THE FIGURE OF THE THIRD IN THE FIELD

Richard Rottenburg

This paper uses a personal experience to investigate one well-known methodological dimension of anthropological study: the fact that anthropological observers are themselves always observed. Anthropologists often receive visits in the field from fellow anthropologists and/or supervisors. Such visits can result in significant disturbances for host anthropologists, which in turn cause them to engage in self-reflection about the new identity they have acquired during their fieldwork. Similarly, the very act of writing field notes necessarily implies a fictive figure of the third. This figure functions as a guide for anthropologists as they drift on a sea of subjectivity and struggle with the threat of sinking into idiosyncrasy. The realization that anthropologists possess the capacity for sceptical, self-reflective awareness about their own role as anthropologists—a process initiated by the figure of the third—in turn shifts attention to an analogous capacity among those who constitute the objects of anthropological study. I argue that anthropological observation necessarily induces a similar level of sceptical self-reflection in the people observed by anthropologists. The recognition of this mutual capacity, I conclude, has consequences for the way in which anthropologists collaborate with their interlocutors to spin new fabrics of meaning about the situations they are in.

Field research is, as we all know, a delicate and uncertain practice despite methodological attempts to objectify it. This is primarily due to the fact that field research requires participant observation, which means 'drifting on a sea of subjectivity' (Herdt and Stoller 1987:177) and is therefore always an intimate experience for the researcher.
Anthropologists, as a rule, would like to conceal this intimacy. This desire is connected with the fact that they cannot make their observations in the field undisturbed. In the first place, they are themselves observed by their hosts, i.e., by the very people they are observing. This strange perspective often brings to light the anthropologist's own idiosyncrasies, which would have remained well hidden at home thanks to well-practiced and long internalized ways of acting. In addition, participating in strange life-forms means that field researchers have to learn new ways of acting, adopt new role expectations, and commit themselves to new loyalties and friendships; at the same time they also have to intervene actively in order to evoke reactions. At times this engagement aims more at blending and fitting in, at other times more at dissonance and friction. The way in which researchers in the field engage in this ambiguous process—their particular adaptations and interventions—also tells us something about the researchers themselves, even if only in an attempt to formulate a joke in the idiom of the field. Very often what we learn is not entirely flattering.

Finally, the diverse practices required by field research at times violate researchers' own deep-seated norms and values, which can cause irritation or desperation and lead to overreactions. I leave aside here the question of whether such overreactions are actually detrimental to participant observation, as is usually presumed. In any case, they do reveal something about the field researcher's own normative orientation that would otherwise not have come to light. The unavoidable intensity of this kind of self-experience in the field often has something existentially threatening about it.²

If field research in this sense means moving from one gaff to the next, exhibiting disconcerting weaknesses, exposing ourselves by adapting to strange ways of life, and permitting ourselves unusual inconsistencies, it is hardly surprising that field researchers would like to remain unobserved while making their observations. This desire, however, is itself suspect. It evokes not only methodological doubts but general irritation because it openly applies two different standards, one for anthropologists and another for their objects of study. For most anthropologists, it is inconceivable that their own prerogative—to observe other people as they engage in every possible activity, recording daily actions and occurrences as minutely as possible—also be directed at themselves.

However, when individual anthropologists abandon this reserve we often suspect that they are no longer engaging in 'real' field research. The implicit argument is that if anthropologists allow others to observe them in the field, they won't be willing to risk their own identity. If they aren't willing to risk their identity and continue to cling to established orientations, they will close themselves off to unexpected, de-centering experiences and perspectives. And whoever closes themselves off such experiences and perspectives will never discover anything of significance. (If this is the case, then the converse must be true as well: Many field researchers do not permit others to look over their shoulder simply because they fear that such openness might raise the suspicion that they were not engaging in real field research.) According to this line of argumentation, the issue of 'real' field research—or less dogmatically, the issue of the kind of field research—cannot be resolved by observing researchers in the field. We may indeed draw speculative conclusions from published texts, but such speculations are better kept to ourselves.

Given that there are many different kinds of appropriate and successful field research depending on the field, the subject, and the personality of the researcher, we might believe—and many anthropologists presumably do—that the idea of one real or correct kind of field research is misguided. For this reason, it would be more adequate to raise limited and more precise questions about the particular methods a researcher has employed. In at least one respect, however, this alternative merely avoids the issue. Whatever the methods might be, it is always human beings who apply them; and in anthropology, field researchers use themselves and their own bodies as instruments of research.³ The fact that almost all anthropologists fervently believe this—even if they might appear to treat the matter with indifference—is evident if we turn to what is universally regarded as the most serious accusation that can be directed at an anthropologist.

A natural scientist can be discredited by the claim that he has manipulated an experiment. At least in principle the truth should prevail here sooner or later, since research notebooks are shared with other laboratories in order to facilitate the repetition of experiments.⁴ An empirical social scientist can be slandered by the claim that he has manipulated his statistics. Here too—alas!—analogous to the natural sciences—it is both possible and necessary to calculate the same data anew. In the discipline of anthropology, in contrast, the sharing of field notebooks or diaries is not suited for the corroboration of knowledge. Indeed sharing an anthropological field notebook (whether through publication or public archiving) raises the question of whether a field notebook that is publicly presentable can be a good field notebook at all.⁵

All of this means that we can most effectively discredit an anthropologist by suggesting that he or she did not engage in 'proper' field research. There are a number of relevant categories that can be invoked in making an accusation of this kind: linguistic competence, length of residence, type of housing used in the field, work with field assistants, and the paying of informants. The central point here is that the anthropologist accused cannot
really refute such accusations. While he is usually able to correct or relativize the individual charges, he is often no longer able to completely erase the suspicion after it has been raised. The reason for this is that the categories invoked here refer to a deficiency in the anthropologist’s intimate participation in the research process, and this intimate domain by definition possesses no definitively verifiable criteria.

In short, when one anthropologist visits another in the field, it is often a stressful encounter. The decisive issue here is whether the field researcher receiving the visit is able to establish an equilibrium of identity. During his research before the visit, this anthropologist has appropriated and embodied a situationally adapted identity, often through a difficult and protracted process. To a certain extent, he has become a different person. As a rule, this self-externalizing is possible only by assuming opinions and ways of acting that the anthropologist would reject outside the field. However, this is not a purely tactical pretending, but rather what we might call, drawing on Goffman (1961/1990:188ff), a secondary adjustment. In the field the anthropologist plays a new role, one that might appear outlandish or at least inappropriate from the perspective of his own culture or cultural institutions. After a while the anthropologist begins gradually – contrary to his original intentions – to appropriate these opinions and ways of acting until they finally become completely familiar to him. If he now receives a visit from someone who shares his own primary cultural affiliations, the visitor’s gaze can bring to light this appropriation or secondary adjustment, thereby making both parties aware of it. The anthropologist receiving the visit feels as if he has been caught acting duplicitously. This becomes particularly and painfully evident in the fact that laboriously established friendships with his hosts (i.e., his objects of study) suddenly appear threatened because a ‘brother’ from home stands at his doorstep and his mere presence involuntarily asserts the anthropologist’s primary adjustment.

In the spring of 1981, Bernhard Streck paid us – Gisella and I – a surprise visit in the field on Mount Lebu in South Kordofan in Sudan. We had moved into a new house of our own shortly before and had spent the previous seven months doing field research. Bernhard arrived with an American colleague who left that same evening, while Bernhard himself stayed for two nights. My pleasure at seeing a good friend was not initially diminished by the fact that Bernhard involuntarily became for me an observer of the most intimate aspects of my own role as an observer, thus assuming the figure of the third. This, however, changed over the course of his visit.

I had spent that morning under a shady tree with my friend Jenson in front of his house, which provided a good view of our neighbourhood on the mountain slope. Most of the men were not in the village that morning, as it was the season for fixing roofs, which required straw from the plains and branches from the forests of the high plateau. Jenson and I had spent the previous day gathering roof materials and were now resting in the shade, making rope. Most of the women sat in front of houses and shelled beans or did other housework; a number of them were threshing grain. Suddenly all of the women ran into the houses and dressed as if they were going to market. A grandmother from the neighbourhood hurriedly borrowed a dress from Jenson’s first wife, Sara. When we asked what was going on, we were told, Lemau ten.

‘Strangers are coming’ is a warning cry that has resounded on Mount Lebu for centuries at the approach of light-skinned travelers. Because these strangers were often superior in force and sought to establish political domination – e.g., the Turkic-Egyptian and later British colonial administrations – the cry Lemau ten expressed something similar to the call ‘the Cossacks are coming’ in a Jewish street in Galicia. The first of these strangers here were probably the Bagga (cattle herders), who have dominated the plains of South Kordofan since the beginning of the 18th century and even today continue to embody the Lemau. While in March 1981 this call had lost much of its existential terror, it remained a warning that marked the transgression of a boundary between inside and outside. In spatial terms, this boundary was altitudinal: Only the inhabitants of the nayou (the high
plateau of Mount Lebu) were authorized to visit the houses on the mount-
ain.

On March 30, 1981, two *lemwu* arrived accompanied by a guide from
another hill community. They entered the *naye* uninvited and unauthor-
ized, thus causing something of a sensation. During the three years we
lived on Mount Lebu there were only two visits by strangers, and both of
these were associated with us. It was not all that easy to reach the high
plateau. First of all, you had to know that there was even a settlement here,
for it was not visible from the dirt road that ran through the plains below
and was separated from it by a difficult forty-five minute hike. If, however,
you knew a settlement existed, as Bernhard had learned from our letters,
and you asked about it at the market place in Umm Dorein, the Jella-
ba traders would have told you (at the beginning of the 1980's) that it
was next to impossible to scale the steep mountain and that there was
nothing to see up there other than primitive infidels if you did. And even if
you made it past these hurdles, you still had to find a guide to accompany
you on the journey, as the path itself was hardly visible, branched off re-
peatedly, and often lead over smooth blocks of granite on the extremely
steep mountain slope. Anyone with less persistence than Bernhard would
probably have abandoned the idea. Yet here he stood on our doorstep with
an American companion and a mountain guide from Umm Dorein.

After the initial greetings, we sat on our neighbour Aicigeldi's shaded
porch, as our own house did not have one. The women, now dressed for the
visit, came in groups of two or three to greet us quickly before going off to
help Gisella prepare the food and drinks. The adolescent girls who brought
us water quivered with excitement, retreating immediately after handling
over the pitcher, while inquisitive children glowered at a safe distance at their
inaptitude. Aicigeldi looked on in astonishment when the American visitor
refused to drink the water from the pitcher, instead pulling out a metal
water bottle from his knapsack. While Aicigeldi was probably merely aston-
ished, I shook with patriotic anger thanks to my secondary adjustment:
This man comes to visit us but refuses to drink our water! After tea, Gisella
and Sara brought us milk porridge with green sauce, which Bernhard and
the American refused with embarrassment. Fortunately the mountain guide
are enough for three, so that the two guards did not have to be returned
untouched. Following the meal he did not eat, the American anthropologist
started on his way back with the guide, so that he could drive his Landrover
(which was parked in Umm Dorein) to Kadugli before sunset.

After the noonday heat had past, Bernhard said that he would like to take
a walk through the village and investigate the surroundings. I was busy
trying to figure out why Jenson had retreated into his house and had not
even come over to greet the visitors, although he and I had sat together the
entire morning. In accordance with the typical hospitality on Mount Lebu,
I would have preferred to keep Bernhard within the inner courtyard of our
house compound. However, given the rules of our friendship and colle-
giality - i.e., the code of my primary adjustment - this would have signaled
that I wanted to deny Bernhard something that anthropologists are never
supposed to treat as a private possession: their research territory. For this
reason I consented, and we walked together through the village. Without
knowing why I felt apprehensive. The reasons quickly became clear.

All of the initial difficulties Gisella and I had experienced two years ear-
lier were repeated during Bernhard’s visit. Children ran in hordes behind
us; women rushed into their houses to get dressed; men questioned us
about the purpose of Bernhard’s visit. Everyone insisted on speaking Arabic
with me, rather than trying to communicate in Dhemwareng, the local lan-
guage, as they had done before in order to help me learn it. Walking
through the village with Bernhard, I had suddenly become a stranger again.
The warning call we repeatedly evoked during our walk through the village
was directed again at me: *lemwu leto*. Everyone now seemed to take for
granted that my primary loyalty was to Bernhard, my ‘brother’, and not to
them, my neighbours and hosts. In particular, I heard the same tiresome
questions that Gisella and I had been asked repeatedly during our initial
months here in 1979: Were we the advanced guard of a larger invasion?
Were we searching for mineral resources? Or were we fugitives on the run?

I became more and more uneasy. Without being aware of it at the time, I
over-identified with the basic emotions of my hosts expressed in the ancient
warning cry *lemwu leto*. I felt increasingly that our walk was a violation, a
sign of disrespect, as if a group of loose acquaintances had assembled on
their own accord in my Berlin apartment for an inspection of the living
quarters, including bed-and-bathroom. At some point Bernhard pulled a
camera out of his pocket and began to take pictures. One of my neighbours,
the target of these photographs, protested to me, this time in Dhemwareng,
a language Bernhard obviously did not understand. I turned to Bernhard
and issued an angry reprimand. I told him to put the camera immediately
back in his pocket, which he did without complaint.?

Embarrassed by my own anger, I explained to him the history of the ban
on photography that Gisella and I had placed on ourselves. When any
of the inhabitants of Mount Lebu wanted pictures of themselves, they went
to a photo studio in the city and had themselves photographed in unnatural
poses wearing urban clothes in front of a studio background of their choice,
e.g., the interior of a luxurious villa. It would never have occurred to them
to have themselves photographed wearing normal clothing engaging in the
mundane activities of their everyday lives on the ngen (hill). If a stranger, however, were to do this, they would have regarded it as an intolerable intrusion. I had no doubt about this in 1981. It would have been as if I had invited a friend in Berlin to dinner, who brought along an American I didn’t know, and the latter had pulled out a tape recorder at some point in the evening so as to have an accurate record of a dinner conversation in Germany. This is how I had interpreted and internalized the position of the host regarding photography at the time. For this reason, I thought it was necessary to vehemently defend their rights as well as my own standing among them.

When we had finally weathered the awkward walk through the village and were sitting again in my house, the men of the neighbourhood stopped by one after the other to greet the visitor. Only now did Jemson come over. A gourd of beer was sent for from a neighbouring house, where a working party was taking place. I began to relax again and enjoy the visit. This relief, however, was short-lived.

My neighbour Akunene, who dealt with his own apprehensions in a curiously inverted manner and had overcome his anxieties about Gisella’s and my presence on Mount Lebu only slowly and hesitantly, reverted abruptly to his old ways. Did Bernhard have a camera, he asked, in order to prove to his wife in Germany that Richard and Gisella had moved to Mount Lebu? And had Richard already shown him the hidden water reservoir in the stone crevice? Akunene was effusive, treating Bernhard with an exaggerated familiarity that everyone present – with the exception of Bernhard – interpreted as a kind of defensive distancing. My best friends, Jemson and Ngaldo, were embarrassed and uneasy. While they politely struggled to make conversation, Bernhard was after all someone Gisella and I had acknowledged as a friend – they could hardly disguise their real concern. They wanted to know in particular when Bernhard was planning to leave? And who was the second white visitor and what did he have to do with the government, for how else could he drive his own Landrover?

Finally, Hakim, the sheik of the village, came to see us. He brought a thermos of tea – which signalled that I was supposed to introduce him to Bernhard in his house as well –, three books in English (Hommer, Shakespeare, and Orwell), and a colorful brochure from Sudan Airways. Like all of the adults on Mount Lebu at the time, Hakim was illiterate (although he did send his three sons to school, and they constituted a quarter of the pupils on Mount Lebu). He passed the books around, and I was forced to look at the airline brochure with him and comment on it. This was a game we had often played in 1979, and when he finally stopped I felt that I had lost the stigma of the stranger, with whom one discusses only strange and distant matters. I was now compelled to play this game again in front of Bernhard because Hakim wanted to demonstrate his knowledge of the outside world.

At dusk Gisella and I noticed that Bernhard had eaten nothing the entire day, because he was not partial to millet porridge. I organized a chicken, the neighbours, slaughtered it, gutted it with Bernhard, and Gisella prepared it. The three of us spent the evening together conversing about life beyond Mount Lebu. The second day of Bernhard’s visit proceeded in a similar fashion. Again there was only millet porridge and cooked beans with peanut butter to eat, so that Bernhard probably embarked on his return journey with an empty stomach. On the morning of April 1, I accompanied him down to the dirt road, where we waited until the early afternoon for a passing truck to take him to Kadugli.

Long after Bernhard’s departure, the people of the village talked about his unusual visit. On that same day, Jemson told me that I was right not to have shown Bernhard the secret water reservoir up on the mountain. Gwajja, a healer from the neighbourhood, told Gisella several weeks later that while it was good that Gisella and I lived on the mountain, we should not bring anyone to live with us. My neighbour Telefoon, whom I occasionally brought rifle cartridges from Kadugli, joked in his peculiar sense of humor that he would shoot dead the next visitor. Akunene, who had invited Bernhard to move with his wife to Mount Lebu, asked me months later if I could be certain that Bernhard had actually made it back home.

I, too, was preoccupied with Bernhard’s visit for a long time, and only gradually did I realize its significance.

For an anthropologist, recording events and occurrences in a field notebook is an attempt to work through experiences in the field, including non-linguistic ones. The author of such a notebook, however, is aware that he is ‘drifting on a sea of subjectivity’ and is in danger of ‘sinking into idiosyn- crasy’ (Herzi and Stoller 1987:177). It is against this danger that he writes. He weaves together different narratives, which connect and make sense of his various experiences and which are intended to protect the coherence of his own identity. In this process, which often assumes rather idiosyncratic forms, the author constructs one or more tertiary figures for his own orienta- tion.10

When one anthropologist visits another in the field, it is as if one of these tertiary or third figures suddenly stepped out of the pages of his field notebook and into reality. This figure then assumes, so to speak, a life of its own: It offers resistance, intervenes willfully, and invokes surprising expectations. In assuming a life of its own, it seriously disrupts the already precarious constellation existing between the anthropologist, his objects of study, and
the fictive figure of the third in the text. If everything goes well, a creative confusion ensues, which leads the anthropologist to engage in new forms of self-reflection and transforms the process of secondary adjustment into a research method.

Bernhard’s visit made clear to me how fragile and contradictory my own role in the field was. When Hakim arrived to greet the stranger with his thermos, his English books, and his brochure from Sadan Airways, I actually had to rejoice. In using the brochure to shift conversation away from the immediate realities of the local life-forms and his own personal concerns (which would have greatly interested Bernhard), Hakim shielded the inner world of Mount Lebu. In doing this, he drew a boundary between Bernhard and me, as he had already begun months earlier to discuss openly with me issues that actually moved him. But in contrast to the ban on photography that I had enforced, I found Hakim’s shielding through selective issues and language somehow inappropriate at the time. Evidently I was uncomfortable with the fact that Bernhard might believe that I had sat around Mount Lebu for seven months conversing about airline brochures with the local people in Arabic. I would have far preferred that we had discussed a local issue in Dhemwareng, so that I could have presented Bernhard a small sample of ‘real field research’. At that time there was a lot of debate about who actually owned the land on which our house was built and who was entitled to let us live there. There must have been a dozen similar issues, but we talked about airplanes.

By observing my own inconsistencies and the discomfort I felt as a result of them and by reflecting on Hakim’s conversational strategy – both set in motion by the figure of the third observing us – I realized that decoupling a protected inner world located on theephy (hill) by no means implied the exclusion of all external worlds. Rather the people of Mount Lebu actively incorporated selected dimensions of the outside world into their own world. After Bernhard’s visit, I continued to follow this issue with heightened awareness. It became a central thesis of my own anthropological work about cultural accretion or hybridization, as it is called today.11

Bernhard’s visit brought to my attention the pitfalls of my own secondary adjustments in the field. This, in turn, sensitized me to the mechanisms of reciprocal role taking between anthropologists and their objects of study. Today I would argue that a chronic weakness of anthropological studies is that they tend to ascribe the competence and capacity of role taking to anthropologists alone. In this way they surreptitiously establish erroneous distinctions regarding role taking, skepticism, and self-reflection. While anthropologists must by definition master these capacities, the objects of anthropological studies are frequently denied them.12 Conventional anthropology tacitly presupposes this asymmetry, which allows anthropological texts to be constructed as the representation of a strange fabric of meaning, in which the others appear as fixed objects. In reality, however, these self-spun fabrics of meaning always include a substantial amount of skepticism and self-reflection. When anthropologists and their informants collaborate in weaving together new fabrics of meaning, the capacity to see through the other and thereby to relativize themselves through skepticism is present on both sides.

While Bernhard’s visit first made me aware of this issue, it was Jemson who made it practically clear to me two years later. In March 1983, our then 10-month-old daughter Miriam was sick and often cried for long stretches of time. The houses in our neighbourhood were so close to each other that we could all easily hear the crying of children, the scolding of husbands, or the beating of bug-ridden beds, particularly in the still of the night. Jemson and Sara were our immediate neighbours. One night, when Miriam again couldn’t sleep and had cried for hours, Gisella walked with her out in the moonlight, while I slept in the men’s quarters of our house compound. I was awakened suddenly in the middle of the night by an angry Jemson, who informed me that it was now my turn to look after my own daughter. I did as I was told and felt ashamed.

This was a notable intervention on Jemson’s part because he himself would never have acted this way in a comparable situation. As long as children are nursing, they always sleep in bed with their mothers, who are responsible for caring for them at night; if need be, older siblings or neighbours’ children provide assistance. Although I suspected that Jemson had referred indirectly to our different role definitions in earlier conversations, I asked him explicitly about his nightly intervention the next day. He said that while he had never carried his own children around, he had noticed that Gisella and I organized many things differently than he did. He added that he found many of our rules very good.

When Jemson confronted me at night with my own rules, he was well aware that I possessed different rules than he did. In order to intervene in this way, he had to believe that our respective rules were not absolute and therefore non-transferable. This, in turn, meant that we were capable of reflecting on, manipulating, and altering our ways of acting through mutual interaction. And this is precisely what we did. The fact that I conversed on a daily basis with Jemson for three years between 1979 and 1983 and that we became good friends was possible only given the presupposition that we each recognized our own rules as different, but that we also found a common ground on which we were able to reflect together to a certain extent ‘from outside’ about both sets of rules, about their inconsisten-
cies, their points of intersection, and their insufficiencies. Even if we had no epistemological foundation for this common ground, we were nevertheless able to establish it pragmatically. In any event, at the beginning of 1983, when my knowledge of Dhenwareng was sufficient, I discussed fundamental issues of life with Jemson in a way that did not differ essentially from the way in which I discussed such issues with Bernhard.

NOTES
1. This article, which is a rearrangement of my contributions to the volume for Bruno Stock of 1983, is a reflection on personal experiences in the field. It is not intended to be a systematic ethnographic observation, but only as an investigation of one dimension of the issue. For this reason, I do not discuss the discrete positions found in contemporary ethnography. The terms of this text here. Stocking (1983) addressed the matter some twenty years ago. Much of the debate later focused on the writing of anthropology: this dimension of the issue, however, is not relevant to my article.
2. This extensive discussion of field notes does not differ if we define field notes as bare text in terms of a second socialization than in terms of communication, as Wagnier proposed (1983).
3. Behrend (1989) offers an elegant example, both Odeuro (1980) and Hunt and Stoller (1987) provide theoretical explorations. I will not discuss the matter further here.
4. The gap between the reproduction of scientific experiments and the naive yet everyday understanding of science is exploited well in Collins and Fort (1990).
5. In this note, the issue is to distinguish a practical level between well kept and less well kept field notebooks. This distinction, however, is not relevant to the issue addressed here.
6. Gibbons's techniques for the management of quoted identities (1986) can be used to describe what occurs here between the "induced" field research and his observer.
7. We know from literary criticism that a narrator requires the figure of the third, i.e., that of the narrator, in order to be a narrator at all. For a more detailed examination of this issue, see the textual analysis of my story "Winogrod's Fingers," in chapter 3 of this book on pages 19-32 (Remmers, 2002). The fact that I was in the field with Geoffr and that we were able to look onto each other's shoulders, so to speak, as observers of the observer, is an intriguing point. However, I believe it can be disregarded in this context because Geoffr and I, as a couple, had at least of our curiosity for the required level and distance of observation, at least in regard to each other.
8. With the exception of her observations in southern Sudan in 1983, the key sources for the discussion of this movement are.
9. Thus, this list on photography, readers can admire the beautiful sketch of a house composed on Master 1 (a box found on page 211 of Bruno Stock's excellent guidebook of Sudan (Stock 1983). Myself began my photography only later in 1981, after one of my neighbors sold me a Japanese camera, which he had found on the street in Khartoum, for ten dollars.
10. On the subject of the third, see footnote 7. For the connection between narrative and identity, see Geiger (1998).
11. In Malinowski's diary, we find the following entry for April 13, 1918. "I am looking about the problem of keeping a diary. How immensely difficult it is to formulate the endless variety of things in the course of a life. Keeping a diary as a problem of psychological matters: to gather the essential elements, to classify them (from what point of view), then, in describing them indicate more or less clearly what your actual impression at the given moment. properties, my subjective reaction, etc." (Malinowski 1967:247). Malinowski's diary is full of examples of his struggles to keep his own idiosyncrasies in check. The latter authorities he claims in his diary serve as models on the "sea of individuality," the point of his frame B.R.M being only the most obvious one.
12. Early anthropologists who provided descriptions of their informants (Craun and 1960) did allow the latter's self-referential and disposition to shine through but did not, as he later on, make a central point of the issue. Contemporary anthropologists tend to camouflage the existence of their informants so to redefine them as co-authors.

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COVER ILLUSTRATION
Designed by Marcello Leroi, Grafisk Kommunikation. Six wooden female figures (H. ca. 10 cm.), collected in 1885 and 1892 from Ammassalik in Eastern Greenland by Gustav Holm and Carl Rydén. The figures most probably functioned as fertility amulets. Photos: John Lee. The National Museum of Denmark.

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