*Lepr. Rev.* (1971) 42, 14-25

# Epidemiology and Leprosy Control*

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Leprosy increasingly interests workers in other fields because of its growing relevance to their own work (for example, immunology). It may be helpful, therefore, to reconsider the rôle that modern epidemiological methods have to offer in leprosy control, primarily in research, but also on the service side.

## Introduction

"Epidemiology" itself needs some definition in the context of leprosy. Hitherto, the word has often been loosely applied to clinical observations—for example, the alleged different sites of first lesions in children being related to their (infected) mothers' way of carrying them—or to the characterization of groups of patients who are often highly selected by virtue of being in hospital, or who volunteer for study. This usage, though formerly understandable, is no longer justifiable. Modern epidemiological methods involve eliminating as much bias as possible in identifying cases of a disease, and relating them to the population from which they are drawn. In practice, these precepts mean looking for cases outside hospitals as much as inside them, making every effort to find *all* the cases in a defined population, and obtaining the same accurate and comprehensive information about the unaffected, as well as the diseased members of the population.

The term "control" must also be defined. Leprosy control is currently most often used in the context of detecting and treating early but established disease. This usage fits into the classic concept of *secondary prevention. Primary prevention* aims at forestalling the development of clinical disease altogether by eliminating causes or by detecting individuals at particular risk (perhaps already with "precursor" disorders) and by protecting them as far as possible. (*Tertiary prevention,* for completeness, is concerned with controlling deterioration in severe, relapsing and disabling disease.)

Primary prevention is obviously the ideal. One of the very few studies of leprosy that has produced incidence figures from year to year (Vellut, 1969) has

* Received for publication 19 November, 1970.

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shown that in spite of a highly organized and intensive "control" programme designed to detect and treat early disease, the rate at which new cases appear is not declining. In other words, there is no evidence from Vellut's study that what is basically "a secondary prevention" approach is in fact "controlling" leprosy in the sense that it is making the occurrence of new cases of the disease any less frequent. Indeed, if very early cases are infectious, or if there is an infectious prodromal period (and evidence is accumulating that suggests this), one would hardly expect secondary prevention to make more than a partial contribution, at most, towards *eradication.* This it may do by rendering a limited proportion of established cases bacteriologically negative earlier than otherwise, but such patients will still have a great potential for infecting other people. For leprosy, no less than for any other disease, "prevention is better than cure", and eradication must ultimately depend on effective primary prevention, so that what is eventually improved—that is lessened or even abolished—is the individual's chance of coming into contact with the bacillus, rather than his chance of being detected, treated and made bacteriologically negative; this depends on reducing or eliminating the "prevalence pool" of infection.

This paper is an attempt to shift the emphasis on "control" of leprosy very much more into the field of primary rather than secondary prevention, and to discuss the difficulties and problems that would arise in the incidence studies necessary to do this.

## Prevalence and Incidence

The *prevalence* of a disease is the proportion of a defined population affected at a given time; it is usually expressed as a percentage. The *incidence* is the rate at which new cases develop in the population, expressed usually as a percentage per annum. (This distinction is crucial, and is made here because frequently, as in parts of Cochrane and Davey (1964), "incidence" is used to describe "prevalence".)

Prevalence studies are relatively easy to mount, can usually be completed fairly quickly, and are generally reasonably inexpensive. Incidence studies, on the other hand, involve an initial prevalence study in order to identify established cases, which are then excluded from the subsequent follow-up; but this may not take place until several years later and therefore involves the costly process of keeping the population under review. (Incidence studies, based on documentary data, in which the initial examinations and/or follow-up have been carried out at some time in the past, are possible, and may reduce the problems and cost of keeping a check on the population; but the investigator is not usually in a position, as he is with on-going clinical studies, to decide on and to influence the kind of data collected at the initial examination. In addition, other problems concerned with defining and keeping in touch with the population may arise.)

There is always a large and stable "pool" of prevalence cases of leprosy in an endemic area. This has in the past provided an open invitation to easily available clinical material, since it was only necessary to go to a leprosy hospital in an endemic area to have the widest possible choice of all the clinical varieties of the disease for study—or, with a little more effort, to carry out a prevalence survey in the certain knowledge that it would not be long before an ample number of cases became available for further study. But in the first case patients in hospital are seldom, if ever, representative of the condition in question; in the second case the

very onset of a disease like leprosy often alters the general social and personal characteristics of those affected (i.e. the very attributes thought to be important in causation), so that what is being studied is the *results* of the disease and not (or only in a very limited sense) its *causes*. (Considerations of this sort form the standard epidemiological basis of work in, for example, ischaemic heart disease.) It follows that incidence, not prevalence, is the vital index in leprosy (as in most diseases), and more detailed reasons for this and their relevance to primary prevention are now discussed.

There are a number of reasons why modern field-study methods have so far been used to only limited purpose in leprosy, particularly where incidence is concerned. Paradoxically, however, and as is well known, the importance of epidemiology to leprosy was clearly recognized as long ago as 1933 by Doull *et al.* (1942) who clearly differentiated prevalence from incidence and discussed the advantages and disadvantages of these two indices in a way that, in the area of epidemiology, placed this group far ahead of workers, not only in leprosy, but in almost every other field. The subsequent history of this group's field studies, which will be discussed, high-lighted many of the difficulties the leprosy epidemiologist faces.

## Importance of Incidence Studies

Leprosy is a chronic disease, though acute events such as reactions and certain types of neurological involvement frequently occur. It is also a disease of multiple aetiology in the sense that while exposure to the bacillus is a necessary cause, it is by no means sufficient for clinical infection; in highly endemic areas with a prevalence of 3% or more, and in spite of the fact that members of the indigenous population must be more or less exposed throughout their whole lives, vastly more do *not* develop clinical disease than *do*. Even of those living in families with a leprosy patient (lepromatous or otherwise), while running a higher risk than those in unaffected families, only a few develop clinical disease (Guinto *et al.*, 1954). Clearly, many other factors such as age, sex, ethnic group, socio-economic conditions and immunological status are involved in the development of leprosy. Finally, leprosy obviously has a prodromal pathological stage preceding its (usually rather gradual) clinical appearance, and definitions of onset therefore present a problem.

In these respects—i.e. chronicity, "multiple aetiology", difficulties of definition of onset, and long prodromal pathological phases—leprosy closely resembles many of the developed world's *non*-communicable diseases, notably ischaemic heart disease (which may even—we do not yet know—have a necessary but not sufficient "cause"). There can be little doubt that our (by now) quite considerable knowledge of the causes of the chronic non-communicable diseases has come chiefly from epidemiological studies, and, in particular, incidence studies. The great contribution of the epidemiological method to investigation of these diseases has arisen chiefly from the fact that it fulfils three functions, namely: (1) the study of *whole* populations, permitting useful comparisons between affected and unaffected groups; (2) epidemiology is the only discipline that can assimilate, process and analyse data that have been collected simultaneously on a very diverse range of variables—medical, social, demographic —that are thought to be of importance; and (3) the method has been well adapted, from the days when infectious diseases were its main concern, to deal

with long-term situations. Diversity of the data that need to be considered, and the importance of long-term study are, of course, two outstanding features of leprosy.

The methods that have been most fruitful in the chronic non-communicable diseases have been those of identifying likely aetiological *risk factors* that characterize *high risk groups,* i.e. particular groups within a defined population who show these risk factors. For example, the risk factors of raised blood pressure and plasma cholesterol level have been used to define a high-risk group of men whose individual chances of developing ischaemic heart disease within 5 years are, at 1 in 7, five times higher than the group in which neither of these risk factors is present (Morris *et al.,* 1966). In other words, the very real possibility of *predicting* the onset of ischaemic heart disease exists, with the obvious implications that this has for prevention; similarly, there is every reason to believe that the same approach would be valuable in leprosy.

These methods of prediction depend on two fundamentals: (1) the studies on which they are based must be incidence studies in which, as already indicated, information on variables thought to have an aetiological rôle is collected *before* the onset of the disease; and (2) that methods of analysis must be used which can indicate the contribution to prediction of each variable, independently of its *association* with the others. For example, living in a household where there is leprosy and a low socio-economic status are both thought to be important in determining the onset of new cases of leprosy. But do both these factors influence onset in their own right, or is it merely that they are often associated with one another, that is, that living in a leprosy household tends also to go with a low socio-economic position? A *multivariate technique of analysis* will give a weighting to each of these factors, so that the importance of each can be identified independently of its possible association with the other (or with others, if the example is made more complex and 10 or 12 variables are included). These methods of analysis enable, often for the first time, valid discussion of the possible separate causes of a chronic disease of multiple aetiology; in practice they usually demand access to computers.

In view of the general, but relevant, similarities between leprosy and certain chronic non-communicable diseases, there is really no doubt that incidence studies of leprosy that satisfy certain criteria (to be discussed) could reveal a great deal in the way of transmission and causation, and also indicate the priorities for preventive measures more clearly and objectively. Indeed, on the question of prevention, it is essential that high-risk groups be identified precisely before mass BCG and DDS prophylactic measures (Russell *et al.,* 1964; Wardekar, 1967; Brown *et al.,* 1968; Bechelli *et al.,* 1970) make it too difficult to do so.

## Experience of Previous Incidence Studies

The reports from Cordova and Talisay, Cebu province, in the Philippines, provide the largest body of data from studies from a single source specifically set up to determine incidence (Doull *et al.,* 1942; Guinto *et al.,* 1954). This series began in 1933, when the first group of a defined population of 21,000 people was initially examined, and its prevalence cases identified (and excluded from further follow-up). The population was followed up at intervals until 1951, with a *mean* follow-up interval of about 15 years, when 275 *new* cases of leprosy (68

lepromatous and 207 non-lepromatous) had been identified, an annual incidence of only 0.09% for all types of the disease and of 0.03% for lepromatous leprosy. The final results were studied in relation to the variables initially recorded (which, systematically and comprehensively, were confined to age, sex and household exposure). The main finding was a confirmation that living in a household which included a leprosy patient (especially one with lepromatous disease) appears to increase the individual's chance of developing leprosy himself.

However, these studies drew attention to several serious problems. First, both prevalence and incidence cases (particularly the latter) were identified, it seems, by a mixture of physical examination and verbal recall, the relative importance of each method depending on which members of a family were available when the team called at any household. A physical examination for leprosy at a particular point in time—and leaving aside problems of observer variability and validity—gives information on those affected *at the time,* but not about those who may have had disease *in the past* and which had resolved and left no residual signs. Verbal recall does, theoretically, deal with the latter problem, but much depends on the informant's accuracy of diagnosis of himself and his family. In an endemic area, where fungal skin infections and other conditions requiring differential diagnoses occur, where taboos and prejudices about leprosy may be strong, and where spontaneous remissions and relapses are frequent, it is obviously too much to expect an (often) illiterate population to give valid answers.

It is not entirely clear from the published material, but it appears that in carrying out follow-up surveys the Philippines group relied more on physical examinations (versus verbal recall) than they had done at the initial examination. If this is so, it might partly explain the apparent (but possibly spurious) fall in incidence noted over the 15-year follow-up period. It seems likely that, at the first survey, many people reporting past disorders (especially of the skin) might have been diagnosed, on little evidence, as having had leprosy whereas the physical examination at the second survey would have provided an opportunity of making a more accurate differential diagnosis, and of excluding fungal infections, psoriasis, and other diseases sometimes mistaken for leprosy. Another reason for the possibly spurious fall in incidence is discussed later.

However, the mixing of verbal recall and physical examination, without applying one method uniformly, does emphasize a fundamental problem of all field studies (especially prevalence studies), namely, that of identifying those who have had acute, or relatively acute events, such as, in the case of leprosy, a hypopigmented patch that comes and goes (or is cured) in the interval between a first examination and subsequent follow-up, i.e. the "evanescent" cases. Obviously, a study that misses these is at fault, and ways of dealing with the problem are also discussed later.

There have been very few other *formal* studies of the incidence of leprosy. Some incidence rates, obtained chiefly from studies which have primarily been set up as "control" projects (again, in the sense of secondary prevention) or as prophylactic trials of BCG and DDS are shown in Table 1.

(Not included in the group of prophylactic trials are the studies of prevention by BCG in Uganda (Brown *et al.,* 1968) and by DDS in South India (Dharmendra *et al.,* 1965) as these were carried out on contacts of leprosy patients, i.e. high risk groups, and as such can give only limited incidence data referable to a whole population.)

In Table 1, studies no. 2 (New Guinea) and no. 3 (Nigeria) are based on small

populations and rather small numbers of new cases; the incidence rates may therefore well be subject to considerable random variation. The rates in studies 4 (Tanzania), 5 (North India), and 9 (South India) are estimates, as either the exact duration of follow-up is not stated or the exact numbers of new cases arising in a given period are not precisely known or clearly stated.

The main feature of these studies is that incidence rates are *very low,* though prevalence rates are high (the latter being the main index, according to present usage, of endemicity). If only lepromatous disease is considered, incidence rates are, of course, much lower.

It is important that one implication of the large numerical difference between the prevalence and the incidence of leprosy be realized in planning for primary prevention or for control, and that is that the temptation to make plans on the basis of much easily available prevalence data must give way to the difficulties of obtaining the much scarcer incidence data. The high prevalence of leprosy is mainly due to the fact that, unlike ischaemic heart disease for example, leprosy is rarely fatal, and that cure, when it takes place, does so very slowly. Thus, the prevalence "pool" once established (and this process is, of course, well described by Wade and Ledowsky (1952) for the island of Nauru), remains large, although the incidence "inlet" is small, because the "outflow" of cases due to death and cure is also small. Leprosy, in fact, is a "static" disease in the sense that very little occurs to disturb the basic composition and stability of the large prevalence "pool". (This situation can be contrasted with a "dynamic" disease like, for example, cancer of the bronchus, where the prevalence "pool" is always small and where death removes cases from it almost as quickly as incidence adds them.) Low incidence may of course be the result of incomplete case-finding, but in most of the studies cited in Table 1 it is most unlikely, as the published material makes clear, that this has occurred to any significant extent. But even if 25% of actual cases were missed, incidence rates would obviously still be very low.

Another important distinction made by considering incidence rather than prevalence is that between the relative load of lepromatous and non-lepromatous disease. Though there are, of course, wide geographical variations, most sources suggest that in a group of leprosy patients the proportion of those with lepromatous disease (i.e. on a *prevalence* basis) is about 25 to 35% of the total. Table 1, however, indicates that only 7% or so of new (i.e. *incidence*) cases are lepromatous. Lepromatous leprosy, in other words, is a *rare* disease in the sense that it develops in only a very small proportion of those who contract leprosy, of whatever type. It is *over*-represented in prevalence figures because its course is so prolonged and because cure is both slow and uncertain compared with tuberculoid disease. This fact only emerges, however, from incidence studies.

Low incidence automatically requires large study populations. Clearly, it is desirable that a study of the incidence of leprosy should, in the follow-up period, produce a sufficiently large number of new cases for meaningful analysis of the least frequently occurring form of the disease. Assuming the need to relate the incidence of each form of leprosy to *at least* sex, 3 different age groups, and the presence or absence of household exposure, and if there are to be 5 new cases in each of the 12 cells of this analysis, then ideally 60 new cases of the least frequent form of the disease (whether this is lepromatous, indeterminate, or purely neural) will be needed. Some studies do satisfy this objective (which is meant to suggest an order of magnitude only, not to be a definitive target) in terms of lepromatous and non-lepromatous disease. But in study 3 of Table 1, Davey (1957) found

TABLE 1

*Prevalence (rate %) and incidence (annual rate %) of all types and of lepromatous leprosy summarized from different reported studies*

| Study no. | Country | Date (approx.) | Population | Prevalence: all types* | Incidence: | |
|---|---|---|---|---|---|---|
| | | | | | all types* | lepromatous |
| 1. | Philippines (Guinto *et al.*, 1954) | 1933-51 | 21,791 | 1.9 | 0.09 | 0.03 |
| 2. | New Guinea (Russell *et al.*, 1968) | 1962-68 | 5063 | 6.0 | 0.60 | N.S.† |
| 3. | Nigeria (Davey, 1957) | 1941-55 | 3057 | 12.1 | 0.66 | 0.02 |
| 4. | Tanzania (Wheate, 1969) | 1961-68 | 12,231,000 (approx.) | 1.8 | 0.08 | N.S.† |
| 5. | North India (Lowe *et al.*, 1941) | 1936-41 | 9561 | 4.4 | 0.15 | 0.03 |
| 6. | South India (Wardekar, 1967) | 1964-66 | 20,000 | 3.1 | 0.70 | 0.05 |
| 7. | South India (Das, 1970; personal communication) | 1968 | 437,231 | 1.7 | 0.12 | 0.01 |
| 8. | South India (Vellut, 1969) | 1955-68 | 152,858 | 5.7 | 0.22 | 0.01 |
| 9. | South India (Suresh *et al.*, 1969) | 1963-68 | 484,038 | 2.1 | 0.23 | 0.02 |
| 10. | Burma (Bechelli *et al.*, 1970) | 1964-68 | 12,983 | 3.4 | 0.80 | N.S.† |

* Includes lepromatous disease.

† N.S.: Not stated, not available, or information not yet complete.

In general, figures are for all ages; original papers should be consulted for details. Data for studies 6 and 10 are drawn from control groups in prophylactic studies based on children and young adults. For further details see text.

only 2 new cases each of borderline and purely neural leprosy, so that if adequate numbers of these types were also required, the need for studying large populations becomes even more obvious.

Related to the number of new cases is the time during which these occur. In study 1 in Table 1 the Philippines group took 15 years to collect their 275 cases. Even today, with the most modern techniques of data-processing and wide experience of keeping checks on populations, not many centres can deal adequately with the problems involved in such long-term studies, particularly in the world's developing areas. Apart from practical difficulties, such as recruiting and keeping staff, and allowing for death and population movement, a 15-year period greatly increases the possible effects of extraneous influences. For example, the war in the Pacific, occurring as it did in the middle of the follow-up period of the Philippines studies, substantially altered, as the group readily admits, the balance between lepromatous and non-lepromatous disease in the area, though the mechanism of this is not known.

It is therefore clear, on the basis of low incidence and the desirability of a manageable follow-up interval, that if a study is to have the requisite numbers of new cases, especially of lepromatous leprosy, its population must be large. To obtain 100 new cases of lepromatous disease in 5 years, where the annual incidence of this form is, say, 0.02%, a study population of 100,000 persons is needed. If, as seems likely, the incidence of indeterminate and neural leprosy is lower than this, but also demands adequate numbers, the population would have to be even larger. In any case, it seems necessary to have populations of hundreds, rather than tens, of thousands available.

## Planning to Study Risk Factors

The principles so far discussed and the studies reviewed have indicated that in order to study the independent effects of several variables that may be risk factors (1) incidence studies which elicit information on many different characteristics are necessary, and (2) large populations—of the order of hundreds of thousands—should be available for study.

These *general* statements can be made in spite of obvious differences in classification and nomenclature and of the undoubtedly real geographical and racial variations that make direct comparisons between the studies shown in Table 1 difficult.

On material so far published, however, only studies 1 (Philippines) and 10 (Burma) of Table 1 provide prospective information, over and above actual incidence rates, on more than one variable such as age, sex and household exposure (study 6 gives some rates by age). A study that is to provide material for defining risk factors and high-risk groups must collect systematic information on as many items as its organization can usefully manage and which are believed to be of aetiological importance. Age, sex, marital status, household exposure to leprosy, educational attainment, occupation, family size, some index of overcrowding, details of eating and sleeping habits and of important religious and cultural attributes are the kind of personal, social, and economic information that may be relevant and can usually be elicited fairly readily and comprehensively during an initial *ad hoc* census. Whether medical information such as the presence or absence of other disease, e.g. tuberculosis, or from skin testing, can also be gathered depends on the study's resources.

There are three main problems which arise from the studies reviewed in Table 1 that are particularly important. The first concerns the handling of "suspicious" or uncertain cases of leprosy discovered at the initial prevalence study. These should probably be considered as "definite" cases, and excluded from further study. Studies 1, 3, 5 and possibly 9 in Table 1 all describe falls in incidence rates with time that are quite likely to have been spurious. This was because "suspicious" and "doubtful" cases at the initial surveys were, for operational purposes, classed as *not* being cases of leprosy. These patients were then included in the follow-up; clearly many of them really did have leprosy when first seen and this became certain within a fairly short period. Incidence rates initially, though not so much towards the end of the follow-up period, were therefore artificially inflated, and tended to suggest that incidence was declining with time. Later observations, however, have not confirmed such a decline (Lechat, 1969; Vellut, 1969).

A second specific point of organization concerns methods of case-finding; the same method should be used at both the initial and follow-up examinations, and the reasons for this have already been touched on. A physical examination by a doctor or a para-medical worker (and in the latter case, confirmation of all suspect cases by a doctor) is preferable to verbal recall on the part of the patient. This is certainly true, without any qualification, for the follow-up examination. At the initial examination, the physical findings may need to be supplemented (but not replaced) by some questioning to elicit a history of episodes of prior spontaneous remission or cure, without residual signs, of earlier lesions; but the interpretation of these answers should be very cautious, and should be related to the cultural and educational status of the group studied. In fact, the only sure way completely to eliminate the need for verbal recall by individuals or their relatives at the prevalence survey would be to base a study on babies initially examined at birth, and then followed-up. But such a study would be impracticable on the grounds of the very long follow-up periods needed.

The third point arises over the need not to miss new, but short-lived, spontaneously healing or "evanescent" cases that might come and go between the initial and follow-up surveys; these may form an important part of the spectrum of clinical leprosy. Related to this, it is also obviously desirable to know the form in which new cases *first* occurred; for example, if a certain proportion of indeterminate cases later become lepromatous it is clearly much more valuable to detect these cases in their early indeterminate form and then to know that they alter, than simply to find them when they have settled into an unequivocally lepromatous form, with no knowledge that they started off otherwise. There are two main ways of dealing with these problems. The first (and in theory the best) is to follow-up the whole population by frequent examinations at, say, 6- or 12-monthly intervals, so that early lesions are more likely to be found; but this method involves a tremendous amount of work for the follow-up team, and few centres could provide the necessary facilities. The second method, if the *whole* population cannot be re-surveyed at short intervals, is to concentrate resources on following up a fairly large part of it after 6 or 9 months (depending on how long the most short-lived "evanescent" case is thought to last). The absolute number of "evanescent" cases likely to be found *at any particular point in time* is not likely to vary very much. At 6 or 9 months after an initial survey, this number can be related to a defined part of the total population, and it is likely that it represents all, or most, of the "evanescent" cases that have newly developed *in that particular group.* After a 5-year follow-up interval, however, the *same* number of

cases cannot be used to calculate the incidence of "evanescent" disease, nor related to the whole population, as an unknown number of such cases will have occurred and resolved in individuals who can no longer be identified.

## Discussion

There would naturally be many problems in setting up incidence studies to define risk factors and high-risk groups. "Control" in terms of primary prevention does, however, depend on this kind of approach, and there is a growing number of centres where it could already usefully be studied in this way, as is evidenced by most of the studies shown in Table 1 and by the increasing number of large-scale approaches organized by, for example, the World Health Organization (Bechelli *et al.*, 1966, 1970), which has also actively encouraged the interest of epidemiologists who are not primarily engaged in the leprosy field (Newell, 1966). The need for large study populations can be helped by considering joint studies at several different centres, and by workers in leprosy collaborating with those interested in other diseases—an approach which may also make it easier financially, as well as offering the chance to collect additional, relevant medical data. Besides information on incidence and risk, studies of the sort suggested could contribute to knowledge of transmission, and could form a basis for studies of space- and time-clustering, which have proved themselves useful in other fields (Knox, 1963; Pike *et al.*, 1967).

It is important that a scientifically established basis for "control", i.e. primary prevention, be established in terms of risk factors and high-risk groups, *before* wide-scale prophylaxis has been introduced with the consequent inevitable distortion of the natural history of the disease. Most centres that could undertake the necessary studies already have "control" programmes in progress, aimed at early diagnosis and treatment of established cases, i.e. secondary prevention (and provision of a service, whether the main approach be primary or secondary prevention, is of course, mandatory). But the use of DDS in these programmes for *therapeutic* purposes has its main effect on the very large prevalence "pool". It is true that treating established cases may, by reducing the numbers of those who are bacteriologically positive, have an effect on the supply of new cases, but this effect is likely to be slow and small in an endemic area with a large prevalence "pool". This therapeutic effect will in any case be far less than interfering directly with the "in-flow" of new cases which, as demonstrated, is so small that any reasonably effective prophylactic measure, bearing as it does directly on incidence, will probably have far-reaching effects, certainly in statistical terms. Obviously both prophylactic and observational studies are needed at the present time, but any one centre can probably deal with only one of these approaches. Much patience and a careful weighing up of ethical considerations are necessary, but we need to know far more about the aetiology and transmission of leprosy, and about which are the high-risk groups, before we can risk spoiling the opportunities for further important observational studies.

Finally, there could be important subsidiary benefits of a clinical and service nature from studies of the kind suggested. First, population-based data of interest to the physician, surgeon, physiotherapist, bacteriologist and others, might become available. It would not be difficult or very costly, once the initial data were on magnetic tape, to extend them in time and in scope and to add additional clinical, social, and pathological data, if not on the whole population, certainly on

selected and intensively studied parts of it. In other words, the way would be open for workers in disciplines other than epidemiology, and with different interests, to base their research on defined populations, thus adding substantially to the meaning and interpretation of their results. *Ad hoc* studies of almost any kind—clinical, social, or immunological—would be greatly assisted by the availability of defined groups or samples and the information already stored about them. Secondly, the whole question of assessing the value of "control" programmes, and of evaluating what, if anything, they are achieving would be greatly facilitated.

## Summary and Conclusions

Leprosy "control" must become increasingly concerned with *primary prevention* (that is, the prevention of disease in those hitherto unaffected). *Secondary prevention,* or the detection and early treatment of established cases must naturally continue as a service, but is unlikely to contribute much towards the ultimate eradication of leprosy.

Primary prevention depends on being able to predict, more precisely than at present, "high-risk groups", towards which prophylactic measures can be especially directed. The ability to predict will come only through incidence (rather than prevalence) studies, which will be concerned with a much wider range of social, economic, demographic and medical variables than has so far been attempted. Multivariate techniques of analysis should be available. There is every reason to believe, from analogies with the epidemiological study of chronic non-communicable disease, that this approach would be fruitful in the leprosy field.

The incidence of leprosy is very low; lepromatous leprosy, in particular, should be regarded (on an incidence basis) as a rare disease. Large study populations (of the order of hundreds of thousands) are ideally needed for epidemiological studies of leprosy; work to date indicates that the obstacles to surveys on this scale are not insuperable.

Three particularly important methodological problems—namely uniformity of case-finding methods, the handling of "suspicious" cases, and the detection of "evanescent" cases—need especial consideration.

"High risk group" studies should be undertaken before the possible widespread introduction of prophylactic measures makes it difficult to carry them out properly.

Useful subsidiary benefits to clinical, pathological and social studies of leprosy would arise from the epidemiological approach discussed.

*Acknowledgements*

I am very grateful to Dr. R. J. W. Rees, Professor J. N. Morris, Dr. I. Sutherland and Mr. M. J. Gardner for help and encouragment in preparing this paper. I am also indebted to Dr. V. P. Das for his permission to quote in Table 1 unpublished figures from the Schieffelin Leprosy Research Sanatorium, Karigiri.

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