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Thick and thinned description

How useful can medical anthropology be?

SJAAK VAN DER GEEST*

'Thick description', a concept coined by Gilbert Ryle (2000) and made famous by Clifford Geertz (1973), stands for detailed ethnography that provides a rich variety of relevant contexts, including the ethnographer's own presence. For the non-anthropologist, the adjective 'thick' may assume the same meaning as it has in connection with forest or thicket: dense and difficult to penetrate. Practical-minded people often see anthropologists as producers of highly complex and theoretical texts that resist 'translation' into concrete action. As a consequence, anthropological descriptions tend to be regarded as irrelevant and paralyzing to policymakers because they merely complicate matters. When anthropologists *do* study concrete practical problems, their analysis of the situation is usually a 'post-mortem' one and lacks useful suggestions as to how to solve or prevent such problems. Moreover, anthropological reports often appear many years after the facts, post-post-mortem one could say.

The miscommunication between medical workers and health policymakers on the one hand and medical anthropologists on the other tends to come from both sides. The former fails to capture the practical relevance of thick ethnography and the latter often regards 'applied medical anthropology' as a dilution of their profession.

After a brief retrospection on my own medical anthropological research over the past forty years I discuss seven considerations that may contribute to better communication between ethnography and practical work:

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1. Concern among (medical) anthropologists about the practical implications of their research is not a dilation of their profession but a sign of theoretical insight and of reflexivity.
2. Cultural brokerage seems one of the most effective outcomes of applied anthropology.
3. Thick description needs to be 'thinned' resulting in transparency of the practical consequences of ethnographic and theoretical work.
4. One mode of 'thinning' ethnographic work is quantification of qualitative insights, an unpopular step in present-day anthropology.
5. Anthropologists should make more haste publishing the results of research that has practical relevance.
6. Research should from the beginning involve policy-makers, health workers, and anyone who has direct interests in the outcome of it.
7. Involvement of various stakeholders should be maintained into the period of writing and publishing.

Looking back on fieldwork

Let me start by looking back on my research experiences and my attempts to do something practical and useful with the results of that research. I am afraid that my achievements in applied anthropology are not impressive.

In 1973 I did fieldwork in Ghana about sexual relationships and birth control. One of the most worrisome findings was that young unmarried students lacked access to reliable contraceptives. In their attempt to prevent a pregnancy that threatened to put their whole future in jeopardy, some resorted to dangerous abortion methods that could damage their health, make them infertile for the rest of their lives, or even kill them. Others used harmless and ineffective methods and had their babies too early. Most of these girls never returned to school and indeed saw their future collapse before them.

The irony of the situation was that the official planning organizations did supply reliable contraceptives to married couples who were *not* interested in having contraception. Only the young and non-married were interested but were denied access on the basis of formal traditional and Christian rules of morality. My attempt to bring these disconcerting findings to the attention of those who had the power to change the situation was limited to writing a series of six articles about the situation for the popular weekly *The Sunday Mirror*. I have no idea what the impact of that series was. It produced one reaction, a letter to the editor by the director of the National Family Planning Programme (NFPP), indicating – rather implicitly – that his organization *did* allow unmar-

ried youngsters access to its services. His statement did not alter the fact that young people continued to avoid the NFPP because they felt they could *not* go there. A proposal by the Dutch World Broadcasting to broadcast a series on young people and birth control in Ghana based on my research and raise awareness about the plight of youngsters did not materialize in the end.

In 1980 I carried out research in Cameroon on the use and distribution of modern pharmaceuticals in hospitals, health centres, pharmacies and informed drugstores and market booths. It proved to be another delicate – and policy-relevant – topic. The outcome of the research showed that public health care institutions suffered from serious shortages of medicines. Health care in the public domain was officially free, but because of the lack of drugs, that free care had become an absurdity. People stopped visiting health institutions that had run out of medicines, or were forced to go and buy medicines outside the hospital, which resulted in them paying even more than those who sought health care in private institutions. The three main causes of the shortage of medicines were: (1) lack of state funding; (2) bureaucratic sluggishness of those responsible for the supply of medicines; and (3) theft and misappropriation of medicines that *had been* delivered. I added practical suggestions to my conclusions and was able to send a 200-page (too thick?) report in French to a hundred of institutions, organizations (including the Ministry), and individuals within a few months of the end of my research. One of the suggestions was that it would be better to make people pay a reasonable price for medicines and actually supply them than to pretend to give them free medication while in fact giving them nothing.

My recommendations did not fall on fertile ground, to put it mildly. The Ministry took offence of my harsh criticism and said they would never turn the fingers of the clock back, meaning: they would not give up the public right of access to free medicine. (Seven years later they did, when they signed the Bamako Accord, an agreement signed by African ministers of health under the auspices of the WHO to make health care more efficient, affordable and sustainable.) I realized – too late – that my approach had not been tactful and had antagonized the authorities. The title of my report, for example, already irritated them before they had started reading it: *La pathologie de services médicaux*. My love for literary style, such as paradoxes worked counterproductively and failed to entice the policy makers.

From 1994 till today I have been involved in anthropological fieldwork among older people in the same community in Ghana where I did my first research. My practical bent in this research has been more modest. Although I do see tremendous policy-challenges in the case of care for older people in rural and urban Ghana. I have mainly limited myself to just writing about the joys and worries of older people, drawing attention to their achievements in life and discussing their relationship with younger generations. Older people do not want to disappear forever into nothingness, but hope to live on in the memories and activities of their children and grandchildren, as ancestors, as educators, as builders of a house in which others can live, as givers of care and other good things they did for the next generation. By writing about them and showing them and their children what I wrote, I hoped to increase their happiness and contribute to their memory. By now all the old people of my research have died and I can only hope that their children have good feelings about the fact that their parents' names and achievements appeared in internationally renowned journals and on in the Internet, sometimes even with their picture. Together with two Ghanaian friends/co-researchers I made a small book with the most beautiful quotes of six elders about growing old and related topics (Atuobi et al. 2005). I distributed copies of the book among relatives and they seemed pleased (especially with the pictures). We hoped that this book would also find its way to other Ghanaians who take an interest in the wisdom of elders, but that hope remains slim. Most Ghanaians do not buy books, however cheap they are. They have other competing priorities and if they buy a book it is more likely to be about Jesus. Two years ago I started filming, largely with the same objective: to show portraits of older people to people in Ghana and to the rest of the world.

Behind this short-term 'application' of enhancing older people's happiness and memory, *are* long-term policy-objectives, however. The Ghanaian authorities claim that they will never give in to 'Western' practices of placing older people in institutions. The African tradition of taking care of one's own parents will not be compromised they assert, but one of the outcomes of my research is that they will soon face conditions that will make it impossible to hold onto their claim. Migration and employment outside their hometown make it increasingly difficult for the next generation to provide that traditional type of care. The reduction of family size is another factor posing problems to care of the elderly. Moreover, life expectancy among the older generation is rising, so everything points at a longer period of care needed versus a diminishing 'pool' of caregivers within the family. In other words, conditions such as in my own country, the Netherlands, appear to be in store also for Ghana, however loudly political and religious leaders decry the 'Western' system of elderly care. For

them, our treatment of older people is the epitome of a cold and dehumanised society.* I have not – or hardly – raised that point, however, and I am still pondering how to broach this topic, respectfully and tactfully.

At this moment I am, among other things, involved in a research project on HIV/AIDS in Ghana, in particular on voluntary testing, counselling, and the distribution of antiretrovirals. In contrast to the earlier research mentioned, I am not the principal investigator. Two Ghanaian PhD candidates and one Dutch post-doc bear the brunt of the research. It is the most ‘applied’ research I have ever been involved in and I feel slightly uncomfortable. Will we be able to achieve our objective to improve conditions for people with HIV/AIDS? I am afraid that the complexity of the social, cultural and moral context of AIDS will resist clear-cut recommendations. Just describing that complexity in clear terms may turn out to be the best ‘recommendation’, *if* policymakers and others are at all willing to read our report and take it into consideration. The fact that the planning and programming of this research and the first findings have been discussed with Ghanaian scholars, policy-makers, health workers, and people living with HIV/AIDS makes it slightly more likely that they will take an interest in it than was the case with my previous research projects.

Let me now, in hindsight, make a few critical comments about my own attempts to do research that would be useful for those I did research on. Four critical analyses on my past research are presented:

1. Three of the four projects I conducted were entirely my own initiative. I did not consult the people concerned about what they wanted me to do (if at all). I chose topics that seemed interesting and relevant to me (although I was – and still am – confident that they *were* relevant also for them). My naiveté was that I ignored the fact that ‘relevance’ also has political, psychological and emotional dimensions. ‘Relevance’ that is imposed upon others may put people off (as happened in Cameroon and during my research on unwanted pregnancy in Ghana) or just keep them indifferent to anthropology.
2. Closely connected with the first point: If I failed to consult those directly involved about their ideas, needs, suggestions, I should at least have tried to ‘sell’ my ideas to them from the start. I did not. I entered through a backdoor,

* African critics of the West European system of elderly care often overlook the enormous financial ‘sacrifices’ the working population makes to provide security to the older generation. In Dutch politics, diminishing pension payments have proved to be an extremely sensitive political topic.

settled, made friends, and did my thing. I felt people appreciated my presence and liked talking to me, but they did not expect results that could help them in their lives. At first, some did expect results and thought I would bring them medical or other facilities. I had to disappoint them and explained that I was writing about them so that others could do a better job helping them. Most did not quite understand, however, what I was doing, except for writing a book. But who would read such a book? In short, they found my presence quite puzzling but accepted me and tried to benefit from me in their own way. In the case of the older people, my conversations with them had a purpose in of themselves. The elders enjoyed conversing with me and if I failed to visit them one day, those who could still walk would come to my house asking why I did not come. They sat down to have a conversation after all; for them it was a welcome change on a dreary day.

People in authority and policy makers in Ghana did not even know I was around. It may sound strange, but I never submitted a research proposal to a Ghanaian official and never applied for research permission. I just went there to 'visit my friends'.* The authorities – whether in Ghana or in Cameroon – were not part of my network and I never discussed with them the purpose or results of my research. In fact, I presumed that they were not interested.

3. My readers have always been first and foremost colleagues in cultural and medical anthropology. The journals where my publications regularly appeared do not reach policy-makers, let alone the people themselves (unless I purposely sent them an off-print copy). Although I detest the obscure and complicated style of writing that some of my present day colleagues practice and try to write clearly and transparently, I am aware that most of my texts are not easily digested by policy makers as they are too long and 'thick' in the Geertzian meaning of the term. Even, if they read them, they would probably find them unfit for application.

4. Finally, most of my publications** appeared long after the facts, another sign that my main concern was not *doing* something to improve conditions but producing texts with interesting cultural data and interpretations. To give one example: I am now working on an essay about greeting in Kwahu (Ghana). A large part of the ethnographic material that inspired me to write this text dates

* Cameroon was a very different story. It took a lot of effort to get my first permission and I was – initially – refused a second one. I frequently had to present my permit when I went to a new place for my research and once the police picked me up from the street and took me to the police station to question me on what I was doing.

** A prominent exception was my Cameroon report (Van der Geest 1981). It appeared a few months after I had left the country but was badly received (see above).

back to 1969-1973 when I was doing my first fieldwork in Ghana. It does not worry me that the data are 'out-dated'; in fact, I am convinced that nothing in culture ever becomes out-dated in the sense that it continues to be true and relevant. But policy-makers have different concerns than of the past.

Enhancing the practicality of medical anthropology

The above self-examination leads to seven remarks or suggestions for enhancing the applicability of research results in medical anthropology.

The theoretical import of practicality

Mainstream anthropologists tend to be rather sceptical about applied research, although this seems to have decreased somewhat over the past ten or so years. They regard applied anthropology as superficial and divested of theoretical reflection. It is 'thin' in order to please the non-anthropological parties that are responsible for policy and practical implications.

I agree to some extent: applied anthropology must refrain from long theoretical discourse when it wants to reach the policy-makers and others. But applied anthropology should not be superficial or entirely without theory. Practical recommendations are the outcome of intense theoretical reasoning. Useful suggestions for change require more 'theory' than cultural interpretations of meaning.

The paradox – and irony – of most policy recommendations is that they are based on insufficient theoretical insight. Many recommendations are naïve, because they do not take into account the complex political and social relations where their recommendations ultimately arrive. Theses of students I have supervised over the years sometimes end with recommendations that fill me with vicarious shame because of their naïveté and implicit insult to the intelligence of policymakers and others involved in the issue they write about and ultimately also to their own intelligence. Many times I had to convince them to drop all recommendations to prevent annoyance, or worse, irritating the practice-oriented readers. Allowing readers to draw their own conclusions is often a better route to application than offending them with platitudes.

Another irony is that recommendations are offered to those who have little or no interest in changing conditions, while those who would benefit from the changes remain uninformed about the suggestions and without any say in the decision-making. To ensure application of research results one needs co-researchers who are at the same time practical workers in health care (cf.

Varkevisser et al. 2001), but I never had such co-researchers. Policy-makers and health care workers are connected to politically loaded linkages among different levels of organization; bureaucratic rules and power differences are likely to complicate or obstruct the application of new insights.

It may sound cynical, but we should realize that policy-makers see it as their main task to produce papers that merely contain words and intentions. It is the appearance of certain words in government documents by which they are evaluated, not by the actual changes taking place on the ground. Anthropologists who claim participant observation as their principal research tool should not be caught in labyrinths of paper trails when they submit their recommendations. They should instead think (theory!) of ways to circumvent the 'paper delusions' of policy-makers and speak directly to those for whom policy change really matters to.

In addition, anthropologists owe it to themselves to think practically when they reflect upon their work and position as researchers. Seeing themselves in the web of conflicting interests and contesting parties that constitute their 'field', they cannot afford to shrug off the practical implications of their presence in that field. Concern about those practical implications shows reflexivity and theoretical maturity. Clever reasoning about cultural and political dilemmas and about social inequality without rendering account about responsibilities in the affairs that have been described is not only questionable on ethical grounds but also problematic for reasons of theory. Thick description that excludes the epistemological and moral reality of the researcher's own presence, misses the point. Glasser, a medical doctor, criticised anthropologists for not taking a stand in their romanticised descriptions of 'traditional' healing practices:

Do they believe that the indigenous healing practices that they report on are effective treatments? Do they exhibit degrees of healthy skepticism in their writings? Do they shy away from outcome studies because of technical problems in performing them or rather is it that they would prefer not to know the answers? ... Do they think that the governments that house these cultures do not have an obligation to these people to supply them with the basic ingredients of sewage, potable water, immunizations, and proper nutrition? The problem is that there is an appalling lack of outcome studies comparing indigenous healers with Western healers. It is as if there was an unstated taboo amongst anthropologists to investigate this area (Glasser 1998: 384).*

* See also Hemmings (2005) for similar complaints.

The fact that medical anthropologists have written abundantly about indigenous healers and their practices but refused to get involved in trustworthy studies of the efficacy of those practices is indeed a worrying sign of anthropological superficiality and gratuity.

Cultural brokerage

The applicability of my own research was mainly in the way of cultural and social/political brokerage. It has become somewhat of a platitude among anthropologists to write that they want “to give a voice” to groups of people that are not heard in the wider world. But platitudes can still be true. I have written about the dilemmas of young people getting pregnant against their wish, about the false appearance of free medicines in a public health system, about the worries of older people, about the plight of people with HIV/AIDS living in a society that refuses to acknowledge their existence and about horrifying toilet facilities. In each case I have tried to transport information about life in one corner of society to other corners where people live who are not aware of those conditions, or pretend they do not know, or do not want to know.

By confronting different categories of people with information about each other I practised cultural and social brokerage. Every society is, in varying degrees, a ‘pluriverse’ of cultures (Weidman, in Van Willigen 2002: 132), or a collection of ‘co-cultures’ (Ibidem). Cultural brokerage implies bringing about communication, interaction and – hopefully – mutual understanding and support. *If* anthropology has usefulness, it is first of all this establishment of respect and understanding between different cultures or different levels of societal organization (Van der Geest et al. 1990). Kinsman (2008.), in a critical analysis of how research leads to policy in the case of Uganda’s fight against HIV/AIDS, convincingly shows that conflicting views and interests regarding the disease at different levels of the country’s political set-up account for ineffective interventions. Better communication and understanding between international agents, ministerial officers, health workers and people suffering from the disease would improve the quality of anti-AIDS policies.

An interesting example of social and cultural brokerage can be found in a research experiment with health workers in Bolivia. Initially, the aim of the research was to have an anthropologist explore the views and needs of the community regarding health care and report the findings and recommendations to the health workers. Then it was decided to have the health workers themselves do the research and experience directly the problems of the populations. The experiment proved to be a success. The health workers gained a much better understanding of the patients’ points of view and applied their new insights

in a more respectful treatment of the patients (De Boer 2004); an ingenious strategy of cultural brokerage.

Nichter (2008: 173-174, 184) speaks of 'translational research'; anthropologists can engage the public through ethnography but they should write in clear and understandable language to explain their observations to the wider 'lay' audience and thus make dialogue possible. Such 'translational research' would fit the 'democratic turn' in the understanding and application of science. Participatory research (involving all relevant parties) is most likely to achieve this translational quality.

Anthropology, finally, should stimulate cultural and social imagination, the ability to imagine oneself in the position of others, either in terms of social hierarchy or of cultural difference. Margaret Mead's (1964) optimism that anthropology may help to bring about a better world, through its promotion of inter-cultural understanding, still makes sense to me. The fact that the present era rather seems to head toward a 'clash of cultures' does not refute her vision. It seems to me an indication of the social and political insignificance of anthropology. Anthropologists have not been able to make themselves heard and to get their message across; they have failed their cultural brokerage. Has our writing been too thick and hermetic?

Thinning

'Thick description', as we have seen, may amount to inaccessible or impenetrable description. Cyrenne (2006), a political anthropologist, admits that he was 'swayed' by Geertz's thick description of Balinese cockfights but at the same time realises that this reproduction of the "blooming, buzzing confusion" of social life is not very helpful to those who expect explanatory conclusions from social scientists. Although I do not expect explanations from anthropology, but rather intelligent interpretations, I do agree that the enticing thickness of some of our ethnography may be lost to those who are concerned with more practical matters while reading our work. That is the reason that I propose intelligent 'thinning' of the thickness.

My use of 'thinning' differs from what Ryle and Geertz understand by the adjective 'thin'. They see 'thin' as superficial, ignoring social and cultural contexts and meanings, as they demonstrate in the example of the 'wink' (Geertz 1973: 7). For them 'thin' in a sense precedes 'thick' whereas I present 'thinning' as an analytical operation following 'thick' description. It would be an intellectual achievement and an act of braveness to proceed from thick to thinning. By 'thinning' I mean that the author lays bare the basic structures of a certain problem or problematic situation. 'Laying bare' implies vulnerability,

as is the case for any form of nakedness. In the Dutch language exists the term *zich bloot geven* (to expose oneself; lit. to show one's nakedness), which means 'to come out of one's hiding', but also carries the connotation of 'committing oneself'. There is gratuity and non-committedness in thick description when it hides the author's position. The author cannot be called to render account. He is hiding in the forest of his text, like a poet in an obscure verse. Writing clearly, openly and with commitment does not violate the complexity of social and cultural situations. Complexity rather requires precise and unambiguous language, as Weber (1972: 123) rightly pointed out:

Scharfe Scheidung ist in der Realität oft nicht möglich, klare Begriffe sind aber dann deshalb um so nötiger.*

The art of writing 'thinly' is, therefore, not to smooth over and blur the ambiguities and contradictions but to show them by clear writing. 'Medical diagnosis' may suit here as a clarifying metaphor although I invoke that metaphor with considerable hesitation. Making a good diagnosis in a clinical setting is the art of reduction. Reduction is a suspect concept in anthropology as it is usually taken to mean that context and complexity are overlooked. I do not agree with that interpretation. Reduction works in medicine because the organ or function that is found defective is part and parcel of a total system of organs and functions. Reduction does not overlook or forget context but rather assumes it, depends on it. Successful reduction is the art of spotting what is decisive.

I admit that social scientists have a much harder task to reach effective reductionist insights, which I call 'thinned description', but we should not run away from it. My inkling is that a sound theoretical underpinning is a successful reduction in disguise. Nothing is more practical than a good theory, the well-known adage goes.

Quantification

Quantification is another concept that is suspect and unpopular in anthropology. Tables are rare in anthropological publications or hidden in an appendix. I would, however, like to call for more recognition of quantitative data in combination with qualitative ones. Providing sensible figures can be an efficient

* "Sharp separations are often not possible in reality, but clear concepts are for that reason the more necessary." My teacher André Köbben drew my attention to these inspiring words of the German philosopher.

way of 'thinning' our descriptions and interpretations. Quantification becomes possible after we have succeeded in what I called the art of reduction.

One way of quantification in particular appeals to me and I have encouraged several students to apply it. The central idea is that the most crucial and relevant insights that emerge from a qualitative research are 'translated' into unambiguous, somewhat extreme statements to which a larger sample of respondents may react with: agree or disagree (or no opinion). The purpose is to establish whether the findings from the qualitative phase are likely to represent a 'trend'. When it concerns a sensitive topic (it often does), the statement will be formulated in such a way that the respondent does not need to reveal his personal views. He will be asked whether he thinks that others have a certain opinion or practice in a certain way. One example:

An Ethiopian researcher, Getnet Tadele (2006), conducted qualitative research on sexual behaviour among young people in the provincial town of Dessie, 400 km north of the capital Addis Abeba. The research focused on young men between the ages of 15 and 25 years, and were, in particular within two additional categories: school going and street youngsters. Like Paul Bukuluki in his essay in this book, Tadele observed that the street youth seemed little concerned about the dangers of HIV/AIDS. Their general insecurity due to poverty, hunger, and police harassment troubled them more. They faced so many problems to survive from day to day that HIV/AIDS became a somewhat abstract risk. They had other more direct worries.

The role of religion was another prominent theme that emerged during the qualitative research. During conversations with young people it was often suggested that religious activities such as attending services were an alibi for meeting lovers and having sex. 'Religiosity' was not an indication of stricter sexual morals.

	yes/agree	no/disagree
Some people say that religion does not have any effect on sexual behaviour of young people.	55 (20%)	218 (80%)
Homosexual practices hardly occur among young people in Dessie.	122 (45%)	150 (55%)
Masturbation should be adopted as an alternative sexual practice to ensure safety from HIV/AIDS.	72 (26%)	199 (73%)
Most young people of my age use condoms.	100 (36%)	174 (63%)

Use of condoms proved a topic about which the boys were very much divided and the researcher suspected that the boys did not express themselves openly on condoms. The same applied to homosexuality and masturbation. Many

denied that such practices occurred at all, but the researcher had his doubts about these statements. He decided to ask the youngsters about these topics in a less personal way through a brief questionnaire that was administered to 274 of them. The survey was both a less threatening approach and an attempt to quantify his findings.

Tadele's mini survey was a fair attempt to give his qualitative data more prominence and relevance. Usually the anthropological case study makes little impression on non-anthropologists, because they want more solid assurance that certain observations or stories represent trends that deserve attention from policy-makers.

Haste

A simple condition for doing applied anthropology, as I already mentioned, is that we make haste in publishing the results of our research if they need speedy action. Urgent matters should not be addressed five to ten years later. Laxity in applied anthropology is self-defeating; it takes away the credibility of the conclusions. I fully agree with Streefland (1991) who in a plea for applied medical anthropology criticised his colleagues for their lengthy procedures. 'Thick' and fat research reports take the writer many years to produce them.

The beginning...

The beginning of much research predicts already that the outcome will not lead to any practical action, because no one of the interested parties was involved in the decisions and preparations that led to the research. My own experiences (see above) confirm this. Any research that is designed for action should from the outset be conceived and planned with, or rather *by* those directly affected by the outcome.

Involving others from the beginning is a matter of sharing 'ownership' of the research and its conclusions with the stakeholders. Ownership feelings are often a prerequisite for the willingness to take the research seriously, identify with its outcome and take action. The role of social, cultural and political brokerage cannot be fulfilled if the relevant parties dissociate themselves from the research. Involving others may not be easy and even require some rhetoric and 'marketing'. As long as the researcher is sincere about his intentions I regard such an approach acceptable.

The end...

Finally, the end of much research is an alienating experience for the parties involved. Even if they became 'owners' when the project started, they still may experience acute dis-ownership when the researcher returns to his academic environment and – sometimes many years later – reappears with a fully completed text in which *they* never had a say. All preceding assurances of shared ownership turn out to have been fake, mere strategies serving the interests of the researcher. The willingness to accept and act upon the conclusions will melt as snow in the tropical sun.

The most obvious way to prevent this from happening would be to keep in touch with the key persons in the field and invite them to continue giving their input during the writing. Over the last ten years I have always kept in touch with such key persons and asked them to comment on or correct my drafts and add their point of view if necessary. The advantage of continued shared ownership is not only that the conclusions are more likely to have an impact but also – of course – that the ethnographic quality of the text will be better.

Conclusion

My soul-searching retrospection and the subsequent suggestions may not have been as 'thin' and unambiguous as I intended. I am aware that some contradictions will always remain.

Let me first, by way of conclusion, and in an attempt to reconcile some contradictions, evoke the figure of the journalist. Good journalism, based on careful research and solid sources and written in an accessible style could serve as an example for applied (medical) anthropology. The objective of brokering information and understanding between parties has more chance to be achieved by clear, short and attractive reportage. Anthropologists would do well if they learned that job from journalists.

Secondly, providing clear-cut recommendations, like prescriptions, may not be advisable. Such recommendations could even be counter-productive. Stakeholders may find the recommendations naïve or an intrusion into their field of competence. It may be wiser, therefore, to only provide clear descriptions and analyses of the 'facts' and leave it to readers to draw their conclusions. Everyman has his trade. Or the writer could phrase his recommendations more indirectly, for example as questions or modest suggestions. Much depends on stylistic skills.

Thirdly, I am aware that applied anthropology, whether 'medical' or otherwise, is a hard task. I have tried to present some of the reasons: application is an advanced form of theory; thinking about solutions requires intense reflection; and writing thinly is a step beyond thick description.

Finally, and by way of brief epilogue, several colleagues have criticised me for being too modest and pessimistic about the practical use of medical anthropology. They pointed at my own work in 'pharmaceutical anthropology' and the training programmes some of us have organised for health workers and shopkeepers to improve their ways of medicine distribution (see Hardon et al. 2004). Members of the International Network for Rational Use of Drugs (INRUD) have repeatedly pledged their indebtedness to anthropological research in the field of pharmaceuticals; my colleagues at Health Systems Research have said the same. In an overview of their work over the past two decades Varkevisser and co-authors (2001) provide several examples of very concrete and successful recommendations, such as more effective referrals, use of maternal services and construction of pit latrines. I hope they are right but my experience is that there is rarely reliable information available on the long-term effects of anthropologically inspired interventions.* Whatever the 'facts' are, my pessimism is not without hope. I cannot believe that 'understanding the other' will not eventually lead to a more humane and efficient health care, as Hemmings (2005: 100) suggests. After all, most pessimism is optimism in disguise.

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* One that comes to my mind is a research by Ventevogel ten years after the much-heralded launch of a training programme for traditional healers in Ghana. The effects proved disappointing: the healers had not put into practice what they had so enthusiastically learned during their course (Ventevogel 1996).

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