

Chapter 3

O Anthropology, Where Art Thou? An Auto-Ethnography of Proposals

Sverker Finnström



Unlike Malinowski, when the magician had stopped incanting his spells, they did not stay to watch the canoe building.

—Maurice Bloch, “The Past and the Present in the Present”

In this chapter I revisit the so-called Malinowskian legacy in light of my efforts to raise funds for my own anthropological endeavors. Today’s almost habitual dismissal of this legacy as parochial risks missing the fact that the anthropology of Bronislaw Malinowski’s days was not simply a colonialist enterprise working under faulty premises. Even if epistemologically ethnocentric, Malinowski’s achievements made possible debates that paved the way for an engaged, reflexive, open-minded and open-ended, and indeed global, anthropology. I will sketch some possibilities and potentials for ethnographic writing, but more, forces that tend to corrode the anthropological mind, a kind of control and restriction of the anthropological and academic freedom that I guess Malinowski never had to deal with in his life. As one of those ancestors who still interfere with our daily anthropological lives, I suspect him to be somehow troubled with today’s predicament.

Perhaps my intervention more than anything else deals with the question of how to keep *thinking* anthropologically when an ever-increasing intellectual effort must be dedicated to the promoting and selling of the anthropological perspective in the so-called free market of research funding and higher education. I see ethnography as both process and product, and to be able to capture lived moments of global coevalness in places as

geographically far apart as Uganda and Sweden, I think of fieldwork and writing-up as intertwined and parallel processes, in which research interlocutors are co-authors, not only in the phrasing of their stories, but more, in the very analysis itself. At the same time, the job of the anthropologist is not to absorb uncritically the stories of his or her informants, neither is it for the anthropologist to impose his or her stories upon the informants. Ultimately anthropology is about the informants' familiarity with the world, not that of the anthropologist. Yet such a goal is not always an easy achievement, as will be illustrated—and paradoxically so—by my auto-ethnographic approach. My aim is to acknowledge the importance of assessing critically our own cultural and symbolic regimes, in academia and beyond. Only then can we realize the potential of ethnography to overcome the legacies of our discipline's colonialist, even torturous, epistemologies (Whitehead 2013; Whitehead and Finnström 2013; Finnström 2015).

Anthropology rests on firm empirical ground, a disciplinary emphasis on fieldwork established by Malinowski and his contemporaries. The ambition today, however, is not really to search for final answers to the equally final questions asked, which would be a kind of parallel with the mapping of the allegedly disappearing cultures so important to Robert Murdock, and indeed, to Franz Boas and his students, notably Margaret Mead and Ruth Benedict. Rather, what anthropologists ought to do is to ask questions that will enable further and, it can be hoped, better informed questions. Such anthropological openness, serendipity, and directionality yet without any preset direction, or a kind of never-ending conversation, does not really pay off with the big research agencies of today. As a matter of fact, all I have said so far goes against the whole idea of "a project," framed by its clear beginning and its even clearer end. Again, as long as we remain uncritical of our own managerial regimes, while we still desperately try to get a piece of the funding cake with applications that we think are catchier than ever before, most projects that we eventually run, framed as they are and must be, will circumscribe the very openness that is ethnography's potential. In other words, if anthropology is coopted by forces whose agendas we not necessarily share, anthropology is again at risk of reproducing its colonialist legacies.

THE LONG CONVERSATION

In 2003 I defended my Ph.D. thesis on a war with northern Uganda as its epicenter, one of Africa's most violent conflicts. After a publishing house in the UK had invited me to submit a manuscript but then withdrew their initial interest, citing a limited market for my suggested book, and one in

Uganda just went silent, I was given a third opportunity to rework the thesis and have it published with Duke University Press. So I did (Finnström 2008). For this accomplishment I am indebted to, among others, my fieldwork co-workers Anthony Odiya-Labol and Otim p'Ojok who took turns to visit Sweden to offer feedback and take part in the rewriting process (see Finnström 2010b). Their visits connected Uganda with Sweden, and my field notes with the interpretations of my research co-workers. It was part of what Maurice Bloch—in an appreciation of Malinowski's keen interest in the everyday, that is, the sacred *and* the profane, the spectacular *and* the mundane—has called “the long conversation view of society” (Bloch 1977: 286; see also Brandström 1990). In the process I sacrificed, or at least postponed indefinitely, the time originally dedicated for writing up a postdoctoral project, but this too I like to view in light of the long conversation.

When I was carrying with me the symbolic capital of a Ph.D., a few articles and a published monograph, a new phase in my academic life started—that of submitting job applications and more, of writing research proposals. True, about halfway through my Ph.D. research, I managed to attract funding needed for finalizing my Ph.D. At one point during my Ph.D. studies, not yet knowing that my proposals would actually land me the necessary research funds, I made a calculation out of frustration, and concluded that I had so far spent six months full-time only writing applications, out of a Ph.D. program that in the Swedish system of higher education is supposed to take no more than four years, fieldwork included. I say supposed to take because there are ways to somehow get around these rules. If nothing else, life itself will intrude on the authoritarian rules, originally designed for strictly regulated research carried out in some kind of natural science laboratory, but universally imposed on the humanities and social sciences as well. This remained a theory of the policy makers, I found out from experience: I was accepted for the Ph.D. program in 1996, went to Uganda for the first time in 1997, got proper funding starting from 1999, and defended my Ph.D. in 2003, making a total of seven years. Yet here I was, about halfway through the Ph.D. program, when my first major application was accepted in late 1998. Besides a salary for the coming four years, it covered traveling and fieldwork costs, even if I was soon to find out that Swedish standards were different from those of some fellow graduates from the UK or the US, who, often under the umbrella of some non-government organization, seemed to have the costs of renting whole houses and four-wheel-drive vehicles covered. I moved around on foot, bicycle, and by public transport, and in 1999 and onward with the help of a small secondhand BodaBoda motorbike that constantly had to be sweet-talked with. But with my co-worker Tonny Odiya-Labol as navigator and supervising mechanic, this bike took us all over the

place. In the midst of an ongoing war, it was hard work. At times, after long hours on the bike on bad and bumpy rural roads, it was literally a pain in the ass. In a story fit for the *Leatherman Tool Tales* webpage and in using an old bicycle tube, we once tied our multi-tool knife to the broken gear pedal. Deep in war-torn rural Uganda, it saved us when no welding was available. Such experiences, even if most often much less dramatic, kept me in contact with the everyday in the shadows of war, and emerging was a truly holistic field-work habitus that could hardly have been outlined in any research proposal. In short, Tonny the social navigator gave life to my proposal and made sure to have me in daily contact with for him urgent Ugandan realities.

When I think back, it somehow reminds me of Jack Goody's story about his receiving of a grant from the Colonial Social Science Research Council to work in Ghana. "The offer of motor transport," writes Goody, "was vetoed by my mentors at Oxford on the grounds I would have less contact with the people with whom I was working and would be unable properly to appreciate their concepts of space and time" (Goody 1995: 153). It was suggested that the mobility that a four-wheel-drive vehicle offers in these parts of the world would make the anthropologist too mobile, moving up and down all the time, and thus less in touch with the lives of the people he or she is supposed to study. For me, it was more a question of money, or rather the lack of it, so the idea of buying a vehicle never really crossed my mind. In retrospect, this was nice. In a war-torn place where white-painted neutral-flagged humanitarian four-wheel drive vehicles were speeding up and down the roads, with the white expatriate always seated next to his African driver, I soon became known in town as the (only) white guy who walked, even if people often complained over what I take to be my non-Ugandan way of goal-oriented walking, more or less straight from A to B, leaving one meeting for another. Embodied as it was, I painstakingly had to unlearn this, if the rumor of me as a US marine was not completely to take root.

BEING SOFT, BEING HARD

Surely the intersubjective encounter, so important in anthropology, becomes difficult if a windscreen—or a questionnaire or a predetermined hypothesis for that matter—seals off the researcher from the informant. I am not objecting to the importance of multi-sited or a more transnational anthropology, indeed important in our times of globalized movements, refugee flows, and increased cosmopolitanism. I think that the fact that I invited Tonny Odiya-Labol and Otim p'Ojok to Sweden testifies to this. Nor am I promoting some kind of colonial nostalgia. Rather I want to be attentive to life's open-

ended conversations. I recall a note from rural Tanzania, a hidden piece of advice found in the writings of my supervisor, Per Brandström, beautifully addressed simultaneously to readers and the writing anthropologist:

In his darker moments the anthropologist often envies his colleagues in those social sciences that boast more sophisticated field techniques. The only time he ever sees them is when they make hit and run visits to the village. They appear early in the morning in their Land-Rover in a cloud of dust. They know what they want, go about their business and are gone long before sunset. Only their field assistants remain behind, and briskly and without wasting time they select their informants according to the sampling schedule, take out their questionnaires, and within one or at the most two days these have all been filled in down to the last square with nothing but “hard” data. In the not terribly cool shade of the tree, the anthropologist vows by all that he holds holy to dedicate his next life to the pursuit of only “hard” data and to forget all his ambitions about the “soft” ones. (Brandström 1990: chap 7: 2)

With Brandström’s thoughtful mentoring, I eventually completed my Ph.D. And as I decided to try to remain in the academic arena and in my ambition to remain anthropologically sane, continuous conversations with him have always remained essential. While surviving academia by way of temporary teaching contracts, my file of rejected applications and proposals constantly grew. When the serendipity of things in late 2011 did land me a senior lectureship in anthropology after some years on the road, which among other things included a spell with Stockholm University, I was tenured at Uppsala University and the very department from where I first set out on my academic journey, a situation that seems all too common in the Swedish university system. With a job secured, I could also see that the job folder on my computer houses some thirty-seven major applications and proposals, all rejected. In other words, perhaps cursed by Swedish academia eventually to return to Uppsala, it gives my anthropological career, so far, an air of failure, of surrender. I have been moving in a circle, and I realize that my professors were right in an insight for which I silently castigated them back then when I was doing my Ph.D. and that I curse still today: they pointed out that there will never be as much time to dedicate to anthropological research and writing, and indeed, to contemplation, as there is when one is doing one’s Ph.D. I wonder if Malinowski felt the same.

I should not only complain. I can also tell that my job folder lists some twenty-seven smaller applications, all successful. Even more, I have to confess that this subfolder of supposed success also lists one application for a postdoctoral studentship and one for a senior lectureship, but these two I eventually turned down. Evolving here was not the flow of anthropological

serendipity that I was searching for, because generally, it seems to me, in a catch-22-like development one such golden opportunity tends to exclude another, or vice versa. If nothing else, life intervenes. Anyway, the folder mainly consists of applications for traveling and conference grants, a laptop, language editing grants, and grants that made it possible for me to invite Ugandan co-workers, guest lecturers, and visiting professors, despite the fact that I have been without any faculty position.

Some success, I have to admit. Yet there is a question that never seems to fade fully from my anthropological being: what if I could have redirected my attention away from writing all those applications, every single one meticulously formatted according to the specific instructions of the various employers and research agencies? Also, my CV had to be reformatted over and over again, to fit the online computerized and thus completely standardized application forms. In a sense, this is also a form of creative writing, as I always tried to secure at least some little anthropological openness to it all, but in the long run it is also a form of intellectual prostitution that risks shutting down the openness that I struggle to nurture. Anthropological creativity is traded for square-minded creativity, if there is such a thing. For example, in ticking the accurate box of preset focuses listed on the cover sheets of the applications, the very box that assumedly would specify my focus, I was circumscribed. Anthropology was circumscribed. Literarily speaking, I found myself thinking *inside* rather than *outside* the famous box.

THE POLITICS OF REJECTION

What if instead I had spent the time writing the real thing? I am thinking of research articles, scholarly and popular, reflective essays and newspaper opinions, even a second monograph perhaps, basically genres Per Brandström encouraged me to try out. My argument is that all that time I have spent writing proposals, and formatting them according to all kinds of guidelines that do not necessarily have anything anthropological to them, has had profound influence on my anthropological being. Take my 2010 application to the Department of Research Cooperation of the Swedish International Development Cooperation Agency. Among other things, the instructions demanded that I summarize my project's relevance "in accordance with the Swedish Policy for Global Development (PGD)" as well its "relevance as regards gender issues." These motivations, shortened to "PGD and gender" and to be clearly framed in a preformatted box called "Relevance and Gender prespective" were to contain no more than 2,500 characters, blanks included. I am not sure if "prespective" is a misspelling or a new word, pre-

scribed perspective? And what is PGD, if not anthropology reduced and dehumanized to yet another unnecessary acronym (Finnström 2008: 134, 240, 2010a: 223)? Again I was left with the feeling that anthropology was reduced to self-pervasive impression management. And again I was inside a box that was not mine. But more importantly perhaps, such agencies would support knowledge production only if it could translate into policy and then be applied to the benefit of the research subjects, not necessarily in ways that they themselves want but rather in ways defined by the intervention of these very agencies. Anthropology becomes part of the exercise of power yet framed as empowerment or in other positive and allegedly neutral ways (Whitehead 2013: 27–29).

As many times before, I complied and surrendered to the format, but basically I proposed a critical scrutiny of the social and political life of acronyms and other dehumanizing policy devices. My proposal was eventually judged by a fellow academic to be “theoretically innovative.” Also “the planned methodology” was described as “innovative and ambitious.” The friendly reviewer even opened the review by stating that “the project is ambitious and relevant, innovative and theoretically cutting edge.” However, in a situation of little funds available and great competition, research agencies tend to listen more to the critical voices, perhaps in search of an easy way out, a rejection. Thus a second reviewer concluded that “the direct relevance for development processes could be further elaborated upon.” Even more, “Gender issues are important for the study and ought to be further elaborated upon in the project description.” In the end also this reviewer judged it to be “an innovative project.” Yet when it came to my anthropological ambitions, it was simply stated that a scientific assessment was “outside the reviewer’s competence.” Well, I thought for myself, if this is to be a scientific evaluation of the anthropological contribution, why cannot the agency hire someone who can do such an evaluation?

In the end these two assessments were boiled down to a rejection that, while acknowledging that the proposal as “ambitious and relevant,” even “innovative,” still stated that “it is not clear how the findings from the project can be transferred into a regional or international context.” These were the wordings of a research secretary at the agency. For a desk bureaucrat to summarize the evaluations would be standard procedure. Yet the very meaning of this sentence, critical as it is since it paves the way for a rejection, is not completely clear *to me*. It becomes even more strange since the research secretary, in a situation whereby the agency on its side had imposed harsh limitations on how much applicants were allowed to write, repeated these very words in the sentence following immediately thereafter, replacing only “not clear” with “unclear”, and I quote: “Further, it is unclear how the

findings from the project can be transferred into a regional or international context.” End of rejection. All in all, these two sentences, almost identical and linked together in a rather comic way by help of the word “further,” made up more than 50 percent of the short text of the rejection. The individual evaluating statements were sent to me only later, on my explicit request.

In other words, if you do not have anything sensible to say, say it twice. I was perplexed, recalling that a previous application to the same agency was rejected on the grounds that it lacked “more detailed research questions” and was “very argumentative,” as a reviewer had put it. A second reader had described this proposal as “daring,” with a “political provocativeness” that was “refreshing,” concluding that it deserved to be supported. However, also this time the agency had followed the critical voice. These are but the rules of the game, and perhaps I need to surrender to them and also realize that anthropology may gesture toward radical equality while at the same time remaining inherently structured in ways that preclude any such equality.

LOST IN TIME AND SPACE

Somewhere here anthropology was lost to me. With the thirty-seven major applications and proposals filed in my folder of rejected submissions, my anthropological being started to crumble, but I also suspect that the anthropological standards of the colleagues of mine who ended up evaluating my proposals are disintegrating in ways similar to what I experience. When (writing and judging) proposals becomes a genre in itself, it seems to have such a force-multiplying effect of mutual corrosion. We are all participants in the process, it seems, together on the proverbial slippery slope. From experience, I slowly learned what my senior colleagues already knew: this game is not about securing anthropological (or even scientific) standards, since the game is no longer about anthropology. Still we all play the game, because just as in Vegas, there might be a jackpot out there. And such a jackpot, rather than any lengthy monograph, will today be a most important proof of your scientific competence. But the jackpot may be that of the bullet of the Russian roulette, a bullet that kills the anthropology of it. I can no longer imagine a situation whereby my university would offer me enough time to develop without restrictions my anthropological skills to have them materialize in thirty-seven articles and essays rather than thirty-seven proposals.

But no, here was a development that actually promoted the proposal as the product. I found myself being subjected to a sudden death, but paradoxically it was a slow death, so slow that I would most likely not realize it before it was too late. As a Ugandan informant living in the shadows of war once

told me, “Do not be surprised if you wake up in the morning only to find your head chopped off” (Finnström 2008: 191). There is no irony here; the old man was dead serious. And his comment was perhaps not that absurd. Indeed, I am also asked to review research proposals. The bullet is coming my way.

At a moment in time when all my research proposals were rejected, I had to focus on teaching. So in a rather dull moment in my brief career as an anthropologist, I turned to the students, the best source of inspiration. And I gave them a reading list with anthropological classics, starting with Malinowski, Mead, Benedict, by way of Evans-Pritchard, Leach, and Lévi-Strauss, to end up with Douglas, Geertz, and Bourdieu. It was truly amazing to see the energy with which they embarked on this old stuff. So, by way of a master course into the history of anthropological theory at Stockholm University I managed to recapture some of my anthropological senses. Even if some students noted that Evans-Pritchard’s (1940) *The Nuer: Description of the Modes of Livelihood and Political Institutions of a Nilotic People* somehow remained a report commissioned by the colonial authorities and directed to their purposes, this very fact offered us a lot to discuss. And it all made me think that Evans-Pritchard’s holistic fieldwork ambitions indeed could be regarded as anthropology purposely lacking the fixity of “more detailed research questions.” This would however be misdirected and, as the philosopher Paul Feyerabend (1993: 188) has shown, a fundamental misunderstanding of the very basics of Evans-Pritchard’s work on the ground, and anthropology more generally. But I also noted that the colonial regime that Evans-Pritchard lived under, and worked for, has been replaced with the development regime of our times, namely, that of “PGD” and other dehumanizing acronyms of the many international development cooperation agencies’ research and policy departments. Our present-day regime is perhaps no less imperial than that of Evans-Pritchard. And, also, perhaps, Evans-Pritchard’s employers tore their hair out when they received his Nuer report on their desks. Indeed, they never contracted him again. Perhaps they too found it unclear how his findings could “be transferred into a regional or international context.” I mean, Evans-Pritchard’s financiers were in the process of conquering the Africans of Sudan, bombing their villages and their cattle, and perhaps the British colonialists too wanted to know why these poor Africans so stubbornly refused the efforts of modernization and development.

When some students rightly said something along the lines of “Gender issues are important for the study and ought to be further elaborated,” I could refer them to the sequel, *Kinship and Marriage among the Nuer* (Evans-Pritchard 1951), or even *The Position of Women in Primitive Societies and Other*

Essays in Social Anthropology (Evans-Pritchard 1965). Like an old-time anthropologist, I have actually produced one monograph of one people, the Acholi. If you like, I hereby embraced a colonial legacy in anthropology. The marketing people of my publishing house even demanded that “northern Uganda” be in the subtitle. Evans-Pritchard, for his part, had the time, skills, and anthropological courage to write no less than three books on the Nuer, the northern neighbors of the Acholi. One of my students actually went home to read Evans-Pritchard’s book on kinship and marriage.

WHEN LIFE INTERVENES

Malinowski taught us a lot of things, indeed, some of which generations of anthropologists meticulously had to unlearn over the years to come. Yet perhaps his best advice for us was to get “off the veranda.” If anthropologists once had been stuck in their armchairs, it was now time for them not only to travel to the so-called field, but also to get their hands dirty. Imagine a research proposal so open that the methods are simply described as an ambition to get off the veranda, leaving the rest in the air, in a sense as it is supposed to be. And what research proposals could be more unfocused than those we can imagine that Evans-Pritchard wrote. He indeed promoted a theoretical and methodological openness that would earn him little money from the research departments of today’s international development cooperation agencies. He sketched this openness in “Some Reminiscences and Reflections on Fieldwork,” a text that eventually ended up as an appendix to the 1976 abridged edition of the classic book on Azande witchcraft: “The anthropologist must follow what he finds in the society he has selected for study: the social organization of its people, their values and sentiments and so forth. I illustrate this fact from what happened in my own case. I had no interest in witchcraft when I went to Zandeland, but the Azande had; so I had to let myself be guided by them. I had no particular interest in cows when I went to Nuerland, but the Nuer had, so willy-nilly I had to become cattle-minded too” (Evans-Pritchard 1976: 242). Evans-Pritchard’s reflections were put in my hands by my supervisor (see also Brandström 1990: chap. 1: 10f), a silent advice to me that appendices and other sidetrack departures, intellectual as well as social, may contain messages of great anthropological importance. But again, to follow Evans-Pritchard’s willy-nilliness is not really what you are supposed to do with your research proposals. I guess it would be deemed as “unscientific.” As Paul Stoller, whose anthropological writings I admire greatly, commented on one of my many draft proposals: “The section of methods is very important as well. Here you need to be concrete

and pay attention to minute detail: types of interviewing and observation; archival work, analysis of data, if appropriate, and as you have included, a schedule of research activities.” But even if one ends up in the field with strict research questions that the informants find highly relevant, the quandary has just begun. The lived realities of the informants will interrupt, and suddenly anthropology, as the intersubjective and dialectical endeavor it is supposed to be, just happens. Again my mentor Per Brandström puts his own thinking into writing, as he contemplates his anthropological being among the Sukuma of Tanzania:

If he studies ritual he cannot avoid taking part in such prosaic everyday occupations as weeding and harvesting. If he studies land use, he must resign himself to whiling away the hot hours of the day during the dry season sitting with the men under the grass roof of the hut or in the shade of a tree and listening to endless and abstruse palaver that deals with every possible subject under the sun but that which he has painstakingly presented in his project proposal. The chances that he will be able to plan and decide how to spend his time are small. A funeral intervenes, and all work in the village is abandoned for three days (Brandström 1990: chap. 7: 1–2).

NEW BEGINNINGS?

In the end, as mentioned, I landed a senior lectureship. I also did get a major research proposal accepted. And suddenly the serendipity of things allowed me to combine these two anthropological tracks. So here I have two confessions to make. First of all, when I was about to finalize this chapter, I did so from the position of having secured a senior lectureship. Secondly, over the two years that eventually landed me the aforementioned grant, I had been hired by a multidisciplinary research center at Uppsala University, with the principal task of initiating research in the form of proposal writing and networking. I should therefore not complain too much. Time was on my side, and besides composing proposals, I was even able to write some stuff less anthropologically compromised. But my efforts to remain true to my anthropological ideas perhaps just prolonged the painful politics of rejection. Existentially I was at a loss, with the nagging feeling that I no longer had a voice in the marking out of anthropology. Still I tried. For example, I opened early versions of the proposal with a narrative that I naively thought of as a smart way to present a case for an anthropology of global war and transnational (in-)justice, of placements and displacement, and that at the same time was firmly anchored in my long-term commitment to war-torn northern Uganda:

I am sitting in a coffee bar [in Uppsala, Sweden], waiting for Olak, one of my research interlocutors. Over the past two years or so, we have met regularly, at periods almost on a weekly basis. When he finally arrives, he is limping, and he walks only with the assistance of a crutch. I know from our conversations that he has some permanent torture injuries in one of his legs, and the Swedish winter climate seems to worsen the pain in the leg. But more, I can also detect a parallel to the developments on the ground in war-torn Uganda, his native country and the subject of my research since more than ten years. When things seem OK in Uganda, with both fighting parties declaring their dedication to end the war with peaceful means, I notice that his leg is better, and this regardless of the Swedish weather. Sometimes, as when the two parties signed a formal cessation of hostilities agreement, he arrived for one of our meetings on bicycle, without the walking stick. We were both elevated, sharing the news. But eventually the Ugandan government launched new attacks on the Lord's Resistance Army rebels, and we both knew the consequences: a lot of unarmed civilians would now again suffer, even die. And the Lord's Resistance Army would again react as the wounded buffalo, as my Ugandan friends often describe it. Nothing is more dangerous than a wounded buffalo. Like a landmine hidden in the soil in northern Uganda, the wounded buffalo seems to strike without any sense of direction, indiscriminately. My friend Olak would again need his walking stick. And the landmine came from somewhere...

A version of the proposal, I have to admit, landed me some good fieldwork money, but as I was preparing the fully financed project proposal, Paul Stoller again provided straightforward guidance. He advised me to skip the opening story. "The last thing they want to read," he told me point blank, "is a kind of narrative, which, they probably think, has no place in a proposal or in an academic text."

So I dropped the opening story. And eventually funds came my way. The research funds allowed me to gather new data, while the lectureship puts me in much-needed contact with new generations of anthropology students. This, so I thought, would allow me to revisit and expand my ethnographic "field," and it would allow me to revisit the writings of Malinowski and other old-time anthropologists as well as those in the making identified by curious and clever-minded students, which then again would force me to reconsider my past readings of my own anthropological heroes. However, I soon realized that less time than ever before was left for my own anthropology and (creative) writing. With the tenure secured, what have I really arrived at, besides a fully booked calendar? There are committees to sign up for, meetings to attend, peer-review work to be done, an ever-increasing mountain of administrative tasks to carry out, and also for me, proposals to evaluate. Robin Wilson summarizes a situation that is, I have to realize, general to academia: "The path to achieving what amounts to higher edu-

cation's golden ring is well marked and includes guidance from more-experienced peers. But once a professor earns tenure, that guidance disappears, the amount of committee work piles on, and associate professors are often left to figure out how to manage the varying demands of the job—and fit in time for their research—on their own” (Wilson 2012). I am still trying to figure it all out. And somehow anthropology continues to be good to me. Tonny and Otim regularly call from Uganda, and I regularly call them. I try to keep up with my research. So I have no regrets. Yet there are days to come, and I can only hope always to remember advising new generations of students to hang around also when the canoe is being built, when life just happens, in liberation of (or despite) any research proposal. Someday the order of things may change. In the meantime we need to continue the critical assessment of our own managerial systems, as we struggle to find ways to avoid any circumscription of the very openness that is ethnography's potential. If not, and as always, we are at risk of reproducing a legacy of colonialist and even torturous epistemologies still persistent in anthropology.

NOTE/ACKNOWLEDGEMENT

Thanks, Per Brandström, for everything. I first aired the frustrations of my anthropological predicament at the Stockholm Anthropology Roundtable in 2009, then again at the EASA meetings in Paris, 2012; my attendance at these two events generously sponsored by the Department of Social Anthropology, Stockholm University. Eventually my thoughts became a paper and if this chapter sketches my efforts to hold on to some kind of anthropological sanity under circumstances when the future of my professional career was highly uncertain, I also want to formally acknowledge that I took it out from my drawer to finalize it while being based at the Hugo Valentin Centre, Uppsala University, working on a project on global war and transnational (in-)justice, funded by the Bank of Sweden Tercentenary Foundation. But I would not have reopened my drawer in the first place had Helena Wulff not been so persuasive, supportive, and wonderfully patient. Again, here is an example of anthropology as the long conversation. Thanks also to Sam Dubal for joining the conversation.

Sverker Finnström is Associate Professor of Cultural Anthropology at Uppsala University. Starting from 1997, and focusing on young adults coming of age in the shadows of civil war, he has conducted recurrent fieldwork in northern Uganda, where the Lord's Resistance Army and other groups have been fighting the Ugandan government. He is the author of *Living with*

Bad Surroundings: War, History, and Everyday Moments in Northern Uganda (Duke University Press, 2008), for which he was honored with the Margaret Mead Award, and he is co-editor of *Virtual War and Magical Death: Technologies and Imaginaries for Terror and Killing* (Duke University Press, 2013).

REFERENCES

- Bloch, Maurice. 1977. "The Past and the Present in the Present." *Man* 12, no. 2: 278–292.
- Brandström, Per. 1990. "Boundless Universe: The Culture of Expansion among the Sukuma-Nyamwezi of Tanzania." Ph.D. diss., Uppsala University.
- Evans-Pritchard, E.E. 1940. *The Nuer: A Description of the Modes of Livelihood and Political Institutions of a Nilotic People*. Oxford: Clarendon Press.
- . 1951. *Kinship and Marriage among the Nuer*. Oxford: Clarendon Press.
- . 1965. *The Position of Women in Primitive Societies and Other Essays in Social Anthropology*. London: Faber and Faber Ltd.
- . 1976. *Witchcraft, Oracles, and Magic among the Azande*. Abridged with an introduction by Eva Gillies. Oxford: Clarendon.
- Feyerabend, Paul K. 1993. *Against Method: Outline of an Anarchistic Theory of Knowledge*. Third Edition. London & New York: Verso.
- Finnström, Sverker. 2008. *Living with Bad Surroundings: War, History, and Everyday Moments in Northern Uganda*. Durham: Duke University Press.
- . 2010a. "Review Article: Political Bodies, Local Realities and Institutional Structures of (In-)justice." *Social Anthropology/Anthropologie Sociale* 18, no. 2: 220–224.
- . 2010b. "The Tide of War and the Mango Trees of Uganda: On Receiving the 2009 Margaret Mead Award, México, March 2010." *Multiethnica* 32: 17–19.
- . 2015. "War Stories and Troubled Peace: Revisiting Some Secrets of Northern Uganda." *Current Anthropology* 56, no. S12, pp 000-000.
- Goody, Jack. 1995. *The Expansive Moment: Anthropology in Britain and Africa 1918-1970*. Cambridge: Cambridge University Press.
- Whitehead, Neil L. 2013. "Ethnography, Knowledge, Torture, and Silence." In *Virtual War and Magical Death: Technologies and Imaginaries for Terror and Killing*, ed. Neil L. Whitehead and Sverker Finnström, 26–45. Durham: Duke University Press.
- Whitehead, Neil L., and Sverker Finnström. 2013. "Introduction: Virtual War and Magical Death." In *Virtual War and Magical Death: Technologies and Imaginaries for Terror and Killing*, ed. Neil L. Whitehead and Sverker Finnström, 1–25. Durham: Duke University Press.
- Wilson, Robin. 2012. "Why Are Associate Professors So Unhappy?" *The Chronicle of Higher Education*, 3 June. <http://chronicle.com/article/Why-Are-Associate-Professors/132071>.