**"Petty Larceny" and "Manufactured Science": Nineteenth-Century Parasitology and the Politics of Replication**

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At the end of the nineteenth century, the concept of replication took on a central role in the emergence of a new sub-discipline. Replication was both an important part of parasitology's scientific methodology and a locus of anxiety for its proponents, a professional network, who, led by Nobel Prize winning malariologist Ronald Ross, strove for individual recognition and priority. For Ross, a man obsessed with re-writing literary classics, re-inventing visual technologies, and re-imagining everything from phonetic spelling to mathematics, replication was a fraught concept determined largely by context. As an experimental procedure, it demonstrated consistency and signified truth. As an investigative tool, it embodied, not just emulation, but modification and improvement. However, when used by his competitors, it also meant plagiarism, piracy, and fraud. Using Ross's mosquito-malaria work as a case study, I will explore the politics of replication in all its forms—as a scientific methodology, as an ideological motif, and as a framework that exposed the politics of this network of scientists, in their disputes over scientific priority. While in speeches Ross referred to priority as "petty inter-tribal advantage,"[1] it was a qualm that clearly haunted him for his entire career, leading him to write in 1924 that he regretted ever investigating malaria. "Humanity," he argued, "is not worth it!"[2]

Born in India to Scottish parents in 1857 at the foothills of the Himalayan mountains, Ronald Ross was the eldest of ten children. At the age of the eight, he was sent back to England for health and education, and—as he tells us in his memoirs—he whiled away his time reading Shakespeare, Milton, Tennyson, Byron, Homer, and the Bible. He struggled and eventually got the better of Euclid. He had "a secret passion for music," and spent time painting, sketching, and

experimenting with watercolor after the style of his father (Ross 1923: 22).[3] In his formative years—and indeed well beyond—he was obsessed with re-writing and re-imagining famous works, acts that might be thought of as replication. He replicated Cuvier and Buffon's natural histories, drawing up his own taxonomies of the natural world with data "drawn"—probably directly copied—from editions of their books in his uncle Dr. William Wilmott's library. He re-wrote several Greek myths, as well as re-imagining William Gilbert's *Pygmalion and Galatea*. In Ross's version, called *Edgar; or the New Pygmalion,* when the statue (now called Niobelle) comes alive, she does not care for the sculptor at all and rejects him. His version also has an opening prologue borrowed almost directly from Goethe's *Faust* (see Ross 1883). Ross argues that Goethe had borrowed it from the Book of Job, noting: "I held (with Goethe) that an artist may borrow whatever he pleases if the perfection, which he is designing requires it" (Ross 1923: 45). This treatise on replication is interesting because it posits replication as a form of borrowing and legitimizes it as a methodology employed during the process of "improvement." However, this is an attitude that he does not apply with the same gusto to science, as I will argue throughout this essay.

In addition, Ross practices a lot of what we might call "soft" replication—writing poems in the style of Shelley and plays in the style of the Early Moderns. He tries his hand at what he refers to as "painful" verse using Byronic templates and has a brief affair with blow-pipe chemistry, where he could be seen puffing out his cheeks before the flame "in emulation" of his uncle (Ross 1923: 33).[4] Replication for young Ross means emulation, imitation, but also inspiration and modification, more congruous with the Middle French root "repliquer," meaning to reply or respond (OED n.p). To replicate is, for Ross, to reproduce in modified form.

When applied to his own work, this philosophy is a positive, progressive methodology; however, when applied to others it is often more dubious. The "Romanticists" he argues are "only revenants of the Elizabethans"—a statement that seems to both praise and criticize the

replicative frameworks of literature and of literary style (Ross 1923: 31). For someone who invented a new diagnostic microscope, attempted to re-imagine phonetic spelling, and closely criticized what he considered to be Dickens's "failed" attempts at replicating the working-class dialect, replication is evidently a significant act. He seems, even after the disputes to which I will refer at length in this essay, to consider replication as a kind of necessary methodology, as the basis for improvement, and as the model by which progress is made. However, this methodology is also the basis for intense professional anxiety. In the rest of this chapter, I will explore the ways in which the sociopolitical pressures of parasitology contributed to anxiety about replication as a scientific and cultural methodology. Ultimately, I will conclude that replication was a highly politicized activity at the fin de siècle, which had the power to both confer and undermine professional authority.

In 1880 when Charles Laveran found the protozoan parasite responsible for malaria in the blood of a patient it seemed like a revolutionary moment. It was the first time that such an organism, which he characterized as an "animal" rather than "vegetable" parasite, had been shown to cause disease.[5] However, this revolutionary moment was slow to come. For more than ten years the causative agent of malaria was hotly disputed. Those who doubted the identity of the malaria parasite did so owing to the difficulty in replicating it visually. Surgeon Colonel Edwin Lawrie of the Indian Medical Service publicly denied the existence of the parasite, failing to find it under the microscope himself. He suggested, like many other microbiologists, that it was instead a degenerate red blood cell or a microscopy viewing error. Lawrie even referred to depictions of Laveran's bodies as "fanciful pictures [...] drawn from the imagination" (Lawrie 1896: 1135). This demonstrates the significance of visual authenticity for tropical pathologists, who often relied on the subjective gazes of their peers. The subjectivity of observation was particularly contentious in the case of malaria, which in just the preceding year had had another contender for its causation.

In 1879, Corrado Tommasi-Crudelli and Edwin Klebs had isolated a bacterium from the Pontine marshes. They claimed that when they injected this bacterium into rabbits it caused the cyclic fevers and enlargement of spleen indicative of malaria (Cox 2). They thus named the bacterium, *Bacillus malariae.* However, just as had been the case with previously suggested agents like Dr Salisbury's "ague plants" (see Anon 1867: 588-9), investigators raised questions concerning the authenticity of the correlation. Was the bacillus to be found in the soil of other tropical regions as well as in Italy? Was it to be found in *all* malarious locales? What differentiated this bacterium from the hundreds that dwelt alongside it in the marshes? (see for example, Russell 725-6). In 1881, Dr Sternberg replicated Tommasi-Crudelli and Klebs's experiments with rabbits only to find that their characteristic temperature spikes were obtainable by inducing a simple fear response. He also discovered that the anatomical changes that they had observed were identical with those caused by septicaemia (Anon 1881: 827). This suggested that Tommasi-Crudelli and Klebs's experimental methodologies were flawed. Dr Charles McMunn, however, countered Sternberg's experiments by presenting anecdotal evidence in support of the malaria bacillus. He argued that he had observed it in the blood of an African traveller, along with the patient himself and "a medical friend who happened to drop in soon after" (MacMunn 935).

Replication of the gaze is here offered as support for the authenticity of the observation. The reliability of this anecdotal evidence is clearly questionable, but it illustrates some of the methodological problems that researchers faced. Replication was prone to error because those who were doing the replicating were not all operating within the same parameters. At no point does McMunn explain how he ascertained the specific identity of the bacterium, nor does he describe it at all; however, his letter to the editors of the *British Medical Journal* was enough to perpetuate the debate. The reticence then of microbiologists to accept Laveran's proposal in light

of this developing trend for identifying and refuting new pathogenic organisms is understandable.

Laveran had observed three different forms of his organism in the blood of his patients: crescentic bodies, static pigmented bodies, and motile bodies that extruded flagella. He identified the crescentic bodies in 148 out of 200 malaria patients and never in a patient without the disease, strengthening his hypothesis (Cox 3). Unlike his rivals, Laveran had a relatively large sample size to back up his claims and he also noted that quinine, the popular treatment for malaria, removed the bodies from the blood. This was compelling evidence of its identity, and after some initial resistance, Laveran managed to convince influential microbiologists in France, Germany, Italy, and England that his organism was the causative agent of malaria. Thus, when Surgeon-Colonel Lawrie resurrected the debate in 1895, it was met with criticism from his peers. Tropical medicine heavyweight Patrick Manson was surprised at such an "extraordinary statement" published under Lawrie's name. "If Laveran's body is not a parasite," he asserted, "then it is the most wonderfully-contrived device in Nature for deceiving the pathologist" (Manson 1895: 394).

In a letter to *The Pioneer* in 1897, Lawrie again voiced his doubts as to the identity of the malaria parasite, highlighting the professional inconsistency regarding its specific nature:

> Having patiently plodded through the works of Laveran, Marchiafava and Bignami, and Mannerberg my doubts only increased, for while the above authorities agree that there is a malarial parasite, they differ on almost everything else about it, *viz.* its situation with regard to the red corpuscle, its morphology, whether there is only one parasite causing all forms of malarial fever, or as many different and distinct forms of parasite as there are types of fever, which are the active and which are merely the degenerate forms of the parasite, &c.[6]

Ross responded to what he called the "considerable amount of gossip" about the malaria parasite that Lawrie had been spreading in the papers. Framing his objections in relation to the integrity of the field at large, he quoted Patrick Manson in saying that Lawrie's views were "manifestly wrong, retrogressive in tendency and their publication in *The Times* [was] calculated to check the slowly spreading belief in a great pathological fact."[7] He further elaborated on the dangerous nature of Lawrie's objections:

> Such a denial after fifteen years of minute study of the parasite by the most eminent microscopists is so ridiculous that it might safely be disregarded, but for the fear that it will weigh with those who are not familiar with the subject, just at a time when it is highly desirable that further enquiry into the parasite needs must to be waged in India.[8]

The positions of Lawrie and Ross as physicians in the Indian Medical Service and their consequent physical location within that country was a key component of this exchange. Lawrie resented the system of mentorship that Ross was benefitting from—a system that enabled him to gain the support of the British medical presses through Manson's influential position in England. This is something that Douglas M. Haynes describes as the "dialectical relationship" between Britain and her colonies. As Haynes argues in relation to Manson's initial filarial work, "their very social, not to mention geographical, marginality within British society as imperial servants placed [imperial doctors] in a dependent position vis-à-vis metropolitan investigators" (Haynes 54). Even though much investigative work relied on imperial spaces, as well as their geographically specific parasites and vectors, scientific authority was still constructed in tandem with metropolitan institutions. Lawrie clearly objects to the political implications of such a system. Criticizing what he sees as the medical press's unilateral support, he laments:

> Men who write of Laveran's bodies as “pets” “beasts” “brutes” or (save the mark) “bugs” have been extolled in the British Medical Journal as scientists in terms of the highest praise, while senior men of the calibre of J. M. Cunningham, Bryden, Rice, Marston, D. D. Cunningham, Crombie, Sanders and a host of others whose names alone call up feelings of respect and admiration throughout the length and breadth of this land, have been as freely reprobated and lampooned (Lawrie 1896: 376).

These geopolitical tensions underpinned much of the dispute regarding the causative agent of malaria—a dispute that was reprinted to considerable readership in *The Times.* Having advised Lawrie to improve his microscopy technique, Ross added, “I have lately found the parasite in sixty-five cases of malarial fever, and I must beg leave to laugh at anyone who disbelieves in it just as I would laugh at anyone who disbelieves in the moon.”[9] Ross’s analogy exposes the emotional politics behind the replicative frameworks at play here. He elaborates:

> Any attempt to prove that the malaria parasite is not a degenerate red corpuscle is like an attempt to prove that the moon is not made of green cheese; most of us, I believe, are pretty confident that our satellite is not composed of that substance, but I fancy it would be somewhat difficult to prove the point, especially in the limits of a letter to the Daily Press, against a person obstinately possessed of the green cheese theory.[10]

Ross implies that the identity of the malaria parasite is something that most investigators accept and that proving it is an exercise in redundancy. His analogy, however, also illustrates the practical difficulties in demonstrating a connection between visual authenticity (the

morphological appearance of Laveran's body) and functional identity (the role that that body plays in disease causation).

Lawrie responded using the language of taxonomy to slander Ross's professional conduct: "this is a fine example of *Criticism rossii"* (Lawrie 1900: 547). Such a quip implies that Ross typifies the kind of investigator that Lawrie rallies against. In a further reference to the institutional biases of the British medical press, and to European investigators' supposed over-willingness to believe each other's theories, he signs off with a parody of Alfred Denis Godley's *Rubaiyyat of Moderations*:

'Tis all they need, they are content with these:
Not facts they want, but soft Hypotheses
Which none need take the Pains to verify:
E.g. the Theory of Anopheles (Lawrie 1900: 547).

He objects here, not just to the identity of the malaria parasite, but to the theory of transmission by a mosquito vector. He suggests that the "soft hypotheses" of European investigators, investigators whom he associates with institutional privilege, had not been verified by empirical study—that they were not able to be replicated. To his mind, the results lacked reproducibility.

As Laura Otis argues in her book, *Membranes,* microscopy was not an objective experience, and what scientists saw at the end of a microscope was influenced by their own preconceptions (1-9). Thus visual replication did not have the kind of legitimizing power that scientists liked to think it did. Ross himself had been a staunch disbeliever in the malaria parasite in the early 1890s. His change of allegiance by 1895 was a product of not just his improved microscopy training, but his newfound friendships with parasitologists Charles Laveran and Patrick Manson, and a willingness to "see" the parasite within new frames of reference. In this

way, the processes of replication that supposedly underscored good science were inseparable from the sociopolitical tensions of conferred authority. Throughout Ross's career he had many more disagreements with other investigators, and replication emerged as a phenomenon in these disputes that negotiated and encoded anxieties about authenticity, priority, credit, and professional integrity. These anxieties in turn were framed within wider discussions of national identity. Regenia Gagnier identifies the latter half of the nineteenth century as an epoch within which the relations of parts to wholes were being conceptualized. She talks about the "tension of independence versus interdependence, specifically of individual development threatening the very functioning of the whole" (3), a dynamic that might in the same way describe the tense relationship between British and European parasitologists in the context of emergent global modernity and cosmopolitanism.

At the end of the nineteenth century, parasitology was a fledgling discipline. Born during a period of increasing specialization in science, this important sub-field of tropical medicine was just gaining its professional identity. The Liverpool and London schools of Tropical Medicine were set up in 1898 and 1899 respectively as the first institutions in the world to offer specific research and training in tropical and parasitic diseases. Founded by merchant traders in the case of Liverpool, and with the support of Joseph Chamberlain (then secretary of state for the colonies) in the case of London, the schools were unavoidably in dialogue with the British imperial project. Consequently—as I've outlined elsewhere—British parasitology was entrenched in and defined by British national and imperial identity. Moreover, parasitologists embellished this connection as a way of investing their own professional identities with cultural authority. They used historic myths of nationhood—drawing particularly on Arthurian mythology—to steep their disciplinary discourse in the chivalry of Britain's imagined cultural homeland (Taylor-Brown 62-79).[11] What resulted, at least for some British parasitologists, was a

tension between the need for international collaboration and the desire for national credit. This is clear in the rhetoric used to position parasitology in relation to imperialism.

Speaking about parasitology in 1900, colonial administrator William MacGregor used the analogy of construction workers to give credit to an international field: “It appears to me to be more or less like this: Manson was the surveyor, Laveran made the road, Ross built the bridges and laid the rails, and Grassi, Bastianelli, Bignami, and Celli provided the rolling stock” (980). This metaphor allows MacGregor to implicate a variety of European investigators in the literal building of empire. However, in reality, the field was fraught with a sense of national competition, as is clear from Ross’s appraisal of it in 1905, when he asserts: “the new [tropical] knowledge is vastly more important to the British Empire than to any other State […] German medicine is far ahead of us; however Liverpool and London, not Berlin and Vienna, are world centres of the study of tropical disease.”[12] Here, he emphasizes British parasitology’s entanglement with imperial politics in order to position Britain within a competitive global market.

The extent to which tropical research was a national concern is also reflected in private research correspondence in which Manson urges Ross to be wary of international competitors: “The Frenchies and Italians will pooh pooh it at first, then adopt it, and then claim it as their own. See if they don't. But push on with it and don't let them forestall you” (Bynum and Overy 55). Letters from Manson’s friends congratulated Ross with the addendum: “you have done the trick and I congratulate you heartily and I congratulate ourselves for do you not belong to us? And you are no Italian, French or German but a plain Briton!”[13] The prevalence of foreign research in the news was clearly a sticking point for many; another friend writes, “you are the first in the field and it is refreshing to find it made by a scientist who is not a Frenchman or a German.”[14] Manson even warned Ross about advancements in foreign research on malaria, clearly making it about British priority, rather than about a solution to prevent human suffering:

> It is evident the Italians are now on the scent. I do hope you will run into the quarry before them. Bignami is a clever little fellow and ambitious. Laveran is working up the Frenchmen. I do not hear that the Germans are moving but they will and so will the Russians. Cut in first. (Bynum and Overy 125)

It is just this sense of national competition that underscores Ross's anxieties about replication.

After years of research and many failed experiments, Ross eventually traced the malaria parasite to the salivary glands of the mosquito. Having been moved around with the Indian Medical Service and finding it hard to procure cases of human malaria, he was forced, however, to turn his attention to birds. In 1897, using these models, he demonstrated the mosquito-malaria connection by successfully infesting birds with avian malaria via bites from infected mosquitoes. This proved the mechanism by which malaria was transmitted, completing the life cycle of the malaria parasite. However, he was shortly moved on again to study a kala-azar (now leishmaniasis) epidemic in Assam and was unable to definitively prove his theory in human beings. This task was taken up by some Italian investigators, who set about offering the final proof. This is also where the encoded anxieties concerning replication came to the fore. In Ross's initial statement regarding the mosquito-malaria discovery to the Secretary to the Director-General of the Indian Medical Service he gives due credit to all involved and demonstrates the legitimizing function of observation and repetition:

> My results have been accepted by Dr Laveran, the discoverer of the parasites of malaria; by Dr Manson who elaborated the mosquito theory of malaria; by Dr Nuttall of the Hygiene Institute of Berlin, who has made a special study of the relations between insects and disease; and, I understand, by M. Metchinkoff, director of the laboratory of the

> Pasteur Institute in Paris. Lately moreover, Dr C.W. Daniels of the Malaria Commission […] ; while lastly, Professor Grassi and Drs Bignami and Bastianelli of Rome have been able, after receiving specimens and copies of my reports from me, to repeat my experiments in detail, and to follow two of the parasites of human malaria through all their stages in a species of mosquito called the anopheles claviger.[15]

The links in the chain and their multiple contributions to the discovery here offer validation. However, later these same links, and his penultimate position among them, form a source of intense professional anxiety for Ross. Eager to get full credit for the Nobel Prize, which was awarded to him in 1902, Ross spent years battling with Italian investigator Giovanni Batistta Grassi over priority. His memoirs, which claim to provide a full account of the great malaria problem and its solution, dedicate a lot of time to the dispute. Using terms like "piracy" (*Memoirs* 403) and "robbery" (401), and phrases like "reconstruct the crime" (403), he differentiates what Grassi and colleagues were doing from the bona fide replicative frameworks needed for scientific progress. He writes, "all the work [the Italian investigators] did from beginning to end was suggested, assisted, and rendered possible by my previous work, my methods, my technique, my specimens, my descriptions, and my drawings" (403), and elsewhere: "their work has generally consisted merely of obvious verifications of the labours of others."[16]

This bespeaks an anxiety concerning attribution. At what point is a discovery made? Does its application to different environments or contexts constitute new ground? Certainly, Ross's own work was not independent of its previous inspirations. After all, it was the research carried out by Sir Patrick Manson in China that established that mosquitoes transmitted the filarial worm responsible for elephantiasis—a model used to reinforce the mosquito-malaria theory. This was itself an adage dating at least as far back as Aristotle. Ancient Roman writers

connected malaria with swamps, which they argued engendered noxious creatures (Gorgas and Garrison 133), and references to biting insects appear in Ancient Babylonian and Sanskrit texts on fever.[17] Giovanni Lancisi in 1717, Louis Daniel Beauperthuy in 1854, Albert King in 1883, and Charles Laveran and Robert Koch in 1884, had all offered support for the connection between malaria and mosquitoes (Gorgas and Garrison 133-4). What remained was not only to empirically prove this connection, but to elucidate the specific transmission route. Were mosquitoes responsible for transmitting the parasites from man to marsh or from marsh to man? William Gorgas and Fielding Garrison argued that Carl Gerhardt's demonstration that malaria can be transmitted directly via the blood in 1884 "abolished the Miasm Theory of malarial fever" (Gorgas and Garrison 138). However, the possibility of transmission via air and water droplets was still a working theory in the 1890s. Ross even argued that Manson's belief in 1895 that malaria might be transmitted via spores released from infected mosquitoes or by ingesting mosquito-contaminated water demonstrated that he was "still under the influence of the miasmatic theory" (Ross 1930: 39). Indeed, Ross himself did not distance his discovery from the previous associations of its namesake, rather he emphasized the continuity between these theories, concluding in information booklets, "malaria *is* due to a miasma given off by the marsh, but the miasma is not a gas or vapour—it is a living insect."[18]

Ross is clearly happy to acknowledge the theory's long history and to place himself within a series of successive paradigm shifts. However, when it comes to official recognition, he is not willing to share the limelight with Grassi. To deny Grassi's claim, he suggests that Grassi's experiment and data itself were not just inspired by his work, but a direct plagiarism of it. He insists: "many of the items in [Grassi's] *Studi* are directly pirated from my Sierra Leone results, and I recognize one of my Indian specimens in his plates. His figure of the attitudes of *Anopheles* and *Culex* is stolen straight from me" (Ross 1923: 408). For Ross, Grassi's experiments represent a "deliberate effort to pirate [his] work" (Ibid 408). He further

countenances this by drawing on the professional branding of parasitology to invest the incident with chivalric overtones. Accusing Grassi and his colleagues of dishonorable conduct, he writes:

> The thief must at least possess the virtue of energy, and the scientific thief, the virtue of scientific enthusiasm. Our Roman friends possessed both these virtues—so rare amongst the Sluggards, the Do-nothings, and the Think-nots! Great would have been their honour—if their honour had been greater (Ross 1923: 410).

This kind of rhetoric is recognizable in much of the private correspondence between Ross and other researchers, suggesting that it is an ideology that has been at least in part internalized. Indeed, Dr George Nuttall praised Ross in these terms in 1913, writing "you are the only man who shows the proper spirit of fair play. All the rest want to bag each other's game in a manner that disgusts me."[19] This sense of fair play supposedly underpins much of Ross's objections to what he sees as the bad behavior of other researchers. However, in defending his own research, he enacts the very censoring that he objects to by trivializing the inputs of the Italian investigators. He argues that his own work eclipses anything subsequent:

> [My] two observations solved the malaria problem. They did not complete the malaria story certainly; but they furnished the clue […] The great difficulty was really overcome; and all the multitude of important results which have since been obtained were obtained solely by the easy task of following this clue—a task for children. (Ross 1905a: 551)

Elsewhere he rejects the replicative frameworks of science altogether by claiming that "the discovery of malaria being carried by mosquitoes was made not by observation, but by a process of induction."[20] By this he means to discount the significance of Grassi's replicative

experiment in which he observed the same results as Ross, but in human subjects. In 1898, Nuttall had written to Ross to say that he had read some of Grassi's papers in which he and Amico Bignami "confirm your observations and in one of them try to bag some of the credit from you."[21] This comment highlights the ambivalent position of replication in professional discourse. In this sentence, we can read the anxieties that would underpin Ross and Grassi's later dispute. How do we approach, and what do we do with, such processes of confirmation?

Experimental replication is necessary to ascertain the truth of the discovery; however, for Ross, "repliquer"—the process of replying—is a politicized activity. Ross's use of the legal lexis of "larceny" and "piracy" reveals much about his understanding of replication as a methodology that is in dialogue with issues of copyright and intellectual property.

So famous was Ross for his priority disputes, first with Grassi and later with Manson, that in 1914 parasitologist Aldo Castellani wrote to him requesting his advice over his own dispute regarding the discovery of *Trypanosoma* as the causative agent of sleeping sickness. Appealing to Ross's authority on the subject, Castellani lamented published correspondence put forward by Sir Ray Lankester, in which Lankester, according to Castellani, "misquotes, distorts, and purposely suppresses portions of the reports when quoting them, for the unscrupulous purposes of personal spite."[22] In 1902, George Carmichael Low, Aldo Castellani, and Cuthbert Christie had travelled to Uganda to investigate sleeping sickness. Initially Castellani and Low thought that a strain of streptococcus bacteria was responsible for the disease; however, after finding trypanosomes in the blood and cerebral-spinal fluid of 70% of cases, Castellani wondered about their involvement. In written correspondence with Ross he wrote "the fact of finding tryp. in the cerebro-sp. fluid is I think too suggestive and means much more than simple coincidence." David Bruce, who joined the commission in 1903, subsequently investigated this connection—efforts that led to its confirmation. Although in his commission report, Bruce admits that without Castellani's initial observation, they "might have worked for months in the

dark" (in Boyd 102-3), he argued that Castellani did not appreciate the significance of it and so should not get credit.

The dispute was taken up by the medical and lay press, with Lankester and Manson, among others, on the one side, and Albert Chambers, David Nabarro, and Ross on the other.[23] Nabarro, who was himself involved in the sleeping sickness commission, resented Lankester's attempt to minimize Castellani's role in the discovery. He assured readers of the *British Medical Journal* and *The Times* that Castellani should be awarded the credit of "having first observed the trypanosome in the cerebrospinal fluid of sleeping sickness patients*;* of having first connected it with the etiology of sleeping sickness, and of having first published it." To himself and David Bruce he gives credit for having "very greatly enlarged the researches on the trypanosome and of having first discovered that the parasite is carried by a tsetse fly" (Nabarro 374-5). Castellani insisted that Lankester abused his position as a member of the Royal Society to appear as their mouthpiece in his letters to the press. He also complained that Bruce was involved in the investigation into his own dispute and even served as a signatory on the committee's final decision—an incredible oversight of impartiality if true.[24] Clearly the history of the discovery of the causative agent of sleeping sickness is complex and not easily narrativized. Indeed, Isabel Amaral argues that Portuguese investigators too laid claim to the elucidation of sleeping sickness (n.p). However, what these disputes demonstrate is that the process of replication, although empirically necessary, brings with it socio-political baggage that is inflected by the investigators' relationships with other researchers.

As a methodological tool, replication helped to confirm *Plasmodium* and *Trypanosoma* as parasites in their respective diseases, putting to bed many alternative hypotheses. However, the reverberations of the controversies produced by such a methodology were felt for many years to come. In 1923, Ross was still bitter about the Italian claim to the mosquito-malaria discovery. In writing, he laments "they have attempted the same thing with Koch. They have forgotten King.

They have traduced Manson's theory. They have not given MacCallum's discovery sufficient prominence […] they do not mention Daniels […] they ignore Sakharoff" (Ross 1923: 18). This incomplete, unauthentic, or disingenuous replication is, for Ross and many others, an endemic problem. Priority disputes arose from almost every major discovery during this period—a phenomenon that is recorded in the *British Medical Journal,* in the lay press, in private correspondence, and in publications like *Science Progress,* which Ross edited.[25] Thus replication for parasitologists at the fin de siècle was a fraught concept—fraught chiefly through its entrenchment in the methodologies of a newly emergent specialism at a time when foundational discoveries were yearly being made. A case study of Ross reveals that the process of replication was a highly politicized activity within the network of its specialists that had as much to do with the intricacies of narrative framing as it did with the competitive practicalities of verifiable scientific discovery.

[1] London, London School of Hygiene and Tropical Medicine. [hereafter LSHTM] The Ross Collection. [hereafter RC]. Ross/28. Fever casebook, MS of speech note on back cover.

[2] London, LSHTM. RC. Ross/152/14-17. Letter to Sir Richard Gregory, Editor of Nature, dated Feb 29, 1924.

[3] His father, General Campbell Claye Grant Ross, painted the Indian landscape in watercolor "incessantly"—a morning ritual which sits vividly in young Ross's memory. Ronald, who learnt painting and music from his father, was bracketed first in England for drawing at the Oxford and Cambridge Local Examination in 1878.

[4] Ross's uncle, Lieut.-Colonel William Alexander Ross, a chemist, wrote several textbooks including *Pyrology, Or Fire Chemistry* (1875), *Alphabetical Manual of Blowpipe Analysis* (1880), and *The Blowpipe in Chemistry, Minerology, and Geology* (1884). Young Ross admired his uncle with an intensity that he describes as "worship" (*Memoirs* 6). In his memoirs, Ross recalls being mistaken for his uncle at the Congress of Arts and Sciences in St Louis in 1904 by a "distinguished American chemist," who admired his uncle's books and his invention of the Aluminium-Plate Support.

[5] Protozoan parasites were often called "animal" parasites, while bacteria were known as "vegetable" parasites. This rhetoric had an impact on the consequent characterization of these organisms with parasitologists like Ross using their apparent "animality" to emphasize the difference between parasites and bacteria.

[6] London, LSHTM. RC. Ross/39/06. Clipping: *The Pioneer,* April 25, 1897: 7.

[7] London, LSHTM. RC. Ross/39/03. Clipping: "The Existence of the Malaria Parasite," *The Pioneer,* Sept 28, 1897, n.p.

[8] Ibid. n.p.

[9] London, LSHTM. RC. Ross/39/03. Clipping: "Existence of the Malaria Parasite," *The Pioneer,* Sept 28, 1897, n.p.

[10] Ibid. n.p.

[11] In his obituary for fellow parasitologist, Joseph Everett Dutton, published in the *British Medical Journal* in 1902, Ross refers to the late researcher as a "Galahad" and a "true Knight of Science." This chivalric rhetoric is echoed in personal and professional correspondence, in speeches, and in newspaper articles, in which tropical medical research becomes a "quest" and its proponents, knight-adventurers. I argue that this mythic imagery formed a "branding" campaign in the late nineteenth and early twentieth centuries that functioned to lionize parasitologists and their profession. For more on the development of the quest narrative in relation to British parasitology, see Taylor-Brown (2014).

[12] London, LSHTM. RC. Ross/113/23/24 Clipping: "Medicine and the Empire" from *The Outlook,* Jan 28, 1905.

[13] London, LSHTM. RC. Ross/48/36. Letter dated Sept 31, 1898.

[14] London, LSHTM. RC. Ross/29/84/01. Letter from E. Harold Brown.

[15] London, LSHTM. RC. Ross/29/112/01. Letter to Secretary to the Director General of the Indian Medical Service, Simla dated Feb 16, 1899.

[16] London, LSHTM. RC. Ross/57/06. MS: "Italian Dishonesty in Science."

[17] See: London, LSHTM. RC. Ross/103/03/12. Clipping: "Mosquitoes and Malarial Fever" *Agricultural News* (1908): 13.

[18] London, LSHTM. RC. Ross/105/06/50. "The Practice of Malaria Prevention by Ronald Ross, Major I.M.S. Ret. Professor of Tropical Medicine, University of Liverpool," p. 3 [emphasis his own].

[19] London, LSHTM. RC. Ross/6/05. Letter from George Nuttall dated April 13, 1899.

[20] London, LSHTM. RC. Ross/102/53/25. Clipping: "Malaria and Mosquitoes," *Dublin Express,* June 27, 1905.

[21] London, LSHTM. RC. Ross/6/05. Letter from George Nuttall dated Dec 21, 1898.

[22] London, LSHTM. RC. Ross/141/01. Letter from Castellani dated Jan 15, 1903.

[23] The dispute was covered in the *British Medical Journal, The Lancet,* the *Journal of Tropical Medicine and Hygiene,* as well as *The Times.*

[24] London, LSHTM. RC. Ross/141/01. Letter from Castellani dated Jan 15, 1914.

[25] See, for example: Robert Boyce, Ronald Ross, and Charles S. Sherrington, "Note on the Discovery of the Human Trypanosome" *British Medical Journal* 2.2186 (1902): 1680. See also: London, LSHTM. RC. Ross/77/07. Letter from George Nuttall dated Jan 24, 1903, in which he laments "being put in a false position" because some are claiming he should have full credit for research on blood and immunity. Ross also recounts Albert Grünbaum (later Leyton)'s claim on an agglutination test for diagnosing enteric fever (typhoid), which was eventally named after Fernand Widal. See: Ronald Ross "Albert E. F. Leyton" *Science Progress,* 16.63 (1922): 441-3. In the same issue, there is a note on Eli Metchnikoff's biography, written by his wife Olga, who argues that Metchnikoff discovered the alternation of generations in certain parasitic nematodes before Rudolf Leuckart, who "stole the credit" (p. 446).

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