CORRESPONDENCE.

20th February, 1951.

TO THE EDITOR OF *Leprosy Review*.

SIR,

It has been questioned for a long time, but more, recently, whether the widespread but unproved assertion that neural or non-lepromatous leprosy is non-infective, is true. The illuminating, if disquieting, article by Dr. A. R. Davidson of Pretoria is a case in point—in which he says "the bacilli will be found with a frequency directly in proportion to the energy expended in looking for them." He stresses the potential infectiveness of neural cases formerly negative—"The totals of those who have either become lepromatous, or have shown bacilli are 75.7 per cent females, and 62.3 males."

From my own experience and observation, I certainly share in his opinion of the infectiveness of neural leprosy. In this Colony, there have been many neural parents with neural children, brothers and sisters, and several husbands and wives. These have no doubt where they got the infection (and they include the educated and intelligent section) although there is no positive proof, and state that there was no contact with another type of leprosy. It is too much to believe that all these are wrong, just as it is too much to believe that all those who suggest contact with an unknown lepromatous stranger, are right. The simple African will instinctively keep his distance from such cases. There are patients here who have been discharged negative, who have always been negative, and who, after some years, have returned, and been found positive.

In a survey of a tribe in this neighbourhood some years ago, it was found that, of several thousand persons examined, only 5 per cent were lepromatous, and the amount of leprosy was 7 per cent of the entire population surveyed. It is difficult to credit that all these acquired the disease from a relatively few positive cases. A lepromatous mother has a lepromatous child. That we know. Can a lepromatous mother infect her child with, what in so many respects, is a different disease? When you have obviously active, virulent, erythematous, and spreading or multiplying skin lesions, who can confidently proclaim that such cases are non-infective?

If this were only an academic controversy, it would matter little; but it makes a profound difference to the human being formerly known as "the leper." Those responsible for anti-leprosy schemes with limited resources are only too ready to accept the view that neural leprosy is of minor importance to be treated with casualness, if not with indifference. It has been openly proclaimed in the press of this country, that neural leprosy is non-infective; that patients can stay in their own homes and come for treatment to out-patient dispensaries. This, many will do, if not otherwise engaged—many will not. And it is unfortunate that an excuse has been put into the mouths of patients of any class accused of having the disease. *Anyone* of them can now say "I am not infective, and am no danger to others."

In this Colony, there are resident roughly 800 lepromatous patients and 2,700 neural or tuberculoid. 2,000 of the latter, apart from the small children, are able to work. The remaining 700 have anaemia, debility from various causes, ulcers and deformities. It is this class—the 700 neural cases—that by far the

greater amount of deaths occur. It is these, with the daily treatment of ulcers, provision of extra drugs, dressings, food and nursing, that claim the greatest amount of our time and attention, and are the greater expense. We frequently do this for months, or years, knowing full well that the case will be lost in the end. It is somewhat of a shock to be told that such cases "need not be admitted."

To say that only lepromatous cases should be helped and supported, that they alone should be admitted to fully organised leprosaria is—in my judgment—a retrograde step, satisfying neither the patients, the public, nor the medical staff. The African may not know much about the classification or the bacteriology of the ailment. To him, a leper is a leper and, more than any of us, he knows the *effects* of the disease, known locally as "Akpamfia" —literally "The white death."

To regard neural cases as non-infective, and to adopt a policy based on that assumption is, in my opinion, to build on a foundation which is unstable, and will not stand the test of time. With many years' experience in the treatment of thousands of patients, I have always considered leprosy of whatever classification a serious disease, either actual or potential, and I submit that such medical care and treatment as can be given to out-patients (when they come) is totally inadequate. But it makes everything, relatively, so cheap—and it is so convenient, to consider neural cases as non-infective. They can stay in their own homes without being a danger to healthy persons—and at their own expense; the babies can, with perfect safety, be nursed by neural mothers—most convenient it is, but there are good reasons to fear that it is just not *true*.

I am,
Yours faithfully,

THE LEPER COLONY, ITU,
CALABAR PROVINCE,
NIGERIA.

A. B. MACDONALD,
Superintendent.

TO THE EDITOR, LEPROSY REVIEW,

With mounting surprise I have read in your October 1949 issue the editorial note on the report by Porritt and Olsen [*American J. Path. 23* (1947) 805-817], of two cases with leprosy lesions developing in tattoos done in Australia on enlisted men of the U.S. Marine Corps, an "extended abstract" of which report you reprinted from the *International Journal of Leprosy* [*16* (1948)

514-519]. Because of the influence of your periodical, I would like to comment on that editorial. I feel in a position to do so because I had the privilege of seeing, with one of those physicians at Ann Arbour in 1947, one of the patients and all of the histological material, and in consequence was led to prepare and publish that abstract.

In submitting the matter to " intelligent scrutiny "—done, by the way, without reference to my editorial note that accompanied the abstract—you say, first, that " no evidence is quoted as to whether this town [from which the men came] was in an endemic area of leprosy," and then that " no evidence is given as to whether friends or relatives of these two patients had ever suffered from leprosy "; and you end with the suggestion that these might have had themselves tattoed to cover existing leprosy lesions, because that has sometimes been done in endemic regions in Asia.

The report as published and reprinted is headed as emanating from Pontiac, Michigan, in the north central United States. The authors evidently regarded it as unnecessary to say that that area is not an endemic one; but, realizing that an international audience might not be aware of that fact, I said in my editorial that it is " . . . a place where leprosy is not and never has been endemic, and so far as has been learned, no case has ever lived. . . . " That statement also applies to the further question about friends and relatives, which the report itself deals with in the statement that there was nothing significant in the family or personal histories of the patients. As for the other idea, both men were tattooed with the same design on the left forearm, and there is no reason whatever to suggest that that was done to conceal pre-existing leprosy lesions. Those men would not have known a leprous macule had they seen one; they had not the slightest suspicion of the nature of the condition about which, some $2\frac{1}{2}$ years later, they sought advice.

Reference to the original report would have revealed the fact that the true diagnosis was first suggested by a third physician. There is nothing in the report which suggests that it was not written conservatively, intelligently and in good faith, and it appeared in a particularly conservative journal The attempt to make an absurdity of it by suggesting a parallel to infection by a crocodile with " leprotic pyorrhea " seems quite unwarranted.

I share the skepticism expressed about the Lagoudaky case, having also seen the man in 1938. In all conservatism, however, I cannot share the view that all reports in the literature of cases of accidental inoculation are of " extremely doubtful significance," just because many are. It is of course a moot question—pertinent

in this connection—whether or not Arning's deliberate inoculation of a Hawaian criminal was successful, but I agree with Vedder who wrote [*Porto Rico J. Publ. Hlth. & Trop. Med. 6* (1930) 106-121], "Personally I believe that leprosy was transmitted in this case. . . ." Difficult to shrug off is de Langen's report [Far East. Assoc. Trop. Med., 1930, vol. 2, p. 499] of a man apparently infected by an unsterilized syringe which had been used to inject a leprosy patient. Still more difficult to deny—quite impossible for me, since when in Paris in 1931 I was consulted about the case and shown the biopsy material—is Marchoux' report [*Internat. J. Lep. 2* (1934) 1-6] of the accidental infection of a surgical assistant by a needle which entered his hand.

If leprosy is an infectious disease it must be transmitted somehow. As for the route, the evidence that infection may occur through and in the skin cannot be ignored. Indeed, that may be the usual portal of entry, if one can accept as significant the apparent primary cutaneous lesions to be found in young children. I submit that, unless it be true that the adult cannot be infected at all, the report which your editorial holds up to ridicule must be taken seriously. More than that, because two persons were dealt with at the same time and both developed the infection, each therefore affording a sort of control on the other, I submit that it is the most convincing event of the kind ever reported. The report is not beyond criticism, of course, but criticism of it should be reasonable. What could have been done, after three years and from the other side of the world from the scene of the inoculation, to give these cases the "much more thorough, and much more careful" investigation demanded is not evident.

Culion Leper Colony, H. W. WADE, M.D.
Philippines.

Dear Dr. Ryrie,

This is just a note to correct an error which has appeared in the *Leprosy Review* of October, 1949, page 119, and also the same error in the Jan.-Apr. 1950 issue, page 6. *Sodium Nitrate* is given as the diazotization agent in the determining of the sulfones of blood and other fluides, and this should be *Sodium Nitrite* instead. I presume it is a typographical error. This, however, may cause confusion if the individual is not familiar with sulphone determination.

Respectfully,

Carville, La., U.S.A. SISTER HILARY ROSS.
June 19, 1950.