Conclusion: Experimentation in Colonial East Africa and Beyond

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> Have we a right to perform experiments and vivisection on man? Physicians make therapeutic experiments daily on their patients, and surgeons perform vivisections daily on their subjects. Experiments, then, may be performed on man, but within what limits?
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> Claude Bernard, *Introduction to the Study of Experimental Medicine*, 1865

> It seems, then, that when the experiments are sufficiently important, the use of African volunteers is justified.
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> J.F. Corson, Medical Officer, Tinde Laboratory, Tanganyika 1938

Just what constitutes medical experimentation and what difference does place make to these undertakings?[1] Some projects that we excavate from the past still resemble what we imagine today when we think of human subjects research. Others now fall well outside such parameters. Identifying these borders and the grey areas in between helps us appreciate the ways in which ethical norms and legal standards have changed over time. It also reminds us, should we need such reminders, of the institutional and social asymmetries in the world. Not all people and places have played equal roles in designing experiments and certainly some groups and regions have been disproportionately the targets. This special issue drives home the need for more comparative and regional studies that help us understand the ways in which colonies and empires have shaped global histories of human experiments.

To illustrate these patterns, we can begin by examining two proposals made at the end of the nineteenth century that had some bearing on East African research and policy-making. The first points to the centrality of blood work to the history of public health and the second delves into now discarded ideas about racial mixing and its consequences. Both highlight how entangled experimental and imperial logics have been. To take the first example, in 1894 Britain's Anthropological Society endorsed a request by Patrick Manson, the specialist in tropical medicine, that "travellers and residents in tropical and sub-tropical countries … make collections of blood slides on a systematic plan and afterwards forward their collections to London." The journal's editor claimed that this research "would confer a boon on zoological and medical science by investigating … blood parasites in [different] districts" and by differentiating samples by "race, age, sex, occupation, and any other point of interest." The blood gathered, the journal claimed, would help investigators map the

[1] My thanks to Melissa Graboyes for inviting me to contribute this piece and my enduring gratitude to Megan Vaughan and Steve Feierman for their inspirational work in African medical and intellectual history.

geographical scope of a variety of parasitical diseases, including the uniquely African pathology, "sleeping sickness." In the editor's detailed description of how to prepare such slides, he stated that "fifty slides from fifty inhabitants of any district would be sufficient," though he made no mention of how these collectors were to gain their subjects' trust or permission. The recommendation to take bodily fluids from people whether ill or well across colonies, required little comment at the time.[2] Indeed, in 1892 the second edition of the British *Notes and Queries on Anthropology* referred, in a new section on bacteriology, to the need for ethnographers to collect "drop[s] of blood" and "send such to bacteriologists who will be most pleased to experiment with them especially where full clinical notes of the particular cases are forthcoming." The authors of *Notes and Queries* discussed collecting blood in much the same way that they suggested "sending home products which are novel, or have a reputation as being medicines or poisons." Blood samples were becoming part and parcel of collectors' inventories and were considered integral to solving problems about specific diseases of the tropics.[3] We tend to think of the phenomenon of "bioprospecting" as limited to plant or geological materials, but by the late nineteenth century it included bodily fluids as well. Thanks to the storage practices of certain North American laboratories, some of these samples from the Belgian Congo—collected in 1959—have played an important role in the genetic time-sequencing of different strains of HIV.[4]

At the other end of the experimental spectrum, also in 1894, Harry Johnston, the High Commissioner for Central Africa (present-day Malawi) and an active naturalist and ethnographer proposed in his annual report that Britain ought to promote in Central and East Africa racial miscegenation between South Asians and Africans. European countries' "scramble" to seize control of African territories was well underway at the time Johnston wrote, though just how the spoils would be divided and what would be done with these new territories remained an open question. In Johnston's eyes this meant putting all kinds of different scientific proposals on the table for discussion. "On the whole," he reported after reviewing possibilities for producing a "satisfactory hybrid" race, "I think the admixture of yellow that the negro requires should come from India, and that Eastern Africa and British Central Africa should become the America of the Hindu. The mixture of the two races would give the Indian the physical development which he lacks, and he in his turn would transmit to his half negro offspring the industry, ambition, and aspiration towards a civilized life which the negro so markedly lacks."[5] Johnston's vision of a British policy endorsing deliberate racial amalgamation was eventually shot-down by other

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[2] All quotations from "Blood Parasites, with Directions for Preparing Slides of Dried Blood," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 23 (1894), 418–20.

[3] John G. Garson and Charles H. Read, eds., *Notes and Queries on Anthropology Second Edition* (London: Anthropological Institute, 1892), 83–84 (bacteriology and blood), and 73 (therapeutics).

[4] T. Zhu et al., "An African HIV-1 Sequence from 1959 and Implications for the Origin of the Epidemic," *Nature* 391 (1998), 594–97. Edward Hooper discusses the origin and storage of this particular sample in *The River* (Boston: Little Brown, 1999), 17–20.

[5] H.H. Johnston, *Report by Commissioner Johnston of the First Three Years' Administration of the Eastern Portion of British Central Africa, 31 March 1894* (London: HMSO, 1894) [c. 7504], 31.

statesmen-scientists, including Frederick Lugard and John Kirk, at the 1895 International Geographical Congress in London, but not because either man objected on ethical or even biomedical grounds. Kirk and Lugard simply wished to avoid creating "an intermediate half-caste race" because this new population might impede the existing experiment of "European colonization" in the East African highlands, an endeavor that at least some of its supporters expected might produce a new "biological type" of European.[6] A full three decades later, Britain's tropical medicine specialists Andrew Balfour and H.H. Scott continued to frame European settlement in the African tropics in experimental terms: "In Kenya … actually upon the equator, an experiment is being conducted on a large scale which has not its exact counterpart anywhere else in the world … Will the race [of those resident in the highlands] degenerate or will it maintain its intellectual and moral vigour?"[7] While theories of racial acclimatization were already waning by the interwar period, suppositions about races' appropriate physiological and generational relations to their environments persisted in trace forms in the biomedical literature for years. Ironically, this so-called racial experiment had as its subject population the settlers themselves, but it could only persist because colonial conquest and policy-making had already excluded Africans and South Asians from owning land in the region.

By exploring these two examples in some detail, we see the different ways in which European actors defined (East) African territories as sites of extraction, experimentation, and knowledge production. To map the region's disease environments, travelers and officials were instructed, among other things, to take people's blood. To develop its territories economically, administrators toyed with scenarios that often reinforced ideas of racial and colonial subordination, mixing biological and social rationales. These patterns may not seem so surprising to anyone familiar with African colonial history, yet we should avoid confusing biomedicine's imperial nature or scientists' geographically expansive horizons with realities on the ground. One of the benefits of historicizing a phenomenon that took place in many parts of the world at similar times is that we can start to put together a composite picture of the intensity and scale of its effects.

If we situate colonial Africa in global and comparative contexts, we begin to see that *formal* or *nontherapeutic* human experiments occurred there with less frequency than in other regions of the world throughout much of the twentieth century. This is true even when we take into account the precedent-setting randomized BCG vaccine trials for tuberculosis that took place between 1930 and 1956 in French Algeria.[8] Indeed, in the decades surrounding the turn of the twentieth century, countries that developed a relatively dense infrastructure for biomedical and scientific research—such as France, Germany, Britain, and the United States—tended to experiment on more of their citizens in a more

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[6] John Kirk [with Frederick Lugard], "The Extent to which Tropical Africa is Suited for Development by the White Races, or Under Their Superintendence," *Report of the Sixth International Geographical Congress, London 1895* (London: John Murray, 1896), 533–34.

[7] Andrew Balfour and Henry Harold Scott, *Health Problems of the Empire: Past, Present, and Future* (London: W. Collins and Son, 1924), 105.

[8] Clifford Rosenberg, "The International Politics of Vaccine Testing in Interwar Algiers," *American Historical Review* 117 (2012), 671–97.

systematic way than anywhere else because their states, and the institutions they supported, had both the power and authority to intervene.[9] The very fact that by 1914 European and American physicians' diagnostic techniques could include taking blood, sputum, urine, and swabs; that autopsies and dissections were routine ways for physicians to investigate specific illnesses and diseases; and that many millions of people encountered annually these and other practices, such as compulsory vaccinations and infectious disease inspections, drives home the point that biomedicine had become deeply enmeshed in patterns of life and death in these regions.[10]

In other words, experimental medicine of the sort Claude Bernard and other specialists advocated began to flourish *first* in places with relatively strong networks of hospitals, laboratories, asylums, and research facilities, networks that allowed experimental practice to extend into sites of everyday life such as schools, factories, prisons, the military, and even plantations. These are the phenomena that so captivated Georges Canguilhem, Michel Foucault, and generations of historians of medicine. Sorting out the differences among diagnosis, treatment, and testing required that people recognize and accept just such a division of labor in the first place. This process was neither self-evident nor inevitable and stemmed in part from scientists' growing interest in combining accurate knowledge of bodies and environments with therapies and solutions that worked. It also arose out of biomedical controversies that caused a public outcry or backlash, such as deaths and debilities caused by contaminated vaccines or drugs that highlighted the dangers of being subjected to novel treatments. Paradoxically, given what came later, some of the strongest policies regulating human research arose in Prussia, which enacted fairly comprehensive ethical guidelines in Berlin in 1900 and in Germany more broadly in 1931.[11]

Still, as this special issue underscores, empires were hardly absent from the rise of experimental medicine, which helps to explain why several early twentieth century efforts to secure "informed consent" in biomedical projects with human "volunteers" took place in Cuba (with yellow fever research), the Philippines (with research on dysentery), and Tanganyika (with research on Bayer 205 for sleeping sickness).[12] It also explains why

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[9] See Melissa Graboyes' introduction to this special issue and Clifford Rosenberg's 2012 *AHR* article for many of the citations relating to European and American histories of human experimentation.

[10] W.F. Bynum, *Science and the Practice of Medicine in the Nineteenth Century* (Cambridge: Cambridge University Press, 1994), 176–77; also Sarah Ferber and Sally Wilde, eds., *The Body Divided: Human Beings and Human Material in Modern Medical History* (Burlington, VT: Ashgate, 2011).

[11] On the context surrounding European discussions of the ethics of human experimentation, including the 1900 Berlin edict and Germany's 1931 guidelines, see the contributions in Volker Roelcke and Giovanni Maio, eds., *Twentieth Century Ethics of Human Subjects Research: Historical Perspectives on Values, Practices and Regulations* (Stuttgart: Verlag, 2004).

[12] Susan Lederer, "Walter Reed and the Yellow Fever Experiments," in Ezekiel Emanuel, ed., *Oxford Textbook of Clinical Research Ethics* (Oxford: Oxford University Press, 2008), 9–17; Susan Lederer, *Subjected to Science*, 110–11; Helen Power, "'For Their Own Good': Drug Testing in Liverpool, West and East Africa, 1917–1938," in John Woodward and Robert Jütte, eds., *Coping With Sickness: Medicine, Law,*

Sinclair Lewis' immensely popular 1925 novel *Arrowsmith* included a fictional account of a plague epidemic on a colonial Caribbean island and explored the protagonist's controlled trial of a drug to treat its victims, in which half the island's population received the drug and half was denied. Lewis actually toured several Caribbean islands with Paul de Kruif, author of *The Microbe Hunters* (1926), as background for the novel.[13]

In sheer statistical terms, it seems safe to assert that colonial populations were never in the majority as test subjects around the world, at least not if we use a narrow definition of experimental subject. This is one of the paradoxes of the interplay between science and empire: though autocratic states were able to strip people of their sovereignty, they were not always able to translate political power into experimental power. The Principal Medical Officer for Uganda, A.P. Hodges, made just this point in 1912 when the British Colonial Office was considering a far-reaching "experiment" to test whether compulsory examinations and treatment of women across the territory might help to control for venereal disease. "[A]n experiment [of this sort]," Hodges replied, "has been already tried in several civilised countries, having very powerful police and sanitary executives, and, in the opinion of a very large number of authorities competent to judge, has failed. Would such an experiment be more likely to succeed in an undeveloped country such as Uganda?" State compulsion, he continued, was only feasible when people "were under complete control," a phenomenon limited to prisoners and perhaps soldiers.[14] He did not have to stress how far this was from reality in Uganda: his readers would have understood his point immediately. After all, the territory's total colonial service amounted to only 279 officers and assistants who were responsible for overseeing approximately three million people distributed across nearly 110,000 square miles. Colonial Office bureaucrats dropped the suggestion, though as Carol Summers and others have shown, less sweeping interventions were tried.[15]

Hodges' remarks also draw attention to the flip-side of the science-empire paradox: while the lines between experimentation, policy-making, and therapeutic practice were being sharpened and standardized in certain imperial centers (though hardly everywhere), in East Africa (and beyond), they remained decidedly blurry and idiosyncratic. Colonial rule itself was always something of an experiment and colonial subjects were rarely asked for their consent, informed or otherwise, when it came to decisions about jurisdiction.

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and Human Rights—Historical Perspectives (Sheffield: European Association for the History of Medicine, 2000), 107–26, esp. 117–19.

[13] Ilana Löwy, "Martin Arrowsmith's Clinical Trial: Scientific Precision and Heroic Medicine," *Journal of the Royal Society of Medicine* 103 (2010), 461–66.

[14] Principal Medical Officer's [A.D.P. Hodges] Report to Chief Secretary Uganda Protectorate, 11 April 1912, CO 879/109, "Further Correspondence relating to Medical and Sanitary Matters in Tropical Africa, January to June 1912," British National Archives, London.

[15] Statistics from Helen Tilley, *Africa as a Living Laboratory: Empire, Development, and the Problem of Scientific Knowledge, 1870–1950* (Chicago: University of Chicago Press, 2011), 19, 338; Carol Summers, "Intimate Colonialism: The Imperial Production of Reproduction in Uganda, 1907–1925," *Signs* 4 (1991), 787–807; Philippa Levine, *Prostitution, Race, and Politics: Policing Venereal Disease in the British Empire* (New York: Routledge, 2003).

Scientific infrastructures in sub-Saharan Africa, even in areas that invested heavily in biomedicine such as the Belgian Congo, were still relatively weak when compared to industrial hubs in Western Europe and North America and to metropolitan centers in Asia and Latin America. These frailties meant that many kinds of scientific interventions doubled as research and certain types of research doubled as therapeutics. Colonial officials often tacitly recognized this broader definition of experimentation because they knew that both research and practice shared a trial-and-error quality and could result in new scholarly knowledge. On occasion, scientists even argued that the very absence of dense infrastructures of medical care and the trappings of industrial life could enable new types of therapeutic understanding to emerge. In 1936, for example, ecologist and scientific administrator E.B. Worthington observed when explaining the different kinds of research on diets across the continent that "Africa, at the moment, may be compared to a nutritional laboratory in which innumerable experiments on controlled diet have been progressing for a hundred years or so. An enormous amount may be learned by simply collecting the results of these experiments."[16] Africans' relative isolation, Worthington and others alleged, had created conditions not unlike those one might engineer in a laboratory and they ought to be studied as such. The British Government went on to fund several different large-scale, interdisciplinary projects across the continent designed simultaneously to gather and produce this data. "Collecting" results, however, proved to be far more complex than supporters of nutritional research imagined, especially given that many field scientists had an incomplete understanding (if at all) of the socioeconomic effects of colonialism itself.[17]

What can often seem insidious about biomedical research in colonial contexts is its advocates' blindness to the rights or interests of those individuals who came under its purview. These patterns stand out especially in the contributions in this special issue by Patrick Malloy, Jennifer Tappan, and Melissa Graboyes. Yet for scholars who have compared more than one location, what is also striking is that such indifference was commonplace in many geographical settings. Helen Power, who has examined British scientists' attitudes towards their human subjects in Tanganyika, the Gold Coast (Ghana), and England in the interwar period observed similarities rather than differences. "There was little more regard for the British soldiers [who served as test subjects]," she writes, "than their African counterparts." She also points out that "standard medical practice [in Britain in this period] routinely involved the use of new substances, and variations in their dosage and method of administration."[18] Christoph Gradmann and Deborah Neill have observed similar patterns with German and French investigators who blurred the

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[16] E.B. Worthington, "On the Food and Nutrition of African Natives," *Africa* 9 (Apr. 1936), 162–63.

[17] Cynthia Brantley has explored the history of nutritional research in Kenya and Malawi, but there were also studies in Tanzania, the Sudan, Nigeria, and The Gambia, not to mention the work on malnutrition or *kwashiorkor* in Ghana and Uganda.

[18] Power, "'For Their Own Good,'" 120–21.

boundaries "between therapeutic and human experiment" and were willing "to try dangerous drugs not just on their patients, but on each other."[19]

The Tanganyikan experiments Power describes are worth exploring in more detail both because they speak to histories of human subject research and because they shed light on histories of self-experimentation, risk, and consent. Indeed, what Power touched upon was just one episode in a much longer-standing biomedical experiment, for almost a quarter of a century (1934–1957), investigating the infectivity of a single strain of sleeping sickness.[20] These efforts came to be known as the "Tinde experiment" named after the Human Trypanosomiasis Research Laboratory at Tinde in the Shinyanga District where so much of the territory's long-term research on sleeping sickness was pursued.[21] At the conclusion of the experiment not only had investigators learned that their single strain of *Trypanosoma rhodesiense* had retained its lethality to humans over the course of twenty-three years (disproving any relationship between *T. rhodesiense* and the non-lethal animal strain, *Trypansoma brucei*), but also that multiple animals species served as hosts to *rhodesiense* and that "infected humans" often "carried [the disease] to new areas … [where it was] then … maintained at a low level of endemicity by infected wild animals."[22] Culling game species as a control strategy therefore would not work.

The Tinde laboratory was located in Sukumaland between the towns of Nzega and Old Shinyanga. Approximately one million Sukuma resided in Tanganyika in the 1930s and about 200,000 lived in Shinyanga district, a "heavily cultivated and grazed steppe … [at about] 4000 feet above sea level."[23] Sukuma were productive agriculturalists and livestock owners who grew millet, maize, cassava, and cotton. A 1934 inventory of livestock in the district counted 420,400 heads, divided unevenly among the population, according to social hierarchies. The Tinde laboratory was founded in 1930 by bacteriologist, James F. Corson, and was incorporated into the Tsetse Research Department in Old Shinyanga when Corson retired in 1939. Besides the rather extensive sleeping sickness and tsetse research undertaken in the area, populations surrounding Tinde and Old Shinyanga were also the subjects, in the 1930s, of Donald Malcolm's land tenure investigations and government ethnographer, Hans Cory's research into Sukuma secret societies and legal systems.[24] Thus, while the biomedical infrastructure was sparse,

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[19] The first quote is Gradmann, the second Neill, both in Deborah Neill, "Paul Erhlich's Colonial Connections: Scientific Networks and Sleeping Sickness Drug Therapy Research," *Bulletin of the History of Medicine* 22 (2009), 74.

[20] K.C. Willett and H. Fairbairn, "The Tinde Experiment: A Study of *Trypanosoma rhodesiense* during Eighteen Years of Cyclical Transmission," *Annals of Tropical Medicine and Parasitology* 49 (1955), 278–92; and M.T. Ashcroft, "The Tinde Experiment: a Further Long-Term Cyclical Transmission of *Trypanosoma rhodesiense*," *Annals of Tropical Medicine and Parasitology* 53 (1959), 137–46.

[21] I discuss this other research in chapter 4 of my book, *Africa as a Living Laboratory*.

[22] Ashcroft, "The Tinde Experiment," 143.

[23] Charles Innes Meek, *Brief Authority: A Memoir of Colonial Administration in Tanganyika* (New York: I.B. Taurus, 2011), 53.

[24] Both men published their work years after it was completed. See Donald Malcolm, *Sukumaland: An African People and Their Country: A Study of Land Use in Tanganyika* (Oxford: Oxford University Press,

the people living in the Tinde/Shinyanga environs were already under other kinds of scrutiny. This helps to explain why Sukumaland became the site of a large post-War development scheme involving extensive land resettlement and agricultural projects.[25]

In early 1937, J.F. Corson decided to recruit Sukuma "volunteers," living in close proximity to the laboratory, to be infected deliberately with *Trypanosoma rhodesiense*, by allowing tsetse flies carrying the parasites to bite their arms. Corson wanted to use uninfected participants because it would help him understand the virulence of the *rhodesiense* strain that he had isolated.[26] More to the point, it would also allow him to monitor volunteers' different symptoms and the "complications" that arose after they were treated with the drug Bayer 205, also known as germanin.[27] Between 1927 and 1932, Corson had already tested or witnessed the effects of two different drugs—Bayer 205 and Tryparsimide—on hundreds of sleeping sickness patients in Northwest Tanganyika and was intimately familiar with published studies on the treatment of thousands more. During this time, he had collaborated in the field with the German bacteriologist, Friedrich K. Kleine, whose work for the League of Nations Sleeping Sickness Commission included a series of "transmission experiments" related explicitly to studying the "effect of Bayer 205 on tsetse trypanosomes."[28] In 1928 Corson had already reported that there was "no other drug that can compare in value with 'Bayer 205' in the treatment of early cases of Rhodesian sleeping sickness" and that, given Europeans' tendency to seek care early, "there seems to be no reason why a white person should, in future, die of the disease." The drug seemed less effective among Africans, in large part, he claimed, because they "usually first come for treatment in a later stage [of the disease]."[29]

Corson's early human subject research at the Tinde lab focused on disaggregating the infectivity of human and animal strains of the disease and also on exploring whether the virulence of these strains persisted over long periods of time in animal reservoirs. His

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1953); Hans Cory, "The Buyeye: A Secret Society of Snake Charmers in Sukumuland, Tanganyika Territory," Africa 16 (1946), 160–78; Hans Cory, *The Indigenous Political System of the Sukuma* (New York: Eagle Press, 1951); and Hans Cory, *Sukuma Law and Custom* (Oxford: Oxford University Press, 1953).

[25] Rohland Schuknecht, *British Colonial Policy After the Second World War: The Case of Sukumaland, Tanganyika* (Berlin: LIT Verlag, 2010).

[26] Corson first began human experiments in 1932, before he had isolated the *rhodesiense* strain associated with the Tinde experiment. I describe that first experiment in this article and also the 1937–38 experiments, but there were a handful of other experiments conducted between 1934 and 1936.

[27] J.F. Corson, "A Record of Some Complications which Occurred in the Course of Experimental Infections of African Volunteers with *Trypanosoma Rhodesiense*," *Annals of Tropical Medicine and Parasitology* 32 (1938), 437–43; J.F. Corson, "A Further Note on Some African Volunteers in Experimental Work with *Trypanosoma Rhodesiense*," *Annals of Tropical Medicine and Parasitology* 33 (1939), 97–99.

[28] Secretary General, League of Nations, to British Under Secretary of State Foreign Office, 10 September 1926, Box R855—Health 12 B—Series 21836, "Tropical Diseases (Sleeping Sickness)," League of Nations Archives, Geneva.

[29] J.F. Corson, "Sleeping Sickness in the Ikoma District of Tanganyika Territory: Notes on Some Cases Treated by Professor F.K. Kleine," *Annals of Tropical Medicine and Parasitology* 22 (1928), 383.

first round of experiments in 1932 included only four volunteers: himself, a "European" veterinary officer, and two "native" participants. In the published record, he says nothing about how he recruited any of his other three volunteers, though we do learn that Corson had no qualms about exposing himself twice to sleeping sickness infection despite the fact that the disease was always fatal unless treated.[30]

Corson's 1937–38 round of experiments included a total of forty-three "African volunteers." He noted that he had also received "offers of a few European men and women to be volunteers," but that he had chosen not to include them because their work schedules would not permit them "to undergo the discomfort of an attack of sleeping sickness and its treatment." In his introductory remarks about this second round of experiments, Corson was explicit about his selection process. "[U]nsophisticated African volunteers should not be used for experimental infection," he wrote, "unless the experimenter is convinced, on good grounds, that the infection will be free from risk of permanent injury to the health of the volunteers." Given that Corson had already subjected himself to infection with *T. rhodesiense* and had recovered with the use of Bayer 205, he felt confident that his experiment was safe. Even so, he noted that his volunteers "had sufficient intelligence and experience of Europeans to believe that such an experiment would not be made without a sure remedy for the disease" and that they had "decid[ed] to offer themselves as volunteers … [because] they had some evidence and knowledge to guide them."[31]

As with the yellow fever experiments in Cuba, in which volunteers were given one hundred dollars at the experiment's conclusion, Corson also offered "a relatively large reward of money," noting how eager participants were "to gain it." The reward alone, he thought, motived many people to volunteer (to the point that he had to turn away approximately forty more potential participants) so that he did not have to make "use of any [other] persuasion or any form of propaganda to induce volunteers to present themselves."[32] The fact that he enlisted participants at different times, beginning with the first volunteer in March of 1937 and adding the last in September of 1938, meant that participants were able to speak to each other about the experiment's procedures and, presumably, reassure each other about its safety.[33] In addition, they probably consulted with the laboratory's "head of African staff," Musulwa Saidi Kapere, who was "largely responsible for the running of the Sleeping Sickness lab at Tinde" and who researchers said had "been of the greatest assistance" during the entirety of the experiment's twenty-three year run.[34]

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[30] J.F. Corson, "Experiments on the Transmission of *Trypansoma Brucei* and *Trypansoma Rhodesiense* to Man," *Annals of Tropical Medicine and Parasitology* 26 (1932), 109–15; the European volunteer was H.C. Smith of the Veterinary Department and the two "native" volunteers were identified only as "Native M. and Native P.B."

[31] All quotations about second experiment from Corson, "A Record of Some Complications," 437–39.

[32] Corson, "A Record of Some Complications," 439.

[33] Names and dates of infection in Corson, "A Further Note on Some African Volunteers," 98–99.

[34] I have pieced this information about Kapere together from multiple sources; he was employed by the Tanganyikan government for a total of thirty-three years (1924 to 1957), and was awarded a British Empire

When Corson drew his research on Bayer 205 to a close in October 1938, he reported only one complication resulting from its use: temporary kidney damage that caused albuminuria, or too much protein in volunteers' urine. Twenty-five of the forty-three volunteers presented with this problem, though it only seemed to appear after the third or fourth dose, when "infection with trypanosomes could be regarded as cured."[35] All the volunteers ended Corson's experiment well, except one. Volunteer number thirty-nine, a man named Mihambo Kukuwa, died approximately three months after receiving his fourth dose of germanin. While Corson suggests that Kukuwa's symptoms—tinnitus, bodily aches in his arms and legs, and general weakness—were unrelated either to infection with *rhodesiense* or treatment with Bayer 205, he never stated this explicitly. What Corson did do, however, was monitor thirty-eight of the original forty-three participants for another six months between October, 1938 and February, 1939. (Four volunteers had moved out of the district.) In his follow-up publication reporting on "complications" with Bayer 205, Corson revised his assessment. At least thirteen of the volunteers, including Kukuwa, he noted, experienced "some disturbance of health during or soon after their course of treatment," including mild to severe bodily pain, diarrhea, and even fever. Though Corson had originally argued that, "such illnesses were not exceptional in the local population to which the volunteers belonged," he now believed that germanin did in fact "play some part in the production of those illnesses."[36] Corson's second article, in other words, went on record acknowledging the physical risks to which he was exposing his volunteers.

What we do not yet know about Corson's experiments, and may never know, is how his volunteers understood their experiences. What were they told about the risks? Did they learn that Corson exposed himself and survived? Just how much of an incentive to participate was the money? Were there other benefits that volunteers sought? Did Corson's insights about Bayer 205's side effects change the way it was administered or used? How much did it matter that Kapere kept many of the lab's records and therefore probably interacted frequently with the volunteers?

Sukuma people would almost certainly have associated the bite of the tsetse fly with illness because they pursued grazing and settlement strategies designed to avoid close proximity to the flies. When Kukuwa returned to his home, just two miles from the laboratory, healers would have visited him before his death and would have tried to interpret and treat his misfortunes. Kukuwa had already been compensated for his participation in the experiment, which may have been taken as a sign of economic prosperity. Rather than associate his illness with the laboratory, healers might have considered his physical symptoms a sign of his ancestors' hostility to another kind of

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Medal in the 1950s before his retirement. The direct quotations are from *Annual Report of the East Africa High Commission for 1957* (London: HMSO, 1958), 40; and Ashcroft, "The Tinde Experiment," 144.

[35] Corson, "A Record of Some Complications," 442.

[36] Corson, "A Further Note," 97–98.

social transgression.[37] With dozens of other former volunteers in the area alive and well, Kukuwa's death, paradoxically, might not have stood out. Indeed, over the course of the next eighteen years researchers at the lab deliberately infected another 373 Sukuma men in the area with the same strain of *Trypanosoma rhodesiense*. Along the way, two senior investigators, H. Fairbairn and George Maclean, also volunteered.[38] Presumably everyone was treated with Bayer 205, though after 1939, no one involved in the Tinde experiment seems to have shared Corson's interest in publishing results on the volunteers' health. Reporting on the infectivity of the *rhodesiense* strain took center stage and the role of the volunteers receded into the background. We do learn, in telling asides, however, that certain volunteers "had to be treated comparatively early, as we dared not risk incurable complications."[39]

While there remains much that we do not know about the Tinde experiment, of one thing we can be certain. Sukuma participants and their relatives and friends had considerable room to make their own meaning out of these experimental experiences. Whether they were mistrustful or grateful, whether they took part opportunistically or felt coerced, whether they understood the risks or interpreted "complications" in a different register entirely, they still retained the ability to make sense of their own lives. The uneven legal, institutional, and professional reach of biomedicine in many parts of colonial Africa, meant that while it had a formal monopoly in most places, in practical terms its effects were shallow, punctuated by acute—and often jarring—manifestations, such as treatment campaigns, medical surveys, nutritional studies, and even human experiments. African historians have pointed to biomedicine's weaknesses to help explain why other kinds of therapeutic ideas and practices endured. Equally important are the sociological and epistemological effects such weaknesses had on people's perceptions of and reactions to biomedical interventions—dynamics that the articles in this special issue place in the spotlight.

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[37] R.E.S. Tanner, "The Magician in Northern Sukumaland," *Southwest Journal of Anthropology* 13 (1957), 344–51, esp. 346–47. Tanner was a district officer in Sukumaland from 1948 to 1961 and published numerous ethnographic studies.

[38] By 1945, experiments had been done on 336 volunteers of whom 61.6 percent were infected with sleeping sickness; H. Fairbairn and E. Burtt, "The Infectivity to Man of a Strain of *Trypanosoma Rhodesiense* Transmitted Cyclically by *Glossina Morsitans* through Sheep and Antelope: Evidence that Man Requires a Minimum Infective Dose of Metacyclic Trypanosomes," *Annals of Tropical Medicine and Parasitology* (1945), 279. Another 130 tests were done between 1946 and 1953 as reported in Willett and Fairbairn (1955), 281–85. According to Ashcroft (1959), fifteen more subjects were recruited between 1954 and 1957, bringing the total to 481 volunteers.

[39] Fairbairn and Burtt, "Infectivity to Man," 304.