quires, Clinical, histological and immunological aspects of dimorphous leprosy. Tim Jorresu, Y (1963) 523-524 (distributed before the beginning of the sessions), has not considered it of interest to register the 8 per cent that we noted in the Central Dispensary of Dermatology (Buenos Aires) for the dimorphous group, which, without doubt, represents the highest figure published up to date.

We agree with Dr. Azulay in his statement: "The possibility that a medical practitioner will be right in his classification of leprosy cases on the basis of dermatoneurologic symptoms is higher than 90 per cent." We would like to add our belief that with some practice the dimorphous leprosy under consideration can be diagnosed in an increased number of cases "as long as one observes and follows up the cases dynamically. The adoption of a static, or purely histopathological criterion is what has given rise to the Byzantine discussions that revolve about this form of leprosy," as I said in the paper I presented at the VIIIth International Congress of Leprology.

In addition I wish to emphasize that due importance does not seem to have been given to the fact that I have repeatedly expressed, in various published works, the fact that in reactional states (the real "borderline" picture in my conception, as long as the term "dimorphous" means for me quiescent states, including macular varieties) no erythema nodosum is seen, even though they are at times accompanied by dissemination of the lesions, particularly on the face, back of the neck, and elsewhere.

Otherwise Dr. Azulay's article is excellent, and in large measure is in agreement with our experience. Differing from what other authors have indicated, we have called attention to the rare neural repercussion in dimorphous leprosy (Leprologia 8 (1963) 48-49). Dr. Azulay stresses the same fact in his casuistic when he states: "Nerve involvement: this is much less than in Lepr cases, not only in intensity but also in frequency."

It is interesting, in addition, to note the 16.6 per cent of cases of Azulay and Alonso that became lepromatous in spite of treatment. In our statistics on 115 dimorphous cases we have noted 19 per cent of lepromatizations in patients treated with sulphonates, including four cases diagnosed by other colleagues as reactional tuberculoid and by us as dimorphous tuberculoid.

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Central Dispensary of Dermatology
Ministry of Social Welfare and Public Health
Buenos Aires, Argentina
May 2, 1966

World-wide Leprosy Survey for Progress in Leprosy Control

To the Editor:
When we completed the isolation of leprosy patients that we knew about in the Philippines in 1912 and quartered them in the Island of Culion, where a separate town of 10,000 inmates was constructed to receive them, we fondly believed that after this was done we could expect a rapid decline in the incidence of leprosy. Apparently this has not happened. A few recent checks seem to indicate that the incidence of the disease has not lessened and there is even a possibility that it may have increased. Since then millions have been spent in laboratory research with the hope of creating new knowledge that would enable better control of leprosy. Unfortunately, so far as I know, very little progress has been made with all this research work. The data that have been developed are nearly all negative. On the other hand, in the field—that is in the hospitals and clinics—better drugs have been developed, which undoubtedly have greatly improved
the treatment of the disease and many arrested cases have resulted and some cured. Information from other countries also indicates it may possibly be that the incidence of leprosy has increased. On the other hand, there is authenticated information that the incidence of the disease has decreased in some countries. This apparently has occurred in Hawaii.

As it seems that little or no progress has been made in the control of the disease, and it is even possible that the incidence is increasing, it seems to me that another approach should be developed. The statistics of leprosy are so fragmentary and unreliable that it is impossible to assess reliably just how many cases there are in each country. Perhaps, then, the first step should be to make a world survey, country by country, which would show, among other things, the incidence and the number under treatment.

In dealing with other diseases it has often happened that an epidemiologic survey has pointed out the road to control. In any case, a base line would be established from which the results of future efforts to control could be measured. The questionnaire for the survey should be prepared by trained statisticians. If this idea should meet with favor, it would seem well to approach the World Health Organization to ascertain if they would undertake the task. Leprosy is a serious problem in which our health efforts have made very little progress and deserves a better effort to bring this most refractory of diseases under control.

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1060 Fifth Avenue
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May 23, 1966

The Temperature Factor in the Growth of *M. Ulcerans*

[EDITOR'S NOTE: The importance of temperature in the growth of certain pathogenic mycobacteria has been stressed frequently. Many of them thrive best at temperatures below that of the human body, but a narrow range as respects variation in temperature appears to be of great significance. The following extract from a letter from Dr. Jean C. Tolhurst, Research Fellow in Bacteriology, Alfred Hospital, Prahran, Victoria, Australia, to Dr. Daniel H. Connor, Geographic Pathology Division, Armed Forces Institute of Pathology, Washington, D. C. is believed to be of sufficient interest in this respect to justify making its contents more widely available. Mr. Glen Buckle and Dr. Tolhurst are noted as the first to cultivate Mycobacterium ulcerans (MacCullum, P., Tolhurst, Jean C., Buckle, G. and Simons, H. A. A new mycobacterial infection in man. J. Path. Bact. 68 (1948) 93-122). The correspondence partially reprinted below was with reference to a recently published article by Daniel H. Connor and H. Fletcher Lunn: Buruli ulceration. Arch. Path., 81 (1966) 183-189. It is presented here with the permission of Dr. Tolhurst and Dr. Connor.]

DEAR DR. CONNOR:

* * * * *

Perhaps I should tell you a bit more about our incubators.

At times we were using all the available space in the incubators in our Department, naturally having been careful to check that the thermometers of the "37°" incubators did read 37°. We made attempts at cultivation at quite irregular intervals over many months, whenever we had a suitable rat to give us fresh peritoneal fluid. The culture tubes were incubated wherever there was space, as the convenience of our diagnostic work for hospital patients came first.

When a few tubes showed colonies of acid-fast bacilli we naturally hoped they were the organism we were looking for, but we could not prove it. We could not produce satisfactory subcultures and we could not repeat the event from new rat fluids. No one could claim to have grown