

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.

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