Contagion and Enclaves
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CHAPTER 7

Tropical Medicine in Its ‘Field’: Malaria, Hookworm and the Rhetoric of the ‘Local’

This chapter studies the dynamics between colonial enclaves and Tropical Medicine in the twentieth century. Despite the acceptance of germ theory, British Indian medical discourse and practice never abandoned miasmatic and climatic theories of disease. In colonial India, Tropical Medicine continued to connect diseases with specific ‘zones’ and ‘localities’. Research in Tropical Medicine reiterated the importance of ‘local factors’ constructed through ecological, climatic or cultural modes. From their contribution to Tropical Medicine, through the ‘experiments’ and verification of disease theories in their localities, to the contribution to the control of archetypical ‘tropical’ disease in Bengal and India generally, the tea plantations were an important site for the exploration of new ideas and experimentation. In the case of research in anti-malarial sanitation, a focus on the local ecological conditions of the tea plantations in Darjeeling foothills merged seamlessly with factors such as the cultural behaviour of plantation labourers – all framed in a set of conditions termed the ‘local’. Simultaneously the political economy of the tea plantations inhibited both anti-malarial sanitation as well as systematic and full use of quinine prophylaxis within these ‘local’ sites. Similarly, when the Indian Research Fund Association (IRFA) proposed a hookworm project to assess the feasibility of its eradication, the medical experts chose the Darjeeling hill plantations as their first, experimental site. The results of the hookworm survey showed a very high incidence among the plantation labourers. The planters, while providing the plantation space for the surveys, were not persuaded to install sanitary facilities for the workers, nor did the government initiate legislation to facilitate it. Therefore, following the survey in Darjeeling and subsequent ones in Duars and Terai, the problem of hookworm eradication in the plantations remained unresolved.

This chapter argues that Tropical Medicine was enriched by studies in colonial enclaves which facilitated wide-ranging studies on preventive and therapeutic
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aspects of the internationally competitive specialism. These studies validated the value or significance of ‘local factors’ in epidemiology as they did in governance itself. There were no structural therapeutic or public health benefits to the enclaves in the process.

Malaria in the Darjeeling Foothills

Most of the sicknesses in the Duars and the Terai were related to malaria and blackwater fever. Tea plantations were established in the Darjeeling foothills and the Duars a few decades before Tropical Medicine was institutionalized and Patrick Manson assigned to malaria the role of the paradigmatic disease of the tropics, ‘the greatest scourge of mankind … eminently a tropical disease’. Historians have pointed out that the institutionalizing of Tropical Medicine was effected in the context of professional interests and imperialist visions. Institutional bases for research in India were set up gradually in the first decades of the twentieth century, but various malarial theories jostled for recognition and the sanitary principles of miasmatic changes persisted for a long time.

Malaria research in the Darjeeling foothills was enmeshed in debates over malaria-control policies in India and in international malaria research. In the early twentieth century, scientific medical research in India focused urgently on cholera and plague, but particularly after 1911 when the Indian Research Fund Association (IRFA) was formed, which invested in malaria research as well. The story of international malaria research and India in the twentieth century is one of incremental increase in specialized knowledge about malaria, the identification of various anophelines, formation of malaria ‘brigades’, mapping of malaria through malaria surveys and various ‘controlled experiments’ to eliminate infected anophelines.

Tea chests from the Darjeeling foothills were familiar commodities in the auction houses of Mincing Lane in London at the turn of the nineteenth century. It was at the turn of the century that the anopheline of the same region would become the subject of study by a scientific institution in London. In 1902, the Indian government invited the Malaria Committee of the Royal Society, which

1 Patrick Manson, ‘On the Necessity for Special Education in Tropical Medicine’, in a speech delivered at St George’s Hospital at the opening of the Winter Session, 1 Oct. 1897, PRO/CO/885/7/9 (APAC), p. 7.
2 M. Worboys, ‘Germs, Malaria and the Invention of Mansonian Tropical Medicine’, in D. Arnold (ed.), Warm Climates, Western Medicine: The Emergence of Tropical Medicine, 1500–1900 (Amsterdam and Atlanta, GA, Rodopi, 1996), pp. 181–207. Also see Haynes, Imperial Medicine, pp. 126–72.
3 Worboys, ‘Manson, Ross, and Colonial Medical Policy’.
5 The first samples of tea from India were sent to Mincing Lane in 1838, these being tea made from indigenous wild Assam tea plants. See ‘The Story of Indian Tea’, in The Tea and Coffee Trade Journal, 56 (March 1929), pp. 372–97.
had been to Africa in 1901, to visit India. Their Indian tour included the city of Calcutta and the Duars, as well as the Jeypore Hill tracts in southern Bihar and the British Punjab. Christophers pointed out later that the visit ‘quite apart from its direct scientific results, had a great effect upon the future course of malaria work in that country’. It was one of the numerous studies on malaria in India; the first three decades of the twentieth century saw a deluge of malaria research in colonial India. The Royal Society committee found the phenomenon of ‘anophelism without malaria’, which suggested that the degree of infection in a place need not be directly proportional to the number of anopheles in the locality. Therefore, in the very first scientific report on malaria in the Darjeeling foothills, the importance of its location was established. The report emphasized also that anopheles were of various kinds; on this trip, the Commission discovered two new species of anopheles in the Duars, which they had not encountered elsewhere in India. Malaria could no longer unambiguously be linked to sanitary conditions or climatic fluctuations: ‘From Calcutta to Duars the places were under practically identical conditions … In Calcutta, however, we had abundant A. Rossii and no malaria, and in the Duars a relatively small number of A. Fluvatilis and a large amount of malaria.’

This linked specific localities with the breeding sites of particular species of anopheline carriers. The report remained within official and scientific circles. The planters, and even the doctors practising in the tea estates, do not appear to have engaged with the details of this first scientific research on malaria in the region. The planters petitioned later, in 1906, for a detailed scientific study of the causation of blackwater fever and malaria in the region. The government acceded to their request, and the reports of the consequent Christophers–Bentley report threw a long shadow on both the discourse of malaria in India and locally on the management of disease in the tea plantations.

8 W.F. Bynum has pointed out that in 1929 when J.A. Sinton compiled a bibliography of malaria in India, it filled 200 pages and included in addition 2200 items from scientific and medical journals, government publications and reports. W.F. Bynum, ‘Reasons for Contentment: Malaria in India, 1900–1920’, *Parassitologia*, 40 (1998), pp. 19–27 (21).
9 S.P. James, ‘Malaria In India’, *Scientific Memoirs by Officers of the Medical and Sanitary Department of the Govt Of India*, NS 2 (Calcutta, Periodical Publications, 1902), p. 76.
10 Ross, who was in the neighbouring Terai while he was in the IMS, also noticed the great paucity of anopheles there. He wrote to Manson on 6 September 1898 from Kurseong, ‘I don’t believe fresh malaria was about at all at Naxalbari at this season.’ Ronald Ross and L.J. Bruce-Chwatt, *The Great Malaria Problem and Its Solution: From the Memoirs of Ronald Ross with an Introduction by L J Bruce-Chwatt* (London, Keynes, 1988), p. 194. See also, *Obituary Notices of Fellows of the Royal Society*, 1 (1933), p. 111.
11 See Chapter 5.
From Locality to Demography: The Tropical Aggregation of Labour

The circumstances in which Christophers and Bentley compiled the report on malaria in the Duars have been highlighted in previous chapters. Here I will re-examine their report in the context of malarial research and the impact of the research on the northern Bengal region. The report linked the malarious Duars to several other parts of India, and indeed, of the world. By referring to research on immunity conducted by Robert Koch, and by the Italian researcher Angelo Celli (in the Roman Campagna), they associated malaria with congregations of a labouring population in any region:

We may say that in our researches on malaria we have for some time recognised the almost constant association of labour camps with severe malaria … [It] lies the explanation of the association of outbreaks of malaria with soil disturbance, opening up of new country and so on. It is not the soil disturbance, we believe, but the occurrence of labour camp conditions, or what we shall call for convenience of description THE TROPICAL AGGREGATION OF LABOUR, in association with these enterprises which has given them their evil reputation. 12

Along with earlier works by Koch and Celli, the report also referred to Stephens and Christophers, and connected their research to malaria in the Duars and posited that

A condition of continual immigration similar to that described by Koch is conspicuously present in the Duars, Assam and elsewhere in India, where the constant introduction of non-immune immigrants may be likened to the continual heaping of fresh fuel upon an already glowing fire … This factor, which when it acts temporarily, is capable of producing epidemic malaria and when long continued must give rise to an increased endemicity, we shall term for descriptive purposes the FACTOR OF NON-IMMUNE IMMIGRATION. 13

Their report concluded that the congregation of newcomers at any industrial sites, harbours, jute mills, tea plantations and railway lines was linked to the persistence of malaria in endemic form. They argued that the centres of endemic malaria were located in centres of ‘industrial activity’, which were constantly in a state of ‘exalted malaria’. When sick workers moved out of the area they carried the malarial infection with them, acting as mobile reservoirs of malaria. 14 The thesis of the tropical aggregation of labour, or the human factor in malaria, gained in credence and attained legitimacy through its reiteration at national and international fora. By 1927 the human factor in malarial infection was an accepted scientific theory through reiteration in published work on malaria in India. 15 In 1929 the League of Nations Malaria Committee carried out

12 Christophers and Bentley, Malaria in the Duars, p. 2.
13 Christophers and Bentley, Malaria in the Duars, pp. 3–4.
14 Christophers and Bentley, Malaria in the Duars, p. 14.
15 For instance, see Patrick Hehir, Malaria in India (London and Oxford, Oxford University Press, 1927), pp. 45–49.
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an inspection tour of India at the invitation of the Government of India. Christophers wrote the preface to their report and cited the factor of non-immune immigration at industrial sites as a major cause of the spread of malarial fever in India, particularly mines, plantations and harbours. The theory became the fulcrum of a discourse on malaria that emerged with great clarity in later years.

Several years after their controversial study on Duars, Christophers, at that time the Director of the Malaria Bureau of India, wrote a report on the prevalence of malaria, blackwater fever and anchylostomiasis in the coal mines at Singhbhum. Some of his recommendations were similar to those made in the Duars some 20 years previously, such as the screening of existing European bungalows, their location outside the sites of infection and the encouragement of the use of quinine. Christophers argued that the aggregation of labour contributed to great malarial infection among new immigrant workers in an industrial site. He claimed that permanent workers at any site acquired an immunity to malaria through repeated infections suffered by the newly immigrant labourers and by newborn children and infants. Therefore, even if the spleen rate (the principal indicator of high endemicity) of a given area showed a high endemicity of malaria, the adult population who comprised the main productive element would not be affected. This realization, he emphasized, would avert the need for expensive sanitary measures and also reduce the expenditure on quinine by discouraging its use as prophylaxis.

Acquired Immunity, Race and Acclimatization

There was one other aspect to the issue of acquired immunity to malarial fever: immunity through racial acclimatization. The question asked by medical experts was, Were some races more liable to acquire immunity through repeated attacks of malaria than others?

16 Christophers and Bentley presented the argument at the Indian Medical Congress in Bombay in 1909. See Arabinda Samanta, Malarial Fever in Colonial Bengal: Social History of an Epidemic, 1820–1939 (Calcutta, Firma KLM, 2002), p. 36. In his preface to the report of the League of Nations Malaria Commission to India, Christophers emphasized again this aspect of malaria in industrial locations. The League of Nations Malaria Committee endorsed the theory of non-immune immigration into industrial locations as a principal cause of malaria: ‘From the results of … investigations and from our own few observations made on the spot, we have come to the conclusion that the hyper endemic areas although sparsely inhabited are very often the areas where large plantations and large industrial undertakings are situated and which are therefore often the site of a considerable immigrant population coming from other districts. It is in these hilly districts covered with forest or jungle, with a sparse population that the immigrants are quickly mown down. Infant mortality in these districts is extremely high.’ Report of the Malaria Commission on Its Study Tour in India Aug 23rd to Dec 28, 1929 (Geneva, League of Nations Publications, 1930), p. 31.


18 Christophers, Enquiry on Malaria, p. 29.

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When Christophers and Bentley wrote their report on malaria in the Duars they referred to the issue and specifically denied that the racial factor was of importance:

Race appears to play but little part in influencing the prevalence of infection, though some races appear to be more profoundly affected by the disease than others. In several instances where we have examined the children of hill and plains people living on the same garden under similar conditions, but with the two races widely separated, we have found the rate of infection and of enlarged spleen to be practically the same.¹⁹

However, Christophers referred to certain ‘susceptible races’ when he wrote his report on Singhbhum in 1923. The European staff and the skilled labour (mostly Hindus and Muslims) were categorized as the ‘susceptible’ races. He stated on the other hand that the unskilled mine workers were ‘indigenous, largely aboriginal’ and as adults were ‘fairly immune to malaria’. This immunity, he noted, came after a period of ‘acute infestation’ of malaria for a period of around two years – a process he compared to the ‘salting of animals in trypnosomiasis’.²⁰

In 1926, when the Public Health department of the government of Bengal reported its findings on anti-malarial measures in a tea estate in Duars, it echoed these sentiments. In an area of malarial hyper-endemicity like the Duars, it sketched out the possibilities of infection very similar to the conclusions reached in the mining areas of Bihar:

If a mixed population of men, women and children who were susceptible to malaria were introduced into such an area, there would in the first instance be an explosive outbreak of malaria amongst the new comers … In a vigorous race, there would be a ‘rally’ in the individual against the parasite and gradually a tolerance or relative immunity would be developed … A time would come when the only persons not possessing a relative immunity would be newly born children. These would all be intensely affected and would suffer from continuous fever until they either died or gradually acquired a relative tolerance.²¹

Such a conception of immunity from malarial infection that posited distinctions between ‘vigorous’ races and others not so vigorous led almost inevitably to the conclusion that certain communities were more likely to acquire immunity than others: ‘Coolies from the Jeypur Hill Tracts will, therefore, be less likely to suffer on arrival than Chota Nagpuris … [T]here are certainly racial differences.

¹⁹ Christophers and Bentley, Malaria in the Duars, p. 23.
²⁰ Christophers, Enquiry on Malaria, p. 30. See also idem, ‘The Mechanism of Immunity against Malaria in Communities Living under Hyper-endemic Conditions’, Indian Journal of Medical Research, 12 (1924), pp. 273–94. This view of acquired immunity in hyper-endemic regions confirmed similar findings by Schaffer in Sumatra.
Santhals, for instance, seem to get immune more quickly than Nepalese, who would appear to possess small powers of immunity production.22

This concept of immunity to malaria through repeated attacks of fever was a little different from the older, nineteenth-century ideas of racial immunity to fever. In the mid-nineteenth century Rennie, in his account of the Bhutan campaign of 1865, mentioned that the Meches, the pre-colonial inhabitants of the region, were peculiarly immune to fevers: ‘[The Meches] are a singular tribe, enjoying excellent health where other races, dark and fair, sicken and die – while again they contract malignant fevers when removed from their own locality into districts considered by us comparatively salubrious.’23

Rennie conceived of racial immunity to fevers as an immunity that was both racial and locational. He linked racial immunity to acclimatization in a specific locality, outside of which the entire race would perish. This notion was current among both medical practitioners and colonial ethnologists. In 1872 E.T. Dalton had thus remarked about the Meches in his Descriptive Ethnology of Bengal:

Their constitutions have become so much accustomed to the malarious influences of the Terai, that apparently they cannot live without the poisonous gases that they imbibe there, and in the purer atmosphere of the plains, or in breathing the more invigorating air of the higher ranges, they pine and die.24

This particular link between racial immunity, location and fever had become irrelevant in the twentieth century, for by the time Christophers and Bentley wrote their report on malaria in the Duars, the Meches had migrated already towards the eastern parts of the district, where tea plantations had not yet been claimed from the forest. The Meches were gradually pushed out of the district towards Goalpara in Assam throughout the colonial period.25

In the twentieth century, the issue of immunity in India remained important, and was linked to both racial and locational categories. A textbook on tropical health written by a Bengali physician, B.N. Ghosh, which was first published in 1912 and went through seven editions, understood immunity to malaria in racial terms:

The question whether immunity is possible may be answered in the words of Manson as ‘yes/no’ … Some races and certain individuals are, however,

24 See A. Mitra, The Tribes and Castes of West Bengal (Alipore, West Bengal Government Press, 1953), p. 224. Dalton’s Descriptive Ethnology was compiled in 1872, under the aegis of the Asiatic Society of Bengal. His ethnography was later used by the decennial census survey that was initiated in 1871–72.
25 Ray, Transformations, p. 79. The Meches’s demographic insignificance and unwillingness to work in the newly established tea plantations resulted in their marginalization. However, the theory that they were immune to malaria persisted in twentieth-century ethnographic accounts of the Meches. For instance, see Charu Chandra Sanyal, The Meches and the Totos: Two Sub-Himalayan Tribes of North Bengal (Darjeeling, University of North Bengal, 1973), p. 7.
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less susceptible of malarial influence than others, but very few are absolutely immune. The Chinese, the Malays and some other dark skinned races also appear to enjoy a comparative immunity – an immunity considerably less pronounced, however, than that enjoyed by the African and West Indian negro.26

The Malay and Chinese labourers, like those in the West Indies and Africa, were recruited as labourers in the various plantations of the Malay archipelago and the Caribbean islands. Such a notion of acquired immunity contributed to the discourse of body typologies of productive labourers and to a certain extent informed recruitment policies. In the seventh edition of his textbook Ghosh echoed the exact words used by Christophers when he referred to the partial immunization of adults in a hyper-endemic area: 'There is thus a definite acquired immunity to malaria, comparable to the “salting” of animals in trypanosomiasis.27

When Patrick Hehir wrote Malaria in India in 1927, although he emphasized that 'Against malarial infection there is no absolute immunity, hereditary or acquired', he also reiterated that ‘dark-skinned races, living in malarious regions, possess a relative immunity to malarial infection. This is explicable as an acquired immunity.'28 He further quoted Koch to argue that the ‘acquired immunity’ occurred relatively rapidly in cases where quinine was not used. Hehir presented complex arguments on the factors that led to acquired immunity in certain groups of people, particularly in adults in hyper-endemic areas.29 This emphasis on acquired immunity from malaria also reinforced arguments against the adoption of more expensive quinine prophylaxis as well as anti-malarial sanitation in the plantations. As we shall see later in this chapter, the focus on immunity studies encouraged some planters to promote research to hasten ‘acquired immunity’ among the labourers.

Prevention versus Prophylaxis: Mian Mir and the Malaria Debate in India

The Malaria Committee of the Royal Society, facilitated by the government of India and supervised by Stephens and Christophers, also conducted an early experiment on anti-malarial sanitation between 1902 and 1909 at Mian Mir, a military cantonment near Lahore in the Punjab. The experiment, according to W.F. Bynum, ‘attracted a passion rivalled in the history of malariology only by the decades long bickering between Ross and Grassi’.30 The reports, in brief,
concluded that anopheline control was not feasible in a controlled area. The Indian medical establishment at Mian Mir was responsible for the eclipse of anti-malarial sanitation in India through government initiatives for some time. It led to the preference for the large-scale use of quinine as prophylaxis.

Sheldon Watts has pointed out that Christophers, as the chief malariologist of India until the mid-1930s, in his official capacity as Director of the King Institute and later at the Central Research Institute, kept the focus of malarial interest in India away from the canals and irrigated rice fields and instead concentrated on a quinine policy. Watts has argued that this was also motivated towards preserving the irrigation policy of the government in British India, particularly the canals in the Punjab, from which private investors in England earned rich dividends and the government of India reaped the benefits of substantial agricultural revenue.

Watts’s thesis requires certain qualifications. The irrigation canals of the Punjab were important, but not merely as a source for generating revenue for the gentlemen capitalists in England. The canals were the basis of the agrarian economy of British Punjab, which was commercialized and expanded greatly under British rule. Two-fifths of the army in colonial India were recruited from the rural peasantry of the Punjab; the loyalty of this army, so crucial after 1857, was tied to the prosperity of the agrarian economy of British Punjab. And as Ira Klein has pointed out, Watts ignored the various studies especially, by Bentley in Bengal, that drew links between ecological degradation and the incidence of malarial fever in colonial Bengal.

Moreover, there was more to the evolving discourse of malaria and practices of anti-malarial sanitation in India than a simple dichotomy between the 'sanitarian' approach and the 'scientific one' of malarial prophylaxis through the use of quinine. Most malariologists in India emphasized quinization, led as they were by Christophers at the Malaria Bureau of India. But even Ross, who battled the IMS establishment to focus more on anti-malarial sanitation, argued that

We do not yet know all the dangerous species of mosquito, nor do we even possess an exhaustive knowledge of the haunts and habits of any one variety … Before practical results can be reasonably looked for, however, we must find precisely (a) what species of Indian mosquito do and do not carry malaria? (b) What are the habits of dangerous varieties?

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Therefore there was no question of indiscriminate implementation of anti-malarial sanitation even by its strongest advocate. It entailed detailed research into the habits and breeding places of anopheline mosquitoes. It is here that the question of 'locality' became important in the research into specific anophelines and their habitations. In the next three decades, malarial research all over the world as well as in India would demonstrate the enormous variety of types as well as habits and breeding of anopheles, which were found to differ from one terrain to another, thereby making the question of locality a crucial one. In 1913, still convinced that the destruction of parasite-carrying anopheles was possible, Ross presented the success of the campaigns towards total extermination of malaria in the Panama Canal, in Ismailia in Egypt (near the Suez Canal) and in rubber plantations of the Malay Straits – these were all colonial enclaves, where controlled extermination of anopheline carriers was possible. Despite the rhetoric of European settlements in the tropics, Ross did not contemplate large-scale prevention in rural areas in either Africa or India. Many years later Malcolm Watson would point out that Ross had insisted that he was 'much misrepresented' and that he had 'never thought and certainly never stated that it would “be possible to exterminate mosquitoes throughout Africa, for instance. I have always referred especially to large towns.”' Therefore it is essential not to posit Ross’s anti-malarial sanitation in direct opposition to the quinization policy of Christophers and the Indian medical establishment.

In India, Ross’s advocacy of anti-malarial sanitation had little impact. The experiments at Mian Mir cantonment by the government of India were said to hinder the possibilities of sanitation in the entire country in the first two decades of the twentieth century. Significantly, as Bynum has pointed out, the Mian Mir experiment involved not only the destruction of anophelines through various means but also attempted control through quinization of the troops at the cantonment. Segregation of the European troops through the removal of native habitations, which were close to the European barracks, ‘first, a syce line, and then a whole bazaar’, was also undertaken. Christophers and some of his colleagues, such as S.P. James and J.A. Sinton, were undoubtedly the most vocal and outspoken partisans of the employment of quinine prophylaxis as the best mode to control malaria. But the quinization approach was combined with sporadic implementation of anti-malarial sanitation as well as segregation, which were all attempted, in various degrees, in the tea plantations.

Anti-malarial Sanitation and the Importance of Location

As research in Tropical Medicine translated into the field, it reiterated the significance of the 'local' conditions, using the plantation enclaves as their

36 See Watson, 'Malaria and Mosquitoes', p. 483.
37 Bynum, 'An Experiment', p. 112.
experimental ‘fields’. This was especially applicable in anti-malarial sanitation programmes. At the turn of the century medical research in India was conducted mostly by individual IMS officials, with very little institutional support from the government. Ross’s own experiences in this regard when he was posted in India was typical of the IMS’s attitudes towards research in the field. The establishment of the Royal Society’s Malaria Committee, and the two institutes of Tropical Medicine at Liverpool and at London, contributed to the gathering of momentum of research on Tropical Medicine. In India, malaria research was initially conducted at the Central Research Institute and carried on by the Indian Research Fund Association, which was set up in 1911 and subsidized partly by the government of India. It was funded in part through private subscriptions and published the Indian Journal of Medical Research, which was edited by the Sanitary Commissioner of India and the Director General of the IMS. It also appointed the Scientific Advisory Board, of which both Christophers and Ross were members. Besides the army cantonments, other sites where they attempted anti-malarial sanitation were at the industrial enclaves of mining towns and tea plantations (Mian Mir, for example, was a cantonment).

The Imperial Conference on Malaria in Simla, 1909, led to the formation of a Central Malaria Committee, to direct the course of anti-malaria operations in the different provinces through their supervision of the special provincial malaria committees. At the 1909 conference in Simla J.T.W. Leslie, the Sanitary Commissioner, pointed out that the Drainage Committee of Bengal had found that the success or the failure of any anti-mosquito campaign depended on the study of the local ecological conditions in a targeted area. Therefore the occasional sanitarian approach to the prevention of malaria in Bengal, as in the rest of India, emphasized a close knowledge of local ecology and disease patterns. The assumption was that preventive work on malaria in any region could only be successful if it were both selective and circumscribed. The logic of location, so pervasive in British Indian medical discourse, outlasted the sanitarian epidemiological models and became a significant factor in malaria research:

Although our knowledge of the etiology of malaria and its treatment is fairly extensive … the scientific study of its epidemiology is only beginning … we cannot devise the simplest and best preventive measures until the epidemiology is thoroughly understood … we have little exact knowledge of the distribution of malaria in the country, of the local conditions which favour it, and of the best means to render these causes inoperative.

40 Harrison, Public Health, p. 297.
41 Sanitary Commissioner to the Secretary, Municipal Department, Government of Bengal Proceedings, Municipal/Sanitation, nos 1–2, March 1911, IOR/P/8686 (APAC), p. 3.
42 Sanitary Commissioner to the Secretary, Municipal Department, Government of Bengal Proceedings, Municipal/Sanitation, nos 1–2, March 1911, p. 3.
43 Extract from proceedings of the Malaria Conference in Simla held 12–18 Oct. 1919.
This was in accordance with the general views of the government of India on sanitation programmes of any kind in India; the failures of which, it stated, were due to the short-sightedness of the local sanitary boards and the ‘apathy, fatalism, and resentment of interference’ of the ‘uneducated masses’.  

Medical officials believed that the success of any sanitary reform for the civilian population in India depended on the knowledge of the local conditions: the people as well as the land. Any move for sanitary reform must recognise the diversity of local conditions in a country which includes numerous communities, castes and creeds and which exhibits almost every variety of climate, temperature, humidity and level of sub-soil water, from the Deltas of Bengal with their steamy atmosphere and dense lush vegetation to the burnt brown hills of the north-west frontier.

The emphasis on locality and local knowledge that formed such a strong element in colonial administration in India informed medical research and practice at various levels. British Indian epidemiological theories emphasized the agency of local factors in the causation of disease. The debate on the aetiology of cholera in the late nineteenth century, and D.D. Cunningham’s rejection of Koch’s theories and then his modified acceptance of the idea of the cholera germ, demonstrated the ecological aspect of the emphasis on the local. The tenacity of the Indian medical establishment’s perception of India’s disease terrain as both unique and as a territory that needed experience and familiarity to be medically understood demonstrates the persistence of the rhetoric of the ‘local’. The insect-vector theory and the subsequent research on malaria reinforced the idea of the crucial importance of local disease factors. This notion was compatible with the British administration in India, where local knowledge was both a condition and a validation of rule. The diversity in the anopheline species, and the variables in their breeding patterns revealed by research, highlighted the local to the degree that, except for the assumption that anopheline mosquitoes caused malarial fever, very little else could be taken as given. This emphasized the point that no steps could be taken regarding the prevention of malaria in any area without exhaustive malarial surveys taken beforehand. Or else, as likely as not, they would prove to be a waste of resources. The link between ecology and epidemics was made by medical men in Britain as well in the interwar years. As Mendelsohn has pointed out, in the interwar years medical scientists...
working on ‘bacteriological epidemiology’ in Germany and Britain borrowed increasingly from older traditions of epidemiology as well as new mathematical models to formulate what he described as ‘holistic’ and ‘non-reductionist’ explanations of epidemics. This culminated in the concept of ‘equilibrium’, which included ecological explanations and mathematical models to analyse the state of ‘natural equilibrium’ between host and pathogen. But although IMS officials working on malaria in the interwar years probably borrowed their epidemiological ideas from contemporary British medicine, or at least were aware of the trends, in India this trend long preceded that in Britain, particularly in the activities of the anti-contagionists in the nineteenth century. Moreover, the concept of the ‘holistic’ medical approach differed between Britain and its colonies. While in Britain it was about combining complementary medical approaches, in the colony this holism really represented locating causation in the wider cultural practices of the disease-stricken, as well as economic factors relating to the inclusion of many parts of India into the network of the colonial economy.

Between 1906 and 1927 sustained malaria research by the IRFA and the Malaria Bureau of India resulted in a ‘malaria map’ of India. Medical experts surveyed specific areas, some of which they selected for malaria research. The identification of these selected areas depended on the links between malaria control and economic productivity or political strategy. For instance, when the plans for a new capital at Delhi were being finalized, a malaria survey recommended locating the imperial capital at the southern site rather than the northern, which was found to be more susceptible to malaria. When a new port was urgently needed on the east coast of India, the safe harbour of Vizag was found to be ideal except for the problem of malaria. In this project, the port authorities took advice from the experts at the Malaria Bureau and funded the necessary measures for anti-malarial works. The metropolitan cities of Bombay, Calcutta and Madras all had certain anti-malarial surveys conducted. The malaria map and the surveys described the conditions of areas that were of particular importance either politically or strategically, or from a commercial point of view.


48 The ‘malaria map’, funded mostly by the IRFA in urban, commercial and industrial sites in India, was highlighted by the League of Nations’ Health Committee, which visited India and compiled a report on malaria at the invitation of its government in 1926–27. Report of the Malaria Commission, p. 17. This followed the widespread malaria surveys of the League’s Health Committee after the First World War.

The recurrent epidemics in rural lower Bengal had devastated many areas in the nineteenth century and contemporary British medical discourse relegated the disease in Bengal to a civilizational degeneracy, beyond even the reforming momentum of British rule. The Bengal government conducted some anti-malarial operations in the highly endemic lower Bengal, particularly Burdwan, between 1906 and 1911, but these were largely unsuccessful. The anti-malarial operation at the Meenglas Tea Estate in the Duars was different, because the area of operation was a tea plantation with boundaries and under the control of its management. The Meenglas estate was owned by the managing agency of Duncan Brothers Limited. The funds for the Meenglas experiment were paid entirely by the Bengal government. The operations lasted about eight years, and in many ways set the precedent for the course of anti-malarial work in the tea plantations for the next 25 years. The issues that emerged from Meenglas were thus of crucial importance.

The work at the Meenglas Tea Estate commenced in 1917. C.A. Bentley made the initial proposal for the project in 1914 and achieved it by diverting funds from the IRFA for jungle-clearing in the Murshidabad district, reinforcing the privileged status of the plantation enclaves. The government simultaneously proposed a second site for a similar project in Asansol, at the site of British-owned mines, but this did not materialize. The aim of the Meenglas ‘experiment’ was to prevent the breeding of carrier-anophelines where the land was cut up by several seasonal jhoras (streams). In the Darjeeling foothills, the seasonality of the streams depended on the terrain; those on the slopes remained dry for most of the year, while those closer to the plains flowed continuously except in the summer. Other ecological characteristics of the area were the proximity of jungles, rice fields and three fast-flowing rivers. The aim of the ‘experiment’, apart from the usual examination of anophelines to determine the carriers, was to attempt subsoil drainage to control the breeding of carrier-anophelines in a small, targeted area. The controlled targeted area was initially three-quarters of a mile, following the view of Malcolm Watson, whose successful policies in Malaya had shown that the flight of anophelines did not exceed half of a mile. Although the Meenglas experiment was not as controversial as that of Mian Mir, medical opinion about its success was still qualified.
At Meenglas, the local characteristics of the anophelines were further emphasized by the identification there of the three most dangerous carriers: *A.maculatus*, *A.listoni* and *A.culcifacies*. These bred in clear, running streams. But the medical experts found that the anopheline which inhabited the jungle near the Meenglas estate, the *A.aitkeni*, was utterly harmless, unlike the *A.umbrosus*

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of the Malayan jungles which was a proven carrier. The Meenglas experiment verified that underground drainage of streams could control the breeding of anophelines. Where they could not implement subsoil drainage researchers used the method of oiling stagnant pools with kerosene to prevent breeding. The experiment demonstrated that the spleen index of the children and malarial fever could be reduced for a limited period within the controlled area.

There were fluctuations in the reports of malaria in the controlled area. In 1924, there was ‘an increase in the crude spleen index, the malaria death-rate, and malaria sickness rate with a decline in the birth rate, and no change in the total death rate’. But in 1925, the Director of Public Health’s department reported that the spleen index in the previous year had declined from 53.2 to 51.8, and therefore the effect of the operations was satisfactory. In the next two years the spleen index at Meenglas was recorded at 56.5 in 1925 (this contradicted the figures given in the report for the year 1925) and 56.6 in 1926, the average of the previous quinquennium being 59.2. The death rate from malaria was 1.5 per mile in 1926, against 4.6 in 1925, and the average of the previous quinquennium was 5.09. The death rate from all causes was 39.2 in 1926, against 33.2 in 1925 and 39.4 the previous quinquennium. These numbers were moreover qualified, because the ‘number of malaria deaths might be more than actually returned’, since the number of deaths under ‘other fevers’ rose in the same period.

One fact stood out from the results of the Meenglas ‘experiment’: the average overall death rate in the Meenglas tea estate did not decline. The labourers continued to die from causes other than malarial fever such as diarrhoea and dysentery, chest complaints and other afflictions. This proved that anti-malarial measures in themselves would not reduce the overall death rates of the workers drastically. Admittedly, the effects of malaria included sickness and debility more than a quick death. The reports stated that the spleen index first decreased and then stabilized, and remained ‘almost static’ from 1923 to 1927.

The Meenglas operation aimed not just to reduce malarial fevers within that particular tea plantation estate, but also to retest Watson’s theses regarding subsoil drainage that had proved expensive but useful when applied in the rubber estates of Malaya. In effect, Meenglas was to demonstrate the feasibility of subsoil drainage operations for larvae control for the tea estates all over the Terai and the Duars. The report concluded,

59 Annual Report for the Director of Public Health, Bengal, 1925, pp. 43–44.
61 Annual Report for the Director of Public Health, Bengal 1926, p. 50.
The measures put into operation at Meenglas for the reduction of anopheles have been entirely successful … As regards the actual reduction of malaria, this is a point on which it is very difficult to form an exact opinion, mostly owing to the factor of shifting population. Only a small population of the labour is permanent, others come and go. Judged by the spleen index in the whole community, there would appear to have been little achieved …

That there was little evidence either way that malaria could be reduced within the plantation enclaves through subsoil drainage in a controlled area reiterates a public health problem quite familiar to any preventive project in the tea gardens – that of the mobility of the free labour. We know that the basti labour flanking the tea gardens were used in peak times but the management did not assume responsibility for them. The system of tea production relied on the seasonal labour from the bastis; yet every epidemic disease in any plantation was rumoured to have originated first from the bastis. Yet the logic of the production of tea demanded a labour force that would work in the peak periods and preferably be settled outside the plantations, so the management was relieved of year-round responsibility towards them. This paradox, referred to in the last chapter, was starkly in relief after the Meenglas scheme.

There was one more, crucial conclusion from the Meenglas experiment. The survey noted that ‘in such a hyper-endemic district as the Duars, anti-mosquito measures in a restricted area are apt to give benefits apparently hardly commensurate with the trouble taken’. The problem was stated in clear terms: all the tea plantations in the area had to invest in anti-larval schemes for the reduction of malarial sickness to be effective. The cost of the entire project was Rs 16,000 (initial cost) and an annual expenditure of Rs 800 for the maintenance of the drainage and oiling. If all the plantations in the area agreed to cooperate and conduct anti-malarial operations simultaneously, the incidence of infection would decrease; otherwise, as an isolated experiment the Meenglas would not be particularly successful. This had, indeed, been the conclusion of the Assistant Director of Public Health, Malaria Research, R.B. Khambatta, who had visited Meenglas in July 1923 accompanied by the Director of Public Health. Khambatta had also served as acting Civil Surgeon for Jalpaiguri and was therefore familiar with the health issues of the tea plantations. The project was jeopardized also by the migration of anopheles from outside the drained areas. When researchers deemed insufficient the quarter of a mile radius for effective control, they coaxed the neighbouring tea plantations into investing a little on spraying kerosene in their streams. This was inadequate, for in the

67 DPAAR, 1923, p. 105.
68 Report of the Malaria Survey of the Jalpaiguri Duars, p. 5. The adjustment from the three-quarter mile to the entire Meenglas tea estate was based on Ross’s calculation of M=1–40/a, M representing the infection rate and ‘a’ the anopheline mosquito per head,
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The final report on the project, a survey, demonstrated that the spleen index in Meenglas itself improved, but the scheme seemed not to have any effect in lowering the endemicity in the area.

whereby, it was found at the end of 1920, ‘that to keep up the spleen rate of 93.58 percent … a minimum number of 667 carrier mosquitoes per head of population was required, that is by the scheme as it stood then, not more than five-sixths of the original rate of mosquitoes per head could be reduced’. The Public Health department in Bengal under Bentley was thus experimenting with the findings of Ross and also Watson, who first implemented subsoil drainage in Malaya. The application of mathematical calculations to determine the extent of infection in a locality was begun by Ross in 1904 and used by the Ross Institute in Ceylon in 1930. See Gordon Harrison, Mosquitoes, Malaria and Man: A History of the Hostilities Since 1880 (London, John Murray, 1978), p. 206.


Table 7.1 Spleen index of tea estates in Mal tea district of Duars in 1926

<table>
<thead>
<tr>
<th>Tea estate</th>
<th>Number examined</th>
<th>Spleen index</th>
</tr>
</thead>
<tbody>
<tr>
<td>Meenglas</td>
<td>394</td>
<td>53.5</td>
</tr>
<tr>
<td>Lower Fagoo</td>
<td>134</td>
<td>60.4</td>
</tr>
<tr>
<td>Nedeem</td>
<td>99</td>
<td>77.7</td>
</tr>
<tr>
<td>Sylee</td>
<td>294</td>
<td>75.8</td>
</tr>
<tr>
<td>Dainkote</td>
<td>194</td>
<td>86.1</td>
</tr>
<tr>
<td>Rangamati</td>
<td>410</td>
<td>89.2</td>
</tr>
<tr>
<td>New Glencoe</td>
<td>255</td>
<td>89.4</td>
</tr>
<tr>
<td>Neora Nuddee</td>
<td>123</td>
<td>86.2</td>
</tr>
<tr>
<td><strong>Total and average</strong></td>
<td><strong>1903</strong></td>
<td><strong>76.6</strong></td>
</tr>
</tbody>
</table>


Table 7.2 Spleen index of tea estates in Nagrakata tea district of Duars in 1926

<table>
<thead>
<tr>
<th>Tea estate</th>
<th>Number examined</th>
<th>Spleen index</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kurti</td>
<td>232</td>
<td>64.2</td>
</tr>
<tr>
<td>Hope</td>
<td>276</td>
<td>82.6</td>
</tr>
<tr>
<td>Jiti</td>
<td>335</td>
<td>84.5</td>
</tr>
<tr>
<td>Hille</td>
<td>208</td>
<td>85.6</td>
</tr>
<tr>
<td><strong>Total and average</strong></td>
<td><strong>2954</strong></td>
<td><strong>77.7</strong></td>
</tr>
</tbody>
</table>


The final report the problem was highlighted yet again: ‘the infected mosquitoes are still migrating from the neighbouring gardens. If this influx could be stopped (and this can be done only when the surrounding gardens would adopt similar measures) the effect would be immensely greater.’
There was a final crucial point in anopheline control in Meenglas, and that was the existence of paddy fields close to the plantations and often within them. As we have seen, many plantations in the region allotted a part of their vast estates (staked out as ‘wastelands’) to the labourers where they grew vegetables and some rice. This served both as an inducement to retain labour in the tea plantations, for the allotments were made on the condition of work, and also to keep the wages low. Moreover the allotments served as instruments of control, for the workers had no tenants’ rights to their allotments and the Royal Commission on Labour (1931) recorded that such bari allotments to workers could be cancelled and the worker dismissed without notice.\(^{70}\) The cultivation of rice required a great deal of stagnant water in the fields and bred carrier-anophelines. For the Meenglas experiment, the cultivation of rice was stopped once the problem was identified, but that was within the Meenglas tea estate. Neighbouring estates did not follow up with like measures, therefore the migration of anopheline mosquitoes continued to subvert anopheline control within the experimental area. The final recommendations for the report unambiguously insisted, ‘Paddy cultivation should not be allowed.’\(^{71}\)

The factor of the cultivation of rice in the increase of malarial fever was acknowledged by C.A. Bentley who was the Director of Public Health in Bengal in 1925.\(^{72}\) Arabinda Samanta reads Bentley’s report uncritically as a condemnation of British policies of creating railways and embankments, which contributed to the lack of inundation, leading to stagnant anopheline breeding water in many parts of lower Bengal. The contribution of roads and railways to malaria in Bengal was the source of great debate in colonial India, and there was nationalist criticism of the destruction of traditional embankments and new construction sites. However, Bentley’s contribution to the debate was significant because his solution was to implement the Italian concept of bonificazione, which concept, he said, ‘embodies measures designed for a double purpose, viz. to improve agriculture and improve health.’\(^{73}\) In the context of (Western) Bengal he advocated anti-malarial sanitation not through the drainage of rice fields, but through further inundation, preferably through irrigation on the model of Punjab and Sindh. Therefore he did not offer a criticism of the development policies of the government, but rather sought to clearly probe the problem and suggested solutions that would lead to more, rather than less, investment in irrigation.\(^{74}\) Bentley first read a summary of his thesis in 1913, in a paper titled ‘Some Problems Presented by Malaria in Bengal’ at the sanitary conference in Madras. His conclusions did not find favour with the British and Indian medical establishment. In an editorial the Indian Medical Gazette declared

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70 RCLI, pp. 384–85.
72 C.A. Bentley, Malaria and Agriculture in Bengal: How to Reduce Malaria in Bengal by Irrigation (Government of Bengal, 1925). Also see Samanta, Malarial Fever, esp. pp. 33–73.
73 See Bentley, Malaria and Agriculture, p. 125.
74 For a criticism of his thesis that flooding, not drainage, would solve the problem of malaria in the Bengal plains, see C.A. Bentley, ‘Malaria and Agriculture in Bengal’, The Lancet, 206 (31 Oct. 1925), pp. 926–27.
that Bentley’s assessment could be ‘misinterpreted’ (doubtless as a critique of colonial modernizing strategy) and stressed that his views were those of an economist, not a scientist. It claimed that depopulation in parts of Bengal had occurred due to the natural silting up of rivers, and ‘Drainage and engineering schemes can effect but little against nature.’ The IMG went on to conclude that natural devastations had always occurred over time everywhere in the world, and that ‘During all great natural changes people who cannot adapt themselves to the changing environment must necessarily suffer.’ The contradictions of the logistics of malaria control within the plantation economy were fundamental and could not be resolved, it appeared, from within it.

There was one further aspect to this contradiction. Anophelines of all varieties caught at the Meenglás Tea Estate and its environs were diligently examined. Some were sent to the Central Malaria Bureau at Kasaulí for examination. The investigations revealed that there were, in total, 14 varieties of anophelines in the area. However, the dangerous carriers were fewer in number, such as the A. maculatus, A. minimus and A. culicifacies. A survey of the ecology of malaria in the jungle areas of Bengal revealed that jungles did not breed anopheline carriers. Rather, the clearing of jungles and the substitution of tea bushes (or rice fields) did away with the harmless anophelines such as A. aitkeni and A. barbirostris. Once the jungles were cleared, they were replaced by the carriers:

The coolie lines nearest the jungle are the least malarious, those situated in the middle of the open area have breeding of carrier species going on all sides of them. Madarihat, in the Duars, situated in a clearing in the jungle area, is notorious for its malaria. Here within the jungle itself only A. barbirostris and A. leucosphyrus were found, where in the cleared area A. maculatus, A. minimus, A. culicifacies, A. fuliginosus, A. maculipalpis, and A. philippinensis are found. Wherever deforestation is carried out, the harmless jungle species of Anophèles disappear and are replaced by the dangerous carrier species.

It seemed the carriers of malaria were to be found in the rice fields and in the cleared areas of the tea estates, breeding in small jhoras or seepage areas. Sustained investigations revealed that malaria was, in effect, compatible with habitation and human livelihood itself. As to whether that led government policymakers to think of malaria as a consequence of modern agrarian development, distressing yet somehow inevitable, is not certain. But these conclusions, and simultaneous studies like those of Bentley on the links between embankments and malaria, led to the conceptual linking of malaria to modernity and development. It also contributed to the nationalist critique of British policies

76 Bhupendra Mohan Khan, ‘Records of Anophelines from the Bengal Dooars’, Indian Medical Gazette, 64 (1929), p. 496.
78 Bentley, Malaria and Agriculture in Bengal, esp. pp. 21–37, 48–64. For the links between nationalist discourse on malaria and the construction of roads, railways or embank-
in India. In the case of the rice fields of Bengal, Bentley had provided a solution: more agrarian development through irrigation. Bentley added another twist to the *bonificazione* scheme – he coined the term ‘human bonification’, by which he meant the encouragement of voluntary anti-malaria cooperative societies in the villages that would carry out anti-malaria sanitation (without cost to the respective district boards or the provincial government) at a local level.\(^79\) In the case of the Terai and the Duars no ecological solutions were conceptualized and local anti-malaria societies were non-existent.

So far as the provincial government was concerned, the experiment at Meenglas demonstrated that anopheline control was possible under two conditions. First, it would require the cooperation of neighbouring tea estates in any region. Coordination between neighbouring tea gardens had simultaneously to begin and sustain malarial operations and this was difficult.

Second, paddy cultivation within the tea estate lands would have to be stopped. Since the political economy of the plantations depended on the labourers’ access to land to grow paddy, this was not a condition likely to be met. The situation was complicated by the fact that the planters used the results of the Meenglas experiment to situate the source of malaria in the rice fields in the *bastis* outside the tea plantations.

Not just in the Duars, but in the whole of colonial Bengal the rice fields were held to be the cause of malarial fever in the twentieth century. Just as they were impossible to obliterate in the rest of Bengal, the case for malaria control in the Duars too was laid to rest with the emphasis on paddy cultivation. Similarly, the study that revealed the direct link between deforestation, the obliteration of harmless anophelines and the rise in the breeding of dangerous anopheline carriers implied a similar condition; tea bushes had been claimed from the wild jungles and malaria seemed an inevitable by-product of the modernization and settlement of these parts. This was ironic, for it had been the call of Tropical Medicine to render the ‘gift of half of the world’, to make it possible for humanity to penetrate impenetrable tropical jungles and make them habitable.\(^80\)

\(\)\(^79\) Government of Bengal Proceedings, Local Self-Government/Local Boards, no. 31, IOR/P/11569 (APAC), pp. 69–70.

A Tale of Two Sites: Mian Mir and Meenglas

Ronald Ross visited India in 1926–27. On this trip he attended the inauguration of the commemorative gate raised in his honour at his old laboratory in Calcutta at the invitation of the Director of the new Calcutta School of Tropical Medicine (CSTM), J.W.D. Megaw. The Indian Medical Gazette pointed out that his visit was ‘exceedingly timely’.  

This was in reference to the increase in the number of malarialogists in the IRFA, and the appointment of qualified malarialogists at some railways and at the Vizagapatam harbour. It referred too to the increase of interest on the part of some industrial and commercial concerns, such as the Bombay cotton-mill owners and the members of the ITA, which, it stated, took ‘malaria in the tea gardens of the Dooars and Assam very seriously’.

Therefore Ross’s visit to the Duars and Terai area, his comments on the Meenglas experiment and his speech to the planters in the region can be analysed to delineate the issues crucial to the management of malaria in the Darjeeling foothills.

Ross made the trip to the plantations in India as a representative of the new Ross Institute at Putney, where research into Tropical Medicine was to be supported by industrial interests. He gave lectures at the Terai Planters Club and the Dooars Planters Club and later visited Malaya at the invitation of Malcolm Watson. He also visited the operations at Meenglas on 20 January 1927 and insisted that the project would have had assured success if the project had been extended to a wider area.

Almost two decades previously, in the acrimonious debate on the failure of the anti-malarial measures at Mian Mir, Ronald Ross had alleged both the lack of adequate data and the faulty application of scientific knowledge in the Mian Mir operations. To him the fact that anophelines could be destroyed in any area had acquired perfect certainty; what remained to be done was to calculate certain variables and local factors:

The logical basis of the great measure of mosquito reduction is absolute … The proposition, like the multiplication table, does not require experimental proof, and is incapable of disproof … We have still to determine (a) the radius of operations required to reduce the density of a given species of mosquito to a given percentage and (b) the percentage of mosquito reduction required in order to obtain ultimately a given percentage of malaria reduction.

One of the criticisms by Ross of the Mian Mir operations was that too little money had been spent, and if a cantonment had to be made free of malaria the

authorities would have to invest in anopheline control in the same way that they would think to invest in drains and sewerage. In Meenglas the malarial surveys were made; the entomological studies and spleen index in Meenglas and neighbouring tea estates were examined. Ross found subsoil drainage too expensive for the terrain; Rs 16,000, plus the annual expenses of drainage, had been spent by the state government in Meenglas. The Meenglas experiment mapped the anopheline infectivity of two districts in the region, and reduced the spleen index of the Meenglas tea garden to a certain extent. The problems faced by the medical experts at Meenglas were not lack of scientific knowledge or unwillingness to apply that knowledge, but the logistic impossibility of extending the area under operation. While anti-malarial operations clearly pointed out that the stream-breeding anopheles could easily migrate from neighbouring tea estates, the management of the estates would not invest in subsoil drainage. Meanwhile, the government’s budget for the operation was limited and would not extend to cover the entire region.

Although Ross found many faults with the anti-malarial operations at Meenglas, it did not generate a controversy to the same extent. The experiment at the ill-fated cantonment of Mian Mir had been such an embarrassment that its very name had to be changed to Lahore Cantonment to avoid the notoriety associated with anti-malarial measures at that site. By the time the Meenglas experiment took place, malarial research of the previous two decades had increased the sheer volume of information about malaria. There was now sophistication in malarial research and the many variables of ‘species sanitation’ prompted more detailed sanitarian measures. They included methods of spraying (spray cans were first used and discarded, and a special pack was designed for the coolie sprayer) and subsoil drainage. The report on Meenglas also recommended locating the coolie lines at the centre of the plantations, as far as possible from the infective bastis and rice fields. Since most of the land in the tea plantations was utilized already, and the coolie lines were usually situated at the borders of the estates, this recommendation was not particularly realistic. Nor was the suggestion that the cultivation of paddy be stopped, for that disrupted the logic of the plantation economy. So far as provincial government was concerned, the agency for control of malaria was now vested in the planters themselves.

The significance of the ‘controlled’ experiment with anti-larval measures at Meenglas lies in its consequences for anti-malarial sanitation in other malarial-endemic areas of Bengal. The Bengal government asserted that the experiment at Meenglas (and a similar experiment at the mining sites of Singaran and Topsi

85 Bynum, ‘An Experiment That Failed’.
86 The term ‘species sanitation’ was first used by Swellengrebel-Graf in 1919, and denotes the destruction of specific anopheline mosquitoes in a particular breeding environment. It involved prior investigation within the targeted area to determine the carriers unique to the locality. See D.J. Bradley, ‘Watson, Swellengrebel and Species Sanitation: Environmental and Ecological Aspects’, Parasitologia, 36 (1994), pp. 137–47.
Information of the greatest value has been gained by these two experiments, which show the extraordinary difficulty of producing a reasonable reduction of malaria … [T]he cost of effective anti-mosquito measures of the kind employed with success at Panama, and more recently in parts of America, is in the present financial condition of the country likely to prove an insuperable obstacle to success.\footnote{Annual Report for the Director of Public Health, Bengal, 1920, p. 15.}

The lessons learnt at Meenglas were used by the Public Health Department to negate the possibility of drainage operations in any of the cultivated sites of the intensely malarial lower Bengal.

Tropical Medicine and Entrepreneurial Patronage

The institutionalization of Tropical Medicine was effected at the turn of century. While the metropolitan government supported the London School of Tropical Medicine, largely private interests supported the Liverpool School. The Liverpool School sent research expeditions to many places in the tropics for malarial research – Sierra Leone, Gold Coast, Panama, Egypt and Greece (which was not in the tropics but was malarious) – between 1899 and 1914. Some of them were commissioned specially by the Suez Company, for instance.\footnote{For a comprehensive list of the general details, finance and research output of the Liverpool School, see Power, *Tropical Medicine*, pp. 249–55.}

In the first decade of the twentieth century, as we have seen, the government of India under Curzon established some research institutions in India. The Board of Scientific Advice did not initially focus on medical research, but rather on botany and geology.\footnote{Roy M. MacLeod, ‘Scientific Advice for British India: Imperial Perceptions and Administrative Goals, 1898–1923’, *Modern Asian Studies*, 9 (1975), pp. 343–84.} But it gave an impetus to organized scientific research ‘contained well within the government’, which was different from the more individualistic and dispersed scientific research of an earlier period.\footnote{MacLeod, ‘Scientific Advice’, p. 383.} A great deal of research on malaria took place in the first two decades of the twentieth century at various research institutes in India.\footnote{O.P. Jaggi, *Medicine in India: Modern Period* (Delhi, Oxford, Oxford University Press, 2000), pp. 161–64.} Most of them were funded and motivated by various government agencies.

The CSTM was instituted in 1921.\footnote{H. Power, ‘Sir Leonard Rogers FRS (1868–1962): Tropical Medicine in the Indian Medical Service’, PhD thesis, University of London, 1993, pp. 143–82.} The CSTM, and attached with it the Institute of Hygiene and Carmichael Hospital, was funded by the governments of India and Bengal, and a large number of donations from the Indian elite. It also succeeded in attracting a few subscriptions from British-dominated industries.
in eastern India such as the jute, mining and tea industries.\textsuperscript{94} After a great deal of correspondence, the ITA at Calcutta agreed to the payment of twenty thousand rupees for five years to support research on kala-azar in the Assam tea plantations. This worked out to one \textit{anna} per acre of tea under cultivation for all members. After four years, research on kala-azar had exceeded the sum sanctioned and now the total cost of the research in Assam was Rs 27,200. The ITA suggested that the kala-azar research in any case overlapped with research conducted by the state of Assam; therefore the research funds might be now directed to malarial studies in the tea plantations. The CSTM was unwilling to abandon the research at this late stage. After negotiations, they decided that the excess of Rs 7200 would be paid by the ITA. On its part, the CSTM would depute C. Strickland, head of the entomology department, to conduct a preliminary survey of malaria in Assam and northern Bengal. The ‘expert survey’ could cost around Rs 13,000 per annum.\textsuperscript{95} The ITA noted that ‘The idea was that, with the results of the preliminary investigation as a guide, it would be much easier to estimate the utility of the suggested malarial research.’\textsuperscript{96}

The CSTM’S survey was done in Assam, where the total production of tea was more than two-and-half times that of northern Bengal. The next year, at its annual meeting, the Chairman of the Dooars Planters Association floated the idea of a research institute in the Duars, perhaps as a branch of the CSTM, to investigate malaria in the Duars. The British doctors employed by the plantations were also in favour of it. However, the suggestion of the Chairman was directed not only to members of his Association but also to government:

to the Government of India … I maintain that the Dooars presents unique conditions for the study of malaria and other obscure tropical diseases in so much as there is a large population living under conditions which allow the history of treated cases to be followed up for years.\textsuperscript{97}

The Chairman of the ITA, who was also present as a guest at the meeting, was more realistic about government investment, rejected the idea of spending around Rs 50,000 annually for anti-malarial sanitation and suggested instead a scaled-down ‘preliminary malaria map’.\textsuperscript{98} The DPA decided this was useful and in 1926 the CSTM sent Strickland to Duars who conducted a preliminary survey. The DPA declared that the malaria survey was ‘a valuable basis for future campaigns’.\textsuperscript{99}

\textsuperscript{95} \textit{IPTAAR}, 1923 (Calcutta, 1924, APAC), p. 27.
\textsuperscript{96} \textit{IPTAAR}, 1923, p. 28.
\textsuperscript{97} \textit{DPAAR}, 1925 (Jalpaiguri, 1926, APAC), p. ix.
\textsuperscript{98} \textit{DPAAR}, 1925, p. xx.
\textsuperscript{99} \textit{DPAAR}, 1926 (Jalpaiguri, 1927, APAC), p. vii. Strickland’s report on Assam included recommendations for drainage and flushing, and also ‘education’ of the coolies. See C. Strickland, \textit{Abridged Report on Malaria in the Assam Tea Gardens} (Calcutta, Indian Tea Association, 1929). Although the anophelines of Assam had characteristics that were
As we have seen, Ross visited the area in 1926–27. He was invited to the annual meeting of the DPA and addressed the gathering of members and guests. Also present was C.A. Bentley, the Director of Public Health in Bengal who had assumed responsibility for the overall supervision of the Meenglas project. One of Ross’s objectives was to gather support for the newly formed Ross Institute at Putney, London. He began by referring to his visit to the Darjeeling Terai 28 years ago, and remarked on the prosperity of the tea industry since then: ‘the majority of those present appear to have thriven on it’. He then demanded,

I now ask what does the Dooars intend to do? There have been several Commissions of investigation in the district in bygone times, including Dr Stephens and Dr Christophers about 26 years ago, and Colonel Christophers and Dr Bentley in 1908, another Commission a year or two later, and more recently you have had the work of Colonel Stewart and the comprehensive survey last year conducted by Dr Strickland.

Despite Ross’s attempts to persuade them, neither the DPA nor indeed the planters in Darjeeling and Terai invested in anti-malarial sanitation. Entrepreneurial patronage facilitated certain studies on malaria in the Darjeeling foothills. The arrangement between the CSTM and the DPA resulted in several malaria surveys of the region, including Darjeeling and the Terai. Strickland published several papers on malaria in the hills, in the foothills of Darjeeling, the Terai and the Duars. Most of the studies located the extent of infectivity of carrier-anophelines and the specific conditions under which they could breed, and in one case a tea estate carried out some anti-malarial drainage in Darjeeling. Other malaria research in the region studied comparative causes of epidemics of malaria in hill-stations, such as Shillong (Assam) and Kurseong (Darjeeling). The Terai Planters’ Association also funded a survey once more through the agency of the CSTM.

In this period the tea plantations were sites of malarial research in terms of the opportunities provided by the terrain, the labourers and a limited financial distinct from those of Duars, for instance *A. umbrosis* was a carrier in Assam whereas the Meenglas experiment proved that *A. umbrosis* was harmless in Duars, there were some familiar problems. The chief one was of the existence of rice fields and the cultivation of rice by some of the workers. See also, Strickland, ‘The Mosquito Factor in the Malaria of Assam Tea Gardens’, *Indian Medical Gazette*, 60 (1925), pp. 514–23.

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100 *DPAAR*, 1926 (Jalpaiguri, 1927, APAC), p. xvii.
101 *DPAAR*, 1926, p. xviii.
patronage. Christophers admitted in his preface to the League of Nations Malaria Commissions report that anti-malarial initiatives rarely proceeded from surveys to preventive operations in the tea estates in India. Although he was at pains to project anti-malarial operations in favourable light in most parts of India, he conceded that

At present, after a 'survey' and recommendations, nothing very much often follows, largely because it is then left to the manager of such estates to do what he can, whereas the proper course would be to engage a suitable man to reside on the area and see to the carrying out of whatever was possible.105

During the visit of the Royal Commission on Labour in 1931, the DPA promoted both the experiments at Meenglas and the survey by Strickland as evidence of the planters’ sincere efforts to control malaria within the area as evidence of good intent.106

A crucial hindrance to the efforts on the part of the tea estates in northern Bengal and in Assam to engage in anti-malarial operations was that the managers of the estate were personally responsible for the finances of the tea estate. Any long-term investment in a tea estate would detract from immediate profits and targets, and thereby from the commission received by each manager. Strickland suggested that the managers not be made responsible for the anti-malarial sanitation work: ‘the practice will act like a dead weight on all the efforts of those who are trying to do some good’.107 But the plantation managements made no structural changes to their system of remuneration in response.

The tea industry’s contribution to the CSTM was meagre and limited in scope. As we have seen, it preferred that the government make the investments in both research and implementation of anti-malarial sanitation programmes. The contributions and the resultant surveys seem to have been for rhetorical effect more than anything else.

From the perspective of managerial priorities, immunity studies presented a more attractive proposition than systematic anti-malarial sanitation or the full-scale implementation of a quinine policy. The Ross Institute at Putney, which was supported partly by the ITA in London, opened a branch in Shillong, India, in 1930, consisting of a director, G.C. Ramsay, and one assistant. When drainage measures did take place in India under the supervision of the Ross Institute, they tended to be concentrated in Assam where the large managing agencies had contiguous territories and several tea gardens under their control. In Assam, which was in more remote northeast India, moreover, the import of labour was much more expensive and difficult than in northern Bengal. When some anti-malarial operations did take place in northern India between 1935–39, they were concentrated in Assam rather than Terai or Duars, where both the acreage and the capital outlay were relatively modest. The Ross

106 DPAAR, 1929 (Jalpaiguri, 1930, APAC), p. 11.
107 Strickland, ‘The Mosquito Factor’.
Institute participated in some anti-malarial operations in northern Bengal as well as Assam and further conducted some reports on anti-malarial preventive measures in northern India from the mid-1930s. Medical experts observed that Duars and Terai lacked in the initiatives towards species anti-malarial prevention.

The shortage of quinine during the Second World War aggravated the problem of malaria in all the plantations, including those in northern Bengal. In 1946 two studies were conducted on the living conditions of the plantation labourers and the medical facilities available there. Both the reports were commissioned by the government in the postwar context of the emergent independent state of India. The Labour Enquiry Report of 1946 and the enquiry into the medical facilities received by the workers in the plantations noted the low standards of health and the high prevalence of diseases, including malaria, among the working population of northern Bengal. Deaths by ‘fever’ remained the single largest cause of death in the plantations. As for anti-malarial works, one of the reports commented that

Very little anti malaria work is being done at present in the Dooars and what little is being done is confined to shading and draining of the streams … In the Terai and Darjeeling also very few gardens have done any anti malarial work, in spite of the heavy incidence of malaria. The little work that is done in a few gardens is confined to spraying wells occasionally in the lines.

Enclaves and Sanitation: Research on Hookworm in the Plantations

While anti-malarial sanitation in the plantation enclaves drew the interest of medical researchers, international scientific organizations and the government, another ‘tropical’ disease, hookworm, also drew similar focus on the plantations. Hookworm, or anchylostomiasis, is caused by parasitic worms that attach themselves to the human gut with hook-like teeth. The parasites enter the human body through contact with human faeces, usually by boring tiny holes through bare feet or, less often, through the mouth. Hookworm infestations in human intestines can cause severe anaemia, listlessness and emaciation. Unlike malaria, the hookworm projects represented government’s and international organizations’ resolve to encourage sustained public health programmes through a bio-medical model. The cycle of the disease and the certainty of its cure through the use of thymol caused medical authorities to assume that it

108 The Ross Institute of Tropical Hygiene, London School of Hygiene and Tropical Medicine, Supplementary Report to the Indian Tea Association, 1934, Mss Eur/F174/1212 (APAC). See also Griffiths, History, p. 357.
111 Rege, Labour Investigation Committee, p. 91.
could be eradicated relatively shortly through mass public health campaigns. In contrast to the anti-malaria campaigns, the hookworm projects therefore eschewed both medical research and wider social and economic constituents of disease.

In 1913, the newly formed IRFA provided the requisite funds for equipment and laboratory staff for Clayton Lane, the Civil Surgeon of Darjeeling, to conduct a hookworm survey in Darjeeling district. The Darjeeling Planters’ Association agreed to allow him to investigate the tea plantation labourers for the disease and to treat them if necessary. Major Clayton Lane found that 70 per cent of the working population of the tea plantations suffered from hookworm infestation and attributed this to the lack of latrines in the coolie lines.\textsuperscript{112} He proceeded to medicate the labourers in selected plantations with thymol, which cured existing infections. Although the after-effects of thymol were very unpleasant, he oversaw the de-infestation of hookworm in the targeted area.

But it required a permanent sanitary infrastructure to avoid re-infestation. Clayton Lane attempted to prevail upon the planters to install latrines and borrow pits in the coolie lines to prevent the high incidence. In the hill-station of Darjeeling, human waste was either cleared through septic tanks in selected bungalows, or collected by night-soil carriers and disposed through a chute to the hillsides outside the boundaries of the municipality. But in the coolie lines in the plantations there were no sanitary or sewerage facilities. Hookworm affected the productivity of the labour force drastically, and using an economic rationale Clayton Lane urged the planters:

\begin{quote}
 obviously, putting aside entirely the humanitarian point of view, and taking no consideration of such moral obligations as the employer of labour has imposed on him by his position, firstly that infected labour is not capable of the same output of work as uninfected labour, and secondly that it is less reproductive.\textsuperscript{113}
\end{quote}

Clayton Lane’s project enthused some planters in Darjeeling in the immediate aftermath of the study, and they promised to provide for sanitation for their labourers. He published letters from grateful planters who claimed that his hookworm project improved the productivity of their workforce ‘to an almost incredible degree’\textsuperscript{114} But in a familiar trend, a majority of the planters in Darjeeling chose not to invest in sanitary provisions within the coolie lines – again, possibly because the plantation system relied on the availability of large numbers of cheap, unskilled labour, and sick workers were easily replaceable.

\textsuperscript{112} Clayton Lane, ‘The Treatment of Ankylostomiasis, or Hookworm Disease’, \textit{Indian Medical Gazette}, 50 (1915), pp. 241–45.


In 1918, the government of Bengal accepted assistance from the Rockefeller Foundation (RF) to do a survey of the prevalence of hookworm in rural Bengal. The International Health Division (IHD) of the RF actively encouraged anti-hookworm campaigns in Canada, Brazil, Chile, India, Ceylon and Malaysia. As John Farley has argued, the Sanitary Commission of the RF first conducted anti-hookworm campaigns among the impoverished black population of the American South. In 1910–17, anti-hookworm campaign in several rural counties in Virginia and North Carolina focused on identifying the extent of the disease among the population, curing the infections through mobile dispensaries. Finally, in collaboration with local governments, they provided for ‘public education’ campaigns through public lectures, leaflets and school essay competitions so that the concerned communities could observe hygienic practices to prevent the disease. This programme provided the model for the activities of the IHD abroad, particularly in areas where they perceived that the productivity of labour was severely affected by hookworm disease. The clear framework of IHD was public health philanthropy which they posited against charity. It involved the participation of local governments and facilitation by the RF for dispensaries and public health campaigns in the initial phase. In the long term, the local administration and the community was expected to provide for disease control on their own. In this case, the IHD provided Rs 6000 for microscopes and staff training for the project. In 1918, after a survey in the Burdwan district and in several jails, the Sanitary Board of the government of Bengal proposed a hookworm campaign in the jute mills, Asansol mining district and the Duars tea plantations.

The Bengal government assigned a deputy sanitary officer to conduct a survey in the Duars plantations. As we have seen previously, Griffin conducted the operation and, as his predecessors Christophers and Bentley, also showed that the sanitary facilities, provisions for piped drinking water and generally bad working conditions all contributed to rampant disease; not only hookworm, but also dysentery, malaria, phthisis, ulcers, malnutrition and low wages within the working population within the plantations. Unsurprisingly, the DPA objected that Griffin’s criticisms were ‘a tissue of inaccuracies’. Although British physicians employed in some plantations in the Duars medicated infected workers with thymol to cure anchylostomiasis, there was no large-scale anti-hookworm public health campaign within the plantation enclaves.

In 1920, the newly established CSTM focused on hookworm, kala-azar, leprosy and diabetes. The areas they chose for research surveys were the largely British-owned jute, tea and mining industries. The CSTM identified three diseases: kala-azar in the Assam tea plantations, anchylostomiasis or hookworm among jute mill workers and epidemic respiratory diseases among the coal

115 Farley, To Cast Out Disease, pp. 2–41.
mine workers. The CSTM planned to conduct surveys of other diseases such as bacillary dysentery, blackwater fever and filariasis at a later stage in the same areas.\(^{118}\)

The hookworm surveys by the CSTM extended to the tea plantations in the Darjeeling foothills as well. In 1927, Dr Maplestone from the CSTM visited the Duars and requested the DPA to allow him to conduct a small survey to gauge the extent of ancylostomiasis among the workers in the Duars tea plantations. His objective was to understand ‘local conditions’ that precipitated hookworm disease. And after the failure of the RF’s mass public health campaigns, he wanted to see if the treatment of, and sanitary provisions for, heavily infected workers would contain the disease in a controlled area.\(^{119}\) He concluded his survey and treatments in a few selected plantations in 1928–29.\(^{120}\) He found the best treatment for hookworm was carbon tetrachloroethylene instead of the more commonly used carbon tetrachloride, which was far more toxic. Later, he communicated his findings to the planters but unlike Clayton Lane and Griffin, he did not insist on large-scale sanitary provisions within the plantation enclaves. This was probably because the CSTM did not view its role as prescriptive at all, and was content to carry out its research for its own sake.

Hookworm infestation continued to remain prevalent in epidemic proportions among the workers in Darjeeling, Duars and the Terai. The Royal Commission on Labour in India reported in 1931 that there was ‘evidence to show that a large proportion of tea garden labourers are infected with hookworm’.\(^{121}\) The Commission recommended mass treatment of workers and provisions for sanitation for the workers:

> In most plantation areas … latrines are uncommon, and although it may be impracticable to have these dotted over the plantation for working gangs, it should be possible to provide a sufficient number near the house lines and in the vicinity of the tea factory. In this connection we deprecate the wholesale exemption of the Assam and Bengal tea factories from compliance with section 13 of the Factories Act on the grounds that such factories are seasonal and built on open spaces where the workers have free access to the jungle.\(^{122}\)

The government ignored the Commission’s recommendation to legislate for comprehensive sanitary facilities for the labourers within the plantation enclaves. The planters themselves did not provide for sanitation facilities, regardless of the long-term economic benefits such provisions might have afforded the tea industry. In 1946, in the first survey of living conditions of labourers in the tea plantations after the Royal Commission report, D.V. Rege noted that ‘dysentery, hill-diarrhoea, and hookworm are common among the workers on the

\(^{118}\) Hon. Secretary of CSTM to Secretary ITA, 7 Sep. 1920, *IPTAAR, 1920* (Calcutta, 1921, APAC), pp. 313–19.

\(^{119}\) Chairman’s address, *DPAAR, 1927* (Jalpaiguri, 1928, APAC), p. xx.

\(^{120}\) Chairman’s address, *DPAAR, 1929* (Jalpaiguri, 1930, APAC), p. viii.

\(^{121}\) *RCLI*, p. 409.

\(^{122}\) *RCLI*, p. 410.
As demonstrated previously in the instance of anti-malaria sanitation, the publications of the medical reports were an end in themselves, and did not lead to action.

Tropical Medicine and the Logic of Location

So far we have seen that the Darjeeling foothills and the plains of the Duars were the subject of studies in Tropical Medicine in London and then in colonial Calcutta. The notion of the causation of malarial fever in industrial locations all over colonial India came to rest largely in the factor of non-immune immigration and the tropical aggregation of labour, confirming Schaffer’s findings in Sumatra. When the Bengal government began the malaria control programme at Meenglas, the medical experts borrowed from the knowledge of anophelines and preventive work done by Ronald Ross (the anopheline count-per-head of the population factor in infectivity) and Malcolm Watson (subsoil drainage) and sought to retest their thesis in that locality. The tracts of Duars and the Darjeeling Terai were at once connected to the entire tropical world and to the world of metropolitan and colonial Tropical Medicine.

In this respect, the Darjeeling foothills were not unique. Colonial realities informed, complicated and challenged the inadequacies of current medical theories in the metropolis. In a recent publication, Helen Tilley, through an analysis of the medical, scientific, ecological and anthropological debates on the African Research Survey (1929–39), argued that Africa was a ‘living laboratory’ for scientists in the interwar years. She has argued that unlike the ‘controlled’ laboratory, the complexity and heterogeneity of African conditions informed medical theories in Britain which challenged ‘reductionist’ biomedicine in Britain as well as ‘vertical’ theories of disease control so far as species sanitation in malaria was concerned.124

The central problem of the ‘living laboratory’ is, to my mind, neither its multifarious nature nor its ability to confound medical theories conceived in diverse conditions. It is rather in the content of its location in the political economy, demonstrated most clearly in the implementation of medical theories, however complex and modified, in the said colonies. In the case of the African Research Survey, Tilley has not analysed the medical practices in rural Africa (as opposed to medical theories of species sanitation in the region), nor has she questioned its use (or experimentation), however limited, in urban spaces or areas of white settlement.

The tea plantations in the Darjeeling foothills were, as we have seen, at the forefront of the latest research on malaria in the early twentieth century. Simultaneously, the prevention of malaria was articulated in the rhetoric of the

123 Rege, Labour Investigation Committee, p. 91.
uniqueness of the local. Contemporary research on such conditions as the hospitability of the different terrain to particular subspecies of the vector anophelines merged seamlessly with concerns over the peculiarities of labouring populations within the tea plantations and outside them, at the bastis – all framed in a set of conditions termed the ‘local’. The objection raised by the workers to the use of quinine as prophylaxis was also an issue of their own particular customs and ways of life. The planters argued that quinine prophylaxis could not be administered to the workers for such ‘primitive’ peoples could not be forcibly brought under a prophylactic regime.

A similar trajectory is evident in the research and control programmes of another typically ‘tropical’ disease: anchylostomiasis. Identified at once with poor, ignorant communities in the USA and Latin America, it fitted the profile of a typical tropical disease. Unlike malaria, the diagnosis, cure and long-term prevention of hookworm disease seemed relatively simple and was uncontested by medical authorities of the time.

Nevertheless, the labouring population of the tea plantations provided the ideal space for anti-hookworm programmes in Darjeeling and the Duars. This ‘research’, or more accurately the eradication programmes, was conducted in the context of international cooperation as well as government and entrepreneurial sponsorship for Tropical Medicine. For instance, R.G. Griffin of CSTM experimented with the alternatives to thymol with chenopodium oil as well as with other ‘anti-antihelmintics … for expelling hookworms’, including an emulsion of liquid paraffin and milk on plantation labourers in the Duars. The ‘experiments’ did not lead to systematic preventive measures within the plantations, and the disease survived in epidemic proportions among the working populations. The ‘surveys’ and ‘experiments’ by the IRFA as well as the CSTM provided evidence that the eradication and long-term prevention of hookworm disease would greatly increase the productivity of the labourers.

Despite the new scientific knowledge and vocabulary, Tropical Medicine in India also persistently sustained the discourse of ‘local conditions’ and their unique link with parasitical as well as helminthic diseases. The discourse of the local was echoed in official discourse, according to which the ‘local conditions’ were manifest, it claimed, in two different ways. So far as the workers were concerned, the planters generally claimed that the administration of prophylactic doses of quinine to prevent malaria or the installation of latrines to prevent hookworm disease was generally futile because the ‘primitive’ labourers were resistant to them. The planters insisted also that the local government should not interfere in the management of disease within the plantations. Instead, the close knowledge of the labourers and their customs that the management possessed enabled them to decide best what needed be done for the health of their labourers. The majestic announcement of the planters’ spokesman, W.L. Travers, after the visit of the Royal Commission on Labour to the tea estates, is representative of managerial claims on behalf of the labourers and formed the

justification for a non-interventionist approach to public health infrastructure within the plantations:

Our labouring population, or the great majority of it is drawn from the races of Chota Nagpur. These races have their own religion, languages, and racial customs to which they naturally cling most persistently. Many of their racial and religious customs tend to impede the work of health improvement and welfare, and therefore it is of great importance that all measures for their uplift in any direction should be under the control and direction of persons who really know and understand the customs, traditions and habits of these aboriginal people.126

On the other hand, the idea of ‘species sanitation’ in Tropical Medicine contributed to the discourse of the local condition in the prevention of malaria everywhere in the tropics. And even in the case of hookworm disease, researchers in the CSTM found it useful to study ‘local’ conditions of the disease.127

The dual imperatives of the local and the international sustained the growth of knowledge in Tropical Medicine, and the tea plantations in the Darjeeling foothills contributed to leading research on Tropical Medicine. They were the sites of many ‘unique’ conditions, conditions for testing scientific theories. As noted above, the Chairman of the DPA promised similar unique local conditions when asking the government to fund malaria research in the Duars. Partly this appeal was rhetorical. The planters’ associations generally sought to shift pecuniary responsibility for any research or sanitary works to the government in its role as the zamindar (landlord) of the district, while retaining the claim to absolute authority over their workers and their plantation. Partly, too, the distinctiveness of the region and its local conditions contributed in various aspects to the knowledge of Tropical Medicine.

At the same time, the political economy of the plantations contributed to a complex set of factors that inhibited both anti-malarial sanitation as well as systematic and full use of quinine prophylaxis within the tea plantations. Anti-malarial operations were not undertaken on a sustained basis, with both the local and provincial governments claiming with scientific authority that the management had to be responsible for the elimination of mosquito breeding in its lines. The management on its part shifted the responsibility from the plantations to the bastis, thereby rhetorically situating the plantations themselves within a sanitary enclave. The responsibility for the prevention and control of malaria, however, was accepted neither by the planters’ associations nor by the provincial or local governments. Some British medical men who were employed by the plantations made isolated attempts to destroy anopheline breeding within a decade of the discovery of the mosquito-vector transmission by Ronald Ross.128

126 DPAAR, 1929 (APAC), p. x.
Managers in several plantations also made similar efforts in a few scattered instances.²⁹² Similarly, while some plantations invested in a few latrines for their labourers and provided for thymol to their resident doctor babus to cure hookworm disease, these measures remained inadequate to prevent epidemic hookworm disease. The logistics and structure of the plantation economy did not have to accommodate any enduring system for long-term preventive health. In colonial India research in Tropical Medicine did not translate from the ‘field’ to the structures of public health.