

The treatment of the exanthemata by so-called antiseptic inunction : with observations on the period of incubation and the duration of infectiveness in scarlet fever a paper read before the Association on June 27, 1894 / by H.G. Armstrong.

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Medical Officers of Schools Association.

THE
TREATMENT OF THE EXANTHEMATA

BY

So-called Antiseptic Inunction:

WITH OBSERVATIONS ON THE PERIOD OF INCUBATION AND THE
DURATION OF INFECTIVENESS IN SCARLET FEVER.

A PAPER READ BEFORE THE ASSOCIATION

On June 27, 1894.

BY

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MEDICAL OFFICER, WELLINGTON COLLEGE.

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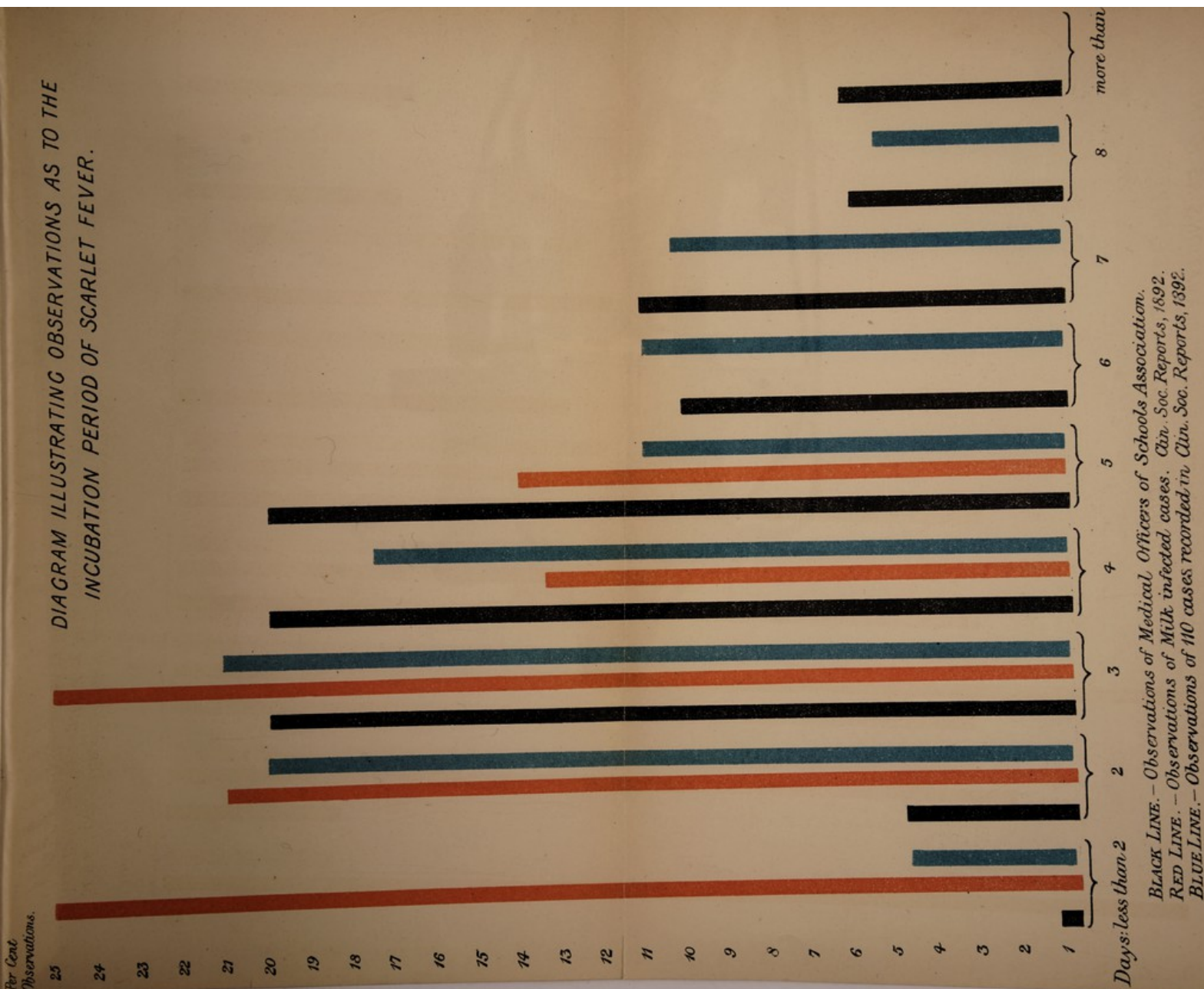
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DIAGRAM ILLUSTRATING OBSERVATIONS AS TO THE INCUBATION PERIOD OF SCARLET FEVER.





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THE TREATMENT OF THE EXANTHEMATA

BY

SO-CALLED ANTISEPTIC INUNCTION.

IN the year 1849 there was published in the pages of *The Lancet* the translation of a paper by Dr. Schneeman, physician at the Hanoverian Court, advocating a new method of treating scarlatina, which materially shortened the duration of the disease, and checked all infection at the end of the third or fourth day. The treatment consisted in inuncting the body with a piece of bacon fat, in such a way that, with the exception of the face, and hairy scalp, a covering of fat is everywhere applied. With a rapidity, bordering on magic, he says, all, even the most painful symptoms of the disease are allayed ; quiet, sleep, appetite, and good humour return, and there remains only the impatience to quit the sick room. In spite, however, of the rapid cure, the rubbing was to be kept up for four weeks, during which time the linen was to be very sparingly changed, and no washing was to be done at all. The desquamation, if there was any, was stated to be entirely completed by the tenth day of the disease.

In the year 1890, and again in 1891 and in 1893, the last before this Association, and published in *The Medical Magazine*, papers were read by Mr. J. Brendon Curgenvén, advocating the treatment of scarlatina and the other exanthemata by the inunction of the patient with a preparation of certain essential oils. The preparation he uses, and recommends, is Tucker's oleusaban, which is stated to consist of two or three essential oils of different densities,

with thymol and camphor dissolved in the oil of eucalyptus. It contains no fixed oil, nor alcohol, and, it is asserted, evaporates completely and rapidly. He directs that, "in scarlet fever the whole surface of the body be lightly smeared over with the oils, night and morning, for three days, not omitting the skin behind the ears, and into the scalp once a day. After the third day the skin is only to be anointed at night after a warm bath, for a further period of ten days." In addition to the inunction of the body, he directs that the oleusaban should also be sprinkled over the bed and pillow, and diffused in the air of the room by means of a spray diffuser, and that from three to six drops in mucilage or on a lump of sugar be administered internally three times a day.

He claims for this method of treatment very similar results as were claimed by Dr. Schneeman for his bacon fat method. The virus of the disease is, he says, destroyed from the commencement of the fever. If the inunction is commenced in the initial stage, the fever does not develop, and no rash appears, the patient being quite well in from twenty-four to thirty-six hours. If on the first day of the rash, the patient is quite well in two or three days; if on the second day, the case may be prolonged for four or five days. In all cases no infection is given off from the patient, who may consequently be safely treated at home. No segregation in infectious hospital is necessary; a most injurious and reprehensible plan, he argues at some length—which argument, he says, will also apply to the segregation of boys in the school infirmaries. In most cases there is little or no desquamation of the cuticle. No albuminuria, or other sequelæ, and the patient is considered free to return to his ordinary occupation, or to school, after the tenth day from the commencement of his illness.

Are we prepared to accept this? Are we as school medical officers prepared to treat our scarlet fever cases in the school dormitories, or to allow them to return to school after ten days? Are we prepared to abolish the disinfecting apparatus? For this, according to Mr. Curgenvin, is unnecessary, the disinfection of the bedding and the room

being accomplished *pari passu* with the treatment of the patient. I doubt not but that we all wish that we could, and more especially that we could see our way to in any way modify the regulation of our code, that there must be a period of at least six weeks' isolation after scarlet fever, a rule which is almost as irksome to the school authorities, including the medical officer, as it is to the patient.

Although Mr. Curgenvén's paper is specially directed to the treatment of scarlet fever, he is careful to tell us at the outset of it that the treatment is applicable to *all* forms of infectious disease, and he makes special mention of diphtheria, measles, and chicken-pox.

He complains of the treatment he has received at the hands of the profession, in that they have not more readily accepted his method. He reminded us of the words of Pasteur, and warned us not to allow ourselves to be taken possession of by a sneering and barren scepticism. I would remind Mr. Curgenvén that equally reprehensible with a barren scepticism is an unreasoning credulity. He asked for a trial of his method. The trial has been given. We are here this afternoon to discuss the result.

In accordance with the suggestion of the Council, I issued a circular letter to the members of the Association, asking for their experience. I also applied to the medical superintendents of the different fever hospitals in the metropolis. The question I put was, Whether in your experience, antiseptic inunction in the treatment of the exanthemata is efficacious :—(a) As a prevention of infection. (b) As exerting any specific power over the disease in shortening its course and preventing complications.

The result I have tabulated in the following digest of the replies received :—

TREATMENT OF THE EXANTHEMATA

Name and Address.	Is antiseptic inunction in the treatment of the exanthemata efficacious as— (a) a preventive of infection.	(b) exerting any specific power over the disease.
Armstrong, G. W., Esq., Royal Hospital School, Greenwich ...	No opinion. Has only used it in single cases, which were well isolated ...	No beneficial effect.
Baker, S. J., Esq., Radley School	Thinks antiseptic inunction prevents spread of infection, but depends more on warm baths and soap...	No beneficial effect.
Martin, H. J., Esq., Mill Hill School	Inclines to a belief that it acts beneficially in preventing spread of infection ...	No.
Alder-Smith, Dr. H., Christ's Hospital ...	Doubtful...	Absolutely useless, and worse than useless.*

* Prior to October last I was favourably impressed with what I had read about antiseptic inunction, and was fully prepared with a quantity of the proper disinfectant and the oil, procured from the firm recommended by Mr. Curgenven, so that, when the outbreak of scarlet fever occurred at Christ's Hospital in October, 1893, I at once treated my first ten cases with it, commencing the treatment as soon as the cases were diagnosed—the first day of the rash. I had the directions of Mr. Curgenven minutely carried out by two competent nurses, viz., inunction with the special fluid—a tent over the bed, the pillows and sheets well sprayed—and also the internal administration of the oil. The two severest cases I had out of the twenty were thus treated from the very first appearance of the rash. In one the temperature was 104° on the fourth day of the inunction, and 102° on the fifth. The boy was very ill, and peeling commenced on the tenth day, and he was still peeling freely at the end of the month, in spite of the inunction. Another case had a temperature of 105° on the fourth day of the treatment, 104° on the sixth, and 102° on the seventh, also a very bad throat, the cervical glands much swollen, and was very seriously ill, in spite of the special treatment, from the first. He commenced to peel about the tenth day, and was peeling freely at the end of the month. I was so dissatisfied with the

Name and Address.	Is antiseptic inunction in the treatment of the exanthemata efficacious as— (a) a preventive of infection.	(b) exerting any specific power over the disease.
Beach, Dr. Fletcher (late) Darenth Asylum	Yes	No.
Pike, Dr. T., Malvern College ...	Always trusted to isolation... ..	No. Has found severe complications to follow antiseptic inunction.
Fyffe, Dr. W. J., Clifton College...	No; on the contrary	No.
Dukes, Dr. Clement, Rugby ...	Useful; but only by its mechanical effect of making scales to adhere to bed and body linen... ..	No.
Jalland, W. H., Esq., St. Peter's School, York	No experience	None whatever.
Freeman, W. T., Esq., Bradfield College... ..	Probably; a step in the right direction ...	No.

results of the eucalyptus oil in my first ten cases that I did not employ it for my second set of ten. I noticed that where the tent was used, and the clothes freely sprayed, that it caused coughing, suffusion of the conjunctivæ, and much discomfort, and the patients complained and begged that it might be left off. After comparing the two sets of cases, I came to the conclusion that this treatment was quite useless in arresting the disease or the fever, in fact, that it did far more harm than good, and it certainly did not prevent the peeling afterwards. I shall not advise eucalyptus inunction again for scarlet fever, or any other disease.—
H. ALDER-SMITH, M.B., F.R.C.S., Christ's Hospital, London.

Name and Address.	Is antiseptic inunction in the treatment of the exanthemata efficacious as—	
	(a) a preventive of infection.	(b) exerting any specific power over the disease.
Birt, Dr., Stonebridge	No	Thinks so. Cases treated by him in this way have been mild.
Clarke, J. Adams, Esq., Leavesden	Yes; found it successful in one instance in his own family	Yes. In the one case shortened it by nearly one-half.
Edgar, Dr. J. W., Giggleswick School	Doubtful	Cases did well enough on which he has tried it.
Mackenzie, Lewis, Esq., Blundell's School, Tiverton	Never treated cases by inunction only	No.
Saul, Dr. W. W., Lancaster... ..	Probably	No.
Shelly, Dr. C. E., Haileybury	No experience	No; rather the reverse.
Lever, Dr., Military College, Oxford	No experience	No.
Gripper, Dr., Female Orphan Asylum, Beddington... ..	Probably useful, but would not think of trying it alone without all the other usual methods of prevention... ..	Have no faith in its "specific" power.
Swift, W. J., Foundling Hospital..	Yes; tried in twenty cases... ..	Yes; throat trouble certainly lessened.

Name and Address.	Is antiseptic inunction in the treatment of the exanthemata efficacious as— (a) a preventive of infection.	(b) exerting any specific power over the disease.
Bruce, Dr., Western Fever Hospital	No experience 	<p>In 1892, Tucker's Eucalyptus was tried in sixteen cases of scarlet fever. The oil was rubbed in all over the body from head to foot, night and morning, for a fortnight. Treatment was commenced on the first day of the rash in six cases, on the second in nine, and on the fifth in one. Desquamation ran the usual course in every case. Eleven cases were transferred to a convalescent fever hospital from twenty-seven to fifty-seven days after attack, in none of which was peeling completed. Four cases were discharged direct to their homes, the shortest duration of residence in these being seventy-seven days. Five cases were uncomplicated; four had otorrhœa; two had albuminuria lasting for at least a week during convalescence; one had simple adenitis; two suppurative adenitis and acute nephritis; one otorrhœa, suppurative adenitis, lobular pneumonia, and albuminuria; and one, which died on the fifty-eighth day from nephritis, had also otorrhœa and adenitis.</p>

Name and Address.	Is antiseptic inunction in the treatment of the exanthemata efficacious as—	
	(a) a preventive of infection.	(b) exerting any specific power over the disease.
Gayton, Dr., Fever Hospital ...	No; after several trials	No experience.
Ford Caiger, Dr., South-Western Fever Hospital	No experience	No personal experience; but knows it has been tried at another hospital with signal failure.
Hopwood, Dr., London Fever Hospital	No experience	The only case on which it was tried died.

I desire especially to refer to the report of Dr. Bruce, of the Western Fever Hospital, as this seems to me to be quite sufficient to condemn the proposed system, but more especially because, as I am informed is probable, it is to this experience that Mr. Curgenven referred in his paper to us. His report, which differs in most particulars from that which I received, appears thus at page 735 of the *Medical Magazine* for last year: "The Medical Superintendent of one of the fever hospitals, who kindly undertook to use the inunction in suitable cases, afterwards favoured me with full reports of the sixteen cases so treated. They were all inuncted twice a day for two weeks. *Ten*, or 62 per cent., of these cases had no complications or sequelæ, whilst *six*, or 38 per cent., had." Now on this point you will observe that in Dr. Bruce's report he says that *five* were uncomplicated, and *eleven* had various complications or sequelæ. Mr. Curgenven proceeds to explain this in this way: "One source of failure in the treatment of infectious disease by inunction in hospital is the delay before treatment can be commenced."

Now we can know exactly what Mr. Curgenvén would have us expect from his method, by a reference to his paper (page 11) read at the International Congress of Hygiene and Demography. If the treatment be commenced on the first day of the *rash*, the fever subsides in twelve to twenty-four hours, no more rash appears, and the patient is quite well in two or three days. If the case does not come under treatment until after the second day, the subsidence of the fever is not so rapid, but uncomplicated cases recover in about four or five days. A reference to Dr. Bruce's note will show that fifteen out of the sixteen cases were under treatment on either the first or second day of the rash. The difference between expectation and realisation is most marked, and worthy of special note. The experience from the Foundling Hospital is noteworthy, and at first sight seems to support the views of Mr. Curgenvén. If the medical officer of that institution is present, I should like to ask him to explain how, if the eucalyptus inunction prevented the spread of infection, he got the twenty cases on which to try it. And, secondly, if the cases were only of a mild type, or whether the progress of the disease, including desquamation, was arrested from the commencement.

My own experience is as follows:—Within a few weeks of the reading of Mr. Curgenvén's paper, there was a small outbreak of measles at Wellington College. In all the cases, of which there were thirty-three, the inunction by oleusaban was carried out most carefully, and exactly according to the method recommended. In addition, each boy was dosed with the oil in from three to six drop doses, the sheets and pillow-cases were sprinkled with it, and spray diffusers charged with the material were continually at work in the rooms. Taking as the test of the progress of a case, the day on which the morning and evening temperature became normal for the first time, and comparing this with a similar number of cases of a previous epidemic, in which this method had not been adopted, I find the following result:—

The temperature became normal in

Days.	Treated with inunction.	Treated without.
3	4 per cent.	20 per cent.
4	52 „ „	44 „ „
5	24 „ „	28 „ „
6	12 „ „	4 „ „
7 or more	8 „ „	4 „ „

According to this, which is Mr. Curgenvén's own test (*vide* appendix to his second paper), the treatment by inunction is considerably inferior to that by simple salines which I had previously adopted. I certainly saw no marked diminution of the rash, which in all these cases was remarkably profuse and intense. There was, however, apparently some relief to cough, and in this particular only can I see that I found any benefit whatever from the use of the compound. I might add that I have found a dose of from three to six drops on a piece of sugar to be very beneficial in all forms of catarrhal cough, and more especially in hay fever, over which it appears to exert a very beneficial influence. I use it now to the exclusion of pure terebene, as being more agreeable and equally efficacious.

In the thirty-three cases there were three with somewhat severe complications, viz., two prolonged otorrhœas, and one of pneumonia. This is the only case of pneumonia occurring as a direct complication of measles in a series of over 180 cases.

My experience with measles is very similar to that of Dr. Shelly at Haileybury, which he recorded in the *Practitioner* (Nov., 1893). He, perhaps more wisely, treated only a limited number of his cases with oleusaban, and was thus able to compare more easily with cases of the same epidemic treated in the more orthodox manner. In the five patients inuncted, he observed a "considerable drowsiness, general dulling of sensibility, abnormally furred tongue, and relatively prolonged pyrexia," which symptoms, he, I think justly, considered, seemed to indicate an undue retention of morbid products, rather than a speedy and

complete destruction of the infective poison. Out of his five cases, four developed laryngeal and bronchial catarrh; and one had a severe attack of pneumonia, affecting both bases in patches.

As to scarlet fever, I have not had any opportunity of trying the oleusaban treatment. I have, however, the opportunity of comparing my results with ordinary treatment with those of Dr. Curgenvin, as given in the appendix to his second paper. In the last outbreak at Wellington College, in which four boys were affected, in two the temperature never rose above 100° ; of the other two, one became normal on the sixth day, and one on the seventh day of the disease. Both these were rather severe, and in one there was considerable throat affection and delirium. This is even a better result than that claimed for the inunction treatment. I do not, however, claim any special merit for my treatment. The fact of the matter is that there can be no doubt but that we are at present, and have been for some years, visited by a very mild type of scarlet fever. Personally, in the twenty years that I have practised, I have not been once called upon to sign a death certificate, "Scarlet Fever." The prevalence of a mild type of this disease for several years together is no new experience. In discussing this question it is worth while to take a glance at a clinical lecture given by Dr. Graves, in Dublin, in 1834. Up to the year 1804, he says, scarlet fever was an extremely fatal complaint. It thinned many families in the middle and upper classes of society, and even left not a few parents childless. Then commenced a long period in which the disease completely changed its character, and a death was quite the exception. The physicians of that day extolled their method of treatment and derided that of their fathers, believing that by their wisdom had come the change. But they were rudely awakened from their dream when, after thirty years, the disease returned with all its old virulence, and they were obliged to confess that, in spite of their boasted improvements, they had not been more successful in 1834 than had their predecessors in 1801. I

venture to prophecy that the day will come when the apostles of these new treatments, whether it be bacon fat or eucalyptus, will have to do the same.

So far I have dealt only with the antiseptic inunction treatment of the exanthemata in regard to its alleged power to cut short the duration of the disease. If, by a reference to the collected opinions of the members of this Association, and of my own experience, I have shown the inadequacy of the treatment to in any way maintain the reputation that has been claimed for it, I have gone some way to establish my second point, namely, that it has no power of preventing the spread of infection. For it is by the specific action of the oil upon the virus of the disease, and thus destroying it *in situ*, that it is supposed to exert any power of prevention.

A reference to the table will show what opinions I have been able to collect from the gentlemen to whom I sent the circular. Only one of them answers in the affirmative.

As far as my own experience goes, I may say that in the measles epidemic at Wellington I had calculated by Shelly's tables of expectancy in epidemics at schools that I should have 34 cases. The exact number was 33. So that the careful and complete carrying out of the treatment did not limit the spread of infection. I had a further and better opportunity of observing its failure in the following case:—In a private family, with four children at home, the eldest, a boy, was on the first symptom of malaise, and three days before the appearance of the rash (measles being anticipated) isolated completely and subjected to the inunction treatment most thoroughly. Twenty-one days after his isolation another child sickened, and was followed at intervals by the other two. Now, it is clear from our knowledge of the incubation period of measles that infection cannot have been conveyed from the first child to the second before his isolation and the commencement of the inunction, &c., but at some time subsequent to the commencement of the treatment. This was, I think, a fair test case, and resulted in a signal failure.

I have but ill succeeded if I have not shown

(1) That antiseptic inunction does not exert any specific power over infectious diseases.

(2) That it has but little, if any, power in preventing the spread of infection.

(3) That cases treated thus are more subject to complications.

And this is what we might reasonably expect if we view the question from a rational standpoint. Mr. Curgenvén claims that this eucalyptus antiseptic is "absorbed into the blood" and "acts directly on the poison throughout the body." And, again, that it is "able to destroy the pathogenic poisons of scarlet fever, diphtheria, and other diseases when brought in contact with them in the human system. He claims for eucalyptus oil that it is rather more than three times stronger than carbolic acid. Even at this estimate it is difficult to believe it possible for a sufficient quantity (from one to three ounces) to circulate for a sufficient time (about twenty-four hours) to destroy the *contagium vivum* of the disease. It is possible that in scarlet fever the oil may, as other oils, act mechanically, as has been suggested, and prevent the infection-bearing flakes of epidermis from floating about. But I venture to submit that even if this be so, such treatment is irrational and likely to be prejudicial to the patient. Our president, Dr. Dickinson, expressed himself very clearly last year in this room when he said that "he held strongly that inunction of a scarlet fever patient with any form of fat or fixed oil was deleterious, and especially liable to favour the development of renal complications." He has since written to me saying "that this opinion was derived from the experience of the Hospital for Sick Children, where it used to be the custom to smear the children, during the peeling stage, with a mixture of lard and olive oil. The large proportion of albuminuria under this treatment was noticeable, and the plan was discontinued." It is claimed by Mr. Curgenvén that the essential oils are on quite a different footing, that they completely evaporate, and do not in any way interfere with elimination by the skin.

This is inaccurate, even as it applies to evaporation from a smooth surface, such as glass, as is illustrated by this glass slide, from which some oleusaban has been evaporated. You will see there is a considerable residue of some crystalline and sticky material, forming an impermeable covering or varnish; but it is still less accurate when the material is applied to a rough and absorbent surface. This you will find to be the case if some is rubbed on the hand, both by the feeling produced and also by the fact that the smell of the oils remains for some time even after washing. Further, you will find that oleusaban will make a fairly good varnish for fixing the smoked paper on which syhygmographic tracings are generally taken. The following observations were kindly made for me by Mr. Fitzgerald in the laboratory at Wellington College with this material:--

- | | | | |
|-----|----------------------------------------------|-------------|---|
| (1) | A piece of filter paper weighed after drying | 1.210 grms. | |
| | Soaked in oil, and again dried at 100°, | | |
| | weighed | 1.24 | „ |
| (2) | A piece of paper, dried, weighed ... | 1.22 | „ |
| | Do. soaked and dried roughly, | | |
| | weighed | 1.85 | „ |
| | Do. dried still more, weighed ... | 1.58 | „ |
| (3) | A piece of cloth weighed | 2.700 | „ |
| | Do., soaked in oil, weighed .. | 6.630 | „ |
| | Do. do. dried for two days, weighed | 3.785 | „ |
| | Do. do. dried further in vacuo, | | |
| | weighed | 3.770 | „ |

It will thus be seen that a very considerable quantity (even after careful drying) of the compound was retained in the article to which it was applied, being, in the last experiment, as much as 25 per cent.

Mr. Curgenvén seems to me to be guilty of a contradiction on this point; for on the same page on which he says that the eucalyptus disinfectant evaporates rapidly and completely, he also says that it is absorbed into the intercellular spaces of the cuticle. Surely this is a paradox, that a substance which completely evaporates is also

absorbed. Mr. Curgenven condemns the practice of applying fixed oils or grease to the exanthematous skin, and advances proofs of his condemnation, such as the fatal results which were found to follow upon the practice of anointing horses with olive oil after clipping. But I maintain that the use of this varnish is still more irrational, still less desirable.

Even if desirable, it is an extremely troublesome method of treatment to carry out, and singularly inapplicable to the treatment of the poor in their own homes. Surely it must be, as Dr. Sweeting says, a gentle satire on the home treatment of the class of case which usually goes to a hospital, to talk about it "as being isolated in his own bed, surrounded by sheets. . . . saturated by disinfectant," &c.

There is another danger connected with the use of this material, which seems to have escaped the notice both of Mr. Curgenven and his critics, that which comes from the extreme inflammability of the substance employed. The bedding, &c., sprinkled with the oil is rendered highly combustible, and the air saturated, as directed, by means of a spray diffuser, readily ignites. This is a real difficulty, and one which has actually occurred in my practice.

Before leaving this part of my subject, I wish to point out how important it is to deal antiseptically with the throat in scarlatina. I am of opinion that most of the cases of a malignant nature, and this applies also to a certain extent also to diphtheria, are septicæmic, due to absorption of septic material by the mucous membrane of the throat and fauces. I think it is very possible that, used as an application to the inflamed, ulcerated, or sloughing tonsils, eucalyptus, which is undoubtedly a germicide, may do good in this way. I am, however, in the habit of depending rather on the frequent application of sulphur by means of an Osborn's Insufflator. Indeed, I am in the habit of using it in all forms of sore throat where there is any suspicion of ulceration—either follicular, aphthous, or specific. If applied early enough, it will, I believe, prevent extension of the mischief. It combines the qualities of a good germicide, namely, that it can be

applied for a sufficient length of time, in sufficient strength, exactly to the spot that has to be attacked. It is in these essentials that a gargle, which is more usually depended on, fails—not to mention the fact that a gargle is probably the most irrational form of treatment for an inflamed part that can be conceived. It necessitates violent movement, when as much rest as possible is indicated.

As to the management of the skin, the use of grease or oil may act mechanically, and prevent the flakes of epidermis from floating about in the air; but as I have already indicated, I believe the practice to be harmful to the patient by limiting evaporation. By frequent use of baths and scrubbing brush, assisted by some such soap as Eichhoff's—a superfatted soap containing 3 per cent. of resorcin and 3 per cent. of salicylic acid—and the application to the more horny parts, such as the heels and the palms of the hands, of some stronger preparation of salicylic acid, the process of desquamation may be materially hastened.

The Council of the Association also requested me to obtain and report upon evidence as to the duration of incubation and infectivity in scarlet fever, and I issued queries upon these points to the members.

My own feeling, with reference to incubation periods, is that they are more constant than is generally supposed. I believe that the great variations which we find in our text-books, are due either to errors of observation or to deferred infection; the poison in the latter instance not being received at once into the body of the patient, but temporarily retained in fomites,* such as wearing apparel,

* On this point it has been suggested by Dr. Shelly, of Haileybury, that "we have hitherto been quite illogically wrong in our recommendations as to quarantine . . . for which we recommend that the person who has been exposed to infection should be segregated for a specified period, *at the end of which* he and his fomites should be disinfected. With a virus of such persistent vitality as that of scarlatina especially, it is clear that the only safe course would be to disinfect clothing, person, &c., *at the very beginning of* the quarantine period; only by such procedure can we guard against (*e.g.*) an individual becoming infected by some article of his clothing shortly before the expiration of the quarantine period."

pocket handkerchiefs, &c. And I would further suggest, what perhaps cannot at present be proved, that there is a definite relation in an inverse ratio between the period of incubation and the duration of infectivity.

The result of the evidence furnished by the Association I have arranged in a diagram, and placed alongside of it, for easy comparison, the results obtained by the Clinical Society in a similar, but more extensive inquiry, and published in 1892.

The figures at the bottom indicate the number of days elapsing between the date of exposure and the development of the disease. The figures at the side represent the number of cases, stated as a percentage, occurring on each day.

The black line represents the observations of the members of this Association.

The red line is an analysis of the milk cases, Table V., page 149, Clinical Society's Report, being a report of cases where the infection was derived from drinking milk at a time which was accurately known.

The blue line is an analysis of Table III., page 147, Clinical Society's Report, being 110 collected cases in which the evidence as to the date of exposure and commencement of attack is accurately known.

The variations of observation may be seen at a glance. Thus, of cases in which the incubation period is less than two days, the black line reaches 1.4 per cent., the red line 25 per cent., the blue line just less than 5 per cent. On the third day each line reaches its maximum; the black line 20 per cent., the red line 25 per cent., and the blue line 21 per cent.

I think it may be fairly computed from this that three days is the average duration of the period of incubation in scarlet fever. It will be observed that the red line disappears altogether after the fifth day, and the blue line after the eighth.

To my last question: "Have you had any cases of infection from personal contact with a patient in whom desquamation has been prolonged for more than eight weeks?" I

received a universal negative. With this experience, and comparing it with the result of the Clinical Society's investigation, I think we may conclude that the few reported instances of a lengthened period of infectivity are not proven. The question from time to time arises whether post-scarlatinal albuminuria, when chronic, should be considered a sign of infectiousness. On this point I would only say that it has not, I believe, been satisfactorily demonstrated that the kidney secretion is at any time infectious, even in the acute stage; we certainly require more evidence before coming to any opinion on this point. As to the infectivity of the early period of the fever, there is good reason to believe that it is only slightly infectious before the development of the rash. The matron of a large preparatory school has told me that through an experience of many years she had never seen the disease spread from a patient who had been early isolated, provided that, and this seems to be important, he had not vomited on the floor or carpet.

In conclusion, I beg to offer my best thanks to those gentlemen who so kindly responded to my circular, and to my audience for having so patiently listened to me.

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H. ALDER-SMITH, } *Hon. Secs.*
CHARLES SHELLY, }