

Health, colonialism, and development: an interview with historian Randall Packard

Saúde, colonialismo e desenvolvimento: uma entrevista com o historiador Randall Packard

Interview with/Entrevista com
Randall M. Packard

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Abstract

Interview with Randall Packard, William H. Welch Professor of the
History of Medicine at The Johns Hopkins University and co-editor of
the *Bulletin of the History of Medicine*. Speaking about his academic
career, his activities as an editor, and his main works, Professor Packard
addresses the topics of health and disease in the history of Africa; the
relation between disease eradication programs and the ideology of
development; the malaria eradication program; medicine, international
health, and colonialism; academic production in the history of
medicine in the Anglo-Saxon world; and the dynamics of scientific
publishing in the field of the history of medicine.

Keywords: history; Africa; malaria; development; scientific periodicals.

Resumo

*Entrevista com Randall Packard, professor titular da cátedra William H. Welch
de História da Medicina de The Johns Hopkins University e coeditor do
Bulletin of the History of Medicine. Ao falar sobre sua trajetória acadêmica,
suas atividades como editor e seus principais trabalhos, ele aborda os temas da
saúde e da doença na história da África; as relações entre programas de
erradicação de doenças e a ideologia do desenvolvimento; o programa de
erradicação da malária; medicina, saúde internacional e colonialismo; a
produção acadêmica em história da medicina no mundo anglo-saxão; e a
dinâmica da publicação científica no campo da história da medicina.*

*Palavras-chave: história; África; malária; desenvolvimento; periódicos
científicos.*

The interview with Dr. Randall M. Packard, published only now, took place during his visit to Rio de Janeiro in April 2007 when he participated in the seminar “Henrique Aragão e a pesquisa sobre a malária: 100 anos da descoberta do ciclo exoeritrocítico da malária” (Henrique Aragão and malaria research: 100 years since the discovery of malaria’s exoerythrocytic cycle) held at the Oswaldo Cruz Foundation. Packard has a degree in History (1967) from Wesleyan University in Connecticut, a Master’s (1968) from Northwestern University in Evanston, Illinois and a doctorate (1976) from the University of Wisconsin-Madison. He taught African history at Tufts University in Medford, Massachusetts and at Emory College in Atlanta, Georgia. He taught courses on history and international health there and headed the Center for the Study of Health, Culture and Society. He was also an international health professor at the Rollins School of Public Health, Emory University, Atlanta, Georgia. Currently, Randall M. Packard is William H. Welch Professor of the History of Medicine at prestigious The Johns Hopkins University School of Medicine and co-editor (with Mary E. Fissell) of the world renowned journal *Bulletin of the History of Medicine*, an official publication of the American Association for the History of Medicine (AAHM) and the Johns Hopkins Institute of the History of Medicine. Since the 1970s, Packard has focused on studying the social history of diseases and curative and prophylactic practices in Africa, as well as the public health policies implemented on that continent by local actors and international agencies. The subject of our interview is the author of various books and articles, including *Malaria: the making of a tropical disease* (The Johns Hopkins University Press, 2007), a must for anyone interested in a modern history of the disease highlighted in the pages of *História, Ciências, Saúde – Manguinhos*.

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The first question is about your studies, your projects, how you entered the field of the history of health.

I began training as an African historian in graduate school. After my first year at Northwestern, I decided I needed to go to Africa to see what I was studying. I spent two years in the Peace Corps in Uganda in a trachoma eradication program.

What year was that?

That was 1968 to 1971. We did training in public health at UCLA [University of California, Los Angeles] for a couple of months, and I became an ‘eye doctor’ immediately. I spent those two years basically treating people for trachoma, tracking down cases, protecting springs, and doing other kinds of things, as well as collecting oral histories in the end. So

that was my introduction to public health and to some of the problems of global campaigns. This was actually a national campaign to eradicate trachoma. Then I came back and finished up my graduate work at the University of Wisconsin and went off to do my dissertation research in what was then Zaire, now the Democratic Republic of Congo. I spent a year and a half in Zaire doing research on the history of nineteenth – and early twentieth – century political competition, but all set in the context of indigenous ideas or local ideas about the relationship between political power and the ritual control of the land. My arguments were about the ways in which people manipulated power through an understanding of how politics affects rainfall, a collective view of the land, and so forth. I was trying to understand what politics looks like from the inside.

Part of that research ended up looking at disease problems. There were some epidemics at the end of the nineteenth century that played a part in the story. I also became interested in the history of misfortune and particular sets of ideas about misfortune associated with what you might call witches, which emerged during the colonial period, in part in relation to a whole series of disease epidemics that were hitting the area – sleeping sickness, plague – and how people were interpreting these problems. I'd finished my first book¹ and had my first job at Tufts University (Boston) and was thinking about a second project. All those various experiences came together and had me thinking about a project around health. The history of health and disease in Africa was then emerging as a new area of historical research in the United States. My original idea was to do a history of tuberculosis in Swaziland. I chose Swaziland because it seemed like it would be a nice place to work and because they had a TB [tuberculosis] problem. My original idea was to extend what I'd been doing on people's understandings of disease. I was less concerned about the epidemiology of tuberculosis than how it affected people's lives, and using that as a kind of lens for understanding the colonial experience. Much of my work on health was driven by using health as a window into colonial and then post-colonial experience.

You had a preference for the nineteenth and twentieth centuries. Do you have a chronological period you prefer to work with?

Yes, absolutely. It's partly because a narrative of colonialism and independence is appealing in some ways. If you were doing histories of health and disease, the documentation was a lot richer for later periods than earlier ones. There was very little early work. My idea was to look at how tuberculosis affected people's lives. Even though that was my main idea, I realized I had to have some idea of what the epidemiology of the disease was before I could actually ask those questions. I began by looking at what the epidemiology of tuberculosis had been from the medical records that existed in Swaziland, and what I found was that if you put it on a graph, it was just a linear line that kept going up and up and up. It's not a datum you can make any arguments about, other than that things are getting bad. It was also clear that you really couldn't understand tuberculosis in Swaziland in isolation from the larger southern Africa region and the migrant labor system

¹ Randall M. Packard, *White plague, black labor: the political economy of health and diseases in South Africa*, Berkeley, University of California Press, 1989.

in which the Swazi were involved, so I began making periodic visits to South Africa. This was also because my wife was seven months pregnant and had had a cesarean with her first child, so we thought it might be better to have our second child in Johannesburg. I began meeting academics in South Africa and learning about resources available. When I went back to the States, I decided to recast my project as a larger history of TB in southern Africa, particularly South Africa. But before I did that, and because I didn't really have a lot of material on tuberculosis and I had some time to kill, I started to look at malaria. Now, malaria has much more interesting data. The graphs went up and down and up and down, so you could begin to actually posit some sorts of hypotheses about the relationship between these patterns of disease and other things going on in society. When you read all the medical records that were produced about malaria from the colonial period up through the 1980s, they're all very consistent in linking malaria to rainfall: more rainfall, more breeding, more malaria. But when you actually graphed it out, it didn't come out that way. So that posed the question of what was actually going on. What was clear when you began graphing it out was that what was really determinate was not the amount of rainfall in any particular year but whether the previous couple of years had exceptionally low levels of rainfall, which was related with hunger, crop destruction, and in some cases famine. Once I realized that, there are all sorts of questions you can ask about what is actually causing the food shortages. It's not just lack of rain – and then there's the whole political economy. I wrote a couple of pieces about that, and then I wrote a piece about the resurgence of malaria in the 1960s and 1970s. That got me interested in malaria, though I should say my initial interest began when I was in the Peace Corps and I actually came down with malaria.

This direct experience was actually very formative. I had come down with it because I had stupidly forgotten to take my weekly tablet, and had obviously been bitten by an infected *A. gambiae* mosquito and so, as luck would have it, I came down with the disease. It was very, very bad, and they eventually got me to the hospital, where I was injected with chloroquine, and I was fine. But I also became aware that I was living among lots of people who didn't have access to this care, because of the conditions of the medical system, because they didn't have the resources to get to the hospital on time, and a whole series of other things. The same could be said about trachoma. We tried and tried to eliminate it, but it just... Trachoma is an infection under the eyelid, and when left untreated, it creates scars that can turn the eyelids in and start scratching the cornea, but if treated soon enough, you can prevent that. We had patients who'd show up with an infection in an eye and we'd begin treating with tetracycline ointment three times a day and sulfa drugs once a week. But they had to come in for 12 weeks, which is always a challenge, especially when you're at some distance. So a woman shows up, she starts treating one eye, she gets treated for a couple of months, it's getting better, and then she disappears. She shows back up a month or so later. "Where've you been?" She says a relative died in Busoga, which is the next district, and she stayed there a month. There was no trachoma control over there, no access to the drugs, and she couldn't come back for them. A whole range of things would contribute to the fact that the treatment was a failure. I learnt a lot about disease and poverty from that experience.

You told us your interest in malaria is personal and based on your life experience in Africa, but you began to write on malaria more from the perspective of the history of international health, not from the perspective of Africa. Why did your interest in disease in Africa shift to the history of global health? What did the Peace Corps mean to you in that period?

Going to Africa was also a way of avoiding the draft and going to Vietnam. But I honestly did want to do something like that, and I really enjoyed it immensely, even if it is, as they say, “the hardest job you ever love,” because you spend two years thinking in a different language, in a different culture. So I was in an international organization, and at the back of my mind I had the notion that you could go out and eradicate diseases. Malaria obviously fell into that category, too. You ask a good question, because I never really thought about how I made that particular jump, but I’m pretty sure it was because of the work I did on the Social Science Research Council (SSRC). I was a member and then chair of the Africa committee. The SSRC is a refunding organization that receives money from Rockefeller, Ford, and bigger agencies, and then funds social science for a number of different areas. For several years, there was a Latin America committee and an Africa committee; they carved up the world into various geographical regions and supported much dissertation and post-doc research in various parts of the world. I was on a committee that selected fellowships. We also did other things, directing social science research into particular areas, like agriculture and health. That really got me thinking about global kinds of prospects and processes. I think I became interested in eradication – because I was already interested in malaria – as an example of what these programs were like. I was actually going to write a book on the history of eradication. I did a lot of research and traveled around, holding conferences and bringing people together, and the book kept getting pushed back. I had funding for parts of it, but it always seemed somehow overwhelming on a certain level, and other things kept distracting me. So I wrote one extended article on why eradication came about.² There’s a chapter in my book on eradication and its consequences and its shortcomings.³ I grew interested more generally in development as a process. Development programs, international programs, were becoming an area of enquiry. The SSRC was really the jumping-off point for my book on international development and social sciences, and for my thinking about the ways in which the social sciences intersect with development.

Was this in the seventies?

No, this was in the eighties and nineties.

Regarding the paper you wrote on the cultural model of international health, I’d like to hear from you what the relationship is between malaria and development, if this is a cultural model to think about.

² Randall M. Packard, ‘No other logical choice’: global malaria eradication and the politics of international health in the Post-War Era, *Parassitologia*, Roma, n.40, p.217-229, 1998.

³ Randall M. Packard, *The making of a tropical disease: a short history of malaria*, Baltimore, The Johns Hopkins University Press, 2007 (Johns Hopkins Biographies of Disease).

That's the introduction I wrote with Peter Brown, the anthropologist – that's why it turned out as a cultural model.⁴ Ask Peter Brown about it! It was temporary contamination.

I don't really think about it in terms of cultural models but in terms of frameworks in which people think about development. More of what comes out in that article is the way he thinks about it, but I think there are intellectual frameworks that are brought to development and to questions like malaria and how you deal with it that need to be interrogated. There's a lot of good work on this that's been done mostly by anthropologists rather than by historians. And then moving beyond that, because a lot of that, the work of James Ferguson and Arturo Escobar⁵ and others and which basically see development as some sort of expression in health, as some sort of biopower, and the way in which development frames the problem to solve its own issues. Development agencies generate development strategies which coincide with their view the world and with the approaches they prefer to employ.

They're colonized by the idea of development.

Yes, that's right; they're colonized by the ways in which development constructs its object. If you look at Tim Mitchell's work on Egypt and the Nile⁶, or at any Usaid [United States Agency for International Development] or World Bank development document on Egypt, they begin with an introductory section on the history of the river Nile and the geography of Egypt, and how there are high ratios of population to land, with people therefore unable to produce enough food, and associated malnutrition issues. Mitchell unpacks all that and says: It's not a question of land; look at the unequal land distribution and how these patterns of hunger don't have to do with the particular geography of Egypt. There's a geographical determinism built into the way these organizations construct the problem, which serves their particular interests of being able to market agricultural subsidies, arable fertilizers, what have you, family planning, rather than investing in land reform. More recently, there is more interesting stuff moving from that level to look at the sociology of how those ideas get produced: How does that document I just described actually get produced? It's not someone just sitting down and writing it. These policies are made with lots of stakeholders coming together and making compromises. It's not that everyone's equal at the table, obviously, and some have more say than others.

You told us you think about frameworks. Was eradication a framework of a group of experts?

It came out of a period of time when there was a tremendous amount of optimism about science's ability to bring about change. That very much framed the way people thought in a number of ways. For example, that expertise lies in particular parts of the

⁴ Randall M. Packard and P.J. Brown, Rethinking health, development, and malaria: historicizing a cultural model in international health, *Medical Anthropology*, Philadelphia, v.17, n.3, p.181-194, 1997.

⁵ James Ferguson, *The anti-politics machine: 'development,' depoliticization, and bureaucratic power in Lesotho*, Cambridge, Cambridge University Press, 1990; Arturo Escobar, *Encountering development: the making and unmaking of the third world*, Princeton, Princeton University Press, 1995.

⁶ Timothy Mitchell, *Colonizing Egypt*, Berkeley, University of California Press, 1988.

globe and needs to be transferred to other parts of the globe where it doesn't exist. It's not only about reading what the so-called developed world is but also about constructing the rest of the world as lacking and therefore needing to have solutions that come from the outside. This frames the problem in ways that completely read out any kind of understanding of what's really going on in these places where development occurs. This happens because this framing occurs on a very unequal playing field, where power resides on an unequal basis as a result of inequalities in resources. Money talks, and people who need money are then subject to these kinds of framings, which they probably don't agree with but they want the money.

When you started writing about eradication, international campaigns had been discredited; there was a crisis of optimism. What were your perceptions of the framing conditions that framed your work? What was your view of this crisis? What were the tendencies at the moment you began?

As you say, a critique of development was emerging in a number of fields at that point in time. But critiques of development certainly go further back. Latin America is a central part of that story going back to the 1940s and the Economic Commission for Latin America. This critique both of notions regarding the benefits to be derived from trade and of particular development models was reified in a number of different studies. In the 1970s, organizations like the World Bank were stepping back and saying "this isn't working." It was then that Latin America saw a shift from large projects driving growing GNP [Gross National Product] to a focus on poverty reduction and small-scale products. You get very cynical when you follow the rhetoric of international development very closely and for very long, because the language keeps changing and there are new, reworked models, but this doesn't necessarily change what goes on. It was in this context when a series of studies were beginning to look at this, or had been looking at this for some time, that I began looking at health. I wasn't coming up with something brand new, but the topic hadn't been approached much from the health side of it. There was a lot more on agriculture and other development programs, while health and international programs were just beginning to be explored from this perspective. We're really at the very beginning even now. The global history projects in which Marcos Cueto and others are involved are the first cut, in some ways. I think historians have not got to where the anthropologists are. They are not nearly as critical as they need to be of organizations and processes or looking at where things are actually happening in very good detail. In fact, there was suppose to be a second volume of the *International Development and the Social Sciences* book (Frederick Cooper was still involved, and other coauthors) that actually looked at what happens on the ground with projects, because most studies have been at 30,000 feet or so in terms of development. We still need a lot more studies of what this looks like on the ground.

One other question regarding the localization of these health initiatives vis-à-vis the rhetoric of large programs: is it possible to move from general programs in the history of international health to local experiences? We know about the production of knowledge and health programs in the center and about national and local policies and knowledge production. What do you think about the local/international relationship in the production of knowledge, policies, and health programs?

I think we do need to move from general programs to local experiences. I'm reading David Mosse's book on culture and development⁷, which looks at an overseas development agency in the 1980s and 1990s, at a British project in India to improve agriculture, but it was very much in the rhetoric of participatory development – Robert Chambers and all that. It's a multi-sided study that looks at everything from the sociology of the development of policies to their implementation, and at what's going on at every level. If we begin to have studies like that, that look at some kind of initiatives – it could be malaria eradication or some kind of development program – and then try to understand them from all points of view – well, maybe this kind of research needs to be done by teams rather than by individuals. It's hard to do. I haven't finished Mosse's book, so I can't tell you if he's successful or not. It's very intriguing. He was intimately involved in the project, so he was an insider doing development and then the outsider, stepping back and watching himself do it, which is what I often think about myself doing, when I go to these development meetings. I'm now on a project for a five-year evaluation of a global fund for tuberculosis and malaria, and so I go to these meetings at big development consorts and sit there and listen as a participant, but at the same time I try to step back and listen as an observer and reflect on what's going on in the room. I did that in the article I wrote with Paul Epstein on anthropology and AIDS.⁸ It came out of going to a USAID conference on AIDS and anthropology, and just sitting there and listening and seeing what was going on and reflecting on it.

Do you think it's a novelty for historians and sociologists to participate in the international forums in which decisions are taken? Do you think the admission of historians may be part of the crisis of scientific optimism and all those vertical health policies? Do you see yourself as a privileged participant, like Elizabeth Fee and Marcos Cueto, writing a non-official version of an organism, but at the same time with their sponsorship?

That's a bit different. This might not apply to what David Mosse is doing, which is inside/outside. I think it's still difficult for historians to be taken seriously in such studies. I was brought into these international decision-making forums because I work on the history of international health and development, and one of the primary contractors, who is in international health, also has a broad view of international health and appreciates this historical perspective; he's convinced they need to have that perspective to really evaluate the materials, because otherwise you're evaluating in the terms of the organization itself, without trying to put it in the larger context of how the organization looks in relation to other organizations, how it looks in terms of global health trends over the last whatever years. His is an enlightened position. He convinced me that it might be useful to do, but we'll see how far it actually gets us. We're at the very beginning, so I'm not sure how useful it'll prove. I think it's somehow easier for people involved in health and

⁷ David Mosse, *Cultivating development: an ethnography of aid policy and practice*, Ann Arbor, Pluto Press, 2005.

⁸ Randall M. Packard and Paul Epstein, Epidemiologists, social scientists and the structure of medical research on AIDS in Africa, *Social Science and Medicine*, Amsterdam, n.33, p.771-794, 1991.

development to think anthropologists have something more useful to tell them than historians do. My guess is the line between historians and anthropologists becomes much murkier, as anthropologists are doing historical and historically-based anthropology. These international forums may be longing for the anthropologist who will go out and tell them why the natives won't do what they want them to do, rather than a historian who takes this critical eye on how the whole process is developed. From my point of view, this kind of critical perspective is, or should be, more useful because it may have more to tell us about why things don't work than simply "we used the wrong shape cup and handed it to them with the wrong hand."

May I return to the Anopheles gambiae analysis you wrote with Paulo Gadelha? What is the importance of your lessons on Gambia to your arguments and the history you wrote on malaria and international health?

That was a very interesting article to write, because I'd gone to the Rockefeller archives to research the study of eradication.⁹ Soper, of course, had been central in that, so I started going through his papers and diaries, which are voluminous; he's very detailed. I read those reports and had a sense that there was a story there that hadn't been written, a story that was very different from the story told by Soper. What I say in the article is absolutely true: I was going through the report and there was this photograph (this doesn't appear in the article; it will in the book)¹⁰ of people fleeing the town and going down to the coast, fleeing famine, yet there's actually no reference to what that was all about. I sat there and said, "Wait a minute." Remember, I'm coming from having written about famine and malaria in Swaziland, so that was in my head and it set off a bell that there's a connection here. So I immediately say I have to understand what was causing these famines, because it was obviously not just drought. It was conditions. We all know that; it's nothing new. So I went through all the secondary literature I could find on the Northeast of Brazil, put it together with stuff from the archives, and constructed a paper that I presented at the meeting at Bellagio (Italy). And that's where Paulo Gadelha comes up and says, "There's a whole bunch of stuff you haven't seen." I knew at some level that that must be the case, but not at any specific level. I knew nothing of Brazil other than what I'd done in that piece of the paper. It was illuminating when he told me that. At some point people say, "Well, that's Packard, that's what he does. He takes a political economy perspective on everything he sees." And it comes through in this book I've just finished¹¹, because I believe it very passionately. For me, in some ways the Brazil story is one more opportunity for making the case that there is much else going on here that often gets written out, ignored. And development tells its own narratives in order to justify what is done and to draw their

⁹ Randall M. Packard, and Paulo Gadelha, A land filled with mosquitos: Fred L. Soper, the Rockefeller Foundation and the Anopheles Gambiae invasion of Brazil, *Parassitologia*, Roma, n.36, p.197-213, 1994.

¹⁰ Randall M. Packard, *The making of a tropical disease: a short history of malaria*, Baltimore, Johns Hopkins University Press, 2007, (Johns Hopkins biographies of disease).

¹¹ Randall M. Packard, *The making of a tropical disease: a short history of malaria*, Baltimore, Johns Hopkins University Press, 2007, (Johns Hopkins biographies of disease).

own conclusions and lessons. When I teach my course on the history of international health and development, we go through different sets of programs and strategies from colonialism up to the present as best we can, and at each point, when you stop after the eradication of malaria or smallpox and say, “What were the lessons learnt from this particular project?” and, secondly, “What were the lessons they should have learnt but didn’t, and why?” it’s the second set of questions that intrigues me and drives a lot of this. I’ll probably get tired of it after a while. I’ve ridden this horse for a long time.

Thinking about the categories you and other authors apply to understand the interventions, or the frames, used by organized international health programs and their relationship with the native categories used by real people during campaigns, sometimes focusing on parasites, sometimes on vectors or on economic conditions, what do you think about the categories of thought of native health workers and our categories as analysts who deal with them?

I think no one approaches a problem without some kind of framework, whether it’s someone living in a rural area of Bolivia who’s sitting in their hut or home, and people come round with these spray things. Even they have a way of understanding and interpreting what’s going on. But their way may not be the same as the person’s living next door. There’s a whole variety of frames. That’s the first thing to say. It’s very easy to say, “This is what the recipients of development think about development.” We don’t really know much about that; it’s much more complex. And the person with the tank on his back has a perspective about what he’s doing and why he’s doing it; the person who’s directing him has a perspective. You could take perspectives for every point of view. My guess is that if you went all the way to the top, to the people who designed the program and put it in place, they would have their own set of perspectives, which would be a different set of perspectives from what you or I might have, who might see this as an exercise in biopower, as an intrusion into people’s lives. I don’t know if the people on the ground think that way. One of the things, certainly in Africa, is that we now have a fairly substantial volume of work on mostly colonial and some post-colonial health projects, yet we have very little that talks about what people think about what happened to them or about the experience of being the subjects of these kinds of projects, or how they even thought about the problems that were being dealt with, because that kind of research just hasn’t been done to any great extent. I have a former student, Julie Livingston, who wrote a book on disability in Botswana, and is one of the first people to engage in that kind of research.¹² At a certain point, she puts the various ways in which the Western medical community conceptualized the problems of health side by side with those that were conceptualized by Botswanans themselves. In many cases there’s not even a translation; it’s just a very different framework for understanding life. But we have so little of that, because it takes a kind of research that a lot of people are neither trained in nor willing to do. In some ways – and I’m thinking particularly about Africa – there was an early period of African history when all of us were trained to do oral research, but we were also looking

¹² Julie Livingston, *Debility and moral imagination in Botswana: disability, chronic illness, and aging*, Bloomington, Indiana University Press, 2005.

at pre-colonial history. There were some studies of colonial history but not an awful lot at that period of time because there was a sense that we needed to recapture the true essence of African history that was getting lost as these people died off. Then there was a rediscovery of colonialism. I say 'rediscovery' because there was an earlier generation that goes back to the fifties and forties that wrote a celebratory literature on colonialism, and a new critical analysis of colonialism, but it very quickly becomes written out of the archives. And people rediscover the archives as a source for doing this, and they read it with a critical edge and often read it against the grain, so they're reading in ways that weren't intended by those writing those reports, but the voluminous records allow them to do a lot and go a long way, but few of them go off and actually do oral history on the early stuff. That tradition carried over when people began looking at health. I don't know what it's like in Brazil in terms of these questions and whether in fact there are historians who are actually looking at what it was like to be involved in campaigns to fight dengue or malaria. If people are doing it, that's great. We need to get out there, because it would be a significant addition to what we know about global health.

When I tried to understand 1950s papers on malaria in Brazil, the conceptions relating health and development and health and economics were so merged that I'm surprised with any analysis that separates that. Mosquito but also nutrition. Sometimes you find a perspective that is very restrictive – against the parasite, the vector, the disease, but nothing to do with economics.

There is a pendulum. I write about this from the point of view of international health organizations and how, with malaria, there's clearly a movement back and forth between a merely technological perspective and a broader, more socially based one. But my guess is that if you map that out and put it against what's happening in Brazil – because Brazil has different political traditions – the timing will be different. That's a really interesting datum. It may very well be that whereas globally, the narrow view has been a dominant paradigm, my guess is that in Brazil there's a momentary paradigm that appears at particular points in time – like 1950s malaria efforts – but which is out of step. Internationally, it's the broader view that's often out of step and often that broader view comes into play only at particular points in time, talking about global or international institutions, and it gets eliminated very quickly. I think in part it's because health organizations are dominated by physicians, but at the same time I suspect physicians in Brazil would not even think about the problem of health without the broader perspective, so I think these institutions are being dominated by particular types of physicians. They're being dominated by the notion that in order to get anything done, we have to do it within some kind of narrow campaign, because we can't deal with all these other issues. We can't go into a country and start messing around with the economy and social organization. We don't have the time or resources to do it, so we've got to focus on what we can do fairly quickly. It would be interesting to take a series of countries and map out the swinging pendulum to see how it varies in different places.

How did you get from South Africa to Johns Hopkins?

You mean academic-wise? I did my South Africa work when I was at Tufts and finished it off when I was at Emory University (Atlanta). Getting to Johns Hopkins is a bit of a

mystery to me, in that it is a position as head of the History of Medicine program. Originally, the History of Science, Medicine and Technology was all one department, but the package was just not working, so we divided it up, because there were too many strains. I don't think that's necessarily inherent. I think it was particular to the Johns Hopkins situation. It was a very strange kind of process because I was not trained in the field – there's so much I don't know in the field that I readily admit – but I think it was a moment in time, institutionally as well as within the field, that things were happening that maybe made it more likely I would be chosen than not. One was a realization that this is a very Eurocentric field, in terms of much of what's being practiced in the United States, in terms of the make-up of the programs, and it doesn't reach out very far. If you look at what's being done, it's all Europe and the United States that's being covered. Mostly in the United States there's a notion that you need to have people thinking about health and development outside that context, so my working in Africa seemed to make me a likely suspect. They also needed someone who had a record of having been an administrator, and I had done a lot of that, having chaired two departments and other kinds of things. To be quite honest, the selection process was really bizarre. Except for the presentation I made to the department, there was hardly any moment when I was actually engaged in any kind of intellectual discussion. The search committee was made up of physicians and basic scientists and one historian, and I think they were much more concerned about my abilities to run a department.

I didn't work my way up. Some people say it's the most important chair in the country. I know people in the field who shook their heads and said, "What's this all about?" They didn't know who I was, unless they'd been interested in the history of disease. And quite honestly, the kind of history of disease I do is not done at all in the history of medicine in the United States. Disease is something that's assumed, since Rosenberg, to be some sort of cultural construct that we have to explain in terms of a wider cultural intellectual universe. The whole idea that you can actually do historical epidemiology is fraught with problems and difficulties, because how do you know that this is what they were talking about sixty years or one hundred years ago, that it wasn't some other disease? You have all these kinds of issues, not to mention what you do when you're talking about political economy and these broader issues, which are not part of the tradition.

When you use framing, don't you have any allegiance? Are you in no way a tributary of Rosenberg?

I am. *The myth of the 'tropical worker'*¹³ was very much about framing disease, but it's a framing that has a political economy thrust to it. What I see as part of the framing mechanism is not just cultural or intellectual but economic and political as well. I've written another piece about malaria that I haven't published yet. It's on the myth of the resistant native: the notion that Africans living in certain areas of South Africa had an acquired immunity to malaria, which affected workers in certain contexts. Essentially, I show,

¹³ Randall M. Packard, The invention of the 'tropical worker': medical research and the quest for central african labor on the South African gold mines (1903-1936), *Journal of African History*, Cambridge, n.34, 1993.

as I usually do, that this myth is created by a lack of information about what actually is happening, but that there is also a will to use these populations in particular ways. I really do take that stuff very seriously, but I try to fit it into the larger political and economic framework in which I work and understand things – it's more of a 'materialist analysis' I guess people would say.

You are co-editor of the Bulletin of the History of Medicine, so you get a lot of papers and articles. What are the major fields that people are submitting to the journal? Where do you see good production coming from?

I'm not sure this is just because of who I am and where I am, but I would say there is a lot more history of public health being done than there has been in a long time. There are a lot of people looking at public health programs and issues having to do with disease. We get a lot on the history of psychiatry, the history of mental health, which is expanding in multiple different ways. We just had a special issue on cancer. There's a big program at Manchester. There are a number of people working on the history of cancer and that's likely to grow for quite some time, which is an interesting question when you think to what extent cancer is on the historical map here. In Africa, it isn't to any great degree. The student I told you about who wrote the book on disability went back to Africa to write a book on the history of pain and laughter. She wrote back to me. (One of the wonderful things is the internet. She's sitting in Botswana and can communicate with me by internet. When I was doing my work in Zaire, I'd send a letter and six weeks later I might get a response if I was lucky.) And she says: "I really think I'm going to work on the history of oncology, because there's so much cancer here and so few resources to deal with it." No one has time to deal with cancer in Africa because they're so busy combating malaria, tuberculosis, and AIDS – and yet there's this growing body of people who are coming in with it. Well, a lot of it is associated with AIDS. She said there's not a dermatologist in the whole of Botswana, and no MRI machines. There's nothing you can do, in some ways. So all we have now on the history of cancer is the history of cancer in the United States and in Europe. I don't know... probably some stuff on China. I don't know if anything's been done in Brazil or Latin America or elsewhere. I think this is a topic that needs to be taken seriously. It depends on where you're looking, but this epidemiological transition is occurring in Brazil among certain populations. I walk along the beach and I say, "This has got to be the skin cancer capital of the world."

That would be a really interesting study, because it would be a cultural history of the sun. In a sense, it's a matter of finding ways of getting the experiences of places like Brazil into dialog with these trends, so we don't just look at and understand this history from the narrow perspective of what's going on in Europe and the United States. It's way too parochial. What else? We get a lot on women and a whole series of things on women's health and women as healers. There's a growing area of cultural history, particularly early modern cultural history, of the body and ideas about reproduction. We are going to do a special edition on women in medicine in early modern Europe. Not the next one, but the third one. The next one out is a volume that Sanjoy Bhattacharya is putting together on smallpox.

That's a good question, and I suppose we should sit back and look at it. We are somehow so close to it. Two or three articles come in a week, we review them and send them out, and we do these reviews at the end of the year, but they're quantitative. We're not doing a trend analysis in terms of the themes. I think that would be more interesting. Two years ago I gave a paper on malaria at the University of Minnesota. They had a graduate seminar and one of the things they were supposed to do was take different journals on the history of medicine and do this theme trend analysis over a period of time. I think that would be very useful and wouldn't be that hard to do either. Again, it's in the back of my head to think about these issues, but they somehow don't come out. Since the day I became editor of the *Bulletin*, I've said one of the things we should do is overview papers that take a theme and say: "Where's the state of the art on this? What's been done? What are the questions that have been looked at, and what still needs to be looked at?" Put those out on a regular basis as a guide to the field. It's just a question of identifying people who want to take a break from their research to do this fairly extensive kind of work. We used to do this when I was on the Science Research Council. There's a whole series of what we used to call 'soap papers': state-of-the-art papers. They were fairly extensive papers done for a thousand-dollar honorarium and submitted to the Council, to what was then the Africa committee, which is made up of really smart people; they'd rip them apart and send them back. Some people who were fairly prestigious in the field didn't like that very much, but it was a very good experience. Those papers are extraordinarily valuable for working out where the field was at that particular moment in the eighties and early nineties. They stopped doing it at a certain point. I really do think we should do that. One of the questions should be: "What's missing from this picture? How do we define it? Where are the materials outside of the US and how do we get them into the picture outside Europe?"

We have good examples of state-of-the-art operations. You told us that the Bulletin has a rate of article refusals of around 80%.

It is high. I don't know what the exact figures are, but it's a high threshold. But what I'd like to figure out is the difference between this initial rejection and the ultimate rejection rate. If you take the papers that get reviewed, sent back for revision, and then come back after substantial revision, I think that rejection rate is quite low. I have suspicions about how seriously reviewers read the papers the second time, in the sense of, "Oh God, can I actually make this person do more stuff now?" We read these papers when they first come in, and we read them at the end, and sometimes along the way.

Do you read all the papers?

We read all the papers when they are submitted. That was something I started when I came on. Before that, the two editors looked at a paper and if it seemed serious, they sent it out without actually reading it. I didn't think that was responsible because it burdens your reviewers when you send out a poor paper, and you can't always tell just because it has footnotes.

So you take on the onus of reading it yourselves. You don't use the journal's counselors or advisors?

No, we don't. Maybe we should. I think what we're reading for is (not that we're the final judge of this): does this seem like a reasonably competent paper that is dealing with a significant issue and in a way that puts it in the context of the field? Now, it may be full of other problems. At that stage, if something comes in that's just not well written, we'll send that back too; or if something is 55 pages, there's no way it's going to get through. I think eight or nine thousand words, about 30 pages, is the limit, with notes. We're flexible on that. We're also flexible on 600-word reviews, too. But it's the 1,200-word reviews that we get nervous about.

You work basically with ad hoc peers?

Yes. For each paper, we sit and decide: who should this go to? Who would be the best person for it to go to? At least three and sometimes four reviewers. The reviews are sent to us in two parts. One has recommendations for the editors and the other, comments for the writer. There's a lot of overlap, but sometimes the language in the comments for the editors is a little bit harsher than the language for the writers. Then we write a letter back that varies in detail, depending on whether we think there are specific issues that need to be emphasized and particularly whether they are conflicting. We may guide the author to think particularly about certain issues, but they get all the reviews.

If you have conflicting opinions submitted by peers, do you direct the author's attention to the commentaries you judge the best?

No, not the best but the ones we think they should look at most closely. Sometimes you'll get seven, eight, nine pages of detailed things they can look at. We advise them to look at everything, but there's a fine line between holding authors to the criteria that are set up by the reviewers, and the fact that this is their paper and they're allowed to have a view and make certain kinds of arguments; if those arguments don't fit with what the reviewer thinks is right, you can't just tell them to change it. What we would say is: "Reviewer A suggests you think about it in a different kind of way, and we encourage you to do so, acknowledging that this is your paper." Sometimes what's being suggested is really a different paper. Sometimes it's actually quite useful for them to think about. It gets subjective. Maybe in our minds having them look at some of these things is really interesting, but it may not be for them.

Is it a rare circumstance that authors counter-argue and send you their objections?

If we're interested in the paper – and we encourage it more and more now when we send these things back – we'll say: "Write us a letter responding to the comments before you start to review." Very often we'll get: "I agree with this, this, and this, but I don't agree with this." "I can't do this; this isn't the paper I wrote," or "I think the evidence I present justifies this particular claim."

Once the paper is rewritten, we send it back out to the peer reviewers, and very often they'll sign off on it, while others will have more suggestions, and some will just never be

happy with it. We have to decide in the latter case, well, if that's just their thing, but the paper is still a valuable contribution to the field. That's the hard line in making an evaluation. You know people are out there in the field – Mary Fissell certainly knows what their dispositions and predispositions are better than I do – so you can use that to make your judgment, but it's subjective. At the end of the day, a solid piece of scholarship that doesn't have holes in its data and makes a particular case, even if a reviewer thinks it's not appropriate, well, I think that's how you move the field and get engaged in discussions. Everyone doesn't need to think the same thing. The authors have been warned that some issues may be raised, but if they still feel that this is it, and we think they have a strong set of supporting documents, then we go ahead.

Do you receive a lot of papers from overseas?

I don't know the percentage, but we do have the numbers. Most come from England. We get a few from France, occasionally from Japan, one from Korea recently.

And when you do so, do you try to find peers in the places of the author's origin?

Not necessarily, because the topic has not necessarily been written about only by people from that area. We try to find people who are familiar with the place and topic of the paper, as best we can, and maybe find someone who's written more generally about the topic. You'll have a paper on cancer in nineteenth-century Britain, say, so you ask: "Who knows about this topic?" And if it's a topic where you know a lot of people, then: "Who's written on the history of disease in nineteenth-century Britain?" or: "Who's written about this topic in America that might be aware of this?" The field is too small to always find people who are specialists in one particular area, so you have to figure out whom.

What do you think about the movement to allow open access to published materials?

I think open access is a very viable thing. You're coming to me after nine months of negotiations with the Wellcome Foundation about open access. They now have required that any article that is published, that's been supported by the Wellcome, has to be open access. "What does open access mean?" is the first question; second, "What does it mean for a history journal?" *Social History of Medicine*, *Journal of the History of Medicine*, and *Medical History* are all published by Oxford. Oxford has developed a particular set of relationships and processes with the Wellcome so they can do their thing automatically. Not us though. We began by asking, "What do you want us to do in order to meet this set of obligations?" What they essentially do is pay the publisher three thousand dollars for whatever expenses, editorial and so forth, it takes to make this available. But the money actually goes through the author, who is in charge of this: he's given money from a grant to pay the press that's publishing this paper. It's fairly complicated. Without going into the details, the problem was that Wellcome was thinking and working on a set of policies, and they set out: this is what you need to do. We started down that road and entered negotiations with the Johns Hopkins University Press, because they don't like giving things away. Next thing you know, the website's changed and there's a new set of requirements.

This kept going on, and we'd send back these requirements and explain all the problems and say, "Maybe we should talk about this." It went on and on, and we're still at it. When I get back, we're meeting with this guy who's coming from the Wellcome to go do another conference to talk about this. We're toward the end of it, because the checks are coming; we think we're OK. They want to have open access to the article, which means you put it on PubMed Central, it has to be in a particular format, and that creates certain problems. There has to be availability of the illustrations. For history journals this creates a problem that is not true for all science journals, because science journals create their own images for their work. In the United States, authors get permission for one-time use in a particular venue. You put that on PubMed, out there for the whole world, and that creates huge copyright issues that haven't been dealt with. That's where we are in terms of negotiations. I think it's a good thing. We just need to figure out a way to do it, and consistently so, and then say, "That's it."

You mentioned to us that you have a student who is in charge of checking all quotations to see if they are correct. How do you deal with this issue of copyright?

Copyright goes to the Press. When authors submit, they sign off copyright and the Press owns the copyright to it.

But are you extremely cautious about things the authors bring to their pieces, in terms of images?

They send us the written permissions they've gotten. In today's world (I just went through the permissions for my book: 21 images) you can do it online, certainly with a science journal. Otherwise, you write to the editor. The tricky part is that journals often give you rights to reproduce images that appeared in their pages, although they don't really have the rights to them. I must say that on the book side, they're much less picky about what these rights are than we are on the journal side. We require evidence that the authors have permission to publish the image. As far as fact checking, we only do it on the bibliographic references we have access to. We can't check primary source data, but in terms of secondary sources or any sources that are available in the library, we'll check it. It's a throwback to German methods and the origins of the *Bulletin*. We may be one of the very few in the world, certainly in history, that still do that kind of checking. But it actually doesn't involve much, financially.

Is this German tradition present in the dynamics of research and academic life at Johns Hopkins, in your department?

It's a tradition that had been there from the beginning. I wouldn't say it's still there in any great degree. We don't have any physicians in the department; we're all PhDs in history, so that tradition's gone. I think we're a very different department in terms of what we do and how we function than the so-called German model upon which Welch, Sigerist, and Temkin designed the program.

What's the next step after your book The making of a tropical disease: a short history of malaria?

That's a good question. I do have a commitment to go back and look at what happened since 1990 with TB in South Africa. I'm interested in the story of the transition and what its impact has been on health. TB in South Africa is a global crisis because it's XDR-TB [Extensively drug-resistant tuberculosis], which is emerging. There are very facile historical explanations floating around, and it's very much being seen as a product of AIDS. I think there's a certain truth to that, but the epidemic begins before AIDS and it's on a trajectory before AIDS comes in. AIDS just accelerates it. There's an interesting dynamic going on in South Africa about history and disease, which has to do with the denialists and Mbeki and the way he initially resisted the idea that this was a product of HIV and a problem that could be dealt with in a biomedical frame; he saw it much more as a product of a long history of colonialism, apartheid, political economy, and so on, which was being read out of the AIDS epidemic in the drive to do something about it. On those grounds, I agree completely that this should be part of the narrative here. But in the political struggle that ensued, those on the side of access to anti-retrovirals and the group opposed to Mbeki's position have taken the position that any references to history are part of that discourse about denial. So history is being written out in a systematic way in the narrative on AIDS. In its place are these very facile historical explanations that don't deal with this larger context. Without getting embroiled in that battle, I still think it's important to step back and see what the broader historical determinants have been and what's happened to TB in South Africa. It's not directly related with AIDS, but you can't not talk about AIDS when you talk about TB. On a number of grounds, I think this is an important piece to be done. After that, I don't know. I've been telling everyone else they should do these multi-sided development projects, so maybe I'll just do something along those lines, but who knows? We have a big Baltimore project going on now and I think that's going to open up some interesting questions as well.

What is the level of interaction you have with African historians?

I have lots of interaction with them. I must say, I don't go to the meetings as frequently as I used to. It's been a challenge to keep up with the literature on African history at the same time as I'm trying to catch up with all this other literature. And I certainly have maintained close contacts with all the people who work on health and disease in Africa.

Is this a strong tradition in African historiography?

Yes. Well, it began being important in the late seventies. There have been a lot of people working on it. It's kind of percolated along.

All the tendencies characteristic of the history that the Anglo-Saxons, French, and Brazilians have been doing since the 1960s, do they apply to African historiography: gender, sex...?

I don't know how long you want to spend on this, but I think more recently, within the last decade, maybe. But for a long time African history was very parochial. Latin American history probably was less so, because it did have a more theoretical frame, driven in large part by historians in Latin America. We had a conference in 1985 in Toluca,

Mexico on the political economy of health and disease in Africa and Latin America, which was published in *Social Science and Medicine*. It was a fascinating meeting in part by the coming together of Africanists, who were very empirical about their work, and Latin Americans, who were very theoretical about their work, and it was productive in a certain way because of that.

There was a group of Africanists who became engaged in Marxism, in material kinds of things. Histories of gender have been around for a while but cultural history has found a place in African history where it hadn't before. To be honest, it's been a struggle, and people who've written on these topics have been subject to critique by those who are more traditional in their approaches to African history. There has been a certain questioning: Is this really African history, or is this the adoption of external theoretical frameworks, applied then to a ground they don't fit? I don't know the answer. Every framework can be used to illuminate things. Unless you're making assertions of absolute reality, then you should be able to apply whatever you want. But in some cases, like Luise White's, very interesting work has been done. She wrote her first book on the history of prostitution, which won the Herskovits Prize for the best book in African studies in 1991.¹⁴ Her book was criticized by Jan Vansina, one of the major figures in African history, because prostitution isn't a topic we should address; it's not important to African history. Her second book, *Speaking with vampires*¹⁵ – this wonderful story about these vampire tales that circulate throughout Eastern and Central Africa – tries to make sense of what they're all about. But how do you make sense of these cultural representations, and what is the level of evidence you need to understand? She tries to use rumors as a way of understanding indigenous perceptions of colonialism and experience, but I think that's really tricky ground: how do you interpret what these stories actually mean to the people who tell them? It takes a kind of research I'm not sure she actually did. So the simple answer is: yes, I think we are tracking these trends, but it's not often easy and not without resistance.



¹⁴ Luise White, *The comforts of home: prostitution in Colonial Nairobi*, Chicago, Chicago University Press, 1990.

¹⁵ Luise White, *Speaking with vampires: rumor and history in Colonial Africa*, Los Angeles, California University Press, 2000.