

A CONTRIBUTION TO THE AETIOLOGY OF PLAGUE.

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THE subject of the following remarks is the mode of spread of bubonic plague in epidemic form as deduced from observations made during the outbreak at Sydney. The nature of the disease, and the whole of the circumstances which accompanied its appearance, having been described at length in my official report, only those points which have a direct bearing on the subject just defined are here mentioned; but a brief preliminary statement of certain local conditions is necessary.

Sydney lies on the eastern coast of Australia in S. Lat. $33^{\circ} 52'$ and E. Long. $151^{\circ} 14'$, and occupies an area of about 256 square miles of sandstone country much broken up by deep fjords. The estimated population of the metropolitan registration district was 438,300 on December 31st, 1899; that of the metropolitan municipalities combined for purposes of sanitary administration was about 456,000. The inhabitants are white; there is among them the sprinkling of coloured people found at every seaport, and a small colony of Chinese which at the last census numbered less than 4000, and which has since diminished. There are no aboriginals, their number being now reduced to less than 8000 in the whole State. The whites are of European extraction; at ages below 15 more than two-thirds are of Australian birth, but at older ages they are largely of immediate European descent or actually of European birth. The members of foreign nations among them are proportionately few. They may be reckoned as English on the whole, and although they inhabit a subtropical climate their institutions and personal habits do not differ much from those of Northern Europe: 293 of the whites were attacked with plague, of whom 95 died; and 10 Chinese, of whom 8 died.

The sea-trade of this port is great, and carried on with almost every part of the world; the total tonnage which entered in 1899 was 2,589,457. Consequently it has been exposed to risk of importing the infection of plague since May, 1894, when the disease first became epidemic at Hong Kong; its distance therefrom by steam is 3 weeks, and 2 lines of steam-vessels furnish a monthly service each; besides which other steam-vessels call regularly during their season, while others still arrive all the year round after touching at various Chinese ports including Hong Kong. Trade with India is almost as great, and with some other infected ports it is regular and considerable; the following are those from which Sydney was chiefly threatened, the date of infection being affixed to each name: Bombay (September, 1896), Calcutta (about or before March, 1898), Mauritius (officially declared February 27th, 1899, but cases had occurred at Port Louis during December, 1898); Kobe, Japan (during December, 1899); Honolulu, H.I. (December, 12th, 1899); and Noumea, New Caledonia (officially declared December 24th, 1899). From 1894 the treatment accorded to all vessels which arrived from plague-infected ports was practically that accorded to clean vessels arriving from cholera-infected ports by the earlier International Conventions; and no vessel ever has arrived which carried or (as far as very careful enquiry and examination of logs revealed) which had carried, either a case or a suspected case of plague. But this practice was varied when the infection of Noumea became known. This port lies but from three-and-a-half to six days' steam away according to the class of vessel making the voyage; and in accordance with the Venice Convention, 1897, ships arriving thence after December 24th were detained at quarantine until expiry of the 12th day from commencing the voyage—2 days having being added to the 10 prescribed by it merely because the French Government had directed its representatives in French colonies to impose 12 days' detention on vessels arriving at their ports. As regards the source whence Sydney immediately derived its infection, all that can be said is that it escaped until shortly after the disease had been admitted to be epidemic at the neighbouring port of Noumea.

We now pass on to enquire whether the first recorded case at Sydney were the first case in fact, as at this present date it is universally believed to have been. Comparison of death-rates under several causes with those for corresponding periods of former years showed that they were rather below the average; and there is no reason at all for suggesting that unobserved plague had caused such fatality as could

impress the register with unusual features. The evidence, therefore, is presumptive. It consists, first, in the ability, public spirit, and number of the medical profession in Sydney; in the racial and social characteristics of the population, and their habit of seeking medical advice on the least occasion; in the prevalence of benefit clubs; and in the number, size, and accessibility of the public hospitals, and of cognate institutions under management either of charitable committees or of the State Government. And, secondly, it consists in the alertness of the medical profession, and the fear felt by the general public. Both had been effectually aroused by news of the infection of Noumea; both had been stimulated on January 15th by an alarm, not clearly seen to be false until long afterwards, of the infection of Adelaide. Lastly, the nature of the first case was publicly announced on January 24th, and was made the subject of lengthy articles in the public press immediately afterwards; and although an interval of 31 days elapsed before the second case became known, only one really doubtful case was referred to the Department of Public Health for diagnosis during this interval; nor at any later time was it professed that cases or doubtful cases were recollected. The exception occurred in a man who presented bilateral, inguinal buboes, in every superficial respect resembling venereal buboes, to the cause of which the most minute enquiry into his habits, occupations, associations, and clinical symptoms, assisted by a prolonged bacteriological investigation, furnished no clue on the one hand, but on the other no support to the suspicion which had made an accurate diagnosis desirable.

Under these circumstances a carman, regularly employed by the Central Wharf Co., was suddenly attacked with severe headache at mid-day on January 19th; 4 hours later he felt some pain in his groin, and found a swelling there. He summoned medical advice on the 20th, and his case was at once reported to the Department. By the 24th rigid proof of the nature of the illness had resulted from the cultural and inoculation tests applied to sanious lymph withdrawn from the swollen gland. This man's business during several months past had been to cart exports from city warehouses to Central Wharf, which was situated rather easterly of the entrance to Darling Harbour; but occasionally to fetch packages which were in course of transshipment from other wharves to that of his employers. It was ascertained from his employers' books, however, that he had had no business at any other wharf for 10 days before the date of attack. Vessels from infected ports had lain at neighbouring wharves during the latter two months

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of 1899 and down to January 19th; namely, from Hong Kong, India, Mauritius, and Noumea, and 4 from Hong Kong at Central Wharf itself. He had no business on board any ship, and said that for long he had not boarded any.

The second case was diagnosed from examination of inguinal glands removed after death, and brought to the laboratories, on February 24th, or, as mentioned above, 31 days after identification of the nature of Case I. It was followed by a third the next day, and by others immediately afterwards. The table below shows what was the progress of the epidemic in time, its extent, and the number of deaths:—

TABLE I. *Showing the number of attacks and deaths recorded during each week.*

	Week ending	Cases	Deaths
	20th January	1	0
	27th January	0	0
	3rd February	0	0
	10th February	0	0
	17th February	0	0
1st week	24th February	2	1
2nd "	3rd March	2	1
3rd "	10th March	5	3
4th "	17th March	12	3
5th "	24th March	10	3
6th "	31st March	23	6
7th "	7th April	29	9
8th "	14th April	29	12
9th "	21st April	16	8
10th "	28th April	26	7
11th "	5th May	38	10
12th "	12th May	23	10
13th "	19th May	24	10
14th "	26th May	7	6
15th "	2nd June	17	3
16th "	9th June	4	3
17th "	16th June	10	3
18th "	23rd June	6	0
19th "	30th June	12	3
20th "	7th July	1	0
21st "	14th July	3	0
22nd "	21st July	2	0
23rd "	28th July	0	1
24th "	4th August	0	0
25th "	11th August	1	0
26th "	18th August	0	1

The weekly notifications showed stages of increase, stasis, and decline in the epidemic. During the first 3 weeks only 9 cases occurred, and they were pretty evenly spaced out. During the 4th and 5th weeks 22 were notified. In the 6th the epidemic became established, and so continued for 7 weeks more; two-thirds (208) of the total cases happened during these 8 weeks. The period of decline set in with the 14th, and continued through the 19th week; it was marked by great irregularity in the number of cases notified, the series having been 7, 17, 4, 10, 6 and 12. The epidemic then ceased. The 20th, 21st, and 22nd weeks yielded but 1, 3, and 2 cases, while the last case of all was noted in the course of the 25th week.

It is important to remark that the contagium had its full virulence from the beginning. The mortality was heavy from February 23rd, when Case 2 died; yet among those which immediately followed it were some which did not exceed Case 1 in severity. The only change observed in the contagium was enfeeblement. This began about May 1st; it was recognised on comparing the state of patients on admission to hospital after May 1st, with the state of admission of those received before May 1st, at corresponding dates of illness. Two other points require notice in this connection. One is that whereas it had taken 7 weeks to furnish the first 100 cases, and 5 weeks to furnish the second, 13 weeks elapsed before the 303rd case had been recorded. The other is that no ambulant cases occurred until quite late in the third period. In addition to the general considerations adduced when the actual priority of Case 1 was being discussed and which may be referred to in support of this statement, mention may also be made of the records which show that throughout the epidemic, 221 suspected cases were referred to the Department for diagnosis, many of them by 69 medical men.

The following table, in which the fatality of the disease in the first and second hundreds, and in the remaining 93 (the 10 Chinese being omitted), shows a diminution in accord with the above clinical observation. But it is not cited in proof of it, because on May 13th Yersin-Roux serum became available, and thereafter was steadily used.

TABLE II. *Comparing the fatality of the disease among three arbitrary divisions of the cases which occurred among whites, the ten Chinese having been deducted as shown below.*

	Less Chinese	
	Cases	Deaths
100 cases, Jan. 20, to April 12*, fatality 37 %	1	1
100 cases, April 12, to May 9*, fatality 37 %	5	4
93 cases, May 9, to August 9, fatality 23 %	4	3

* The cases which occurred on these days have been divided.

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The epidemic was not maintained by any of the usual modes of spread. As to direct communication, it appears, after certain cases have been deducted which cannot be justly cited, that it took 276 households to furnish 289 cases; that the number of persons exposed to primary patients was 1752; and that the duration of exposure was as shown in the table:

TABLE III. *Showing the day of illness on which 289 patients and households were removed to isolation. Also the number of secondary cases which occurred after isolation in four households.*

Day of illness	No. of cases	No. of contacts	No. of cases among contacts attacked in isolation
1st day	10	156	—
2nd „	35	170	—
3rd „	77	458	1
4th „	53	320	5
5th „	31	240	—
6th „	27	142	—
7th „	14	66	—
8th „	13	91	—
9th „	7	40	1
10th „	5	24	—
11th „	2	13	—
12th „	1	2	—
13th „	1	1	—
14th „	1	5	—
17th „	1	2	—
20th „	1	1	—
22nd „	1	—	—
46th „	1	—	—
Uncertain date	1	21	—
	282	1752	7

The 7 secondary cases are those of persons who fell ill in isolation. But in fact, 10 households yielded 13 secondary cases; one yielded 4, the others 1 apiece. They occurred under different circumstances: The 7 just mentioned (4 households) began within 3 days of separation from the primary patient and the dwelling; 3 others occurred before removal; while in the remaining 3 the patients fell ill after removal, cleansing of the dwelling, disinfection of its contents, and reoccupation on the 6th day—2 of them while the primary patient still remained in hospital, one after he had rejoined the household. It is clear that the epidemic was not maintained by direct communication and, while the disease can at most have been directly communicated but exceptionally,

the facts concerning these cases raise a strong presumption that it was not so communicated even in them.

As regards mediate communication the foregoing statement also suffices to show that it did not operate within households; but then, it could not have operated outside them either. Inacquaintance of the members of infected households with each other, and separation of the dwellings they occupied by distances which were either literally great or which, in relation to city conditions, were practically great was a marked feature of the earlier cases of the series; and when, as rarely happened, it was found that the patient had been acquainted with one previously attacked, it turned out that the link between them was not family acquaintanceship, but association at the same place of employment. It was impossible to imagine how mediate communication could have operated, at all events on the requisite scale. A great majority of the cases occurred in the families of respectable and provident artisans; this class does not employ laundresses (and as a matter of fact no laundresses were attacked); they do not either choose the first days of an illness which is usually alarming from the beginning to disperse their household goods (and only one pawnbroker's assistant was attacked, quite early in the outbreak). Usually within 10 hours, always in less than 24 hours from notification, the patient and the inhabitants were removed, the infected house was isolated, its contents were in part removed for disinfection, and in other part were cleansed together with the house itself; these latter operations being always completed within 5 days, during which the police interdicted communication with the premises. Unconscious communication of infection to articles of commerce alone remains; here, again, the suddenness of attack and its rapidly incapacitating character must be referred to; in almost all cases it was for this reason hardly possible that infection could have been communicated to them. In short I have no more doubt that the disease was not spread by mediate, than that it was not spread by direct, communication from the sick. The facts negative both beyond reasonable doubt.

It is hardly necessary in these columns to say that there is no ground for supposing that the infection was spread with food and taken by ingestion; the evidence that man can be thus infected with plague, though perhaps good as far as it goes, shows also that he is so infected very rarely. It may be added as regards water, concerning which all that is known is that the bacillus can survive in it for a variable number of days according to its quality—not that animals can be

infected by drinking water carrying *B. pestis*—that Sydney as a whole is supplied from one primary source; that more than one service-reservoir must have been infected to account for occurrence of the disease over the whole area, either from which cases were removed or on which the infection was taken; that the village of Manly where a distinct sub-epidemic occurred has its own separate catchment-area and works; and that the time and place distribution of the infected premises negatives diffusion by this means.

The most important indication of the mode of spread has already been mentioned in connection with inacquaintance between the members of successively infected households. When the total infected houses were charted it appeared that they stood in almost every neighbourhood on the extensive area mentioned at first as being occupied by the city. But as soon as the cases were charted, not in accordance with their place of residence but with their place of employment, it appeared at once that a majority were associated by resort to a particular part of the city; and even in many instances by resort to the same house of business at which they were employed. It was still more singular to note sometimes that they were often employed in quite different departments of the same great establishment, and were hardly acquainted with each other by sight. It became obvious—notwithstanding certain apparent exceptions—that infectivity attached in some way to localities and even to premises; and yet the percentage of attacks among the persons resorting to the locality thus first identified was in all likelihood almost infinitely small. Natural resistance to this infection is probably a negligible condition; it seemed, therefore, that while the infection was in some sense or other localised, resort to the locality involved no great risk. Some special condition rarely existent, seemed to be necessary to permit communication of the virus to man.

However, following this indication the progress of the epidemic in place must be examined as well as may be without maps. What was observed was this. The primary focus of infection having been identified in the manner described, its bounds (never sharply defined, of course) were seen to extend by continuity; it gradually covered to the east the whole of a strip of the city between Darling Harbour and a line of parks a mile long; but, though these spaces remained open to the usual traffic and were traversed by thousands of persons daily, it was there permanently stayed. It never attained the populous city district beyond the parks. On the other hand it did continuously

extend to the south where there was neither water nor open spaces; but again its extension was sharply and permanently limited to the east by a small park, a small reserve on which barracks stand, a cemetery, and thereafter a railway terminus and the permanent way running from it. It spread on a southerly course, thus limited to the east, for two or three miles; and as the distance from the starting point increased the percentage of invaded houses diminished. Now, between the northern end of the last mentioned spaces and the southern end of those first mentioned, is a gap occupied with streets in the usual way. The infective area extended as before by continuity over these streets; and having thus attained an outlet it subsequently spread two or three miles along and in the neighbourhood of the eastern highway. That is one set of clearly distinguishable facts. But there was another. Almost from the beginning outlying cases had occurred which could not then be connected with the original focus; their serial numbers were 4, 6 and 7, the latter representing that household which yielded 5 cases. It will suffice for the present to refer to it alone. Case 7, M. aged 2 years, occurred on March 8th, and was then isolated not merely by position of the house which stood two miles from the original focus at Darling Harbour, but also by its being impossible to connect the patient with the latter in any way. Eleven days later Case 23 happened, not near 7, but about half a mile away to the west. In the neighbourhood of the house occupied by 23 other cases were met with shortly afterwards; and eventually a large area over which the infection spread from east to west instead, as happened on the primary focus, from west to east in the first place, yielded indigenous cases. After spreading westerly it extended northerly. Thus it appeared as though an independent centre had spontaneously arisen, from which, however, the infection was diffused in the same continuous and comparatively slow manner, as from the original centre. A similar event was witnessed in relation to the village of Manly, which has a population of about 3000, and is so placed as to have frontages to the harbour on one side and to the Pacific on the other. It is a favourite resort visited by several thousand people on holidays; its inhabitants are also for the most part engaged in business in Sydney. It is reached by a seven-mile ferry journey which occupies half-an-hour; and can be otherwise reached only by a land road 13 miles long which involves crossing water twice by ferry. On May 1st Case 164, and on May 2nd Case 175 (unconnected with the former) were notified; and thereafter a total of 9 cases occurred on a comparatively small area within a few hundred yards of the main pier, which

itself yielded one case in a person who lived at the refreshment room built on it. Here again the proportion of persons attacked to those habitually crossing the area was exceedingly small; it was also insignificant in relation to the numbers who inhabited the area.

Lastly, while occurrence of single cases in households was above cited as evidence that the disease was not directly or mediately communicated from the sick, it can now be adduced (but with reference only to those houses which yielded what were judged to be indigenous cases) in proof that the infection might and commonly did exist on premises, and yet rarely attacked more than one person. Thus, again, it seems that something more than neighbourhood of man to the source of infection was requisite to diffusion of the disease, and something which (in individual houses) rarely existed. This was even clearer when on business premises which harboured during the daytime from one hundred to several hundred workpeople, only from 2 to 5 persons apiece were attacked, all of whom probably, and a majority certainly, received their infection within them.

The object with which the above observations have been set out in the foregoing manner is to show that as soon as plague occurs among a wholly civilised white population, and therefore under circumstances which permit cognition of all the important facts, it appears at once and clearly that this disease is diffused by none of those means which are effectual in causing (for instance) epidemics of influenza, or of cholera. In its mode of spread it plainly resembled in some important respects the epizootic Tick-fever. No theory could be devised, I think, which would coordinate the observed facts unless it assumed at all events an animated host which should not be human for the infection. In fact two such hosts are requisite; one to diffuse the infection in place, the other to communicate it to man. It is hardly necessary to say what we now believe these two to be; but it is likely that a majority who have not had our practical experience still regard them with the same doubt with which a majority among ourselves regarded them at the beginning of 1900. They are the rat, and a suctorial parasite; and in connection with them the names of Hankin and of Simond must be mentioned, whose papers alone among a large mass of writings appear to me to possess a real and great epidemiological value¹.

¹ EDITORIAL NOTE. The literature on the relation of insects to the dissemination of plague and other diseases has been exhaustively treated by Nuttall (see *Journal of Hygiene*, Vol. 1. p. 77 for reference; also *Centralbl. f. Bakteriologie*, xxiii. p. 625, and

Case 1 presented a feature which has not yet been mentioned. The bubo occurred in the lowest gland of the femoral chain on the left side; and in the external retro-malleolar space of the same extremity I observed a circular, purplish-red spot rather less than 3 mm. in diameter. The cuticle, which had been raised, had fallen and was then adherent to the cutis; at one point of the circumference of the spot it was ragged. There were the remains of a very small bleb; and, in accordance with the received opinion that the infection is commonly taken by inoculation, it appeared to indicate the site of inoculation. It also seemed more probable that inoculation had been effected into the delicate skin of this part of the body, and in a situation which was well protected from ordinary injuries of the kind by the boot and sock the patient invariably wore, by a suctorial parasite than in any other way. The inference was drawn that there were already in some part of Sydney rats which had died of plague, and search was at once made for them both by advertisement and in more direct ways. It subsequently appeared that a mortality among the rats at a wharf at which an epizootic first became manifest, had been observed early in January; but nothing was discovered at the time (there being nothing to direct attention to the neighbourhood of this particular wharf) until February 14th. A landing-waiter then reported that he had first observed unusual mortality among the rats there on February 9th or 10th. Sick rats and carcasses were at once collected; and in the course of five days the disease in them had been rigidly proved to be plague.

Dropping, for a moment, the assumption that the epidemic was a consequence of the epizootic, we have to enquire whether it possibly could have been so; that is to say, whether plague-rats became sufficiently diffused over the several areas to account for their infectivity. In the first place, then, the presence of plague was rigidly demonstrated by the usual cultural and inoculation tests, by Dr Frank Tidswell, Micro-biologist to the Department, in 17 rats which were taken on premises at widely separated points of the original focus, where the incidence of the disease was heaviest; in other two taken at the

xxii. p. 87, and *Hygienische Rundschau*, 1899, Vol. ix.). A large series of experiments with *Cimex* and *Pulex* have given uniformly negative results with animals suffering from various septicaemic affections (anthrax, chicken-cholera, mouse-septicaemia, plague). See also Galli-Valerio (*Centralbl. f. Bakteriologie*, 1900, Vol. xxvii. p. 1; 1900, Vol. xxviii. p. 842). The experimental data presented in these publications (1897—1900) do not tend to support Simond's hypothesis.

distant suburbs of Manly and Woollahra; and in one cat sent in from a house in which no case of plague occurred, but which stood on an infected area. Secondly, sick or dead rats in greater or less number were seen by the disinfecting corps at 70 houses in which plague had occurred, and which were scattered over every part of the various infected areas; they were noted (and often seen) by Dr W. G. Armstrong, the medical officer of health for the metropolitan combined sanitary districts. It is worth noting that the residents in these houses often knew nothing about the rats, which were only discovered in course of cleansing operations. Lastly, in many other cases the presence of sick or of dead rats on infected premises was reported by common observers. Briefly, the area over which the epizootic extended coincided with the area over which the epidemic was seen to have extended after all cases had been referred to their probable place of infection—a locality fixed upon after carefully considering the separately recorded facts concerning each of the 303 cases in man.

To gather and record the foregoing observations required merely intelligence, industry, and perseverance, all of which qualities were conspicuously shown by the members of the Staff over which I have the honour and good fortune to preside. It is afterwards that the real difficulty is encountered. How can a septicaemia of the rat be so frequently communicated to man as to give rise to an epidemic? Many must have asked this question and, until an answer was suggested by Simond, must have hesitated to admit more than concurrence between plague in man and in rats. My object being merely to record our experience in this place, I need only express the opinion that the communication is effected by fleas very commonly and, indeed, usually. To be bitten by a suctorial parasite which has lately bitten a plague-rat, is I think, that special circumstance not (in any individual household) commonly encountered, the need for which has been suggested more than once above. It accounts both for the erratic incidence of plague on houses, and for its erratic incidence on the inhabitants of each house.

Our evidence as regards fleas is the following. Excluding Case 1 the patients were searched for the phlyctenulae or the bleb described as occasionally resulting by Simond; but this search was not systematic. Phlyctenulae were noted on the area of skin drained by the gland which furnished the primary bubo in 6 cases; this was but a small proportion, however, of those who were examined. Two of these phlyctenulae were still surmounted with a minute unbroken vesicle. From each smears

were made; and in one a bacillus morphologically resembling *B. pestis* was seen in small numbers. Secondly, narcotised fleas taken from plague-rats were examined in small number by the Micro-biologist, and in one *B. pestis* was found; in this case its identity was proved by inoculation into a guinea-pig. Nine fleas removed from rats were referred to the Government Entomologist for identification; he pronounced two of them to be *P. serraticeps*, the remainder *P. fasciatus*—described by Bosc as the rat-flea, as far back as 1801.

We are now at liberty to revert to Case 7, which has been left unexplained so far. The cottage in which it occurred stood within a couple of hundred yards of a place where the refuse of the city is still dumped, and among it much rotten fruit and other vegetable matter from the infested wharf and others adjoining it. It is on every ground probable that the carcasses of deceased plague-rats were thus carried to the dump, and there devoured by the horde infesting it. At all events dead rats were found after attack of Case 7 in a little outhouse attached to the cottage where the children of the family habitually played; and it happened that the only persons attacked were 3 other young children who alone frequented the outhouse, and their father who cleaned it. The premises moreover were found by the disinfecting corps to be infested with fleas in quite extraordinary number; and the bodies of the younger children were almost literally covered with their punctures. Case 4, it may now also be pointed out, worked for a hay and straw dealer; the patient himself had not for a fortnight at least been near Darling Harbour. But his employer got his supplies from a wharf at back of the house occupied by Case 2 (and this patient had removed 5 dead rats from a water-closet 2 or 3 days before his attack), where the bales of hay, etc., often lay for some days before being removed. Rats were probably conveyed to Manly by ferry-boats, which daily carry both provisions and produce from the same set of wharves; one of the cases happened at the pier, where many dead rats were found, and all the remainder either in persons who frequented the pier, or who lived in houses within two or three hundred yards of it.

The epizootic was first manifested at one of a line of wharves frequented by ships coming from foreign ports, and among them were some from ports known to be infected, including the port of Noumea (New Caledonia). It seems most probable by far that it began in the landing of plague-rats. The alternative seems to be importation of the infection with merchandise, communication of it to the locality, and passage thence to rats. But, while many cities have for years past

daily received immense quantities of merchandise shipped at, and even coming from, epidemic areas, and yet have for the most part escaped, it never has been satisfactorily shown that plague resulted from importation of trade under circumstances which excluded the intervention of rats, or of insects, or of both. Besides, in the present case it has to be supposed that infection which neither attacked susceptible rats on board (for as there was nothing to prevent the ship-rats from landing, if the contrary be supposed *cadit quaestio*), nor equally susceptible men ashore, did nevertheless infect shore-rats after a sojourn in the soil. Introduction of a hypothetical soil-stage seems superfluous, although no doubt the bacillus can rest in soil.

The notion that plague can be epidemically diffused by agency of the soil has received unacknowledged support, in all probability, from the preponderating occurrence of groin-buboes among the bare-footed population of the East. But the facts do not at all support it. Of those of our 303 patients who exhibited buboes at all, namely 286, no less than 73 per cent. had them in the region of the groin, and nowhere else. Yet the inhabitants of Sydney no more go barefoot than do the inhabitants of London.

I conclude these remarks with a word or two on management of a current epidemic. The word "contact" is much in use; it has a certain convenience, but unfortunately no defined meaning. It does not necessarily mean one who has lately been associated with a plague patient. It means one who has been exposed in more or less close association with plague-rats within five days past (but sometimes with a septicaemic case in man, or with a person dying or dead of plague in any form). If the case probably arose within the house at which it has been discovered, it will be prudent, and it may be necessary, to remove the contacts until the premises have been cleansed. Evacuation of infected localities is the only measure which can be proved to have been useful in India or elsewhere, and I have no doubt of the reason. In a civilized community isolation of contacts must not however be indiscriminately enforced. The patient has often taken the infection away from home, and in fact his illness predicates nothing of the rest of the household.

The question whether the sick must be isolated is to be discussed on different grounds. In the first place, plague can be communicated from the sick, though epidemics are not maintained by that means. Primary plague pneumonia is infectious; secondary pneumonia is not uncommon, and the sputa then often carry the bacillus; discharges

from buboes always carry the bacillus at first, and according to Calmette and Salimbeni generally continue to yield it in viable (though perhaps not necessarily in virulent) form for many days; the solid and liquid excreta carry it, at all events when submucous haemorrhages discharge into bladder or bowels. Infectious discharges should not be turned into sewers except after disinfection; but special care to disinfect them should be taken with plague, because of the possibility that rats may contract the disease from them in the sewers. I believe this possibility has not yet been experimentally investigated; but Indian records are not wanting in instances which, if they do not prove that plague in man has preceded infection of the local rats, yet suggest that this may sometimes have been the true sequence of events. So that both for the sake of preventing those cases which would be likely to arise occasionally by direct communication—a misfortune which would affect only the household to which it happened, as well as for avoiding the risk of infecting sewer-rats with the disease—which would be a public misfortune, there is ample reason for collecting and destroying everything thrown off by the diseased body. That Authority would be imprudent which left this to the care of individual householders. All the sick must be removed to isolation whenever possible; when it is not possible they should be placed in charge of nurses responsible to the authority and (in all matters relating to prevention) under direction of its own Medical Staff.