“The Malaria Imbroglio”: Ethics, Eradication, and Endings in Pare Taveta, East Africa, 1959–1960*

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Introduction

For five years in the late 1950s, the World Health Organization in partnership with UNICEF and the Institute of Malaria and Vector-Borne Diseases ran a large malaria elimination experiment in the British East African colonies of Kenya and Tanganyika, called “the most comprehensive malaria survey mounted in East Africa.”1 The main intervention was providing indoor residual spraying (IRS) to roughly 15,000 houses a year with the insecticide dieldrin, which was meant to reduce mosquito populations and malaria levels. Pare Taveta was one of a few small pilot programs to test the feasibility of malaria elimination in Africa, and was part of the WHO’s Global Malaria Eradication Program (GMEP) which attempted to eradicate the disease globally.2 In 1959, the results from Pare Taveta were in: after four years of spraying, the project had “almost completely eradicated” malaria.3 The IRS had brought malaria to very low levels of transmission, but it would be impossible to eliminate the disease using only spraying. And while the project was officially over in 1959, there were stark realities that had to be reckoned with, most

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1 “Five Year Experiment Shows the Way to Defeat of Malaria,” East African Standard (Nairobi, Kenya) July 10, 1959, Box 24, National Institute of Medical Research, Amani (hereafter “NIMR, Amani”).


3 My emphasis. “Five Year Experiment Shows the Way to Defeat of Malaria,” East African Standard, July 10, 1959, Box 24, NIMR, Amani. The organizations involved in this project used the terms “elimination” and “eradication” interchangeably. I will use the terms as they are understood today: “elimination” to completely remove a disease from an area, “eradication” to remove from the entire world. Thus smallpox has been eradicated (globally) while malaria has been eliminated from particular places like North America and Italy.

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notably the risk of “rebound” or “resurgent” malaria. The temporary reduction in malaria transmission had modified local disease ecology in dangerous ways: Pare and Taveta residents had lost some of their acquired immunity, which normally provided a degree of protection against the disease. When malaria returned to the area, it could return in a more deadly, epidemic, form. Rebound malaria was known to the researchers, and was more than just a hypothetical risk; nonetheless, no plans were in place to help protect community members as malaria returned. Predictably, as the indoor residual spraying stopped and mosquitoes returned, the number of malaria infections slowly and steadily rose. Luckily, in both Pare and Taveta, malaria did not return in the deadly rebound form that scientists had feared, although no one could precisely say why. And had rebound malaria emerged, there were not adequate financial or drug distribution systems in place to respond to an epidemic.

By July 1960, the East African High Commission, the Institute of Malaria and Vector-Borne Diseases, the Tanganyikan and Kenyan governments, and regional research oversight committees all agreed that they faced an uncomfortable situation. Elimination was not possible, malaria had returned, Pare and Taveta community members were at risk, and the local governing councils were arguing it was “morally wrong” for the researchers to end the spraying and force them to “suffer the consequences unaided.” Opinions about what to do ranged widely. On one end of the spectrum were those who argued that the researchers could pack up and leave (“retire gracefully over the horizon”) since the experiment was over. On the other end were those who believed researchers had created the problem of resurgent malaria among a non-immune population and thus owned it, meaning they needed to take “responsibility for the situation.” The five groups involved in the project became entangled in a “malaria imbroglio” that forced colonial scientists and administrators to face a set of thorny scientific, pragmatic, and ethical questions. How should a failed malaria elimination attempt be concluded? What should be done as malaria returned to the research area? When the experiment was formally concluded, were there ongoing obligations to African participants or the larger communities? If there was an obligation, what was it, and who was responsible? Were researchers compelled to consider the desires and demands of the African participants about how they believed they should

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4 Some articles make a distinction between “resurgent” malaria where case rates return to the level of pre-intervention levels versus “rebound” malaria where the number of cases may temporarily rise higher than pre-intervention levels. I am using these terms interchangeably since in either case, acquired immunity has been lost and these newly returned cases of malaria are more dangerous, and more likely to cause mortality.

5 Malariologists had already observed the large toll it took on other African communities when malaria control measures had ended, notably after stopping control measures in Monrovia, Liberia, in the late 1940s and again in 1957. James Webb, “The First Large-Scale Use of Synthetic Insecticide for Malaria Control in Tropical Africa: Lessons From Liberia, 1945–1962,” *Journal of the History of Medicine and Allied Sciences* 66, 3 (July 1 2001), 347–76.

6 District Commissioner Tanga to Unknown, “Pare Taveta Malaria Scheme,” September 1, 1960, Box 21, NIMR, Amani.


8 Timms to Pringle, Letter, September 6, 1960, Box 2, NIMR, Amani.
be treated as the experiment ended? And what of the disturbing fact that most Pare and Taveta people did not even know they were participating in an experiment?

The ending of the Pare Taveta Malaria Experiment raises critical questions about the important—but often ignored—process of concluding a project, and researchers’ responsibilities upon leaving. These questions of endings and obligations are even more salient when discussing an elimination attempt for a disease like malaria where there are great risks if the bid fails. This paper provides a starting point to explore the obligations of researchers as a malaria control experiment ends. Critical to the discussion of ethical endings, the East Africans who participated in this experiment believed they were the beneficiaries of a public health intervention. There is no evidence that the Pare and Taveta people knew they were subjects in a large-scale experiment that included the risk of resurgent malaria or could reduce their own acquired immunity. These different understandings of what the activities were—medical experiment or public health intervention—help explain why communities considered it to be “morally wrong” for the researchers to stop the spraying.

Research is meant to be a systematic investigation to create new knowledge, and it is well accepted that with the pursuit of this knowledge will come some degree of risk. As the original creator of the Pare Taveta project, Donald Bagster Wilson wrote: the purpose of a scientific experiment was “to ascertain what effect will follow from such and such an action.” He did not go on to discuss an experiment’s need to benefit participants or deliver a public health good. Sinclair-Loutit, a WHO medical advisor, stated that when it came to designing experiments, it was not the concerns of the local community or the individual participants that took precedence, but the concerns of science. “Putting it brutally, we do not reckon the cost of a laboratory per guinea pig, but to the gravity of the problem under investigation.” And when referencing the work of the Pare Taveta experiment, he was unmistakable: “we are not undertaking this project as a service to the people of the Pare Mountain area.”

The hypothetically distinct line between the needs of science and the need to deliver a public health good is often blurry in reality. In the case of experiments to control or eliminate malaria, the distinction between research and public health is over-stated; an experiment that eliminates malaria instantly benefits the public’s health. On the other hand, a failed attempt creates a new disease environment that puts the entire public’s health at risk. And as an entire community’s health is either improved or threatened, researchers are forced to engage with questions of public health. As the Malaria Institute’s second director,

9 In the case of malaria, after a nearly successful elimination attempt, local people—and especially infants and small children—had lost or never gained their acquired immunity, putting them at greater risk of death should a malaria epidemic begin. Gordon Cook and Alimuddin Zumla, eds., Manson’s Tropical Diseases (1898; reprint, China: Elsevier Saunders, 2008), 171–72, 447.

10 Bagster Wilson to Director of Medical Services TG, “CONFIDENTIAL.—Study of Malaria in Hyperendemic Areas,” May 1, 1951, Box 23, NIMR, Amani.

Gerry Pringle, saw it, it was possible to combine research work and benefit to participants, noting, “In this way the wishes of the Wapare and our own scientific requirements could be met.”

This paper engages with a few different sets of literature. Most obviously, it helps to fill gaps in the history of malaria eradication, especially the history of these attempts in Africa. Past works have claimed that Africa was all but ignored during the first global eradication campaign, but information from Pare Taveta shows this was not the case. More broadly, this article adds to the growing literature on colonial science that presents a more nuanced portrayal of both colonial scientists and African participants. Some of the earliest pieces that focused on scientific institutions in East Africa tended to merely list achievements without any attention paid to the discord between rhetoric and action. Any analysis of the Pare Taveta Malaria Experiment can’t help but focus on the malarialogist Donald Bagster Wilson, who was the initiator of Pare Taveta, the founding director of the Malaria Institute, and an important figure in East African medical history. The private and confidential correspondences referenced here provide a more complicated assessment of Bagster Wilson, bringing into stark relief the differences that often exist between public pronouncement and private action.

A focus on a malaria experiment in East Africa allows the history of medicine to be placed in more direct dialogue with African history by focusing on how biomedicine engaged with, and was understood by, African participant communities. Recent literature in Africanist medical anthropology has done much to document how African expectations, preferences, norms, and visions of their own “moral worlds” have shaped biomedicine.

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12 I will often refer to the Institute of Malaria and Vector-Borne Diseases as the “Malaria Institute,” following the organization’s own habit. Pringle to Timms, July 8, 1960, Box 2, NIMR, Amani.

13 Dobson et al., “Malaria Control in East Africa”; Packard, The Making of a Tropical Disease; Stepan, Eradication; Webb, Humanity’s Burden.


Recent works have been careful to document the diversity of opinions existing within “colonial science” and to consider the roles of colonial subject/participants. These works have indicated that African communities interacting with biomedicine have their own ideas about what fair relationships should look like, carefully choose which ideas to adopt or ignore, and are often cognizant of the global inequities and geopolitics that shape the interventions they are offered. Although the sources capturing the views of Pare and Taveta participants are limited, we can derive new insights about how East Africans understood the malaria experiment (as anything but an experiment) and their expectations about long-term obligations (that researchers had a clear duty to community members). This article also presents new information related to the history of medical ethics and medical research in Africa—a topic that has only been researched by a handful of scholars. (Although the broader topic of medical research globally is increasingly drawing attention.) Past work has indicated that it was common for colonial medical research to be characterized by incomplete or absent consent practices, for risks to be only partially and poorly explained, for offered benefits not to be perceived as “fair” from the point of view of participants, and for there to be very little formal involvement of African participants—all conclusions reaffirmed by evidence from the Pare Taveta experiment.

Archival research was carried out in Amani, Tanzania at the former site of the Institute of Malaria and Vector-Borne Diseases, using materials found in situ. Amani has


been a site of agricultural, ecological, and medical research since 1900, and is now controlled by the Tanzanian National Institute of Medical Research.\textsuperscript{19} A large set of materials pertaining to the Pare Taveta Malaria Experiment was left unorganized in the main library. The documents include research notes, maps, and many private correspondences between the scientists involved in the experiment. Some of these materials may exist in duplicate in archives in Dar es Salaam or London, but many can only be found in Amani. They thus present a genuinely new set of materials to help us understand the Pare Taveta experiment specifically, and a failed malaria elimination attempt, generally.\textsuperscript{20}

The rest of the paper will proceed with some basic information about malaria and malaria elimination programs, followed by a description of the Pare Taveta Malaria Experiment from start to finish. The following sections will detail how each of the five groups involved responded in 1959–1960 when decisions had to be made about how the project would end. I discuss each group in turn, beginning with the East African High Commission, moving on to the Tanganyikan and Kenyan colonial officials, the Malaria Institute itself, the regional advisory groups, and then finally exploring the opinions of the Pare and Taveta communities. The conclusion will discuss the similarities between past and present malaria elimination campaigns.

\section*{The Basics: Malaria and Eradication}

Malaria is a parasitic vector-borne disease transmitted by anopheles mosquitoes; it is also a deeply ecological disease. Malaria transmission patterns are highly dependent on temperature, rainfall patterns, soil types, nearby flora and fauna, and local prevalence of human infections; malaria epidemiology responds to even slight modifications of the environment. In sub-Saharan Africa particularly, malaria varies tremendously from one locale to the next and seemingly small differences related to mosquito biting and resting behavior can have important ramifications when determining an appropriate control strategy. One of the reasons malaria is so difficult to eliminate on a large scale is because control and eradication strategies have to be equally localized. Thus, an elimination campaign relying on indoor residual spraying will only be effective if the mosquito feeds primarily on humans and then rests indoors. (This is because indoor residual spraying poisons the mosquitoes as they rest on the treated walls. The mosquitoes don’t die instantly, but perish before the malaria parasite is able to fully develop, and long before another person is infected.)

There are four species of malaria infections, but only one—\textit{Plasmodium falciparum}—is responsible for a majority of the malaria morbidity and mortality in East

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\begin{itemize}
\item \textsuperscript{19} Christopher A. Conte, \textit{Highland Sanctuary: Environmental History in Tanzania’s Usambara Mountains} (Athens: Ohio University Press, 2004), 58–67.
\item \textsuperscript{20} While in Amani I compiled a rough catalog to the documents, which can be downloaded as a PDF here:  \url{http://pages.uoregon.edu/graboyes/Melissa_Graboyes_Website/Writing_files/Amani%20Finding%20Guide.pdf}.
\end{itemize}
Falciparum malaria is often referred to as “cerebral” or “deadly” malaria because of how frequently it results in cerebral infection, severe disease, and death. Those deaths most typically happen to children under the age of five who have had little exposure to the disease. But with high mortality comes a valuable biological adaptation: acquired immunity. Acquired immunity is a complex phenomenon that is still not fully understood. What is known is that a child born in a region with high levels of malaria is challenged with multiple malaria infections in the first few years of life. Tragically, many of these infections will result in death. However, if the infant is able to survive to the age of five, a degree of immunity is conveyed, and malaria shifts from being a disease of mortality (death) to one of morbidity (sickness). If an adult remains in a highly endemic region for most of her life, the acquired immunity remains, and death from malaria is unlikely. But when adults who have acquired immunity are not regularly exposed to malaria (because of moving away or temporary malaria control measures), their immunity fades and future infections are likely to be more severe and potentially fatal.

Acquired immunity, and the risk inherent in losing that immunity, has been recognized for over a century. It was described in nineteenth-century German research and a League of Nations report from 1933 stated it would have been “extremely imprudent to brutally interrupt the processes that caused immunity.” As Dobson, Malowany, and Snow recounted, participants at the 1950 Conference on Malaria in Equatorial Africa, held in Kampala, argued over the risks of starting malaria control measures and then stopping them, and the damage that could be done to Africans’ acquired immunity. Bagster Wilson was a vocal participant in these debates and was one of the scientists labeled as “anti-interventionists” and “conservatives” because of their concerns about the loss of acquired immunity. What the scientists did agree on was that very little was known about effective malaria control techniques or the true health effects of malaria, and that additional research was needed. (There was also a vocal contingent of scientists arguing that malaria elimination in Africa would be especially challenging—if not impossible—for a whole

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21 The other forms of malaria are *P. vivax, P. ovale*, and *P. malariae*.

22 Sickle cell is another biological adaptation to malaria that provides partial protection against vivax malaria. Cook and Zumla, *Manson’s Tropical Diseases*, 189.

23 Ibid., 22, 1205. When this phenomenon is graphed, you see a steep drop off of child deaths at five years of age and again at twelve. This general premise of low adult mortality rates has been questioned by Murray et al. (2012). They argue that in 2010 the WHO underestimated adult malaria deaths by more than 400,000. Their findings, if validated, imply that our understanding of acquired immunity and the morbidity/mortality toll on adults may still need to be revised. Christopher J.L. Murray et al., “Global Malaria Mortality between 1980 and 2010: A Systematic Analysis,” *Lancet* 379, 9814 (February 4, 2012), 413–31.


25 Dobson et al., “Malaria Control in East Africa,” 156.
host of ecological reasons.) Early justification for Pare Taveta centered on the lack of prior research about malaria elimination in Africa. As Bagster Wilson wrote in 1951:

> The present state of knowledge does not allow us to express an opinion whether such activities are wholly or partially beneficial, or even harmful, to the community concerned. It follows that we are therefore quite unable to assess the importance of the malaria problem in relation to other diseases … We are in fact building on a shifting sand of ignorance.…

In 1951, Wilson was right to emphasize all that was unknown in regard to effective malaria control. Yet in the following decade much information came to light that could have created a different ending for the Pare Taveta Malaria Experiment. In the mid-1950s to early 1960s (the same time span Pare Taveta was spraying and debating the merits of a continuation scheme), more than twenty different WHO pilot projects attempted to eliminate malaria in Africa. As these experiments began and were concluded, and the results shared both informally and through formal publications, new knowledge was created and dispersed about insecticide resistance, rebound malaria, and the general likelihood of success for malaria elimination in Africa. With this new knowledge, the researchers could have more adequately planned how to handle resistance, rebound malaria, and potential failure in their own experiment.

Much of the design of the Pare Taveta Malaria Experiment should be attributed to Bagster Wilson, a polarizing figure in East Africa medical circles and something of a regional malaria kingpin. He was the dominant figure shaping Pare Taveta, and he began planning for the experiment as far back as 1946—often complaining at the slow pace of work by reminding colleagues that he “shall not be able to direct this investigation from Heaven, or the other place.” He was a complicated man and a review of his private correspondences presents some challenges to prior assessments of his work, which are based on published papers and public statements. In private correspondences, he emerges

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26 Presently, we know that many of the vectors in sub-Saharan Africa have extremely high vectorial capacity (i.e., are extremely efficient vectors), that *A. gambiae* have a strong preference for biting humans, and that year round malaria and a high percentage of *falciparum* infections in sub-Saharan Africa distinguish it from other malaria regions.

27 Bagster Wilson to Director of Medical Services Tanganyika, “Study of Malaria in Hyperendemic Areas CONFIDENTIAL,” May 1, 1951, Box 23, NIMR, Amani.

28 In South Cameroon, malaria transmission was interrupted and it was “virtually” halted in Uganda. Other WHO pilot projects occurred in Nigeria (Garki); Kenya (Kisumu); South Cameroon, southwestern Uganda, Liberia, Upper Volta, Senegal, and Ghana. L. Molineaux and G. Gramiccia, *The Garki Project: Research on the Epidemiology and Control of Malaria in the Sudan Savanna of West Africa* (Geneva: World Health Organization, 1980); Webb, “Synthetic Insecticides,” 28.


30 Dobson, Malowany and Snow present a largely sympathetic portrayal of Bagster Wilson, and make much of his public statements about the need to preserve Africans’ immunity, and the great risks posed by
as an eager researcher, reluctant to modify projects even when public health was at risk, and hostile to those who questioned his techniques or dared to present undesirable research findings.31 There are also deep incongruities between his public statements and the projects he designed and led. In 1936 he wrote that “no measures [should be] taken … to prevent” the acquiring of immunity, which he considered “so valuable an asset in later life;” but the indoor residual spraying in Pare Taveta reduced malaria transmission levels and led to a waning of acquired immunity.32 In the same vein, he cautioned in 1950 that “a major control scheme in a hyperendemic area of Africa might be followed by malaria of a different character, and of much more serious import, than that to which we are accustomed, if control measures slackened;” yet he undertook just such a short-term control scheme and then allowed measures to slacken, putting East Africans at risk of rebound malaria.33 If what Dobson, Malowany, and Snow write is true, that one of his “greatest fears” was to begin a project in an area of intense transmission “which could not be sustained” and which could lead to “an epidemic following the cessation of controls” why was the end of Pare Taveta so poorly theorized and designed? Why did Bagster Wilson refuse to plan early for the possibility of an epidemic by creating a stockpile of drugs or funds? Why did he ignore the concerns of other researchers and colonial employees about risks and leave unanswered the questions of longer-term obligations?34

Shortages of space and materials do not allow me to fully answer these questions, although what follows is a preliminary and partial answer that will hopefully spur additional research. While Bagster Wilson can’t be solely blamed for the poor planning that characterized the end of Pare Taveta, he was deeply responsible.

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31 After an early review of Pare Taveta’s design, Bagster Wilson responded to the report by writing: “I have no many criticisms to make of your criticisms, but I would like to mention those there are …” and went on to detail how wrong the assessments were. Bagster Wilson to Dr. H. Cullumbine, December 30, 1954, Box 23, NIMR, Amani. For hostility toward undesirable research findings: in 1953, Shelly Avery Jones presented data indicating drug resistant malaria in Kenya (which was later proved accurate), and asked for the experiment to be modified to better protect public health. Bagster Wilson argued that “the experiment has not been developed fully … and it seems to us that it would be most undesirable for the original design to be abandoned at this stage simply because some undesirable public health trends appear to be resulting in this small town.” Bagster Wilson to Avery Jones, cc: Director of Medical Services, Nairobi; Senior Parasitologist Nairobi; Professor Garnham, April 24, 1953, Box 23, NIMR, Amani.


34 One potential answer—which cannot be definitively answered with the materials currently available—is that Bagster Wilson wanted Pare Taveta to end without any long-term control measures because it could more clearly demonstrate the health effects of malaria, and the risks of rebound malaria among a population who had lost acquired immunity.
The Pare Taveta Malaria Trial: “A Basic and Classical Experiment”

The Pare Taveta experiment was the largest malaria survey conducted in East Africa through the 1950s. The survey’s activities covered 2,000 square miles, 53,000 people in treated areas, 79,000 people in the mountains above, and 9,000 people as part of a shifting labor force. During the first phase of the project, researchers collected 6,000 blood samples a year; vital statistics from 10,000 people; and made medical examinations of 2,500 adults and 2,000 babies. There were also 15,000 houses sprayed each year with the insecticide dieldrin—which was the experimental intervention. The region where the project was taking place was “an area of lush plain and tropical forest” in the Pare district of Tanganyika and the Taveta district of Kenya, in the South Pare Mountains. The area covers several thousand square miles of inland plains where malaria was endemic at a high level, transmission occurred year round at moderate to high levels, and epidemics were uncommon. There was not one, but two different species of anopheles mosquitoes responsible for transmitting malaria.

As the designer of the experiment, Bagster Wilson wrote in 1951 that the goal was to study the impact of malaria, “on the life and activity of people born and brought up in the most intensely affected areas … to define the importance, both physical and economic, of malaria under these conditions.” In reality, the “goals” of Pare Taveta were surprisingly malleable. In other settings, the aim of the project was described as: “to find an economical means of arresting malaria transmission”; “to reduce malaria and improve health”; “to see if malaria transmission could be arrested by insecticides”; and to determine “the importance of malaria to the ‘immune’ African community.” In addition to enumerating the physical and economic costs of malaria, they would also test the feasibility and effect of malaria elimination. A 1959 newspaper article in the East African Standard provided a slightly different slant, stating that the purpose had been “to

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35 Dobson et al., “Malaria Control in East Africa,” 161.
36 “Five Year Experiment Shows the Way to Defeat of Malaria,” East African Standard, July 10, 1959, Box 24, NIMR, Amani.
37 The two vectors are A. gambiae and funestus. A. funestus feeds almost entirely on humans, frequently coming indoors to bite and rest. A. gambiae feeds on both humans and cattle and prefers to rest outdoors rather than on the walls of a house.
40 Bagster Wilson to Director of Medical Services Tanganyika, “Study of Malaria in Hyperendemic Areas CONFIDENTIAL,” May 1, 1951, Box 23, NIMR, Amani.
reduce malaria and improve the health of 120,000 people over an area of 100,000 square miles.\footnote{51}

Although it was not originally planned this way, there were ultimately three distinct parts to the Pare Taveta Malaria Scheme. Phase 1 (1955–1959) involved spraying thousands of houses with insecticide four times over a five-year period as a way to reduce mosquito populations and malaria transmission rates. Phase 2 was a short term “continuation scheme” which distributed malaria treatment to Pare and Taveta residents who presented with fever in an attempt to ward off resurgent malaria. This distribution of treatment was not mass drug administration (MDA) given to the entire public; the medicine was only offered to residents presenting with fever. It’s questionable whether this is an effective strategy to prevent epidemic malaria, or if it was responsible for preventing an epidemic in Pare and Taveta. The WHO roundly criticized this practice when used in Liberia.\footnote{52} It was during Phase 2 that resurgent malaria would have been most likely to return to Pare and Taveta. Scientists and observers were unable to say precisely why rebound malaria did not occur, as there were plenty of documented cases as malaria control measures lapsed on other parts of the continent. Phase 3, the Vital Statistics Survey (VSS), ran from 1961–1966. The project was overseen by Gerry Pringle, the new Director of the Malaria Institute, and collected thousands of physical assessments and blood examinations, in addition to continuing entomological studies.\footnote{53} The goal was to collect data on age-specific mortality and fertility rates in two different communities where malaria had returned. That meant recording all of the pregnancies, births, and deaths in at least two thousand families in South Pare, and another thousand in the Taveta area.\footnote{54} No free malaria treatment or widespread malaria control activities of any type (spraying, mass drug administration, environmental control) were part of Phase 3, which is best considered as disease surveillance. Information on all three phases of the experiment are shown in Table 1.

\footnote{51} “Five Year Experiment,” \textit{East African Standard}.

\footnote{52} In the case of resurgent malaria in Liberia, the WHO noted, “ill-planned and unmethodical drugging may have some propagandistic value and may be prompted by humanitarian consideration, but their [sic] value as an effective means of malaria control is more than doubtful….” Webb, “Synthetic Insecticides,” 20. Quoting “Report on the Joint UNICEF/WHO Malaria Project, Kpain (Liberia),” 2, SJ 2, JKT III, Liberia, WHO 7.0022, WHO Archives.

\footnote{53} When the Vital Statistics Survey ended is a bit unclear. While Dobson et al., list the date as 1966, documents from Amani indicate that Thomas Fletcher was still in the Taveta region collecting “vital data” in 1968. This may have been data related to his work on sickle cell, but it may also have been a reference to the conclusion of the VSS. Dobson et al., “Malaria Control in East Africa”; “Taveta Pare Survey, Dr. Trant,” Unknown Date, Box 2, NIMR, Amani.

\footnote{54} Pringle to District Commissioner Same, February 3, 1961, Box 2, NIMR, Amani.
Table 1. Pare Taveta Malaria Experiment, 1954–1966

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<th>Phase 1</th>
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<td><strong>Other names</strong></td>
<td>Malaria Scheme</td>
<td>Continuation Phase</td>
<td>Vital Statistics Survey (VSS)</td>
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| **Research Questions/Goals** | - could malaria transmission be broken in a holoendemic area?  
- could malaria-transmitting mosquitoes be eliminated with yearly IRS with dieldrin? | - prevent a resumption of malaria transmission  
- “provide for the eventual return of malaria” and to “soften the impact of a wave of fresh infections”  
- encourage cessation of IRS so the VSS could begin | - health effects when malaria returns to a place where nearly eliminated |
| **Intervention** | IRS with dieldrin of approximately 15,000 homes 4 times over 5 years | - distribution of anti-malarial drugs (chloroquine or neviquine) to residents presenting with fevers  
- house to house visits to treat all fever cases and acquire blood slides for testing | - registration of 16,500+ people and collection of births, deaths, etc  
- interviews with 1500+ families  
- blood slides, physical examinations |
| **Director**   | Donald Bagster Wilson    | Donald Bagster Wilson     | Gerry Pringle                     |
|                |                          | Gerry Pringle (April 1960-) | Thomas Fletcher, Acting Director, (July 1966-) |
| **Researchers Involved** | Christopher Draper, malariologist  
Alec Smith, entomologist  
MT Gillies, entomologist | William Marijani (MPare)  
Adulrahman Salim Msangi, assistant scientific officer (MPare) | Ndoloi Macharia, fieldworker (Tanzanian)  
Yohana Matola, fieldworker (Tanzanian; eventually became director of the institute)  
William Marijani, Officer in Charge, Pare Station |
| **Related Projects** | - nutrition study (Hope Trant)  
- serum proteins of children, sickle cell (Thomas Fletcher)  
dieldrin exposure of sprayers (Thomas Fletcher) | | - haemoglobin in children and sickle cell (Thomas Fletcher) |
| **Funding**    | WHO, UNICEF, EA governments, Malaria Institute, Tropical Pesticides Research Institute | Malaria Institute | Nuffield Foundation, Malaria Institute, East African Common Services Organization |
The Vital Statistics Survey did not appear to be a formal or anticipated part of Bagster Wilson’s original plan; rather, it emerged from a combination of compromise and serendipity. It was a compromise in that post-1960 discussions, detailed in this article, show the Malaria Institute acknowledged they had some obligation to the area, but they did not want to pay for ongoing residual spraying. It was serendipitous in that they received grant funding from the Nuffield Foundation for additional years of work collecting vital statistics in the region after Bagster Wilson submitted an application.45 Sadly, acceptance of the grant was dependent upon local communities choosing to discontinue indoor residual spraying. The grant allowed the Malaria Institute to remain in the area and collect data on a question that could only be researched in those very conditions: what happens when an elimination attempt fails, malaria returns, and all control strategies are stopped?46

The results of the first phase of Pare Taveta were equivocal. One aspect that was indisputable was that malaria was still present. *A. gambiae* vectors remained, human reservoirs of the parasite remained, and malaria transmission continued at low levels. This failure to achieve elimination was due to a few different reasons. First, the spraying program effectively eliminated one vector (*A. funestus*) and reduced the levels of the second vector (*A. gambiae*) by up to 90 percent in some areas. However, the remaining *A. gambiae* had grown resistant to the insecticide and had changed their biting and resting behavior—meaning no amount of spraying would kill them.47 Second, a regular influx of malaria-infected people from neighboring regions meant the parasite was continually being reintroduced into remaining mosquitoes and formerly “clean” local populations. Third, yearly spraying with dieldrin was found to be too infrequent to be effective, and an approach that focused entirely on the vector without also providing mass drug administration (to target the parasite in the human body) was unlikely to be fully successful.

There were real public health effects to the reduction in malaria, even without complete elimination. Due to the indoor residual spraying, there was a reduction in malaria prevalence and a 50 percent reduction in infant and child mortality (200/1,000 to

45 The original grant was awarded in June 1960, and expected to provide a total of £29,000 over five years. It appears that in 1964 the Vital Stats Survey’s annual budget was £58,000 with the following contributions: £4000 Foundation; £20,000 East African Common Services Organization; £24,000 British Government Matching Grant. Hyder to Pringle, November 1964, Box 24, NIMR, Amani.

46 Anti-malarial medicine was still available to purchase privately.

And even five years after ending the indoor residual spraying, malaria had stayed at levels lower than those prior to the intervention. (There was speculation that human malaria endemicity in Pare remained steady in spite of rising malaria risk because East Africans were self-treating with anti-malarial drugs.) On the negative side, when the intervention ended, malaria returned and transmission rose. Child mortality returned to prior levels, although infant mortality remained low. There were also no apparent positive health effects by reducing malaria and individual health didn’t seem to change as malaria decreased. Later papers written about Pare Taveta found that the longer-term effects of the spraying and temporary malaria reduction were even harder to identify.

At least one important and seemingly straightforward question remains to be answered: what to call the activities that were carried out in Pare and Taveta? The scientists and many of the colonial workers involved in this work frequently referred to the activities in Pare and Taveta as an “experiment.” In some documents, the official name of the project is listed as the Pare Taveta Malaria “Scheme.” Regardless of whether the scientists referred to the spraying and data collection as a scheme, project, program, experiment, or inquiry, it was understood to be a short-term experiment with a limited lifecycle of a few years, depending on funding and initial results. Some of the vagueness about what to call the work in Pare Taveta was calculated and pragmatic (as with the malleability of the

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49 Conacher to Provincial Commissioner Tanga, “Pare Taveta Malaria Scheme,” August 10, 1960, Box 21, NIMR, Amani.


51 As just a few examples, it is referred to as an “experiment” in Thompson to Provincial Commissioner, Coast Province, Mombasa, CC Bagster Wilson, “Taveta/Pare Malaria Scheme,” May 28, 1959, NIMR, Amani; and “5th Meeting. East African Medical Research Scientific Advisory Committee,” January 11–12, 1960, NIMR, Amani.

52 It was referred to as a “scheme” in the official Progress Reports from 1954–1958 and in the Annual Report of the East African Institute of Malaria and Vector-Borne Diseases, from 1955–1960.
experiment’s stated goals). For a scientist, this was a classic experiment; for a community member, it was public health intervention; for a government medical director, it was a way to test and develop efficient medical policies. One could argue that the experiment’s ability to be many things for many people was necessary for gathering resources and securing the agreement of so many different organizations.

In 1952, the district commissioner of Pare wrote to Bagster Wilson describing how he “explained to one or two locals that they have had the good fortune to be chosen for an experiment and they are eager for it.” While the district commissioner was right to label Pare Taveta an experiment, he was presumptive to claim it was residents’ “good fortune.” And the last piece of his quote—that people were “eager” for it—is even more questionable. Throughout the years of the project, evidence shows that Pare and Taveta residents understood the primary intervention to be indoor residual spraying, the primary goal to be reduction or elimination of mosquitoes, and the primary indicator of success the reduction of mosquitoes. It’s possible that some people understood that malaria was really the focus, and that child health should also be improved. Residents developed these expectations based on what they were told by researchers, what they directly observed, and what they already understood about the nature of malaria.

The first spraying with dieldrin killed cockroaches, bed bugs, lice, and maize weevils. People appreciated the reduction in mosquitoes and other bugs because they were nuisances—buzzing and biting, keeping people awake at night, even if they weren’t transmitting malaria. Unfortunately, by the second spraying, insects were already growing resistant to the dieldrin. A letter from the chief of Gonja complained that, “These mosquitoes aren’t dying, they are bothering us even worse at night, and everyone says that if the medicine [insecticide] is working, why aren’t the mosquitoes dying?” This persistent misunderstanding of the project annoyed the researchers, who continued to restate that the aim, “was to rid the people of malaria and not necessarily of mosquito bites and that the spraying was specially designed to kill malaria-carrying mosquitoes … and was not specially designed to kill other types of mosquitoes.” Despite the researchers’ statement, public grumbling continued about the insecticide’s perceived inefficacy. While conducting “public relations work” one of the field workers noted, “Practically everywhere there were minor complaints about the present dosage [of insecticide] being ‘weak’ and not as effective as the first one. ‘Mosquitoes were biting people inside houses even on the nights following spraying.’”

The belief that the goal was to kill mosquitoes was reinforced by the fact that many local residents didn’t understand malaria in the same way biomedical researchers did. To

53 District Commissioner, Pare, to Bagster Wilson, “Medical—General—Malaria Research,” June 7, 1952, Box 23, NIMR, Amani.

54 “… hawammbu hawakufa wanazidi kutusumbua sana usiku, na kila mtu anasema kama dawa imekuwa kazi bure mbona mmbu hawafi....” Mfumwa wa Gonja to Malaria Officer Gonja Juu, “Dawa ya Mmbu (Dieldrin),” February 11, 1957, Box 24, NIMR, Amani.

55 Msangi, “Public Relations Work,” Unknown Date, Box 23, NIMR, Amani.

56 Ibid.
some extent, it didn’t matter how frequently or loudly the researchers re-stated their focus on malaria rather than mosquitoes since this was a misunderstanding rooted in different systems for explaining disease. Thus, although Pare and Taveta people may have been familiar with the idea that the bite of a mosquito could lead to malaria, they were unlikely to make a distinction between different types of mosquitoes, or to believe that mosquitoes were the only way to become infected.\(^{57}\) It’s also possible that participants considered malaria in adults a different disease than malaria in children.\(^{58}\) There was no shared understanding of malaria’s etiology or pathology, which contributed to misunderstandings about the goals of the overall project, and about how to measure the effectiveness of the interventions.

Swahili language documents also indicate the experiment’s activities were most frequently discussed as an assumed-effective intervention to reduce malaria, rather than as an experiment with very real risks if elimination failed. This impression comes through most strongly in the researchers’ and residents’ reference to the insecticide as \textit{dawa} (medicine). The problem is that \textit{dawa} is assumed to be effective, and three different Swahili documents from 1957 refer to it as \textit{dawa ya mmbu} (mosquito medicine) or \textit{dawa ya nyumba} (house medicine).\(^{59}\) In a majority of the references to the project, the researchers discuss the development (\textit{maendeleo}), blessings (\textit{baraka}) and benefits (\textit{faida}) that will be delivered to people.\(^{60}\) This was a calculated decision on the researchers’ part since there were Swahili words available to explain an experiment or a test intervention. In a propaganda piece the researchers wrote for a local newspaper, they defended the potency of the insecticide by noting it had been previously “tested and investigated” (\textit{ilipimwa na kujari}). And they did, on occasion, accurately refer to their work, once calling it an “investigation” (\textit{uchunguzi}) and in another instance describing the project as “the work of trying to reduce malaria” (\textit{kazi ya kujaribu kuondoa ugonjwa wa malaria}) and the goal (\textit{nia}) as “to greatly reduce and see if it’s possible to eliminate entirely malaria” (\textit{kupunguza sana na ikiwezekana kuondoa kabisa ugonjwa wa malaria}).\(^{61}\) Despite these few cases of the researchers using words that indicated the experimental nature of their work (\textit{kujaribu}, \textit{uchunguzi}), the norm was to imply the project was using medicines that had been tested, and that mosquitoes would be killed, and malaria reduced as a result. In none of these


\(^{58}\) Modern anthropological research in East Africa has shown that children affected by the high fevers and seizures characterizing falciparum malaria are often described as having an entirely different disease.

\(^{59}\) Mfumwa wa Gonja to Malaria Officer, Gonja Juu, “Dawa ya Mmbu (Dieldrin),” No Date. [February 1957] Box 24, NIMR, Amani. “To the Editor, Habari Za Upare, Same, for Publication,” February 11, 1957, Box 24, NIMR, Amani.

\(^{60}\) Msangi to the Editor, Habari Za Upare (Same), “Baraka za Kupungukiwa Na Malaria Katika Wilaya Za Upare na Taveta,” February 23, 1957, Box 24, NIMR, Amani.

\(^{61}\) Ibid.
documents was there any reference to potential risks, rebound or epidemic malaria, what would happen when the spraying stopped, or even that there would be a definitive “end” to the work.

There are a few documented examples of Pare and Taveta citizens recognizing that the goal was malaria reduction, even if the concept of it being experimental wasn’t entirely clear. A local chief told one of the fieldworkers that:

Many of the people, especially the enlightened ones, also seemed to have noticed a considerable decrease in the occurrences of fevers among their people and the disappearance of enlarged spleens among their children … At Mata they said that whereas before spraying they used to have one or two infants dying in the village every month, now hardly a single death of an infant occurred over a long period in the village.62

This “accurate” understanding of the experiment should be taken with a grain of salt. The information came from a local chief, who was both more knowledgeable about the project than an average citizen and had a vested interest in saying what a researcher wanted to hear. It’s also questionable how much information the researcher had prepped him with, considering that the links between enlarged spleens in children, fewer fevers, lower infant mortality, and the insecticide spraying would have been far from obvious to the average observer. (It also belies the fact that many diseases other than malaria cause fever and infant mortality.) Finally, even when the chief was able to identify the project’s ultimate goal of malaria elimination, there is no indication that he understood it was experimental in nature, would not be ongoing, or that there was any element of risk when spraying stopped and malaria returned.

“The Malaria Imbroglio:” Obligations When Ending a Project63

In many official documents related to Pare Taveta, the experiment appears to have smoothly and logically transitioned into each of its three phases; this belies the reality of a great deal of disagreement about what should happen as each stage drew to a close. Particularly vexing was what to do as the indoor residual spraying stage ended in July 1960 and the reality set in that malaria would not be wiped out. Pare and Taveta residents wanted to know what was going to be done since they observed malaria returning, and both governing councils passed resolutions demanding that the IRS be continued. But the researchers had not planned for any additional indoor residual spraying, had not planned anything to blunt the return of malaria, and UNICEF and the WHO made it obvious that as their original funding commitment ended, so did their involvement.

At the core of the other issues being debated was a central one: whether the researchers had any obligation to the communities where the experiment occurred once the official research was over. The secondary question would be determining what that obligation was, and identifying the responsible party. It was ultimately decided that there was an obligation to the region, in the form of working to prevent rebound malaria. As the

62 Msangi, “Public Relations Work,” Unknown Date, Box 23, NIMR, Amani.

63 Timms to Pringle, September 6, 1960, Box 2, NIMR, Amani.
imminent threat of a malaria epidemic passed, the Malaria Institute would modify the form of their “obligation” into one that more suited their role as a research institute: collection of fertility and mortality data under the auspices of the Vital Statistics Survey, funded by the Nuffield Foundation. The grant expressly prohibited any malaria treatment or control activities, which created a conflict of interest for the Malaria Institute.64 It was a good public health strategy for the communities to continue spraying, but it meant that the researchers would have to turn down the Nuffield grant. This meant the Malaria Institute and the East African High Commission had an incentive to dissuade communities from continuing spraying. As Gerry Pringle, the new director of the Malaria Institute, informed his field workers, when they were asked by community members to continue spraying, “such suggestions should be side tracked as gently and as diplomatically as possible” and to emphasize that no more spraying would happen “whatever the justification.”65

**East Africa High Commission: “Bury the Scheme and Retire Gracefully over the Horizon”**66

The East African High Commission (EAHC) was in charge of administering a number of regional research centers that were created in the late 1940s and early 1950s, one of which was the Institute for Malaria and Vector-Borne Disease. The Malaria Institute designed and conducted its own projects, but a large portion of its budget was made up of EAHC funds and a smaller part came from contributions by the governments of Tanganyika, Kenya, and Uganda. Although the EAHC did not advise on research protocol or design, they were involved in the institute’s financial planning. Thus, when discussions about how to end Pare Taveta turned into discussions about who was responsible for future activities, the EAHC balked. They had allocated only a limited amount of funds, and they did not want to become financially responsible for additional years. The EAHC understood that the Malaria Institute would be found responsible for cleaning up after the scheme, since it was their project that had changed the disease environment, failed to eliminate malaria, and put people at risk of an epidemic. They also knew that if the Malaria Institute was roped into providing additional spraying or malaria treatment for an indefinite amount of time, the EAHC would likely have to pay for at least part of it.

In July of 1960, Pringle accurately summarized the EAHC’s position, which was fear that the Pare people would demand a continuation of spraying once it became obvious malaria was returning. The High Commission was afraid that if the Pare Taveta Scheme was still active:

> the main demands for action would be directed at them rather than at the Administration and the Provincial Health authorities. The [EAHC] Administrator is trying, therefore, to shrug off responsibility for the situation that develops after the

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64 The Malaria Institute delayed accepting the grant until it was absolutely certain local communities would *not* continue with indoor residual spraying. The grant had been awarded by September 1960, but was not formally accepted until March 1961. Unknown to Mac, “Personal,” October 6, 1960, Box 2, NIMR, Amani.

65 Pringle to Officer in Charge, Pare, “Malaria Cases,” July 18, 1960, Box 21, NIMR, Amani.

66 Pringle to Clyde, July 27, 1960, Box 2, NIMR, Amani.
end of this year as, in his view, the Directors of the Medical Services … have sanctioned the High Commission to bury the scheme and retire gracefully over the horizon.67

In his own words, the High Commission administrator was worried that “pressure of public opinion” could force the spraying to be resumed, and that the presence of Malaria Institute workers in the area for any reason would direct “the pressure to us instead of upon those to whom it more properly belongs.”68 It was unclear to whom the EAHC believed the pressure should be “more properly” applied, but they were clear it was not them.

**Tanganyikan and Kenyan Colonial Governments: “Eliminate … Suffering to the Human Beings Who Are Involved”**

Of all the groups involved in Pare Taveta, it was the district-level colonial officials that asked the toughest, and earliest, questions about what would happen when the experiment reached its conclusion. The British men who worked as district officers in Pare and Taveta were responsible for keeping the peace, carrying out colonial directives, and assisting with special projects. These men often spoke Swahili and local dialects and worked hard to develop good relationships with the communities where they lived and worked. These colonial field workers were the most invested in having a definite and feasible plan for the end. In fact, at the very beginning of the experiment, these officials were imagining a worst case scenario that the research team refused to address: what would happen if the activities failed to eliminate malaria?

In April 1956, one year into the experiment, the district officer in Taveta, Kenya sought information about the researchers’ long-term plans in the region. He wrote directly to one of the British entomologists who lived in the research area and was involved in the day-to-day fieldwork. His main concern was that “it is absolutely essential that when your funds run out at the end of the scheme, the Taveta have sufficient confidence in your work to wish to continue with it, with either their own money or money subscribed through the Native Authority.” 69 The note implies that the researchers’ obligations were not necessarily financial, but to make sure their work was successful and well liked, and that it would be the groundwork for a new public health intervention. As the district officer understood the experiment, the goal was not necessarily to eliminate malaria, but to convince local residents of the benefits of ongoing malaria control through indoor residual spraying. This questioning about post-experiment plans eventually made it up to Bagster Wilson. He was unmoved, writing in December 1956, “It is still, in my opinion, premature to formulate a plan for the continuation, or otherwise, of control in this area after mid-1959 …”70 The question appears to have been dropped for a period of years until everyone was forced to face it in 1959, as the spraying was scheduled to end.

67 Pringle to Clyde, July 27, 1960, Box 2, NIMR, Amani.
68 Timms to Pringle, “Nuffield Foundation Grant,” July 5, 1960, Box 2, NIMR, Amani.
69 Leask to Draper, April 24, 1956, Box 23, NIMR, Amani.
70 Bagster Wilson, “The Future of Control in the Taveta/Pare Area,” December 1956, Box 22, NIMR, Amani.
In May 1959, another district officer in Taveta wrote to his supervisor to express his concerns about “the dangerous uncertainties” of the Vital Statistics Survey being planned. He accurately understood that the project lacked any active malaria control measures. He questioned the availability of funding to resume spraying if malaria returned, argued that the observational project had not been designed to minimize either risk or suffering of local people, and that a poorly-run project would have lasting, negative, effects on peoples’ perceptions of future research projects. The Taveta district officer acknowledged the importance of the planned VSS, but felt it was “equally important” that the project be run in a way that would “preclude all risk of malaria being allowed to reemerge without prompt measures being taken.” Near the top of his list of worries was that:

funds be available immediately for reintroduction of spraying if there is a resurgence of malaria here?… Dr. Bagster Wilson informed me that he will not have any funds available for “contingencies.” This means that, if there were a resurgence of malaria over large areas of the Scheme, he would have to ask the territorial governments for funds to pay for spraying to be resumed … ⁷¹

The official went on to note how difficult it was to obtain government funds that hadn’t been requested long in advance and admitted, “I am afraid lest there might be a delay in obtaining funds for the spraying.” Later in the note, he wrote how he found it “most disturbing that funds have not been provided at the outset to prevent” the reemergence of malaria. ⁷²

Although the Taveta district officer was not a doctor, he was aware of acquired immunity, and what might happen if malaria returned after a few years of control. Any delay in funds being distributed, and thus a delay in treatment being provided, “could have disastrous results on the health of the children who have grown up without developing immunity to malaria and consequently on the morale of the Wataveta and their general outlook towards Government schemes …” ⁷³ Even though money was short, he argued that, “if a scheme is to be run, it should be properly financed so as to eliminate all unnecessary risk of causing suffering to the human beings who are involved.” ⁷⁴ This district officer recognized the real health effects people would suffer if malaria returned—especially infants without any acquired immunity. He also recognized that people saw the Pare Taveta Experiment as a government enterprise, and that they would be reluctant to participate in future government activities if the project ended poorly. He concluded by noting that if the research was properly planned, it could “be carried out without risk to the health of the people here.” ⁷⁵

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⁷¹ Emphasis in original. Thompson to Provincial Commissioner Coast Province, CC Bagster Wilson, “Taveta/Pare Malaria Scheme,” May 28, 1959, Box 23, NIMR, Amani.

⁷² Ibid.

⁷³ Ibid.

⁷⁴ Ibid.

⁷⁵ Ibid.
During the heat of the debate in July and August 1960, government authorities in both Kenya and Tanganyika wrote in with specific questions about the plan for the future, and to clarify what they would not tolerate. In Tanganyika, the permanent secretary for Health wrote to the Malaria Institute to inform them of two things: that it was no secret that malaria was returning to Pare, and that it was “causing considerable concern.” He believed the Pare would press for a resumption of indoor residual spraying, and wanted to know whether it was best for local authorities to begin the spraying immediately. Although the Tanganyikan government was not happy about the possibility of paying for the spraying, they were forced to think about picking up where the researchers left off.

*Institute of Malaria and Vector-Borne Diseases: “Ameliorate the Sufferings of the Sick”*

When negotiating about how to end the Pare Taveta experiment, the Malaria Institute had a new director who ultimately made surprisingly broad admissions about the researchers’ obligations to East African participant communities. Gerry Pringle charted a very different path from his predecessor Bagster Wilson. Shortly after becoming director, Pringle acknowledged the specific obligation of the Institute to prevent epidemic malaria and the more general responsibility to blend research with benefits to participants. Among his research colleagues in the late 1950s and early 1960s, Pringle was unusual in his sensitivity to both the needs of the Malaria Institute (to gather data, conduct research, and end a project without being financially responsible for years in the future) and to the needs of local people (to minimize risk, provide tangible benefits, and to leave people with an overall positive impression of research and government interventions.) Pringle also admitted that part of the Institute’s duty to reduce suffering stemmed from the fact that the suffering was a direct result of their work.

In early July 1960, Pringle responded to advice from the East African High Commission about how to end the project. Their advice amounted to a recommendation to get out fast and to deny all responsibility. Pringle strongly disagreed:

> I do not think that timidity is an appropriate sentiment.... While I share the [East African High Commission] Administrator’s concern at the delicacy of the situation, I think that we will come out of the position with less odium by remaining on the spot and doing all we can to ameliorate the sufferings of the sick rather than to stage a retirement from the scene … I have always felt the need to combine the work of the field investigators with some sort of travelling dispensary of a very crude type. In this way the wishes of the Wapare and our own scientific requirements could be met.  

Pringle implies that the reputation of the researchers had suffered already, and that they needed to do the right thing by offering malaria treatment and paying attention to the “wishes of the Wapare” and not just their own scientific needs. At this stage, Pringle was willing to support the reintroduction of indoor residual spraying as long as the Pare would pay for it. This seemed like a reasonable compromise since the Malaria Institute could not

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76 Evans to Medical Research Secretary Nairobi, CC Provincial Commissioner Tanga, Provincial Medical Officer Tanga, Director Amani, August 8, 1960, Box 2, NIMR, Amani.

77 Pringle to Timms, July 8, 1960, Box 2, NIMR, Amani.
pay for IRS indefinitely, but it was also unsavory to think about an institute dedicated to malaria research trying to block local people from undertaking effective malaria control strategies.

There was an added challenge mixed in: the researchers also had to determine how to end the continuation scheme, which consisted of the fieldworkers treating fever cases they came across with malaria treatment. The program was “unquestionably popular” and Pringle guessed it would be “just as much a headache stopping this as it was stopping the spraying.” His plan was to taper the drugs “with great care and delicacy” once the Pare had endured next wave of transmission, which he predicted would be the beginning of 1964. He considered withdrawing drugs any sooner to be not “possible or humane.”

Regional Advisory Committee: “Give All Assistance Possible”

There were two medical research oversight groups functioning in East Africa in the late 1950s: the East African Council for Medical Research, which was responsible for allocating funds; and the East African Medical Research Scientific Advisory Committee, which provided technical guidance. Together, the groups dispensed advice about the design of projects, provided feedback for research difficulties, allocated funds, and helped distribute findings; the scientific work of the Malaria Institute was also subject to the approval of the Council. The group consisted of fellow researchers and scientists, and they typically provided sensitive and nuanced advice. The advisory group, as did the local government officials, thought early on about worst-case scenarios and the need for contingency plans. At their 1956 meeting, the group reassured members of the Tanganyikan and Kenyan government that “in an extreme emergency such as an epidemic … the Institute would certainly interrupt its current programme to give all assistance possible.” Even at this early stage, the Council emphasized that research would take a back seat to minimizing risk to African participants. Providing widespread malaria treatment could muddy or invalidate the Institute’s research findings related to indoor residual spraying, but the advisors were adamant that public health would be prioritized over continuation of the experiment.

Two years later, the Committee members weighed in with a lengthy discussion about what should happen when the spraying campaign stopped. The director of the advisory committee laid out two possibilities. First was that malaria transmission would have ceased or be very slight and there was no insecticide resistance, in which case it

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79 Pringle to Marcus, June 25, 1962, Box 24, NIMR, Amani.
80 Notes from a 1957 meeting of the Committee indicate that the following people participated: the Director of Medical Services, Uganda, Zanzibar, and Tanganyika; the Deputy Director of Medical Services for Kenya; The Directors of the Malaria Institute, the East African Virus Institute, and the East Africa Leprosy Research Center; members of the Colonial Medical Research Committee; the dean and members of the faculty of medicine at Makerere University; members of the East Africa Trypanosomiasis Research Organization and the Veterinarian Research Organization; the Wellcome Institute.
81 “East African Council for Medical Research, Second Meeting,” Unknown Date [1956], Box 22, NIMR, Amani.
would be necessary to have a small continuing scheme to see how best to keep the area free of malaria. The second possibility was that malaria was still active and that insecticide resistance had appeared. In either case, he was certain that “there would be a research responsibility” and if malaria was still present, “the people in the area should be protected from an upsurge of malaria … and the cost of this should come from research funds.”

This stark assessment of the research team’s ongoing responsibility to the local communities generated a great amount of discussion as to whether the African communities also had a financial responsible. Wasn’t it a better plan for the Pare and Taveta councils to levy taxes to pay for the continued spraying? Although the idea of shared financial responsibility was appealing, someone finally pointed out the obvious: the Pare “were very poor and that it was unlikely that they would be able to afford very much.”

It was eventually agreed that the Committee should recommend a small continuing research scheme where the goal would be to determine the best and cheapest methods of maintaining an area free of malaria. They believed “the results of such a scheme would be of immense value to the East African Governments.” However, they were also adamant that the costs of a malaria epidemic shouldn’t be the responsibility of the colonial governments. The advisory committee provided no clear advice about what shape the project should take as it moved ahead, but they did indicate that researchers had a continued direct obligation to any participants who suffered from a malaria epidemic.

The Council for Medical Research met one month later, in February 1960, and they too spent a great deal of time debating Pare Taveta and the proposed follow up projects. They ultimately approved continuation of the project, “but wished to emphasize that it should proceed on the basis of the search for, and treatment of malaria as a practical public health measure, and not solely as a scientific investigation.”

The advice given by these two oversight committees indicated that other scientists and researchers working in the area understood that how the Pare Taveta experiment was handled would reflect not just on Pare Taveta scientists, or on the Malaria Institute, but on future research projects. Committee members were wise enough to recognize that research projects left behind a residue, and it was important to think carefully about lasting impressions.

Local Communities: It Is “Morally Wrong” to Withdraw the Spraying

Notably absent from the advisory meetings and written correspondences were opinions from the Pare and Taveta people who had the most to lose as malaria returned. Throughout 1960 information emerged about what Pare and Taveta community members thought should happen as the experiment ended. Public opinion was strikingly unified: the spraying should be quickly restarted and continued indefinitely. The message emerged with such clarity that even the East African High Commission had to reluctantly admit, “there is a

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82 “East African Medical Research Scientific Advisory Committee, Nairobi,” January 20–21, 1958, Box 22, NIMR, Amani.
83 Ibid.
84 Ibid.
growing urge among the local inhabitants towards a resumption of spraying even, if they have no alternative, at their own expense.”\textsuperscript{86} The Pare Council formally stated that the spraying scheme had brought “considerable benefits which were widely appreciated and that it was morally wrong” to withdraw the spraying and let residents “suffer the consequences unaided.”\textsuperscript{87}

As the Pare Council pushed for additional spraying, similar conclusions were being reached among the Taveta Council in Kenya. The field researcher stationed there wrote that the Taveta, “took it for granted that they would have to put the money up themselves, that the council could not help from its present revenue and that this council would have to levy a special house tax to cover this. Everyone I spoke to seemed keen and willing that this should be done.”\textsuperscript{88} He predicted that if and when the demand fully materialized it would be “far more pressing than that made by the Pare Council.”\textsuperscript{89} A separate survey of people in the Taveta area about their impression of the spraying returned nearly identical results. People regarded the spraying as a “great blessing” but “fear was expressed … as to what would happen when and if the scheme came to an end.”\textsuperscript{90} A majority of the people interviewed said they “would be willing to contribute financially towards its continuation if this became necessary.”\textsuperscript{91}

This final opinion expressed by the Pare and Taveta local governing councils is a bit contradictory when compared with evidence gathered from earlier in the experiment. While the spraying was ongoing—and especially during the first and second cycle of spraying—many residents were critical of the perceived “weakness” of the insecticide and questioned whether the program was effective at killing mosquitoes. At some point, opinions changed and the spraying came to be valued to the extent that they did not want it to end. As one of the original entomologists on the project wrote, “in general, the malaria scheme was greatly appreciated by the inhabitants to the extent that people living outside the area asked to be included in it.”\textsuperscript{92}

\textbf{Conclusion: \textit{“Good Intentions Could Cause Adverse and Unanticipated Harms”}}\textsuperscript{93}

The Pare Taveta Malaria Experiment bridged the start and end of the global eradication era—an era that is starting anew. Privately and publicly funded projects to try to “roll back” malaria have pinned their hopes on new technologies such as transgenic mosquitoes, malaria vaccines, and long lasting insecticide treated bed nets, in addition to drawing on

\begin{footnotesize}
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\item \textsuperscript{86} Timms to Farrer-Brown, September 29, 1960, Box 2, NIMR, Amani.
\item \textsuperscript{87} The actual language used by the Council has not been recorded. District Commissioner Tanga to Unknown, “Pare Taveta Malaria Scheme,” September 1, 1960, Box 21, NIMR, Amani.
\item \textsuperscript{88} Field Officer to Pringle, “Taveta Council,” December 19, 1960, NIMR, Amani.
\item \textsuperscript{89} Ibid.
\item \textsuperscript{90} Msangi, “Public Relations Work,” Unknown Date [1960], Box 23, NIMR, Amani.
\item \textsuperscript{91} Ibid.
\item \textsuperscript{92} Alec Smith, \textit{Insect Man} (London: Radcliffe Press, 1993), 81.
\item \textsuperscript{93} Pam Das and Richard Horton, “Malaria Elimination: Worthy, Challenging, and Just Possible,” \textit{Lancet} 376 (2010), 1516.
\end{itemize}
\end{footnotesize}
older strategies such as indoor residual spraying, environmental management, and mass drug administration. The Bill and Melinda Gates Foundation is currently the largest funder of malaria eradication and control activities that range from laboratory based attempts to develop genetically modified mosquitoes to field-based trials of malaria vaccines. To date the Foundation has committed nearly US $2 billion in malaria grants, and more than $1.4 billion to the Global Fund. (For some perspective, the Gates Foundation’s annual budget is now larger than that of the entire WHO.) While their website accurately states “malaria is preventable and treatable” it goes on to make a more dubious claim: that “history shows it can be eliminated.” Yet all past attempts at global eradication have failed and cases of successful regional and national elimination have been limited to places that are some combination of wealthy, geographically contained, or under socialist leadership, and that do not have year-round, intense forms of transmission. The historical record and predictions by experts indicate that future attempts at eradication using the same methods will fail in the parts of sub-Saharan Africa and Asia that have the most intense transmission.

While debating the feasibility of global malaria eradication, we lose sight of the ethical questions that continue to vex modern malaria control experiments. Most of these questions would have been familiar to any of the actors involved in the Pare Taveta experiment. Again, we must ask: What kinds of obligations do researchers have to individual participants or the larger communities where experiments take place? Are there unique responsibilities that come with malaria control/eradication attempts because of the future risks of rebound, epidemic malaria? We know that endings are inevitable, and also that poor planning needlessly puts subject populations at risk. It may finally be time for a re-imagining of what an ethical ending can, and should, look like. Based on what we know from Pare Taveta, an ethical ending relies on researchers and participants sharing an understanding of the goals and potential risks of an experiment; it means speaking early and often about the possibility of failure; it provides benefits to participants that are equivalent to both the short and long term risks of participation; it incorporates responsible planning for worst case scenarios (including rebound malaria); and it makes clear that research will cease when public health is at risk.

This question of ethical endings is particularly timely as GlaxoSmithKline concludes trials of its experimental malaria vaccine, RTS,S/AS01. Thousands of African


95 Packard includes a very nice chart listing the places where malaria has been successfully eliminated, and groups them as “economically developed” “island nations” and “socialist countries.” Packard, The Making of a Tropical Disease, 160.

96 A recently published article in the Lancet showed that it was “least feasible” to consider eradication in sub-Saharan Africa, and that it may not even be economically prudent. Das and Horton, “Malaria Elimination,” 1515. Marcel Tanner and Don De Savigny, “Malaria Eradication Back on the Table,” Bulletin of the World Health Organization 86, 2 (2008).

97 Few articles have addressed the risks specific to the malaria vaccine trials, and even those have failed to fully discuss the risks of resurgent epidemic malaria. Joseph Fadare and Olusegun Ademowo,
infants and toddlers in eleven different African locations—including five sites in Kenya and Tanzania—are being given the vaccine, which past studies have shown can reduce the number of severe cases of malaria by 30–50 percent. Despite the known risks around loss of acquired immunity in a high transmission areas there are no publicly stated plans for what will happen when the vaccine trial ends. At some point, the vaccine’s protection will wane and the infants will continue to live in a malarial zone without the benefit of acquired immunity. In the best-case scenario, these children will have a better chance of surviving their initial malaria infection because they will be slightly older, and will acquire their partial immunity through subsequent infections. In a darker scenario, the vaccine’s protection will merely shift the malaria mortality to a slightly older age group. Or, worse still, since immunity is still not fully understood, the vaccine could inhibit in some way the development of acquired immunity among older children.

Modern international ethical codes and laws in the United States and Europe are vague about what should happen when experiments end, whether there is any longer-term obligation to research participants, or who might be financially responsible if a malaria epidemic occurs when a control experiment ends. For the most part, these documents encourage a degree of responsibility, without going so far as to define it or mandate it. Even drug companies such as GlaxoSmithKline are comfortable stating that at the end of a trial they “strongly support the goal of improving access to medicines” but at the same time, voice their concern about “the suggestions that research sponsors should be routinely obliged to provide treatments to participants post-trial.” Such vague guidelines and contradictory statements mean that not only are researchers’ ongoing obligations


debatabile, but we are no closer to having a model for what a fair and equitable ending might look like.

This article’s closer look into the discussions about how to end the Pare Taveta Malaria Experiment reveal both unsavory and uplifting aspects of the debates. On the one hand, it is abundantly clear that some participants—such as the East African High Commission, the WHO, UNICEF, and the Nuffield Foundation—did not want to acknowledge any long term obligation to Pare and Taveta communities, nor did they want to engage in a discussion of researchers’ responsibilities when trials end. For these groups that regularly funded field research, beginning a discussion about financial obligations after a trial ended appeared a fool’s task. On the other hand, many of the participant organizations—the regional medical advisory groups, local colonial government officials, and the Malaria Institute itself—were strong and vocal critics of the idea that there was no obligation and that communities should be left to suffer malaria alone. Field researchers, colonial district officials, health officers, and even the Director of the Malaria Institute spoke eloquently to the practical and ethical concerns of ending a project. Many of these men spoke humanely of the responsibility to protect people from an upsurge in malaria by eliminating unnecessary risk and ameliorating the sufferings of the sick. Those with the most foresight knew that for research to continue productively, the wishes of scientists had to be balanced with the needs of local people, and that research ought to be conducted with great care, delicacy, and humanity.