

THE PRESENT POSITION OF THE RAT-FLEA THEORY OF PLAGUE; RECENT OBSERVA- TIONS IN CALCUTTA.

By W. C. HOSSACK, M.D. (ABERD.), D.P.H. CAMB.
District Medical Officer, Calcutta.

(*Friday, December 18, 1908.*)

Introductory.—Doubtless to many of you the rat-flea theory of plague—that is to say, the explanation of human plague as due to the bites of plague-infected rat-fleas—is one of the established facts of modern medicine. In short, it is rapidly acquiring the status of a text-book dictum, as exemplified in Bullock's article in Clifford Allbutt's "System of Medicine," Roger's "Tropical Fevers," and, with reservations, in Manson's last edition of his "Tropical Diseases." By most of the medical journals the theory is accepted without any reserve at all. In fact, there is a certainty that unless the other side of the question is promptly and vigorously put forward, a stage will be reached where the matter will be practically beyond discussion. This stage I am anxious to anticipate. My reason for doing so is that my knowledge of plague is based on ten years' experience of practical plague work—that is to say, of carrying out measures framed in accordance with accepted theories. Unfortunately, amongst my experiences is that of devoting all my energies to the carrying out of measures which I knew to be comparatively futile, the futility of which I subsequently demonstrated up to the hilt by an experiment on an enormous scale. I refer to disinfection with 1 in 500 acid perchloride of mercury, under Oriental conditions. Let it

be remembered that measures based on a wrong theory necessitate not only the waste of hundreds of thousands of pounds of public money, but also the impossibility of following out other lines of action which seem to promise definite beneficial results. As an example, demolition of infected centres was deferred in Calcutta for fully two years owing to the prevalence of erroneous ideas as to the value of disinfection.

To bring home to you how intimately the theory of plague is connected with practice, an incident in Calcutta may be given. The Chairman of the Corporation had just listened to a lecture in which the current *Mus rattus* theory of plague had been given due prominence. His first remark after the lecture was: "According to what we have just been told, our measures for rat-destruction may do harm rather than good. The brown rat kills the black, which is the one that causes plague; and if we trap more brown than black we will cause more damage than good." It was only when he realized that the current views put forward did not represent the opinion of the lecturer that he was reassured, and went on with rat-destruction.

In view of these considerations, it will be clear that if I have any doubts as to the truth of the rat-flea theory, it is my duty not only to bring forward the facts against it, but also to call your attention to what fallacies there may be in the evidence in its favour. For, as has just been indicated, the measures on which depend the lives of millions, and the expenditure of hundreds of thousands of pounds, are framed in accordance with theories; so that no personal or other considerations can interfere if the verification or otherwise of these theories is in question. This is my justification for what might otherwise be deemed unnecessary and captious criticism.

History of the Rat-Flea Theory of Plague.—The most desirable mode of dealing with the subject would have been the purely chronological, but a difficulty has crept in. There are first the early stages of the theory based on the work of Yersin, Tirolia, Hankin, Nuttall, Ogata, and Simond. These I shall only mention. Next comes the middle stage of the rat-flea theory—the stage inaugurated by the epoch-making experiments of Liston with guinea-pigs in Bombay. Here we have the theory in its prime, but only indicated in Liston's pamphlet, not quite definitely put forward in print. But, though not in an authoritative print, it was to be found in many of the medical papers and in the mouths of all Indian officials—medical and otherwise. The theory then was a beautiful and splendid production, “*teres totus atque rotundus.*” It fitted in with all the known or assumed conditions of plague. Plague was most rampant where the *M. rattus* was most prevalent; its season corresponded with the season of the maximum prevalence of rats as indicated by the breeding-season; and, finally, plague was correlated with the maximum prevalence of rat-fleas, which in turn was correlated with the habits of *M. rattus* as compared with the habits of *M. decumanus*. The former was a roof-haunting rat liable, when dead, to drop his fleas on man; the latter a sewer and burrowing rat which left his fleas on his demise in his burrows where they could not attack man.

In 1906 began the issue of the reports of the Advisory Committee, or the Commission, as I shall call it throughout. Their experiments and observations all pointed in one direction—the prepotency of the rat-flea as the cause of human plague, but no definite final conclusions on the subject were published until the production of “The Etiology and Epidemiology of Plague,” a summary of the

Commission's work and conclusions. Here comes the chronological hitch. I recently read a paper before the Calcutta Medical Society in which I criticized many of these conclusions under the mistaken impression that they represented the findings of the Commission. I am now informed that this pamphlet represents not the official views of the Commission, but the personal views of the member who drew it up, Major Lamb, I.M.S. I must express my regret at the error I have fallen into in attaching to the Commission misdirected criticisms. The criticisms, of course, stand ; but they should have been directed not against the Commission, but against Major Lamb. The only excuse I can give for the error is that, as far as I know, it is still widely prevalent in India.

The M. rattus Theory.—I have already indicated what this theory was in its prime, how it was supposed to fit in with the prevalence of *M. rattus*, with the breeding season of *M. rattus*, the habits of *M. rattus*, &c. So little doubt was there about the predominant part played by the *M. rattus* that the difference in the rat population of Bombay and Calcutta was brought forward as the explanation of the difference in the severity of attack. Bombay had a rat population in which, according to Liston, *M. rattus* predominated, and accordingly it suffered severely ; Calcutta had a rat population in which *M. decumanus* predominated, and hence it was comparatively mildly attacked. I shall have to return later to the mildness of the Calcutta attack and show how very comparative it was. So I started to verify the supposed fact that *M. decumanus* was the common rat of Calcutta. A year's work showed that the principal plague rat of Calcutta was one never dreamt of before—the Bengal rat, *Nesokia bengalensis* ; that *M. decumanus*,

the brown rat, also played an important part, and that the rôle of *M. rattus* was almost negligible. Incidentally, it was also found that rats bred freely all the year round, and that the difference of habits between *M. rattus* and *decumanus* had been exaggerated. I will not go into details, as the work of Dr. Crake and myself as to prevalence and relative habits of the different rats has been fully confirmed by Captain Gourlay, I.M.S., in Dacca. In consequence of this Calcutta work there was a re-examination of rat conditions in Bombay, with the result that the presence of the Bengal rat was verified there, too; still more unexpected was it that the commonest rat and the one which suffered most from plague was the brown rat. The exact figures, as given in the Commission's second report, were that 70 per cent. of all rats examined, and 84·6 per cent. of all rats found infected, were *M. decumanus*—the brown rat.

One would have expected after this that the black rat would have retired into a decent obscurity, but the Commission in their third report brought him to the front again and produced a modified *M. rattus* theory. They hold that the epizootic appears earlier amongst *M. decumanus* and reaches its culminating point ten days sooner than in *M. rattus*. While a few cases of plague in man may be caused directly by *decumanus* as he roams about the gullies and lower stories, it is only when plague has spread from *decumanus* to the house-haunting *rattus* that man becomes seriously infected. The explanation of the priority of the epizootic in *decumanus* is that he is more predisposed to attack than *rattus* in consequence of harbouring twice the average number of fleas—eight against four. There are many common-sense objections to this theory. What about plague in Ulmarra, in which instance, though *M. rattus*

was numerous, only *M. decumanus* played any part in connection with human plague; or plague in Calcutta, where *M. rattus* is almost a negligible quantity; or plague in the Punjab, where *M. decumanus* is non-existent, and *M. rattus* is as much a burrowing rat as is *M. decumanus* in Bombay; or the first epidemic in Cashmere in which infection was direct from man to man and rats played no part at all? The only objection I wish to deal with is a very serious one. The chart demonstrating priority is one based on the percentage variations from the corrected weekly mean of rats collected and found infected. The crude figures on which this chart was based show, however, no priority in the *decumanus* epizootic. On the contrary, they show that the highest percentage of plague-infected rats was found in the same week in both *rattus* and *decumanus*—the week ending March 17. In that week *decumanus* showed 42·3 per cent. infected and *rattus* 32·7 per cent. infected. (Table XXIV. following p. 795, *Journal of Hygiene*, Vol. VII., No. 6, December, 1907.) At the very end of the fourth and most recent report of the Commission appears another chart of *rattus* and *decumanus* plague. The apices of the two curves here coincide exactly.

After this it may seem rather needless to take up their conclusion "that the (high) co-relationship between plague in man and plague in *M. decumanus* is probably spurious, depending on the co-relationship between plague in *decumanus* and in *rattus*." My reason for mentioning it is that it is one of the main arguments brought forward by Major Lamb in an attempt to rebut the criticisms published in the Calcutta Plague Report of 1907. The common-sense objections have already been dealt with; there are also most serious technical mathematical objections. Rats were not collected uniformly through-

out the year. In the height of the epidemic season the average collected per week was nearly 5,000. In the slack season it averaged only about 1,300 per week, with a minimum of 961. Again, the total rat population of Bombay can hardly be estimated at less than 5,000,000; that is to say, the percentage of the series examined per week varied from 0·1 to ·02 per cent. Another fallacy is that a weekly mean for one year has been taken. Now plague is most distinctly seasonal; one might as well attempt to express the snowfall of the British Isles in terms of departure from the weekly mean taken over every week of one year, including the summer months. The modified *M. rattus* theory, then, must go the way of the original one.

Recent Conclusions of Plague Commission.—Before passing on to Calcutta observations, I will say a few words as to the findings of the Commission in their last Report (*Journal of Hygiene*, May, 1908). Searchings would, perhaps, be a better word than findings. They have tried to establish that plague is associated with certain temperature conditions which are favourable to the retention of the plague bacillus in the stomach of the flea and favourable to the development of septicæmia in the rat, &c., and that these conditions are correlated to the mean temperature found to prevail in the seasonal period of plague activity. In the first place, they have failed completely to establish a mean temperature for plague epidemics. It is unnecessary to cite examples from their own records, as their failure can be best gauged from their own words: "*A plague epidemic, however, may come to an end when the temperature is most suitable*," p. 287. As regards their experiments on septicæmia and flea transmission at varying temperatures, it is difficult to make out which temperature they consider

the most suitable one, as in their final conclusion, p. 301, they find that a "suitable temperature is one somewhat below 85° F., and in general over 50° F." As a matter of fact, it does not matter which temperature they take as most suitable, as whatever they fix on within limits is upset by their own experiments. They obtained 100 per cent. of successes in flea transmission at 40°, 50°, 60°, and 70° F.; the last experiments were carried out in the non-plague months of July and August. At 90° F. 72 per cent. of twenty-seven rats developed septicæmia, and 33 per cent. showed abundant bacilli. These experiments and conclusions speak for themselves. The experiments are in absolute agreement with general clinical experience in India, and the conclusions are at variance with both experiments and experience.

Calcutta Flea Conditions.—My statements as to flea prevalence in Calcutta are based on the following observations:—

(1) Ten years' experience of the absence of fleas in connection with plague in Calcutta, corroborated by colleagues and subordinates. (2) A series of over 149 guinea-pig experiments extending over three epidemics, placed out as flea traps in presumably infected houses. In seventy-five of these houses the history was that of typical acute plague with buboes. (3) Flea counts on over 4,935 rats, extending over two epidemics. (4) Microscopic examination of over 1,338 fleas dehydrated and cleared. (5) Miscellaneous observations not recorded in statistics, including special enquiries at admission rooms of Calcutta hospitals. (6) Flea-biting experiments.

Flea Prevalence in Calcutta.—The rat-flea of Calcutta is as in Bombay, *Pulex cheopis*.¹ Other fleas are, practically, never found on the rat. *P. irritans* is not found at

¹ The latest name is *Læmopsylla cheopis*.

all. The counts, so far, show that the average per rat varies from about two in the later months of the year, the non-epidemic time, to about six in the epidemic time. Fleas on man are practically never found, an observation confirmed by Major Vaughan, I.M.S., and others in the admission wards of the hospitals. In fact, I have still to find my first *P. irritans* in Calcutta. This observation has been confirmed by Captain Gourlay, I.M.S., in Dacca. Except for a very few imported fleas (*P. irritans*) on the persons of hillmen, man is not a host for fleas in Dacca. *P. felis* or *canis*, the dog-flea, is occasionally found in an excessive prevalence, especially when dogs or cats have been removed. This, however, is mainly amongst Europeans and is in no way correlated to plague. There is no mistaking dog-flea prevalence, as I know from practical experience; the whole of the cane mat is alive with fleas, and the moment your naked foot touches the floor there is a black ring round your ankle. A lady confined to bed in the next room was very severely bitten, so that the discomfort and disfigurement were extreme. The importance of this observation you will gather later.

A large number of observations have been made as to the number of fleas found on wild rats, counts having been made on over 4,935 rats, some fully protected from the light, others not. It is unnecessary to give much detail as to these counts, as full particulars will be found in the tables at the end. They show that when precautions are taken to get a full count there is an increase in the numbers found in the epidemic season averaging about four to six as against about two in the off plague season. It is extremely difficult to come to any reliable generalization as to the average number on the various species or the margin of difference between the numbers

found on rats protected from the light and those which were not protected. For the thing that strikes one most is the great capriciousness of the variations found and the extreme contradictoriness of the results obtained. While fleas were increasing on a particular species in one ward, they might be decreasing in the next ward. The rats obtained from a particular source of supply might be almost flea-free for weeks and then suddenly for a few days fleas would be found in numbers, only to disappear again as rapidly as they had appeared. Another point on which there can be no doubt is that the tendency of the flea to leave the rat when it is exposed to light has been exaggerated. Many of our highest counts have been got on exposed rats, and Dr. Crake latterly gave up the precaution of bagging the rats at daybreak as the difference in the results was totally incommensurate with the enormous trouble involved. Probably the flea is very much less nocturnal than he is supposed to be; witness the innumerable biting experiments in tubes and otherwise that have been carried out during the day by all experimental observers. Man rests motionless at night, and therefore conditions are more favourable to his being attacked. In cases of acute flea infestation, as I know from the instance I have already quoted, and from other experiences, the daylight activity and feeding propensities of the flea may be quite marked. I have seen terrier pups at midday, in open sunlight, swarming with dog fleas.

So far, we have shown that flea conditions are quite different in Calcutta to those found in Bombay, that *Pulex irritans* is practically unknown, that *Ctenocephalus felis* is found only in connection with cats and dogs, and that though the rat-flea occurs on rats in numbers comparable to those found in Bombay, neither it nor any

other flea has been found in plague houses unless exceptionally and in insignificant numbers. This general observation has been confirmed by a series of definite negative observations carried out by means of guinea-pig flea-traps used with all the precautions recommended by Liston and the Commission. In all, 149 guinea-pigs have been put out overnight in presumably infected houses and in houses evacuated on account of the death of rats. The result has been totally different to that found in Bombay. There 275 guinea-pigs picked up 4,681 fleas, or an average of 17 each (*Journal of Hygiene*, vol. vii., pp. 436-445), whereas in Calcutta 149 guinea-pigs yielded 164 fleas, or an average of 1·08, though the counts included individual counts as high as 41, 18 and 15. One hundred and four of the experiments showed no fleas. Not a single guinea-pig contracted plague, and only 2 died, fifty-seven days after the experiment, with no signs of plague. It may be noted, on the authority of Mr. Haffkine, that these guinea-pigs are exactly the same as those used in Bombay, Calcutta being one of their sources of supply. In some of the 149 houses there was some doubt as to the diagnosis of plague, but in 75 there was none, a clear history of typical acute plague with bubo being obtained. The flea-count in those houses did not depart from the average. Of course, there was no delay in placing out the guinea-pigs, and in a few instances the animals were placed in the same room as the living patient. You may think that those results are so much at variance with the Bombay experiments that you have some difficulty in accepting them. In a recent paper I collected from the Commission's third report instances of fleas failing to infect guinea-pigs in nineteen instances where the fleas recovered totalled 520. The most marked instance was

in Parel, where 108 and 150 failed to convey plague to the first guinea-pigs, though when transferred they gave it to two other guinea-pigs. Again, in Dhand, out of twenty-four houses showing human plague only five proved infective to guinea-pigs, and none of those houses had furnished plague rats. Out of 32 guinea-pig experiments in the town 22 were negative, and 3 of the 10 positive were in one house. In Kasel only 3 guinea-pigs died out of 18, though the rat epidemic was marked—283 acute cases and 23 chronic. Thus it will be seen that the divergence between the Calcutta experiments and the later observations of the Commission is much less than might have been predicted from their early observations. Full details of our experiments will be found in the Calcutta Plague Report for 1908.

Does the Rat-Flea bite Man? (P. cheopis).—We now come to the crucial question, a question that answered in the negative destroys the fundamental principle of the rat-flea theory. The experiments that were carried out may seem to be rather bold in that fleas were taken direct from wild rats and put on men who were most of them not inoculated, but it must be remembered that in addition to a number of weighty theoretical considerations, which I have gone into very fully in the Calcutta Plague Report for 1907, there were very reassuring practical observations. Thus special enquiries at the end of the plague season showed that those engaged in regularly collecting rats showed no special tendency to contract plague, nor did the men engaged in handling the rats at the rat depots.

"Flea-feeding Experiments.—The fleas were obtained almost entirely from *Nesokia bengalensis* by the double chloroforming method which I have mentioned in my last paper on pulicides.¹ The species of the fleas

¹ Hossack, *Ind. Med. Gazette*, October, 1907, p. 366.

was not determined, but from the figures as to the preponderance of *P. cheopis* it is clear that they must all have been of this species with possibly one *P. felis*. The fleas were placed in enamelled mugs on the bottom of which was some damp sand, the top being covered with a plate of glass. In the pulicidal experiments it was noted that fleas kept in an empty test-tube or an empty mug were practically all dead in forty-eight hours, showing only four survivors out of sixty-eight in two instances, but it was found that the addition of damp sand made a great difference to the period of survival. It also seemed to make considerable difference to their power of jumping, for while no fleas managed to jump out of the empty mugs several were lost from the sanded mugs. At first, when the fleas had been reduced by death or loss to one or two, they were kept in a test-tube with damp sand as instanced by flea No. 10, which was in a test-tube from the third to the ninth day of starvation; in the later experiments they were put in tubes from the first day on which feeding was attempted. As to loss, in addition to that caused by jumping out some were lost by climbing up the damp sticky sides of the mug and several by slipping under an irregularity in the edge of the test-tube when they were being given an opportunity to bite; others were lost while separating the sand from the flea. These details are necessary as an explanation of the paucity of experiments in the later days of starvation. In order to eliminate the personal element as far as possible, for it is well known that some individuals are relatively exempt to attacks from fleas, eighteen individuals were used as subjects, and the same flea as far as possible was never put twice to the same person. On several occasions fleas having refused one person were at once put on another, but in only one case did this induce them to bite. The individuals used as subjects comprised, besides Mohommedans, the following castes of Hindus, Brahmin, Kyastha, Subernabanic, Dosad, Dome, Mehtar, and were drawn from the staff and the rat-catchers. I only used myself once or twice, as I am not very liable to attack from fleas. Some of the men had hairy arms, some had smooth.

“*Technique*.—The method adopted to isolate the flea in the tube was rather troublesome and tedious. The tube was placed on top of the flea as it sat on the sand; a bent spatula was then inserted in the sand under the tube, and thus the flea and a lump of sand were removed together. With some manipulation and the aid of a wire scoop the sand was then removed. The fleas were generally put to bite in pairs, and the site selected was the inner surface of the forearm where the skin is thin. At first the tube was left on for three to five minutes, but the period was latterly reduced, experience having shown that if a flea was going to bite it did so immediately it touched the skin. As a rule, the fleas

evinced a great desire to get off the skin and climbed up the tube and had to be repeatedly shaken down. It was sometimes observed that a flea would apparently settle down to bite as soon as he touched the skin, but would immediately change his mind either from distaste or inability to draw blood and would make no further attempt. When a flea did feed he bit as soon as he touched the skin, and remained sucking from two to three minutes, three and a half minutes being the maximum. Thrice a fresh site was selected after one and a half or two minutes and the feeding was renewed, but as a rule the feeding was uninterrupted. The total duration of the longest divided feed was five and a half minutes. Generally a very faint ecchymosis could be made out in light skins within five minutes of biting, sometimes so small that without the guide of the pressure mark of the test-tube it would be very difficult to find. On one occasion when the man had been bitten twice it was observed forty-eight hours after the bite that there were minute papules where he had been bitten. On one occasion the bite caused first a faint ecchymosis and within half an hour a white bleb $\frac{1}{2}$ inch by $\frac{1}{4}$ inch appeared. On this occasion the flea bit for only one and a half minutes, and no blood could be seen passing into it. It may be suggested that it was making violent and more or less unsuccessful efforts to suck, as the result of which there was an excessive salivary outflow. Blebs were only seen twice altogether.

“*Possible Criticism of Feeding Experiments.*—There are several points in these experiments in which it is desirable to meet objections which may reasonably be raised. First there is the objection that the conditions under which the experiments were conducted were unnatural and not conducive to biting. The answer to this is that the conditions are much less unnatural than those under which a number of successful biting experiments on rats and guinea-pigs were carried out by the Commission, for in these the fleas were not only applied in test-tubes, but had to bite through muslin. The fact that the experiments were made in the day and not at night is discounted in the same way by the successful biting experiments of the Commission carried out through the day, and also by the observations which I have already called attention to which tend to show that *P. cheopis* is not so purely nocturnal as it is supposed to be. The last factor that remains to be considered is the possible effect of the common custom in Bengal of applying to the body mustard or cocoanut oil. In the first place, oil was not applied by the Mehtars and low-class Mohommedans who were the subjects of the experiments. In the second place, even in those subjects who did apply oil there was no trace of it to be found either by touch or smell. The explanation of this is that only a very small quantity is applied; about a teaspoonful is put in the hand, the head is then well rubbed and only a scanty remainder is left for the trunk and limbs, and this is well rubbed in.

TABLE A.

TYPICAL EXAMPLES OF FLEA-BITING RECORDS. X = BITE, 0 = REFUSAL.

Ser. No.	Date of capture	Date of first experiment	Days of starvation									
			2	3	4	5	6	7	8	9	10	
10	21/8/07	22/8/07	0	0	0	—	0	0	0	0		dead
19	22/8/07	23/8/07	0	0	—	x	0	x	—	—		—
21	26/8/07	27/8/07	0	x	—	—	—	0	0	0		dying
34	2/9/07	4/9/07	—	0	0	0	0	—	0	0		dead
51	4/9/07	6/9/07	—	x	—	—	x	0	0	0		0
52	4/9/07	6/9/07	—	0	—	—	0	0	dying	—		—
60	4/9/07	7/9/07	—	—	0	—	0	0	lost	—		—
63	4/9/07	7/9/07	—	—	0	—	x	0	0	x		0
68	4/9/07	9/9/07	—	—	—	—	x	0	x	0		feeble
66	4/9/07	9/9/07	—	—	—	—	0	0	0	0		0

TABLE B

ANALYSIS OF RESULTS OF FLEA-FEEDING EXPERIMENTS.

Day of starvation	Number of fleas per day of starvation	Number of bites	Ratio
2nd	41	1	1 in 41
3rd	36	5	1 in 7
4th	31	9	1 in 3½
5th	28	5	1 in 5
6th	21	6	1 in 3½
7th	20	4	1 in 5
8th	11	1	1 in 11
9th	8	2	1 in 4
10th	4	0	0
Total	200	33	1 in 6

For the 200 experiments 100 fleas were used; biting was confined to 26 out of the 100, and only 7 out of the 26 bit twice; 45 out of the 100 fleas had opportunities of biting twice. Of the 7 fleas that bit twice, all but one, No. 85, had refused more than once. Of the 26 fleas that bit all had refused once or oftener, except No. 85, and 5 fleas which had no chance of refusal after the first bite, Nos. 56, 71, 76, 87, 100.

One flea refused to bite at all in seven trials extending over nine days and seven different subjects. Another failed to bite after six trials on eight different subjects extending over nine days. The accidental losses were so numerous that I did not arrive at very definite conclusions as to

the duration of life in fleas that did bite. In the later days of the experiment I noted some as being very feeble in spite of having bitten once or twice. On the second day of starvation only one flea out of 41 would bite, but as starvation progressed bites became more frequent. Of opportunities to bite only 1 in 6 was taken, and of the fleas experimented with only 1 in 4 bit at all.¹

Acutely Infected Calcutta Houses.—From the early experiments of the Commission I arrived at the conclusion that, where the flea counts were low, plague was either not transmitted at all or with difficulty. The Commission themselves have shown that one flea does not generally transmit plague (one success in sixty experiments) and admit that the rate of progress of an epizootic is conditioned by the number of fleas present. Verjbitski had noted in his own experiments what I have called attention to in the Commission's results and proceeded to define the limit. He found that even with very highly virulent plague "no infection is likely to take place when the number is less than five" (*Journal of Hygiene*, vol. viii., p. 185). Now the rat is more susceptible than man, and leaving out of account the enormous difference in bulk, man will probably require more bites, say ten. If my statement be accepted that of even starved rat fleas, only one in six will bite, then there must have been sixty starved fleas on man to produce the necessary ten bites in order to infect him. At the very least it is clear that there must be numerous fleas on him. Take now the cases of acutely infected houses in Calcutta, such houses as 19, Armenian Street, with twenty cases; 14, Roop Chand Roy Street, with twelve cases, and other houses with eight and ten cases all occurring within a few days. If these cases were due to flea-bites, then the houses should have been swarming

¹ Dr. Crake confirmed these experiments; only 8 bit out of 67 fleas starved up to twelve days.

with rat-fleas, something comparable to the dog-flea infestation I have already described, or the flea infestation repeatedly described in Bombay. No fleas were noticed in these houses, though the circumstances were peculiarly favourable to my detecting them had they been present. For I was at that time busily engaged in collecting clinical data, data I may observe which showed that in 30 per cent. of the cases, there was evidence of some involvement of the lung as is to be expected in a septicæmic disease, and in consequence I spent hours in these houses every day, often actually kneeling or reclining on the filthy platform beds on which the patients were lying. Yet I never saw a flea.

Conclusion.—When these observations are taken in conjunction with our general experience in Calcutta and with the negative result of guinea-pig flea-traps, it is evident that rat-fleas can have played only an insignificant part in the acute plague which in certain wards of Calcutta attacked in a single epidemic 1 in 40 and 1 in 30 of the population.

Biting Experiments by the Commission.—The Commission have carried out various experiments on the capacity of the flea to feed on man and bite him. It has already been pointed out that most of their early observations were carried out under artificial conditions or under exceptional circumstances such as those of 280, De Lisle Road, and even then referred not so much to fleas biting as to fleas being found on men. I also criticized the feeding experiments as being carried out in such a way as to make it impossible to get exact observations as to the number of bites. This criticism is to be found repeated by themselves in their last report. In experiment II., p. 251 (e) they admit it is a matter of considerable difficulty to know how many had fed. On

pp. 249-253, *Journal of Hygiene*, May, 1908, will be found a large series of experiments. In those very large numbers of fleas, which had been kept starved in boxes or go-downs, were given an opportunity of biting man. In some of the experiments, namely, II., III., II. (e) (1) and I., p. 252, it is possible to give definitely a minimum count. The fleas in these experiments exceeded 696 probably by a large margin, but only eleven bites were obtained, *i.e.*, only one in sixty-three bit and the fleas were starved. In the other experiments, starved fleas were present in very large numbers indeed—100 fleas per box is taken as a comparatively small count—but the proportion of bites was extremely small. From the figures given it cannot be stated accurately; it varied from about 1 in 20 to 1 in 100. Is it unreasonable to maintain that these experiments absolutely support my contention that *Pulex cheopis* unstarved will hardly bite man at all and even starved only occasionally? If Verbitski's conclusion, that even for the rat a minimum of five bites is necessary, be accepted, and in view of the Commission's results it must be, it follows, quite apart from plague in Calcutta, that the rat-flea can only play an occasional and unimportant part in human infection. It is a part comparable to the rôle of the human-flea and the cat-flea as established by Verbitski, the rôle of flies, spiders and cockroaches as put forward by Hunter, and the rôle of the bed-bug as established by Verbitski and quite recently by Jardanski and Kladnitsky. In fact, it seems to me that the whole position will have to be reconsidered.

This paper is already so lengthy that I cannot take up the various general arguments against the flea theory, nor the facts in favour of there being not one but many modes of plague infection. Moreover, I have already dealt with

them pretty fully in the Calcutta Plague Report of 1907. Amongst recent writers who are more or less in sympathy with my views may be mentioned Galli Valerio, Simpson, King, Klein, Baxter-Tyrie, and Hunter.

In conclusion, I should like to express my gratitude to the Commission, or rather to one of its leading members (Dr. Martin) for the courtesy and assistance he has so kindly shown me in arranging for me to carry on my work in this country in the plague laboratory in Elstree. He is only repeating what I have already experienced at the hands of Major Lamb in Kasauli. It is gratifying to find that acute controversial differences have only had the effect of establishing pleasant personal relations.

REFERENCES.

Reports of the Advisory Committee, *Journal of Hygiene*, vol. vi., September, 1906; vol. vii., July, 1907; vol. vii., December, 1907; vol. viii., May, 1908.

LAMB, "The Etiology and Epidemiology of Plague," 1908.

LAMB. *Indian Medical Gazette*, May, 1908, p. 161.

HOSSACK. "Diagnosis of Plague," *Lancet*, November 24, 1900.

HOSSACK. "Calcutta Plague Reports," 1906, 1907, &c.

HOSSACK. "Rats of Calcutta," *Mem. Ind. Mus.*, vol. i., 1907.

HOSSACK. "Some Recent Developments in the Study of Plague," *Calcutta Medical Journal*, May, 1908.

CRAKE. "Calcutta Plague Reports," also *Indian Medical Gazette*, October, 1907, p. 363 (1908).

MITRA. "Report on Outbreak of Plague in Cashmere, 1903, 1904."

LISTON. "Plague Rats and Fleas," *Bombay Nat. History Soc.*, 1905, xvi., p. 253, also pamphlet p. 13.

HUNTER. "Research into Epidemic and Epizootic Plague," 1904, p. 56.

GOURLAY. *Records Ind. Mus.*, vol. i., part 3, No. 21, October, 1907.

BAXTER-TYRIE. *Journal of Hygiene*, vol. v., p. 77.

KLEIN. "Bacteriology and Etiology of Oriental Plague."

JARDANSKI and KLDNITSKY. *Ann. Past. Inst.*, May, 1908, from Epitome, *British Medical Journal*, December 5, 1908.

APPENDIX.

TABLE I.

CRUDE FLEA COUNT, AUGUST-DECEMBER, 1906, BY DR. HOSSACK.

	<i>M. decumanus</i>			<i>M. rattus</i>			<i>N. bengalensis</i>			<i>N. bandicota</i>		
	Rats	Fleas	Max.	Rats	Fleas	Max.	Rats	Fleas	Max.	Rats	Fleas	Max.
August ..	56	57	7	10	19	4	28	30	22	21	1	—
September ..	7	1	—	3	—	—	32	28	2	3	—	—
October ..	7	47	40	2	4	—	2	1	—	—	—	—
November ..	138	194	—	—	—	—	30	71	—	—	—	—
December ..	—	—	—	—	—	—	22	56	—	—	—	—
	208	299	—	15	23	—	114	186	—	24	1	—
	<i>M. decumanus</i> , 1·42 fleas per rat.			<i>M. rattus</i> , 1·53 fleas per rat.								

Total rats all kinds, 337 = 508 fleas—1·50 fleas per rat.

CRUDE FLEA COUNTS, *N. bengalensis*.

			Rats		Fleas		
Dr. Hossack.	June	..	21	..	7	..	·3 per rat
„	July	..	166	..	392	..	2·36 „
„	August	..	209	..	284	..	1·36 „
Dr. Crake.	August	..	216	..	335	..	1·55 „
			612		1018		1·66

TABLE II.

BAGGED NESOKIA FLEA COUNTS, 1907.

			Rats		Fleas		Ratio
Dr. Hossack.	July	..	51	..	107	..	2·09
„	August	..	108	..	184	..	1·70
Dr. Crake.	August	..	216	..	686	..	3·18
Special collection.	August	..	45	..	139	..	3·08
			420		1116		2·65

TABLE III.

FLEA COUNTS BY DR. HOSSACK FOR 1908.

BAGGED COUNTS ON MIXED RATS, DISTRICT II.

			Rats		Fleas		Average
January	..		228	..	908	..	4·49
February	..		168	..	962	..	6·7
March	..		183	..	1134	..	6·5
April	..		209	..	1280	..	6·8

COUNTS ON UNBAGGED MIXED RATS, DISTRICT II.

			Rats		Fleas		Average
January	..		103	..	255	..	2·4
February	..		126	..	385	..	3·0
March	..		64	..	173	..	2·1
April	..		12	..	43	..	3·7

FLEA COUNTS ON UNBAGGED RATS, DISTRICT III., BY SPECIES.

	<i>M. decumanus</i>			<i>M. rattus</i>			<i>N. bengalensis</i>			<i>Mixed rats</i>		
	Rats	Fleas	Av.	Rats	Fleas	Av.	Rats	Fleas	Av.	Rats	Fleas	Av.
January ..	18	26	1·5	—	—	—	35	65	1·8	5	12	2·4
February ..	50	276	5·5	2	—	1·5	152	415	2·7	62	181	2·9
March ..	9	62	6·7	—	—	—	145	706	4·8	29	121	4·1
April ..	6	95	15·8	—	—	—	89	376	4·2	18	91	5·0

FLEA COUNTS IN DISTRICT I. BY DR. CRAKE.

FIRST SERIES, 1906-07.

Average number of fleas per rat, all types, all seasons of year, 1·19 (219 fleas on 183 rats).

Average number of fleas per rat, all types : August to December, 1·3 ; January to March, 1·5.

Mus rattus.—Average, August to December, 1·7 ; January to March, 3·0 (numbers small, not recorded till confirmed by later observations).

SECOND SERIES, 1908, QUIESCENT PERIOD.

(a) Bagged *N. bengalensis*, 271 rats gave 803 fleas, average 2·9 per rat (between July 16, 1907, and September 13, 1907).

(b) Unbagged 214 *N. bengalensis* gave 335, average 1·5 per rat.

Up to December no increase at north of town.

Certain rats from Ward V yielded large numbers ; 92 *N. bengalensis* giving 335 fleas, average 4·3 per rat.

In District I., December, 151 *Nesokia* gave 174 fleas, average 1·1 per rat. January, 137 *Nesokia* gave 391 fleas, average 2·9 per rat. (Ten from Ward V gave 54 fleas).

In February separate counts for each species made. Following table is for February to June, 1908 :—

TABLE IV.

FLEA PREVALENCE. DR. CRAKE, 1908.

Unbagged Counts.

Month	Species of rats	Rats	Fleas	Average
February ..	<i>M. rattus</i>	81 ..	596 ..	7.2
„ ..	<i>M. decumanus</i>	37 ..	302 ..	8.1
„ ..	<i>M. bengalensis</i>	353 ..	1,011 ..	2.8
„ ..	All types	471 ..	1,909 ..	4.05
March ..	<i>M. rattus</i>	8 ..	31 ..	3.8
„ ..	<i>M. decumanus</i>	33 ..	365 ..	11.06
„ ..	<i>M. bengalensis</i>	326 ..	767 ..	2.3
„ ..	All types	367 ..	1,168 ..	3.1
April ..	<i>M. rattus</i>	<i>Nil</i> ..	<i>Nil</i> ..	<i>Nil</i>
„ ..	<i>M. decumanus</i>	31 ..	383 ..	12.3
„ ..	<i>M. bengalensis</i>	305 ..	610 ..	2.0
„ ..	All types	336 ..	993 ..	2.9
May ..	<i>M. rattus</i>	4 ..	11 ..	2.7
„ ..	<i>M. decumanus</i>	11 ..	75 ..	6.8
„ ..	<i>M. bengalensis</i>	475 ..	337 ..	0.7
„ ..	All types	490 ..	423 ..	0.86
June ..	<i>M. rattus</i>	14 ..	12 ..	0.8
„ ..	<i>M. decumanus</i>	5 ..	4 ..	0.8
„ ..	<i>M. bengalensis</i>	270 ..	111 ..	0.41
„ ..	All types	289 ..	127 ..	0.43