

Clifford Dobell and the Making of Paul de Kruif's *Microbe Hunters*

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While researching the activities of Paul de Kruif, an American microbiologist and medical science journalist,¹ I came across some previously unknown correspondence of interest for the history of malaria research and of the history of medicine generally. It sheds new light on the origin of De Kruif's book *Microbe hunters* and more particularly on his description of the discovery of the transmission of malaria parasites by mosquitoes. The letters were written by Clifford Dobell, a British protozoologist, to his friend Paul de Kruif.² They triggered a little-known sequel to the priority battle between the two discoverers of the transmission of malaria parasites, Ronald Ross and Giovanni Battista Grassi.

Colleagues and Friends

Before earning his living from writing, Paul de Kruif (1890–1971) had carried out front-line bacteriological research at Michigan University, as a US medical corps officer during the First World War in France, and thereafter at the Rockefeller Institute, New York. He chose, however, to become a writer and, specifically, a science journalist. One of the reasons was his dissatisfaction with the unscientific and arrogant attitude of the American medical profession in general. It resulted in a rather critical book *Our medicine men* (1922) that cost him his position as a researcher. The next episode in his career was the collaboration with Sinclair Lewis on *Arrowsmith* (1925), a medical novel. In their hunt for peculiar medical characters for that book, Lewis and De Kruif visited London in 1923. They met Clifford Dobell, a protozoologist of whose scientific publications De Kruif was well aware.³ In his autobiography, De Kruif described the personal encounter as embarrassing. “Dobell was an acid-tongued detester of all things American, especially noisy Americans . . . Mr. Dobell—he scorned a doctorate—[was] a mixture of objectivity and prejudice”.⁴ Dobell's trenchant, sardonic and non-diplomatic language must have appealed to De Kruif, who also used a rather plain and coarse way of expression and writing.

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¹ J P Verhave, 'Paul de Kruif, medical conscience of America', in Robert P Swierenga, Jacob E Nyenhuis, and Nella Kennedy (eds),

Dutch-American arts and letters in historical perspective: Proceedings of the 16th Biennial Conference of the Association for the Advancement of Dutch-American Studies, Holland, MI, Van Raalte Press, 2008.

² Four Dobell letters to De Kruif 1923–1926 are held in the De Kruif dossier, Holland Museum Archives, Holland, Michigan.

³ Paul H de Kruif, 'Mutation of the bacillus of rabbit septicemia', *J. Exp. Med.*, 1922, **35**: 561–74.

⁴ Paul de Kruif, *The sweeping wind: a memoir*, New York, Harcourt, Brace & World 1962, pp. 94–5.

Clifford Dobell (1886–1949), “a lean, wiry, blond-haired, enormously ascetic Englishman”,⁵ was a highly respected researcher working at the National Institute of Medical Research, Hampstead, and since 1918 a Fellow of the Royal Society. He had written *The amoebae living in man* (1919) and *The intestinal protozoa of man* (1921). Dobell was also fascinated by the discoveries of the seventeenth-century Dutchman Antony van Leeuwenhoek, who had described various “little animals” with the help of his self-designed microscopes. The two microbiologists shared an interest in medical history and thus the idea emerged of teaching the public at large about the great discoveries that would allow for the fight against infectious diseases. Dobell put his notes on van Leeuwenhoek at De Kruif’s disposition for the writing of an essay that would become the first chapter of *Microbe hunters*.⁶

In reply to a casual question from Sinclair Lewis: “Wasn’t Sir Ronald Ross’s discovery of the mosquito transmission of malaria remarkable?”, Dobell icily said: “Ross did not discover that. The Italian, Grassi, did.” Ross had, in fact, just published his *Memoirs*.⁷ Convinced as he was of the honesty of British researchers, De Kruif’s interest was kindled and now he smelled something rotten. Dobell knew both Ross and Grassi, and lifted a veil from a new aspect of the story of the transmission of malaria.

Transmission

This story is now well known. It was Ronald Ross (1857–1932), who, after several years of dedicated research in British India under the mentorship of Patrick Manson, had found the clue: having allowed female mosquitoes to take blood from a carrier of malaria parasites, he found a new stage of the parasite in their abdomen. Unfortunately, after the publication of his discovery in 1897, Ross had no patients to give the final proof. Thus, he used birds and certain other mosquitoes as a model and showed how the parasites developed on the midgut wall and accumulated in the salivary glands, and that at that stage the mosquitoes could infect other birds. The discovery was presented in 1898 as a great triumph for Britain. Immediately after that, however, an Italian researcher, Giovanni Battista Grassi (1854–1925) demonstrated that human malaria could be transmitted only by mosquitoes of one particular genus, *Anopheles*; and Grassi claimed the final proof. It caused a bitter controversy that was further intensified by the fact that Ross alone was awarded the Nobel Prize in 1902. The conflict haunted both men until their deaths.

The Prompter

The comments of Dobell intrigued De Kruif, but he had to rush back to the United States, and the two men kept in contact by letter. De Kruif had only the American handbook on medical history by Fielding Garrison at hand, which did not deal with the details of the discoveries.⁸ He decided to go for the challenge and write a chapter on the transmission of malaria for his book. The story and the quarrel had hardly ever before been recounted by

⁵ Paul de Kruif, ‘I collect people’, *Ladies’ Home Journal*, Oct. 1941, pp. 129–33.

⁶ Clifford Dobell published his research in 1932: *Antony van Leeuwenhoek and his ‘little animals’*, London, J Bale, and New York, Harcourt, Brace, 1932.

⁷ Sir Ronald Ross, *Memoirs: with a full account of the great malaria problem and its solution*, London, John Murray, 1923.

⁸ Fielding H Garrison, *An introduction to the history of medicine*, 3rd ed., Philadelphia, W B Saunders, 1921.

a neutral outsider. When De Kruif was back in the States, the controversy between Ross and Grassi flared up. The following part of Dobell's letter to De Kruif, dated 16 April 1925, must be read against the background that Dobell was helping the old man Grassi to publish a defence. Ross had been inaccurate in his *Memoirs* and used offensive terms like "plagiarism" and "fraud" in his chapter 'Roman brigandage'. It presented Grassi with a challenge, and he fired back in *Nature*, pointing to gross mistakes and omissions. He even threatened Ross with a libel action. Dobell provided the translations in English and Grassi found a dedicated supporter in his British colleague.⁹ Dobell then tried to persuade De Kruif to come back soon to London because:

In one evening I could tell you more about Ross, Manson, Grassi, and everything else, than you could dig out for yourself in one year in N.Y., U.S.A. And I could tell it to you accurately and fully, from my own completely biased standpoint—so that you would only have to go home and type it all out, ready for press. Well, if you won't come, I must send you some references. You must read:

1. "Memoirs" by Ronald Ross (London. John Murray. 1923) (Largely lies.)
2. A letter—very moderate, for I retranslated and castigated it myself—in "Nature", last year.
3. A short paper by Grassi in "Parasitology" Vol. XVI, No. 4, p. 355 December, 1924 (Translated by me.)

... No. 3 is correct in all details, and is a moderate (and true) statement of the case. If you want to know more, you must learn Italian, and must read the original documents. This is a most complicated story, but the facts—very briefly—are as follows:

- (1) Ross, instigated by Manson in England, and helped at every time by him, tried to discover how malaria is transmitted from man to man in India: and failed utterly.
- (2) Ross then,—again egged on by Manson—tried to discover the mode of transmission of bird-malaria: and succeeded.
- (3) Ross and Manson were unable to apply the knowledge gained from birds to the study of human malaria—because they knew next to nothing about zoology. Consequently, they couldn't do anything more—only guess (and guess wrong).
- (4) Grassi and his collaborators in Italy had meantime got on the track of the true story regarding human malaria. They had already made some progress, when they heard of Ross's results with birds: and as Grassi was a good zoologist, he at once saw their significance, and went ahead and solved the problem.

Ross now claims that his work on birds solved the problem of human malaria. But it didn't, because even after he had finished the work on bird-malaria, he was hopelessly in the dark himself regarding the transmission of human malaria. So was Manson—whose ideas throughout were nearly all wrong. It was Grassi who discovered that malaria in man is transmitted by Anopheles, and who worked out the complete development of the human parasite in this mosquito.

Ross is a very dirty dog. He told — that he "intended to live on the discoveries for the rest of his life." He has done so. Grassi is dirty, but not a dirty dog. He is a great zoologist, but savage or almost rabid when roused. I believe he is honest—after spending a long time corresponding with him and testing his statements in every way I can. He has always played the game with me, and I admire him as a worker, though not as an individual human being.¹⁰

⁹ Battista Grassi, 'The transmission of human malaria', *Nature*, 1 March 1924, **113**: 304–6; *idem*, *Nature*, 29 March 1924, **113**: 458.

¹⁰ The correspondence between Dobell and Grassi is kept in the Wellcome Library, London, Archives and Manuscripts, MSS 6041-5.

Anyway, you can't possibly understand this problem unless you read all the literature—including that in Italian, which is most important. Ross . . . knows he will be found out when he is dead, but he means to have a good time while he is alive.¹¹

It is obvious that Dobell, as he himself admitted, was very biased. In the middle of the flare-up of the quarrel with Ross, and during De Kruif's writing process, Battista Grassi died on 4 May 1925. Dobell wrote a laudatory obituary in *Nature*, in which he stated: "[He] succeeded in 1898 and 1899 . . . in solving once for all the problem of the mode of transmission of human malaria. . . . and he worked out, for the first time, the entire life-history of the human malarial parasites in these insects [i.e. *Anopheles*]". Dobell refrained from mentioning Ross and the priority quarrel.¹²

The Chapter

Meanwhile, De Kruif did thorough and careful research for his chapter on Ross versus Grassi. He read all the literature Dobell had suggested, and he even sought out a copy of Grassi's *Documenti* in Rome and had it translated into English at his own expense.¹³ De Kruif tried to be more neutral in describing the quarrel. Yet, the outspoken views of his friend were insidious, and made him portray Grassi in a more sympathetic light than Ross could stand:

. . . unhappily for the Dignity of Science . . . Battista Grassi and Ronald Ross were in each other's hair on the question of who did how much. It was deplorable. To listen to these two, you would think each would rather this noble discovery had remained buried, than have the other get a mite of credit for it. Indeed, the only consolation to be got from this scientific brawl—aside from the saving of human lives—is the knowledge that microbe hunters are men like the rest of us . . . Like two quarrelsome small boys they sat there.¹⁴

After reading *Microbe hunters*, Dobell wrote to De Kruif on 25 July 1926: "[I]ts fineness consists in bringing home the history of microbes to the common man (including, of course, the average 'microbiologist'). In other words . . . it is an extraordinary achievement in popularization." Apparently, De Kruif had protested, stating that it was much more than that, because Dobell continued:

Do you think that you have written a serious scientific history of the subject? . . . You wrote it, in the first place, to make money . . . In the second place, you wrote the book in order to teach the ignorant; and you put its truths in vulgar (and therefore often slightly inaccurate) words, so that vulgar minds could grasp them. In this you have succeeded.

¹¹ Letter from Dobell to De Kruif, London, 16 April 1925, de Kruif dossier, Holland Museum Archives.

¹² Clifford Dobell, 'Obituary: Prof. B. Grassi', *Nature*, 1925, **116**: 105–6, on p. 106.

¹³ The first professional translation of Grassi's *Documenti riguardanti la storia della scoperta del modo di trasmissione della malaria umana*, Milan, A Rancati, 1903, in the De Kruif dossier, Holland Museum Archives, Holland, Michigan: 'Documentary

evidences on how the way of transmission of the human malaria was discovered'. Dedicated to Patrick Manson (Ross was not amused by this); see P Manson, 'Professor Grassi's recent pamphlet', *Lancet*, 1903, **i**: 923; W F Bynum and Caroline Overy (eds), *The beast in the mosquito: the correspondence of Ronald Ross and Patrick Manson*, Amsterdam and Atlanta, Rodopi, 1998, letters 230–234.

¹⁴ Paul de Kruif, *Microbe hunters*, New York, Blue Ribbon Books, 1926, p. 279.

I enjoyed reading the book, anyway, and I learnt much from it. But regarding the parts dealing with subjects about which I really do know something, I frequently find myself disagreeing with your interpretation. I don't agree with all you say about Ross, for example, and I know Ross better than you do,—and far more about his work. You, it seems to me, have taken his 'Memoirs' too seriously. Many statements in them are downright lies.

From the above it seems that De Kruif had tried to be less biased than Dobell, giving Ross a fairer role. But with regard to the priority battle, De Kruif gave it only a few lines. He depicted both researchers with their characters and backgrounds, their scientific abilities and research approaches. Some of these sketches are recognizable in the items that Bernardino Fantini formulated in an epistemological analysis of the discovery: their differences in the concept of specificity, the comparative method, analogical reasoning and intuition versus systematic inquiry.¹⁵

Ross's *Memoirs* and Grassi's *Documenti* were De Kruif's guides, and the quotations of the correspondence with Manson, quoted therein. Even compared with the now easily accessible full correspondence,¹⁶ the events of Ross's activities are described rather well. In page after page, chapter 10 parallels the description of the drama by Philip Manson-Bahr.¹⁷ But De Kruif added his personal impressions, for instance in his description of Ross: "He was like any tyro searcher—only his innate hastiness made him worse—and he was constantly making momentous discoveries that turned out not to be discoveries at all." "He tried everything. He was illogical. He was anti-scientific." "He did marvelous things in spite of himself."¹⁸

Touchy Ross

To the subject himself, these judgments, set in expressive American slang, were like a red rag to a bull. Ross was outraged (as always when it came to Grassi) and he wrote an article, entitled 'Simian journalism', protesting against "one Paul de Kruif, whose name is entirely unknown to me". He felt personally offended by the portrayal of him in the chapter 'Ross vs. Grassi', but he was especially stung by the favourable light in which Grassi was set. "The further adventures of Grassi, as related by Mr. Paul de Kruif, remind one of the equally surprising adventures of Sindbad the Sailor or of the late Baron Munchhausen."¹⁹ There is not a single word of appreciation about the way this unknown De Kruif had used Ross's writings.

When the book was to appear in Great Britain, Ross joined some other offended colleagues in a disclaimer.²⁰ They threatened to bring a libel action and the London publisher, Jonathan Cape, was forced to leave out the chapter.²¹ De Kruif agreed, provided a statement

¹⁵ Bernardino Fantini, 'The concept of specificity and the Italian contribution to the discovery of the malaria transmission cycle', *Parassitologia*, 1999, **41**: 39–47.

¹⁶ Bynum and Overy (eds), op. cit., note 13 above, letters 230–234.

¹⁷ Philip Manson-Bahr, 'The story of malaria: the drama and actors', *Int. Rev. Trop. Med.*, 1963, **2**: 329–90.

¹⁸ De Kruif, op. cit., note 14 above, pp. 286, 287, 298.

¹⁹ Ronald Ross, 'Simian journalism', *J. Trop. Med. Hyg.*, 1926, **29**: 335–7, p. 337.

²⁰ A Castellani, G C Low, D Nabarro and R Ross, 'Disclaimer', *Br. Med. J.*, 2 Oct. 1926, **ii**: 617; *idem*, 'A public denial', *Lancet*, 1926, **ii**, 729; *JAMA*, 1926, **87**: 1321.

²¹ Also chapter 9 on the discovery of trypanosomes as the agents of sleeping sickness was left out, due to protests of the above four (note 20), plus C Christy.

be included in the British edition that this had been done at Ross's request. Of course, Ross flatly refused. De Kruif wrote a response to the medical journals that had published the disclaimer, stating: "It is not my intention to stir up again the notorious quarrel that raged in the early 1900's between Ross and Grassi". He revealed his sources in a restrained, but decisive manner. But first he sent a draft to Alf Harcourt, his publisher. The proposed rigorous improvements came too late, but the letter was published anyway in the *Journal of the American Medical Association (JAMA)*, except for De Kruif's last lines, which the editor left out:

Is "Microbe Hunters" a true or a fanciful work? That is to be determined not by the perusal of venomous and slanderous letters, but rather by the thorough study of the above cited sources and by careful investigation of all other original studies bearing upon the work.²²

JAMA's editor forwarded the letter to Ross, but he did not react. The editor then could not resist quoting from the 'Ross vs. Grassi' chapter: "He would have Grassi say 'The facts of science are greater than the little men who find those facts'". Then referring to the published disclaimer of Ross and others, the comment concluded: "The history of medicine is as romantic a story as the novelists might tell—but if it purports to be history it should not be too romantic."²³ For De Kruif that was the end of the affair and he enjoyed the success of the book. In his autobiography, *The sweeping wind* (1962) he hardly touched on Ross.

The American parasitologist Eli Chernin described the affair between Ross and De Kruif in some detail and with a clear bias against the latter.²⁴ What is lacking in his publication is the role of Dobell as a prompter of De Kruif. He could have guessed, because he listed De Kruif's autobiography among his references, in which Dobell's view on the priority matter is mentioned. There were also Dobell's published translations of Grassi's work, and the obituary that could have given him a hint. Chernin either overlooked or failed to check the Wellcome Library, London, where the Dobell–Grassi correspondence is kept, though he did trace Jonathan Cape's archives at the University of Reading. In the correspondence with Dobell, Grassi gave his views on many events and persons involved in the discovery of the transmission of malaria. An annotated edition of these letters that sheds new light on the sequel of the priority question is in preparation.

Lasting Friendship

Dobell does not figure in the biography of Ross.²⁵ Apparently, and not surprisingly, the two never had a close encounter. Moreover, Ross had the reputation of being quick to take offence and to threaten libel actions, as other British malaria experts, like Sir Patrick Manson and Colonel Sidney P James, had experienced.²⁶

²² Letter from De Kruif to the editor of *JAMA*, 15 Oct. 1926, De Kruif dossier, Holland Museum Archives. *Idem*, *JAMA*, 1927, **88**: 1097–98.

²³ Current comment, 'The facts of science', *JAMA*, 1926, **87**: 1307–8.

²⁴ Eli Chernin, 'Paul de Kruif's Microbe Hunters and an outraged Ronald Ross', *Rev. Infect. Dis.*, 1988, **10**: 661–7.

²⁵ Edwin R Nye and Mary E Gibson, *Ronald Ross, malariologist and polymath: a biography*, London, Macmillan, and New York, St Martin's Press, 1997.

²⁶ Eli Chernin, 'Sir Ronald Ross vs. Sir Patrick Manson: a matter of libel', *J. Hist. Med. Allied Sci.*, 1988, **43**: 262–74.

What happened with the friendship between Dobell and De Kruif? The follow-up of the challenging correspondence (“You would be an angel, if you were not a devil; but whichever you be, you will still be my beloved Brother”²⁷) may have continued, but has not been kept. At any rate, they met again on the second trip of De Kruif to Europe in 1930 and dined together every night he was in London.²⁸ In 1932, when Dobell published his book on Antony van Leeuwenhoek on the three hundredth anniversary of his birth, the dedication in Latin reads: “This work of a dead Dutchman the English editor . . . (as one animalcule to another animalcule) gives, devotes & dedicates it to his . . . dear, bastard little brother, Paul de Kruif, an American.”²⁹ At De Kruif’s request the American publisher Harcourt sent advance copies to several influential friends, among them Ludvig Hektoen, the respected pathologist and microbiologist at Chicago University, and Raymond Pearl, the famous biologist at Johns Hopkins University. To the latter De Kruif wrote: “I worked like a dog all last week trying to get the Dobell started on the right foot . . . Please, give this all the breaks you possibly can. It is a marvellous book. Do your goddamnedest! [*sic*]” Also Morris Fishbein, the editor of *JAMA*, got a copy directly from De Kruif for reviewing.³⁰ Once published, De Kruif sent the reviews to Dobell. In 1936 Dobell enthusiastically reviewed De Kruif’s book *Why keep them alive?* for *Nature* and emphasized the role of Paul’s wife Rhea in the making of that book.³¹ It may thus be inferred that Dobell and De Kruif kept sympathetic feelings for each other. This neutralizes the information of the historian Brian Ford, who learned from Dobell’s widow Monica that, when De Kruif came to Hampstead with his new book *Microbe hunters*, Dobell threw him out of the house, believing the book to be crude, simplistic and vulgarized. Monica Baker married Dobell only in 1937 and never met De Kruif. She must have garbled the stories of her husband. Ford rightly analyses the relationship of the two scientists as: “Dobell’s forthright crisp directness did not fit easily with De Kruif’s informality and colloquial discursiveness”,³² but they certainly did not part in an atmosphere of recrimination, either in 1923, 1926, or 1930.

In Summary

The letters from Dobell to De Kruif that have surfaced reflect a very critical attitude towards Ronald Ross. Dobell must have refrained from public debate about the priority question because he knew how touchy Ross was. Not only did Dobell dislike Ross as a person, but he also chose the side of Grassi in the priority debate that had flared up in the scientific press in 1924. In this choice he was very biased and hardly recognized the merits of Ross.

²⁷ Letter from Dobell to De Kruif, 16 April 1925, Holland Museum Archives.

²⁸ De Kruif, *op. cit.*, note 4 above, p. 160.

²⁹ Dobell, *op. cit.*, note 6 above. De Kruif was one of two friends to whom the book was dedicated: “Fratriculo aequo caro nothoque Paul de Kruif Americano”.

³⁰ Correspondence between De Kruif and Raymond Pearl, and De Kruif and Morris Fishbein. Raymond Pearl Papers, American Philosophical Society Library, Philadelphia, series 1, box 4; and

Morris Fishbein Papers, University of Chicago Library, Special Collections Research Center, box 2, folder 1.

³¹ Clifford Dobell, ‘A text-book of unapplied biology’, *Nature*, 1936, **138**: 523–5, on p. 525. The British publisher was Jonathan Cape.

³² Brian J Ford, ‘The Leeuwenhoekiana of Clifford Dobel (1886–1949)’, *Notes Rec. R. Soc.*, 1986, **41**: 95–105. Most of Dobell’s other paper legacy was destroyed (personal communication from Professor Brian Ford, Jan. 2009).

Twenty-five years after the beginning of the quarrel between Ross and Grassi, no historian had had the guts to comment on it, probably because both contenders were still alive. De Kruif did so and he was not a professional historian. He followed Dobell's indication for the sources and thus did the dirty work, with, one may say, verve. The story in *Microbe hunters* made a public of millions aware of the originality and the pettiness of these researchers. Only for British readers did it remain veiled for another twenty-five years.

The commentaries were highly diverse when the book appeared. It was a new type of instructive entertainment for the reading public in America, and all over the world.

De Kruif recounted the ways in which these two men and ten other medical researchers and doctors had done their major experiments; it showed their endurance and sacrifices, as well as their breakthroughs and small-mindedness. Despite a thorough study of his sources, De Kruif's aim was not to contribute to academic history writing. He put words in the mouths of certain of his microbe hunters that they never spoke. For lay readers this made his personalities real and the stories fascinating, but for more knowledgeable readers this was intolerable, even though most of them were amateur historians themselves and organized medical history had only just started.³³ The criticism of one of them, Eli Chernin, is scathing,³⁴ but virtually none of the other more recent historians, writing on the discovery of the transmission of malaria and its aftermath refer to the 'Ross vs. Grassi' chapter.³⁵ Dobell sharply analysed this point: "Would you rather it were a Best-Seller now, or that some obscure unborn student of the subject—a century hence—should consult it for hard historic facts?"³⁶

Microbe hunters became an overwhelming success, and it presented thousands of young people with the choice of studying biology or medicine, including several future Nobel laureates. Though the book is still in print, the flamboyant and romantic style of writing may appeal somewhat less to today's generations. For Paul de Kruif it became the basis for his vast oeuvre on health matters.³⁷

³³ The *Annals of Medical History* was established in 1917, and the American Association for the History of Medicine was founded in 1924.

³⁴ Chernin, op. cit., note 24 above.

³⁵ Paul F Russell, *Man's mastery of malaria*, Oxford University Press, 1955, Manson-Bahr, op. cit., note 17 above; Gordon Harrison, *Mosquitoes, malaria and man: a history of the hostilities since 1880*, London, Murray, 1978; Nye and Gibson,

op. cit., note 25 above; Bynum and Overy (eds),

op. cit., note 14 above. There is one exception: R Ottaviani, D Vanni and P Vanni, 'The centenary of G. B. Grassi's discovery', *Int. J. Anthropol.*, 1998, **13**: 177–9.

³⁶ Letter from Dobell to De Kruif, 16 April 1925, Holland Museum Archives.

³⁷ William C Summers, 'Microbe hunters revisited', *Int. Microbiol.*, 1998, **1**: 65–8.