, together with merciless self-criticism, is the only possible means of exposing the numerous sources of error. We have no desire to allay our anxieties from a false sense of security. Therefore it is only in the laboratory, and not in the operation theatre or in the sick-room, that this question can be determined, and whoever desires to carry out such work must be essentially a fully trained bacteriologist. I need hardly add that the experimenter must keep in mind that it is only with regard to the question of the possibility of the sterilisation of the hands that an answer can be expected from such experiments in the laboratory, and that by reference to clinical work he must seek to guard himself against conclusions of too sweeping a nature.

More than ever do I now lay stress on the care of the hands, and on the avoidance of possible infection. We are better informed and have learned wisdom. Our aim is not to exclude the germs wholesale from the hands—that we cannot do—but merely those which are

pathogenic and virulent; and these are found in the vicinity of patients—or mainly so.

As soon as I enter the Klinik I put on gloves. I avoid touching any door handle, any object in the vicinity of the patient, any patient himself, unless my hands are thus protected, and I do not find that in consequence of this I experience any inconvenience or complication. Anyone who has once seen the quantity of dust which accumulates on the white thread glove of a surgeon during his clinical visit is taught to respect this protection to the hand in a hospital where the dust must always contain pathogenic organisms. When I prepare my hands before the first operation I extend the mechanical cleansing to a period of fifteen minutes, and I never neglect to polish the flexor aspect of the ungual phalanges with smooth pumice-stone.

With regard to the use of soft soap, I have never noticed any appreciable damage to my hands—which are, on the whole, very sensitive—from its daily application in considerable quantity. When the operations are finished, it is certainly a matter of no small importance to thoroughly anoint the skin of the hands with some greasy material. This necessitates a less prolonged process of Chemical disinfection (Alcohol, three to five minutes, Corrosive Sublimate, one minute), since the Chemical process takes place more quickly.

From my experiments, I am unable to accept the view of Leedham-Green that antiseptics, when dissolved in 70% Alcohol, are more active than in aqueous solution. When the skin is imbued with 70% Alcohol pure and simple, the subsequent brief process of washing with an antiseptic lotion more quickly produces the desired chemical combination than when the antiseptic is dissolved in Alcohol.

As a rule, when operating, I wear sterile thread gloves,

and these I subsequently change several times. I am well aware that these gloves are permeable; but, on the one hand, they prevent the surface of the hands from coming in direct contact with the wound or the ligatures, and, on the other hand, they permit of smooth organs—such as loops of intestine—being handled with much greater certainty. Still, I do not shrink from introducing my bare hand into the abdominal cavity in prolonged investigations of its contents, or in other manipulations.

In the case of all operations on inflamed tissues, I wear rubber gloves.

No reliance can be placed on a solution of Gum, of Paraffin, of Gutta-percha, or of Wax, as a covering for the hands, as none of these afford any further security when once the hands are at work. Thus all these so-called impermeable coverings of the hand—which sound so well—run but a brief course.

I am deeply indebted to Mr Charles Heron Watson, who has undertaken, and has so ably accomplished, the task of rendering more accessible this work to my English-speaking colleagues.

CARL S. HAEGLER.

BASEL, 29*th November* 1905.

Translator's Preface

A WORK of such originality and of such interest to the Medical profession as that of Professor Haegler requires no apology for this attempt to render it more accessible to a larger circle of readers.

It has been my earnest endeavour throughout to render as literally as possible the Author's own words and views as they stand in the original work, so that his individuality might in no way suffer at the hands of the translator. What the translation may thus lack in fluency and in elegance of diction, I trust may have been more than recovered in accuracy, and in individuality of thought and expression.

I am deeply grateful to Professor Haegler for his kindness in entrusting to me the translation of his work, and I beg to offer him my warmest thanks for the invaluable assistance he has rendered me by the compilation of a new Bibliography and a new Preface, and by his advice with regard to the correction of the proof-sheets.

CHARLES HERON WATSON.

16 CHARLOTTE SQUARE, EDINBURGH.

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DISINFECTION OF THE HANDS

INTRODUCTION

"What, will these hands ne'er be clean?"
SHAKESPEARE, *Macbeth*, v. 1.

ANYONE who at the present day devotes himself to the problem of the treatment of wounds must acknowledge with admiration how clear and far-seeing a view Semmelweiss took of the situation, which enabled him to trace[1] the source of puerperal infection to the hands of accoucheurs and attendants. He came too soon and was misapprehended; he has, however, gained the great reward of not only being the prophetic forerunner of our present methods of treating wounds from a scientific standpoint, but still more of having

[1] In the same way that to study the history of the dressing of wounds affords great instruction and enjoyment to a thoughtful practitioner, it is exceedingly interesting to follow, in the manuscripts of the old doctors, the degree of care which they devoted to their hands and to the cleansing of their hands. It is beyond the limits of this work to give extracts, for this would necessitate as well the consideration of all and sundry surgical theories. I will merely mention here that, as a matter of fact, surgeons (of secondary rank apparently, and very busy practitioners, whose work was not trammelled with doctrines) never forgot the admonition that the hands should always be washed before commencing an operation or a dressing. Pfohlspeundt, for instance, says (*Buch der Bündt-Ertzney*, 1460, herausgegeben von Haeser und Middeldorpf): "He shall also wash his hands before he commences to dress cases." Felix Würtz (*Wundartzney*, 1563) also repeatedly advises surgeons and midwives to cleanse and take care of their hands, "since it greatly benefits anyone to have clean hands." A French surgeon, Doublet (sixteenth century), appears to have been a strict "aseptician." Brantôme (*Œuvres de Brantôme*, Edition elzévirienne, 1878, vi.—cit. nach Follet, *Chronique médicale*, 1899, No. 20) writes of him that he drew great crowds on account of his wonderful cures, "et toutes ses

drawn the attention of operating surgeons to their hands. The surgeon's hands were also included in the well-organised antiseptic system which owes its introduction to Lister, though without receiving the outstanding recognition which they deserve. It was reserved for Kümmell in the first place, but more particularly for Fürbringer, to call especial attention to the subject of the disinfection of the hands, and to place it on a basis approved by experiments.

After the many vicissitudes through which theories and methods as to the treatment of wounds had passed, with the introduction of the so-called aseptic system a settled period of calm seemed to have been entered upon, both in general and also with regard to the preparation of the hands. Everyone washed and disinfected his hands according to the method which he most favoured (chiefly Fürbringer's method), and the operative results were generally brilliant, good, or satisfactory—according as the reports ran. But again during the last ten years innovations have been introduced with regard to the methods of cleansing the hands. It is interesting to note that the movement in this connection, which Semmelweiss originally started in a Klinik for Women, has chiefly been carried on by those working in this department. The reason for this is particularly clear. Whilst the surgeon, in infectious processes occurring after operations, can ascribe the fault to a whole collection of factors, and is only too willing to lay the blame of this state of affairs on the materials, in midwifery cases the air, dressings, swabs, and ligatures, and all other dumb witnesses, cannot be taken into account, and it is only the hand which has been engaged in the work that must be dragged into the light.

It was thus mainly the accoucheurs who, after a

cures faisait le dit Doublet par simple linge blanc et belle eau claire venant de la fontaine ou du puy." They certainly for the most part lay claim to a knowledge of sorcery, with which a surgeon was then credited, and still more to a knowledge of plasters and ointments; whilst particular emphasis is laid on religious beliefs, with their ecclesiastical obligations, and on moral attributes, and little or no further attention is paid to the outer man.

time of relative calm, again set agoing the question of the preparation of the hands. Numerous investigators sought to prove that it might not be possible to render the hands sterile by means of the hitherto accepted methods, and the problem soon again became the theme of active discussion by gynæcologists and surgeons.

The grounds on which this discussion was renewed were various. People had with time acquired the courage to avow that, in spite of careful attention to the rules of the aseptic system, all cases did not heal by first intention or "without some reaction," and that, after a long series of cases healing faultlessly by first intention, some cropped up in which unimportant or more severe complications of the wound made their appearance. And this, indeed, did not occur merely in isolated provincial hospitals; for an operator, who was ready to undertake a case in the happy security of the belief that nothing could go wrong with the wound under his hands, might well be sought for in vain.

It is a common enough occurrence for a surgeon to remain in relative uncertainty, and to anxiously watch the temperature and general condition of the patient, during the days which follow an operation. It must, however, be admitted that a great improvement has taken place in the process of healing, in cases after operation. Puncture suppurations and small skin-abscesses are indeed complications which disquiet us, but more severe cases taking a pyæmic course can fortunately be counted as unusual, though by no means impossible.

Schleich[1] describes the tendency to obtain a rapid union by suturing under all circumstances as a "mistaken mania for primary union"; "the suture is the vulnerable point of surgery," says he. Undoubtedly the practice of packing a wound with gauze avoids a great number of complications—besides being an older practice—but this is again simply providing a convenient corner in which, under cover of "the individual debility of the organism," the sum-total of

[1] *Neue Methoden der Wundheilung*, p. 58 *et seq.*

errors can be concealed, whilst the errors really lie in the precautionary measures which have been taken for the operation. One can understand that, in the event of complications arising in a wound after operation, the idiosyncrasy of the patient will have to be considered in the investigation of the matter; still, there are a sufficiency of cases (and these, indeed, occur most frequently) in which the individual weaknesses do not suffice to explain the matter, and in which the *materia peccans* must be exclusively sought for in the preparatory measures which have been taken. Even though these wound-complications are of little after-importance, and in reality only retard the healing of the wound to a slight extent, still it is the duty of every conscientious operator, without regard for his personal vanity, to investigate the cause, since even a slight complication will always act as a danger-signal.

A further reason why in recent years even more attention has again been turned to the question of the preparation of the hands, is to be found in the *reports of the presence of micro-organisms in operation wounds.* One had soothed oneself with the thought that, owing to the use of the aseptic system, microbes would be excluded from the wound, while now it appeared that no operation wound was to be deemed sterile. Whence, then, come these germs, in the event of the preparatory measures having been found to be sufficiently in accordance with the appointed regulations?

It does not lie within the compass of this work to investigate all the sources which may furnish septic material to operation wounds; it is only permissible here to draw further into the light of day the factor of the hands. In all proposals of cleansing and of disinfection, the use of permeable and impermeable operating gloves is still an unsettled problem, and disputes continue to surround the old question:—Is it always possible, with any certainty, to render the hand free from germs? While Ahlfeld, Schleich, and others answer in the affirmative, the majority deny it.

The next consideration is to show that in this question

it is justifiable to form conclusions from purely bacteriological experiments. One knows at present (Büdinger, Tavel, Brunner, Riggenbach, and others) that both operation and accidental wounds can heal after primary suture without any inflammatory appearances, although the bacteriological investigation has proved the presence of germs, more or less numerous, and recognised in some degree as pathogenic, either primarily in the wound or secondarily in the wound-secretion. One may thus be readily tempted to assume that the few germs, which are introduced by the well-cleansed hand of the surgeon, can do little to really disturb the progress of a wound. What one has to consider above all things is the good of the patient, and not scientific research; and one might imagine clinical experience to be of more value than the investigation report of the bacteriologist, were it not that, alongside numerous cases healing by first intention, others occur in which more trifling or more severe wound-complications afford ground for reflection. Very wrongly, laparotomies are still constantly reputed to be fit tests of the preparatory measures taken—and this particularly in the case of gynæcologists. It is undoubtedly owing to the frequently tragic course of wound-complications in such operations that many are prevented from adopting a proposition which has been put to the test—that the peritoneum deals more easily with microbes than do other tissues, that in this region small sins of omission thus avenge themselves less, and that here technique plays the chief part. In operations for goitre and hernia, and operations on large joints, the tissues react more readily to the microbes than does the peritoneum.

Since it is possible to render instruments, swabs, and dressings free from germs, to operate in places where even the air is not to be feared as a conveyer of germs, to eliminate the septic vapours that are expelled from the mouth by means of mouth-masks or by the avoidance of speech, it is thus necessary, if wound-complications do arise, to lay the blame on *those* factors that most readily escape the influence of the mechanical

control—the *hands*, which come in direct contact with instruments, swabs, and, by no means least of all, with the wound. We know from experiments on animals that, in the event of wound-complications arising, the number of germs which are present plays a part; we know, however, equally well that the *quality* of the germs is of great importance. One must not forget that undoubtedly the surgeon's hand carries on it more developed pathogenic septic material, although in smaller quantity, than does the soiled hand of a labourer. Thus, in spite of all statistics showing brilliant results of healing, which unfortunately only refer to a series, it is the *bacteriological control of the hand* alone which can bring to account this apparently incommensurable factor.

CHAPTER I

METHOD OF PROCEDURE

In bacteriological research the vulnerable point is the *method of procedure.* Everyone who has busied himself experimentally with this question, and who, in his experiments, is accustomed to subject himself to ruthless self-criticism, is well aware of the numerous sources of error which have here the power of falsifying his conclusions. Geppert's recent valuable experiments have taught us to avoid one principal source of error—the insufficient elimination of the antiseptic in the removal of matter for inoculation; all experiments that neglect this factor cannot warrant accurate conclusions.

The method of procedure in the removal of matter for inoculation, *i.e.* the withdrawal of the material from the hand to be tested, has undergone various changes. The plunging or pressing of the tips of the fingers into fluid or solid nutrient media is now, and very rightly, almost universally held in discredit, although Mikulicz and Poten still hold to it (pressing the tips of the fingers into a plate of fluid Agar for the space of thirty seconds).[1] It is certainly true that the tip of the finger, together with the space under the nail, forms the most suitable nidus for septic material, for it is the most difficult of access for cleansing purposes. But the fact that the septic

[1] Poten (*Die chirurgische Asepsis der Hände*), just as Ahlfeld had already shown, damns the method itself when he suggests (p. 24) that a medium, into which a single finger has been pressed, remains sterile even when the fingers have not been cleansed.

material here lies firmly established in recesses and interstices, and clings tenaciously to the fatty menstruum of the surface of the body, stands in the way of its transference to the nutrient medium (particularly after a vigorous mechanical cleansing), apart from the fact that in this short space of time the nutrient material (especially if it consists of tenacious Agar or Gelatine) cannot penetrate fully into all the recesses and delicate fissures in which the germs are concealed.

The method of scraping out the space under the nail with small pieces of wood, ivory, or wire, which, according to Fürbringer's fundamental work, should always be used to test the methods employed to cleanse the hands, certainly answers well enough; but even this is in various respects insufficient. It does not permit a uniform investigation of a great part of the hands and nails; it is particularly inadequate for the surface of the hand; nor do the pointed instruments penetrate to an equal depth thoughout the whole of the space under the nail. Anyone who has examined the space under his nails with a magnifying glass or with a microscope must have clearly seen how impossible it is for such unyielding instruments to thoroughly wipe out the recess-like space under the nail.

I had employed these methods for many years—the latter method in particular—and, although aware of their deficiencies, I had not been able to find any satisfactory substitute for them. But three years ago, when I had occasion to investigate the question of suppuration caused by ligatures, I hit upon a modified method of investigating the condition of the hands, which, in the first place, fulfilled as far as possible the normal conditions occurring in operations, and, in the second place, enabled both the space under the nail and the surface of the fingers and the hands to be uniformly examined at one and the same time. I shall endeavour to prove in a subsequent work that most of the grave sequelæ, occurring in connection with wounds which have become septic after operation, are to be traced to the ligatures and to the buried sutures. These foreign

bodies, which remain buried in the tissues, run the greatest risk of being infected by the septic material on the surgeon's hands—if any is present on them. His hands and fingers rub against the ligature when a knot is tied, or forcible traction is exerted, and germs are thus removed from his hands on the rough surface of the ligature; these germs are either primarily present on the surface of the surgeon's hands, or have been expelled from the deeper parts of his skin by pressure or by traction. Ligatures which have thus been drawn through the hands are, accordingly, the best means which we have of testing the degree of asepticity or of septicity of our hands.

In the case of this *thread method* the form of procedure is as follows:—Silk threads, of the thickest size and about 20 cm. in length, are sterilised in short test-tubes.[1] If the threads are sterilised in the dry steriliser, one must take care that the temperature is not elevated to an extent sufficient to singe the threads. I found from my experiments that the threads, when slightly blackened, are not so suitable for the purpose. If the temperature does not rise above 150°, the silk will then remain a brilliant white.

When the septic condition of a hand is to be tested an assistant removes the thread, which is coiled up, from the test-tube (the mouth of which has previously been heated in a flame) with sterilised artery forceps, and offers the thread to the person whose hand is to be examined. He now draws the thread through his hand, and then rubs with a sawing movement the space under the nail, or the root of the nail; in this way the palm of the hand, the gaps between the fingers, the dorsum of the hand—in short, every part of the hand—can equally be included in the perform-

[1] I began by removing all the fat from the threads, before they were sterilised, so as to ensure that the moist, septic material should make its way in more easily. I have, however, given up this practice of removing the fat—for plate cultures at least—because, when the fat has not been removed from the threads, the germs can be distributed much more easily in the surrounding medium by shaking and rocking the Agar plate, and this makes the enumeration a much simpler matter.

ance. When the hand has thus been rubbed, the thread is placed in the nutrient medium.

The nature of the nutrient medium is of importance. The first requisite, at all events, is that the nutrient medium which is employed should be kept at incubation temperature. Plates of Gelatine—at least, if Bouillon is not used along with them—give results that have no pretensions to accuracy. At any rate, the most delicate reagent is a tube of Bouillon kept at incubation temperature;[1] in such a medium, germs, which do not develop in a plate of Agar, can make a start, for they lie in a fatty menstruum that prevents the entrance of the nutrient medium.[2] But a tube of Bouillon can only prove whether the hand is, or is not, free from germs; it gives no information as to the number of germs present, nor does it help towards their classification. Thus plates of Agar and tubes of Bouillon should both be used in every case, the thread being divided into two or three portions.

After the thread has been drawn through the hand, it is placed in a Petri's capsule which has been filled with fluid Agar. The closed capsule is then carefully shaken for some time on a level basis, so as to symmetrically distribute the germs on the thread throughout the nutrient medium, and it is now left to solidify.[3] (Examples of these thread-agar capsules are

[1] Or, according to Geppert's advice, Gelatine kept at incubation temperature. This modification has the advantage that the Gelatine, when removed from the incubator, solidifies, and enables one to examine the colonies microscopically, unless the solidification is prevented by the liquefaction of the medium. This modification is naturally not adapted to allow one to determine the number of the germs.

[2] Poten's assertion (p. 34) that, of the 35·8 per cent. non-sterile cases, the growth on Agar was often so trifling "that little or no appreciable turbidity would have been produced in tubes of Bouillon," will give rise to considerable astonishment amongst bacteriologists.

[3] In the course of more elaborate experiments, in which several plates are used, it is better to add the fluid Agar later. Still more accurate information is obtained with regard to the number of the germs if the thread is well shaken in a test-tube with fluid Agar, which has, however, been allowed to cool, and the Agar and thread are then together poured for the first time into the capsule. The thread, however, is apt to catch on the test-tube. If the Agar is poured on to the thread when it

shown in Plates I.–IV.) I always allow the other half of the thread (which has been cut with sterilised scissors, and then rolled to some extent between the fingers of the person under examination) to fall into a tube of Bouillon that is standing ready for the purpose. This is kept in the incubator for twenty-four hours, and half of the contents can then be used for development under anaerobic conditions.

The superiority of this method of procedure was proved by a number of parallel experiments, in which no cultures resulted from Kümmell's original method (pressing the tips of the fingers into nutrient media), or from that introduced by Fürbringer (the use of small pieces of wood or rods), whilst cultures were obtained from silk threads that had been drawn through the same hands.

I must again insist on the fact that, to test the value of a method for cleansing or disinfecting the hands, *the whole hand* must always be included in the experiment.

Ahlfeld's procedure, which only tests a single finger, is not sufficiently thorough, and *must* lead to incorrect conclusions. If it requires five minutes to purify a single finger, then seventy minutes should hardly suffice for the whole hand, when one considers the extent of the hand, even though the cleansing of the whole hand were carried out with the same mental concentration as that bestowed on the individual finger.

My investigations were carried out partly in the Surgical-bacteriological laboratory (so far supposed to be under my direction) or in the theatres of the Surgical Klinik and Poliklinik of the former Professor A. Socin and of the present Professor O. Hildebrand, and partly in the Klinik for Women under the charge of Professor Bumm, who showed great interest in my

is already lying in the capsule, some of the colonies will develop in direct continuity with the thread, and it will thus be impossible, without frequent recourse to the microscope, to count the colonies separately which have run together into a clump. The stereoscopic microscope of Zeiss affords invaluable service in examining such plates —just as in the case of all cultures in solid nutrient media.

work, and placed himself and his assistants at my disposal in the most liberal manner. To him I now offer my most sincere thanks. I am equally indebted to Dr Knapp, chemist to the Hospital, who constantly assisted me with his advice in the chemical portion of the work. Unfortunately, it is no longer in my power to thank the late Professor Socin, my greatly respected chief for so many years, who, in the most generous fashion, placed everything at my disposal. To his memory I dedicate this work.

CHAPTER II

ON THE LOCALISATION OF MICRO-ORGANISMS ON THE HANDS

If we desire to obtain *a priori* information as to the manner in which our measures of cleansing and of disinfection act, and as to their possible result, it is essential that we should know precisely *where* the micro-organisms lie which we propose to remove from the hands.

The *open space under the nail*, that great natural hiding-place of dirt, is the simplest answer to this question. To anyone who has examined under the microscope longitudinal sections through the nail and its substratum, or the spaces under his own nails with the stereoscopic microscope, and has thus seen the irregularities and fissures of the corneal layer, forming the boundary between the nail-bed and the skin of the finger, it is quite evident *where* the micro-organisms can hide themselves. Therefore, in investigations with regard to the cleansing and disinfection of the hands, attention is chiefly directed to these parts; and rightly, too, for this reason, that it is obviously most difficult to remove the germs from these parts. The fact that the tips of the fingers, directly or indirectly, come most in contact with the wound, and that therefore the septic material in the space under the nail is particularly dangerous, is not in my opinion—presupposing a careful mechanical cleansing—another altogether satisfactory reason why, in such investigations, one should focus one's attention—and often exclusively—on the

open space under the nail. It is principally when the tissues are scraped with the nails that the rough parts of the space under the nails (which are usually covered by the nail) are rubbed against the surrounding parts, to which the flexor aspects of the fingers are, to a considerable extent, exposed during an operation. If no such scraping of the tissues occurs, it is, as one may imagine, less likely that the septic material, which has such a firm hold of the skin that it has already been able to withstand the ample cleansing measures which have been brought to bear upon it, will be diffused throughout the wound.

I have no desire to minimise the importance of the germs which are present in the space under the nail, or the importance of cleansing the nails themselves (this is a point to which I shall again have to refer in greater detail); but I do consider it to be of importance that the *same degree of attention should be bestowed on the skin of the hand in general.*

There can be no doubt that it is more difficult to come to a definite decision with regard to the situation of the germs on a relatively smooth surface such as this. That germs are always found to be present in large numbers in the fatty layer which covers the epidermis, may be accepted as indubitable; the only question is whether, in the first place, these germs can penetrate into the epidermis, *i.e.* are also present *under* the most superficial layer of cells, and, in the second place, to what depth the germs can penetrate into, or can develop in, the ducts of the sebaceous glands.

So far as I have been able to find, the literature on the subject supplies no answer to these questions. Bizozzero and Bordoni-Uffreduzzi certainly originated numerous investigations, with the microscope and with cultures, dealing with the subject of the germs to be found on the corneal layer of the hands, and these have been continued by von Sehlen, Michelsen, Boek, and others, and by Unna in particular. These investigations, however, deal mostly with *bacteria lying on the epidermic scales, i.e.* portions of the corneal layer of

low vitality, which fall off of themselves or can be removed artificially, or with germs attached to *hairs which have been extracted*, or mixed with the *sebum expressed from sebaceous follicles*. These two latter instances neither afford any definite information with regard to the depth to which the germs can attain, nor to the extent of their dissemination, for the mere process of removing the hairs, or the contents of the comedones, implies their contamination by the superficial parts of the skin, which are rich in germs, and they can thus be secondarily infected with germs from end to end during their passage outwards.

Attempts have been made at the present day to elucidate these questions in other ways—without the help of direct microscopic examination; thus, for instance, *by excising small portions of skin* (which were afterwards reduced in size), *which had previously been thoroughly cleansed*, and by placing these pieces in nutrient media—a process which has certainly been employed in every Klinik at one time or another since its introduction by Lauenstein;[1] or, again, by scraping the skin and placing these scrapings, which must be from various layers, in nutrient media (Petruschky, Gottstein - Mikulicz, Bunge). The former method affords no information[2] as to the situation of the germs; and, even supposing the latter process to be more accurate, it is still much too clumsy to supply the desired information; and, besides, the sources of error which occur can scarcely be discarded.

It is by means of *direct microscopic examinations* alone that positive information with regard to the

[1] Samter (*Archiv für klin. Chirurgie*, B. 53), who worked on the ame lines, erroneously describes the method as "autoptical"; such a designation is certainly only applicable in the case of direct microscopic examinations.

[2] "Six years ago I carried out similar experiments on the living body" (Troller, *Stichkanalinfektionen*). "Bandages were firmly applied, over a pad of sterilised cotton-wool, forming a considerable expansion, to extremities which had previously been well cleansed, so that scrapings from the superficial epidermic layer proved to be sterile. When the bandages were removed (four to six days later) the superficial epidermic layer was found to be no longer sterile."

situation of the germs can be secured. The reason why, in numerous works on the disinfection of the hands, this method was not employed, is to be found in the difficulty and—it may as well be said frankly—in the tediousness of such an undertaking. Apart from the fact that it is difficult to differentiate bacteria in sections made from horny tissue, the germs of our skin, especially in the case of those which are more deeply situated, appear to be relatively few in number in proportion to the delicacy of the microscopic sections, and, without a series of several hundred sections, it is impossible to expect a satisfactory result.

From the first hundred sections of cleansed or uncleansed skin, which were obtained from different individuals and from different parts of their bodies, I was unable to obtain full information with regard to the situation of the germs, probably on account of the fact that I had not so far sufficiently surmounted the technical difficulties in connection with staining. All that I was sure of was that the germs of the skin do not adhere except to the most superficial epidermic layer, and that they are not to be found at a deeper level; the furthest they penetrate is below the partially detached lamellæ of this superficial layer of low vitality, a fact which was very clearly shown on the roughened palm of the hands of labourers.

In my opinion, it was quite permissible to employ artificial means in order to elucidate the question of how far germs are generally capable of penetrating into the deeper parts of the skin; and, in order to facilitate the microscopic differentiation, I had recourse to Indian ink, a menstruum of the most delicate corpuscular elements, which are proof against any chemical reaction.

As I was chiefly interested in the hands, I made use principally of fingers, which had been amputated, in carrying out these experiments, and in the further experiments with regard to bacteria; for reasons which I shall later discuss at greater length, I considered it important that the skin should be prepared whilst it still possessed all its vital properties, and even the

functional activity of the glands. For purposes of comparison, and more especially for the reason that fingers, which are relatively intact, seldom require amputation, experiments were also carried out on other extremities which happened to require amputation.

If *Indian ink* is smeared on the skin with a brush or is brought in contact with the skin for several hours (up to twenty-four) by means of compresses soaked in Indian ink and kept moist, a microscopic section proves that the delicate black particles are adherent merely to the surface of the outer epidermic layer; they neither penetrate into the hair-follicles nor into the sweat-glands in the deeper parts, though they form small depôts in the minute folds of the skin.

If Indian ink is rubbed into the skin for two or three minutes by gentle massage with the tip of the finger, the appearance under the microscope is the same as in the former case, unless the massage has been able to press the corpuscular elements into the deeper layers of the epidermis (Plate VI.). The Indian ink certainly appears to have penetrated through one more layer of cells than it did in the former experiment; but stronger magnifications show that the margin of the devitalised cells has been elevated—due to the rubbing, no doubt—and has thus allowed the black particles to be pushed inwards.

A fact of great interest was in this way brought under my notice (Plate VI., *a*). The skin was that of the extensor aspect of the middle phalanx of the little finger. The patient, who confined his attention to clerical work, had delicate, well-preserved hands, and with the naked eye one could see no sign of injuries or roughness on the finger in question.[1] I had examined numerous sections, one after the other, which all showed the integument to be normal throughout, before I came to a piece which pointed to the fact that a small wound must have

[1] The patient desired that the finger should be disarticulated on account of an extreme condition of Dupuytren's contraction, which always recurred, in spite of repeated operations.

been present.[1] The small channel of the wound was loaded with Indian ink right up to its blind end, and along all the delicate fissures which branched off at the sides.

In sections prepared from this finger, and from other extremities which had been treated in the same way, I repeatedly, by chance, came across similar small wounds which merely implicated a portion of the epidermis. I noticed in all of these cases that the impregnation with particles of Indian ink (or, in other experiments, with bacteria) was very marked, which gave it a diffuse appearance as seen under the low power of the microscope, because the particles of Indian ink had penetrated into the numerous, small, delicate fissures which, in the deeper parts of such wounds, branch off on all sides.

As regards the *hair-follicles*, the gentle rubbing of the parts also carries the particles of Indian ink to a certain depth, at all events beneath the level of the Rete Malphigii, nearly to where the sebaceous glands anastomose (Plates VI. and VII.). In the case of the sweat-glands, the particles of Indian ink are found in the first, and, at most, in the second corkscrew spiral; anyhow, they never reach the level of the Stratum Granulosum. A fact which is certainly worthy of mention is that, in these experiments, the particles of Indian ink (and bacteria) were found in comparatively much smaller quantities in the hair-follicles and in the sweat-glands than in the small wounds which happened to be present in the skin. This is to be explained by the more or less constant presence of a layer of fatty material over the mouths of the sweat-glands, which acts to some extent as a protection against their being entered by the particles of Indian ink, which have been rubbed into the skin in aqueous solution, whilst no such protection is afforded to the traumatic lesions.

When *bacteria* were rubbed in, the results were similar.[2] A culture of *Staphylococcus albus*, which I

[1] The microscopic appearance showed that it was several days old.

[2] The technical difficulties of staining are considerable—in connection, at least, with the epidermis. The results were not satisfactory which I obtained with the numerous recipes given by Unna in his

had previously tested on my own body to ascertain whether it was slightly virulent or innocuous, or a culture of *Bacillus subtilis*, was rubbed into the skin of fingers, or of other extremities, which had been amputated, care being taken not to lacerate the skin in any way. Plate VIII. shows a section taken from the skin of the extensor aspect of a finger which had been treated in this way.[1] The cocci seem in this way to be carried to a somewhat greater depth than the particles of Indian ink (the *Bacillus subtilis* not so deep); as the section in Plate IX. proves (flexor aspect of third phalanx of the hand of a labourer), this is particularly well shown in the ducts of the sweat-glands. This certainly represents the furthest extent to which the particles penetrated into the deeper parts, so far as I could observe from numerous sections through the sweat-glands. But it will be observed that even here the germs have not, for the most part, penetrated further than the second corkscrew twist.

An objection which may certainly be raised here is that this artificial infection does not afford conclusions which are applicable to the usual conditions of infection. One must admit that this is true so far as "quantity" is concerned, but not with regard to "quality"; for, just as the surface of the skin is actually covered by a layer which, even if thin, contains micro-organisms, our hands are not lacking in movements which involve the rubbing and squeezing of the tissues, and which enable the germs to be conveyed into the deeper parts. The whole work of the hand during the day consists in a rubbing and

Färbung der Mikroorganismen im Horngewebe; Boek's recipe gave better results, yet the preparations were quite unreliable. In the majority of the sections I made use, with good results, of a modification of Gram's method (a preliminary staining with Picrocarmine; Aniline Gentian violet, 4 minutes; wash off in Aniline water; solution of Iodine, 10 seconds; Aniline oil, 10 minutes; Alcohol, to which some drops of acid Alcohol have been added, 1 to 3 minutes; Xylol, Balsam). I afterwards came to know of Unna's modification of Gram's method with Picroaniline (*Natürliche Reinkulturen*), with which I also obtained good results.

[1] The one half was rubbed with Indian ink, and the other half with culture scraped from the surface of a plate of Agar.

squeezing of the parts, and I again emphatically state that the process of artificial infection consists in gently rubbing the skin, with the exertion of little pressure, and certainly, no laceration. When, by the aid of these sections containing such a number of germs, I was better able to understand the technique of staining and the meaning of what I observed, I was in a position to detect the presence of germs in the natural openings of the skin, even when the skin of the finger had received no preparatory treatment. Certainly many a section had to be examined for the presence of bacteria in the deeper parts, and it cost me much time and labour to prepare and go over a series of 500 sections in which the skin had been artificially infected or had undergone no preparatory treatment; still, in dozens of the preparations germs (cocci, for the most part) could be detected in the hair-follicles. They were naturally found in greatest numbers in the infundibula; less plentifully, and often but singly, in the deeper parts, but never deeper than about where the sebaceous glands anastomose. No evidence of any germs was ever to be noted in the sweat-glands; the entrance of corpuscular elements by this route is made more difficult by the minuteness of the aperture, the tortuous nature of the gland duct, and, perhaps, even by the flow of secretion. Here again the small wounds of the epidermis, such as are frequently met with on the flexor aspect of fingers, gave readiest evidence of the presence of germs; where these happened to occur in the sections, germs were always present in the deeper parts beside particles of dirt.

With regard to the subject of the disinfection of the hands, it is now important to know *whether these stray germs, which are present in the peripheral parts of the hair-follicles, can, under normal circumstances, multiply*, or whether they are merely individual germs, which have been accidentally conveyed to the deeper parts by friction.

There can be no *a priori* assumption, in the case of the hands, that the germs multiply in the alkaline, or

feebly acid, secretion of the skin. Unna[1] says: 'It is only in the corneal layer, or the sheaths of the hairs, that vigorous germs are to be found, where they are capable of a definite degree of self-multiplication without giving rise to the formation of pus." A multiplication of the germs does not appear to be favoured by the secretion of the hair-follicles which forms the sebaceous matter of our skin. Gottstein found that not only do the germs not multiply in Lanoline, but they actually die. I can confirm the former statement; I was unable to prove, in a number of experiments which I carried out with different germs, that the germs multiplied in Lanoline, though in some cases they remained viable for months on end; nor did they develop in sebum squeezed from large comedones which had been inoculated with staphylococci.

I now tried experimentally to solve the question whether micro-organisms, in the deeper parts of the skin, can, under normal circumstances, multiply of themselves. This I did as follows:—I rubbed germs into the skin some considerable time before its removal from the body, and then examined it microscopically after it had been removed. For purposes of comparison, I treated another portion of skin in the same way, but just before its removal from the body. These experiments were performed on a leg twenty-four hours before it was amputated, and on my own thigh seventy-two and forty-eight hours before the excision of the skin. The staphylococci were rubbed into the skin by means of gentle massage, so as to avoid any laceration or disturbance of the parts concerned—just as occurred in the experiments which I have already mentioned.

In both cases the microscope showed that after twenty-four or forty-eight hours the germs had completely disappeared from the sweat-glands; that in the hair-follicles the germs were present in much smaller numbers than in the sections prepared for the purpose of comparison (where the germs were rubbed in

[1] Unna, "Impetigo Bockhardt," etc., *Berliner Klinik*, Heft 46, S. 23.

immediately before the removal of the skin), and were only to be found at the periphery (just within the corneal layer, in the skin taken from my thigh), or were altogether absent. The microscopic appearance of the sections prepared for the purpose of comparison was, on the contrary, just such as I formerly described. Apart from the difference of the nutrient media, the flow of secretion would seem to have proved an obstacle in the way of the further development of the germs, and even to have been capable of eliminating individual germs which had made their way inwards.

Germs on the surface of the skin *penetrate inwards* towards the deeper parts, by way of the natural interstices of the skin, just as little as they multiply in the deeper parts. I have saturated my skin, for a period of several days, with virulent *Staphylococcus pyogenes aureus*, by means of compresses kept moist under gutta-percha tissue, and applied to different parts of the surface of my body, without the slightest alteration occurring in the skin. Had the cocci been capable of growth in the deeper parts, impetigo and boils would undoubtedly have made their appearance. No positive result was obtained if, even before the application of the germs, fluid Agar was gently rubbed into the skin (with practically no squeezing of the parts, and without any laceration, so as to avoid the production of any wounds), in order that there might thus be provided a favourable area of nutrition towards the deeper parts.

The microscope gave full confirmation of these results. Sections of skin taken from extremities, to which, twenty-four hours before their amputation, luxuriant cultures of staphylococci, together with Agar (the whole contents of a Petri's capsule), had been applied and preserved from evaporating, proved that the germs had not once penetrated into the peripheral extremities of the hair-follicles. They all lay on the surface of the skin, less plentifully on the elevated parts, but more numerously in the folds and the depressions, in which they formed complete depôts; also small epidermic

cracks, which chanced to come under one's notice, were crammed full of germs.

Never once were the germs, in skin detached from the body, able to penetrate inwards from the surface—a fact, which I confirmed on pieces of skin which I kept in the incubator from twenty-four to forty-eight hours, with their superficial surface pressing down on an Agar plate covered with luxuriant layers of staphylococci.

It is not permissible, at present, to pursue further the subject of these investigations—investigations which are well constituted to throw a partly new light on the ostensible permeability of our skin, and on the origin of impetigo and boils—because this would lead us too far from our true subject; I reserve to myself the privilege of further reporting on this subject elsewhere. As concerns the question of the cleansing of the hands, it is of interest to draw the following inferences from these investigations:—*That the germs on our hands are but relatively seldom and scantily present in the hair-follicles, and then only in the peripheral parts; that they are not usually present in the sweat-glands, and do not naturally penetrate into all these natural openings in the skin, but can be introduced there by rubbing; that, once there, they do not, under normal circumstances, increase in number, but—no doubt by the flow of secretion—they are got rid of again; that in the accidental openings of the skin, such as small wounds, the case is different, for here the germs are constantly proved to be present, and one may take it for granted that they both increase in number in the deeper parts, and can penetrate inwards from the surface of the skin towards the deeper parts of the small wound-channels.*

The only further question which remains to be solved is whether the germs increase in number on the surface of our hands, *i.e. on* the epidermis, or between the partially detached corneal epidermic lamellæ. In the event of suitable nutrient material being present, a multiplication will take place here, as on every other surface—a fact which certainly requires no experimental proof. If pus, blood, or any other organic medium be retained for

long moist on the hands, it does not prevent the germs from multiplying. Even a steady, profuse sweating of the hand may, especially from the maceration of the epidermis, enable the germs to multiply in spite of the fact that the sweat on the surface of the skin has an acid reaction, though this reaction will indeed be very weak if the sweating is intense. In a further experiment, which I shall afterwards describe more minutely, in which, for the space of forty-eight hours, I wore gutta-percha gloves over my hands, which had previously been cleansed mechanically, and where I removed the gloves from time to time so as to test the degree of septicity attained by my hands, it appeared that in the first four hours the increase in the number of germs on the hands was but trifling;[1] the increase in the number of germs during this period may be thus explained—that the more deeply situated germs had come up to the surface. But, at the end of ten hours, the number of germs had increased more than a thousandfold (45,000), and the increase was then so rapid, that already within the space of from two to three hours the number of germs obtained was from 30,000 to 40,000, although the hands were again each time thoroughly cleansed mechanically—a proceeding which was able to reduce the number of germs to 65–153.[2] The fact that the germs, which were originally present on the hands, first increased in number after a definite period of time had elapsed, and that they again, in the space of a few hours, increased in number to the same extent, is greatly in favour of the view *that we obtain the septic material of our hands in great part from the dust of our surroundings*, since this septic material requires a period of time in which to germinate, and is not propagated like the vegetable forms which are actually growing.

[1] I need hardly say that, after investigating the state of the hands and then cleansing them, I again drew on each time a pair of freshly sterilised gutta-percha gloves.

[2] It was impossible to continue the reduction of the germs further, because the skin was now somewhat macerated, and consequently rendered a thorough cleansing more difficult.

The multiplication of the germs is a phenomenon which is very unlikely to occur on the well-cared-for hands of anyone who is cleanly in his habits, and especially on the hands of a surgeon, always supposing that his hands are in good condition; the germs cleave to the material which forms the sebaceous matter of the skin, on or between the two most superficial layers of the epidermis. The nutrient material, which the hands have been able to collect from external objects, is removed by their being repeatedly washed, and, moreover, the surface of the skin is normally dry; these are plain facts which are very unfavourable to a multiplication of the germs.

That this can be confirmed with the help of the microscope is proved by the following experiment:—Uncleansed skin (with its subcutaneous cellular tissue attached), taken from the flexor aspect of the finger of a labourer, was kept hermetically sealed, so as to prevent evaporation, at a temperature of 28° C., and every six hours pieces were excised and converted into microscopic sections.

The germ layer on the surface of the skin remained unchanged for forty-eight hours (the experiment did not extend over any very great length of time); no multiplication of the germs occurred, except perhaps in some small epidermic cracks which were to be observed here and there in the sections. It seemed to me that, at the end of twenty-four hours, the germs were present in these cracks in somewhat larger numbers. There can be no certainty with regard to this supposition, because such chance-occurring cracks cannot be considered as sufficient objects of comparison.

If we may thus take for granted that the septic material, which is conveyed to the surface of our hands from our surroundings, cannot multiply there of itself, we are greatly tempted to consider, with a sigh of relief, that the flexor surfaces of our hands and fingers, which, with the exception of the tissues about the nail, come most into contact with the surrounding parts in operations, can be most easily dealt with by the measures

which are employed to cleanse them. There are no hair-follicles on these parts into which individual germs may make their way; the ducts of sweat-glands, which are present here in such large numbers, either do not allow the germs to enter, or are soon able to get rid of them; and, if the germs merely lie on or in the superficial epidermic layers, there is no insuperable difficulty in removing them, since the superficial cell-layer is devitalised, and can be removed by mechanical means.

But, unfortunately, it is only too true that the flexor aspect of the hands and fingers is most difficult to cleanse in the bacteriological sense of the word. To begin with, there are the numerous *folds* and *little creases*, which add to the difficulties of cleansing. Further, there are the claims which everyday life makes on the hands, which render it almost inevitable that *wounds*, whether microscopic or macroscopic, will be present in the outer cell-layer of the much thickened and hornified epidermis. This can be seen in microscopic sections, and also on the living hand, when examined by means of a stereoscopic microscope. Even the apparently faultless hands of surgeons, when examined in this way, will seldom be found free from appreciable wounds of the epidermis. To what an extent such little cracks and small traumatic fissures create lurking-places, in which the septic material of the surface of our hands can lie, has already been shown, and is, moreover, but too evident to require further reference. For these reasons the flexor aspect of the hands and fingers places greater obstacles in the way of the cleansing measures which are applied to them, than does any other part of the skin, even though it appear to be faultless. But if this part be actually cracked or rough, no satisfactory result is to be anticipated from a mechanical cleansing, or disinfection, of the part.

CHAPTER III

THE MECHANICAL CLEANSING OF THE HANDS

A. *The Cleansing of the Surface of the Hand*

It should scarcely be possible to dispute the fact that, of the preparatory measures taken to cleanse the hands, the mechanical cleansing, *i.e.* the removal of the germs present on the skin, must rank first; yet, for long, the value of a purely mechanical cleansing of the hands was undervalued, and perhaps, here and there, this may still be the case. Antiseptics were all the rage, *i.e.* lotions by the use of which the germs on the hand were not to be removed, but were to be destroyed—a procedure which appeared to be considerably simpler, and at all events less laborious, than a thorough mechanical cleansing. The belief in the apparently unbounded superiority of this method of disinfecting the hands for long blinded surgeons to the truth, and stood very much in the way of the mechanical cleansing being perfected. However, more recent experimental works on the subject refer forcibly, with or without design, to the value of the mechanical cleansing—that the object of all media employed should be the removal of the septic material present on the surface of the skin, *i.e.* on the outermost epidermic layers, in the creases of the skin and the orifices of the hair-follicles. It is clear that such measures have no direct effect on the germs lying in the deeper parts, *i.e.* in the gland ducts or the crevices of small wounds of the skin; yet I must insist that the vigorous rubbing, pressing, and

squeezing which occurs in conscientious cleansings of the hands has an influential effect on all parts of the hand, and even squeezes out and removes a portion of the deeper-lying germs, and that thus the "massage" effected by the mechanical cleansing is also of importance in this connection.

To what a very general extent in surgery has the use of antiseptic lotions lost credit as a means of preparing the hands! The blind faith, which we formerly placed in them, has been greatly shaken, since different investigators endeavoured to prove experimentally that germs set free in antiseptic solutions mostly remain viable and capable of development for a space of time equal to that occupied by our use of antiseptic lotions in the disinfection of the hands, and that the germs on the hands are far less easily affected by these lotions than had been supposed to be the case.

By such considerations as these, custom, or good clinical results, Péan, Koeberle, Lawson-Tait, Neuber, Bircher, Schleich, and others have been induced to adopt the purely mechanical cleansing. As one is very apt to rush from one extreme to another, it appears to me that, in the case of antiseptics, their undervaluation has been carried too far. If I had not been able to convince myself by means of experiments that the use of antiseptics, in dealing with the septic condition of the hands, is so totally devoid of results, I should not be justified, by the investigations of others, and more particularly by those carried out by myself, in declaring *that the mechanical cleansing*, i.e. *the removal of septic material from the surface of the hand, is most deserving of consideration*, whether antiseptic lotions be subsequently used or not; for, as I shall prove later, no benefit is to be expected from the use of antiseptics unless a thorough mechanical cleansing has previously been carried out.

It may appear somewhat superfluous that, by means of experimental results, I should subject to re-examination the elementary manipulations of the mechanical cleansing, as I subsequently do on the hand. But it is

impossible to deal summarily with such a highly important and complicated performance; for the facts with regard to it are not yet sufficiently firmly and surely established to warrant that no further steps should be taken in following up experimentally its numerous details. Although I was chiefly induced to commence my investigations at the very root of the whole question by my own need for enlightenment on the subject, still, reference to the investigations themselves may well be of interest; for they were all carried out on the same hands (my own), and are thus best calculated to serve as objects of comparison.

But, before referring to the experiments, there is something further to be said with regard to this point. Undoubtedly the most serious objection which can be brought against such investigations belongs to the category of errors of minor importance, viz. that the experiments do not afford an opportunity of comparing the number of germs obtained, since the original supply of germs present on the hands varies very considerably. The "hand in everyday use," as one may call it, is certainly a variable quantity, since its state depends not only on the uses to which it has previously been put, but also on the frequency with which it has been washed, and the fatty material which has been removed from it on the same day; whether, in fact, it has come in contact with solutions capable of killing the germs or of arresting their development; and, above all, on the condition of the hands themselves—in short, it depends on a whole collection of factors which may easily give rise to mistakes and to erroneous conclusions. It is still a disputed point whether the "*hand in everyday use*" or the *artificially infected hand* is preferable for the purpose of such investigations. With regard to my own investigations, I had considerable difficulty in coming to a definite conclusion in the matter.

The method of *artificial infection* has certainly many inducements to offer. There is but one form of germ to be dealt with, which can easily be recognised in all experiments, and which, at any rate, cannot have its

presence attributed to chance impurities. Reinicke, who was very partial to artificial infection, insisted that, in the case of such experiments, it is of considerable importance that the resistance power of the special form of germ employed should be known, as this ensures a degree of certainty with regard to the conclusions arrived at when chemical tests are used.

In my experience, a hand which has been artificially infected is a much more variable quantity than the "hand in everyday use." One must remember that, over and above the germs which are thus artificially introduced, there are the germs which usually infest the hand, and which have also to be taken into account, and that these latter are, as a rule, more deeply situated, and more firmly adherent, than the germs which have been artificially introduced. In a large number of my experiments, in which I artificially infected the hand by rubbing in tainted blood, pus, or Bouillon cultures, I found, as had Gärtner, Henke, and others, that the germs, which had thus been artificially introduced, could no longer be identified after the hands had been cleaned first by mechanical and then by chemical means, whilst *Staphylococcus albus*, and other recognised germs common to the skin, could still be obtained from the hands in every case.

This apparently remarkable phenomenon may be thus explained—that the sebaceous matter of the skin impedes the penetration and the adhesion of the septic material which has been artificially conveyed to the hands. The result is no longer the same if, before the artificial infection is carried out, the sebaceous matter is removed from the skin by means of Kaolin or a mixture of powerful fat-solvents (Ether and Chloroform). A preparatory washing of the parts with soap and hot water must be avoided, for this in itself will alter the surface of the skin to some extent, and will interfere with one's judging as to the value of any one method of cleansing. Septic material, which has been artificially conveyed to the hands, adheres to them best when it, together with a fatty menstruum, is rubbed little by

little into the surface of the skin in small quantities. For this purpose I employed Lanoline or Adeps Lanæ which was mixed in the mortar into a paste with germs taken from the surface of Agar, or from the sediment of Bouillon cultures. Like Gottstein, I was unable to observe any multiplication of the germs in the paste; but I cannot endorse his statement with regard to the prompt death of the germs, since in three examinations of such pastes (containing *prodigiosus* and *subtilis*) I found the germs still viable at the end of three months. These pastes were rubbed into skin from which the sebaceous material had previously been removed; the skin was not examined till two hours had elapsed, and during this time I went on with my ordinary occupations.

Although the results which I obtained with artificial infection of the skin proved to be fairly uniform, I do not consider it to be a process which is requisite to illustrate the manner in which measures of cleansing and of disinfection act, and, in my opinion, the results which are obtained with the "hand in everyday use" are more certain. Fürbringer is doubtful with regard to conclusions drawn from artificial infection, because the germs are conveyed to an abnormal depth.[1] As against this, one must bring forward the fact that even bacteria which, under normal circumstances, are present on the surface of the hands, can be conveyed into the deeper parts by the constant squeezing and rubbing to which the hand is subjected, though not in such large numbers, one must admit. So far as concerns the flexor aspect of the hand and fingers, which are of special interest to us, the natural interstices of the skin, and the ducts of the sweat-glands in particular, are only of secondary importance, for the

[1] "I am thoroughly convinced that in the inunction of cultures into the hands the germs can be conveyed to such a depth in the many recesses, and more particularly in the case of the hair-follicles and the spiral sweat-ducts, that not even the additional use of the cautery itself could kill them *en masse*" (*Deutsche med. Wochenschr.*, 1897, S. 82).

principal recesses are formed by the small wounds which may happen to be present, and by the roughness of the epidermis, and *these are already filled with dirt* (*and germs*) *before the artificial infection of the hands is even begun.*

Artificial infection is unnecessary for this further reason, that it is less fitted than the "hand in everyday use" to supply *statistics with regard to the number of germs which are present*; for the latter—within the limits which I shall mention later—produces more constant proportions. Artificial infection is thus unnecessary so long as mechanical and chemical means are unable to eliminate the germs which are present on the "hand in everyday use."

As the result of numerous experiments dealing with this subject, I decided to make use of the "hand in everyday use" for these investigations with regard to the action of a cleansing by mechanical means—making certain exceptions for purposes of comparison. For weeks the hands did not come in contact with antiseptic lotions, as they were protected by rubber gloves; neither were they washed for one to two hours before being examined; they were always examined at the same time of day (in the evening), and after performing the same sort of work in the laboratory; numerous investigations, which were carried out for purposes of comparison, proved that, under these circumstances, both in point of quantity and of quality, the primary septicity of the hands was fairly constant. This, at any rate, depends less on the use to which the hands have previously been put, than on the condition in which they are kept. Where such investigations are carried out for purposes of comparison, it is indispensable that the hands which are to be examined, should always be those of one and the same person, because, as I shall afterwards show, much depends on the condition of the hands, on practice, and on the intelligence which is shown in carrying out these methods of cleansing. It is self-evident that, under these circumstances, the hands can only be examined once in the day, and that these

individual examinations must be repeatedly performed at different times.

Although it must be allowed that the primary septicity of the hands was liable to variations, and that the threads would at one time, as if by chance, remove a considerable number of germs, and at another time a much smaller number, still, I must point out that the comparisons were merely considered relatively, and they can only thus far rank as conclusions.

Very seldom do we have it more clearly proved to us, than in a discussion such as this, how subtle a question the bacteriological method of procedure really is, how much time it is apt to waste, and with what difficulty satisfactory conclusions can be obtained. We are much in need of a real *autoptical* method which would enable us to follow with our eyes the effects produced on the hands during life by the measures employed to cleanse them. No doubt the use of the microscope is of great advantage, especially as enabling us to investigate our own hands, and I believe that much assistance can be gained from a conscientious and intelligent appreciation of what a mechanical cleansing implies. Anyone who has seen the rough appearance presented by the flexor aspect of the hands, with their many ridges and furrows, and well-marked folds and small superficial wounds, or the surroundings of the nails, a mass of irregular cracks and fissures, comes indeed to understand what a difficult matter it is in such cases to control and to influence what Lauenstein has so strikingly described as "the realm of the invisible"; but, unfortunately, it is impossible directly to follow up, even with the aid of the microscope, the measures which are employed to cleanse the parts.

It was after I had completed the greater part of the bacteriological investigations, that I hit upon the novel plan *of substituting for the bacteria on the surface of the hands minute (though visible) corpuscular elements.* It is self-evident that whatever is chosen for this purpose must remain unaffected by chemicals, *i.e.* cannot be further broken up or dissolved by the action of the

media which are usually employed, such as occurs in the case of germ-cells; *cinnabar* and *soot*, or *Indian ink*, comply with these conditions. In the case of the latter, the corpuscles are somewhat more delicate than those of cinnabar; at any rate, they are more readily seen—as was proved by experiments which were carried out for purposes of comparison—and I therefore principally used the latter. I used it in the following forms—Soot delicately combined with oil or Lanoline, or in an emulsion with Benzene, or, finally, as a watery emulsion, as Chinese ink. If the hands are not particularly greasy, preference should be given to the latter form. The Indian ink is dropped on to the surface of the hand, into which it is rubbed till it is quite dry, which takes but a short time to accomplish. That part of the Indian ink, which is but loosely adherent to the hand, can be removed previous to the employment of the method of cleansing which is to be investigated. Even when the surface of the hand appears to the naked eye to be perfectly clean after it has been washed, minute remains of the Indian ink can still be detected with the help of the microscope in the little cracks and crevices.[1] The smallest particles of Indian ink are, roughly speaking, as large as staphylococci; *any cleansing process which leaves Indian ink on the surface of the hand* (perceptible macroscopically, or microscopically) *is not in a position to remove septic material which is present on the hand, and, undoubtedly, the Indian ink particles which remain behind indicate the recesses in which the germs have escaped the attention of the cleansing process, or in which they can escape it.*

Such examinations of hands, which have been impregnated with Indian ink, are very instructive, and can be warmly recommended for the purpose of ascertaining the cleansing capacity of different hands, or the effect of different cleansing methods. There can be no doubt that the examination of a hand, which has thus been treated with Indian ink, clearly shows how unequally

[1] Here again Zeiss's binocular (stereoscopic) microscope is to be recommended.

the different parts of the hand are usually dealt with in the act of cleansing it. Thus if hands, which have been treated with Indian ink, and dried, are washed once, for the prescribed length of time, with the eyes shut, they are found to be still just in the same condition as that in which they were before they were washed; this is a lesson to carry out the washing with greater concentration and to more purpose. As I have already remarked, the macroscopic examination is not, however, sufficient, since the microscope reveals hiding-places which remain concealed to the naked eye.

The examination of the hand with the help of Indian ink also shows that, in the case of a washing process, which, so far as it can, deals equally with all parts of a well-cared-for hand in a methodical manner, some parts of the hand are cleansed much less thoroughly, as compared with other parts, because they are less easily got at, and accordingly require that they should have more time devoted to them than do the other parts. Naturally, this principally applies to the *open space under the nails*, whilst the matrices of the nails can be cleansed with comparative ease. If the free end of the nail is irregular, the Indian ink granules remain fixed there for a considerable length of time.

In the hollow of the hand, the part which requires most time to be devoted to it is where the thenar and hypothenar eminences approach one another, and next come the parts over the proximal joints. The spaces between the fingers are difficult to cleanse, and are often neglected in this respect. On the back of the hand and fingers the same may be said of the infundibula of the hair-follicles, where the Indian ink granules cling very tenaciously. But what is best brought out by this Indian ink experiment is the tenaciousness with which *small wounds of the skin or roughnesses of the surface, occurring on ill-cared-for hands*, are able to retain the small corpuscular impurities, so that often it is only possible to remove them by actually shaving them off, or scraping them away with a knife; it also shows us to what an extent small wounds are

present at the points of our fingers, which would otherwise escape the notice of our unaided eyes.

In large and continuous investigations with regard to the cleansing of hands, where different individuals (students) have to be dealt with, I consider that the Indian ink experiment[1] is at least as valuable as the tedious and often inaccurate bacteriological experiment, because it permits of all parts of the hand being attended to. Anyone who succeeds in completely removing the Indian ink from his hands by washing them, so that none is visible even when examined under the microscope, thus proves that, in the first place, he has sufficient intelligence to carry out the process of mechanical cleansing, and that, in the second place, his hands are in a condition to allow of their being cleansed.

After this digression I must give a more detailed account of the experiments which were carried out with regard to the process of mechanical cleansing. I shall spare the reader a detailed account of the proceedings as carried out by Fürbringer and others, and I propose merely to give a short account of the results.[2]

[1] Some drops of fluid Chinese ink, such as is commonly offered for sale, are shaken into the hollow of the hand, and are thence distributed over the whole hand by means of rubbing (corresponding to the movements used in washing), and are even conveyed to the open space under the nails by means of scraping with the nails. The rubbing is continued for some little time after the hands are dry. Ten to twenty minutes should now be allowed to elapse before commencing the washing. The hands must not be too greasy, for otherwise they do not take up the Indian ink to a sufficient extent; the grease on them should not be removed by means of Alcohol, on account of the injurious effects which it produces on the skin; it is better to wash the hands for a short time in hot water with soap.

[2] I have already referred to the method of procedure in describing the subsequent steps with regard to my thread method. I need hardly say that everything, which was employed in these experiments, was sterile—water, utensils, instruments, and towels were sterilised as usual, partly by boiling and partly by steam at high pressure. The enamelled basins, in which the hands were washed, were rendered aseptic by setting fire to Alcohol distributed over their entire surface; they stood covered with sterile towels, and even the process of washing the hands was mostly carried out under cover, *i.e.* in a small wooden box, which I shall describe later. The soap was not sterilised because, according to Reinicke and others, and from my personal investigations, it is only in

In this series of experiments the surfaces of the hands and of the fingers were alone examined, whilst the region of the nails was omitted, because my object was to come to a general conclusion as to the value of the individual manipulations in connection with the skin of the hands. The region of the nail has many characteristics which have nothing exactly in common with the skin, so that it must receive special consideration by itself.

The "hand in everyday use," when quite dry, and when it has not been washed and has not perspired for some length of time, gives off extraordinarily few germs when rubbed with a dry towel; this may seem somewhat strange, as one would expect to obtain a large number of germs from a hand which had not been washed. But, to obtain even an approximate idea as to the true septicity of the surface of a hand, either the hand itself, or the thread which is used to remove the septic material from the hand, must be moistened.

If the hands were washed[1] in *cold water* with good toilet soap, but without in any way being scrubbed, quite a considerable number[2] of colonies sprang from the thread. But the number was still larger if the hands were then washed in *hot water*, or *water just hot enough to be still bearable*, and not even the use of a piece of soap appeared to diminish the number of germs.

If the hand, after it had been washed with soap and hot water, was dried with a towel (applied like a swab), the number of germs obtained from it was not substantially less than if no drying had taken place. But when the hand was well rubbed with the towel the number of germs was at once very much reduced.[3]

the superficial layers of the better kinds of soaps that germs are to be found, whilst the deeper parts are sterile. The most superficial layers were accordingly removed with a sterile spatula. Whilst the investigations were proceeding, portions of the soap were removed for bacteriological examination, so as to check the results, but no germs were found to be present.

[1] The washing just occupied five minutes.

[2] As I have already mentioned, these numbers can, of course, only be considered relatively.

[3] Here, as in all examinations of the dried hand, the hand was freshly moistened with sterilised water before the thread was drawn

The choice of the towel was also of importance here, since smooth towels of fine texture were almost ineffective; coarse towels answered the purpose best.

If the hands were washed with *a cake of soap in hot water, together with the thorough use of a nail-brush*, the number of germs was less than if they were washed without the use of a brush.[1] The number was even further reduced, and, upon the whole, the best results were obtained, if this procedure was followed by the hand being rubbed for some minutes with a coarse dry towel.

It seems paradoxical that, after the use of soap and hot water, more germs can be obtained from the hands than is the case after the use of cold water, with or without soap. But the reason is clear, if one considers that the germs do not lie on the surface of the hand, as on a glass plate, but are stored in fat between the epidermic scales, and that what first enables the germs to be freed from the hand is the vigorous maceration of the outer layer of the epidermis, together with a partial removal of the fatty substance. Binaghi obtained similar results in examinations of the surface of the body after baths.[2]

It follows, therefore, from these examinations, that the principal provisions for a successful mechanical cleansing are—*hot water, soap, a scrubbing instrument to be used during the washing, and a rough towel with which to rub the hands.*[3] This last point is indeed

through it, because, as has already been mentioned, it is more difficult to detach the germs from the hand when it is dry.

[1] Brushes with coarse bristles—so-called vegetable brushes. The ones employed here had not been used before.

[2] *Method.*—The skin is scraped with a sterilised knife-blade, and nutrient media inoculated with the scrapings. *Policlinico*, Nov.–Dec, 1897.

[3] This is the point at which we may deal briefly with the apparatus necessary for carrying out the process of washing. Water from the main is never sterile, nor is hot water any more so, unless special arrangements are made to ensure that it is. (Bircher used condensed steam.) The germs which are present in running water are, however, almost entirely harmless in character, so far as concerns our wounds. If, in experimental investigations, it is of importance that the water used should be sterile, this is not the case in ordinary practice. *It is much more*

important, and must be insisted upon, and all the more for this reason, that in visiting different hospitals and Kliniks I observed that in many places they omitted thus to rub the hands with a dry towel, so that the hands were treated with Alcohol, or an antiseptic lotion, directly they had been washed. By the help of cultures it is very easy to convince oneself that this towel can remove from the hand, after the latter has been washed thoroughly for some considerable time with a variety of different sterile materials, colonies of germs, to an extent that would scarcely be anticipated from the use of one of the ordinary methods of examining the hand. For anyone who doubts them, the bacteriological conclusions may be easily confirmed macroscopically, showing that well-washed hands can still always rid themselves of a fairly considerable portion of their epidermis, when rubbed in this way with a towel; and the experiment with Indian ink shows that, apart from the epidermic cells, still other corpuscular elements, which have withstood the washing, can be removed from the hands in this way.

A successful mechanical cleansing must thus be compiled from a number of different component parts, which, theoretically, stand widely apart from one another.

1. *The fatty material on the surface of the skin must be*

important that the hands should not be washed in standing water, but in running water. By washing in running water, *i.e.* right under the tap, the danger is avoided of those germs which have already been removed being again rubbed into the hands; but what is more important still, is the greater intensity produced by the mechanical effect of the jet of water, arranged in the form of a spray. The adjustment of surgical washing conveniences has, to a considerable extent, called into play the mechanical genius of many surgeons; the manifold contrivances, as ingenious as they are complicated, for using the feet to obviate the necessity of touching with the hands either the water-tap, the turnover basin, or the plug in the sink, bear eloquent testimony to this. But the simpler the arrangements, the better they work. If one cannot accustom oneself to allow the attendants to open and shut the water-tap for one, levers, which are curved at the end, will be found to answer the purpose, as they enable the water to be turned on and off by means of the elbow. Further, the arrangement used by Kocher is undoubtedly the best and simplest—a large trough for the whole series of preparatory washings, and in which, as a general rule, standing water never accumulates.

detached and removed, because, so long as the surface of the skin is not freed from this fatty material, the cleansing of the hands, by means of water or aqueous solutions, is interfered with.

2. *The outermost epidermic layers must be loosened*, because the septic material lies on the cells themselves or between the corneal elements of low vitality; this loosening is principally achieved by causing the cells to swell up.

3. *These outermost epidermic layers, on which the germs are present, must be removed by means of rough media*, which must be able to thoroughly wipe out the small grooves, and the openings of the sebaceous glands.

It may be worth while to glance at this question of *the removal of the fatty material* from the surface of the skin. It is clear that this of itself is not capable of freeing the hand from germs, even if one can accept the view that the germs are partly suspended in the fatty material on the skin, and are to this extent removed along with it. The principal part, however, which is played by the removal of the fatty material from the surface of the skin is that of a *preparatory* measure, and its efficacy will lie in this, that it lays the hand fully open to be acted upon by the mechanical cleansing, and by chemicals. One ought to doubt this, after the unfavourable results from experiments carried out with fat-solvents such as Ether, Ethyl Chloride, Chloroform, Benzene, etc., as reported by Fürbringer and others—results which I must fully corroborate from experiments of my own, in so far at least as they were conducted on the lines followed by these authors, *i.e.* from consideration merely of the open space under the nail.[1] Fürbringer's assumption is at all

[1] Schleich (p. 83) mentions experiments in which sterile results were obtained by repeatedly rubbing the hands and dipping the points of the fingers in a mixture of Chloroform, Ethyl Chloride, and Ether! The material to be examined was removed from the dry finger by means of a platinum needle! I shall refer later to the general inefficiency of bacteriological technique; the only point to be emphasised here is that scarcely anything can be obtained in this way from a skin-surface which is quite dry; even the "hand in everyday use," when dry, yields but few germs—even when rubbed.

events correct, that, with these rapidly evaporating media, the fatty material, which has been dissolved, will be partly deposited again where it was before; therefore a removal of the fatty material, by means of these fluids, cannot be otherwise than incomplete. Thus it is only possible to completely free the surface of the hand from this fatty material if the fat is not merely dissolved, but is removed into the bargain. In the case of investigations which deal exclusively with the effect of the removal of this fatty material, neither these quickly evaporating fluids, nor Alcohol, to which the skin is not indifferent, are suitable, for the reasons which I have already given; nor, lastly, is soap suitable, for, whilst it loosens the epidermis, it causes it at the same time to swell. To bring about a pure and simple removal of this fatty material from the skin, the use of pastes or pulpy substances answers best, and Plaster of Paris and Kaolin are particularly to be recommended for the purpose.

If moist Plaster of Paris is smeared on the hand, and under the nails, till it is completely dry, and the plaster is then removed with the help of water (but without rubbing, in the sense of a mechanical cleansing), the dry, rough surface of the hand is now unpleasant to the touch; it is completely freed from the fatty material. The bacteriological examination, conducted by means of the thread method, of a hand, which had thus been treated and again moistened, showed that fewer colonies, and at all events far fewer germs, could now be obtained than from the "hand in everyday use." If the hand was now washed with hot water and soap (or Bran water), the number of germs obtained again increased considerably for the first few minutes. But, in order to obtain good results, this mechanical cleansing did not require to be continued for so long as would have been the case had the previous treatment with Plaster of Paris been omitted. If the act of cleansing was extended to five minutes, and the hand was well rubbed with a rough towel, it was then shown to be possessed of a smaller number of germs than was the case after the employment of any other method.

The explanation of these experiments is very simple —the Plaster of Paris was able to remove the fatty material from the skin, and simultaneously to take away with it the loosely adherent corpuscular impurities.[1] Further septic material could not be obtained from the hand at this time, for it was firmly lodged on or between the outer epidermic layers, or in the interstices of the skin, into which the Plaster of Paris could not be induced to penetrate. If now the outer epidermic layers are made to swell by washing with hot water and soap, and are removed, the number of germs is again increased; the mechanical cleansing is, however, essentially facilitated and shortened by this complete removal of the fatty material.

These experiments have no practical value, since the hands would never stand a constant repetition of this procedure. But a first-rate substance for removing the fatty material from the hand, and which does not injure the hands, is *Kaolin—Bolus alba.* I first employed this substance in the form of a powder, *i.e.* powdered Kaolin ground on the moist hands, and several times renewed. But this procedure implies scattering the Kaolin, and a waste of material. Both of these are avoided if the Kaolin be used as a *paste*;[2] this, with the help of cold water, but without the use of any scrubbing instrument, removes the fatty material from the surface of the skin quicker than Alcohol, and quicker even than Schleich's Stearine paste. The bacteriological results were similar to those in experiments with Plaster of Paris. This Kaolin paste, which does not injuriously affect the skin, can therefore be warmly recommended as a means of removing the fatty material from the skin, as, by its action, it removes

[1] Everyone who has had much to do with the application of Plaster of Paris bandages knows to what an extent Plaster of Paris can cleanse the dirtiest part of our hands—the open spaces under the nails.

[2] After numerous experiments to ascertain the right consistency, the following composition proved to be the most suitable: 1 kg. of Kaolin to 700 cm.^{3} of water; mix together in a dish to form a uniform paste. The paste is of the consistency of thick honey. It should be ground on to the hands, and, after being washed off, should be several times renewed.

the loosely adherent portions of dirt, which still remain, together with the fatty material.

By this method of employing the Kaolin the skin will be completely freed from its greasy material. If hot water be simultaneously used, the outer epidermic layers will also be loosened to some extent, though the employment of the Kaolin does not dispense with the necessity for using soap, and an instrument to scrub with—*i.e.* a thorough removal of the fatty material from the skin, loosening of the outer epidermic layers, and removal of the loosened epidermic elements of low vitality are three factors of great importance to everyone, which, in part at least, call for the employment of special measures. In my opinion, the removal of the fatty material from the skin must precede the loosening of the epidermis, if both procedures cannot be carried out at one and the same moment, since a mechanical cleansing is not to be thought of—for the recesses of the skin, at least—until the fatty material of the skin, which envelops both the coarse and the fine particles of dirt, has been removed.[1]

Soap, however, causes a simultaneous removal of the fatty material and a loosening of the epidermis. In the case of the former the process is slower and less complete than if the Kaolin paste, for instance, is used. The possibility of its accomplishing both these processes depends upon its preparation, *i.e.* on the nature of its composition. In the experiments mentioned above good toilet soap was used. But it is clear that, in order to bring about a loosening or swelling of the outer epidermic layers, a neutral or superfatty soap is less suitable than an *alkaline soap*. The green soap used in many places seems, therefore, to be very suitable for the purpose. This soap contains free alkali,[2] and even though it be true that this makes it tenacious, and of a

[1] More particular reference will be made later to the value of Alcohol, and to the position it occupies with regard to the removal of the fatty material from the skin.

[2] So soon, at least, as it comes in contact with water ; according to the Pharmacopœia, the pure perparation should contain no free alkali, or mere traces of it.

viscous consistency, I do not, like Schleich, consider this to be a disadvantage ("for the bristles of the brush and the sand act more gently on the skin the more their extremities and edges become enveloped by a mucilaginous covering of soap"), since the brush bristles will not become clogged with undissolved soap during the whole period of the mechanical cleansing, if the process be properly carried out.

Fürbringer did not obtain favourable results with strong alkalis (5–10 per cent. Caustic Potash and concentrated solution of Soda) which caused a very considerable swelling of the epidermis in a short space of time; he always obtained comparatively numerous colonies. One can quite understand this, since he examined the hands and fingers, which he had subjected to this treatment, after a cursory rinsing in water. Thus the epidermic layer on which the germs were present was merely loosened, though not mechanically removed, after the process of swelling, and the fact that a large number of germs could subsequently be removed from the skin shows the value of the alkalis.[1]

Thus, although the soft soap appears to be suitable (it is perhaps this ill-sounding name which brings the preparation into discredit), it still greatly depends for its power of action *on the manner* in which it is employed. The brush is, as a rule, rubbed either on the surface of the soap or merely in the fluid which is always to be found in the soap-dish, and is thus dipped in a concentrated solution of soap. In either case the soap has little chance of penetrating into the skin, and it can thus have but a partial effect. For some time past we have made use of a procedure which has also recently been recommended by Odebrecht and Schleich. About a tablespoonful of the soap is thoroughly ground into the hand for one to two minutes, until the surface of the hand appears to be fairly dry; water is now added to this, the hands being still rubbed all over as before, and finally they are vigorously scrubbed with the brush.

[1] *Loc. cit.*, 103.

It is easy to convince oneself of how deeply the soap is made to penetrate by this manner of procedure, for if the hands, which have thus been treated, are thoroughly rinsed in hot water and then rubbed again for some time, a lather of soap is formed anew.

The bacteriological examinations gave the following results:—If the investigation took place immediately after the washing, for which no brush was used, the number of colonies was astonishingly high (350, 780, 860). The result was at once changed if a brush was used, but more particularly when the hand was rubbed with a dry towel. If this latter detail was omitted, the results were not materially altered whether the soap was spread upon the brush directly, or was first rubbed on the hand and then later worked in with the brush; but if, in addition, the hand was vigorously rubbed with a dry towel, the number of colonies obtained was relatively smaller when the soap had first been rubbed on the hand than when the other form of procedure had been employed. These experiments show that the former manner of procedure loosens the epidermis more thoroughly than does the latter, and, therefore, more readily enables the mechanical operation to remove the outer epidermic layers.

I was unable to observe, either in the case of others or in my own case, that the hands were in any way injured by the frequent use of green soap—a complaint which is frequently made. For the last twelve years I have daily cleansed my hands in this way at frequent intervals, without having noticed any alteration of the skin which should necessarily be ascribed to the use of the soap. The dry condition of the skin, about which people complain, is just exactly what we have in view in the process of cleansing—the thorough removal of the fatty material from the skin. The hand of the surgeon, which has thus been thoroughly freed from its fatty material, requires indeed a considerable degree of cosmetic attention. By simply washing and disinfecting the hands, we are merely taking half-measures to secure their purity, and the care of the hand is a principle

which stands in close relationship to the cleansing and disinfection of the hand.

The importance of this loosening and maceration of the outer cell-layer is best shown by those media which —with or without soap—are able to promote and to expedite these changes, and amongst others to be included in this category are *Bran infusion*[1] and *Size*.

If the hands were washed tor five minutes in *Bran water* (subsequently with a simultaneous use of soap), the number of germs obtained from the undried hand was relatively larger than when ordinary sterilised water was used; but, after a thorough rubbing with a rough towel, the surface of the hand was found to be less septic than when washed with ordinary water. The same process with Bran water, together with the aid of soap and a brush, if prolonged to ten minutes, yielded on several occasions even sterile results.[2]

Size also loosens the epidermis by its action ; anyone who has had much experience of Size in orthopædic work knows the feeling of the softened hand which occurs after the half-dried Size has been removed by a process of washing or of rubbing. The experiments with Size, which had been rubbed into the hands for some minutes, till it was finally rubbed into the form of small cylinders, gave results similar to those obtained after washing with Bran water.

It is impossible to familiarise oneself with Size, or Bran water, in the ordinary practice of cleansing the hands, as their use would complicate the proceedings. Bran water certainly deserves notice when a special need arises, such as in the case of rough hands; a

[1] Bran infusion, which we often use to moisten dressings, is thus prepared :—A small bagful of Bran is well squeezed and kneaded with the hand in hot water for about five minutes. This solution is alkaline, it contains common salt and a fair amount of albumen (gluten). Ahlfeld has already recommended obstetricians to use baths, containing Bran, for their hands in the evening.

[2] It is certainly important, when using such macerating media, that the drying process of the skin should always be undertaken with fresh parts of the towel or with fresh towels, for otherwise the germs, which have already been removed by means of the towel, will be again rubbed into the skin.

compress of Bran water, in combination with an antiseptic lotion, can also be recommended as a means of preparing the field of operation, on account of its macerating, though non-irritating properties.

Schleich's Marble-dust soap, which reduces to one act the removal of the fatty material from the skin, the loosening of the outermost layer of the epidermis, and its removal from the hand, brings us to the question of the use of a *scrubbing instrument* in the process of washing. The *brush*—that old-fashioned, one might almost say classic, instrument—has fallen into discredit. To read the chapter by Schleich on the subject of brushes is to experience the same sensation stealing over one which accompanies the reading of Dante's "Hell." It is quite natural to deviate from the beaten track, pursued in former years with regard to these brushes; but one should at least ask oneself if the drawbacks are really so great when the brushes are properly used, and if there is an equivalent substitute with which to replace the forbidden brush.

With regard to the first point, it is unanimously held that the brush is the most difficult to cleanse of all the instruments we use. But for some time past, in the hospitals with which I am acquainted, a further stage has been reached than that cited by Schleich as occurring in what he regarded as model hospitals, where he says, "No one sees more than five or six brushes in simultaneous readiness for use, and anyone can observe that the same brush is used for a variety of purposes." A statement which seems to me to offer strong evidence *in favour* of the brush is that innumerable germs could be bred from brushes, which had previously been sterilised, after they had been used. This I consider to be favourable to the brush, *because these germs are less dangerous when in the brush, than when on the hand from which they have been removed by the brush.* Blood, pus, and all coarse dirt (such as were used in these experiments) scarcely ever come in contact with the brush in practice. A surgeon does not wash his hands in the operation theatre for the first time in the day, and when the hands are soiled

with blood and pus they are, with us at least, first rinsed and washed before they are again cleansed with the brush.

What exactly do we aim at in our use of the brush? For one thing, to remove the loosened outer epidermic layer; and further, to properly scrub the ridges and recesses of the palm of the hand, the small creases on the dorsum of the hand, the space under the nail, and the matrix of the nail.

The corpuscular elements, which are removed by the brush, are partly dispersed in the air and partly enter the brush, *i.e.* upwards along the fibres to the wooden back of the brush. A simple experiment can illustrate these conditions.

If the fibre-ends of a moist brush are dipped in fluid Indian ink, and the brush is then rubbed on the dry, or slightly moist, hand, a portion of the Indian ink undoubtedly passes to the hand, and there remains suspended in the recesses of the skin—as will be understood from the experiments mentioned above. But if a brush, which has thus been dipped in Indian ink, is once rubbed here and there under the water-tap, no Indian ink is conveyed even to the dry hand.

If soap and hot water are used (soft soap rubbed into the hand), the brush, even though fully impregnated with Indian ink, cannot convey the black particles to the hand, if it has previously been washed in plenty of water; even under the microscope the deeper parts of the skin surface are found to be free from Indian ink granules.

The fibre-ends of a brush, which has been dipped in Indian ink and repeatedly used for scrubbing, when examined under the microscope, still show numerous black particles to be present; but, after washing the hand with soap, these fibre-ends are entirely free from the presence of black particles; the brush then generally contains relatively few black granules, and these are situated on the parts of the bristles which lie in close proximity to the wood of the brush, or on the wood itself.

If microscopic information is sought with regard to the nature of the coarse impurities present on brushes, which are much in use and are taken little care of, these impurities are always found to be present in the neighbourhood of the wood, and not on the ends of the bristles. They consist of filaments of cotton-wool (wadding), but more especially of soap. This is particularly the case if the cleansing has been carried out with soft soap, and if it has been primarily smeared on the brush in large quantities; sand and marble dust are also found to be present, in the event of their having been used to cleanse the skin. Cotton-wool filaments are not found on brushes which are kept solely to be used by the surgeon to cleanse his hands (where no proper supervision is exercised, a brush will naturally be used as "a maid-of-all-work"), and the soap can easily be got rid of again by washing or boiling the brushes, without their sustaining much damage.

The assertion that, by the use of a brush in the process of washing, septic material will be rubbed into the hands, or will be conveyed to the hands, can also be discounted by means of bacteriological experiments.

If a brush is rubbed with a large quantity of *Prodigiosus* culture, scraped from the surface of Agar, and the hand, with the assistance of soap and plenty of water (whether sterile or not is here immaterial), is then well scrubbed with this germ-infected brush for the space of five minutes, and its surface is then washed and dried, the bacteriological examination of the hand by every method proves that, amongst the more or less numerous colonies which can be obtained from the surface of the hand, there are no *Prodigiosus* cultures, or—in a few exceptional cases—that the presence of single colonies can be confirmed. If the brush fibres are examined for cultures, it is also proved that *Prodigiosus* cannot be bred from the fibre-ends, though it is still possible to do so from the fibres at their origin from the wood of the brush.

Thus the great septicity of a brush, which has been used on one occasion, merely proves that numerous germs have

been removed from the hands, and that the brush is thus a good cleansing instrument. The septicity of the brush increases with its use, and though it is not altogether impossible that such a brush conveys germs to the hands, it will, as a general rule, pick up and remove them, according to ordinary physical laws.

It is far from my present intention to say anything in favour of the old custom of using any brush, which happens to be lying about, for cleansing and disinfecting purposes. Those exceptional cases, in which germs from a brush may settle on the hands, must always be kept in mind, especially for this reason, that in a bacteriological examination one often works with greater concentration than one does in practice. A brush, however, like every other instrument, can be sterilised by boiling, which can scarcely be doubted, even by anyone who has done no bacteriological experimental work in this direction. Cheap "vegetable brushes" stand this boiling process dozens of times without being too much damaged; and, at all events, they stand it better than the more costly brushes made from pigs' bristles, which soon become soft from boiling. Thus the question of price need not weigh in the scale, even in the case of infirmaries with small endowments, and economical private hospitals.

Our procedure in the operating theatre is as follows:[1]—The brushes are branded with the thermocautery on the wood forming their back (a cross, or a notch), as they are later to serve different purposes. They are boiled[2] in the morning, and are then deposited in antiseptic solutions. According to the marks on their backs, the brushes are placed in glass receptacles which are labelled "mechanical cleansing" or "antiseptic cleansing"; thus, the one set serves to treat the hands

[1] It resembles what Mikulicz recommended at the Surgical Congress of 1898, but it was introduced quite independently of that.

[2] If a brush is boiled in water, or in soda solution, the boiling process must last ten to fifteen minutes if sterility is to be ensured; if it is boiled in antiseptic solutions, as occurs, for instance, in the Poliklinik, then sterility will be attained in from five to seven minutes, if the brush is not too much soiled with coarse dirt.

with soap and water, and the other set is used in Alcohol or in the antiseptic lotions. The brush used for the first mechanical cleansing is put aside, *i.e.* is again boiled before it is used a second time. If several operations follow one after the other, and if one has not been working with any very seriously infectious material, such as pus or fæces, it is unnecessary to cleanse the hands with the same degree of care, because there are fewer germs present on the hand than when the first cleansing took place. The brushes which are used for the later mechanical cleansings are again immersed in the antiseptic solutions, and are thus later made use of again and again. Even though these disinfecting solutions cannot, in the period of time between two operations, kill all the germs on the brushes, which are, withal, little soiled, we now know that their action on the vital energy of the germs is not without effect.

I have often examined for cultures the bristles of brushes which have been once used and then replaced in antiseptic solutions,[1] and have never been able thus to obtain germs which were capable of further development, after a sojourn of three-quarters of an hour in a disinfectant. The results obtained with Lysol, or with Carbolic, which cannot be completely eliminated in a bacteriological examination, are not absolutely certain; but in the case of Corrosive Sublimate solution (0·1–0·5 per cent.) there is no such uncertainty. It is important that the antiseptic solutions, in which the brushes lie, should be frequently changed. The exceptional cases, to which I referred above, but which seldom occur, in which some germ or other may be conveyed to the hand by an excessively contaminated brush, are to be avoided with absolute certainty by the use of such a procedure. Twelve to thirty brushes are sufficient to do all that is requisite, even in the case of several operations, and, besides, the brushes can be boiled even

[1] Lysol solution is best adapted for this purpose, because it is able to emulsify fat to some extent; it can, moreover, thus be used in a highly concentrated form without injuring the brush or the hands.

during the operation without the need of procuring special assistance.

If these brushes are to be condemned, an efficient substitute must be found to replace them, and in that case I shall be the first to forsake the brush.

Neither *Wood-fibre* nor *Loofah* supplies such a substitute. Apart from the fact that these materials cannot wipe out the environment of the nail to an extent which is approximate to what the brush can do, the effect of their action on the palm of the hand is considerably less powerful, *i.e.* to remove the outer epidermic layers, and to scrub out the folds and recesses. Wood-fibre and Loofah are far less efficient materials than a brush, with its bristles directed perpendicularly towards the surface of the hand. Hands with excoriations and projections, for the cleansing of which Reinicke recommended the use of Loofah, are much more difficult to render free from germs than are hands in good condition, and consequently they stand much more in need of a brush. It is better in such cases to wear impermeable gloves, than to be influenced by a wrong consideration for one's personal convenience.

According to my experiments, *Pumice-stone soap*, *Sand soap*, and even *white sand* (*Sänger*) in conjunction with soap, are only of service when used simultaneously with a scrubbing instrument, when they certainly act more successfully than pure soap. With special regard to the use of white sand, my experiments did not corroborate Reinicke's results,[1] not at least to the extent that, when the brush was used in addition, its influence was always visible, *i.e.* that if, along with the soap and sand, a brush was simultaneously employed, relatively fewer germs were always obtained from the hand, as also from the open space under the nail, than if the use of the brush was omitted. The cause of this difference in the results may have been due to the different manner in which the nail was cleansed—a subject on which I shall enter more fully later in this treatise.

[1] *Loc. cit.*, p. 1193.

Of all the modifications of the mechanical cleansing, the most noteworthy is *Schleich's Marble-dust soap.* Its pretensions are as follows:—To afford the possibility of compressing into one act the dispersion of the fatty material of the skin, the loosening and the removal of the outer epidermic layer of the skin, and, in addition, the closing of the excretory ducts of the sebaceous glands and of the hair-follicles—in other words, to *produce in one act an absolute sterility of the surface of the hand on every occasion.* For the realisation of this ideal state of affairs, all that is necessary is a cleansing of the hands with the soap and sterilised water lasting about three minutes, to be followed by a vigorous process of drying and rubbing with a sterile towel.

Such promises must be received at first with some degree of reserve, even when they are advanced in the original and attractive literary style which characterises Schleich's writings; for one knows that manifold sources of error, for which experimental investigations with regard to the cleansing of the hands form an advantageous soil, play a part even in such brilliant results, and that these will sooner or later come to be revealed. But Schleich's soap composition demands a closer inspection, for the reason that the premises for the attainment of a successful mechanical cleansing of the hands are rightly held theoretically to be wide apart. Again, even though the soap does not strictly correspond to what its inventor promises for it, still Schleich deserves much credit for having once more forcibly drawn attention to the importance of the mechanical cleansing of the hands, and for having placed its component or individual factors on a sound working basis.

The soap is composed of good yellow resin soap, ammoniacal wax and stearine pastes, and coarsely granular marble dust (sieve No. 4; as coarsely granular as sea sand).

The Ammonia acts as a fat-solvent and saponifier, as does also the stearine paste, which, according to

Schleich, is eminently capable of emulsifying fat.[1] The loosening of the outer epidermic layers is procured by means of the soap molecules and the Ammonia; dirt and keratine are removed and swept away by means of the uniformly distributed marble dust, or, more properly, marble *sand*. The composite soap is sterilised for one and a half hours in a water-bath, during which it is vigorously stirred. As Schleich does not make use of antiseptic solutions, he uses sterilised water for washing, which stands ready in several basins. In order to cleanse the hands, the soap, which is of a doughy consistence, is removed from its receptacle by means of a sterile spatula, and is rubbed all over the hand till it is almost dry; the hand is rinsed several times in water, and once more rubbed with fresh soap; in between times the space under the nail and the matrix of the nail should be cleansed with a sterile nail-cleaner, and after the hand has been washed for the last time it is dried with a clean towel; the hand has still to be "polished" with a second towel, *i.e.* the residual wax is to be vigorously rubbed into the hand.

According to Schleich, the hand, as also the environment of the nail, was always[2] rendered sterile by this method of procedure, even when earth from potatoes, or pus of every sort and description, was rubbed in and allowed to dry on the hand before it was cleansed.

The tests to which I subjected these results (I used my own hands and those of another person) proved *that the hands were never thus rendered sterile*, whether the "hand in everyday use," or the artificially infected hand, was chosen for the purpose.[3] The abundance of the germs was indeed considerably lowered, but neither the nail environment nor the surface of the hand proved to be sterile—even when the procedure was extended to fifteen minutes.

[1] As one knows, this is attained with stearic acid.

[2] With one exception, where there appeared "one culture at the end of eight days" (p. 134).

[3] The soap was procured from the firm recommended by Schleich—Kohlmeyer of Berlin.

To anyone who is familiar with the technique requisite for such experiments, and who has read Schleich's book, this difference in the results will come as no surprise. The technique, which the author himself there describes as "a brilliantly exact method of bacteriological testing,"[1] is seen at a glance to be totally insufficient. Schleich employs a platinum loop, or platinum needle,[2] for removing from the hand the material to be tested, and stab cultures in nutrient Gelatine.[3] He removes the material to be tested from the dried, "polished" hand. But I have already pointed out that but few germs can be obtained from the dry surface of the hand, even when the means employed for removing the material are of the most efficient description, and when the hand itself has not been washed.[4]

Some examples may be given here from among the large number of experiments which were carried out according to Schleich's method of cleansing. Schleich holds that his soap composition acts mechanically with pre-eminent efficacy because it should be able to remove the outer epidermic layers, and he seeks to prove this by means of the following experiment (p. 100). A concentrated solution of methyl violet is applied in streaks to the dorsum of the hand; when the colouring matter is dry, at the end of fifteen minutes the pigmentation of the skin disappears as the result of the use of his soap composition, owing to the tinged epidermic scales having been removed. In testing this experiment, it struck me that the soap was already stained a deep violet in the first few seconds, at a time when there could be no

[1] *Loc. cit.*, p. 137.

[2] There is no explanation given as to how this was employed, especially in removing the material to be tested from the *surface* of the skin.

[3] And yet the number of the colonies is to be determined by this means! (p. 128 *et seq.*). As has already been mentioned above, only material removed for testing, which is kept at incubation temperature (Bouillon, Agar), can be employed as a criterion.

[4] It is worthy of note that even in experiment xx. (p. 133), where "ordinary water running from the tap" was used, a negative result was obtained with all the material removed to be tested. Ordinary running water is never sterile!

question of any appreciable mechanical effect. *Result*—The streaks are more quickly and radically removed by means of a swab, moistened with Ammonia, than by means of the soap composition; therefore the action of the soap here is not a mechanical one but a *chemical* one.

The assertion, that the soap should be able to remove the outer epidermic layers, was easily tested. I was aware that Corrosive Sublimate was absorbed only by the most superficial epidermic layers. If, after disinfecting the hand with Corrosive Sublimate, the surplus Corrosive Sublimate is washed off, and the hand is treated with Ammonium Sulphide, the skin becomes of a deep brown colour. Schleich's soap was unable to remove this discoloured outer epidermic layer, even though the hand was washed for as long as fifteen minutes; the colouring matter retained its original intensity, especially in the folds and creases. But, with the help of pumice-stone, the layer of epidermis, which was thoroughly saturated with Hydrargyrum Sulphuratum, could be removed completely in a few seconds.

Still more instructive was the experiment when reversed. After washing the hands with Corrosive Sublimate, they were treated for ten minutes with Schleich's soap composition in the prescribed manner, washed, and then submitted to the Ammonium Sulphide test, when the brown discoloration of the skin made its appearance indeed, and was fairly pronounced, particularly in the palm of the hand, with its numerous folds. This experiment not only shows that Schleich's soap is unable to completely remove[1] the most superficial epidermic layer (especially in the recesses), but that *the action of the Corrosive Sublimate penetrates deeper than does the soap composition.*

I may mention, with regard to parallel investigations, that the (extended) method of Fürbringer not

[1] Anyone who examines with the stereoscopic microscope the "marble dust" of Schleich's soap composition, and the surface of the hand, will understand this at once.

only proved to be much more successful, and produced sterile results after fifteen minutes where germs were still always obtainable after washing with Schleich's soap composition, but that even the reviled brushes yielded better results, if the cleansing lasted five minutes.

It is thus quite clear, without adding anything further, that the hand, in the case of gross impurities adhering to its surface, can in general be more quickly relieved of these by the use of Schleich's soap composition, because by this method an extensive surface can be mechanically scrubbed all at one and the same time; whilst in a cleansing performed by means of a brush the attention paid at the moment is but to relatively small portions—one after the other. What the soap composition excels in in extent, the brush, when properly used, abundantly makes up for in intensity.

The results with Marble-dust soap will be considerably improved on by the co-operation of the brush, banned by Schleich; yet this procedure cannot be often employed without injuring the hands, since the chips of marble are somewhat pointed and sharp; the environment of the nail especially suffers to a considerable degree in this way. I was, as a general rule, able to observe, both in my own case [1] and in the case of others, that even without the use of a brush the nail matrices easily became roughened and cracked—no doubt in consequence of small wounds caused by the chips of marble; otherwise the hands remained smooth and soft.

It does not lie within the province of this work to enter on the subject of Schleich's *experimentum crucis*, or the good results he obtained in operations, which were indiscriminately performed, one after the other, on purulent and sanious cases, strumous cases, or herniæ. There are still other factors which weigh in the scale with regard to the issues of such operations, and the reports on the cases are, in general, constructed on too aphoristical a principle to allow of their critical examination.

[1] I used the marble-dust soap for the space of five months.

Thus, even supposing that Schleich's soap composition has great advantages over other materials in the way of bringing about a mechanical cleansing, yet this carefully compiled preparation is not successful in rendering the surface of the hand sterile "with certainty and on every occasion," nor can it replace the brush.[1]

B. *The Cleansing of the Nail Environment*

Most of the investigations which deal with the question of cleansing the hands have exclusively, or principally, confined their attention to the *environment of the nail.* This is but right, in so far that this region, and especially the open space under the nail, is the most difficult to obtain access to for cleansing purposes, in the bacteriological sense of the word, and this for reasons which have already been indicated. But investigations with regard to the cleansing of the hands, which concern the space under the nail alone, are one-sided, and their conclusions do not adequately deal with the actual conditions—in the case of surgeons at least—since the region under the nail only comes in direct contact with the wound if the operator uses his nail to scrape the tissues, or as an instrument with which to do the work he requires, a proceeding which should be avoided, however convenient it may be. There is but partial truth in the plea that during an operation blood and juices from the tissues can float away the germs from the space under the nail. One cannot suppose that after a thorough mechanical cleansing there is much septic material left at this region of suspicion to be still floated away. On the contrary, one must admit, with regard to the hand, that by reason of the maceration which is produced by the blood, as by all albuminoid fluids (for instance, even Bran water), the germs which still remain behind (and there are indeed always such) can be loosened in the course of time, so

[1] Reference will be made later to the significance of the wax contained in this soap preparation.

that they can be detached if the region of the nail is rubbed once by chance against the tissues, the instruments, or the threads. It is far from my purpose to wish to detract from the importance of a thorough mechanical cleansing of the region under the nail; but I believe that the importance of cleansing the hand *in toto*, and more especially the *condition* of the hand in general, has often been thrust into the background, and has been sacrificed to the space under the nail, because the germs could here be most easily identified, and such investigations require least time, material, and consideration.

In the case of the investigations carried out as above described, I have chiefly taken into consideration the surface of the hand, for I considered that, so long as these parts cannot with certainty and on every occasion be rendered sterile by a method of mechanical cleansing, it is impossible to sterilise the region of the nail. But the importance of attaining the highest degree of freedom from germs, in what is certainly the most extensive of the germ reservoirs—the environment of the nail—is self evident, and the following investigations prove how important is the manner of procedure in this region also.

First, a word as to the length, or shortness, of the nails. Ahlfeld recommends the complete removal of the free extremity of the nail, so that, from the nail-bed onwards, the skin lies open to the day. There is, at all events, to be said in favour of such a procedure that the mechanical cleansing is thereby facilitated, and that fewer impurities can, as a general rule, accumulate about the part.

I have for weeks thoroughly tested this procedure on my own hands, and cannot spare it a single reproach. It is quite impossible to pare away the nail so completely as to do away with every vestige of an open space under it; the nail-cleaner must always cleanse a small zone, and the instrument then digs anew a more extensive open space under the nail. Again, from another point of view, even though the free extremity of the nail is removed, the irregularities of the transition from the nail-bed to the skin of the finger remain,

and all care of the hand, together with the use of pumice-stone and other cosmetic substances, is unavailing to restore the surface of the skin to a condition of smoothness. Now if these rough corneal portions lie exposed, and come in *direct* contact with the wound and the instruments, they are more dangerous than if covered by the nail, so long, at least, as we know of no means of disinfection which will spare the body cells and destroy merely the bacteria. Compared with this, the nail of medium length provides a secure protection for the nail-bed and finger-tip, which is of importance even in the interest of the cleansing of the hands, which depends so essentially on the condition of the hand. Judging by my experiences, I must accordingly reject this plan of removing the whole free extremity of the nail, and must express myself in favour of a nail extremity of medium length—2–3 mm.

I need hardly say that the *individual condition of the nail environment* is of the greatest importance. I have been able to trace this in the investigations on my own hand. If the region under the nail and the matrix of the nail were rough and cracked (as frequently occurred after chemical and staining work in the laboratory), the mechanical cleansing had to be prolonged by three to five times its usual duration, if results were to be obtained that were relatively similar to those attained to at times when the region of the nail was well cared for and soft. It may well be that the great differences, which occur between the results of investigations carried out by means of different methods of cleansing and of disinfection, may arise in part from the fact that the condition of the nail environment was very different in the case of those whose hands were examined.

Another explanation of the differences which occur may lie in this, that the cleansing of the nail environment, especially of the open space under the nail, was undertaken during the course of different phases of the cleansing process. Fürbringer recommends that the nail-cleaner should be used *before* washing—thus when

the hand is dry. This procedure is decidedly to be rejected, as it injures the hard keratine layers at the point of transition, and can only have an æsthetic effect, but not a radical one. Even a carefully employed brush will deal with the coarse particles of dirt which supply the free extremity of the nail with a "mourning band." This "mourning band" does not, however, depend upon the presence of coarse dirt alone, but glistens through the dry skin of the cracked, rough, duskily tinged extremity of the nail-bed. Æsthetically, it is easily removed, and conventional society, in which great stress is placed upon appearances, has caused us since childhood to follow this procedure almost instinctively. To bring this about, the under surface of the nail must be somewhat roughly scraped with a sharp instrument, and then there appears the white border to the nail. But this cosmetic artifice does not satisfy the surgical conscience. Too much attention cannot be called to the fact *that what we dread, that what is most difficult to cleanse, in the surgical sense of the word, is not the comparatively smooth nail as such*, from which, and from its under surface, impurities can be mechanically removed without difficulty, *but is the skin, the rough end of the nail-bed*, which emerges from under the free border of the nail.

If one keeps this in mind, two different points will have to be considered as material requisites of a surgical nail-toilet. In the first place, that the instrumental cleansing of the nail should not take place until the space under the nail and the matrix of the nail are sufficiently prepared by the measures which have been taken to remove the fatty material, and to loosen the epidermis; and, in the second place, that the nail-cleaner should not be too acutely pointed, or too sharp,[1] so as not to further roughen the under surface of the free border of the nail; for if the nail-cleaner is too pointed, not only will slight wounds be caused, but new, deeper-lying fissure will be dug—reservoirs for germs—which are always more difficult to cleanse.

[1] A knife, and the pointed blades of scissors, are not suitable for the purpose of cleansing the nails surgically.

This cleansing of the nail with an instrument is comparable to the scrubbing of the surface of the hand with a towel. As the latter can first be successfully employed after the thorough preparation of the epidermis, this tallies with the cleansing of the nail. The same claims for preparatory measures hold good in the case of the nail as in the case of the surface of the hand—a thorough removal of the fatty material, and a loosening of the epidermis to as great a depth as possible. In fact, in the case of the nail, even greater thoroughness is required, since more fat is present, and the keratine layer is stronger here than at any other part of the hand. To what extent these principles are able, in themselves alone, to cleanse the environment of the nail, has already been briefly mentioned above, in connection with the experiments with moistened Plaster of Paris; the coarse dirt of the nail, in so far as it does not lie in or between the keratine layers, is completely removed by means of Plaster of Paris, as also by Kaolin. But a more efficient maceration of the rough epidermis is indispensable for the more tenaciously adherent germs. An effective application of potash soap, together with the use of a brush and hot water, acts capitally here. As was shown by a number of experiments, the use of the hottest possible Bran water, with the assistance of a brush and soap, is also successful in its action; the result being that a larger number of germs could again be set free, than by means of an ordinary washing, which still yielded but a trifling number of germs. When the region of the nails is particularly rough, washings with Bran, or Bran baths—used from time to time—can thus be recommended.

The metal or bone *instrument for cleansing the nails* should be smooth and not ribbed[1]—not that it may

[1] The trimming, or filing, of the free extremity of the nail undoubtedly forms part of the preparatory measures; for, from my experiments with Indian ink, the border of the extremity of the nail in everyday use is always rough, and can retain a fairly tenacious hold of corpuscular impurities. This measure should, however, take place *before* the hand is cleansed.

be more easily sterilised, but that it may be again cleaned by being wiped, or rinsed, whilst the nail is being cleansed. The instrument is, as a rule, carefully sterilised by being boiled, together with the other instruments, before the operation, and yet one is often oblivious of the fact that, during the toilet of the nail, the instrument becomes charged with more germs than are to be found on a non-sterilised, but carefully wiped, nail-cleaner. In hospitals of the most varying description, I saw how the "sterile" nail-cleaner, charged as it was with dirt from the nail, was wiped with the fingers as soon as a nail had been cleaned; in other words, the dirt which had just been removed from the nail was rubbed into the skin of the flexor surface of the fingers. Not quite so much harm is done if the hand is afterwards carefully washed with soap and a brush, because in this way such dirt as is adherent to the surface of the fingers can be again mechanically removed; at any rate, such a procedure is not wholly without risk, and it is advisable, after the cleansing of each separate nail, to wash the instrument each time under the tap, or to clean it with a little bit of muslin.

If the cleansing of the nail with this special instrument should not be undertaken before the general mechanical cleansing, neither should it be relegated to the end of this procedure, because the instrument only removes a portion of the septic material which has been rubbed free, and the remainder is strewn about in the space under the nail. It is well, however, not only to continue the mechanical cleansing with soap and a brush, *but to repeat the process of cleansing the nail with the instrument.* The following experiment (an example of a number of experiments carried out on similar lines) will serve to illustrate this:—

Maceration of the hands for two minutes in hot soapy water, without the use of a brush, in order to determine the primary degree of septicity.[1]—The Agar plate showed that 162 *colonies* were obtained from the

[1] Naturally, fresh sterile material (water, brush, nail-cleaner) was used in every phase of the experiment.

material removed by the nail-cleaner from under the nail of the middle finger of the right hand.

A mechanical cleansing for five minutes with hot water, potash soap, and a brush—the mechanical cleansing was confined to the environment of the nail, and to the whole of the rest of this one finger. 65 *colonies* were obtained from the whitish substance removed from under the nail of the middle finger of the right hand, which consisted chiefly of epithelium, with no coarse dirt to be detected.

The mechanical cleansing continued, as before, for five minutes—16 *colonies* were obtained from what was removed from under the nail of the same finger.

Mechanical cleansing continued, as before, for five minutes—whitish particles, still visible to the naked eye, could even yet be removed with the nail-cleaner, though these diminished with every washing and cleansing of the nail. Result—5 *colonies.*

I must add that the cleansing of the space under the nail was conducted intentionally with no greater vigour than what commonly occurs in a surgical cleansing.

These experiments thus show that the nail-cleaner is still able to remove septic material from the space under the nail when it is used a second or third time in a continued washing process, or, in other words, that the loosening of the epidermic layer, carrying the germs, always proceeds during a washing process.

If one takes these circumstances sufficiently into consideration, one is forced to acknowledge that the classic instrument for cleansing the space under the nail is far from being an ideal one; it clings far too little to the rough epidermic elevations, which are here present, to enable it to uniformly wipe out the part, as does the pliant towel on the surface of the hand. It is an instrument which has been borrowed from the toilet requisites of polite society, where it is much more a question of white nail-borders than of germ-free spaces under the nails.

The space under the nail can be but imperfectly

wiped out with a towel, for this is too clumsy in its action; but the following experiments prove how efficaciously *a pliant article* can polish even such a locality as this:—

A mechanical cleansing of the finger, and particularly of the region of the nail, for five minutes, just as in the experiment already described. The space under the nail of the *middle finger of the right hand* is now scrubbed out with a sterile *silk thread of the thickest size.* Result, 125 colonies.[1]

A second thread employed forthwith yielded 55 colonies, and a third thread 3 colonies.

A continuation of the mechanical cleansing for five minutes. The thread, worked like a saw throughout the space under the nail of the same finger, yielded 14 colonies.

No further material, visible to the naked eye, could be removed after this by means of the nail-cleaner, and when its point was dipped into fluid Agar the plate remained sterile.

A second silk thread, which was now used, allowed the development of 8 colonies, and a third thread resulted in a sterile plate.

In the same experiment, the space under the nail of the middle finger of the *left* hand was wiped out with a nail-cleaner, after the first mechanical cleansing, when the result was 75 colonies.

After the second mechanical cleansing, the nail-cleaner yielded 23 colonies, and the thread, which was then employed, 97 colonies; the nail-cleaner, when used immediately after this, yielded 2 colonies, and the thread, which was subsequently used, 16 colonies.

It follows from this experiment, which, when thus varied in every possible manner, always gave relatively the same results, *that the wiping out of the space under the nail with a rough but pliant medium is far more efficacious than is the work of the nail-cleaner, and that*

[1] The dry sterile threads were not fingered, but were held by an assistant between two clamps, and the space under the nail was thus wiped out by a sawing movement of the stretched thread.

the efficacy of the thread is increased by the progressive maceration of the epidermis.

Moreover, nothing in the shape of cost or of detail stands in the way of this manner of carrying out the mechanical cleansing of the nail. A similar result can be attained with a slender cord of hemp, which can be sterilised with the dressings. This procedure seems, indeed, to be *a priori* more reliable, since the same piece of thread is not to be used more than once—no more, in fact, than is the towel which has once been used to energetically scrub the surface of the hand. The procedure may be simplified in this way, that a longer piece of thread (about 1 metre) can be utilised to cleanse all the nails in turn. I attach the one extremity of the silk thread, which has been boiled in Corrosive Sublimate solution, to the water-tap; the dependent thread, up to the time of its use, dips into a vessel containing Corrosive Sublimate solution which stands below, and during the mechanical cleansing the region under the nail is scrubbed out by a sawing movement, whilst the thread is put on the stretch by means of the unemployed hand. It must be kept in mind here, that our object is the removal of the germ-laden keratine substance in the space under the nail, and that the germs, which may be conveyed by chance to the hands by the thread, lie loosely, and can easily be removed again by a subsequent cleansing with a brush.

One knows that in cleansing the nail special attention must be devoted not only to the space under the nail, but also to the *matrix of the nail.* But whilst, in the case of the former, the difficulties to be surmounted can be but partly eliminated by attending to the state of the hands, in the case of the matrix of the nail the cosmetic attention bestowed on the condition of the hands is of more decisive importance. Nail matrices, which have been well cared for, and are neither cracked nor crannied, can be fairly well mechanically cleansed with soap and a brush (in hot water), and all that remains to be done is to attack the part with a fairly pliant nail-cleaner.

In experiments, corresponding in their arrangements to those which I have mentioned above, it was impossible, in most cases, to remove by means of the nail-cleaner any further septic material from the matrix of the nail after washing the part for five minutes, followed by a cleansing with the nail-cleaner and by a repetition of the washing process for two minutes, *i.e.* the Agar plates and Bouillon tubes remained, as a rule, sterile, if the condition of the nail matrix was faultless; but if it was cracked or crannied, it was impossible to obtain such a result by means of a cleansing process of this duration.

C. *Individual Factors*

Thanks to the attention which in latter years has been bestowed on the hands engaged in operations, there is now but one opinion held with regard to all methods—*That the cleansing of the surface of the hand is less the result of one definite method, than the result of the individual manner and style in which this method is employed*; and to Ahlfeld belongs the merit of being the first to clearly express this idea.

This individual power is composed of different factors. The condition of the hands, and their individual peculiarities, certainly play a great part; but the keystone of the whole matter lies in the *intelligence, and in the mental concentration, with which the washing process is carried into effect.* Much has been gained for the whole question of the cleansing and the disinfection of the hands through the recognition of the fact that these manipulations represent a science, some idea of which can, no doubt, be conveyed by means of printed directions and theoretical discourses, but which must be *learnt* through practical experience. The best teacher, the sharpest spur, is for a man to bear the responsibility of the operations performed. If Schleich offers the suggestion, that short personal courses should be held for students, with this object in view, the

soundness of his suggestion cannot be denied. At all events, the practical advantages of such a course might be of great value, especially in the case of those students who have no opportunity later of acquiring and practising the art of cleansing the hands in surgical or gynæcological hospitals; and such measures might well be more effective than the categorical ordinance enjoining a period of abstinence, or of probation, after contact with infectious material.

But the act of cleansing the hands does not merely demand full comprehension and practice, but, in addition, considerable mental concentration; one must have an ever-ready appreciation of what one is about, unless one is prepared to spend the time, prescribed for cleansing the hands, in purposeless rubbings. Amusing or instructive talk, carried on during this act of cleansing the hands, leads only too often to a withdrawal of the attention, which ought to be bestowed on the individual manipulations. It has already been pointed out above that, apart from the environment of the nail, different parts of the surface of the hand are also disproportionately accessible for cleansing purposes.

A thorough and properly supervised bacteriological control is of great moment in the case of students and assistants who have but newly entered on their duties, because, when the results are demonstrated *ad oculos*, the incitement is greater. The experiments, in which the hands are impregnated with Indian ink, are very instructive, and are considerably simpler, and here the microscopic control naturally cannot be at fault; if these black particles cannot be removed from the hand, the result will be exactly the same in the case of invisible, tenaciously adherent micro-organisms.

I may, at this point, briefly refer to an experiment which was instituted with the purpose of elucidating the question of the practicability *of cleansing the hands in the case of individuals following different occupations.* I must preface it by saying that the persons who were experimented on were fully enlightened as to the object of the experiment, and that the instructions,

which were enjoined (with special reference to the form and intensity of the mechanical cleansing), were carefully supervised in their execution.

The experiments were carried out on the following persons—a *cook*, a *laundress*, (both young and intelligent persons), a *housewife*, a *young lady*, whose well-cared-for hands have no rough work to do, a *nurse*, a *chemist*, and a *medical man*.[1] The hands were cleansed according to Fürbringer's extended method (four minutes for each act); the examination of the hands was carried out by means of my thread method, and both Agar plates and Bouillon were used. A joint examination was made of the region of the nails, and of the surface of the hand, by means of the same thread.

	Cook, hands somewhat rough.	*Laundress*, hands well-conditioned, yet skin macerated.	*Housewife*, hands well-conditioned.	*Young lady*, hands faultless.	*Nurse*, hands chapped.	*Chemist*, hands faultless.	*Medical Man*, hands somewhat rough.
	Colonies.	Colonies.	Colonies.	Colonies.	Colonies.	Colonies.	Colonies.
fore the cleansing[2] .	780	*ca.* 50,000	910	350	460	410	570
ter the mechanical cleansing[3] . .	560	650	320	150	350	170	95
ter the washing with Alcohol[4] . . .	76	127	34	12	140	14	6
ter the washing with orrosive Sublimate[5]	182	75	35	5	221	12	3

It was thus the *medical man* who, although the condition of his hands was not altogether faultless, was

[1] A qualified surgical assistant.

[2] After a short sojourn in hot water.

[3] After the mechanical cleansing the hands were scrubbed with a dry towel, and then again bathed in hot water for a short time. The cleansing of the nail with a metal instrument took place during the cleansing procedure.

[4] With a brush; the hands were afterwards bathed for five minutes in hot common salt solution.

[5] The Corrosive Sublimate was precipitated on the thread by means of Ammonium Sulphide.

able soonest to cleanse them; the *nurse* was least able to do this, on account of the utterly wretched condition of her skin; the *laundress*, whose macerated skin yielded most germs to begin with, had a better result to show in the end than had the *cook*, for, on account of her occupation, the outermost epidermic layers were already well loosened, and could thus be more completely removed. I must add that the *housewife*, the *young lady*, and—to a lesser extent—the *chemist*, suffered from the cleansing process; the skin of their hands was red and smarting after the experiment.

Apart from the condition of the hands from their treatment with cosmetics—a question which I shall have to discuss later—the power of cleansing the hands in a surgical sense still depends, to some extent, on *the individual peculiarities of the skin.* A strong growth of hair increases the difficulty of cleansing the skin, for reasons which have already been stated; but the peculiarities, which count for most, are to be found in the wholly dissimilar action of the sweat-glands. There are certain hands from which it is much more difficult to remove the fatty material than is the case with others, either because the secretion of fat is more abundant, or because what is secreted is possibly more tenacious in form.[1] Anyone whose hands are so greasy that, in order to free them from this fatty material, a more vigorous procedure is required, is on this account much better off than another whose hands secrete an abnormally small amount of fat; for, in the latter case, the hands are much more sensitive to the measures which are taken to cleanse and disinfect them, and this subjective sensitiveness will no doubt often lead to a diminution in the vigour with which the hands are cleansed. But even objectively, such hands suffer much more, and are easily roughened by being subjected to such treatment; they require unremitting cosmetic attention. This sensitiveness of the hands I have

[1] As Leubuscher (Deutscher Kongress für innere Medizin, 1899) pointed out, the fat-secretion varies very considerably in different individuals.

remarked as principally occurring in the case of nervous individuals whose hands also sweat on the slightest provocation.

The theory, which Ahlfeld was the first to suggest, that in some cases the hands are more easily cleansed, so as to free them from germs, than are others, is thus quite consistent with fact, and upon this must depend the duration and vigour with which the cleansing process is carried into effect.

CHAPTER IV

THE CLEANSING OF THE HANDS WITH DISINFECTANTS

It is difficult to find a logically correct definition of the fluids which will be discussed in this section. A "disinfectant" is, after all, the mechanical cleansing, since it removes the infectious material from the hands. Even the designation "chemical agent" is not altogether correct, as distinctive from the term "mechanical," which we discussed before, since the removal of the fatty material from the hand is, in part at least, also a chemical process, and we have no definite certainty as to the micro-chemical processes which occur in the cells during the mechanical cleansing. Thus, for our present purpose, we can only hold those media to be conformable with the customary usage of the term disinfectant which shall aim, not at the removal of the germs from the hands, but at the enactment of an injurious influence upon the germs in their vital qualifications, as they are present on the hand.

We may here take for granted *that these disinfecting lotions are, when employed by themselves, useless for the purpose of sterilising the hands.* Numerous experiments, which were carried out by others, have arrived at this result, and a large number, which were performed by myself, have confirmed it. Whether the experiments were conducted on the "hand in everyday use," or on the artificially infected hand, whether solutions of Mercury salts, or solutions of the derivatives of Tar, were selected for the purpose, the results—after the most accurate elimination possible of the chemical

medium—were bad enough, even when the sojourn of the hands in these solutions extended to more than half an hour.[1]

The result was to some extent improved if a brush was used simultaneously, or if the sojourn of the hands in the hot solutions was interrupted on several occasions by an efficient polishing of the hands with a rough towel—thus by calling in mechanical assistance.

These results are admitted, and are, in truth, self-evident. The value of the antiseptic medium is in no-wise diminished by this, in its general action as affecting the germs. The task—a removal of the fatty material from the skin and an unrestrained penetration of the antiseptic into the deeper parts—is more than it can accomplish. It had unfortunately become customary to regard antiseptics as higher things, possessed of unlimited power, and to invoke their assistance as saviours in the hour of danger. One must harden oneself to the fact that the antiseptic qualities, which are in question here, are not absolute, but merely relative, and that certain preliminary conditions must be present before the antiseptic medium can possibly act. These preliminary conditions are but partially recognised at times. So long as we are not better enlightened as to the manner *in which* antiseptics exercise their action, and so long as we are also unable to come to a logical determination beforehand as to the *where* and *when* in this matter, we are not entitled to condemn them at pleasure, just because an antiseptic is not inclined to adapt itself to the ideas of an experimenter or clinician, but rather tends to take the opposite course.

Since an antiseptic medium (Alcohol will be left out of count for the present) cannot remove the germs from the hand, but merely affects them disadvantageously in their vital conditions, it is quite evident that a scrupulously employed mechanical cleansing of the hands, together with a conscientious regard for the three fundamental factors which have already been mentioned,

[1] Lysol, on account of its soap characteristic, gives, relatively speaking, the best results in such experiments.

must in every case precede the use of such a medium for the disinfection of the hands, if, after all, any result is to be expected. The most important thing of all is certainly the pure *removal of the fatty material from the skin*, which first affords to the aqueous solutions the possibility of their being able to penetrate at all. A simple experiment will illustrate this.

If a "hand in everyday use," which has received no preparation, and has not been washed for several hours, is steeped for five minutes in 1–1000 Corrosive Sublimate solution, which is then washed from the hand for five minutes in running water, the outermost epidermic layer will show a scarcely perceptible brown discoloration with Ammonium Sulphide. But if the hand [1] is efficiently rubbed several times with renewed supplies of Kaolin paste, which is then washed off, and is steeped for five minutes in 1–1000 Corrosive Sublimate solution and then washed in water for five minutes, the brown discoloration with the Ammonium Sulphide test will be considerably more marked and more general. It will be still more marked if the hand is treated with Kaolin and bathed in hot Bran water, or is washed with alkaline soap and a brush, and then scrubbed with a dry towel.

Thus, Corrosive Sublimate in aqueous solution impregnates the skin to the utmost when the skin is relieved of its fatty material, and when the outermost cornified epidermic layers are removed, or loosened. These are precisely the very points which require no experimental proof, but which are often not sufficiently called to mind, for it is a common enough sight to see operators and medical men, if time presses, omit the mechanical cleansing in the case of small operations, and just wash their hands in cold Corrosive Sublimate solution. Were I compelled to choose between the use of an antiseptic fluid and a thorough mechanical cleansing with hot water and soap, I should decide in favour of the latter, so great is the faith and esteem I possess for antiseptic media in general, as I shall show at another stage!

[1] On another day, of course.

But in view of the results obtained with the use of the mechanical cleansing, which have been given in an earlier section, I am of opinion that we certainly stand in need of a further safeguard, if this can possibly be attained. It is impossible to render either the surface of the hand or the region of the nail free from germs, with certainty, by mechanical means; sterile results are as rare as white ravens. In most cases it is not a question of large numbers of germs being present; but, *if we take so much care to ensure the absolute sterility of the dressings* (even of the cotton-wool, and bandages), *it is surely only logical that we should give the same attention to our hands, which come in still closer contact with wounds, particularly with the deeper parts.* If, then, it is possible to obtain a better result with the help of antiseptics, it is our duty to take this course, and not to be disconcerted by the brilliant statistics published by "one who pretends to absolute asepsis"—statistics, which are unfortunately not uninterrupted.

A. *The Value of Ethylic-Alcohol in the Cleansing and Disinfection of the Hands.*

The question as to the use of Alcohol in the disinfection of the hands is, at present, one of the questions of the day, and all the more so that experimental and clinical results are at variance with regard to the solutions which are included under "antiseptics." The explanation of this is simple. Like all investigations which have to do with the disinfection of the hands, the elucidation of the question with regard to the use of Alcohol as a disinfectant is one of the subtlest of problems. Numerous are the small but consequentially important sources of error which occur; but what is most to be remarked is the constant difference in the conditions which underlie the experiments as conducted by individual experimenters, however much they may strive to proceed on the same lines. Thus, the difference in the condition and in the greasiness of the hands, the

frequency with which the hands have been washed on the day of the experiment, the use to which the hands have been put beforehand, the temperature and the dustiness of the place, the energy and the intelligence shown in carrying out the cleansing process, the materials employed, the complete difference in value as regards the nutrient media used, and a whole collection of other incommensurable factors, make it a totally impossible matter for the results arrived at by the separate experimenters to be directly compared with one another, and for correct conclusions to be drawn from these results.

There was a very general unanimity of opinion with regard to the important part which Alcohol could play in the process of disinfecting the hands. Fürbringer it was who introduced it as an intermediary into the practice of disinfecting the hands. Landsberg had already, though almost with reluctance, indicated its value; but Reinicke first pushed it prominently forward, and Ahlfeld finally placed it in the front rank, as an independent means of disinfecting the hands. But the real *position* occupied by Alcohol with regard to this matter must remain obscure and doubtful, so long as no sufficient light can be thrown on the manner in which it acts, *i.e.* the whole question amounts to this —*Is Alcohol a disinfectant? Can it alone, by itself, in the time usually allotted to the employment of preparatory measures, kill the germs present on our hands, or arrest their development?*

Attention was first drawn, to any noteworthy extent, to the value of Ethylic-Alcohol as a disinfectant by means of experiments with regard to the disinfection of the hands. It was solely its efficiency in the preparation of the hands which induced Ahlfeld to proclaim it to be an energetic disinfectant of particular value, and the test-tube experiments, which at first occupied but a secondary position, could only serve in addition by acting as further practical supports to these experiences. Since the thousands of experiments with regard to the cleansing of the hands had apparently proved the

efficacy of the Alcohol with such crushing force, it seemed hardly necessary to prove this again in another way.

But it is certainly simpler and more prudent, before commencing to answer the question as to the efficacy of Alcohol on the hands, to first inquire as to what form the answer to the first question may assume—Is Alcohol a disinfectant, of whose efficacy there can be any question in practice?

After Koch's classic experiments, the results of which were very depressing with regard to the value of Alcohol (Anthrax spores successfully withstood the action of Alcohol, whether absolute or diluted, for the space of four months), there was for a time no inclination to seek in Alcohol a means of disinfection. Even Fürbringer, who recommended Alcohol as an intermediary in his process of disinfecting the hands, credited it, in his fundamental work (1888), with solely preparatory qualities: "It dissolves the fat and allows the antiseptic to penetrate in more easily." But, in his last communication,[1] he associates himself with those who see in Alcohol the best antiseptic for this purpose; but all the same he is of opinion that the subsequent use of one of the recognised antiseptic lotions should never be omitted. Reinicke, also, attributed in part the action of Alcohol in the disinfection of the hands to its power of dissolving fat; but at the same time he concluded from his experiments that Alcohol is able to remove germs and epithelium along with the fat, and may thus act *mechanically*, and that the paucity of germs, in respect of the freedom of the hands from germs, must be explained in this way—a view to which Poten also mainly attached himself. Neither Reinicke nor Poten considers that Alcohol can, by its action, influence the germs of the skin in their vital capacities to any appreciable extent, during the short space of time that there is a question of its action in the cleansing of the hands. One argument in favour of this view already lay in Reinicke's observation that the

[1] *Deutsche med. Wochenschr.*, 1897, No. 6.

germs, with which he had artificially infected the hand, could still be identified by means of cultures in the Alcohol lotion in which the hands had been washed —a washing from which the hands issued "sterile." Krönig also recognises the fat-solvent properties of Alcohol; but, on account of his exact experiments, he comes to the conclusion that the sterility of skin, which has been washed with Alcohol, is but apparent, since the surface of the skin is rendered so hard by the action of the Alcohol, that it is impossible to remove the germs from its surface when it is scrubbed for a short time with wood-fibre.

Ahlfeld it is in particular who now claims for Alcohol the position of a dominant disinfectant of especial value. He bases his opinion upon a series of over one thousand separate experiments in practice, and upon test-tube experiments. He considers, as does Fürbringer in his last communication, that he has refuted both Reinicke's theory, and that of Krönig.

Goenner, with a series of practical experiments on the hand, and with test-tubes, takes up an intermediate position. He acknowledges that Alcohol is fairly efficacious as an antiseptic; but, according to his experiments, it is less efficacious than a solution of Corrosive Sublimate, and it cannot therefore replace the latter in practice.

Pure test-tube experiments are of particular value which do not deal with the disinfection of the skin, but have principally to do with the general question of how Alcohol can influence germs by its action on them. The experiments of Schill and Fischer merit a brief reference in this connection. They proved that Tubercle bacilli in sputum were not, as a general rule, killed in twenty-four hours by about 80 per cent. Alcohol (Absolute Alcohol in five times the quantity of the sputum mixture). But it was especially Epstein, and almost simultaneously Minervi, who, with larger series of exact experiments, sought to decide the position of Alcohol in the ranks of disinfectants. Epstein came to the conclusion that Absolute Alcohol has no power

of killing germs, but, in its diluted forms, can do so in a moderate degree. 50 per cent. was the most effective concentration. In 80 per cent. Alcohol, for instance, *Pyocyaneus*, *Prodigiosus*, and *Staphylococcus* were not killed in a space of ten minutes, whilst in 50 per cent. Alcohol these germs, at the end of ten minutes, either showed no signs of further development, or developed more slowly. Epstein worked with Koch's old method of using infected silk threads, but he materially improved upon this method by using only the elementary fibres of a disorganised silk thread.

Minervi proceeded in like manner; he was so far able to confirm Epstein's conclusions, in that, by his experiments also, Absolute Alcohol displayed but a trifling germicidal action. None of the germs employed for the purpose of this investigation were killed under twelve hours (*Staphylococcus aureus* was, at the end of three days, still capable of development). 70 per cent. Alcohol acted the best; but even here the death of the germs did not occur until the Alcohol had exerted its influence for ten minutes. 50 per cent. Alcohol proved to be less powerful in its action. The germicidal action of boiling Alcohol, or of Alcohol under pressure, increased with the proportion of water present.

Now, in comparison with these results, the test-tube experiments of Ahlfeld and Vahle, and of Fürbringer and Freyhan, are somewhat irreconcilable, and one is forced to seek an explanation of this circumstance.

It is a well-known fact that in all bacteriological works, but particularly in those dealing with the action of antiseptic fluids on micro-organisms, experiments, which are carried out with regard to the self-same matter by different experimenters, almost always yield different results. The difference in the present case arises from the dissimilarity in the experimental conditions, from the difference in the power of resistance of the germs of like species to the differently constituted nutrient material, and from a whole collection of other unknown or incommensurable factors which cannot here be brought to account. Such experiments can, therefore,

never be directly tested from another quarter, not to the extent, at least, that anyone undertaking such a test can, on the ground of his results purporting to be somewhat different, cast incorrect observations in the teeth of the original experimenter, unless he is in a position to reveal directly the sources of error of the original experiments.

But such a striking—I might almost say fundamental—difference, as occurs in the results of these test-tube experiments with Alcohol, must, considering the practical importance of the matter, incite every observer to elucidate the subject. Thus, in my own case, what induced me to investigate the subject was precisely the fact that, to begin with, I obtained results which were very dissimilar—results which were, at all events, more dissimilar than any that I have obtained in any other department of antisepsis to which I have devoted my particular attention during the twelve years of my work in the laboratory; in this way I was often forced, in the further course of the experiments, to take up questions which apparently led me somewhat astray from the original subject-matter.

It would be going too far here to relate the precise particulars of these experiments with Alcohol—there are thirty-two series in all; I confine myself in this matter to a short outline of the experimental method of procedure which I employed, and of the results.

After experimenting at first with a number of different germ species, I soon arrived at the conclusion that the self-contradictory results could not be cleared up in this manner, unless one was willing and able to increase the experiments *ad infinitum*. We have not always at our disposal septic material of a like degree of resistance to put in place either of the permanent germ forms, or still less of the vegetative germ forms, and which can pass as a general example of the individual germ species—a state of affairs to which von Esmarch, C. Fränkel, Ficker, and others have already had occasion to refer. Accordingly, in my opinion, in order to throw light on the germ-influencing properties

of well-defined chemical or other factors, less advantage was to be gained generally by extending the experiments over a large number of *different* germ species, than by confining the varied experiments to the use of one single species of germ—to perhaps the most important species of germ for the case in question. Anyone who has had much to do with such experiments is aware that even under such conditions the work is still sufficiently great and difficult.

I selected the *Staphylococcus aureus*,[1] because experience has shown that, of the germs which commonly give rise to trouble in connection with the infection of wounds, this is the germ which is most resistant to such inimical influences as desiccation and disinfection. But even now a restriction was necessary, as was soon proved. Staphylococci which originated from different sources led to the experimental results being again wholly disproportionate. Accordingly, most of the series of experiments were carried out with germs cultivated from *a single* source—an abscess following an operation for hernia, from which pus was transferred to Bouillon and Agar, day by day, during the course of the experiment.[2]

But not even by this means was a homogeneous basis procured. The reason for the difference in the results had therefore to be sought for in the method of experimental procedure, or in the age of the germs and the manner in which they were cultivated. First, with regard to the *method of procedure*, I had recourse to Koch's old thread method, in order to test the investigations of Ahlfeld, Epstein, Minervi, and Goenner. But this method, as I shall show elsewhere, is very apt to lead to fallacies. Still, the modification of it proposed by Epstein—the reduction of the silk thread into its

[1] By way of comparison, experiments with sporogenous Anthrax were regularly carried out alongside of these.

[2] I may mention here that on different occasions I succeeded in producing abscesses in rabbits by means of germs from this source. I do not lay so much stress on this, because it was done merely with a view to clearing up the question of the influence which is exerted by Alcohol on micro-organisms in general.

elementary filaments, and the impregnation of these with germs—is very successful. As Goenner has already pointed out, it is not a matter of indifference whether the thread is merely smeared with septic material (Agar culture), or quickly imbued with it (Bouillon), and then straightway dried, or whether, before it is dried, it is left lying for some time in the Bouillon culture at incubation temperature; this has special reference to silk threads which have not been disintegrated, but also, to a less extent, to the delicate elementary filaments. If the filament was kept in Bouillon culture at incubation temperature for merely eight to ten hours, the resistance capacity was noticeably greater than in the case of filaments which had been infected by means of a process of imbibition of short duration. Now, whether this is brought about by the fact that the germs in the former case nestle deeper into the thread, or whether still other factors co-operate, which depend on the action exerted by the toxine on the albumen of the silk, is a question which must remain for the present undecided; at all events, the matter has to be taken into account.

But in neither form of procedure can *irregularities in the degree of the impregnation with germs* be avoided. Therefore the plan suggested by Geppert—that the well-shaken Bouillon culture, or germ emulsion, should be filtered, and that only the slightly turbid filtrate should be used—is to be recommended, at least when the imbibition of the threads is of short duration; in this way the results will be much more uniform.

If *quite smooth, impervious bodies* are used on which to dry the septic material, a whole collection of possible sources of error is thus already withdrawn. This is a point to which Minervi has already referred in his experiments with regard to Alcohol, as also have Krönig and Paul in their important and wonderfully correct experiments, which were carried out with diverse disinfectants. Minervi observed that a more subtle reagent was obtained if the septic material was dried on steel needles than if silk filaments were used for the purpose.

But even though the antiseptic properties of iron are small, they should not, according to our investigations, be left out of count, and we must therefore have recourse to an indifferent body such as glass. Krönig and Paul employed the ordinary glass balls which are for sale; I have had glass beads prepared (2 mm. in diameter), which were duly scoured and dried before they were infected with germs.

The *manner in which the septic material is dried* is also of importance, with regard to the resistance capacity of the germs. If young vegetative forms are rapidly dried in a hot-air chamber, they are less resistant than if the process is gradually carried out at a temperature of 37° C.[1]

But not even yet were the experimental results more uniform. The mistake could now only lie in the *age of the germs employed and the manner in which they were cultivated.* In view of this, I set on foot a more extensive series of experiments, both with Alcohol and other antiseptic solutions; here I used cultures on the surface of Bouillon and of Agar, which dated in their growth from a period of two months to twelve hours (a culture for every twelve hours). It was proved from this that *in the first forty-eight hours the germs of the Agar culture are more resistant than those of the corresponding Bouillon culture*; *that the germs of the Bouillon cultures have, on an average, arrived at their highest power of resistance at the end of three days, and so remain for four to six days*; subsequent to this period (constantly maintained at incubation temperature) their resistance capacity towards antiseptics slowly diminishes.

Further, there is no doubt of the great importance that in such experiments the *culture medium* should be

[1] How often the outcome of experiments may be modified by trivialities I had proved to me by a series of experiments in which silk filaments were dried in a hot-air chamber on an arrangement of brass wire which had previously been sterilised by heat. These threads proved to be much less resistant than others, which were dried simultaneously on glass in another hot-air chamber—due, no doubt, to the small dynamical action of the brass from the presence of copper.

very carefully prepared, and that in all series of experiments it should emanate from the same source, *i.e.* by some well-defined process. I could observe, for instance, that the mere introduction of another peptone preparation had a perceptible influence on the resistance capacity of the germs.

Now that I have briefly described these circumstances, further reference to which I must here deny myself, I may shortly state what were the results of my experiments.

The experiments of Epstein and of Minervi already showed conclusively that the degree of concentration of the Alcohol is a matter of great importance with regard to its effect on the viability of the micro-organisms. My investigations have fully confirmed this. While Epstein awards the greatest efficacy to 50 per cent. Alcohol, Minervi finds that 70 per cent. is the best degree of concentration; in my opinion the most efficient degree of concentration lies between 60 and 70 per cent.—at least, I obtained the best results with a dilution of 35 per cent. of water. I shall attempt at a later stage to throw more light on this observation; it is, at any rate, of importance in reviewing the hitherto recognised experiments which deal with the bactericidal action of Alcohol.

Glass beads, which had been dipped in the clear filtrate of a twenty-four hours' Bouillon culture and had been dried in the hot-air chamber (the beads were thus clear as glass, the germ layer being very thin), gave no evidence in Bouillon of any further process of development taking place after they had been placed for two minutes in 65 per cent. Alcohol, whilst cultures could still be obtained—protractedly, indeed—after they had been for five minutes in 96 per cent. Alcohol; but after ten minutes the results, even in the latter case, were sterile.

Elementary filaments of silk thread, which were soaked for several minutes in the clear filtrate of a twenty-four hours' Bouillon culture, and were dried in the hot-air chamber, gave no signs of any further

development taking place after they had been in 65 per cent. Alcohol for four minutes; but the formation of cultures still took place after the filaments had been acted on for six to seven minutes by Alcohol of a high degree of concentration (96 per cent.), though after ten minutes, even in the latter case, the results were sterile.

With the same procedure, Bouillon cultures of three days, or superficial Agar cultures of two days, already showed a greater power of resistance. But if silk filaments were kept for twelve to twenty-four hours in a three days' Bouillon culture at incubation temperature, and were dried in a thermostat, the germs still continued to develop, almost in every case, after they had been subjected for ten minutes to the action of Alcohol of every degree of concentration.

If the glass beads, or the silk filaments, were slightly greased with Lanoline after they had been dried, no placing of them in Alcohol of any degree of concentration for less than ten minutes could prevent a subsequent development of the germs from taking place, not even in the case of germs of the lowest vitality.

These experiments thus show *that vegetative germ forms of low vitality, which have been rapidly dried in quite a thin layer, can certainly be interfered with in their further development by a sojourn of a few minutes in dilute Alcohol, but that their resistance capacity towards Alcohol increases with the relative strength, with the increased thickness of the layer, and with a process of drying of a less injurious description, and that even the thinnest layer of fat enveloping them can prevent the Alcohol from taking effect.*

Ahlfeld's test-tube experiments cannot be spared the reproach that they are too few in number. It is impossible to lay too much stress upon the fact that, in such investigations, a complete set of parallel experiments must always be carried out, if sources of error, which insinuate themselves so readily, are not to lead to erroneous conclusions. Ahlfeld had, at all events, carried out these experiments before he sustained the crushing blow from his practical results on the hands,

and their object was simply to confirm the opinion at which he had arrived, with apparently good reason; he must therefore have had his mind fully set at rest by the propitious result of the first test-tube experiments. Still, in the case of the thread experiments he should have been startled to note that on *dry* threads the staphylococci still proved viable after a sojourn of one hour in Alcohol; he must, however, have supposed that a preliminary soaking in water for five minutes may have been able to duly moisten the threads, though scarcely the desiccated, fat-covered germs. I have, besides, been able to confirm this difference in the behaviour of moist and of dry threads, when the Alcohol used was very highly concentrated, but never when the concentration was from 50 to 80 per cent.

It is certainly a matter of difficulty to explain this remarkably unstable behaviour of the moistened threads in Ahlfeld's experiments, and all the more difficult because one would suppose that the method of impregnation, from the thickness of the germ layer (a "broth" obtained from Agar and Bouillon cultures), would render it impossible for the Alcohol to exert a general influence in this short space of two minutes. I have dried broth, prepared according to Ahlfeld's recipe, on small glass plates and on glass beads, and, after a preliminary moistening, have subjected it to the action of Alcohol for five minutes, without the growth having sustained any perceptible injury, even at the end of thirty minutes. In Ahlfeld's experiments, at all events, the broth adheres to the *surface* of the thread, and it is not impossible that, after the impregnation of the thread with water, from the strong diffusion which occurs, this layer of germs, which intervenes between the thread and the Alcohol, is ruptured, partly separates, and falls off.

Five further experiments of Ahlfeld's dealt with undried septic material which was conveyed direct into Alcohol from the Bouillon or Agar culture; he took one or more loopfuls, transferred them into about 2 cm.^{3} Alcohol, and, from the Alcohol, inoculated

Bouillon with one loopful per minute; he found that already at the end of *one* minute no further development of the germs was possible. I have repeatedly tested these experiments. I found that Ahlfeld's statements were, for the most part, confirmed if I confined myself sufficiently to his recipe, which is, however, not free from objection, since it is improbable that a platinum wire loop can repeatedly extract from 2 cm.^{3} Alcohol a sufficient supply of germs for one or more loopfuls.

I have modified the experiments to this extent, that I deposited each loopful of Agar culture between thin layers of glass wool, then placed these little packets in Alcohol, washed them in sterile water, after a definite length of time, and transferred them to Bouillon. In the majority of the cases the developmental process still continued at the end of ten minutes, if allowance was made for the maximum resistance capacity of the culture.

With regard to the practical disinfection of our hands, the test-tube experiments of Fürbringer and Freyhan now appear to be very misleading, since, in the investigation, attention was directed to the *dirt of the nails*, in order to determine the effect of the action of Alcohol on germs. For here, as Fürbringer rightly insists, the removal of infectious material from the skin should be just as thorough, and as free from all reproach, as the purely mechanical effect in the acceptation of Reinicke and Poten. Particles of nail dirt, of the size of hempseeds, were, after a preliminary moistening, placed for three minutes in Alcohol of various degrees of concentration, and were then apportioned to solid nutrient media (Gelatine, certainly, from the note). To quote the number of the colonies, which were thus obtained, is of minor importance, since the septicity of nail dirt, even in the case of one and the same individual, is a variable quantity, and it should scarcely be possible to obtain, for the purpose of inoculation, supplies of nail dirt of sufficient similarity; thus the numbers of colonies are, for purposes of comparison, only to be used with caution. What is of importance, is that

Fürbringer observed that with 90 per cent. Alcohol the nutrient media remained sterile in 45 per cent. of the experiments; with 70 per cent. Alcohol, in 33 per cent. of the experiments; and with 50 per cent. Alcohol, in 28 per cent. of the experiments. This result is, in many respects, astonishing. In one respect, because, according to the experiments which have been mentioned above, it seems almost impossible that Alcohol, within the space of three minutes, should kill bacteria, which —as in the case of germs from the nails of uncleanly patients—include [1] forms which are, at all events, very resistant; and in another respect, because such nail dirt is always very rich in fat,[2] and therefore not only withstands water, but also renders it a much more difficult and tedious affair for the Alcohol to obtain access to the germs.

Since, as I have already pointed out, the *number* of germs in this connection is of minor importance, I tested Fürbringer's experiments to the extent of subjecting particles of nail dirt, placed between layers of glass wool (according to the method which has already been described), to the action of Alcohol of different degrees of concentration. After ten minutes of this treatment

[1] In nail dirt of this description I have repeatedly been able to identify Tetanus bacilli, which have attained, at any rate, to the formation of spores under the nail.

[2] As Fürbringer himself shows in his fundamental monograph (p. 24), and as I can fully corroborate. I have transferred to water nail dirt obtained from the most diverse sources, and have prevented the water from evaporating by hermetically sealing it. At the end of four weeks the easily perceptible particles still floated on the surface. In this connection, Fürbringer's observation must also be kept in mind, that the bacteria of nail dirt, which has been softened in water, make a better start, *i.e.* give rise to more colonies, than do those where the nail dirt is dry. This is simply due to the fact that the preliminary moistening breaks up, to some extent, the small lumps of dirt, and thus gives rise to the formation of a larger number of individual colonies, whilst if no such moistening takes place massed colonies preponderate. The relative greasiness of the nail dirt may also impede the Gelatine from gaining access to the germs, so that a number of germs remain undeveloped. Nail dirt, which had been dried at the temperature of the room, whether dry or moistened when subjected to the action of the Alcohol, gave, according to my investigations, the same results as particles of dirt which had just been freshly removed.

with Alcohol, cultures could still, as a general rule, be obtained in Bouillon, although, in some cases, the growth of the cultures was manifestly retarded.

The experiments are more delicate when carried out according to my method of drawing silk threads over the surface of the parts to be examined, *i.e.* the cleaning out of the space under the nail by drawing threads of this type backwards and forwards, because by this means the layer of dirt is much less thick. In a series of experiments (ten), dealing with the nail dirt of a variety of persons, the development of cultures could still always be confirmed in Bouillon, after the threads (which were divided into pieces 1 cm. in length, and were scarcely darkened in appearance) had lain for ten minutes in Alcohol of the most different degrees of concentration; these cultures did not differ to any appreciable extent, either in rapidity of growth, or in abundance, from those of the control tubes.

Here again it is difficult to say wherein lies the cause of the differences. It is certainly possible that, in many cases, Fürbringer was not successful in removing out of the Alcohol organic material from the dirt particles, which had been "broken up to some extent with needles, and, in fact, disintegrated," and that he merely secured the mineral ingredients which remained visible, from which the organic material had in great part been separated or floated away by the Alcohol.

It is thus impossible to deny that Alcohol possesses an appreciable capacity of acting injuriously with regard to bacteria; *it*, like all media capable of coagulating albumen, *must be numbered amongst antiseptics.* It has the power to kill within a few minutes vegetative germ forms which are unable to resist its action, or to arrest their development, in so far as it has unrestricted access to the germs, *i.e.* in so far as the germ layer lies wholly on the surface and is very thin; if these premises are not fulfilled, its power is then essentially destroyed, and, according to test-tube experiments as carried out by myself and by others, *it is incapacitated from destroying with certainty germs like those to be found on and in the skin*

of our hands within a space of time such as is generally available for the practical disinfection of our hands.

So much for the test-tube experiments. How do matters now stand with regard to testing the action of Alcohol on the skin, and, more especially, its action on the hands?

The brilliant results obtained by Reinicke, Ahlfeld, Fürbringer, and Poten confront those obtained by Krönig and Leedham-Green. The works of both these latter authors are irreproachable, as regards their experimental arrangement, and Krönig's experiments in particular are exemplary with regard to their lucidity, and the subtlety with which sources of error are eliminated. They have been hotly attacked—not materially, since, up till now, their facts remain unrefuted, but in the conclusions at which they arrived, and this certainly without reason. Krönig sought to decide the question whether or not the skin could be rendered free from germs, and the germ forms with which he experimented were not without resistance, but were particularly resistant—such, for instance, as are habitually used at present to test a steam steriliser for dressings.[1]

Finally, no proof is anywhere afforded that virulent forms (Anthrax, at least) are not also present on the hands of surgeons; in such investigations the cultures should be grown under anaerobic conditions, so as to eliminate the possible presence of, for instance, the germs of Tetanus or of Malignant Œdema. The chance of obtaining Tetanus spores on the hands is not so rare as is commonly supposed.[2] Here, however, one must

[1] Reinicke also worked with the exceptionally resistant spores of the Potato bacillus. How careful one must be with regard to conclusions—the outcome of practical results, as to the germicidal action of Alcohol—is particularly well shown by the only observation of Reinicke which is at all explicit. He infected his hands with these spores, and, after a short treatment with Alcohol, the culture results which he obtained were sterile; whilst, on the contrary, he was able to prove that these spores were in nowise arrested in their development by being placed for three-quarters of an hour in Alcohol.

[2] I have repeatedly succeeded in obtaining Tetanus germs from the nail dirt of persons who were not agricultural labourers—though neither were they surgeons. I refer more particularly to the details of these experiments at another stage.

admit that there is no great likelihood of this occurring, though one dare not forget that, in the annals of surgery and of obstetrics, cases are on record of Tetanus having given rise to complications in connection with operations.

It is, however, quite another consideration which, to some extent, reassures us with regard to experiments undertaken with Anthrax spores—viz. that the guinea-pig reacts much more sensitively to Anthrax, than does a human being to the germs which usually give rise to pus. In the case of the guinea-pig, few germs suffice to give rise to a general infection, whilst in man the usual illnesses connected with the infection of wounds are localised to start with—as is also the case with Anthrax. From the numerous investigations on the subject in recent times one can further assume that, for the production of disease in the human body, due to the infection of wounds (Staphylococci and Streptococci), the majority of the germs must have been introduced. Experiments conducted on animals with Anthrax spores undoubtedly form the best basis for such investigations, for by this means not only do we obtain information with regard to the maintenance of the developmental capacity, but also with regard to the virulence. But, unfortunately, in the case of practical investigations, the information which we can thus obtain is unsatisfactory, because micro-organisms which, in the case of human beings, are able to produce the diseases of wound-infection are, in experiments on animals, generally more feeble and more irregular in their action.

The large germ numbers recorded by Leedham-Green are indeed surprising, and are, at a distance, inexplicable. It is possible that, owing to the condition of the space under the nail (long nails, rough termination of the bed of the nail), the germs were better retained by the tissues; that the small ivory instrument, which was intentionally roughened on the surface, and which was used to wipe out *all* the nails, was better fitted to carry along with it the flakes of epidermis on which the germs lay; and that the removal of the Alcohol from

the hand was more strenuously carried out, and to better purpose.[1] At least one gross experimental blunder appears to have been overlooked in the detailed account which he gives of his procedure, and of what he observed.

My personal investigations of the hands (with the thread method), after they had been treated with Alcohol, appeared, at first at least, to confirm the results obtained by Reinicke, Ahlfeld, and Fürbringer, since (in forty-two separate investigations), both in cultures of Bouillon and of Agar, the threads remained sterile in 82 per cent. of the experiments, when the hands had previously been mechanically cleansed in hot water, with soap and a brush, for five minutes, had been efficiently scrubbed with a dry towel, had been brushed for five minutes in Alcohol (96 per cent. or 70 per cent.), and the Alcohol had then, for the space of five minutes, been washed off in sterile water; the investigation (rubbing with the thread) included not only the surface of the hand, but also the environment of the nail, and the individuals, on whom the practical experiments were carried out, were surgeons and assistants of every sort and description, whilst in the laboratory the artificially infected hand was employed.

This result surprised me, because, from personal experiments, I already knew both the effect of the mechanical cleansing and the value of Alcohol as an antiseptic. I was all the more astonished at this result because, in quite a small percentage of cases (two out of twelve), the threads remained sterile even where no mechanical cleansing had preceded the treatment with Alcohol—at least if this had extended over a period of seven to nine minutes. I was forced to admit that the capacity of Alcohol to remove the septic material *mechanically*, as advanced by Reinicke and Poten, might be the cause of this. As a matter of fact, I succeeded, almost without exception, in obtaining more or less numerous colonies (especially in the case of those germs

[1] No mention is made as to the length of time which was devoted to washing off the Alcohol in hot water.

which had been artificially introduced) from the sediment of the Alcohol in which the hands had been washed—a circumstance which, by way of parenthesis, affords the strongest evidence against the dominating antiseptic efficacy of Alcohol. All the same, it seemed to me to be impossible that the entire germ-laden epidermic covering could be removed in this manner.

Further experiments, which will appear later on, dealing with the efficacy of antiseptic solutions, now helped me also with regard to Alcohol. If a hand, which, after being treated with Alcohol, proved to be sterile, was washed for over ten minutes *in sterile Serum at the temperature of the body* (in imitation of the conditions appertaining to an operation), one was not only successful in always obtaining germs, which were capable of development, from the surface of the hand, and from the environment of the nail, but the Serum, when examined for cultures, showed that already at the end of five minutes germs were furnished, which increased in number with the duration of the washing in the Serum, and which, at the end of fifteen minutes, yielded one to thirty colonies per cm.^{3} Serum, according to the condition of the hand and the measures which had previously been taken to wash it.[1]

It follows from this that either the Alcohol had not been completely removed from the hand, by means of the subsequent washing of five minutes in hot water, or that the washing had not succeeded, in this short space of time, in making up for the alterations which the treatment with Alcohol had produced on the surface of the skin. The truth of the latter supposition explains what one so commonly notices, that, after such a washing with water, the hands always still retain the peculiar sensation which is produced on them by Alcohol—a creaking sensation when the surfaces of the hands are rubbed together;[2] this sensation is also

[1] I shall later refer more fully to the details of the method of procedure which was adopted.

[2] This sensation of creaking is markedly different from the gritty sensation which is noticeable on a hand from which the fatty material

retained, but to a lesser degree, in the course of a washing with water of seven to ten minutes. It goes without saying that this washing in water should in nowise include anything of the nature of a coarse mechanical cleansing of the hands, if the object of the experiment is merely to form a criterion with regard to the action of Alcohol.

In view of this observation, the result of the investigations of the hands[1] was now very different. The presence of germs on the hands could be confirmed in every case (eighteen experiments) by means of the thread method, whether in the case of the "hand in everyday use" or the artificially infected hand, which, before its investigation, had been treated for three to five minutes with Alcohol, and had then been washed for ten to fifteen minutes in hot Serum. The number of colonies which developed from the thread was usually not large (six to thirty-two), yet there are also to be taken into account the germs which were suspended in the lotion, and thus the numbers attain to a fairly considerable total.[2] Still, the fact remains that the total number of germs is altogether perceptibly less than in control experiments in which the pure mechanical cleansing was also extended so as to occupy the same length of time as the treatment with Alcohol.

In order to explain what is the nature of the action of Alcohol on the skin, *one is essentially compelled to have recourse to chemistry and the test-tube.*

That Ethylic Alcohol precipitates albumen in solution is a well-known fact; for the albuminoid fluids of the human body (Serum, Ascitic fluid, and Hydrocele

has been thoroughly removed with Kaolin; it is thus not merely a sequel to the removal of the fatty material, but is also a sequel to the abstraction of water from the skin.

[1] For reasons which I shall discuss later, one is obliged, in investigations of this kind, to confine oneself to the use of hands which have not come in contact with antiseptic lotions for the space of several days.

[2] When the condition of the hands was bad, the number of the germs was very much increased.

fluid) the use of concentrated Alcohol is requisite, as a rule, in order to produce a lasting precipitate; the more concentrated the albuminoid solution, the more Alcohol is required. How this precipitation is brought about has not yet been determined; it certainly is not exclusively produced through the removal of water, since Glycerine, which is three times more efficacious than Alcohol, does not precipitate albumen, although, by its action, it removes water. But now, whether the Ethylic Alcohol produces this precipitation by the removal of water from the albumen in solution (always colloid in nature), or whether this process enjoins a chemical change, or, finally, merely a simple addition, is here a matter of indifference. The point of importance lies in the question *how quickly the albumen can again part with the Alcohol, in the sense of how quickly the action of the Alcohol can be neutralised.*

If precipitated albumen (formed by the action of Alcohol on human Serum) had been efficiently washed with water, and centrifuged, Alcohol could always still be identified[1] in this water, and in the precipitated albumen, even after the precipitate had been washed for the fifth time, the washing having on each occasion lasted for one to two minutes; thus the Alcohol had not been completely removed by means of this treatment. This precipitate slowly dissolves in excess of water, more quickly in alkalis—particularly if it be gently heated at the same time; it dissolves most readily in human Serum—at blood heat. But it is all the more difficult to dissolve, the longer it has stood, *i.e.* the longer it has been exposed to the action of Alcohol.

This was confirmed by means of an experiment which can be easily carried out.

[1] The identification was carried out with the Iodoform test. Iodoform, which can be recognised by its characteristic odour, even when present in small quantities, is formed in the presence of Alcohol on the addition of Soda or Liquor Potassæ, and Tinct. Iod. when heated at a temperature of 50° C. I must add that in continued investigations one's sense of smell becomes blunted to slight whiffs of Iodoform, and one must then have recourse to the noses of others, which are still uncontaminated by the smell.

2 cm.^{3} of Hydrocele fluid was drawn off into a number of test-tubes of like size. If, now, 2 cm.^{3} of 99 per cent. Alcohol was slowly added to this, whilst it was shaken, the precipitate which formed was soon redissolved; a lasting precipitate first appeared on the addition of 6 cm.^{3} of Alcohol. If, now, an equal amount of water (8 cm.^{3}) was added to this mixture, the precipitate was completely dissolved if the water was added thirty seconds after the precipitation of the albumen; if the water was added at the end of one minute, a scarcely perceptible opalescence persisted; if the water was added at the end of five minutes, the opalescence was already very clearly perceptible, and so on; at the end of twenty-four hours only a very incomplete solution was produced with this quantity of water. *Thus, the longer the precipitate had stood, the more difficult was its solution in a definite quantity of water.*

Alcohol acts in the same way with regard to the cells of the body, only the imbibition is slower in consequence of the precipitation of the cell albumen, and is indeed all the slower the thicker the cell envelope; and when the imbibition by the cell envelope is once an accomplished fact, the removal of the Alcohol is still more difficult than in the case of amorphous albumen.

Fresh pus (cellulitis) was freed from soluble albumen by repeated washings[1] in physiological common salt solution, and divided into two equal portions of about 1 cm.^{3}; these were shaken up with 80 per cent. Alcohol for *five to ten minutes respectively*;[2] the sediment was washed with water, which was changed seven times—quite 50 cm^3; the washing lasted thirty minutes.

In the case where the Alcohol had exerted its influence for *five minutes*, its presence could still be determined in lotion No. 6, but in No. 7 this was no longer clear;

[1] Between the separate washing processes Runne's centrifuge, which is driven by water and acts very rapidly, was made use of, as also later.

[2] After having first experimented with 99 per cent. Alcohol, I was soon able to observe that the Alcohol is more quickly incorporated with the cell when used in lesser degrees of concentration—an observation which I was able to confirm in the experiments which I later partly carried out with regard to the influence exercised by Alcohol on germs.

after the seventh washing the presence of Alcohol could no longer be determined even in the cell sediment. In the case where the Alcohol had exerted its influence for *ten minutes*, none was found to be still present in the lotion after the seventh washing, when it was examined immediately after being centrifuged, whilst the cell sediment still showed an undoubted Alcohol reaction.[1] In both tests the Alcohol reaction, after the fourth washing, was more obvious in the cell sediment than in the lotion.

The following experiment stands in still closer relationship to the conditions which are to be met with in a practical disinfection of the hands.

Pieces of epidermis from the flexor aspect of the finger, such as were to be plentifully obtained in the surgical Poliklinik from cases of Hæmatoma or other contusions, had their fatty material vigorously removed with Kaolin, were rubbed over with Traumaticin [2] on the inner surface and along the edges, in order to prevent the Alcohol from gaining access in that way, were shaken up in 80 per cent. Alcohol for five, ten, fifteen minutes, were washed in running water, and were investigated after a definite interval of time. It was proved that, when the treatment with Alcohol had lasted five minutes, the Alcohol reaction was positive only in solitary cases after a washing in water for five minutes, whilst, when the Alcohol had exerted its influence for a longer space of time, the results were positive in the majority of the cases. Here, however, the epidermis which was employed came almost exclusively from the hands of labourers, the cornified layer of which is very thick.

The experiment, as carried out on my own hands,

[1] The cell sediment, here also, finally gave up all the Alcohol which remained after it had lain for some time longer in a fresh supply of water.

[2] If this precaution be omitted, or if pieces of the cutis, or of the subcutaneous cellular tissue, still remain adherent to the epidermis, the experiment may easily lead to erroneous conclusions, because the Alcohol more quickly and easily attacks the cells which lie deeper, and are not cornified, than it does the horny superficial epidermic layer.

showed that the conditions are different in the case of a delicate epidermis, which has been loosened by a process of washing in hot water with soap and a brush.

If the hands, after they had been carefully washed with hot water, potash soap, and a brush, and scrubbed with a dry towel, were rubbed for five minutes in 80 per cent. Alcohol,[1] and then washed free of this in water for five minutes, the Alcohol reaction was nearly always found to be positive.

It is, moreover, a recognised fact that preparations (even in the form of small segments), which have lain for a brief period in Alcohol, always require a considerable space of time in order that the Alcohol may be fully removed from them. Everyone who wishes to cut with the freezing microtome sections of preparations which have lain in Alcohol, knows this well enough.

Thus, *the result of all this is that it is more difficult to remove Alcohol from the skin, than Ahlfeld and others assume to be the case, and that, therefore, the alterations, which have taken place in the skin from its treatment with Alcohol, cannot be neutralised by a washing with water lasting five minutes.*

Anyone, who is in the habit of regularly or repeatedly investigating the surface of his hands with a stereoscopic microscope, can in this way follow with his eyes the action of Alcohol on the skin. He can thus see that the small creases are more numerous, that on the dorsum of the hand the orifices of the hair-follicles form deep little tunnels, and that on the palm of the hand the infundibula of the sweat-glands are deepened —that they are, at all events, much more clearly marked than on a hand untouched by Alcohol.

These phenomena, of which we have for long been fully aware in the case of preparations hardened in Alcohol, should impress upon us the conclusion that *corpuscular impurities, as also bacteria, are removed from the hand with greater difficulty from the mere fact that it has been treated with Alcohol*, because the folds, the

[1] Alcohol of even higher degrees of concentration has a similar effect, because in such circumstances the skin is very moist.

gland infundibula, and the minute wounds of the corneal layer, in which the germs lie most tenaciously, are deepened by the shrinkage which takes place, and are thus less accessible to the measures taken to cleanse them. It is remarkable that quite the contrary has been asserted. Reinicke and Poten, who both deny that Alcohol acts as an antiseptic, explain the manner of its action thus—that Alcohol may be specially qualified to cleanse mechanically.

This view is no doubt correct in so far as the object to be cleansed has a *smooth surface*, and the *mechanical action* can simultaneously exert a powerful influence. Dirty window-panes can be thoroughly well cleaned when energetically rubbed with Alcohol, as can also, to a certain extent, the fingers of labourers. Such dirt is always greasy, and there can be no doubt that the power of dissolving fat here plays a successful part in what occurs. Poten observes very rightly that on this account Ether acts on the hands less efficaciously than does Alcohol, because the latter combines well with the water which is always present in the skin of our hands, and more particularly so after they have been washed, which is not the case with Ether ; Alcohol can thus, in addition, remove the fatty material from the recesses of the skin by its action. But, besides the removal of the fatty material from the skin, the *mechanical action* is requisite for the removal of the impurities of the skin, and this common action will be rendered less efficacious the more that certain portions of the skin (creases, excretory ducts of sweat-glands, small wounds) are more or less withdrawn from its influence by a shrinkage of the surface of the skin.[1]

[1] Poten attempts to weaken Krönig's objection—that the sterility of the skin, after it has been washed with Alcohol, may be explained simply by the shrinkage of the skin, and a consequent greater difficulty in removing material from it—by means of experiments, in which he never obtained sterility of the skin from merely immersing the hand in Alcohol for ten to fifteen minutes. This experiment is strong evidence, indeed, against the view that Alcohol can act as a germicide, but not against that held by Krönig ; when the skin shrinks, certain parts of it always project with especial prominence, and whenever these parts are not mechanically freed from the septic material which covers them (by

Reinicke and Poten find evidence in favour of this theory in the fact that much epithelium is always to be identified at the bottom of the basin after the skin has been cleansed with Alcohol. According to my experiments, this observation is correct in so far as brushes or other hard implements are used with which to scrub the skin when it is washed with Alcohol; and it is to be supposed that a portion of the most superficial particles of epidermis, which have been loosened by the process of washing, can, after the shrinkage of the skin has been produced by the Alcohol, be removed by the brush, though they were not removed by the previous process of washing. For obvious reasons, it is easier to identify such a sediment in Alcohol, than in the water in which the hands have been washed, particularly if the latter is further mixed with soap. But the more intense has been the process of washing with soap and water, and the more intense has been the subsequent process of scrubbing, by which the skin is dried, the more difficult it is to identify the deposit of epithelium in the Alcohol after the washing.[1]

The simple experiment with soot, or Chinese ink, proves the mere conditional correctness of the theory, which was first suggested by Reinicke, as to the cleansing action of Alcohol.

If a hand, which has been freed from its fatty material by means of Kaolin paste, is rubbed with Indian ink till the skin is completely dry, and the superficial black particles are now removed by means of soap and hot water (without the use of a brush), and the outer epidermic layers are simultaneously loosened, *any further cleansing which occurs by washing the hand in Alcohol is but of a very limited description.* The hand may be rubbed for a quarter of an hour in Alcohol, of

washing the hand in Alcohol, and rubbing it), it is not surprising if they yield germs, together with their epithelium.

[1] Poten (as also Reinicke, so far as I understand him), previous to this treatment with Alcohol, omitted to scrub the hands with a dry towel, a manipulation which largely removes the loosened portions of the epidermis—a fact which is easily verified by a microscopic investigation of the towel.

any degree of concentration, and still it is obvious to the naked eye that the infundibula of the hair-follicles, for instance, remain darkened. If one brings the stereoscopic microscope to one's aid, one finds that the black particles are still present in the infundibula, in the deeper folds of the skin, and in all the irregularities of the surface of the skin (small punctures), which are seldom absent from the hand of a surgeon. If a thread is drawn through hands which have thus been treated, black particles can seldom be detected microscopically on its surface, for it is scarcely possible for such a thread, in its passage across the hand, to penetrate to the bottom of the recesses of the skin, which have been deepened by the action of the Alcohol.

It is thus easier to remove the corpuscular impurities, which are visible to the eyes, by means of hot water, potash soap, and a brush, than by means of Alcohol.

Ahlfeld's experience, apparently so paradoxical, which, however, was also that of Mikulicz[1]—namely, that the disinfection of the skin with Corrosive Sublimate, subsequent to its treatment with Alcohol, has a direct deteriorating effect on the results—is therefore easy to explain; for, by reason of the prolonged washing, the Alcohol and the action of the Alcohol were better eliminated, and accordingly septic material was sooner obtained from the skin.[2]

But Alcohol is more easily removed from the hand by means of hot fluids of the body than by water, and

[1] Mikulicz (*Verhandlungen d. deutschen Gesellsch. f. Chir.*, 1898, p. 29), stated that after a disinfection with Alcohol the hands were found to be sterile in 59 to 78 per cent. of cases, though in only 47 per cent. after an accompanying disinfection with Corrosive Sublimate.

[2] It is not impossible that these antiseptic lotions, particularly the Mercury preparations, on account of their peculiarly strong influence on albumen, are especially qualified to oust the Alcohol. Whilst the creaking "Alcohol sensation" persists on the hands, as I have already stated, even after they have been washed in water for a short time, and Alcohol is still perceptible on the hands, even to the sense of smell, this sensation ceases *almost in a moment* when the hands are dipped in Corrosive Sublimate solution. The creaking sensation is deadened, and the Alcohol is no longer perceptible, even to the sense of smell, unless larger quantities of the Alcohol are conveyed with the hands into the Corrosive Sublimate solution.

hands, which are professedly sterile, soon lose their reputed sterility during an operation.

According to these investigations, the brilliant results, which were obtained in investigations of hands after they had been treated with Alcohol, are to some extent, perhaps, to be ascribed to the influence of the Alcohol[1] on the germs; but, as Krönig already admitted, they are for the most part dependent on the shrinking and the hardening of the skin produced by the action of the Alcohol, which prevents the removal of matter from the natural or traumatic depressions of the surface of the skin. Thus, these qualifications may prove sufficient for operations which last only for a very short time, but, even in such cases, they assure no absolute certainty.

There still remains one point to be shortly discussed —*the concentration of the Alcohol.* Most investigations (Epstein, Minervi, etc.), as also my own, show that in this respect concentrated Alcohol acts with less intensity on bacteria, than do aqueous solutions of the same. This sounds paradoxical. The rapidity with which the Alcohol can be diffused in the cells must certainly, to some extent, be ascribed to its fat-solvent power and to its capacity for mixing with water, and its action on the cell albumen itself, to its power of coagulating albumen; and one would imagine, from the experiments with albumen in solution, that these capacities might be exerted with greater effect, the more concentrated was the Alcohol.

It is undoubtedly this coagulation of albumen which supplies the explanation of this apparently paradoxical behaviour. Concentrated Alcohol coagulates the albumen so quickly that it is only with difficulty that further quantities of Alcohol can penetrate deeper, on account of the coagulated superficial layer of the

[1] The test tube experiments do not afford any conclusions with regard to the practical disinfection of the hands, because, to some extent at least, the germs on the hands are more resistant than those in the Bouillon tubes, and they are, for the most part, not so directly exposed to the action of the Alcohol as is the case in test-tube experiments.

cell, whilst in the case of dilute solutions the rapidity and solidity of the coagulation of the albumen is at first less, the process is more gradually accomplished, and there are in consequence fewer obstacles to oppose the penetration of the solution into the deeper parts. It is impossible to go further into this matter here, except to point out that the explanation suffices both for the experimental results with bacteria and detached cells of the body, and for the experimental results dealing with the whole surface of the hand.

Since our hands are already moist, from their having been washed with water, the Alcohol is, as a matter of fact, never employed in a concentrated form; yet the effect is better—and most experimenters are at one in this matter—if 70 to 80 per cent. Alcohol is employed here;[1] that the procedure is in this way considerably cheapened, is an advantage in itself.

I shall again have occasion to refer, and in greater detail, to the question with regard to the value of Alcohol as a connecting link in the disinfection of the hands, as understood by Fürbringer.

B. *Spirit Soap*

The good results, which have been obtained with Alcohol in the process of cleansing the hands, should suggest to us the propriety of turning our attention to the combination of Alcohol and soap, as we find it in the form of *Spiritus Saponatus*, since, theoretically, it might be assumed that, by means of this combination, separate washings with soap and with Alcohol could be carried out as one act, and thus a saving of time, and a considerable simplification of the procedure, might be gained. Long before Mikulicz published the good results which he obtained with Spirit Soap, I had

[1] If neither time nor circumstances stood in the way of it, it would certainly be most expedient, in the preparation of the hands, to proceed step by step, as occurs in the hardening of preparations, *i.e.* beginning with well-diluted Alcohol (30 per cent.), and again, and again, advancing to higher degrees of concentration.

instituted experiments with this means of purification, because my friend Professor Siebenmann, director of the Otological Klinik, had used it for many years as a means of cleansing the hands and in operations, and had often recommended this method to me for bacteriological testing work, as supplying a quick method of cleansing the skin, as little irritating to the hands, and—so far as could be gathered from operative results—as being very efficacious.

The results of bacteriological tests, following a cleansing of the hands with this soap, which had been made public up to that time, were not by any means encouraging. Landsberg, who experimented with Hebra's Spirit Soap, obtained rather unpromising results, and came to the conclusion that its employment could not be recommended for cleansing the hands. Reinicke equally thought little of its efficacy. The former believes that the soap, the latter that the Alcohol, may be incapable of exerting its action when thus combined.

For the purpose of my experiments, I principally availed myself of *Spiritus Saponatus Officinalis* (*Pharmacopœia Helvetica*, iii.).[1] The experiments dealt both with germs (*Staphylococcus aureus*) dried on glass beads or on silk threads, and with germs in culture media, to which spirit soap was added, and finally with the hands, which, having in some cases received preparatory treatment and in other cases not, were washed with Spirit Soap for varying lengths of time.

I should now be led too far afield were I to enter into the individual details; I therefore confine myself to the statement that the test-tube results were fairly approximate to those which were in each case obtained

[1] "Olive oil 100, Liq. Potassæ 52, Spirit of wine 100, are digested in a closed vessel, during which process they are frequently stirred, until complete saponification has taken place, and until a few drops of the fluid form with water a clear solution. There is now added Spirit of wine 400, rose-water 348; it is allowed to stand for some days, and is then filtered." The preparation is pretty much the same as that described in the *Pharmacopœia Germanica*. Both preparations have a specific gravity of 0·925–0·935. They contain about 10 per cent. Potash soap and about 50 per cent. Alcohol.

in the corresponding experiments with 50 per cent. Alcohol.[1] As has already been fully discussed in connection with the experiments with Alcohol, such investigations are extraordinarily subtle, their results are dependent on the experimental technique, and numerous small sources of error may very materially modify the results. I merely mention here the further circumstance that the septic material is, to some extent at least, washed off from the smooth surface of the beads, and, to a lesser extent, from the threads, by the Spirit Soap itself, though more especially by the subsequent washings. I may, perhaps, refer here to an experiment which I have several times carried out, and which gives a clearer illustration of these circumstances, than does the bacteriological experiment.

In place of the germs, fine soot is carefully mixed with Bouillon in a mortar, is ground down, and then dried, in the form of an emulsion, on glass beads which have been purified by means of heat. One can now perceive with the naked eye (and afterwards with the help of the microscope) how the different fluids (Alcohol in different degrees of concentration, water, Spirit Soap, soap solution, etc.) are capable of dissolving the envelope of soot, in the way of washing it off, and what further influence the subsequent washing processes have upon it.

I need hardly say that these experiments merely throw light on the circumstances connected with bacteriological research, and not on experiments on the hands, where the surfaces are far from smooth, and the fatty material of the skin, etc., places a wholly different aspect on the cleansing effect.

[1] The experiments were in each case threefold. Spirit Soap, then its component parts—50 per cent. Alcohol, and 10 per cent. Potash soap solution. I may mention here that the Potash soap solution can, by its action, arrest the development of the germs, if brought in contact with the nutrient medium. But the reason of this lies chiefly in the altered reaction of the nutrient medium which is thus brought about. If the soap solution had been washed away before the septic material was introduced into the nutrient medium, it had no appreciable effect on staphylococci—not, at least, for any period of action of which there can be any question in the process of cleansing the hands.

In the case of Spirit Soap, one is also tempted to make the mistake of neglecting to pursue the subject oneself, on account of the brilliant results which, apparently, have been obtained in practical experiments. The reasoning is so very simple. That the Spirit Soap contains *soap solution* (about 10 per cent.) and *Alcohol* (about 50 per cent.), both of which are capable of exerting an influence on germs. Now, it is a recognised fact that soap solutions have the power of arresting the development of germs. We know, from the works of Jolles, and from investigations which have originated from various sources with regard to the question of the presence of germs in soap, that germs are never found to be present —so far, at least, as concerns the deeper layers of the soap. Still, these are properties which can have but little effect on the question, one way or another, in the short space of time which is occupied in cleansing the hands.[1]

It might now be supposed that the efficacy of the soap would be increased by the action of the Alcohol. This is, however, quite the contrary of what really occurs, since the active agent in the soap is the free alkali, which, in Potash soap (*Pharmacopœia Germanica* and *Helvetica*), is first set free through its decomposition with water.[2] *But this decomposition does not occur in alcoholic solutions, or only imperfectly.* A simple experiment will demonstrate this—that the officinal Spirit Soap contains no free alkali, as proved by the Phenolphthaleïn test.[3] When water is added in equal quantity to the Spirit Soap, traces of the alkali appear for the first time; thus, the more water, and the less Alcohol, the more free alkali is there present.

Thus the results obtained with regard to the action

[1] Jolles only included in the scope of his tests the germs of Cholera and of Typhus, and the *Bacillus coli*. It is a well-known fact that the majority of cocci, which give rise to pus, are more resistant.

[2] In the officinal Potash soap, according to the Pharmacopœia, no free alkali must be present, or merely traces of it. But the moment that water is added, even in the smallest quantity, the Phenolphthaleïn test is strongly positive.

[3] According to the *Pharmacopœia Germanica*, the officinal Spirit Soap still contains some free unsaponified oil; the deficiency of free alkali is thus self-evident.

of aqueous solutions of soap on bacteria cannot be made use of to explain the action of Spirit Soap. Therefore all that remains is the *Alcohol* itself, which, according to my experiments, is not restrained in Spirit Soap from exerting its influence on germs, or, if at all, only to a slight extent. It is self-evident that this equally applies to the alterative action on the surface of the skin.

Investigations carried out on the hands, after they had been washed with Spirit Soap, also yielded very much the same results, when tested bacteriologically, as those which I was able to obtain with the use of 50 per cent. Alcohol in a series of experiments carried out at the same time. There was only *one* difference to be noted —at least, when a mechanical cleansing with soap preceded the experiments with 50 per cent. Alcohol— that with Spirit Soap the visible impurities were less completely removed, and the outer layer of the epidermis was less efficiently loosened and shed. The former observation can easily be tested with the Indian ink experiment.

A macroscopic or, later, a microscopic investigation of a hand which has been rubbed with Indian ink, dried, and washed with Spirit Soap and a brush, shows that the black particles are by this means less completely removed than if the hand, thus stained with Indian ink, is first washed with soap, hot water, and a brush, and then washed with 50 per cent. Alcohol.[1] The latter solution contributes little more to the process of cleansing, as I have already shown, so far, at least, as concerns the non-prominent parts of the skin. *Thus, in cleansing the hand the important factor is the washing with soap in water, and the use of Spirit Soap, as compared with this, gives no more assistance than does a washing in Alcohol.*

Mikulicz speaks of the action on the deeper parts

[1] In these experiments, in which the one hand was washed with Spirit Soap, and the other with Potash soap and water, and later with 50 per cent. Alcohol, the hand, which was not in use at the moment, was each time covered with a rubber glove, which was drawn on dry.

which the Spirit Soap may be able to exert. An action on the deeper parts is certainly effected by *Alcohol*, though not by soap. The loosening and swelling of the outermost epidermic layer is checked, as there is no free alkali present, which, as I have already stated, is only formed by decomposition of the soap in water, a decomposition which is checked in spirit. Undecomposed soap is certainly deposited on the surface of the hand and in its recesses, which easily explains what Mikulicz observed, that hands which have been treated with Spirit Soap and then covered with thread gloves give up fewer germs in a definite space of time, than do hands which have been treated with Alcohol. The layer of soap, which lies on the hands, increases the difficulty of the septic material being given up. Unna already in the year 1886 proposed that, with this object in view, the hands should be covered with a layer of soap—a proposal which received no support, because on uncovered hands gaps will be formed in such a layer after a few minutes.[1]

If Mikulicz is correct in describing a disinfection with Alcohol as an "apparent disinfection," this description must equally apply to a disinfection with Spirit Soap.[2] If the desire is to aim at a thorough cleansing of the hand, a separation of the ingredients is always to be recommended, *i.e.* that the treatment of the hand with soap and with Alcohol should be undertaken separately, although this separation is more complicated, and occupies more time.

C. *The Value of the Antiseptic Lotion in the Disinfection of the Hands*

As it was impossible to sterilise the hands with certainty, either by the use of pure mechanical means or

[1] It is quite absurd to wash the hands with Spirit Soap in order to prepare them for a subsequent disinfection with Corrosive Sublimate, for instance, because, on account of the soap which adheres to the hands, the Sublimate will be decomposed (insoluble combination with Mercury) and will therefore be unable to act.

[2] *Verhandlungen der deutschen Gesellschaft für Chirurgie*, 1898, p. 30.

of Alcohol, the next step was to have recourse to those old friends—the recognised antiseptics.

Since Geppert has taught us to eliminate the permanent influence of antiseptics in artificial nutrient media, the confidence in the power of antiseptics to kill germs, or to arrest their development, has undoubtedly been diminished to some extent on account of the familiar results which have been obtained by this process. At any rate, a large number of the brilliant experimental results, which were formerly obtained with the use of antiseptics (even those of Fürbringer recorded in the year 1887), must be laid to the account of this source of error.

But it is only in the case of the *preparations of Mercury* that we can with certainty avoid the introduction of the antiseptic into artificial nutrient media. In the case of the Phenol group we have, unfortunately, no precipitant which acts as promptly and as certainly (and, by its action, in no way harms the germs) as does Ammonium Sulphide on preparations of Mercury, and, so long as we know of nothing of this description, the bacteriological test results, which have been obtained in disinfecting the hands with Phenol, as also those dealing with the value of antiseptics in general, are not above reproach. It is generally believed that Phenol and Lysol can be again removed from the skin by washing the part in water. On what this admission is based I could nowhere discover; but our sense of smell alone tells us the contrary, and certainly everyone must have had an opportunity of discovering that the smell of Phenol clings to the hands all day long, in spite of any amount of washing.

What a difficult matter it is to again remove Phenol from albuminoid substances is easily shown chemically:—

It is a recognised fact that albumen is precipitated by Phenol. A relatively large quantity of 5 per cent. Carbolic solution is required, in the case of Serum, to produce a durable precipitation, because the precipitate is each time redissolved in excess of albumen. A

plentiful precipitation is immediately effected with liquefied 90 per cent. Phenol, and the precipitate, when it has stood for some minutes, can no longer be completely dissolved in water.

If 1 cm.^{3} of albumen, which had thus been precipitated by Phenol, was well shaken up for one minute with 50 cm.^{3} of water, and centrifuged, Phenol could no longer be identified in the lotion after the sixth washing (the precipitate did not appear to further dissolve; the albumen reaction was negative in the lotion), whilst the sediment yielded a strong Phenol reaction.[1]

An experiment with the *cell* elements of fresh pus (cellulitis) gave the same result. After the tenth washing in water the Phenol test was negative in the lotion, and strongly positive in the cell sediment.

In view of the practical experience that *Alcohol* removes the smell of Carbolic from the hands more easily than water, these experiments with albumen, precipitated by means of Phenol, were extended so as also to include washings in Alcohol. Here indeed it was shown that Alcohol is better able than water to extract the Phenol. After the sixth washing the Alcohol still contained traces of Phenol; the sediment showed a strong Phenol reaction, which was, however, perceptibly less than when the washing took place in water.

Also, *on the hands themselves* it is easy to prove the presence of the tenaciously adherent Phenol. If they are washed for five minutes with 2 per cent. Carbolic solution, and this is washed off in running water for five to ten minutes, and they are then rubbed with dilute Liquor Potassæ and treated with Diazobenzene-sulphonic Acid, the characteristic red stain makes its appearance.

Thus Phenol cannot be removed from the albuminoid substances of the human body, either by water or by Alcohol, in a space of time which, so far, has been employed for investigations with regard to disinfection.

[1] Two tests were used which proved to be very sensitive. The test with *Diazobenzene-sulphonic Acid* (a strong alkaline solution of Phenol is coloured dark red), and that with *Chloroform* (alkaline Phenol, when heated, forms with Chloroform a carmine red efflorescence).

Since we know of no precipitant of Phenol which is non-injurious both to the skin and to bacteria, the Phenol preparations could not be employed to test bacteriologically the disinfection of the hands, and I accordingly confined myself to the use of *Corrosive Sublimate*, which can be eliminated without difficulty with Ammonium Sulphide. I have all the more readily remained faithful to Corrosive Sublimate, because, from investigations to which no further reference can be made here, I must continue to regard it as the best antiseptic with which we are acquainted (even amongst the various preparations of Mercury), and because, when properly used, and careful attention is paid to the preservation of the hands, it is considerably less hurtful to the skin than is, for instance, Carbolic.

Since, in such investigations, it is necessary to regard the complete elimination of the antiseptic as the primary condition which ensures the attainment of conclusions free from objection, it is well that I should refer here to the difficulties and the doubts which this stringent postulate affords the means of avoiding.

The investigations of Lockwood are well known, which show that Bouillon, to which has been added 1 : 5000 Corrosive Sublimate, still admits of luxuriant growth taking place. These results are much cited (Lauenstein, for instance, lays great stress on their "importance as first principles"); but, so far as I know, Fürbringer alone, of those who have worked at the subject of the disinfection of the hands, has tested them—though with this modification, that, in place of Bouillon, Gelatine was used. He found that, in spite of the addition of one drop of 1 per cent. Corrosive Sublimate solution to 10 cm.^{3} of Gelatine containing nail dirt, colonies could be obtained, and from this he infers that the importance of the introduction of an antiseptic into the nutrient medium has been overestimated, since in practice scarcely does one drop of Corrosive Sublimate solution reach the nutrient medium.

It is a remarkable fact that, with regard to the English experimental results, which, in testing the dis-

infection of the hands, are certainly very accommodating for the experimental technique, it has been forgotten in Germany that these investigations have been repeatedly set on foot by *German* investigators of first importance, and certainly with fairly consistent results. According to the first investigations of Koch, the development of Anthrax spores and of Anthrax bacilli is interfered with by the addition of 1 : 1,600,000 Corrosive Sublimate to the nutrient solution,[1] and is arrested by the addition of 1 : 330,000. Behring and others have fully confirmed these results. Geppert pointed out that *still smaller* additions are sufficient to interfere with the development of the spores, if these had already been previously exposed for a brief period to the influence of Corrosive Sublimate, though not killed.

Behring then showed that the efficacy of the Corrosive Sublimate in the nutrient medium is in inverse ratio to the amount of albumen which is present, that in *Serum* it has considerably less effect on the developmental power of the germs than it has—with the same degree of concentration—in Bouillon; but that still, even in Serum, an addition of 1 : 10,000 Corrosive Sublimate is sufficient to check Anthrax spores in their development. The staphylococci, which Behring put to the test on this occasion, proved to be somewhat more resistant to the action of the Sublimate in the nutrient medium than were the Anthrax spores; in *Serum* the developmental arrest first occurred when the concentration was 1 : 5000. No mention is here made of experiments with Bouillon, or with other nutrient media, but it is clear that, in nutrient media which contain little albumen, the Corrosive Sublimate must act with almost proportionately greater intensity on staphylococci, than was shown to be the case for Anthrax spores and Anthrax bacilli.

In the pursuit of other questions, I instituted a

[1] Peptonised solution of meat extract, or extract of meat gelatine. Koch insists, with regard to this, that the numbers can only hold good for these nutrient solutions, or, at most, for nutrient media composed on wholly similar lines.

number of experiments in this direction, of which I can only briefly convey some of the results. For *Bouillon*,[1] an addition of 1 : 200,000[2] Corrosive Sublimate was sufficient to prevent staphylococci (in their greatest possible resistance) from further developing, when the amount of septic material which was introduced was but of a trifling nature (one platinum needle).

When the concentration was from 1 : 50,000 to 1 : 75,000 the development ceased, even with the addition of larger quantities of germs (0·5 culture fluid). An addition of 1 : 300,000 Corrosive Sublimate procured, in isolated cases, a cessation of development—at all events, always a considerable interference with it, as compared with the control cultures. The experiments with Gelatine gave at first a disproportionate result, for it is difficult to equally distribute the Corrosive Sublimate throughout the Gelatine, especially when it is no longer semi-fluid.[3]

Evidence in favour of the correctness of Geppert's results is also to be found in experiments where, after I had disinfected the hands with Corrosive Sublimate, and eliminated the Sublimate with Ammonium Sulphide, I obtained sterile results in culture media with threads which had been used to investigate the condition of the hands, when the culture media were mixed with Corrosive Sublimate in a dilution of 1 : 300,000 to 1 : 500,000; whilst, in one set of nutrient media, in which no Corrosive Sublimate was present, cultures developed.

The amount of Corrosive Sublimate which is present in the nutrient medium after such investigations

[1] Meat-peptone Bouillon of the usual description.

[2] This, computed according to the experiments of Fürbringer, was about one drop 1 in 1000 Corrosive Sublimate solution to 10 cm.^{3} Bouillon. In my investigations, the measurement was carried out by means of a burette, as I soon observed that the smallest inaccuracies were in a position to very considerably modify the experimental result.

[3] It is self-evident that the process of sterilising the nutrient medium must occur *before* the addition of the Corrosive Sublimate, *i.e.* that the Corrosive Sublimate is not to be heated along with the nutrient medium, for otherwise it is decomposed. Gärtner and Plagge, in investigations of this description, also obtained disproportionate results with Gelatine; more than once, on adding 1 : 200,000 Corrosive Sublimate, the developmental process ceased.

of the hands, is—at least, in a definite form of procedure—larger than the surplus Corrosive Sublimate solution, which has been introduced, would lead one to suppose; for there is, in addition, the Corrosive Sublimate, which has become lodged in the epidermic cells, and which, in nutrient media containing albumen, is again partly extracted; and, when it comes to be a question of the smallest details, Corrosive Sublimate, even from such a source, must not be disregarded.

There can, thus, be scarcely any doubt that, in such investigations, the complete removal of the Corrosive Sublimate must alter the experimental result, and that, whenever this does not occur, the result cannot be held as free from objection.

But, for reasons to which I shall again refer later, there is nothing now so very astonishing in the supposition that, even without this elimination of the Corrosive Sublimate, positive culture results *can* be obtained; but, all the same, what is astonishing is the observation of Mikulicz,[1] that, when in 28 per cent. of cases of disinfection with Corrosive Sublimate the Mercury had been precipitated by means of Ammonium Sulphide, he was quite unable to observe, from the number of colonies obtained, any material difference between the cases in which this precipitation had taken place and those in which the investigation had proceeded [2] without the Mercury having been precipitated. Poten, who employed the same method of procedure in his experiments (dipping the points of the fingers into plates of Agar), was at least able to note a great difference in the experimental results each time that he had precipitated the Corrosive Sublimate, or had washed the hands merely for five to ten minutes in hot water.

The importance here of the precipitation of the

[1] *Verhandlungen der deutschen Gesellschaft für Chirurgie,* 1898, p. 26.

[2] Gottstein (*Beiträge zur klin. Chirurgie,* xxv.) certainly states, in the work which he published from Mikulicz's Klinik, and which appeared after I had concluded this work, that the difference observed was not one to be underrated—75 : 56% sterility, and that in a few individual cases it was relatively even greater.

Mercury was impressed upon me by investigations which I carried out on my own hands by means of the thread method. At first the results which I obtained with cultures, after the hands had been thoroughly disinfected with Corrosive Sublimate, were almost always negative.[1] But when, surprised by this astonishing result, I treated a portion of the threads with Ammonium Sulphide, a wholly perceptible difference in the experimental results became apparent; frequently the control threads, which had not been thus treated, remained sterile, whilst the threads, which had undergone this treatment, still produced cultures. This was most astonishing in certain cases where my hands were slightly rough; here the threads, which had not been treated, remained sterile for the most part, whilst those which had been treated furnished cultures without exception. The microscopic examination of threads which had been treated with Ammonium Sulphide indicated that, as a matter of fact, Mercury was present here, from the slight brown discoloration of the threads at different points. Corrosive Sublimate had thus been transmitted to these threads, in spite of the skin having been thoroughly washed, by the process of rubbing the thread on the surface of the skin and in the nail environment; and, though certainly very small in amount, it had been able to check the development of the few germs, a fact which is not at all surprising after the experiments of Geppert.

A simple experiment soon enlightened me with regard to the extent to which the Corrosive Sublimate becomes established in the skin.

After the hands had been disinfected according to the extended method of Fürbringer (five minutes for each act), they were first washed for five minutes in Alcohol (as Corrosive Sublimate is more easily dissolved in this), and were then washed free from this in hot water for ten minutes, dried, and rubbed with 10 per cent.

[1] I regularly endeavoured to remove the Corrosive Sublimate by washing the skin for five to seven minutes in sterilised water, which was changed repeatedly.

Ammonium Sulphide solution. The result is shown on Plate V.;[1] the hands were uniformly stained black in colour, and certainly to a greater extent on their flexor aspect; the skin was dry and hard, and in the course of the next few days became rough and cracked.

Since the nature and the extent of this discoloration of the skin by Mercury can explain the nature and the extent of the impregnation of the skin with Corrosive Sublimate, I shall therefore deal with the question somewhat more fully.

It appeared, under the stereoscopic microscope, that the discoloration was almost exclusively localised in the recesses, while the most prominent parts of the epidermis were not discoloured. The numerous folds and small creases on the palm of the hand were shown to be dark brown in colour, and the discoloration undoubtedly extended to the deeper folds, though not to the deepest parts, for on opening the hand or stretching the skin these last-mentioned folds were shown to be free from the discoloration.[2] On the dorsum of the hand the colouring matter was thinly deposited in the shallow folds, but somewhat more plentifully in the orifices of the hair-follicles. In the space under the nails and the matrix of the nails, the discoloration was particularly well marked; the nails themselves showed quite a faint brown discoloration at their upper (more delicate) extremity.

Clear striæ, arranged in an oblique, or transverse, manner, showed strikingly on the flexor aspect of the ungual phalanges (in Plate V. they are particularly noticeable on the index finger). Their presence here was due to the fact that silk threads had been drawn through the hands, for the purpose of bacteriological investigation, before the precipitation of the Mercury. This statement is sufficient to show that Mercury is

[1] In order to illustrate the differences in colour, the hand of a colleague was jointly depicted.

[2] This observation gives us a hint how important it is that the surface of the hand, which is to be cleansed, should, in the course of the various processes of cleansing, be thoroughly stretched and smoothed out.

removed by means of such threads, and is in this way conveyed into the nutritive medium.

This discoloration of the skin, from the deposit of Sulphuretted Mercury, merely affects, however, *the superficial epidermic layers.* I have, whilst operating, excised small portions of skin, which had as usual been disinfected with Corrosive Sublimate, and these I duly washed in water to remove the Sublimate; the Mercury which remained was then precipitated with Ammonium Sulphide solution, and the pieces of skin were cut into sections. The discoloration of the skin naturally appears but very faintly in such sections, and can only be recognised in the folds of the skin with the help of a microscope. It nowhere extends beyond the superficial epidermic layers, and certainly nowhere penetrates into the gland ducts and hair-follicles, no doubt on account of their fatty contents. That the parts of the skin, which had thus been stained brown, could, with the aid of the stereoscopic microscope, be easily removed by being scraped with a knife; that, on the smooth parts of the skin, the discoloration could be removed by a vigorous scrubbing with pumice-stone; and that no fresh discoloration of the parts of the skin, which had thus been rendered white, could be reproduced by a subsequent application of the Ammonium Sulphide test—proves, in addition to what I have already pointed out with regard to the threads, that this deposit of Corrosive Sublimate is confined to the outer epidermic layers alone.[1]

[1] This result is, besides, but little modified even when a *compress of Corrosive Sublimate* is applied to the skin. I shall now briefly refer to one or two results of investigations which were thus carried out both on the living skin, and on sections of skin, after the application of a compress of Corrosive Sublimate for twelve to twenty-four hours. The Corrosive Sublimate was found to have impregnated the outermost epidermic layer, which had at the same time been macerated by the compress. *If this macerated layer is rubbed off, but little more Mercury is present below;* if the layer is left *in situ*, the reaction with Ammonium Sulphide is naturally considerable. *Here again the Corrosive Sublimate does not penetrate further than the superficial layer of the epidermis.* It is interesting, in this connection, to note that the epidermis absorbs from the compress a great part of the Corrosive Sublimate; a distinct Mercury reaction is produced only at those parts which lie upon the surface of the skin, and

Without the help of artificial mechanical interference, the discoloration of the skin, especially in the region of the nail, lasts for several days, even though the hand during this time be subjected to a frequent mechanical cleansing, *i.e.* it persists till the most superficial epidermic layer is cast off or is rubbed away. The entire surface of the skin is rendered very dry by the process of precipitation, and therefore the process of scaling is more rapidly effected than if the skin was in its normal condition.

Thus this experiment, which was frequently repeated, proves—

(1) *That the Corrosive Sublimate cannot be further removed from the skin by means of the ordinary solutions*, and that, undoubtedly, the layer containing the Mercury is to some extent first shed when the epidermis commences to scale, which occurs more rapidly where the surface of the skin is smooth, than where there are folds present.

(2) *That after the skin has been subjected to the action of the Corrosive Sublimate for a space of time of which there is any question at all in the process of disinfecting the skin, the Mercury is present only in the most superficial layers of the epidermis, and has not the power to penetrate to the deeper parts*; it does not reach the bottom of the folds, for the good reason that whilst the hand is being washed it is held in a flexed position, and the fatty material cannot be satisfactorily removed from the depths of the folds; nor does it reach to the deeper parts of the natural interstices of the skin, and accordingly the germs which lie there are not affected by the Corrosive Sublimate.

(3) *That, owing to the manipulations which occur during*

which form part of the epidermis. Such a compress is always valuable as a means of preparing the field of operation, since it macerates the layer of epidermis on which the germs lie, which can then be easily removed by a process of washing. A compress of *Bran* infusion may, in this respect, be more efficacious. According to Salzwedel, a compress of Alcohol does not macerate the skin, nor is it able (at least in a concentration of 45 per cent.) to kill all the germs of the skin, nor to arrest their development in artificial nutrient media.

an operation, and especially the tying of ligatures, *the layer of epidermis, which is impregnated with Mercury, is removed*, and therefore the underlying portion of skin must be regarded as "not disinfected."

It has been shown that, when the hands are thus treated with Ammonium Sulphide, it is merely the *surplus* Corrosive Sublimate which is eliminated, though experimenters, who have carried out this process of precipitation with care and sufficient thoroughness, cannot doubt the fact that the Corrosive Sublimate adheres to the skin with extraordinary tenacity. This can easily be proved at any time. Anyone, who is in the habit of using Corrosive Sublimate to disinfect his hands, can at any time stain the epidermis of his hands, and particularly in the region of the nail, by means of Ammonium Sulphide. This occurs even though the hands have not come in contact with any preparation of Mercury for the space of twenty-four hours,—indeed for several days, unless his skin is one which peels off very rapidly—and even though he has repeatedly cleansed his hands mechanically during the day.

A small excursion into the domain of chemistry will sufficiently explain this phenomenon.

Corrosive Sublimate, like most of the inorganic preparations of Mercury, is included amongst those media which precipitate albumen. A precipitate produced with human Serum is wholly insoluble in water, even though it be immediately washed; but it is easily dissolved in diluted alkalis and acids, *but principally in excess of albumen solution* (*serum*). It is about time that the fable, which is still commonly cited as to the "insolubility" of Mercury-albumen, should disappear. I shall prove elsewhere how important this solubility in excess of albumen is for the comprehension of the antiseptic action of Corrosive Sublimate; but let me simply state here *that the precipitate, which is formed by the action of Corrosive Sublimate on fluid albumen of the body, is easily dissolved as soon as sufficient fresh quantities of albumen in solution*, i.e. *in excess, are added*, and that the process of solution takes place more easily, and more rapidly

with excess of Serum, than it does with physiological common salt solution.[1]

Corrosive Sublimate also forms a chemical combination with cell albumen. From amongst my numerous experiments which deal with this point, I may here quote but one as an example:—

If the cells of fresh pus, freed from fluid albumen, were shaken up with a weak solution of Corrosive Sublimate, were repeatedly washed in water and then again centrifuged each time, the water in which they were washed soon proved to be free from any trace of Mercury, whilst the cell sediment gave a distinct Mercury reaction. No further washing in water and in Alcohol had any power to extract more Mercury from the cells.

The affinity which exists between cell albumen and Corrosive Sublimate (or its Mercury) is so great that, if the Sublimate be used in considerable dilution (1 : 7000 to about 3 cm.^{3} cell sediment), and the mixture be frequently shaken up for several hours, the fluid, which, by its being centrifuged and repeatedly filtered, has been wholly freed from cells, now no longer contains any Mercury ; *thus the whole amount is absorbed by the cells.*

The cells, however, in their combination with Mercury, only differ from the Mercury-albumen combination which is formed in Serum, in so far that the Mercury can be removed with greater difficulty and less completely in the presence of fluid albumen in excess; and if the Sublimate has exerted its action for any length of time, it is certainly only by the disintegration of the cells that the possibility occurs of the Mercury being reproduced in solution.

Thus, after washing the skin with Corrosive Sublimate (and, naturally, carefully washing this off again), *the Mercury, which can still be shown to be present in the skin*

[1] Therefore, in order to form a permanent precipitate with serum, a multiple quantity of 1 in 1000 Corrosive Sublimate solution is required, because the precipitate which is first formed in the upper part is wholly, or partially, dissolved, so long, generally, as albumen is present in excess.

is in chemical combination with the cells, and can, therefore, no longer be removed by washing in water, or in Alcohol. If leucocytes, and pus cells, form with the Mercury a firmer chemical combination than does the precipitate of fluid Serum, the combination is even firmer in the case of epidermic cells, where, however, the impregnation occurs more slowly than in the case of the more delicate cell elements.

If hands, which have been treated according to Fürbringer's extended method, and then washed in water for ten minutes, were washed in Serum for half an hour (approximately the duration of an operation), and rubbed dry, they showed a fairly marked brown discoloration, which was particularly noticeable in the folds, when treated with Ammonium Sulphide. This discoloration is, however, less marked than that which was obtained before the hands were washed in the Serum [1]

Since it appeared to me that the extent of this combination of cell albumen with Corrosive Sublimate, its durability, and the ultimate fate of the cells themselves were all questions of considerable interest, I set on foot a large number of experiments (96) with small pieces of skin, which were obtained from cases under operation. I made use partly of the skin as a whole, with some subcutaneous cellular tissue attached, and partly of the epidermis alone (especially that of fingers). In the former case, the principal object in view was the deep action of the Corrosive Sublimate.

The skin was experimented on in the form of a number of small pieces, which were as much as possible alike, some freed from their fatty material, and others not. The pieces lay in 1 in 1000 Corrosive Sublimate solution for different lengths of time, were then washed in running water for five to ten minutes, and were cut through the middle. These segments, which in the centre were unaffected by the lateral penetration of the Corrosive Sublimate, were, after they had been treated with Ammonium Sulphide and repeatedly washed,

[1] This previous discoloration was carried out on separate fingers.

examined with the stereoscopic microscope (also later in sections), in order to ascertain the extent to which portions were discoloured by the action of the Ammonium Sulphide.

The pieces of skin, from which the fatty material had not been removed, absorbed, by their external surface, the Corrosive Sublimate only very slowly, even when the solution was rubbed in. The discoloration which, at the end of fifteen minutes, was produced by the action of the Ammonium Sulphide test, was scarcely perceptible. The discoloration was still very faint even when the treatment with Corrosive Sublimate was prolonged to thirty minutes. The removal of the fatty material by means of Kaolin had in itself a fairly beneficial effect, as had also a preliminary treatment of the segments with Ether; but a *preliminary treatment of the part with Alcohol* was most efficacious in assisting the penetration of the Corrosive Sublimate, a point to which I shall again refer later. But even here the deeper layers of the epidermis were still free from the presence of Mercury, when the cutis (from within) had already been fully saturated with Mercury. The hair-follicles and sweat-ducts, finally, still proved to be free from the presence of Mercury when the adjacent layers of epidermis were already fully saturated. Here again a preliminary treatment of the part with Alcohol, of some hours' duration, helped somewhat to modify the result, but without complete success.

I now sought, by experimenting with pieces of *epidermis* pure and simple, to obtain information, especially with regard to the durability of this combination of the epidermis with Mercury, with regard to the fate of the cells, and with regard to the ultimate antiseptic value. An objection, which may be raised with regard to this, is that the conditions are very different in the case of living tissues; but we know, especially with regard to the epidermis, that the cells, when they no longer form part of the body, can be maintained "viable" for days. Undoubtedly, in experiments on pieces of skin which have thus been removed from the

body, the absence of secretion from the sweat-glands must be taken into consideration, and this for two reasons—first, that with aqueous solutions impregnation occurs more easily; and secondly, that the outer cornified epidermic cells are of lower vitality.

The pieces of epidermis, which had been separated from the cutis as the result of accidental injuries or by dissection, were thoroughly freed from their fatty material by means of Kaolin paste, were washed with soap and a small brush, and then dried—in fact, received the same treatment as our hands undergo when they are mechanically cleansed. In order to prevent the Corrosive Sublimate from being absorbed by the under surface, and by the edges, these were covered over with Traumaticin (solution of gutta-percha in Chloroform), which answered the purpose very well.

The results resemble those which I have already quoted, in so far that here again the absorption of Corrosive Sublimate was confined to the outer epidermic layer, when, at least, the treatment of the part with Corrosive Sublimate did not extend beyond the time which is commonly devoted to the practical disinfection of the hands. A preparatory treatment of the parts with Alcohol facilitated the absorption very considerably, in rapidity of action and in extent.

The Mercury, which had once become established in these pieces of epidermis, could no longer be extracted with water, even though the experiment was prolonged for several days (with frequent changes of the water in which the parts were bathed). At incubation temperature, the outer epidermic layer separated at the end of two or three days in the form of a membrane; the surface, which was now exposed, proved to be free from the presence of Mercury, whilst the portion, which had separated, gave an obvious Mercury reaction.

The extraction of the Mercury failed even in *Alcohol*, though the pieces which were washed in it for a period of several days exhibited a slightly lighter shade of discoloration than those which were soaked in water. With *physiological common salt solution* a partial extraction

occurred, if the pieces were washed for several days; the result was still better in nutrient Bouillon, and best of all in *Serum* (Hydrocele fluid). Pieces of epidermis, which were kept in this medium at incubation temperature, showed at the end of twenty-four hours a Mercury reaction which was scarcely perceptible.

Corrosive sublimate, or its Mercury, which has become attached to the epidermic cells, is not, however, antiseptically inactive, if this be regarded in the restricted sense of influencing the nutrient medium in a manner disadvantageous to the germs.

Pieces of epidermis, which, after being thoroughly prepared, had been shaken up for one hour with 1 in 1000 Corrosive Sublimate solution, and then efficiently washed in water, were placed in tubes of Bouillon, and Serum (5 cm.^{3}); these tubes were then immediately, or later, inoculated with minute quantities of septic material (one platinum needleful of staphylococci, or Anthrax bacilli). In every case the cultures developed much more slowly than in the control tubes; in Serum, especially, often no development occurred till two or three days had elapsed. The Anthrax bacillus proved to be most affected in this way, and in some cases it did not develop at all in tubes in which were segments of skin to which Corrosive Sublimate remained attached. If the tubes had stood in the incubator for some hours before they were inoculated with the septic material, the interference with development was still more marked, and then, even in the case of staphylococci, often no development took place at all.

This is the place at which we must now consider the preparatory action of Alcohol, in a disinfection with Corrosive Sublimate. Though, from my own investigations, I am forced to deny to some extent that Alcohol, from a practical point of view, is of any value as an antiseptic, or as a means of cleansing the skin (so far, at least, as concerns *the recesses* of the skin, which come most under suspicion), still, full recognition must be bestowed on what Fürbringer laid down as the principal qualification possessed by Alcohol—its capacity to dis-

solve fat and to extract water, and thus to prepare the skin for the antiseptic which is conveyed to it in aqueous solution.

This preparatory action of Alcohol has frequently been corroborated since then in practical experiments, but it is principally by means of experiments conducted in the laboratory, which are less liable to harbour sources of error, and which, by comparison with such practical observations, can be used as a means of control, that we are able, in some measure, to obtain an explanation with regard to the nature of this action.

I now propose to give a short account of the experiments which I carried out with this object in view.

My first endeavour was to simulate the conditions in practice. Pieces of epidermis, which had undergone no preparatory treatment, and were just as they had come from the flexor aspect of the hands (the hands of labourers for the most part), were placed, some in 70 per cent. Alcohol for ten minutes, some in Alcohol for five minutes and in 1 in 1000 Corrosive Sublimate solution for five minutes, some in 1 in 1000 Corrosive Sublimate solution for ten minutes; and, after they had been thoroughly washed in sterilised water, they were placed in tubes of fluid Serum. Whilst, in the case of the pieces which had been treated with Alcohol alone, or with Corrosive Sublimate solution alone, cultures developed (certainly after a longer interval, and less plentifully, than in the control tubes), no cultures in most cases developed from the pieces which had first been treated with Alcohol and then with Corrosive Sublimate solution.

This is well demonstrated by the *chemical* experiments, to which I have already to some extent referred, in which pieces of skin, or of epidermis, which had been treated in various ways with Alcohol and Corrosive Sublimate, then washed, and treated with Ammonium Sulphide, were examined with the stereoscopic microscope after they had been cut through the middle, or into sections. The preparatory effect of the Alcohol was very striking, for the Corrosive Sublimate was

absorbed relatively much more quickly, than in the case of any other preparatory process.

Thus, in the preparation of the skin, if Alcohol is used for the purpose, the process does not confine itself to a mere removal of the fatty material and of the water from the surface and recesses of the skin, but combines with this a greedy absorption of the Corrosive Sublimate by the *cells* themselves.

This is sufficiently well proved by the following experiments, which were frequently repeated :—

Pus (acute cellulitis of the neck), which had been freed from fluid albumen by repeatedly washing it in physiological common salt solution, was then divided into two equal parts (about $\frac{1}{2}$ cm.^{3}). The one portion of the sediment was shaken up with 70 per cent. Alcohol for one minute, and centrifuged for five minutes; both portions of the sediment were shaken up with 1 : 2000 Corrosive Sublimate solution for half a minute, were centrifuged for eight minutes, were then washed five times, and centrifuged. After the last two washings, no more Mercury could be identified in the water. Both portions of the sediment were treated with Ammonium Sulphide. Whilst that portion of the sediment, which had undergone a preliminary treatment with Alcohol, was stained black, the other portion, which had undergone no such preliminary treatment, was merely stained brown. The difference in colour was always very striking.

I merely mention here in passing that, when this experiment is carried out with *germ sediment* (from cultures on a large scale), precisely the same result is obtained, and that, by means of a preliminary treatment with Alcohol, the destruction and the developmental arrest of bacteria (dried on silk threads and glass beads) with Corrosive Sublimate solution is promoted to an astonishing extent—a result which can easily be understood from the above experiments.

Thus, *a preliminary treatment with Alcohol facilitates in an astonishing manner the entrance of aqueous solutions of Corrosive Sublimate into the cells.* With regard to the

explanation of this fact, I cannot now say more than that, according to my experiments (which were carried out with a succession of media for removing the fatty material and the water from the skin), it is, to some extent, the power possessed by Alcohol of removing the fatty material from the skin, which enables the Corrosive Sublimate solution to penetrate more easily into the cells. But the probability is that the more or less advanced dehydration of the cells is the real explanation of the greater ease with which the aqueous antiseptic solutions are enabled to make their way into the cells, when such solutions are used immediately after the Alcohol has acted for a relatively brief period.

This suggests the propriety of simplifying the method by means of which the preparatory action of Alcohol is brought into play, by using Alcohol and the disinfectant simultaneously, *i.e.* the *disinfectant in alcoholic solution.* But, since the time when Koch pointed out that antiseptics, which are more or less active in aqueous solution, lose their power as disinfectants when dissolved in Alcohol of high percentage, his statement has been confirmed by a number of experimenters (Epstein, Minervi, and others), and my own experiments, dealing with the subject, have given a like result. The explanation of this phenomenon depends on the fact,—as can easily be proved by experiments carried out according to the methods which I have already mentioned,—that, with solutions of Corrosive Sublimate, for instance, in Alcohol of high percentage, the passage of the disinfectant into the cells is rendered much more difficult, at all events proceeds much more slowly, than is the case when aqueous solutions (without previous treatment with Alcohol) are used. I cannot as yet come to any certain conclusion as to how this comes to pass. We know that Absolute Alcohol, or Alcohol of high percentage, as such, exerts a weaker antiseptic action, for the good reason that it cannot make its way into the cells on account of the coagulation of the surface of the cells, of which it is the cause; and, in addition, the Corrosive Sublimate

is not so fully broken up in Alcohol as in aqueous solutions.

Epstein found that Thymol acts as a better disinfectant in 50 per cent. spirit than in water, whilst, according to the experiments of Minervi, the germicidal action of antiseptics in alcoholic solution varies inversely with the strength of the Alcohol. I can add, as the result of different series of experiments, that a small addition of Alcohol to the disinfectant solution (10–20 per cent.) intensifies its action on germs as compared with simple aqueous solutions, and that it assists the antiseptic to gain better access to the cells—as may be shown by experiments such as I have already mentioned.

After this disquisition with regard to experiments carried out in the laboratory, which may appear to be somewhat of a digression from the subject of the disinfection of the hands, we shall now again turn our attention more definitely to the practical side of the question.

When we hear from Mikulicz, that the elimination of Corrosive Sublimate on the hands by means of Ammonium Sulphide has no effect on the number of bacteria obtained; when we learn from Ahlfeld that the use of Corrosive Sublimate solution, after washing in Alcohol, has simply the effect of making the results worse; and when we call to mind the numerous investigations of former days, and particularly those of Fürbringer in his first work, in which, despite a disinfection with Corrosive Sublimate and without any elimination of the Mercury, cultures were more or less often obtained from the hands by one or other of the methods of investigation, we are certainly tempted to again take a pessimistic view with regard to the results obtained by Geppert as to the value of Corrosive Sublimate as a disinfectant, in relation to the practice of disinfecting the hands.[1]

[1] It is remarkable that these investigations of Geppert's are constantly considered as affording proof of the inefficiency of Corrosive Sublimate to act as a disinfectant. Anyone who critically examines the works of Geppert, not merely with regard to individual series, but rather with regard to the matter as a whole, will note how strongly his experiments

But now we know that the Corrosive Sublimate and the cell albumen form a combination which is hard to break, there is nothing so very surprising in the positive bacteriological reports with regard to hands which had been washed with Corrosive Sublimate, and from which the Mercury had not afterwards been eliminated. They merely prove that in the region of the nail (the region, which was almost exclusively considered) germs were present, more or less loosely embedded in a fatty menstruum. We can now see that, as a matter of course, the Corrosive Sublimate, which is firmly lodged in the cells of the body, is unable to exert any further influence on these germs. Apart from the individual peculiarities of the skin, which had no doubt a very great deal to do with the dissimilarity of the results, we can only suppose that these deplorable results were due to the fact of *the mechanical cleansing of the skin, and the removal of the fatty material from the skin, having been carried out either with too great rapidity, or in an improper and disproportionate manner.* With Corrosive Sublimate, or any other antiseptic, one must be well on one's guard against the possibility of any small sins of omission; it should never be forgotten that the action of antiseptic solutions is limited, and, more especially, that in their action they follow a well-defined chemical course, which is not well adapted to the requirements of the investigator.

The question whether a 1 in 1000 Corrosive Sublimate solution, employed for five minutes, affects the vitality or the developmental capacity of germs, is one which may here remain undecided; it is admittedly a very difficult question to solve. From my investigations on the subject, I am forced to believe in the existence of such an effect. But, in any case, it is a preliminary condition that the antiseptic should sufficiently come in contact with the germs, and this cannot occur so long as the germs

give evidence *in favour* of the efficiency of Corrosive Sublimate to act as a disinfectant. It is beyond the compass of this work to go further into this matter; I shall, however, refer later to individual results obtained by Geppert which are of importance in this connection.

are enveloped by a fatty menstruum, or are protected by the outer horny layer of the epidermis.

At this point I must again refer to the results of investigations which I have already mentioned in connection with the *mechanical* cleansing of the skin. It is only possible, then, to expect advantage from a disinfection with Corrosive Sublimate when (1) the skin has been thoroughly freed from its fatty material; (2) the skin has been relaxed by means of soap and hot water, *i.e.* its folds and recesses have been somewhat flattened out, and, in particular, the infundibular glands have to some extent been bevelled; (3) both the coarse impurities, and the most superficial horny epidermic cells, which are, besides, of low vitality, have been removed by means of the mechanical action of an instrument, which is always best accomplished by the use of a brush, followed by a good rubbing with a rough towel.

It is very evident that, in the case of the nail environment, recourse must be had to the use of special instruments, which must be repeatedly employed in the case of a primary cleansing. *It is quite useless to wash hands, which have had no preliminary treatment, in antiseptic solutions.*

It is only by taking these factors into account that the Corrosive Sublimate can be enabled to impregnate the skin, so far as it is at all in a position to do this. As I have already pointed out, there are natural limitations to this, for the Corrosive Sublimate does not penetrate deeper than the epidermis (on the palm of the hand, no further than the outer epidermic layer)—not, at least, during its use for a period of which there can be any question in practice; it is lodged, for the most part, in the minute crevices, and can only be removed again from these with the greatest difficulty, even by mechanical means, because the rubbing to which the parts are subjected (in operations) only affects the prominent portions of the epidermis; it does not penetrate into the excretory ducts of the sebaceous glands, or into the hair-follicles, because the fatty

material is not sufficiently removed from them. Still, it is quite possible that the infundibular orifices of the hair-follicles, where the corpuscular impurities are so tenaciously adherent, can be so freed from the fatty material, by a suitable process, that here, even, the Corrosive Sublimate will be deposited.

I have already pointed out how the addition of Alcohol (in dilution) plays such an important part in the matter. The impregnation of the skin with Corrosive Sublimate is then incomparably deeper than when any other means is used to prepare the skin—always presupposing, of course, that a mechanical cleansing process has already taken place. For the surface of the hand a paste of Kaolin, when applied with suitable energy, does excellently to remove the fatty material from the skin; but Alcohol penetrates to a greater depth, and prepares the way for the Corrosive Sublimate solution, not merely by its ability to remove the fatty material, but principally by its power of dehydration. There is no doubt that the deeper, and the more powerful, the penetration of the Corrosive Sublimate, the more the superficial epidermic layers become lowered in their vitality, *i.e.* the more a delicate hand suffers. When the hands are of this delicate description, they are less liable to be injured by the use of the Kaolin paste, and the loss of the deeper action can, to some extent, be made up for by repeatedly washing the hands in Corrosive Sublimate solutions during the operation. The effect is, however, never quite the same, and, besides, careful and unwearying attention bestowed on the condition of the skin helps greatly, in most cases, to do away with the tenderness of the hands.

When the disinfection of the hands with Corrosive Sublimate is regarded from this point of view, its limitations are clearly defined. Even supposing that one is willing to believe that the Corrosive Sublimate can affect the developmental capacity of the germs, when thus employed for a restricted length of time, still one has to deal with the fact that the Corrosive Sublimate does not include in this action the orifices of the sebaceous glands

and the hair-follicles, and, what is more, that bacteria, which are encased in an envelope of fat, can remain behind in the small accidental wound-cracks of the epidermis, which are seldom absent from the flexor aspect of the terminal phalanges of the hands of surgeons, as well as in the irregularities of the space under the nails, where they lie secure from the action of the Corrosive Sublimate.

The question thus comes to be whether, under such circumstances, there is really any object at all in treating the hands with Corrosive Sublimate. From my own investigations, I can unhesitatingly reply to this question in the affirmative. *Our object is to achieve as much as we possibly can, if we cannot achieve everything, and a disinfection of the hands with Corrosive Sublimate is unquestionably a further means to this end.*

When I now proceed to give a short report of the results of the bacteriological tests to which I subjected hands after they had been disinfected with Corrosive Sublimate, I am not altogether satisfied as to the advantage of employing *percentage figures* to illustrate the value of any one definite method of cleansing the hands. Such figures are of trifling import in an experimental work which aims at showing whether the hands can be rendered sterile with the help of definite methods, or to what extent they can be purified. If the figures refer to a *large number of persons engaged in carrying out the experiments*, then differences arise with regard to the duration and the intensity of the measures employed, with regard to the extent to which these measures were understood, with regard to the condition of the hands, and, not least, with regard to the procedure by means of which the investigation was carried out. These are factors which cannot, for the most part, be checked, and which cannot, therefore, be taken into account. But even supposing that the figures have been obtained by investigations in the case of a *single individual*, they cannot express the absolute functional capacity of a definite form of procedure, for, as I had sufficient occasion to experience in my own person, the results become

better and better with practice, and still more so with a general comprehension of the process of cleansing the hands, which is gradually acquired by means of experiments, and by careful consideration given to the subject. Such figures cannot, therefore, be arranged in the form of a series.

It is far from my present purpose to wish to discredit, by means of what may appear to be plausible theories, such percentage figures, in so far as these are intended to illustrate the *general usefulness* of a method, without special reference to the individuals engaged in the experiments; even though up till now this has passed as the best means of affording "proof," it would almost appear as if the more material one individual was in a position to estimate by means of percentage figures, the fewer sources of error there would be. But even then the value of such figures is merely conditional, *for they can never be compared with the results obtained by anyone else*; and the fault lies with the method of procedure, which, however accurately it has been described, will, when used by someone else, differ in trifling details from the original, which is capable of very materially modifying the result. To read of the bad results which Leedham-Green obtained with Alcohol, or, on the contrary, of those which Schleich obtained with his Marble-dust soap, where sterile results were obtained in almost 100 per cent. of the cases, suggests grave doubts as to the accuracy of the conclusions arrived at, and one assumes a source of error to have been somewhere present in the method of procedure, till one has oneself investigated the matter by means of experiments of one's own, which, if they give a different result, make one doubt, once and for all. However, as a rule, one accepts without more ado, for purposes of comparison, the figures which lie between the two extremes, and I do think that in this respect these figures have given rise not only to a great deal of fuss and unnecessary strife, but have also been the cause of one's having often neglected, in one's enthusiasm over a method the value of which has been illustrated by such figures, quietly and critically

to consider the grounds on which these results were based.

If, in spite of what I have said, I now avail myself of percentage figures in quoting the results of investigations with regard to the disinfection of the skin with Corrosive Sublimate, this is simply for the reason that they can always serve—to some extent, at least—as comparisons for the results which I obtained from the use of other methods, and because, in these investigations, and in the ones I have already mentioned, I carried out the experiments myself, and with the same method of procedure,[1] and thus, in this respect at least, the sources of error were trifling, and were, at all events, always the same.

The 135 individual cases, in which the hands had been washed in 1 in 1000 Corrosive Sublimate solution and then investigated by means of threads, either immediately before the commencement of operations or in the laboratory, are divided into different sets for operators and for assistants. They cannot be considered as a whole, because the preparatory measures were different, and because in one set of cases the Corrosive Sublimate had not been removed by precipitation. All that can be said is that in spite of the difference in the preparatory measures, the cases in which the Ammonium Sulphide test had been omitted appeared undoubtedly to give the better results.

There still remain eighty-four individual cases in which the preparatory measures were approximately the same (Fürbringer's extended method, five to ten minutes for each act, the hands washed in sterilised water), and in which the Mercury was precipitated with Ammonium Sulphide;[2] in the case of these, at least, no direct objection can be taken to their representation by means of percentage figures. In 81 per cent. of

[1] The procedure is the same as that which has already been described for the mechanical cleansing.

[2] On account of the discoloration of the skin, and the injury sustained by the hands, it is impossible, in continued investigations, to completely precipitate the Mercury on the hands ; each experiment is a sacrifice. The Mercury was, on this account, usually precipitated on the thread

these cases the culture result was negative. Here again it was proved how important a matter is the bestowal of mental concentration on the processes of cleansing and of disinfection; if the individuals, whose hands were to be examined, had the whole question previously explained to them, the results were, as a rule, better than if they subjected their hands to investigation in ignorance of what was wanted.

Out of fifty-six investigations, which were restricted *to my own hands*, of which twenty-one were carried out before commencing to operate, and the remainder merely for purposes of research, 89 per cent. of the threads failed to produce cultures. But even here there was an absence of uniformity, for I obtained a series of unfavourable results owing to my hands having been somewhat rough from previous treatment with Ammonium Sulphide. Some further experiments of a similar nature, which I carried out on my hands, cannot be included along with the above, because the preparatory measures were totally dissimilar, in that the removal of the fatty material from the hand with Alcohol, or with Kaolin paste, was *antecedent to* the mechanical cleansing,[1] or that the mechanical cleansing was carried out with Schleich's Marble-dust soap without the use of a nail-brush, and that the disinfection with Corrosive Sublimate directly followed the mechanical cleansing.[2]

In disinfecting the hands with Corrosive Sublimate the best results were obtained when the hands were first freed from their fatty material by means of Kaolin paste

after it had been drawn through the hand. This did not appear to give rise to any difference in the results.

I may mention, in this connection, that there is no authority for the objection that Sulphuretted Mercury may possibly affect the vital capacities of the germs in the nutrient medium. I have introduced up to 1 cm.^{3} of this combination into Bouillon, and, if the precipitate had been properly washed, I was never able to observe that the germs which had been introduced simultaneously, or later, were interfered with in their development.

[1] The results, which were thus obtained, were, in every respect, worse than those obtained by the use of Fürbringer's method.

[2] It will be easily understood, from the chemical explanation that I have already given, that the results here were unsatisfactory.

for one to two minutes, were cleansed [1] for five minutes in the hottest possible water by means of potash soap and a brush, were rubbed on a dry towel, and were brushed [2] in 70 per cent. Alcohol for three minutes, and in 1 in 1000 Corrosive Sublimate solution for another three minutes—a process, in fact, which in principle corresponds with that of Fürbringer. Of eleven experiments which were carefully carried out in this manner, in one case only did two colonies develop (in the Agar plate), whilst cultures were also obtained in a tube of Bouillon which was inoculated at the same time; in the remaining ten cases the nutrient media remained sterile. Stress must be laid on the fact that these experiments were in great part carried out towards the close of the experimental work, and thus at a time when experience had already supplied a much better understanding of what were the essential points to be considered in cleansing the hands, and the hands were therefore in a faultless condition and had, as far as possible, been kept free from every impurity for weeks beforehand.[3]

[1] The time mentioned here merely refers to the washing of the hands with soap and a brush ; it does not include the cleansing of the nails, which, though it occurred repeatedly, did not occupy more than one to two minutes.

[2] The blending of the Corrosive Sublimate with the albuminoid substances of the skin (and of bacteria), like all chemical processes, takes place more readily with *heat* than with cold. This has been fully demonstrated in the experiments with cells and pieces of skin, to which I have already referred ; but proof of a chemical nature might also be obtained from investigations of the hands (the Ammonium Sulphide test). It is therefore strongly to be recommended that a *hot* solution of Corrosive Sublimate should be used. The best way is to have in readiness a supply of 1 in 500 Corrosive Sublimate solution, and to dilute this half and half with hot water (in a basin before use).

[3] I may mention here that in most of the investigations, and, at all events, in all those which were carried out on my own hands, the thread test was, as a general rule, employed *before* the hands were cleansed, and *after each of the separate acts.* Such a procedure certainly wastes a great deal of time (five to eight plates of Agar and as many tubes of Bouillon were required for the experiment) ; still, this is the only way in which the investigator can obtain any definite information as to the course followed by the cleansing in its various stages, and as to the effect of the various measures which have been taken. But, however instructive the fairly complicated details of an experiment, which is con-

But, again, in the case of these experiments, the definite impression which I was able to gain was *that the whole question depends upon the mechanical cleansing.* Still, there are limits to its efficacy : in control experiments, where the mechanical cleansing occupied quite as much time as in the procedure which I have already described, the Agar plates remained sterile twice out of nine cases, whilst the corresponding Bouillon tubes showed the development of cultures. In the remaining cases cultures developed more or less abundantly (five to seventy-eight colonies) both on Agar and in Bouillon.

From these experiments, and from the others to which I have already referred, it is impossible to indicate a single effect which is produced on the septic condition of the surface of the hands, *i.e.* on the developmental capacity of the germs which are there present, by the treatment of the hands with Corrosive Sublimate, after the fatty material and the water have been removed from the skin by means of a thorough mechanical cleansing. Certainly no interval was allowed to elapse between the act of cleansing and the carrying out of the investigations, and I shall later have occasion to show that, *in the long run*, the Corrosive Sublimate also affords but a conditional protection. Still, the investigations with threads undoubtedly prove that when these threads are drawn through the hands (just as occurs in practice in tying ligatures and in suturing), small pieces of epidermis, containing Corrosive Sublimate, or Mercury, are removed along with them, and I have already been able to show by means of experiments that—in artificial nutrient media, at least—the development of the few germs, which are thus simultaneously introduced, will be interfered with. It would be a mistake to suppose that I attach to this observation any optimistic expectations, for the mere elementary knowledge of students at the present day is sufficient to tell one that the human body does not behave exactly in the same

ducted on such lines, may be, it is impossible to reproduce them here, out of regard for the space at one's disposal, and for—the patience of the reader !

manner as a test tube. But I am not forgetful of the results of Geppert's investigations—that an addition of 1 : 2,000,000 Corrosive Sublimate to the nutrient medium is sufficient to check the development of Anthrax spores, which have lain for ten minutes in a solution of 1 in 1000 Corrosive Sublimate; and that a guinea-pig runs little risk of being infected if the spores and the Corrosive Sublimate are introduced together under the skin, and that the risk becomes less the longer the spores have lain in the solution of Corrosive Sublimate previous to this infection under the skin.

Thus a successful disinfection does not necessarily require to be identical with a condition of sterility. What we aim at—in the disinfection of the hands, as elsewhere—is not the destruction of the germs; we are quite satisfied if we are able to arrest them in their development.

An objection, which might be brought against this statement, is that it is illogical if, on the one hand, there is an urgent demand for the total elimination of the Corrosive Sublimate in investigations dealing with the action of disinfectants; and then, on the other hand, stress is laid on the effect produced by small quantities of Corrosive Sublimate on bacteria in artificial, or natural, nutrient media.

All that I can say in reply to this is, that the manner in which an antiseptic acts under such circumstances can only be experimentally explained, with any degree of lucidity, when the after effect of the antiseptic is completely removed.

If this has been omitted, sources of error crop up without number, or are, at all events, impossible to estimate in an individual case. But the objection does not apply to any exact test when the after effects of the disinfectant, which has been introduced, occur in the body. Here I refer you, on the one hand, to the results of chemical considerations, and, on the other hand, to the results of bacteriological experiments on animals, where the germs (such as Anthrax) possess a definite degree of virulence with regard to the animals experi-

mented on which is sufficiently accurate for purposes of control. But every one who has done any work of this kind knows that when it comes to be a question of the germs which give rise to pus, such as are of primary importance in the disinfection of the hands, experiments on animals afford no certain conclusions in such subtle questions, and all the less for this reason, that the germs of the hand, from their having been bred in artificial nutrient media, have become somewhat altered in their capabilities.

One thing, at all events, seems to be certain—that the experimental results of the disinfection of the hands, after elimination of the Corrosive Sublimate, *represent the minimum of its functional capacity.* Germs which cannot further develop in artificial nutrient media (Bouillon, or Serum), after the antiseptic has been completely removed, cannot further develop either in the body.

With regard to the practical disinfection of the hands with Corrosive Sublimate, there is still another circumstance to be considered here as a possible obstacle to its use, in addition to the fact that it damages the hands (to which I shall again refer in the chapter on "Prophylaxis")—viz. *the poisonous effect it has upon the operator.* It has been reported from various sources that attacks of colic have been observed to follow the use of Corrosive Sublimate; in almost all the cases there was an absence of salivation, and of the other symptoms of Mercurial poisoning. That this intestinal disturbance was due to the use of Corrosive Sublimate was proved by the fact that the disturbance ceased when the disinfection with Corrosive Sublimate was discontinued.

I do not intend to dispute the fact that the Corrosive Sublimate was really the cause of these attacks of colic, but I do consider it to be very improbable that the Corrosive Sublimate made its way into the body through the skin of the hands—at all events, unless these showed signs of numerous wounds of recent date. It is much more probable that the individuals, who were thus attacked, *absorbed the particles of Corrosive Sublimate by inhalation, or by deglutition*, because they had not

sufficiently removed the surplus Corrosive Sublimate from their hands after operating, and that the Corrosive Sublimate was, in consequence, conveyed to the mouth in the food which they had thus contaminated with their hands, or that it reached their mouths in some other way whilst they were operating. I know of one case where the wetting of the beard before an operation with Corrosive Sublimate solution was undoubtedly the cause of such intestinal disturbances. The fact that in these cases of intestinal disturbance neither salivation, nor any other symptoms of Mercurial poisoning, was present, is evidence in favour of the view that the intoxication was purely intestinal. I have myself more than once suffered from such attacks of colic, and, on one occasion, even to the extent of hæmorrhage from the bowel. But I have suffered from no further disturbance of this kind since I took care to cleanse my hands mechanically after I had made use of Corrosive Sublimate, *i.e.* since I have done my best to again remove the Corrosive Sublimate from my hands, as far as possible. This is important, not only as a precaution against colic, but also as a protection of the skin. What answers very well is to wash the hands in an infusion of Bran, because a solution such as this, which is rich in albumen, is able to extract the Sublimate better than any other lotion.

In order to avoid any injury to the skin of the hands, and the danger of suffering from intestinal intoxication, recourse has been had to *other soluble preparations of Mercury* (Oxycyanide, Asterol, etc.), which have the recommendation of being *less injurious to the skin, and less poisonous.* After the experiments to which I have already referred, this recommendation is in itself sufficient to give rise to the suspicion that these media are less efficacious, than Corrosive Sublimate, in their action on germs. That there were good grounds for this suspicion has been fully proved by chemical and bacteriological investigations which I carried out with these media; but with regard to this I cannot at present say more.

D. *Antiseptic Soaps*

Though, in clinical institutes, and in hospitals, the question of the cleansing and the disinfection of the hands is one which receives the greatest attention, and no endeavour is spared to bring about improvements, which are welcomed, even though they are relatively costly in the way of time and of money, still with the general practitioner the subject, when thus spun out, arouses either a feeling of indifference, of gentle derision, or of resignation. It arouses in him, at all events, a distinct feeling of impatience, because the complicated apparatus which such a relatively successful disinfection of the hands implies, not to speak of the length of time it occupies, inspires a busy man with horror. One can easily understand that every *simplification in apparatus, and in time*, is at once hailed with delight, and particularly in the case of general practitioners.

Asepsis proves, as a rule, no attraction to the general practitioner; he is not adapted for this. For his hands, and the wounds he treats, he has recourse to antiseptics. Since he very rightly feels that he cannot remove the germs from his hands (unless he happens to be a blind supporter of Schleich's Marble-dust soap), he tries to affect their vital capacities by chemical means; and even supposing that he does regard antiseptics in the light of an unknown deity, possessed of unlimited power, which he trustingly worships without reservation, still he is quite right, in principle, to trust to antiseptics. General practitioners, with us at least, have not yet adopted Alcohol, or *Spiritus Saponatus*, as disinfectants; these still belong to the realm of debatable questions, and one is distrustful, and perhaps very rightly too, of novelties which are highly praised to-day, and then, perhaps in some weeks' time, are again abandoned. No doubt the relatively high price of these specialities has somewhat of a deterrent effect.

But the advertisements with regard to *antiseptic soaps*,

which promise so much for them, sound very alluring in the ears of a general practitioner. There is the article which is to enable him " to sterilise his hands in a few minutes," which can be conveniently carried in the pocket in compact form (cakes of soap, or solutions of soap in metal tubes), whilst the entrancing bacteriological results, which are never absent from advertisements with regard to such soaps, assure him as to their capabilities.

And why should they not perform what they promise ? Still, *a priori*, it is altogether plausible that such antiseptic soaps are efficacious. The soap (with hot water and a brush) pretends to remove the corpuscular impurities and to loosen the epidermic cells, and, especially if it is semi-fluid in consistency and can therefore be well rubbed into the skin, it is to convey to the deeper parts the antiseptic with which it is combined. This, then, might be a disinfection without the interpolation of Alcohol, such as is employed in Kliniks where the disinfection also occurs separately. The only question is, *whether the antiseptic does not interfere with the efficacy of the soap, or whether the soap does not decompose the antiseptic, and render it inactive.*

I have always taken a great interest in the question of antiseptic soaps, as I have greatly desired that the matter should be simplified, as every one undoubtedly does. It is impossible to go further into the question of the different soap preparations; they have for the most part been discarded. *Corrosive sublimate soaps*, such as are still offered for sale, have little to do with Corrosive Sublimate except in name. If it has not already taken place in the soap itself, the Corrosive Sublimate is decomposed on the addition of water, from the formation of insoluble fatty acid salts, or partial reduction to the Chloride (Calomel); in this way the action of the soap also is, to some extent, at least, paralysed.

A preparation of Mercury, which will not be decomposed by soap, has therefore been sought, and this is found in the Biniode of Mercury, *i.e.* the Perchloride with

Potassium Iodide, which is soluble in water. This soap is sold in a semi-fluid form (in metal tubes) under the name of *Servatol soap*, and enjoys with us a fairly wide popularity.[1] A number of bacteriological investigations, with cultures and experiments on hands, were conducted in the laboratory of the firm I have mentioned, and always gave wonderfully good results.[2] I cannot enter on a detailed criticism of these experiments, for the reason that there was no elimination of the Mercury. All that I can say is that my test experiments did not confirm these results. The hands were never rendered "sterile," even after using the soap for ten minutes.

It was, however, also proved that the *cells* absorb but little Mercury from the soap.

If the hands were treated for two minutes with Servatol soap (according to the recipe), and were well washed in running water, the Ammonium Sulphide test produced a scarcely perceptible brown discoloration of the skin. If the hands were treated with 1 in 1000 Corrosive Sublimate solution for half a minute, after they had undergone an ordinary washing with soap, the discoloration with Ammonium Sulphide was incomparably more marked.

I have gone more closely into the question of this Servatol soap, for it is still the best preparation, theoretically, and practically, of its kind. The bad results did not surprise me ; I knew from earlier investigations that preparations of Mercury, which are not decomposed in soap, cannot be very efficacious—for as double salts they are too easily soluble, and are not capable of coagulating albumen.

Thus, unfortunately, it is scarcely possible to realise the idea of an efficacious antiseptic soap.[3]

[1] This preparation is made and sold by the famous "Swiss Pharmaceutical and Sanitary Company" (formerly F. Hausmann). The soap should contain 0·5 per cent. Biniodide of Mercury.

[2] These were published in the business report of this firm, 1898, No. 10 (Dr. Weyland).

[3] In order to solve the problem of how one can conveniently carry with one the necessaries for a disinfection of the hands with Corrosive

Sublimate, I have had prepared (by the firm of Knoebal & Laubscher, of Basel) a small metal case which contains the following :—

One *brush*, in a separate compartment of light metal perforated with a number of small holes. The brush, *in its case*, is boiled in water, and is removed for the first time when it is to be used ; it can be, thus, conveniently kept sterile. It is more correct to carry two such brushes —one for the mechanical cleansing, and one for the cleansing with Corrosive Sublimate ; the case will then, no doubt, be somewhat less compact.

One box, or tin tube, containing *Potash soap.*

One small box containing *tabloids of Corrosive Sublimate.*

One *nail cleaner.*

The Alcohol is awanting ; this, however, can be easily remedied at any time, should any one desire to use it, especially as we now know that it acts much better when diluted (50 to 70 per cent.), than when in a high degree of concentration.

CHAPTER V

ON THE SEPTICITY OF THE SURFACE OF THE HAND DURING OPERATIONS

I HAVE already shown to what extent the septic condition of the surface of the hand is affected by the measures taken to cleanse and to disinfect it. But what is really of importance is not whether the surface of the hand can be freed from germs by any one process, *but whether this condition of sterility, when once attained, is maintained throughout the course of an operation*; for what is the use of starting to operate with the surface of the hands aseptic if they are to become septic again—just when they are busy, indeed, in the deeper parts of the wound! Thus the point of real importance is not the possibility of producing a momentary condition of sterility, but *is the length of time that this condition can be maintained*—a point on which but little attention was bestowed till a short time ago.

What first drew general attention to the question was the introduction of operating gloves. Investigations of thread gloves, which had been worn during the course of operations (Doederlein, Brunner, and others), proved that such coverings for the hands become loaded with an enormous number of germs during an operation.

If germs are present on the thread gloves, they will not be absent from the bare hands. I knew of this before thread gloves were worn. Out of 176 experiments, some of which were carried out in past years, where I requested the operator whilst he was at work to draw a silk thread through his fingers, or, if I was

operating, I carried out this test myself, *not a single thread proved to be sterile* when the manifest sources of error had been eliminated as far as was possible; and certainly the colonies were, for the most part, not simply isolated ones, but were more or less numerous.[1]

Great merit is due to Doederlein for having pointed out the importance of investigations *during the course* of operations. The very encouraging experimental results which in 1898 he reported to the Surgical Congress in Berlin, and which, in conjunction with the report of Mikulicz, have evoked an animated discussion, very properly gave rise to considerable sensation; as the investigations, so far, have not been tested, which was what Doederlein himself desired should take place, they must to some extent be looked into more closely.[2]

First let me say something with regard to the investigation procedure. Doederlein set to work to test the germs of the hand in this way. The Corrosive Sublimate was removed [3] from the hands by repeatedly washing them in hot sterilised solutions of common salt, after they had been disinfected according to Fürbringer's method, but without the interpolation of spirit; the hands were then rubbed together for one to two minutes; the fluid which now dripped from them, permeated with exfoliated epidermic cells, was placed in sterilised Petri's capsules, and was at once mixed with Agar.

This method of procedure is certainly the most unsuitable of all those which have been published up till now. It is far from my purpose to wish to suggest that germs cannot be removed from the hands in this

[1] Agar (on plates) and Bouillon were always simultaneously used. Either two threads were used, or the one thread was divided with sterilised scissors.

[2] Since the completion of this portion of the book such tests have certainly been applied, and for these the Klinik of Mikulicz is chiefly responsible (Gottstein and Blumberg: *Berl. Klin. Wochenschr.*, 1899; Gottstein: *Beiträge zur Klin. Chirurg.*, Bd. 25). Doederlein himself has also somewhat altered his views since then (*Münch. med. Wochenschr.*, 1899, No. 26).

[3] According to a foot-note, the hands were "afterwards" (thus, at any rate, after the termination of these principal investigations) treated with Ammonium Sulphide.

way; Doederlein himself admits that in 15 per cent. of the cases individual colonies made their appearance. Still, that after a vigorous mechanical cleansing of this sort the germs of the surface of the hand should be easily separable from the hand, is altogether at variance with what we know—on the one hand, with regard to the action of the mechanical cleansing, and, on the other hand, with regard to the situation of the germs of the hand. It is certainly hardly conceivable that large numbers of exfoliated epidermic cells should be removed from the hands after a vigorous mechanical cleansing where the hands were simply rubbed together;[1] but, even supposing that this was the case, these cells, in Doederlein's experiments, could only come from the *projecting parts* of the surface of the skin, and, moreover, from a layer which had already been acted on by Corrosive Sublimate. Thus, even supposing that germs were present in, or between, these epidermic cells, either they would not develop at all in a standard nutrient medium, or would only do so to some extent.

There is, however, another point of equal importance. Any one who has often examined his hands microscopically, knows that the corpuscular impurities which are most difficult to remove (I am only referring here to the surface of the hand) lie in the recesses of the folds and creases, in the orifices of the sweat-glands, and in the small accidentally produced wounds of the epidermis. But, by simply rubbing the hands together, these parts will remain absolutely untouched, and thus the information to be obtained from the process adopted

[1] I have often tried with the microscope to convince myself of the truth of this statement of Doederlein's, but, on treating the hands as he describes, the resulting epidermic cells were but few and far between. Detached epidermic cells are mostly found in the water in which the hands have been washed with soap, after the outer epidermic layers have been loosened; a large proportion of the elements of low vitality, which the brush has been unable to remove, are fully detached by means of the towel, or are removed on its surface. After the hands have been washed in Alcohol, there is certainly but little to be found in the drops which fall from the hands, when they are rubbed together; little or nothing in the way of cells is to be obtained after the hands have been washed in Corrosive Sublimate.

by Doederlein, as to the true septic condition of the surface of the hand, cannot be otherwise than wide of the mark.

A question which the removal of germs from the hand cannot answer, is whether the germs of the air, on the importance of which Doederlein laid such stress, may not, to some extent, affect the fluid as it drops from the hands on to the plates of Agar. Besides the germs which fall as part of the dust of the air, one has to take into consideration those which have fallen on the hands, which, by their active movements, have exposed their relatively extensive surfaces to the dust of the air for one to two minutes, whilst even the drops as they fall from the hands can also carry individual germs along with them. Doederlein seems here not to have taken the germs of the air sufficiently into account.

Thus, in answer to the question whether the surface of the hand can be freed from germs, no decisive value can be attached to the results obtained by Doederlein with such a process, although he himself describes his evidence as "incontestable and indisputable."

His evidence is based upon the fact that after an operation which lasted one and a half hours, and during which rubber gloves were worn, it was impossible, with the procedure such as I have described above, to confirm the growth of any germs in the plates. Doederlein concluded from this that not only was the hand sterile after the act of cleansing, but that its surface *remained* sterile for one and a half hours. Mikulicz, at the Surgical Congress, expressed the suspicion that perhaps the introduction of Corrosive Sublimate might have been the cause of the continued sterility of the nutrient medium, and I can add that, as a matter of fact, Corrosive Sublimate must have been present in the fluid in addition to that which was in conjunction with the epidermic cells, because, according to my investigations, common salt solution, with which the glove had been filled before it was drawn on, is

able to extract to some extent Corrosive Sublimate from the cells of the body.[1]

With regard to the septicity of the hand under a rubber glove, I shall again deal with the question later. Let us first dwell once more on the fact that, during an operation, septic material collects to a greater or less extent on rubber gloves (and on the naked hand), and then let us turn our attention to the question regarding the origin of these germs. As regards the various measures which we take, it is exceedingly important that we should know where the germs come from. As it is impossible they can come either from the instruments, which can be sterilised with certainty, or from the gauze, which is equally safe, only three other sources remain — *the skin of the patient*, which may be accidentally touched; *the air*; or *the recesses of the hand* itself.

As to the first possibility it is an exceedingly remote one, since, apart from the fact that during an operation we avoid touching with our hands the uncovered skin of the patient, his skin is usually covered with towels; so that if the skin of the field of operation has been subjected to a thorough mechanical cleansing, and is preserved from active interference, it is scarcely in a position to communicate to the hand of the surgeon germs of which there can be any question here. It is not impossible that septic material, which is present in the discharge from operation wounds, partly arises from the patient's skin; but the probability is not great so long as we are unable to eliminate the hand of the surgeon as a means of introducing sepsis, and the

[1] I should mention here that neither I, nor others who carried out these experiments, were able to express drops of a milk-like fluid from our hands after having worn the rubber gloves, which we had drawn on moist, for a considerable length of time. The fluid which we did express was frothy, and nearly always so small in quantity that scarcely a drop could be obtained after the hands had been rubbed and kneaded—no doubt on account of our gloves having been somewhat tight. Microscopically, the fluid was found to contain a few epithelial cells, together, however, with numerous particles of Talc. This is explained by the fact that ordinary rubber gloves, when they are bought, are always powdered with Talc.

truth with regard to this is shown by investigations of thread gloves, and of the naked hand.

Thus the only possible sources which still remain are the air, and the skin of the hand itself. Doederlein, according to his investigations, considers the source of the impurities to lie exclusively in the air, *i.e.* the germs of the air—inclusive, certainly, of fluid molecules spat from the mouth, upon which Flügge lays so much stress. I, together with Rydygier, have undoubtedly been the first to again draw attention to the dangers (relatively slight dangers, no doubt) of the dust of the air in operating theatres. I have subsequently extended these experiments, and have thus in no way underrated the influence of the dust of the air. But, from my experiences, I consider it very improbable that "the vast quantities of heterogeneous cultures," which Doederlein was able to obtain from thread gloves, proceeded from the dust of the air alone.

But even some of Doederlein's own experiments give essential evidence against his view. Whilst the thread gloves were loaded with such an astounding mass of germs, only some ten to twenty colonies developed in plates of Agar which were simultaneously placed in different parts of the operating theatre. What astonishes one most is the result of the experiment in which a variety of people with gloves on their hands, which had been moistened with sterilised common salt solution, moved briskly about, and in which colonies were first obtained on testing the fluid of the thread gloves after an interval of half an hour had elapsed.

Doederlein explains this discrepancy by remarking that blood is more viscid than common salt solution, and therefore more quickly lays hold of the germs of the air. As a general rule, dust does not again become detached from a moistened surface upon which it has once settled; it must, however, be allowed that blood in the form of small coagula affords a better representation of a *moist surface* than does the common salt solution, which is completely absorbed by the texture of the glove and has also partly evaporated.

But even this assumption cannot alone explain the occurrence of an "immense collection" of germs on the gloves. Doederlein endeavours to find a further explanation of this statement in the assumption that the dust of the air does not contain individual, but conglomerate, germs, and that the alkaline blood with its albumen, in conjunction with mechanical influences, may be able, during the course of an operation, to break up the conglomeration of germs; and, further, that during this time the number of germs is multiplied by their propagation in this favourable nutrient medium.

With regard to the first part of this assumption, his averment should present difficulties. Particles of dried pus, or of blood (of such a size that they naturally cannot be whirled up into the air as dust), are not perceptibly broken up in Serum at blood heat—even when shaken—during the space of half an hour, and the conglomerations of dust are so small that one can scarcely imagine their being rubbed into the meshes of thread gloves.

From what we know with regard to the rapidity of the reproduction of micro-organisms, *the reproduction of germs* in the gloves might carry more weight with it. But the experiments of Friedrich—whose results I can fully corroborate from investigations of my own, which were instituted quite independently of his—prove that these germs of the air do not, as a general rule, develop in the first few hours; they are in a condition which Friedrich describes as a "state of dryness," and, according to his assumption, they require a certain period in which to adapt themselves to the moist nutrient media before they are capable of reproduction. Whatever may be the explanation of this phenomenon, the fact remains that the germs of the air are not capable of reproduction during the first few hours, and that thus Doederlein's assumption cannot hold good as an explanation of the enormous number of germs present in the thread glove. If, therefore, it is to be taken for granted that a portion of the germs, which, during an operation, are present on the thread gloves, or on the naked hands, originate from

the dust of the air (directly or indirectly, by way of the instruments or the wound), *it is certainly very improbable that the dust of the air* (including the small particles of fluid spat from the mouth) *should form the one and only source of this septic material.*

Thus, in our quest as to the source of these germs, we are thrown back *on the hand itself.* As the septic material, which is present on the surgeon's hands, demands, at any rate, greater vigilance on our part than do the germs of the air, which are for the most part harmless, it became a matter of first importance to discover whether the hand could, as a matter of fact, be so sterilised that no germs (apart from those which come from the field of operation, or the air) could be transferred from it to what it touched, or rubbed, *during an operation.*

The experimental procedure as selected by Doederlein was the quickest way to the solution of this question, *viz.* isolation of the cleansed or disinfected hand by means of an impermeable covering, and investigation of the surface of the hand after a definite period of time.

Not on a single occasion did I obtain negative results in the experiments (eighteen) which I instituted with this method.[1] In these experiments I was careful to eliminate the sources of error to which I have already referred, *i.e.* the traces of Corrosive Sublimate, either on the hand, or on the thread, were precipitated; and the material which remains on the hand and is unable to drip off, was also taken into account by means of the thread method. In most of the cases the number of germs obtained was certainly small, a circumstance which at first caused me some surprise in consideration of other experimental results which are printed below. I shall again refer to this matter later.

In twelve of these experiments the pieces of thread,

[1] The experiments were, for the most part, carried out on my own hands (eleven), but also to some extent on the hands of others. Fourteen of the experiments took place in connection with operations (duration, half hour to two hours); the remainder were carried out in the laboratory.

which had been inoculated *before* the gloves were drawn on, remained sterile in the artificial nutrient media. Thus, from the results of later inoculations, it was proved indubitably that germs did reach the surface of the hand even under the rubber glove, and that the initial sterility of the skin, therefore, was merely ostensible, and was, at all events, non-durable.

In order to obtain an approximate estimate of the question whether the germs on a thread glove come entirely, or principally, from the air, it is now easy to advance a step further in the experimental procedure suggested by Doederlein, and to draw on sterilised thread gloves over the sterilised rubber gloves. If Doederlein is right in his view as to the origin of the germs of the gloves, the number of germs present on the thread glove must be approximately as large whether it is protected, or not, from the hand by the interposition of a rubber glove.

Of the six experiments which I carried out in this manner I quote *in extenso* two as examples; for it is impossible to give a sufficient idea of the procedure, or of the result obtained, by means of a summary alone.

I. *Operation Theatre of the Surgical Klinik.* (10*th November* 1898.)[1]

The hands were washed and disinfected according to Fürbringer's extended method (five minutes for each act). The Corrosive Sublimate was rapidly washed off in Alcohol, and then for five minutes in sterilised water which was repeatedly changed.

The finger-tips of the *right* hand were rubbed and pressed in plates of fluid Agar. The plates remained sterile.

The *left* hand was tested by means of sterilised threads, which were drawn through it.[2] Two threads were employed, which were cut in two after they had

[1] No clinical class was held that day; the theatre was thus relatively free from dust.

[2] The threads were treated with Ammonium Sulphide solution before they were placed in the nutrient media.

been used, and were placed in two Agar plates, and in two tubes of Bouillon. The two Agar plates and one tube of Bouillon remained sterile; Staphylococcus albus slowly developed in the second tube of Bouillon.

The Corrosive Sublimate, which was present in the skin, was precipitated with Ammonium Sulphide solution: the hands became brownish black in colour.

The hands were washed in sterilised water, and were dried with a sterilised towel.

A sterilised rubber glove, filled with sterilised common salt solution, was now drawn on to the *right* hand, and over it a sterilised thread glove; a thread glove only was drawn on to the *left* hand. (The surface of the thread gloves, as tested by means of the thread method, proved to be sterile.)

An *operation for Goitre* was performed, which lasted half an hour.[1] At the end of the operation three drops were squeezed from each of the thread gloves, which were now soaked with blood, and the drops placed in a plate of fluid Agar.

Result.—Thread glove of right hand—17 colonies.
Thread glove of left hand—65 colonies.

The right hand (under the rubber glove) was slightly moist. It was tested by means of the thread method;[2] two plates of Agar, and two tubes of Bouillon were used.

Result.—Cultures were obtained in all four nutrient media:—

Agar film I.—Six free colonies, and twenty-three colonies attached to the thread.

Agar film II. — Ten free colonies; about thirty colonies attached to the thread. It was impossible to determine the number accurately on account of the confluence of the colonies.

A plate of Agar, simultaneously exposed in the vicinity of the operating table, showed twenty-one colonies.

[1] Instruments boiled in soda solution and laid out dry when sterile; swabs dry and sterile; no irrigation.

[2] For this purpose, one end of the thread was held by means of a pair of sterilised artery forceps.

II. *Lecture Room of the old Surgical Poliklinik.*
(*22nd November* 1898.—After the lecture.)[1]

The hands were cleansed and disinfected, as in Experiment I.

The hands, when tested subsequent to their disinfection, were found to be sterile.

A sterilised rubber glove, filled with fresh sterile ascitic fluid,[2] was drawn on to the *right* hand, and over this a sterilised thread glove. A sterilised thread glove [3] only was drawn on to the *left* hand.

The gloved hands were dipped for a short time in ascitic fluid. The surface of the gloves, when tested by means of threads, was found to be sterile.

For fifteen minutes the hands were vigorously wielded, and the hands and fingers were energetically pressed and squeezed together whilst one moved about the room and engaged in conversation.

Three drops, which were squeezed from the thread glove of the right hand, showed a result of thirty-five colonies in a plate of Agar; three drops, which were squeezed from the thread glove of the left hand, showed a result of fifty-eight colonies in a plate of Agar.

The *right* hand (which had been protected by a rubber glove), when tested bare, showed :—

In both tubes of Bouillon, a strong development of cultures.

In Agar plate I.—twenty colonies (originating almost entirely from the threads).

In Agar plate II.—twelve colonies.

A plate of Agar, which was exposed during the time the hands were in motion, showed 116 colonies.

Similar results were obtained in the four remaining experiments, half of which were carried out according to Experiment II.

[1] An unsuitable locality, and one difficult to cleanse.

[2] As blood in large quantities can, only with difficulty, be kept sterile, Serum had to be used here.

[3] The acts of drawing on and drawing off the gloves were, on this occasion, performed with precautions, such as I shall describe later, in order to eliminate the germs of the air.

Even though the number of germs obtained from the thread gloves does not represent a value which can be expressed arithmetically, still these experiments consistently proved that a thread glove contained more germs, when it lay on the bare hand, than one which was preserved from direct contact with the hand by means of a rubber glove; further, that more germs developed in the plate of Agar, which was simultaneously exposed to the air, than developed from the three drops which were obtained from the thread gloves; and that the surface of the hand under the rubber glove was not found to be free from germs even though it had been unable to part with any germs to a thread which was energetically rubbed over its surface immediately after the act of cleansing.

Still, I was surprised to find that the number of germs was relatively so small, since the following experiments had given me reason to expect a larger return.

Hands[1] which had been cleansed and disinfected according to the process described above, were bathed in half a litre of sterile ascitic fluid for ten minutes to half an hour, were squeezed and rubbed together, and the fluid tested[2] in order to ascertain the number of germs which it contained.

[1] It is self-evident that in such experiments the Mercury which is present *on the hands*, after the disinfection with Corrosive Sublimate, must be precipitated.

[2] In order to eliminate any errors which might arise in connection with the dust of the air, which may here, as in all other investigations, easily lead to erroneous conclusions, the following precautionary measures were taken—*as, indeed, they were taken in almost all the investigations which I carried out with regard to cleansing by mechanical means and with Alcohol.* A wooden frame, like a chest, with smooth walls, 40 cm. in height, 40 cm. in depth, and 50 cm. in breadth, which was open at one side, was scrubbed inside till moist; the side which was open was hung with a damp sterilised towel; a sterilised enamel basin stood on the floor of the box. Immediately before the experiment water, Alcohol, sterile ascitic fluid, or whatever fluid was intended to be used, was poured into the basin under cover of the towel, the hands were introduced beneath the towel (which was slit for the purpose), and the towel was then completely closed over the forearms by means of clamps. No dust of the air could make its way in from without. The plates of Agar which, during the experiment, were placed in this hollow

The results showed that already at the end of ten minutes the lotion was, as a rule, no longer sterile; plates of Agar to which 1 cm.^{3} of this fluid was added, were usually able to produce individual colonies. After the hands had been bathed for half an hour, 1 cm.^{3} of the fluid invariably proved to be no longer sterile; and in one case, where it had been impossible to obtain any more germs from the surface of the hands *before* they were bathed in the Serum, six to eleven colonies developed from 1 cm.^{3} of this fluid. The same result was obtained on repeating the test several times. If eight be taken as the mean, this would amount to 4000 germs, or colonies, for the total quantity of Serum (half a litre).

This experiment certainly afforded additional proof of the undoubted superiority of Fürbringer's method of disinfection with Corrosive Sublimate, as compared to a disinfection with Alcohol alone, since the number of germs obtained from the hands after they had been treated with Alcohol alone was generally larger than after their disinfection according to Fürbringer's method—although I must admit that the difference in the first ten minutes was not so obvious.

The result of this experiment was very remarkable, when one considers the small number of germs obtained from hands which were covered with rubber gloves after they had been disinfected; and it was all the more remarkable because the surface of the hand, after having been thus washed in Serum, was proved, when treated by means of the thread method, to be possessed of far more germs than when covered by the rubber glove. This implied the presence of some element of discord, and since the experiments, in which Serum was used for the purpose of washing the hands, had been carried out with a vigilance which could admit of no mistake, the error

box for purposes of control, remained sterile, or merely produced one to three colonies from the splashing about of the lotion. Whatever fluid was used, it was tested before, during, and after the experiment by the removal, by means of a sterilised spoon, of ½ or 1 cm.^{3} of the contents of the basin.

had to be sought for in the experiment with the rubber glove.

When I came to check the experiments with the rubber glove, I was particularly surprised to find that where the one hand is covered with the rubber glove (filled with Serum) and the other hand with a *thread* glove, and both hands are bathed and kneaded in Serum for fifteen to thirty minutes, only relatively few epithelial cells are present in the fluid from the inside of the rubber glove and on the inner surface of the glove itself, whilst they are present in fairly large numbers in a single drop squeezed from the *thread* glove inverted.

Another experiment showed the matter in a still clearer light. If hands, from which the fatty material had been removed, were rubbed with Indian ink till they were dry, and, at the end of quarter of an hour, the loosened Indian ink molecules were removed by washing the hands in water (Indian ink could still be recognised in the folds, etc. of the skin with the naked eye, but more especially with the help of a microscope); and now if the experiment as described above was repeated, it appeared that but few Indian ink molecules were present in the fluid inside the *rubber* glove, whilst they were present in great abundance in the fluid expressed from the *thread* glove. It was possible, with the help of the microscope, to recognise their presence in large numbers on the inside of the thread glove; they lay partly between the meshes of the glove and partly also on the outer surface of the glove.[1]

A microscopic investigation of the surface of the hand, after an experiment such as this, showed that the Indian ink had almost entirely disappeared from the recesses

[1] This experiment with soot, or Indian ink, shows very well how the germs of the hand make their way through the meshes of the glove. A number of experiments (in which hands covered with thread gloves were vigorously moved about in Serum, and in which at different times a microscopic examination was made both of the inside and outside of the gloves, as also of the Serum sediment) proved that the Indian ink molecules begin to make their way through the gloves in the first few minutes, and that they have accomplished this in considerable numbers by the end of ten minutes.

of the hand which had been covered with a thread glove, whilst molecules were still harboured in considerable numbers in the creases and indentations of the hand which had been protected by a rubber glove.

After these experiments there can be no further doubt that, *in a space of time of which there is any question for the performance of an operation, fewer epithelial cells and fewer corpuscular impurities* (*as also micro-organisms*) *become detached from the surface of the hand under the rubber glove, in spite of all maceration, than is the case where the hand is covered by a thread glove, or is quite uncovered;* this is to be accounted for by the fact that the rubber glove compresses the folds and indentations of the hand, but more especially by the fact that it prevents the surface of the hand from being rubbed.[1]

There is now no doubt that the germs of the surface of the hand are able to *multiply* in the moist retreat which the rubber glove provides them with. The only question is whether or not this multiplication occurs in the first few hours. Certainly the experiments, which have so far been adduced, assert the contrary, or otherwise the number of germs obtained would have been much larger. An experiment, to which I have already shortly referred in the first section, is very instructive.

The hands were cleansed for fifteen minutes in sterilised water, which was frequently changed, with the help of Potash soap and a brush (the nails were cleansed punctiliously), were scrubbed with a sterilised towel, and were once more washed in sterilised water in order to remove the pieces of epidermis which had thus been loosened.[2]

The hands, when tested by means of the thread method, showed two colonies in Agar plate I., whilst Agar plate II. remained sterile.

[1] Doederlein appears, however, to have used for his experiments the Zoege von Manteuffel glove, which compresses the hand to an even greater extent.

[2] In this experiment the use of antiseptics was avoided, for it seemed to me to be a matter of importance to eliminate every factor with regard to which it was impossible to estimate the bearing.

Sterilised rubber gloves were now drawn on and were worn for forty-eight hours. In order to check the number of germs present on the surface of the hands the gloves were from time to time removed, under cover of the dust-proof shelter, and the hands were subjected to a thorough mechanical cleansing, after which fresh gloves were again drawn on.

It was now shown that the number of germs increased but little in the first four hours (from seven to thirty-six colonies). But by the end of ten hours the number of germs had increased more than a thousandfold (about 45,000), and the increase was then so rapid that, at the end of two to three hours, 30,000 to 40,000 were obtained from the surface of the hands by means of threads, notwithstanding the fact that at this stage of the experiment the hands were each time subjected to another thorough mechanical cleansing before fresh gloves were drawn on.

Thus the germs, which were obtainable from the surface of the hand in the first four hours, came from the recesses of the skin ; the trifling increase in this number is not to be ascribed to a multiplication on their own part, or otherwise the number of colonies might well have been considerably greater. Another argument against their having multiplied of themselves is, that the colonies which were obtained at this period of the experiment included various forms of germs. But the rapid increase which, at the end of ten hours, took place in the number of the germs, can only be ascribed to an actual multiplication of the germs themselves. This observation also suggests that the septic material of our hands, like that of the dust of the air, requires a period of adaption in order that it may multiply. As I have already pointed out, the staphylococci in the skin of our uncovered hands, which have been subjected to repeated washings and scrubbings, do not of themselves multiply under normal circumstances.

We have still to turn our attention to another statement—*viz.* that the germs, which are present in the thread gloves after an operation, come in great part from

the hands themselves; *i.e. the nature of the germs.* I have so far only briefly referred to this subject, and have not entered more fully into the matter in the experimental results which I have quoted, because the question with regard to the possibility of sterilising the hands has, for the most part, nothing to do with the *nature* of the germs which are to be found on the surface of the hand. But it is certainly of importance to affirm here that there is an essential difference between the appearance presented by a plate of Agar into which drops of fluid have been squeezed from a thread glove after its use, and plates which have been freely exposed to the dust of a room. *In the former, the prevalent germs are those which are customarily to be found in the skin—the white staphylococci,* and this can hardly be a coincidence. The test can just as well be applied in the case of the plates of Agar which are freely exposed at the same time, which inform us as to the nature of the germs present in the air of the locality in question. The difference is so striking that it seems a remarkable fact that such a phenomenon should have failed to attract the attention of other investigators.

It thus follows from what I have said that the dearth of germs, *i.e.* their absence from the surface of our hands, as was found to be the case immediately after the hands had been cleansed and disinfected, is a mere transitory condition; that Doederlein's explanation with regard to the origin of the germs which are present on the thread gloves is only conditionally correct; *that the septic material partly comes—certainly the greater part of it—from the hands themselves which have worn the gloves.*

CHAPTER VI

OPERATION-GLOVES AND IMPERVIOUS COVERINGS FOR THE HANDS

When it became evident that a mere cleansing and disinfection of the skin was not sufficient to insure the freedom of the hands from germs, an attempt was made to isolate these suspect surfaces by means of external coverings.

The idea itself is, relatively speaking, a matter of past history, since, in the year 1886—in a discussion dealing with the reports published by Kümmell—Unna [1] proposed that the hands should be covered with a layer of soap in order to prevent, during an operation, the detachment of additional layers of epidermic cells and germs. Nor is the wearing of gloves altogether a new departure. Halsted, in the year 1891, recommended the use of sterilised thread gloves, though only in the case of assistants whose duty it was to hand the instruments; no further attention was paid to this suggestion—at least on the Continent—for the good reason that it was merely an incidental observation, and had appeared in the literature of a foreign country.

When, however, renewed attention began to be bestowed, especially in the last few years, on the septic condition of our hands, and results of a somewhat depressing nature were brought to light by the numerous investigations on the subject, the idea of covering over the surface of the hands followed as a mere logical sequence of thought. The idea was wide-spread,

[1] *Deutsche med. Wochenschrift*, 1886, No. 32, p. 557.

which accounts for the fact that proposals, enjoining the use of operation gloves, were almost simultaneously made from Surgical Institutes of the most varied description.

Zoege von Manteuffel first suggested the use of rubber gloves such as are worn by chemists, and such as surgeons have also for long employed in the teaching of operative surgery on the dead body. These stiff and heavy gloves, which I used for long before I became acquainted with the preparation recommended by Friedrich, diminish the delicacy of one's sense of touch, and tire the hands. If a large size is used, the delicacy of the sense of touch is materially curtailed, because the glove does not fit the hand accurately; if one selects a size which fits tightly, the sensibility of the hand is diminished from the localised anæmia which rapidly ensues, and a sense of fatigue soon supervenes.

The proposals on the part of Mikulicz and Perthes, which emanated from the Klinik of Trendelenburg, that woven gloves should be used, appeared almost simultaneously. The thread glove recommended by Mikulicz had this advantage over the silk preparation recommended by Perthes, that it was cheaper, and I fancy that in most Kliniks the thread glove came to be used for a longer or a shorter period of time. Certainly the suggestion offered an infinite number of inducements —isolation of the suspect surface of the hand from the wound by means of a covering of undoubted delicacy of texture, which fits smoothly to the hand, and can be sterilised by steam, together with the dressings. Nor does the adoption of this glove in any way materially add to the complications, or to the cost of the procedure. The interference with the delicacy of the sense of touch, and with the free movement of the hand, to which it gives rise, is but trifling, as is universally admitted; and but little time is required in order to become accustomed to its presence on the hand. It has, perhaps, this disadvantage, that needles, rake retractors, and other pointed instruments, when misdirected, become easily entangled in the meshes of the glove, and that in tying

ligatures in deeply situated parts, a protruding finger-tip becomes occasionally included in the knot.

But any one would gladly put up with such trifling inconveniences as these, if, in addition, the glove would fulfil its mission by rendering the wound secure from those germs of the skin which cannot be removed by a process of cleansing and disinfection.

Doederlein, Brunner, and others, now pointed out that during an operation the tissue of the glove absorbs germs with considerable rapidity. I have already shown where these germs come from. The dust of the air may directly, or indirectly, supply some of them, but the remainder, and certainly the majority, come from the surface of the hand on which the glove is worn. As Doederlein has already shown, the thread glove is more septic than the outer surface of the rubber glove, which can easily be proved experimentally at any time; and, as I can add from experiments of my own, the thread glove is usually even more septic than the naked hand. It is easy to surmount such statements by supposing that the thread glove plays the part of a filter, and that the germs which it contains need not necessarily be transmitted to external objects. The investigations, which have so far been carried out with regard to the septic condition of thread gloves, have only dealt with drops of fluid squeezed from the gloves, or with small portions of the glove tissue which have been excised, and it certainly does not necessarily follow that the germs, which were thus obtained, had lain on the outer surface of the gloves, for they may just as well have come from the inner surface. When, in discussing the subject with men who are in favour of thread gloves being worn for operating purposes, I drew attention to the fact that the gloves contain so many germs, I was regularly met with the retort that, even supposing that some of the germs do come to the surface of the hand, others will always remain behind in the recesses of the skin, and that this in itself guarantees a fair amount of security. This implies that the woven glove is always to be credited with the

power of acting as a filter. It is difficult for any one to gravely accept this hypothesis who has examined the meshwork of such gloves, and who is able to form a mental picture of the size of the micro-organisms, especially in view of the great assistance that the germs, in their advance to the surface of the thoroughly soaked gloves, and into the surrounding parts, obtain from the squeezing and pressing to which the hands are subjected during an operation. Opitz takes even a more optimistic view of the matter, for in his opinion the advantage of the thread gloves lies in the fact that, like swabs, they suck up germs out of the wound. In the first place, he persists in erroneously explaining the origin of these germs in the wound (which, for the most part, undoubtedly emanate from the hands); and in the second place, he forgets that a swab, when it has once or twice been used, is then removed as a measure of precaution in order that its contents may not be squeezed into the wound; whilst a glove, when it has once been soaked, must first get rid of its contents before it can again suck up a fresh supply of secretion and of germs.

The investigations, which I carried out for purposes of comparison with regard to germs which are present on the hands, when covered with rubber or with thread gloves, or when bare, and the results which I have already briefly mentioned, had always to do, however, with the surface of the hand, or with the surface of the glove; this is easily done by means of the thread method, and is most in accordance with what occurs in practice.[1] The explanation is thus simple why, in nearly every case, fewer germs were obtained, not only from the surface of the rubber glove but even from the surface of the bare hand, than were obtained from the surface of the thread glove; for the secretion, together with the septic matter it contains, accumulates in the thread gloves, which does not occur in the case

[1] Either a thread glove was worn on the one hand and a rubber glove on the other, or the one hand remained uncovered whilst the other was covered by a thread, or a rubber, glove.

of smoother surfaces which are unable to absorb the fluid. Thus, although in the course of an operation it is always possible to thoroughly wash the smooth surfaces of the hands so as to again render them free from germs, this advantage is lost when thread gloves are worn, and even a frequent change of gloves affords but little help.

Therefore, according to my investigations, Doederlein and I are wholly at one with regard to the view that the wearing of thread, or of silk, gloves during an operation does not, so far as bacteria are concerned, afford any greater security, but does rather the reverse. The tissue of the glove represents a limited reservoir, which must always first be emptied before it can again be filled, and if those who are in favour of such gloves being worn assert by means of statistics that the results of operations have been substantially improved by the use of thread gloves, I am compelled, like others, to discern the reason of this improvement merely in the fact that greater care and attention is now generally bestowed on the hand, because the importance of it as a factor has been recognised.

The impregnation of woven gloves with Paraffin, as recommended by Menge, certainly forms, to begin with at least, an undoubted obstacle to the migration of the germs outwards from the surface of the hand (of a hand artificially infected), as I can affirm from personal investigations; but this obstacle is by no means to be relied on when the hand is at work amongst blood or Serum at body heat, for, even by the end of ten minutes, it will to a great extent have been demolished.[1]

The *kid glove*, as recommended by Wölffler, is a great improvement. In this case there can be no doubt about the genuineness of the filtration, *i.e.* that the germs of the hand are arrested. Wölffler recommends the ordinary military glove; he has decided in favour of cheapness and solidity; but in my experience

[1] The melting-point of the Paraffin employed for this purpose was—in accordance with the recipe of Menge—45°, and was thus nearly the same as the temperature of the body; already in the incubator this Paraffin becomes softened and semi-solid.

a more useful glove is one of thin (so called) Swedish, or Danish, kid, as it interferes less, perhaps, with the delicacy of one's sense of touch. The gloves, when moist from having been disinfected by a prolonged sojourn in antiseptic lotions, such as solutions of Corrosive Sublimate or of Lysol, can be drawn on without trouble. They are very much pleasanter to wear than thread gloves, because they fit smoothly to the hand at all points; they thus interfere but little with one's sense of touch (especially when made of delicate kid), and practically to no extent with the dexterity of the hand, when one has become accustomed to them. An undoubted drawback is the irritation which is produced on the skin by the antiseptic with which the glove is impregnated; this irritation is somewhat distressing, especially when the gloves are worn for any length of time (during several operations), and it necessitates the use of cosmetics. In my experience the irritation is greater when the gloves have been impregnated with Lysol, than when Corrosive Sublimate has been used for this purpose.

Even in the case of kid gloves, however, the impermeability is only relative. As Wölffler[1] himself has shown, germs on the surface of an artificially infected hand can reach the outside of the glove by the end of ten minutes, and not even the impregnation of the glove with Lysol can altogether prevent this from occurring, since, in the course of an operation, the Lysol in the pores of the kid soon comes to be replaced by blood and Serum. Schloffer's experiments, which he communicated to the Surgical Congress of 1899, certainly prove that the impermeability can be maintained for more than half an hour. He found that when he smeared his fingers with germs which were particularly virulent towards rabbits, and then drew on a kid glove and worked his fingers about in the peritoneal cavity of rabbits, he was unable to note—except in one experiment—the appearance of any manifestation of infection. It must, however, be admitted that this

[1] *Kongress der deutschen Gesellschaft für Chirurgie*, 1898.

experimental method of procedure deals with practical considerations after a fashion not hitherto considered in experiments with thread gloves, and I am convinced that, in the case of these latter gloves, such a method of procedure would have led to results of a deplorable nature. Still, this manipulation of the peritoneal cavity of rabbits cannot be regarded in the light of an *experimentum crucis*, since it did not include any vigorous squeezing and pressing such as occurs in practice in the tying of ligatures, and in other ways. Schloffer certainly mentions that, although it had been the custom at operations in the surgical Klinik to hand the silk with aseptic precautions (out of Alcohol), it is only since the introduction of kid gloves that all sepsis in connection with ligatures has practically ceased. This evidence does not, however, strictly refer to the impermeability of the gloves, for the simple reason that the Alcohol has to be taken into account, being ranked as an antiseptic. But, to judge from reports, sepsis in connection with ligatures is of rare occurrence, even in the case of those who favour the use of thread gloves, and certainly there can be little further doubt with regard to the permeability of thread gloves.

Thus experimental investigations with regard to the permeability of kid gloves are already brought face to face with great difficulties, for it is not an easy matter to eliminate, in artificial nutrient media, the after effects of the antiseptic which the glove contains, whether in direct or indirect contact with the germs.[1]

I have disinfected kid gloves by laying them in 1 in 5000 Corrosive Sublimate solution for a fortnight; they were then washed in water,[2] which was repeatedly changed, and were drawn on over fingers which had been artificially infected. When tested by means of the thread method (in the dust-proof box), it was undoubtedly proved that a large number of the threads

[1] The glove cannot be sterilised by means of heat; it must, therefore, be sterilised by chemical means—by being deposited in antiseptic solutions.

[2] A considerable portion of the Mercury adheres to the kid.

remained sterile, even fifteen minutes after the gloved hands had been squeezed together in Serum and there employed in the manipulation of ligatures;[1] but from some of the threads it was possible, at the end of ten minutes, to obtain germs which had previously been rubbed into the fingers. I must, however, add that even after longer intervals of time the number of colonies always remained relatively small.

Thus the kid gloves afford a considerable degree of security against the germs of the hand, especially during the first quarter of an hour, but the security is by no means absolute.

The ideal glove for operative use should fulfil the following conditions—it must be altogether impervious, and must have a smooth surface; it should not interfere, to any appreciable extent, either with the delicacy of one's sense of touch, or with the dexterity of the hands.

The *rubber glove* most accurately fulfils these conditions; and, since our acquaintance with the preparation recommended by Friedrich,[2] the manufacture of the article has certainly been brought to a state of perfection,[3] so that now its advantageous qualities —the maintenance of the delicacy of one's sense of touch and of the flexibility of the hand—are to be found in the disadvantageous—the ease with which it can be torn. Any one, however, who is accustomed to operate in these gloves, will be able to preserve them

[1] Before the threads were placed in the nutrient media (Agar and Bouillon) they were treated with Ammonium Sulphide solution.

[2] Silk gloves covered with gutta-percha, as recommended by Perthes, have no advantage over this preparation; the delicacy of one's sense of touch is better preserved in Friedrich's preparation. In Perthes' glove the silk casing is absent at the finger-tips, which are the most vulnerable points; it is not, therefore, calculated to last longer than the rubber glove pure and simple; besides, it is very much more expensive.

[3] This is only theoretically the case, for, unfortunately, one must admit that the quality of these gloves—even when obtained from the same manufactory—is very variable. Specimens are sometimes obtained which, even before they are sterilised, tear on the slightest pull. Therefore, before sterilising gloves which are to be used for important operations, one should never omit to submit them to a regular test, in order to prove their strength.

intact for long.[1] The smooth surface is a great advantage, and it even admits of mechanical sterilisation. Thus the glove will stand not only being rubbed with swabs, but even a good scrubbing with a brush without being damaged.

But there are two drawbacks which cling to this otherwise perfect preparation, which can either endanger the security of the hand within the glove, or can interfere with its dexterity during the course of an operation. Gaps may easily be made in the glove by means of instruments (rake retractors, dissecting forceps, needles, and even scissors), and reservoirs are thus formed underneath, which may become a source of danger. One must admit, however, that a great deal depends on the customary use of gloves (besides the excellence of the preparation), and that these injuries can, for the most part, be avoided with practice, though not as a matter of absolute certainty. What is of less importance is, that the glove always to some extent becomes displaced, especially at the fingers (this is all the more the case the less the glove fits the hand), which always detracts to a certain extent from the dexterity of the hand. Kid gloves are far less liable to this drawback.

Thus we know of no glove preparation, and are not likely ever to discover one, which will afford absolute security, and will not interfere with one's dexterity when used for operative purposes.

There is no doubt that operation gloves have done good work, for they have caused attention to be bestowed on the hands with a thoroughness which had never before been aroused. Their use, in my opinion, must, to some extent, be considered as a transient phenomenon, on account of their deficiencies and the complications which they introduce. From my investi-

[1] Personally, I have so far modified Friedrich's recipe that I draw the gloves on *dry*. They are regularly rubbed with powdered Talc, are sterilised in a casket with the dressings, and can easily be drawn on over one's fingers, after these have been disinfected and well dried with a sterilised towel; one thus avoids the unpleasant sensation that one's hand is damp.

gations I am forced to come to the conclusion that it is not only more advantageous (in view of the dexterity), but is even safer, to operate *without* the use of gloves, if the condition of the surgeon's hands is quite irreproachable, and if, in employing the measures of cleansing and of disinfection, he applies his mind sufficiently to what he is doing, both with regard to the details of the processes and the length of time to be occupied by each. But, when the condition of the hands is not irreproachable, the use of gloves in operating is an indispensable resource until cosmetic care has rendered the hands once more "fit for an operating theatre"; as has already been proved, hands in such a condition make a mockery of any mechanical and chemical cleansing. In such cases *thread* gloves are as unavailing as they are dangerous. The choice lies between *kid* and *rubber* gloves; the latter is to be preferred if one can wear a convenient size—*i.e.* if the ready-made sizes fit one's hand—and if one can obtain good preparations.

There is only one part of an operation—at the conclusion, when the ligatures and sutures are tied—when, even though the hands are irreproachable, the use of gloves may be of service. Then is perhaps the time to protect the surface of the hand in some measure, not only because the effect of the primary cleansing is least efficacious, but because the implantation of germs on foreign bodies, as represented by sutures and ligatures, has the most serious consequences. An advantage, which is not to be despised, is that the hands are protected from needle pricks (if no needle-holder is used), as well as from injury whilst tying ligatures and sutures; for such injuries to the skin are usually aggravated when the hand is subsequently cleansed. As the bleeding has then been arrested, and as this part of the operation is, on the whole, of short duration, even thread gloves may be of appreciable service at this stage.

I shall again refer to the question of the value of thread gloves as the subject crops up later in direct or indirect connection with operations. So long as thread

gloves are not thoroughly moistened they afford absolute security—against the germs of the hand, at least. Such sterilised thread gloves are of great assistance in the preparation of dressings, and particularly in the case of antiseptic gauze dressings, which it is better not to sterilise by means of heat.[1] It is also advantageous that those who assist at operations (theatre attendants, and nurses in charge of the dressings), whose hands are generally so far from perfect that they can never be cleansed with any hope of success, should wear gloves —just as Halsted directs. As their gloves do not become moistened, or only to a slight extent, by the duties they have to perform, thread gloves, in their case, afford considerable or absolute protection.

It is but natural, considering the inconveniences connected with the use of gloves for operating, that one should seek for other means to enable one to cover and to isolate the skin, and thus do away with the necessity of wearing an actual glove. Unna's proposal, which I have already mentioned, is one of the many that have been advanced ;[2] but it is impracticable, as the layer of soap is soon dissolved, and disappears in the course of the operation. The proposals of Menge and Schleich seem to be much more suitable; the former attempts to close the pores of the skin (and the irregularities of the epidermis) by means of *Paraffin*, whilst the latter hopes to attain a like result by means of the *wax* contained in his Marble-dust soap.

Why I have subjected these proposals to experimental tests, and now go closely into the question, is because such an impermeable covering would represent the ideal of what we ought to avail ourselves, to judge from our experiences with regard to the possibility of maintaining the hands free from germs during an operation.

[1] I have already (*Über Airol und ähnliche antiseptische Pulvermittel, Beiträge zur Klin. Chirurgie*, XV.) referred to the fact that Iodoform gauze, for instance (and still more Airol gauze), has its antiseptic value considerably diminished by being sterilised with steam.

[2] Schneider, for example (quoted by Schimmelbusch, aseptische Wundbehandlung), recommended a paste prepared with Carbolic Acid, Camphor, and Kaolin.

There is no doubt that such a procedure is a great deal simpler than the use of operation gloves; the dexterity of the hand is in no way interfered with, and it would, besides, be much cheaper to carry out such a process.

Of the many other advantages which Schleich, in his picturesque language, places to the credit of his Marble-dust soap, I desire to make special mention of this one, that the soap (together with the subsequent rubbing with a dry towel) invests the skin with an impermeable covering of wax, "forming a thoroughly aseptic glove as revealed under the microscope."

This assertion appears on the face of it to be somewhat unlikely, since wax is composed of different fatty acids, which are dissolved or emulsified in alkalis. Now Schleich declares that his soap is enabled, by the fat emulsifying Stearic acid and Ammonia which it contains, to thoroughly dissolve the fatty impurities of the skin, which are composed of "fats and acidulated sweat, Cholesterine and acidulated wax." One would need to presuppose that the soap was possessed of a regular power of thinking and of acting of itself were one desirous of believing that it first removes fat, wax, and Talc, from the surface of the skin, and then of its own accord deposits wax in the infundibula and in the other interstices which it has just cleansed.

All that Schleich brings forward as evidence in favour of his assertion is, that the skin looks as if it had been "waxed," *i.e.* polished, after it has been washed with the soap and well rubbed with a dry towel. I can fully confirm this statement, only I must add that this "waxing" does not depend on the use of Schleich's soap alone, but that *the effect can just as well be produced on any dry hand which has been well rubbed*, even though the hand has not for long come in contact with wax, if it is simply well washed with Benzene and then rubbed.[1] Whether there is any connection between this polishing of the surface of the skin and the fat, or wax, which is normally present in the skin, or

[1] The finer, the smoother, and the more tense the towel, the quicker the effect is produced.

whether it is simply due to a smoothing down of the corneal layer, is a difficult matter to decide, since it is impossible to identify wax by chemical means when it is present in such minute quantities.

But, even experimentally, Schleich's assertion can be proved to be erroneous:—

If hands, which have been cleansed and "waxed" according to Schleich's recipe, be washed and rubbed in sterile Serum at body heat, relatively more micro-organisms will thus be extracted by the Serum than in the case of hands which have heen cleansed according to Fürbringer's extended method.[1] Thus, at all events, the deeper parts of the surface of the skin and the orifices of the glands are not shut off by means of a "microscopic glove."

But it is quite possible by means of Schleich's soap to remove wax from the hand, after it has been placed there and then "waxed."

Particles of soot[2] were mixed in a mortar with pure beeswax[3] dissolved in Benzene (concentrated), and the dark-coloured mixture was then rubbed into the skin of the hands till it was dry. If the hands were now carefully rubbed with a dry towel, but little of the pigment was removed; but the blackened hand shone, and was thus "waxed." The use of Schleich's soap for one minute was sufficient to remove every trace of the black wax. The result, however, is the same with Potash soap, with or without a brush, but quicker, of course, if a brush is used.

[1] The presence of additional micro-organisms is due to the fact that the surface of the hand cannot be rendered as free from germs by means of Schleich's recipe as with Fürbringer's extended process.

[2] The use of soluble pigments, which, on the one hand, can be absorbed by the cells, and, on the other hand, can be chemically affected by the soap, must, in such processes, inevitably lead to erroneous conclusions, as I have already proved.

[3] I obtained by chance a supply of "pure beeswax." Schleich lays great stress on the absolute purity. It is, however, almost impossible to buy beeswax which is quite pure; it is usually mixed with yellow Paraffin. One can only *count* upon its purity when it is procured from an apiarist; but it is very expensive, as it takes the bees longer to produce 1 kilo. of wax than 5 kilos. of honey. The apiarist uses this precious wax himself for breeding purposes.

The wax is certainly able to block the deeper parts of the surface of the skin, and the hair and gland infundibula—a fact which is easily proved if, for instance, one microscopically examines hands which have been rubbed with Schleich's wax *paste*. The wax which is situated in these parts cannot, however, be "waxed," because the towel in its rubbings does not penetrate into the recesses; the wax remains in such parts in a semi-solid state (more in the form of a paste), until it is again removed by mechanical means.

Serum, which is faintly alkaline, is also able, to a certain extent, to loosen and to emulsify the wax covering the hand.

If hands which have been treated with the mixture of soot and wax, as above described, and then "waxed," are kneaded and rubbed in Serum at body heat, a great part of the black covering is removed.

Thus, if wax is to be employed as a covering for the hand, it ought essentially to be applied and rubbed on the skin *after* the hand has been washed with alkaline soap—it ought, in fact, to be applied last; but, as these experiments prove, this will in no way guarantee that the surface of the hand is isolated during an operation in which the hands are at work in contact with hot alkaline fluids.

Menge's proposal to cover the hand with a thin *layer of Paraffin* has *a priori* this advantage over wax as a covering, that the Paraffin is not dissolved in alkaline fluids; it is thus, even during an operation, more durable than wax. It was further expected that the solution of Paraffin[1] could easily be rubbed into the folds and interstices of the skin. Menge states that a hand thus covered with Paraffin absolutely ensures the water running off it (after evaporation of the Xylol). I can fully corroborate this, and can further add that the covering of Paraffin adheres to

[1] According to Menge — Paraffin (melting-point 45°) gr. 10 in Xylol 100 cm³. I used this composition to start with, but later soared to 20 per cent. solutions. In every experiment the former solution proved to be too weak.

the surface of the skin with greater tenacity than does a covering of wax, and therefore isolates it better.

The following experiments illustrate this:—

If a solution of Paraffin was mixed in the mortar with particles of soot, and the hands were well rubbed with this dusky mixture, the black discoloration of the hands withstood the mechanical cleansing measures far better than proved to be the case when the skin was rubbed with black wax and "waxed." A vigorous mechanical cleansing for the space of two minutes (Potash soap and brush, or Schleich's soap) was not sufficient to remove the mixture of Paraffin and soot from the delicate folds and interstices of the skin. Even a washing of ten minutes' duration in Serum at body heat was not sufficient to cause the black discoloration to disappear—though it made it considerably paler, which was undoubtedly due to the process of rubbing.

If hands, which had been rubbed with a solution of Paraffin and then covered with thread gloves, were kneaded and rubbed for ten minutes in Serum at body heat, the fluid of the gloves contained far fewer epithelial cells than if the surface of the hands had not been treated with Paraffin.

If hands, which had undergone a thorough mechanical cleansing for ten minutes, were rubbed with a solution of Paraffin and then bathed and kneaded in sterile Serum at body heat (in the dust-proof box), the Serum received but few germs in the first five minutes, and even at the end of ten minutes the number of germs to be obtained from the Serum was considerably less than in a similar experiment in which the hands had not been treated with Paraffin.

Thus Paraffin, when rubbed over the hands, is able to maintain its position there for a definite length of time, if no vigorous rubbing takes place; it is able, to some extent, to prevent the shedding of epidermic cells and germs from the hands.

As this Paraffin is softened to a considerable extent

at the temperature of the body, and thus facilitates the removal of the covering, I have looked for other media which are exempt from this drawback. Of the many media with which I experimented I shall refer to *gutta-percha* alone, which, in dilute solution,[1] can easily be rubbed into the skin, to which it adheres fairly tenaciously. The hand is at first somewhat sticky, though this disagreeable sensation soon disappears, especially when the hand is washed in cold water. I refrain from giving any more detailed account of the experiments which were carried out with this medium; it was undoubtedly proved that guttapercha (when applied in very weak solution) for long remains adherent to the skin, in the folds and interstices, even when subjected to friction. As soon, however, as the covering becomes widespread, *i.e.* as soon as the solution is used in such concentration that the whole surface of the hand is simultaneously covered with guttapercha, it is fairly easily removed by friction, and comes off like a film. Thus it is useless to completely cover the hand with guttapercha; so that Paraffin, of all the media, retains the premier place.

There are, however, three reasons which induce me to reject the use of such a covering of the hand for operative purposes—not so much that it affords no help, as that it is insufficient.

(1) The mechanical forces, which come into play during the course of an operation, are able to create gaps everywhere in the covering.

(2) The media, which are applied to the infundibula to close them, will always be elevated to some extent during an operation by the functional activity of the glands, and will then be mechanically removed.

(3) Such a covering interferes with the action of the antiseptic or aseptic lotions which are used during the operation.

With regard to the first objection it is easily proved

[1] It is impossible to make an accurate statement with regard to the concentration, as the guttapercha does not completely dissolve in Benzene.

by means of one's sense of sight and by bacteriological experiments.

If a silk thread is drawn through fingers which have been rubbed with a mixture of soot and paraffin, soot and wax, or soot and guttapercha, it gives rise to a white streak across the black surface. At the tips of the fingers, for instance, the covering is wholly removed (unless any small wounds are present), since the grooves here are shallow.

A sterilised silk thread, which is drawn through fingers covered by a sterile medium, is never found to be sterile, even although the hands have been subjected to a thorough mechanical cleansing before the covering was applied to them. Thus the thread always removes germs from the hands along with the covering. What the silk threads are able to accomplish by this action, instruments and swabs can also accomplish—a fact of which I have made sure.

The supposition that the infundibula of sweat glands and hair-follicles can be blocked for some time by means of such media—and this on a hand at work and in active motion,—is an illusion which certainly deceived me for a time, but is one that our physiological knowledge alone is sufficient to shatter. We are just as impotent to block for any length of time the excretory gland ducts by means of such media (such, at least, as are practicable for our purpose), as we are to remove the fatty material of the skin from the ducts. It is quite superfluous for Schleich and Menge to mention that these obstructive media can easily be removed again, and that therefore the functional activity of the skin undergoes no permanent damage; for, when the hand is at work, Nature herself sees to the removal of these obstructions, which are always weak and unstable in the tenure of their position, and with their removal the protective layer is at once demolished.

Finally, these coverings possess the great disadvantage that by their presence they render illusory any washing of the hands which may take place during the operation; for, even though there are gaps in the covering,

the lotions will be ineffective, and the germs from the deeper parts, as also those from the air, remain adherent to the surface of the hand.

For these reasons one cannot recommend—for operative work, at least—that the hands should be covered with such media, even when they possess the advantageous properties of Paraffin. Still, I shall show later that such media play an important prophylactic part.

CHAPTER VII

PROPHYLAXIS

A. *On Protection of the Hands, and Infectious Material*

A SURVEY of the results of those investigations, to which I have so far referred, might well impress one with a hopeless sense of impotency; for though neither mechanical nor chemical means are able to maintain the hands aseptic during the course of an operation, still we are forced to consider the hands as primarily responsible for the presence of germs in operative wounds, and the one possibility of excluding this source of infection—the wearing of smooth impermeable gloves—affords but an unreliable protection, because the gloves are thin and easily torn by reason of the demands which we make upon them for the sake of the operative technique.

A sense of irresponsibility might easily be added to that of impotency—one does what one can, and the result must be left in the hands of Providence.[1]

Yes, if one really does what one can! The proposition that all the germs on our hands, even those in the excretory ducts of the glands, come from *without* is a saying which should properly never be given voice to now; for, to any one who has really been impressed by this natural reflection, to whom it has become a matter of instinct, to whom it is flesh and blood—*sit venia verbo*—it is clear *that the crucial point of the whole question of the cleansing of the hands does not lie in the energy with*

[1] "Je le pansai et Dieu le guarit" (Ambroise Paré). A small amount of superstition takes root in every surgeon.

which they are cleansed before an operation, but in the prophylaxis!

Reference has been made from many quarters with regard to this question of prophylaxis, but, for the most part, merely in the form of some general statements which in the end amount to this—that one should avoid coming in contact with "infectious material." This somewhat vaguely defined prophylaxis is usually expressed by the obstetrical terms "period of abstinence" or "period of probation," as applied to students. To judge from a number of opinions on the subject, which were published by Sarwey in the year 1895, obstetricians, with few exceptions, require that students who have been working on the dead body or have merely in a general way frequented the pathological-anatomical institute, shall be debarred from attendance at cases of labour for, on an average, twenty-four to forty-eight hours (Winkel, three days; Zweifel, four days). The duration of this period of probation varies, and is optionally determined. So far as I can gather from the literature on the subject, it depends less on accurate conceptions with regard to the efficacy of the period of probation than on the number of unfortunate experiences which have fallen to the lot of this or that Klinik, and which have been ascribed to the circumstance that students or assistants had come in contact with infectious material before attending the case in question.

One must, however, ask oneself the question—Is it possible, or probable, that the dreaded micro-organisms or the chemical material (Fehling) will be removed or rendered innocuous in twenty-four to forty-eight hours?

The latter part of this question must receive an unqualified denial. There could be no question of the destruction of the germs in such a space of time, not even if they were kept quite dry. I have already proved that staphylococci and streptococci can, when dry, be maintained viable for weeks and months, and, by the continuation of these investigations, I am able to further extend the time limit which I then quoted and can add that—during the first week, at least—the degree

of virulence will, for the most part, undergo no material change.[1] Binaghi certainly attributes a power of disinfection to the healthy skin. He found that if pure cultures, or infectious matter emanating from the human body (fæces, septic urine, sputum), were spread on the skin and allowed to dry, not only had the number of the germs, but also the degree of their virulence, diminished by the end of six days. But, according to Binaghi's specification, the skin was disinfected before the septic material was applied to it, and we now know that antiseptics cannot be further removed from the albuminoid substance of the skin by means of the ordinary solutions; the antiseptic will thus have had some say in the matter. A diminution in the number of the germs does undoubtedly take place, as I have already proved in a previous section. This, however, proceeds from elimination of the septic material by the secretion of the glands and the desquamation of the skin. In numerous experiments in which I left staphylococci of great virulence to dry on the skin of my thigh I was unable to observe any diminution in their virulence during a space of six days (the duration of the experiment). Nor is it clear what would induce the destruction of the germs and the loss of their virulence in this relatively short space of time. As my investigations have proved, Lanoline, for instance, is not in a position to exert within a week an unfavourable influence on the vitality of micro-organisms (*Prodigiosus*, *Subtilis*); I was still able at the end of three months to cultivate germs from Lanoline which had been infected with bacteria.

Thus, by merely waiting forty-eight hours, no abatement can be looked for in the developmental capacities, or in the pathogenic properties, of germs which are present on our hands.

According to the express instructions of most obstetricians at the present time, students must avail themselves of this period of probation in order to remove from their hands, by means of a repeated process of cleansing which is to be thoroughly well carried out, the germs which they

[1] *Die chirurgische Bedeutung des Staubes*, 1892.

have acquired in the pathological-anatomical institute. The only question is whether this is possible or not. Unfortunately, no definite answer can be given to this question in general, either by means of experiments conducted in the laboratory, or from one's practical experiences, since individual factors play such an important part in the matter. Of these factors, the condition of the hands comes before everything else; then come others, such as the length of time that the septic material is handled, and of what it consists; the circumstance whether the hand, so soon as it has been infected, is thoroughly cleansed mechanically, or whether the material is allowed to dry on the skin for a longer or shorter time; how often, and with what degree of intelligence, the hands, during the period of probation, are washed and disinfected;—in short, quite a number of factors, which not only differ according to the individual concerned, but which, even in the same personality, are liable to variations.

There is thus no doubt that the experimental method employed by Henke to provide a general elucidation of the question is, if not erroneous, at all events insufficient. Even supposing that in these relatively few experiments there were no sources of error to be taken into account, still the experiments could in no way be entitled to supply a general answer to a question of such extraordinary importance.

I entirely agree with Gärtner, Fürbringer, and Henke, that if the hand has been artificially infected with pus, or even with pure cultures,[1] the micro-organisms of these media can more easily be removed from the hand than can the germs of the "hand in everyday use," which undoubtedly lie deeper; for, in most cases, the effect which was produced by a thorough mechanical cleansing was that, at the end of the act of cleansing, the germs which had been artificially introduced could no longer be identified on the hand, whilst other micro-organisms could always still be obtained. *This, however, only*

[1] So far, at least, as the infectious material was a sufficiently accurate representation of the normal impurity of the hands.

occurred provided that the cleansing followed immediately after the artificial infection, and that the condition of the hands was faultless. In the absence of these factors, if the artificially introduced infectious material had dried on the hands, and if the hands had not been washed for several hours, and, above all, if the surface of the hands was rough and cracked, then, in spite of repeated mechanical cleansings, individual colonies of the germs[1] which had been artificially conveyed to the hands could still be obtained from the hands after seventy-two hours. And this mechanical cleansing in no way differed in vigour and in mental concentration from that which is commonly carried out by students without supervision of the smallest degree!

As a rule, it is only the individual who has to bear the responsibility of the subsequent operation who can bestow on the washing of his hands the bodily and mental attention which they ought to receive; and even he must have learned this, or must have been taught prudence by his previous misfortunes. Thus this period of probation may, from the point of view of discipline, serve indeed as a warning; but I hold that one should not be content to merely acquiesce in the exclusion of students for an optional length of time, but that one should eradicate the evil itself.

Apart from the condition of the hands, the decision as to whether the infectious material may again be easily removed from the hands, or is to remain firmly adherent to them, rests with such departments as the pathological-anatomical institute, where it is possible for those at work to become seriously contaminated with pathogenic organisms. In such places measures should be enforced to ensure the protection of the hands—a point with regard to which I shall speak later; satisfactory provisions should be made for washing the hands in hot water, and their use should be enforced; there should be no lack of a sufficient supply

[1] Pyocyaneus and Prodigiosus which were grown in sterile pus, or spores of the *Bacillus subtilis*.

of clean towels,[1] so that after the hands have once been washed they may not again be infected by germs which have already been conveyed to the towel by the hands of other people, and which would then be rubbed into [2] the loosened surface of the hands which had just been washed; the clothes should also be protected from impurities by the wearing of overalls. If, in addition, the regulation is once posted that students, whose hands are not altogether in a satisfactory state (Indian ink test), shall no longer be allowed to attend obstetrical cases, students will thus come to have a better idea of what are the essential points at issue, and they will be supplied with a greater inducement to cleanse and to take care of their hands than is afforded them by the demand that they should undergo a "period of probation"—a demand which may or may not be wholly intelligible to the students. Still, a student knows well enough that later, in country practice, for example, he cannot and dare not continue to live up to this doctrine without running the risk of eventually falling foul of the law by his refusal to give his professional services in cases of urgency.

Thus moist infectious material, which has recently been deposited on the hands, can be removed without any very great difficulty by means of a vigorous washing, since little can penetrate to any depth when the hands are moist. Nowadays it is a matter of instinct not only for a surgeon, but for every well-educated person, to wash his hands as soon as they are soiled with pus, or

[1] It is especially worthy of note that Nauwerck (*Sektionstechnik* III., Auflage, 1899) insists that "on each occasion only freshly-washed towels which have not previously been used" should be employed. Where there are many dissectors (as in a class) this certainly complicates matters, and implies an increased expenditure, yet this is nowhere incurred to better purpose than in such places.

[2] The brush question is especially the vulnerable point here; and even though wood fibre and Schleich's soap cannot replace the brush, they may still, under such circumstances, be preferable to the usual sole-existent brush, which is kept in a state of dryness, and is used for months on end. Here again Nauwerck's carefulness is worthy of recognition; he keeps his brushes in glass boxes, which are filled with 5 per cent. Formaline solution, which is frequently changed.

fæces, or any other impurity. But the surgeon knows that it is not sufficient to wash off impurities that are visible to the naked eye, but that it is undoubtedly of greater importance to cleanse the hands at the moment by mechanical means, and later by chemical means. This is a universally accepted fact.

Thus the germs of the surface of our hands which we have to dread are not, for the most part, those that come from the coarse and visible impurities and that we instinctively remove at once, but are those that come from another source—a source which is but very insufficiently expressed by the conventional term *infectious material.* What, then, precisely is infectious material? Certainly not merely pus, fæces, and the constituent parts of dead bodies,[1] but all media that include the elements capable of giving rise in the human body to diseases of wound infection. These germs are present in and around all granulating wounds, on the skin of patients, in the dressings of discharging wounds, in the mouth more than in the anus[2]—on everything, in a surgical hospital, wherever the hand is introduced. This may sound like exaggeration and nervous imagination. But it is a fact—and everyone can easily put the matter to the test—that in the dust of surgical wards and of dressing-rooms, and on the articles used in them, it is nearly always possible to find the micro-organisms which, as we know, give rise to the infection of wounds. *Streptococci* are not often found. In dry surroundings their vitality is lower than that of staphylococci; they are, besides, less easily identified in plate cultures, and are easily stifled by germs that

[1] In bodies, where death was not due to a general invasion by cocci, these elements were less to be dreaded than, for example, the vaginal discharge of a woman suffering from puerperal fever.

[2] As I often had occasion to note, the pure æsthetic sensibilities of educated humanity are very markedly shown with regard to this part—even in the case of surgeons: the use of a finger-stool is seldom omitted in examinations of the rectum and vagina, whilst the mouth is palpated with the hand unprotected, and as a rule the subsequent cleansing is merely superficial, because—the hand has not acquired an unpleasant odour!

develop more rapidly and more plentifully. But the *Staphylococcus pyogenes albus*, and *aureus*, are germs which are always reported as present in granulating and discharging wounds, as also on the surface of the bodies of the sick and the sound, and in the dust of a sick-room. The Staphylococcus albus certainly appears the oftener, and it is the germ that can also be bred most frequently from our hands.

It is now customary to regard this *Staphylococcus albus* as a negligible factor.[1] This is absolutely incorrect. In more than a thousand specimens of pus that, during the last thirteen years, I have had an opportunity of examining, the Staphylococcus albus was present in a large percentage of the cases, both pure and mixed with aureus.

Any one who has much to do with pus, or with other media containing staphylococci that emanate from the human body, must observe how *uninterrupted is the series of delicate shades* between the *pure white* and the deep *golden-yellow* stained colonies of staphylococci, so that it is often difficult to make a classification according to the shade. Sometimes in the same culture it is possible to obtain colonies of most diverse shades from one and the same specimen of pus. Now, if staphylococci are to be classified by the shade of the culture (and we have no other indication), there are not merely three to four forms, but ten to twenty. Lubinski has already shown that, by carrying on the growth under anaerobic conditions, it is possible to permanently arrest (even for aerobic growth) the production of the colouring matter in the case of Staphylococcus pyogenes aureus, which primarily develops of a golden-yellow colour.

[1] This only refers to the *Staphylococcus albus*, and not to the *cereus albus* as well. It is possible that non-pathogenic forms are included in the class of Staphylococcus albus. It is, at all events, impossible to draw distinctions by means of one's eyes and the nutrient media; and the results of experiments with animals—negatively, at least—can only be used with caution. I may mention here, however, that I have often succeeded, by experiments on animals, in establishing pathogenic facts with regard to white cocci grown from the hands, and by experiments on my own body.

I have seen the yellow Staphylococcus pyogenes aureus become permanently divested of its colour, even though its growth was carried on without the elimination of oxygen, where chemicals (antiseptics), which had accidentally, or intentionally, been introduced, had an important bearing on the matter. Similar observations have also been made by Courmont, Rodet, and Netter.[1]

This should not be so very surprising, for we certainly know that it is possible to cultivate white prodigiosus cultures on Agar, or potatoes, according to choice (Schottelius),[2] and that both Prodigiosus (Reinicke) and Pyocyaneus (Wasserzug)[3] can, in the presence of antiseptics, be deprived of their power of producing colouring matter.

In artificial developmental experiments it is certainly much more difficult to restore to staphylococci, which have already developed as white, the power of producing colouring matter. Such observations are mentioned by Bertoye,[4] Courmont, Rodet, and Netter, as also by Riggenbach in investigations carried out under my direction. Perhaps the explanation of why it is scarcely possible, in artificial nutrient media, to restore to an "albus" the power of producing colouring matter, is to be found in this, that it is impossible, as a rule, to use the *human body* for the purpose of experimental investigations, and from it alone the "aureus" springs. Now, I was enabled to get to the bottom of the whole matter by experiments on my own body. I shall deal more closely with the subject of these investigations elsewhere, but at present I confine myself to mentioning that with white staphylococci (partly grown from normal

[1] Rodet et Courmont, Société de biologie, avril 1890; Courmont et Jaboulay, Société de biologie, mai 1890; Courmont, Société de biologie, juillet 1891; Netter, Société médicale des hôpitaux, 18 mai 1894.

[2] Schottelius, *Biolog. Untersuchungen über d. Micrococc. prodigiosus*, Leipzig, 1887.

[3] Wasserzug, "Sur la formation de la matière colorante chez le bacillus pyocyaneus," *Annal. de l'instit. Pasteur*, 1887, p. 580.

[4] Bertoye, "Microbes of Infective Osteomyelitis," *Lancet*, 1886.

skin), which I rubbed into the skin of my thigh or forearm, I was able to produce colonies of every shade of colour up to that of intense golden yellow, from the boils and impetigo pustules which were produced by this treatment of the skin.

So long, at all events, as we are not better acquainted with the conditions under which colouring matter is produced by bacteria, we must remain completely in doubt as to whether or not we are justified in drawing a distinction between the forms of white and yellow staphylococci, and this in spite of the ridicule to which such a doubt is usually subjected. From my investigations I must altogether ally myself with the opinion of Lubinski—that the different staphylococci "are not independent forms, but are merely physiological varieties of one and the same form, which varies under the influence of opposing vital conditions."

In forming an estimate with regard to the germs of our hands, a fact of especial importance is that the pathogenesis of the staphylococci tallies on the whole with the production of colouring matter, and that we must, as a general rule, regard the primarily yellow form as more pathogenic (as possibly the more highly developed form).[1]

Thus, I consider it to be unquestionable that the sick-room and the dressing-room are more likely to provide

[1] The observation of Fischer and Levy (*Deutsche Zeitschr. f. Chir.*, Bd. 36., S. 95), which has often been quoted, that in Strassburg the white staphylococcus has been found to occur much more frequently than the yellow, stands quite by itself; and, considering the unanimity of the reports from every other source, it is inconceivable. Only eleven cases (Osteomyelitis) are mentioned by the authors, in which the albus could be bred nine times, and the aureus twice. I have already shown with reference to this that an intentional, or unintentional, addition of certain chemicals (antiseptics, for instance) to the nutrient medium, can deprive the aureus of the power of producing colouring matter. I can further add that there may also be something with regard to the manner in which the artificial nutrient medium is prepared, which produces the same result. All the staphylococci, which developed on a portion of Agar which I prepared two years ago, were white or only very faintly coloured. I could not be certain as to where the source of error lay; it may, however, have been due to the fact that this portion of Agar had been heated and centrifuged in metal vessels.

our hands with their pathogenic germs, than is the operating theatre (even one for septic cases). The infectious material is most easily established when the hands are dry, and it *imperceptibly* reaches the hands in minute quantities in the form of dust, dried discharge from wounds, scabs, soiled dressings, etc., and is quickly rubbed into the deeper parts.[1]

An observation with regard to my own hands shows how important these factors are.

Towards the end of the summer term of 1898, my hands were somewhat rough as a result of my experiments with regard to the cleansing of the hands (Ammonium Sulphide), and other work of a chemical nature. Notwithstanding the fact that I had at that time carefully avoided coming in contact with pus, or with suspicious secretions, or excretions, and that in the event of my having by chance come in contact with such discharges, I had immediately washed my hands thoroughly and disinfected them, the numerous investigations to which I subjected my hands showed the presence of yellow staphylococci, besides numerous other germs, with a regularity that was quite alarming.

I devoted a four weeks' summer holiday to the careful treatment of my hands, and as the condition of my hands was irreproachable when I returned to my duties in the Poliklinik (or as acting-substitute in the Klinik) and to my private practice, I performed my work from the first day onwards wearing rubber gloves. I only removed the gloves when the operations I had to perform were on non-infected and unsoiled tissues.

The result was splendid. The number of germs on my hands had very considerably decreased (which is partly to be ascribed to the good condition of the hands), and in the numerous investigations which I

[1] A fact which should not be forgotten in this connection is, that (as I shall again discuss later) a surgeon's hands are more easily infected, *i.e.* loaded with impurities, than those of any one else, because the corneal layer of a surgeon's hands becomes loosened from the frequency with which they are washed, and the surface of the skin, together with the infundibula of the sebaceous glands, are relieved of their fatty material.

subsequently carried out I was no longer able to cultivate from my hands staphylococci which were primarily yellow in colour.[1]

Thus, in order to ensure that the hands are clean, such precautions must be taken apart from the work in the operating theatre; but it is unnecessary for a surgeon to operate in gloves if he protects his hands with gloves (or their equivalent) in the event of his touching patients other than those prepared for operation.

Even this dictum will seem to be somewhat exaggerated, and one is inclined to suppose that it is still time enough to protect the hands with gloves when one is confronted with material of a doubtful nature. But, so long as "infectious material" gives no sign of its presence, either by some peculiar colour or smell, or by some such flag of warning, one must be prepared to encounter it in any part of a hospital.

Those who do not approve of rubber or kid gloves, which are certainly the best guarantees of safety, can, for this form of surgical work, avail themselves of coverings of paste, or of soap. Schleich's *wax paste* affords a fair amount of protection against micro-organisms settling on the hands; but it is more easily removed by friction than a *covering of Paraffin*, such as Menge recommends, which can be used with little expenditure of time and of material.

[1] I have thus far avoided making any statement with regard to the success or failure of operations where the hands were prepared according to this or that method, because, in a purely individual experimental treatise, no definite conclusions can be drawn from the result of an ordinary operation—not, at least, if assistants and theatre attendants took any part in the operation, and could thus affect the result. I have, however, nothing to conceal. At the time that I have referred to above I had, as substitute for the late Professor Socin in the Klinik, to perform fifty major operations on cases that were primarily aseptic, and that healed by first intention (hernias, goitres, laparotomies, amputations, excisions of joints). In two cases the healing of the wound was interrupted—an abscess of the abdominal wall after a laparotomy, and a ligature that gave rise to suppuration after an operation for goitre. I had also partial charge of the Poliklinik and of the septic department during this period; but, whilst at work in the hospital, with the exception of the operating theatre, I always wore gloves, though never for operating.

I have already suggested that great stress should be laid on this manner of protecting the hands in pathological-anatomical institutes, and it should certainly be made obligatory in the case of students.

It is self-evident that when this form of surgical (or pathological-anatomical) work is concluded, the paste and everything attached to it must be thoroughly removed from the hands by means of a vigorous mechanical cleansing.

Prophylaxis should begin in the operating theatre. Helferich, Berndt, and others have suggested that it may be important to disinfect the hands *after* an operation. I consider that a conscientious mechanical cleansing—in so far, at least, as one has not been working with infected tissues—is not only sufficient but is more efficacious, for reasons, to which I shall again refer later. The whole matter at stake is pretty well this, that blood and serum—these breeding grounds for germs—must be thoroughly removed from the hands; and this certainly requires a vigorous cleansing, since, at the end of an operation, some blood has dried in the small folds and cracks (particularly in the region of the nails).

B. *Care of the Hands*

As I have already repeatedly emphasised, it is impossible by any process of cleansing or of disinfection to remove or render inoffensive the germs which are present on rough or chapped hands, especially when the epidermis in the region of the nail is cracked or ragged.[1] The germs, lying as they do in the recesses of these fissures of the epidermis, are protected from the influence of mechanical and chemical interference.

How true this is is shown by experiments in which Indian ink or soot is rubbed into the hands before they

[1] Reinicke mentions, that cultures could always be obtained from his hands when they were affected with a dry scaly eczema the result of washing them in Alcohol, which at other times always rendered them sterile.

are examined microscopically. It is quite possible that the hands may appear perfectly clean after they have been subjected to the different acts of cleansing, and that even under the microscope, as the case may be, nothing further may be visible in the places where the black particles are usually to be found. *Still these minute black particles are so firmly established in every little wound of the epidermis, that no mechanical process of cleansing, however it may be carried out, is able to dislodge them.*

The condition of the surface of the hands is the most important factor with regard to the possibility of cleansing and disinfecting the hands—and here my investigations are strong in their unanimity.[1]

With regard to these epidermic wounds, they are much less likely to be a source of danger when they extend to the cutis, and, from the fact of their bleeding, attract our attention to them, than when they are merely small wounds of the epidermis proper, such as are very easily produced by instruments. Needle-pricks on the points of the fingers are particularly liable to form holes which cannot be cleansed. After sewing for ten years with my hand unprotected I now have recourse again to the needle-holder, because wounds are almost inevitable in forcing the needle through dense tissues.

But such small wounds, on hands that are otherwise in good condition, are mere trifles as compared with those fissures of the epidermis which are so often visible

[1] Felix Würtz lays great stress on the condition of the hands. Thus, for example, he lays down the law with regard to midwives: "She must live an honourable and upright life and refrain from arduous toil such as vineyards, fields, or gardens entail, together with washing, sweeping, and attending to the fire, or such like duties which tended formerly to make her hands rough . . . . and, that I may explain the matter by means of a simile, as one is in the habit of keeping the hands pure, clean, soft, and delicate when one deals with silks, pure flax, pearls, or gold work, how much more precious and valuable is the human frame that it be kept pure and clean." (*Wundarztney*, 1563.)

The good operative results, which, for instance, Schleich obtains, are principally due to the fact that he pays the most careful attention to the condition of his own hands and those of his assistants, which are more easily cleansed on this account.

on the hands of surgeons. The latter are undoubtedly produced by the vigorous mechanical cleansings, and, still more, by the chemical disinfectants which are used to purify the skin. It is quite unnecessary for any one with rough hands, or merely with trifling fissures at the points of his fingers, to have recourse to intricate bacteriological tests, if already he has subjected his hands to the experiment with Indian ink or soot; the stereoscopic microscope shows him clearly enough that his hands are incapable of being effectively cleansed.

Any one who does much operative work generally sighs for an *antiseptic lotion which is non-injurious to the hands.* Trial is made of the various media, one after the other, as they are offered for sale with the airy recommendation "that they neither injure the hands nor precipitate albumen." But no efficacious antiseptic lotion which fulfils these conditions will ever be granted on earth; for the imbibition by the cells—this chemical union between the albumen of the cells and the antiseptic — is the very condition that an efficacious antiseptic must fulfil.

Different antiseptic lotions certainly attack the hands in varying degrees. Lysol is less injurious to the hands than are solutions of Carbolic acid, since soap, in addition to the antiseptic, remains behind in the skin. This, to some extent, acts as a protection to the skin, though perhaps it has also the effect of somewhat interfering with the absorption of the antiseptic. Many are the complaints with regard to solutions of Corrosive Sublimate, and yet we cannot revile such a solution just for the very effect which we desire—its efficacy on albuminoid substances. Thus to seek for antiseptics which do not injure the hands is unavailing. Those who pin their faith to a disinfection with chemicals, must just reckon with this fact, that antiseptics cannot act electively amongst albuminoid substances; and, accordingly, the epidermic cells, just as the microorganisms, are subject to the influence exerted by the antiseptic, more particularly when it is allowed to act for some length of time. Therefore one must seek in

some other way to remedy this drawback—the injurious effects which are produced on the hands.

It is of primary importance that the chemical disinfectant should, after the operation, be again removed from the skin as far as is possible; what is of great value for this purpose is a thorough mechanical cleansing (with hot water and soap) carried out at the conclusion of the operation, as also the omission of a further process of disinfection—in the event, at least, of the operation being at an end. I can strongly recommend in this connection that the hands be washed and bathed in an *infusion of Bran*. As I have already had occasion to mention, this rich albuminoid fluid is in the highest degree qualified to extract Corrosive Sublimate, for instance, from the skin. I merely refer to the fact that the black discoloration of the nails—a source of much annoyance to those who favour the use of Corrosive Sublimate—is either totally absent, or is only evident to a slight extent, if, as a matter of course, the hands are washed in a solution of Bran at the close of the operations.[1]

To a lesser extent, however, *our hands suffer from the disadvantage that we subject them to too frequent processes*

[1] This black discoloration of the nails, which may be the cause of considerable distress to the æsthetic feelings of a surgeon, is no doubt due to the formation of Sulphuretted Mercury in the nail. Horn contains a large amount of loosely held Sulphur, which, however, combines but tardily, and little by little, with suitable reagents. (Suter, "Über schwefelhaltige Abkömmlinge der Eiweisskörper," *Dissertat. und Zeitschr. f. physiol. Chemie*, 1895.) This black discoloration of the nails is the more intense, the more extensive were the preparatory measures taken to disinfect the hands. A vigorous use (rubbing in) of Potash soap furthers the discoloration, since the alkali is able to detach some portion of the Sulphur, and the penetration of the Corrosive Sublimate into the horny substance (or into its outermost layers) is, in addition, facilitated by the soap and by the subsequent use of Alcohol. No doubt the skin of our hands would also in time show a slight dusky discoloration, on account of the Sulphur (in very small quantity, certainly) which is present in the corneal layer, were it not for the fact that this layer is so low in vitality and is so often changed, whilst the superficial layers of the nail cannot be spontaneously shed. Thus the only way in which this discoloration of the nails can be avoided is by dispensing with the services of Mercury as an antiseptic, or by protecting the region of the nail, by means of some impermeable substance (Traumaticin), just before the act of disinfection

of mechanical and chemical cleansing. It no doubt sounds paradoxical for me to say that the oftener we wash and disinfect our hands, in the vigorous fashion to which we are accustomed, it is all the more difficult for us to free them from the presence of germs; and yet this is the case. When the epidermic covering of the hands is thus loosened and is freed from its fatty material, the impurities, which have settled on them in the form of dust, or otherwise, not only adhere much better, but are able to make their way between the loosened epithelial cells (apart altogether from the small wounds); and when some hours later the hands are again dry, the impurities are, as a matter of fact, lodged, to some extent at least, in the deeper parts.

These conditions may be very easily demonstrated by means of experiments with Indian ink or soot.

If soot or Indian ink be rubbed, till it is dry, into a hand which has already been freed from its fatty material and washed (the condition of the hand being, however, perfect!), it is quite possible to almost completely remove such an impurity by means of one careful washing. If, however, the experiment be continued, *i.e.* soot be rubbed in several times and then again washed off, the third supply of soot *can no longer be removed by any form of cleansing*—and this visibly to the naked eye; the black particles between the epidermic cells and in the tiny wounds can thus defy for days any process of cleansing.

Each vigorous process of cleansing signifies injury to the hands. Surgeons wash their hands too often, *i.e.* they do not lay sufficient stress on the necessity of one vigorous cleansing. This does not so much apply to the operating theatre as to wards and dressing-rooms.

As various different authorities rightly insist, it is unnecessary in an *operating theatre*, when the hands have once been thoroughly cleansed, to again subject them to an equally severe process of cleansing for each subsequent operation, unless indeed the tissues which have been operated upon were infected (when the use of gloves

is to be recommended), or the operator has had his hands soiled to any considerable extent; still, even in such a case, it is sufficient to at once wash off the dirt whilst it is still fluid or moist.[1]

In order to avoid injuring, during an operation, both the hands and the wound by the use of an antiseptic lotion, sterile water or common salt solution is used in many places to wash the parts. Here we also employed these unirritating lotions for this purpose until I was horrified to find that a primarily sterile lotion was very markedly contaminated by the hands having been once washed in it during an operation; more or less numerous colonies could on several occasions be grown from even 1 cm.^{3} of such a lotion. If the surgeon has at his disposal a sufficient number of unoccupied attendants, the lotion should be renewed each time after the hands have been washed; and, in my experience, they cannot too often be washed in this way. Apart, however, from the complication and the moving about in the theatre which this entails, such a procedure can in no way guarantee the sterility of the solutions from the mere fact of the basins being emptied and then refilled. Nor can the attendants who perform this task have their hands sterile, for they must handle everything. Generally speaking, the presence of every additional attendant at an operation is an evil on account of the difficulty of control.

For this reason we again have had recourse here to antiseptic solutions (Corrosive Sublimate) for the purpose of washing our hands during operations—less, certainly, in the hope of affecting, by means of such a brief process, the germs which are present on the surface of our hands, than with a view to rendering innocuous

[1] It is particularly absurd of a surgeon to insist on subjecting his hands to a mechanical and chemical process of cleansing when he is about to operate on infected tissues (in acute inflammatory processes). He does not benefit the patient in the least by this, and may do himself (or other patients) a great deal of harm; for, after the skin has undergone such a thorough process of cleansing, it is extremely liable to absorb the infectious material and to keep a firm hold of it. For such cases the hands should simply be *protected*.

the septic material, which has thus been conveyed to the lotion, when the lotion is again used to wash the hands. It is relatively unimportant whether the germs are thus killed, or are merely disadvantageously affected as regards their power of development. At all events my investigations with solutions of Corrosive Sublimate, which had thus been used, show that after definite intervals of time it is either altogether impossible to cultivate germs from these solutions, or only in very small numbers and much more slowly than usual.[1] It is self-evident that even here there are limitations, and that we have no right to expect the antiseptic to accomplish more than the laws of chemistry permit. Solutions of Corrosive Sublimate, when they contain much blood, must be changed, for the blood itself combines with a considerable portion of the Mercury.

The objection that the Corrosive Sublimate, which is conveyed to the wound by the hands, may be capable of injuriously affecting the process of healing, can scarcely be taken seriously. The trifling quantities have less association with the cells than with the fluid in the wound. I shall show elsewhere that the injurious effect of trifling quantities of Corrosive Sublimate on the cells of the body, *i.e.* on the process of healing, has been greatly exaggerated. When I compare the statistical results, I find that the substitution of weak solutions of Corrosive Sublimate for sterile lotions had, on the contrary, an excellent effect on the process of healing. I do not attach great value to such statistics, for, as I have already shown, no individual factor can by such means be shown to be blameless.

It is not, however, so much in the operating theatre as in *dressing-rooms* and *wards* that the mistake is made of washing the hands too frequently or too thoroughly.[2]

[1] The investigation did not take place immediately after the hands had been washed, but 3–7–10 minutes later, *i.e.* at the time approximately when the surgeon's hands again require to be washed. I need hardly say that in the experiments with cultures the Corrosive Sublimate was eliminated by precipitation with Ammonium Sulphide.

[2] This applies more to the assistants than to the surgeon in charge of the hospital, as he does not personally change the dressings, or only

What, then, is the object of again subjecting the hands to a thorough mechanical cleansing and disinfection before each dressing? Is it necessary, as a rule, to touch a wound of this kind with one's fingers? Exceptional cases may occur now and then; but, as a rule, it is merely the instruments, or dressings grasped by instruments, which require to come in contact with such wounds, and these can easily be sterilised, and are, as a matter of fact, nearly always ready in a state of sterilisation where they are needed. It is seldom necessary to investigate the wound by touch; and all pressing and squeezing of the parts is usually injurious.

It is only conceivable that such wounds (if, at least, the dressings have not been changed for the first twenty-four hours) become infected when the granulation surface has been injured. An intact granulation surface, which is outwardly exposed to a more or less constant and firm pressure, does not allow the germs which are deposited on its surface to penetrate inwards, nor to develop, in general, when the physical conditions (dressings) are favourable.[1] It is far from my purpose to seek to deny that a wound can still be infected when the dressings are changed. But the blame of this does not so much lie in the introduction of septic material, as in a lesion of the granulation tissue, brought about by some deficiency in the technique or by a want of

very exceptionally; but the assistants do, after all, affect the result of the operations at which they have had to assist. The reason why the condition of assistants' hands leaves, as a rule, very much to be desired, is, that they are frequently obliged to mechanically cleanse their hands and to wash them in antiseptic lotions, not only during operations but throughout the course of the whole day.

[1] These investigations, which were carried out under my direction and have not been published till now, in which the fate of the germs was mainly traced by means of microscopic sections, have been fully confirmed by the results obtained by Noetzel and Afanasieff. The retention of the germs on the granulation surfaces entirely depends on the physical conditions affecting the wound. Whilst the germs are quickly removed from exposed surfaces, they can, when covered by dressings which absorb badly (silk protective, adhesive plaster), not only multiply on the surface, but also grow in the deeper parts from the granulation tissue having been injured by the non-escape of the matter secreted. The sterility of dressings is much less important than their capacity to absorb quickly and uniformly.

intelligence, and more especially in the use of unsuitable dressings. Preobajensky's meritorious work has received too little attention in this connection, and "physical antisepsis" is still too much overlooked on account of the hysterical pursuit after chemical disinfectants of every possible and impossible description. Any one, for example, who straight away has closed up with adhesive plaster or "pflastermull" a granulating wound, even though it is but a small one which is to all appearances clean, has no right to be surprised if inflammatory symptoms make their appearance.

Thus in wards and in dressing-rooms there is much less danger of the surgeon infecting patients than there is of his *charging his hands with pathogenic septic material.* The essential remedy is the *protection* of the hands. Every difficulty is obviated if he can accustom himself to wear rubber gloves whilst paying his ward visit and changing the dressings; for then he can cleanse his hands mechanically as often as he likes, and wash them—according to formerly received ideas—in antiseptic solutions; the surgeon's hands will thus be proof against germs, and, above all, *will be treated with consideration.*

Much is thus attained, but not everything; for the condition of the hands is still seriously affected by the preparatory measures which are taken in connection with operations. It is not usually sufficient, after operating, simply to remove the blood and the soluble antiseptic from the hands. *A very busy operator can only keep his hands in faultless condition, when he earnestly attends to their condition with cosmetics which can in no way damage them.* The objection, that there is not time for all this, must count for little when one considers that an ill-conditioned hand requires considerably more time to be spent on the almost impossible task of cleansing it, and when one realises to what a great extent the possibility of eliminating the dangers, which emanate from the surgeon's hands, depends upon the condition of the surface of the hands.

It is of primary importance *to restore to the skin some*

of the fatty matter which has been so thoroughly extracted by the hands having been washed with Potash soap and other media which remove the fat. It is impossible to continue with impunity removing fatty material from the skin, unless this is replaced in some form. The rubbing of lanoline, wax, or other ointments or pastes into the hands immediately after operations, *i.e.* when the hands have been washed and properly dried at the close of operations, certainly goes a long way towards preserving the hands. When this is done the object is not so much that the ointment should form a thick layer on the surface of the hand, as that it should be thoroughly rubbed and worked into the skin for some length of time; the surplus can be removed later with a dry towel.

It is for dermatologists to criticise the individual preparations. Most of the fats are, in my experience, of great service in this respect, and, in particular, the preparations with lanoline—but without the addition of an antiseptic. Glycerine, when used pure and simple, injures the hands, because it extracts water; but still there are preparations containing glycerine which act excellently.[1] Schleich's wax paste also helps greatly to preserve the hands, as I have satisfied myself.

If the hands are rough and cracked, the best treatment is to bathe and wash them in Bran infusion;[2] in conjunction with this the best plan is to wear, during the night, kid gloves over the smeared hands. An excellent way of removing the roughened epidermis, especially at the tips of the fingers, is to carefully rub the parts with smooth *pumice-stone* under the water-tap. I always rub my hands in this way with pumice-stone after prolonged operations, and this has

[1] "Glycerine and Honey Jelly," which is sold in tubes, has for long been in use in the surgical Klinik at Basel, and makes the hands very smooth. It does not, however, contain any fat, and therefore necessitates a subsequent greasing of the skin.

[2] I have already shown that by thus washing the hands in Bran the antiseptic can be removed from the skin.

increased the delicacy of my hands, though not their subtlety.

But the important point is not so much the detail or the preparation, as the circumstance that *the surgeon should regard this cosmetic treatment of his hands as being as much a nightly duty as the winding up of his watch.*

CONCLUSION

THE time has now come to close this discussion with regard to the various methods of cleansing the hands, and to abandon this restless search after fresh means of disinfection. The surface of the hand cannot with any certainty be freed from its germs, either for a brief, or for a prolonged, period of time; this must be accepted as inevitable after the investigations I have recorded, and the results of others that I have quoted, and it is a fact that has to be reckoned with. No mechanical cleansing, however sagely conceived and scrupulously carried out, and no disinfectant, is capable of penetrating into *those* recesses or hiding-places in which some portion of the septic material has settled. Antiseptic solutions are arrested by the wall of sebum or fat that surrounds these germs, and that, for natural reasons, either cannot be broken down, or only to a partial extent; Alcohol renders their action in the deeper parts even more difficult, for it has the effect of still further deepening and closing the openings. It is vain to seek for more powerful disinfectants, for it is rather the epidermic cells that fall victims to strong chemical agents than the germs which are well sheltered in the deeper parts.

If we balance the facts of the case as they are known to us, the result sounds familiar—"Prevention is better than cure." The tendency towards *prophylaxis* has been particularly strengthened and fortified by recent bacteriological knowledge within the last ten years, and has penetrated like a breeze of the morning into the mist

of polypragmatic therapeutics; it is time that this guiding principle were better understood with regard to the cleansing of the hands. The principle of aggressive action has claimed most attention; protection has no doubt been recommended, but it has, on the whole, been carried out to very little purpose and has not been subjected to the same scathing criticism that has been meted out to the various methods of cleansing the hands.

New methods of cleansing and of disinfecting the hands can do little more to improve matters. This question has now been carried as far as is possible. So much the more can the question of prophylaxis be carried further, and much still remains to be done in this connection. In addition to the narrow interpretation of the word prophylaxis as the "protection of the hands," it should also include here the technique of surgical operations and of dressings, the performance of operations [1] with purpose-like swiftness so that the tissues are cleanly and surely severed—an art which requires not merely practice and experience, but perfect topographical knowledge and a power of rapid determination; it should further include the use of instruments in place of fingers, and, not least, the comprehension of "physical antisepsis"—the suitable and appropriate application of the dressings. Once each and all of these factors can be equally attended to, the time will have come when hope in Providence may be relegated to the things that are past, and the surgeon, at the conclusion of an operation, will no longer be faced with uncertainty as to the result.

[1] As E. Bumm has often forcibly remarked.

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PLATE I.—Septicity of the Surface of the Hand (inclusive of the Space under the Nail), as tested by means of the Thread Method.

A Hand in faultless condition, and not subjected to a process of cleansing, after it had been immersed in hot, sterile water.

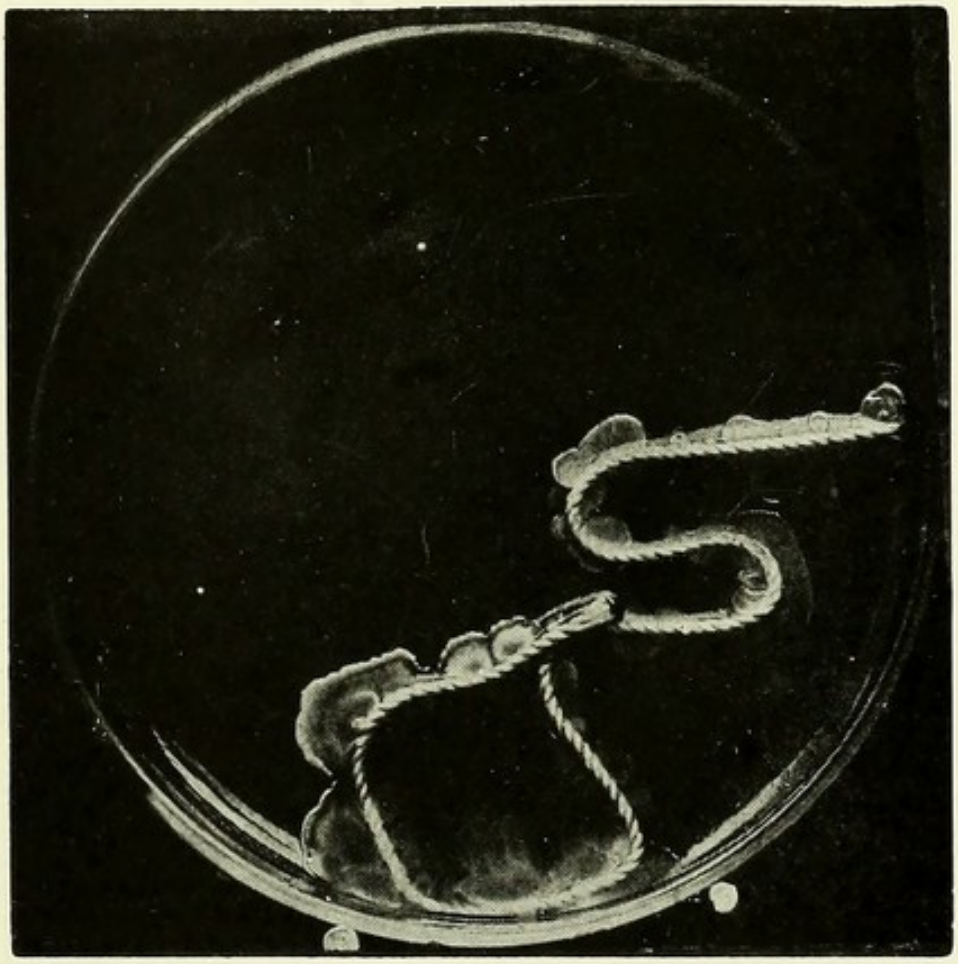

PLATE II.—Septicity of the Surface of the Hand (inclusive of the Space under the Nail), as tested by means of the Thread Method.

A Hand (the same as in PLATE I.) after it had been subjected to a process of cleansing for seven minutes with hot water, potash soap, and a nail-brush.

This also serves as an example of a Plate in which the germs had not been properly distributed, *i.e.* mostly emanate from the Thread, partly from the commencing solidification of the Agar, and partly from the insufficient oscillation of the Plate.

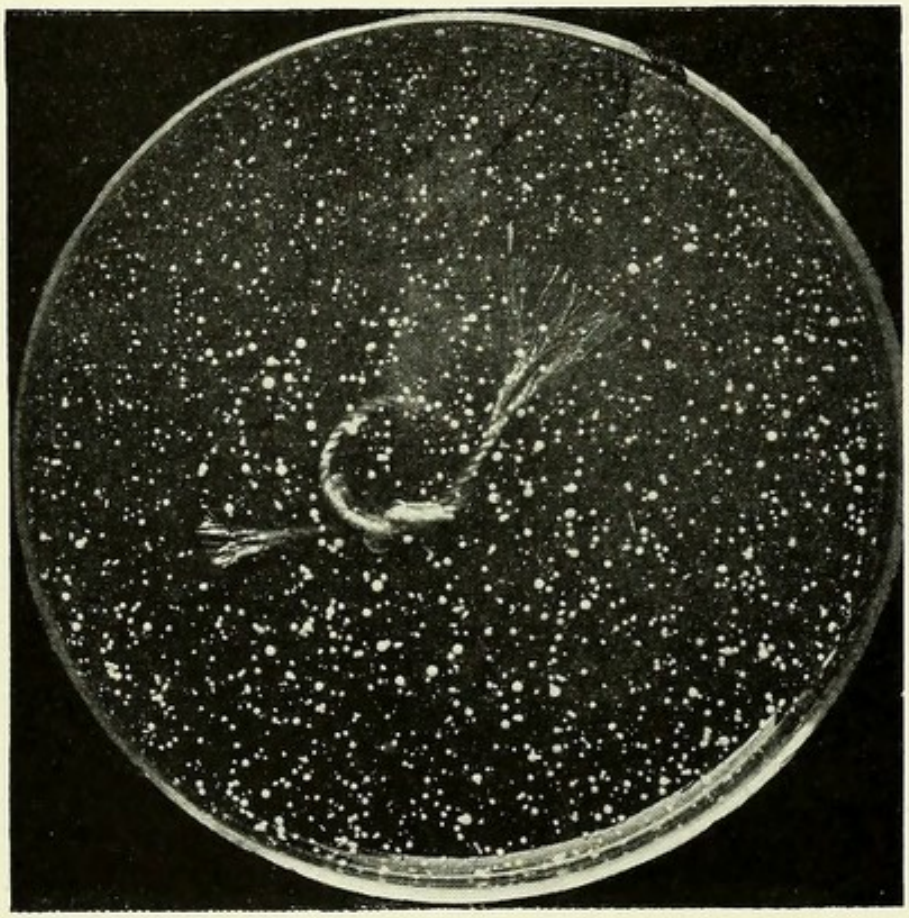

PLATE III.—Septicity of the Surface of the Hand (inclusive of the Space under the Nail), as tested by means of the Thread Method.

A Surgeon's Hand, which was chapped and in bad condition, immediately after it had been washed for five minutes with soap and hot water, but without the use of a nail-brush—thus a pure process of maceration.

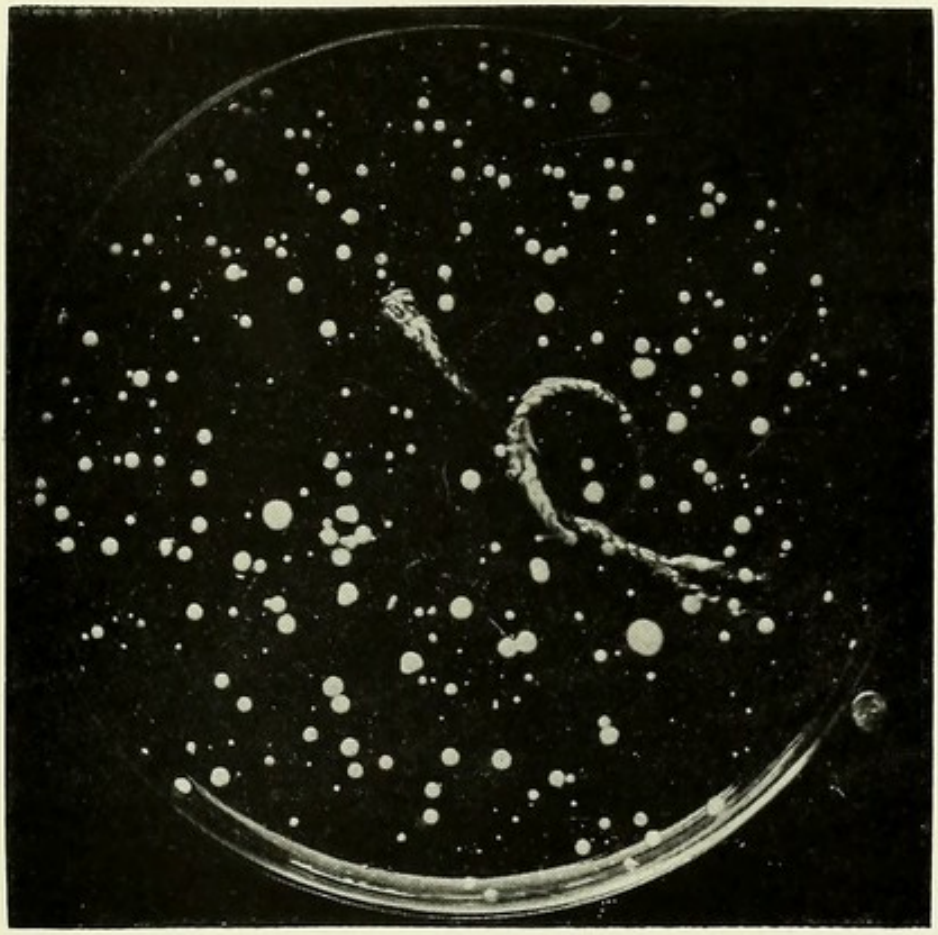

PLATE IV.—Septicity of the Surface of the Hand (inclusive of the Space under the Nail), as tested by means of the Thread Method.

A Surgeon's Hand, which was chapped and in bad condition, after it had been washed for five minutes with soap and hot water, but without the use of a nail-brush, and had been well rubbed with a dry towel, and once more rinsed in sterile water.

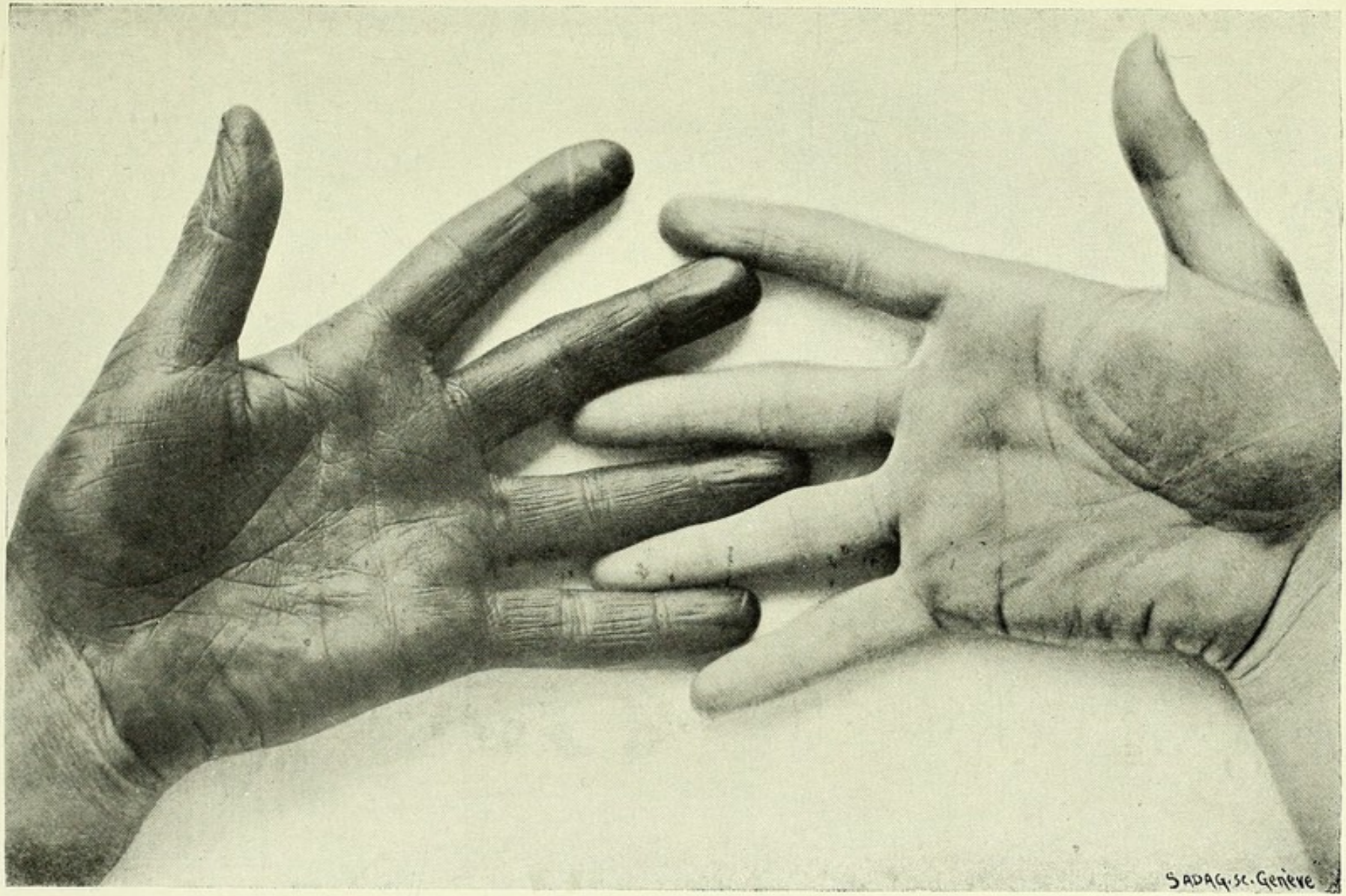

PLATE V.—Corrosive Sublimate still retained by the Epidermis after the disinfected Hand had been washed in Alcohol for five minutes and in hot water for ten minutes.

The Hand is stained black from the precipitation of Mercury by a 10 per cent. solution of Ammonium Sulphide. The "hand in everyday use" of a colleague is depicted to the right, in order to show the alteration in colour.

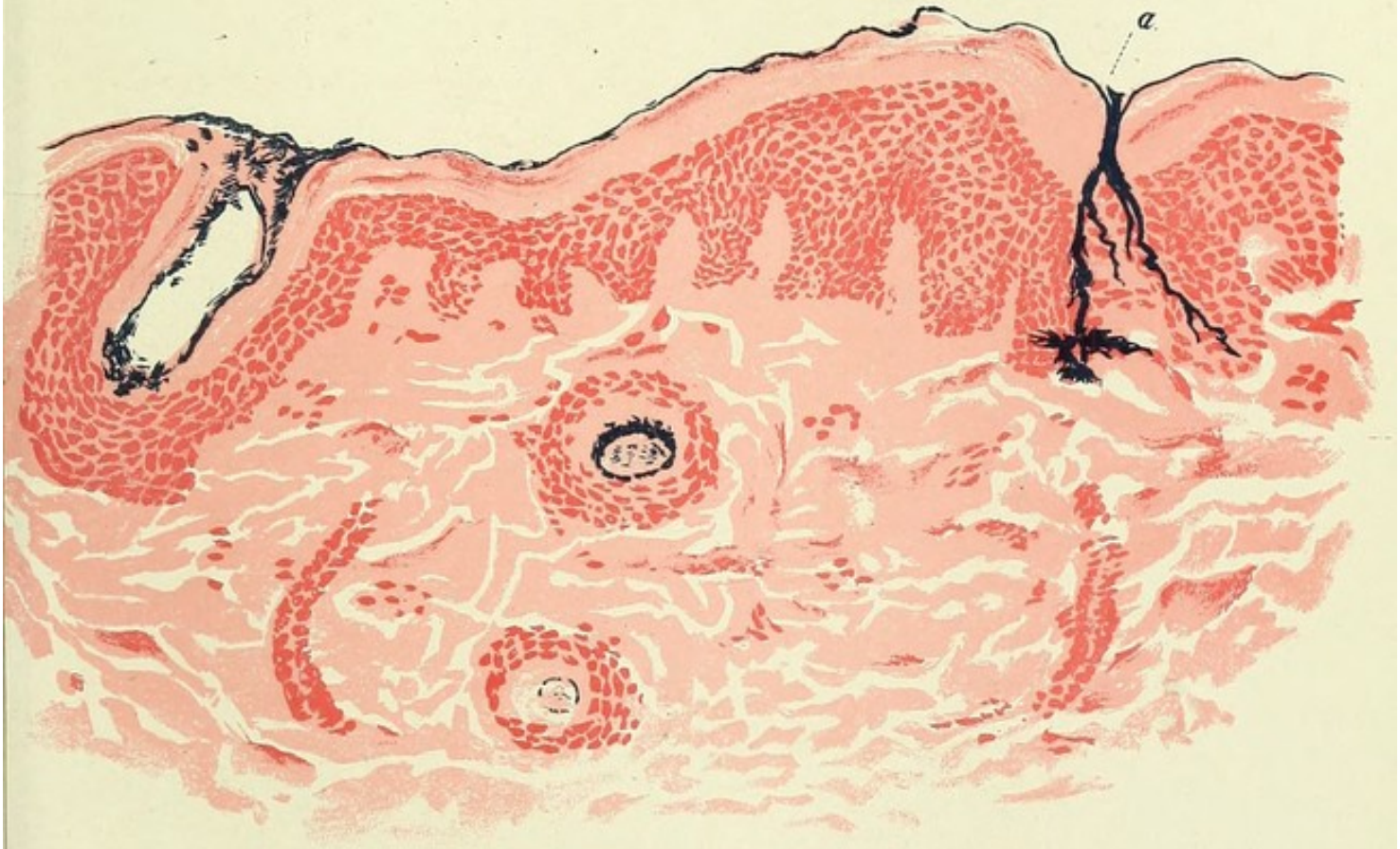

PLATE VI.—Section through the Skin of the Dorsal Aspect of the Second Phalanx of the little Finger of a well-preserved Hand.

Indian Ink had been rubbed into the Skin before its removal.

(*a*) Small Wound.

PLATE VII.—Transverse Section through a portion of Skin (the same as described in PLATE VI.) slightly above the level of where the Sebaceous Glands anastomose.

PLATE VIII.—Skin from the Extensor Aspect of the little Finger of a well-preserved Hand (the same as described in PLATE VI.) into which Staphylococci (from the surface of Agar) were rubbed before its removal.

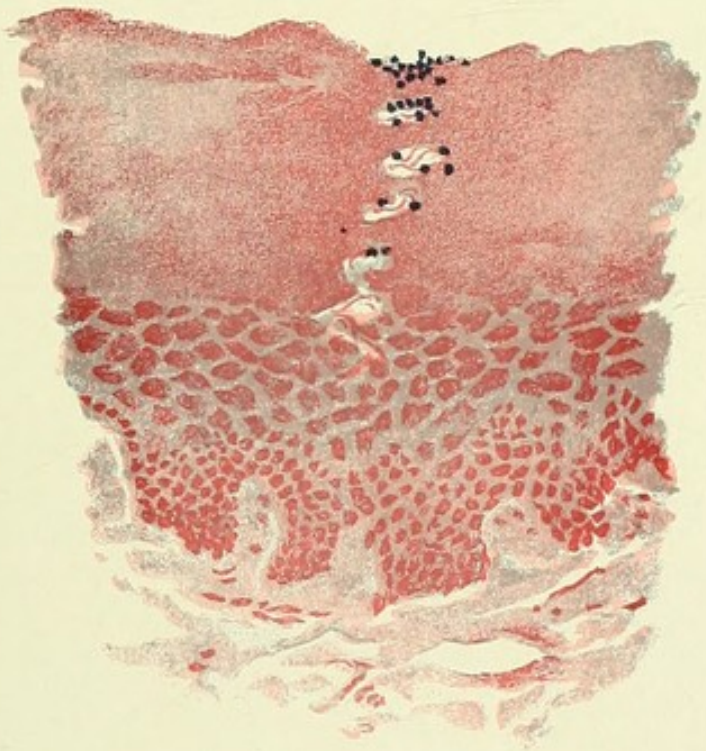

PLATE IX.—Skin from the Flexor Aspect of the Terminal Phalanx of the middle Finger of the Hand of a Labourer, after it had been rubbed with Staphylococci (from the surface of Agar).

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