PUBLIC HEALTH,

The Journal of the Incorporated Society of Medical Officers of Health.

SUBSCRIBERS' COPIES of PUBLIC HEALTH are supplied at 10s. 6d. per annum. Single copies One Shilling.

BOUND VOLUMES.—Copies of Volume VI. are now ready, bound in cloth, gilt lettered, price 10s. 6d. A few copies of Vols. I., II., IV., and V. are also on hand.

CLOTH COVERS for Binding the Volumes, price 1s. 6d., may be had by order of any Bookseller.

Orders should be addressed to E. W. Allen, 4, Ave Maria Lane, E.C.

ADVERTISEMENTS.—Communications relating to Advertisements should be addressed to Messrs. W. H. & L. Collingridge, 148 and 149, Aldersgate Street, E.C.

Every Reader of a Paper before the Society, or Contributor of a Signed Article, is entitled to twenty-five copies of the issue of PUBLIC HEALTH containing the contribution in question; but application for this or any smaller number must be made to the Editor not later than the TWENTY-FOURTH day of the month previous to insertion of the Contribution.

Medical Officers of Health and other Correspondents are requested to send regularly all published reports and matters of interest bearing upon Sanitation to the Editor, 11, Gloucester Place, Brighton.

NOTICES.

ALTERED SUBSCRIPTION FOR MEMBERS OF THE INCORPORATED SOCIETY M.O.H.

The attention of Fellows and Members of the Incorporated Society is called to the fact that dating from October, 1894, the subscription to the Society will be one guinea instead of half-a-guinea as hitherto. It must be noted that this will include the subscription as members of a branch of the Society, the guinea subscription being inclusive for all purposes.

It will be seen from this that there will be no very material increase in the subscription for those who are members of the parent Society and of one of its branches. There will, however, be considerable saving of trouble in collecting subscriptions, etc., as one subscription only will be payable. The subscriptions (and those for Associates, which remain half-a-guinea) should be sent to the Treasurer, Dr. KEMPSTER, Chesterfield, North Side, Clapham Common, London, S.W.

CONTRIBUTIONS FOR NEXT SESSION.—The Hon. Sec. will be glad to hear from those members of our Society who are wishful to read papers during the next Winter Session, as it is desirable to complete the programme at an early date.

THE INCORPORATED SOCIETY OF MEDICAL OFFICERS OF HEALTH.

20, Hanover Square, London, W.

NOTICE is hereby given that the Provincial Meeting of the Society will be held at the Town Hall, Portsmouth, on Saturday, July 27th, at 12.30 p.m., for the transaction of the under-mentioned business:—

AGENDA.

1. Minutes.
2. Report of Council (if any).
3. To receive nominations for the election of Fellows, Members and Associates.
4. An address will be given by Dr. C. Kelly, Professor of Hygiene in King's College, London, President of the Southern Branch.

After the meeting there will be excursions to places of interest, etc.

Full details will be sent to all Members of the Society by special circular.

Note.—A convenient train for Members coming from London leaves Waterloo Station at 9.5 a.m.

JOHN C. THRESH,
The Limes, Chelmsford.

REGINALD DUDFIELD,
19, Bromfield Road,
Maida Vale, W.

Hon. Secs.

IS LEPROSY A TELLURIC DISEASE?*

By J. ASHBURTON THOMPSON, M.D., D.P.H., Examiner in Hygiene at the University of Sydney, N.S.W.

ARGUMENT.

I.

HEREDITY an inappreciable factor in leprosy. Maintenance of leprosy independent of heredity. The contagion hypothesis. Hirsch's critique thereof still holds. True significance of the discovery that leprosy is a bacillary disease. Occasional direct communication of leprosy by the sick cannot be formally denied, but direct communication not the means of maintenance. Illustrations. General critique of evidence usually adduced to prove the contrary.

II.

Long-continued intimate contact with lepers often suffered with impunity; leprosy often contracted when there has been no conscious contact with any leper. Inference, that leprosy may be easily contracted under favourable circumstances, among which presence of an actual leper not essential. Reply to indirect objections to this argument:—(a) Insignificance of a prolonged latent period when communicability is examined by epidemiological methods; (b) hypothesis of indirect

* A paper read in the Public Health Section of the Australasian Association for the Advancement of Science, at the Brisbane Session, 1895.

communicability not supported by ascertained facts. Importation of lepers does not give rise to new areas of endemicity. General critique of assertions to the contrary. Their crucial importance: if well-founded, the *maintenance* of lepra by communication with lepers would be established beyond dispute. Their want of foundation in ascertained fact. Illustrations. Negation.

III.

Of comparative susceptibility. The physiology of defence. Poverty and filth incapable of reducing the efficiency of the defensive function as against lepra so as to account for *maintenance*. General critique of opinions to the contrary. Remarkable example of the island of Madeira.

IV.

Epidemiological facts show that lepra attaches to locality, or inheres in locality. Lepra comparatively rare among the population living on any endemic area nevertheless. Possible explanation. Value of the defensive function. *Microbes favorisants*.

V.

The liberty of lepers severely restricted by law in five colonies of Australia. The origin and course of lepra on that continent entirely unknown. The five separate enactments consequently not based on any Australian evidence apparently showing that lepra is there *maintained* by communication with lepers.

UPON the whole, I fail to see that anything has been added to our knowledge of the ætiology of lepra, or clipped away from our supposed knowledge, which requires alteration in the critical remarks made several years ago by Hirsch,* save in one respect. The exception regards heredity. It has always been known, and of course Hirsch noted it, that heredity is no necessary factor in leprosy. We see that leprosy in whites almost always occurs in Australia under circumstances which absolutely exclude it; I am aware of but one exception out of many instances. Secondly, it appears that if heredity have any influence at all in this connection, at all events it is not the disease itself which is transmitted. No child ever has been born leprosy, nor has any ever become leprosy so young that it might reasonably be suspected of having suffered from birth. But whether heredity were a factor of more or less importance in determining leprosy—that is to say, whether lepers could transmit a predisposition to their offspring—was a question which Hirsch inclined to answer in the affirmative. Recent investigations made on a large scale seemed to show that at all events heredity exercised no such indirect influence of that kind as could be appreciated. The Leprosy Commission in India did good work when they pointed out that leprosy in parents was not associated with an incidence of leprosy on their children greater than the incidence on children born of healthy parents, and living on the same leprosy area, on the one hand; on the

other, that the parents of lepers were not lepers themselves in any greater proportion than other persons living on the same leprosy area whose children were healthy. And as to the practical question whether hereditary disposition were among the causes of the *maintenance* of lepra, the Commission showed very clearly that it might be excluded from consideration in all discussions of that point: for they showed, first, that lepers produce but few offspring at most; and secondly, that such as they do produce are extremely liable to die at young ages from different causes. Probably, then, heredity has no share in determining leprosy, and certainly it has no share in *maintaining* lepra present among mankind.

Only one other suggested explanation of the maintenance and diffusion of leprosy has received very general attention, and that is the contagion hypothesis. In accordance with the opinion I expressed at first, I think we cannot do better than follow Hirsch's account of it.

Hirsch pointed out in the first place, that the doctrine of contagiousness had been in favour from the earliest times; but, then, there is no doubt the diagnosis of leprosy was but ill-established, and several other chronic and disfiguring diseases, and especially syphilis, were confounded with it. As long as the diagnosis of syphilis continued uncertain, so long did the doctrine that leprosy was communicable by the sick hold its ground. No sooner did men begin to learn to recognise syphilis with some certainty than the doctrine of the communicability of leprosy began to fall into disrepute; and at last it came to be almost entirely rejected. But soon after Dr. Hansen's discovery of the *b. leprae*, in 1872, opinion began to change again; and when Dr. Neisser, in 1881, sufficiently established a necessary or causative connection between Dr. Hansen's bacillus and leprosy, the doctrine of communicability began once again to come into favour. That doctrine fits in so well with preconceived notions which date from antiquity, and is at first sight so natural, so easy, and so complete, that it is not surprising that many of the profession as well as the public in general should have adopted it. But Hirsch pointed out—and his remark has all the force to-day which it had when he first made it—that in reality Dr. Hansen's discovery did not at all justify the inference commonly drawn from it. He said:

It is only an *à priori* proof of the conveyance of leprosy by contagion which Neisser adduces, when he states, on the ground of the finding of bacteria, and of the hypothesis therefrom deduced, that "the malady is contagious in its specific products," and "contagious not only directly, but also indirectly, by articles which serve to carry the bacilli and their spores."*

* It should be noted that Dr. Neisser expressed this opinion nearly fifteen years ago. I do not know whether he has more recently confirmed it; if not, it is, of course, possible he might now modify or explain it.

* "Geographical and Historical Pathology."

That, evidently, is a just criticism; for what is required is proof that leprosy actually is directly or indirectly conveyed, and not merely an inference from analogy that it ought to be, or must be so conveyed. But no sufficient experimental proof of Dr. Neisser's assumption ever has been given.

In this discovery of the *b. leprae*, then, and of its causative connection with leprosy, we ought not to see a proof that leprosy is communicable by lepers; we should see in it only the proof that leprosy must be classed among the infective processes. All the infective diseases are not maintained by communication with the sick, it will be remembered. Thus, tetanus is an infective disease; yet, as a matter of practice and of fact, tetanus is not in the least maintained by communication between those suffering from it and the healthy.

Now, as to the direct communicability of leprosy, probably no important difference of opinion longer exists. Probably no one of great weight would roundly deny that leprosy may perhaps be directly communicable from the sick to the healthy. We have, I think, no proof of it, but that is not a sufficient reason for denying it; and, indeed, there is one case*—though, as far as I know, one only—in which direct communication seems to have occurred: one only, I mean, in which the circumstances seem to have been observed and recorded with accuracy, and in which all causes but direct communication seem to have been excluded by them. However, Hercules is not known by his foot alone. On the other hand, I scarcely think that anyone of very great weight would at this day assert that leprosy was *maintained* by direct communication, in the face of well-ascertained epidemiological facts concerning this disease the world over. Here, again, we have not to go outside Australia in order to perceive that as a matter of fact leprosy cannot be directly communicated, except very rarely indeed at most. I know of only three cases out of many in whites (among whom alone the details can be accurately learned) in which known direct communication with a leper could have operated to cause the second case; and those instances are on all-fours with the examples usually cited in support of direct communicability, and, like them, are liable to criticism of damaging kinds. On the other hand, though we imprison our lepers rigorously as soon as we discover them, generally we do not discover them until they have been at large for several years; and yet fresh cases do not usually occur in the household in which they lived, and which was certainly thus exposed for long to whatever risk attaches to free communication with the sick, but by far the most often in fresh households altogether.

That Australian experience is far from being ex-

ceptional. Everyone is familiar with the fact that lepers who have contracted their disease abroad return to Europe and live there in quite ordinary contact with others—sometimes at home, sometimes in the general wards of an infirmary or an hospital, and yet do not communicate their disease. There is, I believe, but the possible exception I have already mentioned, and one or perhaps two others which give ground for suspicion. We know also, and on the unusually good authority and personal observation of Boeck in 1870 and of Dr. Hansen more recently, that 160 lepers who emigrated with their families from Norway to some of the northern States of America, and lived there under no restrictions at all, were found on being traced to have communicated their illness to no one—Norwegian, American, or other. So that, as regards direct communication of this disease, we may well admit that it is a possibility (although we have no proof of it), and yet we can point to well-ascertained facts which at least forbid us to regard direct communication with the sick as the means by which leprosy is *maintained*, or to regard a leper as constituting any important danger to those who come into contact with him.

Now, if you feel inclined to disagree with that conclusion (notwithstanding your own Australian experience), you will have no difficulty in finding a hundred accounts which have been tendered to prove that leprosy is easily communicated by the sick, and that the disease is so *maintained*. I have but one word to say in reply. I merely ask you for the future to read those contrary accounts critically, and to accept those alone which are recorded with sufficient fulness as regards the essential details, and, at the same time, are entirely free from the *post hoc* fallacy. You will find that a vast majority of such instances are spoilt from the beginning by having been observed on areas of recognised endemicity, so that primary and secondary case alike were under the influence of locality. Apart from that, you will find very few indeed which answer to the two requirements mentioned before—one or both.*

After a not inconsiderable course of study, I take it that, in any mind seized of the facts, and in the habit of weighing evidence, there can be no doubt at all about this—that leprosy, if at all communicable by the sick, must be so only with great difficulty, and under special and quite unknown circumstances. Even some of the writers

* Dr. Hawtrey Benson's, *Dublin Journal of Medical Science*, 1877.

* Nearly thirty years ago the Committee of the Royal College of Physicians remarked in their Report (1867), that the cases cited to them in support of contagiousness either rested on imperfect observation, or were recorded with too little attention to the necessary details to be of service. If the reader doubt whether it be still necessary to make a similar criticism, he need only refer to Dr. Hillis' work, 1881, pp. 177, *et seq.*, and to a work published more recently (Leprosy, by George Thin, M.D., London, 1891), and peruse the very long string of "cases" cited therein, in order to satisfy himself that is so.

to whom I have just referred, and who have asserted that leprosy was so communicable (though without distinguishing between mere occasional communication, and such a common or frequent communication as would account for its *maintenance*), seem dimly to have perceived this; and, consequently, they usually postulate long-continued and intimate contact between the sick and the healthy as a necessary condition of communication. But you must perceive that this postulate is a sacrifice to a speculative opinion; for the records are full of cases of leprosy in which the sufferers never had been in long-continued, nor in close, nor in any conscious contact at all, with any leper. You will see this at once, too, because Australian experience of leprosy in whites shows, beyond possibility of doubt, that this disease certainly can be contracted quite independently of intimate contact with lepers. Our own experience shows us that this can happen, and even that it most usually is the case. Now, if known long-continued contact with lepers is most often supported without the disease being communicated—I mean, of course, without any second case occurring in the household of which the solitary primary leper was a member, and therefore without even apparent or *prima facie* evidence of communicability being afforded—we are logically warranted thereby in saying, not in general terms, that leprosy is a disease difficult to contract, or one to which a majority of persons are naturally resistant (for that would beg the question), but merely that at all events lepra is a disease very difficult to contract *from lepers*. And therefore, when on the other hand we observe that lepra very often does occur in persons who have been in no conscious contact with any leper, when we notice that close or long-continued contact with lepers is certainly not essential, we get (as it seems to me) a very important suggestion. The two facts taken together—for they are both well-established, commonly known, and such, moreover, as might be safely relied upon even if there were no other evidence of them than our own Australian experience—these two facts taken together seem to me to point to this: that leprosy may be a disease not at all difficult to contract, provided the virus in efficient form be met with under favourable external circumstances, among which presence of an actual leper is probably not essential.

There are two views which must be met after setting out the foregoing argument. They are entertained by writers who either do not explicitly state that prolonged and intimate contact with lepers is necessary, or who implicitly deny its necessity by relying on indirect communication of the virus by the sick.

Those who do not explicitly require long-continued contact have pointed out that as a very long latent period is commonly conceded to leprosy, it must always be difficult, and sometimes

impossible, to trace the human source of the infection. I think this remark must have originated with someone not in the habit of tracing the course of outbreaks of communicable disease. If individual cases of lepra were alone to be examined, it would have great force. But it would have equal force if examination of individual occurrences of influenza or cholera were examined to learn whether those diseases were communicable by the sick, brief as the latent period is in them. It is not thus, in short, that the communicability of diseases is, or can be established. The communicability of diseases can be examined only by comparison of the broadest epidemiological facts.

The other point has been indirectly raised by those who have suggested that lepra was *maintained*, not by direct communication with the sick, but by indirect communication—that is to say, by way of the soil infected by bacteridia cast off from the bodies of (tuberculous) lepers. This view seems to have been based on an analogy with tuberculosis. In that disease efficient bacilli are known to be cast off, and are known to be capable of surviving in efficient form for long after they have left the body. Tuberculosis is, doubtless, thus indirectly communicated by the sick; though such communication is not the only, nor, it may turn out, the most important means of maintaining this disease.

Now, first I must remind you, however tempting and probable the analogy may seem, that we know nothing whatever of corresponding facts concerning the *b. leprae*. We do not in the least know whether it is an endogen or an ectogen. But, beyond that, we have seen that leprosy is communicated by the direct channel (if at all) with great difficulty. Whatever the reason may be, that seems to be the fact. That being so, I do not understand why it should be supposed to be more easily communicable by an indirect channel. If, nevertheless, it demonstrably were so (but there is no better evidence of indirect than of direct communication within households), the explanation would surely lie in the *b. leprae* being an ectogen, and in its return to the earth or to some habitat outside man being a condition of the easy infection of fresh persons. It may be that indirectness of the channel was not supposed to enhance communicability, but merely to facilitate communication by bringing the virus into contact with a larger number of persons, and affording it, consequently, a freer opportunity of encountering the few among them who were susceptible. In that case we should still have to remark, first, that indirect communication does not seem to occur within households any more than direct communication does; this reply would hold until susceptibility should have been proved to be a very special condition: and, secondly, that in that case imported lepers ought to establish new leprosy areas. This second point requires examination at

some length. It is crucial for any mode of communicability, and for my own part I do not think the facts support it.

We know perfectly well that imported lepers do not always create a new area of endemicity (if they ever do so), as they ought on the hypothesis of direct or indirect communicability. The case of the Norwegian lepers in the States is one in point; that of lepers returned to the cities of Europe is another. I refer again to these two examples because the facts concerning them are perfectly well-established, and especially because the fact that these areas are not areas of endemicity is ascertained. I know you would have no difficulty in culling from books many round statements that such-and-such areas were contaminated by imported lepers, and were, in consequence, newly-made areas of endemicity. Now you will observe, no doubt, that if these assertions were substantial, if they were warranted by perfectly clear evidence, the whole question of the ætiology of lepra would be thereby settled at once for all, and in the least disputable way. It would be proved thereby that leprosy was maintained by communication with the sick, and whether the mode were direct or indirect would become a secondary question, and, for preventive purposes, no very important one. I lay stress, therefore, on the counter statement which I am about to make. It is simply this:—In not one of the cases of alleged importation of leprosy which thus far I have studied, have I been able to find anything that could reasonably be regarded as evidence of the assertion, vital though it obviously is. The datum, without which the whole case must fall to the ground, never is ascertained; namely, the freedom of the dwellers on the area under examination up to the date of the known importation of lepers.

Thus, for instance, the prevalence of leprosy in Europe in the Middle Ages is frequently—and I venture to add, lightly—asccribed to the return of leprosy crusaders from the East. But according to Haeser,* quoted by Sir John Simon, notices of leprosy in Europe go back to the fourth century, and in the sixth and seventh centuries there were already leper-houses in France and Lombardy; and that makes it impossible to exclude the influence of locality, and to distinguish the alleged epidemic from recrudescence of a local infectivity. Then again, you are all aware, doubtless, that the present prevalence of lepra among the natives of the Hawaiian group has frequently been, and indeed is usually, ascribed to the importation of Chinese lepers about 1855 or 1860. But if you consult the papers on leprosy in Hawaii, collected by the Board of Health, and the review thereof by the Hon. Walter M. Gibson, the Minister who presided over the Board and over collection of the documents referred to, you will have no difficulty

in perceiving, first of all that the beginning of leprosy in that group is unknown, and secondly that there is strong reason to believe that the disease was present among the aborigines as early as 1822—and how much earlier no one knows, nor ever will know now. With these opinions Dr. Arthur Mouritz, who, as well as Mr. Gibson, had taken great pains to learn the history of the endemic there as far as it could be learned, agreed in an Appendix to the collection, the whole of which was issued in 1886.* Another example may be found in Dr. Goldschmidt's recently-published account of leprosy in the island of Madeira.† The author said that Madeira was discovered in 1419, and was found to be already inhabited; the Portuguese occupied it, and many immigrants from Portugal were introduced, a great many convicts and people of the lowest class among them. Then, leprosy was present in Portugal at that date, as it was over the rest of Christendom. Lastly, before the year 1500 it became necessary to establish a lazaret on the island. These data are ascertained, and the author's probable supposition that some lepers were present among the Portuguese immigrants may well be accepted. But when, on the strength of these facts, he asserts that leprosy appeared, spread, and persists to this day in Madeira in consequence of that early importation of Portuguese lepers, it is impossible to go with him, for he has said nothing of the state of the aborigines. They may have been lepers when the island was discovered; nothing at all is known about that. It is misleading to speak of such cases as those I have mentioned, and some others which you will notice from time to time if you read with sufficient cautiousness, as though there were evidence to establish a fact, and especially a fact such as this, which really is of fundamental importance. The truth seems to be—and all who have critically considered writings on the course of leprosy cannot but agree with me—that writers on this subject usually entertain the prepossession that lepra is *maintained* by communication with the sick, and forthwith either unconsciously bend the facts to accommodate their prejudice, or, which comes to the same thing, take for granted in favour of their prejudice a great many important points as to which there is in reality no evidence at all.

Hitherto I have spoken only of a contagium, and of man, as though conjunction of the two were all that was necessary, and, as though all men were equally susceptible. If it were thought that the contagium resided in localities, then very little could be said as to comparative susceptibility, for reasons which I shall give before concluding;

* Report by the President of the Board of Health to the Legislative Assembly of 1886, on Leprosy. Honolulu, H.I., 1886.

† “La Lèpre; Observations et expériences personnelles.” Par le Docteur Jules Goldschmidt, Paris, 1894.

* “English Sanitary Institutions,” 1890, p. 36.

but if it were assumed that the contagium was communicated by the sick, then it would be pretty clear that all men were not equally susceptible of it. Now, therefore, I must make further reference to the report of the Leprosy Commission in India, from which I have already drawn important details, although I find myself unable to accept all its conclusions.

The Commissioners espoused a theory of maintenance by indirect communication, which I have already mentioned, and probably they were aware of the difficulty I have just pointed out—that either there was in reality no good evidence of the spread of lepra by importation, or else that in ascertained cases, such as the importation of lepers in modern times to Europe or to America, it was clear that importation did not, and does not, give rise to new areas of endemicity. At all events, their reference to the function of natural resistance was by way of meeting this difficulty among others. They suggested that lepra would be likely to spread or to be maintained by indirect communication when the disease was introduced, or existed, among a people whose natural resistance was reduced by poverty in general—by food in some measure inadequate to physiological needs, and by the insanitary conditions usually found concomitant therewith. Notwithstanding the source of this view, I venture to say that I have not yet succeeded in seeing anything more than a platitude in it, no suggestion having been made (though an implication there unavoidably was) that reduced efficiency of the defensive function stood in any special relation to leprosy among all the infective processes. It seems to me, therefore, to be a generality. We are but just beginning to learn something of the physiology of defence, it is true; but clinicians have always observed that of all the not specially protected persons exposed to infection, whatever the disease might be, some resisted it and escaped. And clinicians have always seen reason to believe that poverty and filth in some form were predisponents to many of the infective processes, and therefore we may suppose it possible that they dispose to leprosy too. So that it must be asked what this proposed explanation amounts to. Does it exclude the inhabitants of any country? Are there at this day no poverty and filth in Norway, where lepra began to diminish long before the measures of partial and imperfect isolation in force there could have taken effect? Are there none in the great cities of Europe to which lepers return, and where they live harmlessly as regards their neighbours? And, after all, is it not the case that of all the poverty-stricken on recognised leprosy areas a very large majority escape the disease—that lepers are found, at most, in but very low proportion to the total poverty-stricken population? From the account of leprosy in Madeira, which I quoted just now, let me give

you a very striking example of this. On that island all lepers were never isolated, though there was a lazaret to which poor lepers only were forcibly removed from all the parishes on the island until 1860; and since 1860 all lepers have been entirely unrestrained. The people themselves do not regard lepra as a communicable disease; they ascribe its occurrence to the use of a certain vegetable, and in consequence regard cases with perfect indifference, and live in quite ordinary contact with the sufferers. Now, the area of the island is very small—about 780 square kilometres,—and its population is about 130,000, so that the density is uncommonly high, or 170 to the square kilometre. The author said—and observe that he was not then dealing with a matter of ancient history, as he was when I quoted him before, but was giving the result of his personal experience during the past twenty-six years—that it might be taken that two-thirds of the close-packed population, or about 80,000 of the people, lived in *la misère* (a scarcely translatable term, which means, on the whole, incessant labour rewarded by earnings which scarcely suffice to provide food adequate to the labourer's physiological expenditure), and that the state of the people was still going from bad to worse. Hence, if leprosy were maintained by direct communication with the sick, or by indirect communication, or if natural resistance to leprosy in particular could be impaired by a lifetime led in *la misère*, or if loss of natural resistance were indeed the determining cause of the endemic persistence of leprosy, then Madeira at all events ought to be ravaged by this disease. But what is the fact? The author reckoned but seventy cases in the whole island.

Thus I consider that the epidemiological facts no more support the theory of indirect communication than they support the simpler hypothesis of direct communication. This seems to me to be clear, but on the all-important condition that facts alone shall be allowed weight, and that guesses and assumptions shall be recognised and discarded. On the other hand, the character of leprosy, as deduced from a general survey of its behaviour the world over, is that of an endemic disease; of a disease which is essentially connected with locality. If a man visit a recognised leprosy area he runs an appreciable chance of contracting leprosy; but if a leper go to Europe his neighbours are in no more danger of leprosy than they were before he returned among them.

If we confine ourselves to a pathological view, many of us will feel inclined to say that probable analogies class the *b. lepræ* with the endogens. But to the extent of my researches, the epidemiological facts seem to me to point to its being an ectogen; and not merely such an ectogen as the cholera vibrio, but rather such as is the bacillus of tetanus. Nor need it be supposed that on this view leprosy should be much more common than

it is, even on areas of its endemicity. The defensive function is not to be under-valued. It may well be that our phagocytes are usually victorious over that ancient enemy, the *b. lepræ*. But beyond that, we now know, thanks to the labours of M. Metschnikoff (cholera*), and MM. Vaillard, Vincent, and Rouget (tetanus†) that successful infection is not always due to weakening of the defensive function by agents which are "depressant" in general terms. Men do not acquire tetanus, for instance, merely because they happen to be out of health when they receive the virus; clinically no fact is better known than that, I suppose. Nor do those who drink too much beer or eat too much fruit during a cholera epidemic succumb to cholera merely because they have become generally depressed by drunkenness or by diarrhoea. No; it is the concurrence with the *b. tetani* and the *vibrio cholerae* of other organisms—quite harmless in themselves, it may be—which gives those bacteria their surest opportunity of evading the natural defenders of the body against them. Thus, if we supposed for a moment that concurrence of some other sort of organism with the *b. lepræ* were necessary to successful implantation of the latter, we could understand how it might happen that, of all the people living on a tract of country freely infested with the cause of lepra, only a few would become lepers. Do not forget that all this is but speculation as regards leprosy. I think it is surely founded on ascertained epidemiological facts; but through experiment alone can the superstructure be safely raised.

When I framed this communication I had no intention of producing a mere polemic. Probably that is evident from what has now been said; and in that case you will, perhaps, have been surprised that I have but little referred to the course of leprosy in Australia. I presume that each of the five governments took the rational precaution of ascertaining what that course had been before enacting the extremely severe laws against the liberty of lepers which they have adopted, and at this moment enforce in five colonies—before they ventured to add the remarkable hardship of imprisonment for life to the affliction of incurable disease. I cannot suppose that they failed to do this, although no evidence of it has ever come to light; for I will not suppose that with ample medical advice at their command they treated their people like chattels and as though, if in those enactments they should ultimately find they had fallen into error, they could defend themselves by airily alleging that at all events their mistake had been on the right side. I will not for a moment entertain a supposition so injurious—injurious not merely to Governments, but to the people who otherwise acquiesced in a flagrant infringement on personal liberty. Doubtless, the facts regarding leprosy over the world in general, and particularly

the facts regarding leprosy in Australia, were carefully gathered and critically examined, before so momentous a step was taken as that which declared that every leper should for the future be rigorously imprisoned for as long as his incurable malady should permit him to survive; that he himself and all his family should for ever suffer because it was essential to the public good.

And yet the people, and yet you yourselves, do not know the precise grounds on which those enactments were made; you do not in the least know what the course of leprosy in Australia has been; you cannot tell whether it has been such as manifestly supports, or merely fails to support, or manifestly contradicts the belief that leprosy is *maintained* by communication with lepers. For want of that information the salient characteristic of those laws must seem in the eyes of some to be renascence in the nineteenth century of the product of mediæval ignorance into mediæval egoism. Should not the strong evidence which no doubt exists in the archives of one or more of the five governments to show that the practice of the middle ages was wise and indispensable, and therefore still is necessary in Australia, be now—though late—produced to us?

In the meantime, though you have not been told anything about the epidemiology of lepra in Australia, you do know the facts concerning individual cases which have occurred among you, and which you have yourselves witnessed. I ask you, therefore, after every prejudice, all imperfect observations, all traditional opinions, and easy-going assumptions have been coolly identified and then rigorously cast out and rejected, to consider whether those cases, as far as they go, do support the popular belief that leprosy is *maintained*, or kept present among mankind, by contagion.

ENTERIC FEVER FROM CREAM CHEESE.—The experience of the European troops at Agra, during 1893, is instructive. Out of forty-eight cases, no less than forty-two occurred in one regiment, the East Surrey. The chemical examiner of the North-West Provinces, Mr. Hankin, found typhoid bacilli in the aerated waters and the *dahi* supplied to the East Surrey regiment. He adds that, owing to precautions which have been taken on his suggestion, the aerated waters are no longer open to suspicion; but so long as bazaar *dahi* is eaten in the shape either of cream cheese or sweetmeats there is danger of the introduction of the typhoid bacillus. The typhoid bacilli found in the *dahi* examined are derived, Mr. Hankin adds, from contaminated well-water. He considers the idea that enteric fever may be caused by "fatigue and exposure without any help on the part of microbes" to be "old-fashioned and grotesque, but labour-saving."

* Ann. Inst. Pasteur, 1893-4. † Ibidem, 1891-2-3.