

INVESTIGATIONS OF PLAGUE.

THE terrible intensity of the outbreak of pneumonic plague now raging in Manchuria, and the presence of plague-infected animals within our own borders, have called forth recently a number of communications on plague in the daily press. A special correspondent in *The Times*, in two well-informed articles (December 22, 1910, and February 6, 1911), summarises the situation, and gives an admirable sketch of the principal facts concerning the modes of spread of plague. Dr. L. W. Sambon has also contributed two letters on the subject to our contemporary. He cites some interesting historical references to the preventive methods adopted during epidemics of plague, but it is a pity that he has allowed himself to fall into error on some essential points in the epidemiology of the disease. He remarks, for example, that in his belief transmission from man to man is probably more frequent than from rat to man. If Dr. Sambon bases this statement upon personal experience of epidemics of bubonic plague, it must be said that his observations are directly opposed to the experience of many competent plague workers. Dr. Ashburton Thompson, an accepted authority, has stated that in Sydney plague owes nothing of its epidemic form to contagion from the sick. The view that bubonic plague is not directly infectious is held unanimously by authorities in India.

The Advisory Committee, appointed by the Secretary of State for India, the Royal Society, and the Lister Institute, has recently issued a further volume of Reports on Plague Investigations in India (*Journal of Hygiene*, vol. x., No. 3). The volume contains a number of articles which cannot fail to interest all those concerned with plague administration. Briefly stated, it may be said that these investigations confirm and amplify the conclusions already recorded.

The first article deals with the experimental production of plague epidemics among animals. In earlier experiments guinea-pigs were used, and it was conclusively shown that epidemics could not be produced amongst these animals except when rat-fleas (*X. cheopis*) were present in the godowns or small huts in which the experiments were carried out. Gotschlich criticised these experiments on the ground that guinea-pigs, unlike rats, do not feed on the carcasses of their dead companions. He believes that among rats, plague is chiefly spread by the healthy animals feeding on the carcasses of those infected with plague. In order to test the validity of Gotschlich's criticism, wild Bombay rats, previously freed as far as possible from fleas, were used in the present series of experiments. The results show clearly that epidemics occur among these animals only in the presence of fleas. The Commission found no reason for thinking that alimentary infection played any part in the production of these experimental epidemics. This conclusion completely agrees with their observations on the mode of infection in naturally infected rats.

The discovery by the Commission, in the early years of their work, of chronic plague in naturally infected rats, at first sight appeared to offer a plausible explanation for the persistence of infection amongst the rat population during the off-season, and for the recrudescence of the infection when the conditions again became favourable for the epidemic spread of infection amongst rats and human beings. From the evidence available, the Commission showed considerable hesitation in ascribing to these chronic plague lesions any important part in the continuance and revival of the rat epizootic. It leaned rather to the view that the quiescent season is bridged over by sporadic cases of acute rat plague. A great deal of fresh light has been thrown upon this question in the volume under

review. A much more extensive experience of chronic plague in rats in Belgaum, Poona, and Bombay has fully convinced the Commission that the pathological appearances described as chronic plague are stages in the process of recovery from the acute disease. For this reason, and because the term has been associated with theories regarding the reappearance of the rat epizootic, they regard the name "resolving plague" as more appropriate. It is evident that the epidemiological importance of chronic rat plague is on this view considerably limited, if not, indeed, abolished.

An interesting contribution to the problem of the spread of plague through districts with numerous scattered villages, will be found in this volume. The collection and arrangement of the extensive data dealing with the recent history of human plague in three districts in the Punjab and the United Provinces were undertaken by Major Lamb, I.M.S., and a statistical analysis of the results has been made by Dr. Greenwood. While the conclusions drawn from this survey are necessarily tentative, they are of value in suggesting a rational basis for effective plague administration in the thickly populated districts in India. It would appear that reimportation of the infective agent is more likely to be the cause of outbreaks in the villages than recrudescence. Again, a study of the distribution of infected villages in maps showing the position of affairs month by month, suggests a dissemination of the infection from various centres. The statistical evidence does not point to the conclusion that the infection of a village renders it more liable to be infected during the next following epidemic.

The Commission has recorded its observations of plague during the years 1908-9 in Belgaum and Poona. The special reason for selecting these towns was that, although not far distant from Bombay, the seasonal prevalence of the human epidemics is different. It had been already shown that in Bombay the rat-flea prevalence varied at different seasons of the year, and that the season of maximum rat-flea prevalence coincided with the height of the epizootic. The intimate relation between rat-flea prevalence and the spread of rat and human plague is well illustrated in the present observations. Moreover, the interesting fact is elicited that a close connection appears to exist between the flea prevalence and the hygrometric condition of the atmosphere.

The results at Poona show that the adverse factors which combine to bring an epidemic to an end are (1) a decrease in the number of fleas, (2) a decrease in the number of rats, and (3) an increase in the proportion of immune to susceptible rats.

Mr. Sydney Rowland gives an account of his work upon plague vaccines. This contribution, which is of too technical a character to admit of a summary of its contents, describes the results of an inquiry into the immunising constituents of the *B. pestis*. The results obtained are interesting, and suggest important improvements in the method of preparation of plague vaccines.

The volume concludes with a brief statement of the provisional conclusions reached by the Advisory Committee as the result of the investigations made under their direction from 1905-9 into the mode of spread of plague in India. The Committee concludes, that in nature plague is spread among rats by the agency of rat fleas, and that, in the great majority of cases during an epidemic of plague, man contracts the disease from plague-infected rats through the agency of plague-infected rat fleas.

A perusal of this volume of reports must impress the reader with the enormous amount of work entailed in order to collect the evidence leading to these conclusions. In this country it is still little under

stood, amongst even the intelligent public, that the scientific study of disease can be effectively accomplished only at the outlay of much time and money. It is gratifying, therefore, to find that in the ably written article on the outbreak of rat plague in Suffolk, which appeared in *The Times* of December 22, 1910, the writer emphasises this point with refreshing candour. He insists that in a crisis of this kind, the effort to cope with the situation must be a national one, and that the Government must authorise the expenditure of ample funds to provide for the establishment of a staff of experienced investigators and administrators to deal with the problem. The history of the organisation of plague measures gives ample proof of the futility of adopting plans, however vigorous, that are not based on clear conceptions of the disease gained by scientific research.

WHAT SCIENCE HAS DONE FOR THE WEST INDIES.

A LITTLE more than a year ago I told in these pages, with a very sore heart, the story of what the late Sir Alfred Jones had accomplished for the West Indies by enlightened commercial methods. That chapter is unhappily closed, for no one has succeeded him. It is a more hopeful task which is now imposed upon me—to give some account of what science has done, and will continue to do. It is worth the telling, and it is more than a mere record of success, but carries a moral of far-reaching extent.

This journal, from its first number, has never ceased to preach the necessity of applying knowledge to the right conduct of human affairs. It continues to preach, and in face of the stolid conservatism of our methods, one might in a despondent mood think with little effect. But if one looks back over long periods it is not so, and the change in public opinion as represented by governmental action is little short of astonishing.

When I first became engaged in colonial work some forty years ago, the doctrine of *laissez faire* was in full swing. It was held that self-interest would determine whether an industry would succeed or fail; if it failed it deserved to do so, and another would take its place. In either case it was best to leave it severely alone. This is not the place to discuss how far such a doctrine is sound. But practically it is continually being abandoned. No industry is now free from governmental interference, and such interference is only tolerable if directed by adequate technical knowledge. Interference must always be of the nature of restraint, and at any rate theoretically one may ask whether some compensation is not justified. It can hardly be doubted that the community will have more and more to provide knowledge for industry of the kind that self-interest is powerless to provide for itself.

Mill, however, and other economists clearly saw that academic economic principles were not universally applicable to agriculture. The reason is obvious: the soil is not removable, but has to be utilised as best it can, and where it is. If it went generally out of cultivation food would fail. It was still, however, left to *laissez faire*, except in some measure in India, where the Government undertook the pioneering work in regard to tea, cinchona, rubber, and some other staples, and then left their commercial development to private individuals. In any other country but our own the work of Rothamsted would have been promoted by the State. There are undoubtedly advantages in scientific research being left unfettered to individual effort, but it is only the richest landowners, such as Coke of Holkham, and the Dukes of Bedford,

who can afford to add to agricultural knowledge by experiment. The average cultivator is powerless to follow other than traditional methods. Yet it is in the interest of the community that he should do better in order that the maximum return may be obtained from the land.

When this country began to acquire tropical possessions, it was seen, however, that something more than *laissez faire* was required for their economic development. It was the Royal Society, at the hands of its president, Sir Joseph Banks, who first took the work in hand. Having the ear of the King, he was able to use Kew, which was then the private property of the Royal Family, for the purpose. The mutiny of the *Bounty* was an incident in an attempt to add to the cultural resources of the West Indies. An indirect result was the foundation of the great Dutch colonial botanical establishment at Buitenzorg. When it was decided that Kew should be maintained as a national establishment, its colonial utility was apparently one of the main reasons for the decision. In a scheme which received the sanction of Parliament the interests of "commerce" and "agriculture" were recognised, as well as the supply "of authentic and official information on points connected with the foundation of new colonies." Its functions in this respect were steadily fostered by the Hookers, father and son. The history of Kew thus affords one of the earliest instances in this country of the recognition of the duty of the State to promote scientific knowledge in the public interest. And the historic meaning of the controversies which have occasionally brought Kew prominently into public notice is simply the attempt of a policy of *laissez faire* to arrest its work.

But anything which is rooted in sound principles cannot be checked, because their necessity insists on asserting itself; and the West Indies again supply the illustration. Obviously their chief asset is solar energy. Our channel islands supply us with early vegetables. In a rule-of-three sum the West Indies stand for the channel islands of the North Atlantic shores. Alfred Jones saw this, and started a line of steamers to flood us with West Indian fruit. But this is anticipating. In the nineties their condition was the reverse of prosperous. And, if it is a paradox that science was indirectly the cause of the mischief, it happily was able to supply the remedy.

The Napoleonic empire left behind it two permanent legacies—the French code and beetroot sugar. When Napoleon's continental system closed the ports of Europe to British colonial produce, the import of tropical sugar was cut off. As sugar is a necessity of modern food there was the strongest impulse to find a new supply. I need not repeat a well-worn story. The chemist and the cultivator lavished all their resources on the unpromising beet, and ultimately dethroned the sugar-cane. Then came the bounties which flooded this country with sugar at scarcely more than cost price, and drove cane-sugar out of consumption.

There is a fundamental principle in agriculture: never to trust to a single crop. Ireland trusted to the potato and Ceylon to coffee, and both failed them; this was from disease. The West Indies trusted to sugar, and in their case the ruin was economic. The balance of solar energy being in its favour, on equal terms the cane should at least hold its own with the beet. But now comes the mistake and its moral. The sugar content of the cane was held to be incapable of increase; the methods of manufacture were often archaic and wasteful. Beetroot-sugar was the product of the most refined scientific skill in both directions. It was the fable of the hare and the tortoise.