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DO YOU CONSENT TO PARTICIPATE IN THIS RESEARCH STUDY?

Paul Verdu

In 2005, my PhD project was to reconstruct at least part of the peopling history of the Congo Basin, using genetic data generated anew from the DNA of existing people. Indeed, population genetics statistical methods allow us to infer the past history of divergences, migrations, admixture events, and population size changes, from the observed genetic diversity of human populations at variable geographical scales (Hellenthal et al., 2014; Mallick et al., 2016). Therefore, these methods offered a promising research strategy to reconstruct the largely unknown peopling history of Central Africa (Bahuchet, 1993a; Bostoen et al., 2015; Cornelissen, 2002; Mercader, 2003). This area is currently occupied by numerous ethnic groups speaking languages from different linguistic families, with subsistence strategies ranging from forest hunting and gathering or lake and river fishing, to cattle herding and slash-and-burn agriculture, all of which often interact in complex socio-economic networks.

I was eager to go to Central Africa for my first season of fieldwork in 2006 to gather the data and samples needed to address my scientific questions. Most importantly, my primary goal was in fact to meet with the people and cultures I had candidly but naively fantasized about, and whose history would, if they consented to participate, become one of the focuses of my research for countless years ahead. Little did I understand how much would depend on that crucial caveat: “if they consented to participate ...”

How exactly does someone design a protocol to guarantee, in advance, the procurement of the volunteers’ informed consent to participate in an anthropological genetics research study? Especially when said-volunteers are not yet known and, what’s more, live somewhere halfway around the world?

The ethical questions encountered in large-scale quantitative and experimental-science approaches often implemented in human biological research might not be comparable with those classically encountered in cultural anthropology. This is mainly due to differences in both the objects of study and theoretical frameworks at play in each discipline, which call for different types of data and substantially divergent qualitative and quantitative methods. However, numerous smaller-scale studies conducted in biological anthropology and genetics have ethical challenges in common with those faced in the field by cultural anthropologists. This is, I believe, evident in interdisciplinary fieldwork involving, at the same site and at the same time, scientists from both fields of research. These projects require a challenging harmonization of both scientific protocols and approaches to ethical concerns across the disciplines. With that in mind, we developed sampling protocols that would allow a cultural anthropologist to co-exist and work with an anthropological geneticist in the field in order to collect relevant ethnographic data and all the biological samples needed to reconstruct the demographic aspects of the histories of Central African populations.

In this book chapter, I will first describe the genesis of the interdisciplinary sampling protocol designed to reconstruct the genetic histories of hunter-gatherer and neighbouring agricultural groups of populations from Central Africa. This project was developed with colleagues in cultural anthropology at the *Musée de l'Homme* in Paris starting in 2005 and illustrates classical difficulties in building multi/inter-disciplinary research projects. Furthermore, I will show how the interdisciplinary approach shapes the framework of scientific research and therefore the deontological and ethical questions that come into play. Second, I will recount some of my experiences recruiting volunteer participants in the course of this project, concentrating mainly on ethical issues concerning; (1) the process of informing participants and the procurement of voluntary consent, and (2) the benefits and compensations volunteers may derive from participation in the research study. The experiences related here come from fieldwork I conducted in various locations in Gabon, Cameroon, and Uganda between 2005 and 2010.

An ad hoc sampling protocol for anthropological genetics studies

The project “ACI Prosodie” was designed through collaboration between cultural anthropologists and population geneticists in the Eco-anthropology and Ethno-biology lab of the CNRS-MNHN in Paris. It relied on a dense network of ethno-ecologists, ethno-musicologists, and ethno-linguists whose fieldwork and extensive ethnographic experience was focused in several countries of the Congo Basin. My goal was to try to bring new insights into a long-standing question in cultural and biological anthropology (Verdu et al., 2009; Verdu et al., 2013). Do the various Central African ethnic groups gathered historically by Westerners under the umbrella term “Pygmy” share a more recent common biological origin with one another than with neighbouring non-Pygmy populations, or not?

The genesis of a multidisciplinary sampling protocol

The question and project thus relied heavily, as is very often if not always the case in anthropological genetics, on ethnographic, sociological, linguistic, and historical criteria to construct the group categories into which individual DNA samples would be placed. The patterns of genetic diversity both within and between populations would then be studied by population geneticists. In our case, numerous ethnographic studies, conducted in significant part by researchers from our lab, had previously deconstructed the historically inherited binary categorization of Congo Basin human groups into so-called “Pygmies” or “non-Pygmies” (Bahuchet, 1993b; Bahuchet, 2012; Hewlett, 1996, 2014; Joiris, 2003). They thus identified numerous criteria for the binary Pygmy/non-Pygmy categorization, which could be used to build an effective prism of interpretation for various anthropological research projects. These criteria included consideration of self and group identity construction, linguistic diversity, diversity of musical and ritual practices, and ethnography of group- or community-specific socio-economic practices and the complex interactions between them. An adequately detailed ethnographic description and analysis was therefore required for each community and its neighbours to determine the appropriate ethnic categorization criteria in each unique cultural context.

From the biological anthropology side of the project, population geneticists need a statistically representative sample of the genetic diversity that exists in each genetic population in order to reconstruct the demographic history of a region. For population geneticists, the classical definition of “a population” refers to a group of individuals more likely to mate with one another than they are likely to do with other individuals outside the group. This definition thus relies heavily on the characterization of limits and barriers to reproductive events among groups of individuals. In humans, such barriers can emerge from geographic isolation of groups, as well as from socio-economic and cultural rules regulating marriages, reproduction, and spouse mobility across social groups and cultural communities. As such, any cultural practice concerning reproduction *sensu lato*, such as marriage, divorce, and polygamy practices, post-marital residence rules, as well as socio-economic, religious, or linguistic barriers against marriages and rules regarding transgression of these rules, etc., may significantly influence the patterns of genetic diversity within and between human groups (Blum et al., 2006; Chaix et al., 2007; Heyer et al., 2012; Oota et al., 2001; Verdu et al., 2013).

Finally, a “statistically representative sample” means, in practice and based on previous knowledge of human genetic diversity, that I needed DNA samples from at least twenty to thirty individuals who did not share biological grandparents (the less degrees of genealogical relatedness between sampled individuals the better), from each genetic population under study. This would also require kinship studies involving extensive interviews with volunteer participants to try to determine beforehand, as much as possible, degrees of biological relatedness among potential study participants.

Therefore, in practice, I needed: (1) numerous DNA samples from biologically unrelated individuals from several human “populations”; (2) a certain amount of information regarding marriage practices to identify in advance possible genetic populations to be sampled in a region (note that this assumption would be formally tested *a posteriori* on the genetic data collected to verify that sampled groups of individuals fit the genetic definition of a population or not); and (3) substantial ethnographic information about identity construction, language, ways-of-life, music, mobility, and relationships among local ethnic groups and populations. This later

information was not only crucial to inform the Pygmy/non-Pygmy categorization criteria, it would also allow population geneticists to compare and evaluate different exogenous and endogenous, or etic and emic, cultural categories identified by ethnographers in light of the patterns of diversity observed in the genetic data (Alvarez-Pereyre and Arom, 2008). This would in turn allow us to decrypt the social mechanisms, past and present, that have influenced the evolution of the distribution of genetic diversity within and among populations. Last, but not least, biological samples need timely molecular biology treatments performed in laboratory environments unlikely to be found in the equatorial forest of Central Africa. It was thus obvious that the already overwhelmed young anthropological geneticist that I was, needed to rely heavily on his ethnographer colleagues to conduct such a sampling project in Central Africa!

At this point, I need to emphasize that conducting real interdisciplinary research requires the deployment of paradigms and methodologies developed in different disciplinary fields (e.g. here, demographic anthropology and human population genetics), without, as much as possible, mutilating one or the other discipline (Alvarez-Pereyre, 2003). This is obviously an immense challenge for any researcher. While I was highly interested in this scientific approach, I also realized that true interdisciplinary research was incompatible with the time allocated to my endeavours in the project and its geographical scale. It is indeed difficult to master rapidly, to the high level of scientific rigour expected in such a research project, all the specialized technical skills from each of the widely differing scientific fields involved (from molecular genetics and statistical inference, to semi-structured interviews and discourse analysis). Therefore, while I tried to train myself as much as possible, and to practise, at a reduced scale, ethnography and demographic anthropology, I cannot claim that my work was truly interdisciplinary. Instead, I explicitly opted for a collaborative *multidisciplinary* approach, with specialized researchers from the different fields, all involved in the project from the fieldwork in Central Africa to the final analysis of data and publication of the results.

The data collection protocol itself, a priori ...

Altogether, these general but very concrete premises led us to design the following sampling protocol. We decided we would only target Congo Basin villages where cultural anthropologists on the team had solid contacts and extensive fieldwork experience. This would enable us to capitalize on the extensive ethnographic work already conducted at a local geographical scale and provide biological anthropologists with a reliable cultural categorization framework for the project. The popular image is often that of human biologists and medical researchers swiftly passing through villages in huge convoys with portable labs, an army of technicians, shiny scientific equipment, and power generators. Needless to say, such protocol was inconceivable given the nature of the information we needed to collect, and the fact that we were not interested in medical research. To answer our questions, we needed time with people. Much more time than a saliva sample would require, but still much less time than an ethnographer would need to conduct satisfactorily his own research, due to the need to rapidly process biological samples. Therefore, we decided to conduct our sampling in small teams of two to four researchers accompanied by local guides and translators when needed, spending between five and fifteen days in each visited village, for a total duration of the joint fieldwork not exceeding one-and-a-half months. We would also carry out this protocol in parallel with different teams in the different targeted sites in Cameroon and Gabon to increase the number of populations represented in our study during the one-and-a-half years devoted to sampling in the project. This protocol allowed us to conduct focused interviews to complement the ethnographic information specifically needed for our project, in addition to the vast body of information collected on the targeted villages during previous work. On site, we planned to first present this new project, answer questions, and, if welcomed to continue, schedule the sampling itself for the upcoming days or weeks.

Once agreed, informed volunteers would, in private if desired, answer a semi-structured demographic questionnaire about their birth and living place and that of their biological parents and spouse(s), marriages and divorces, as well as post-marital residence. We then administered a brief questionnaire about self-reported ethnic, clan, and lineage identity, as well as any relationships with other neighbouring ethnic groups,

including marriages. In addition, linguistic data would be recorded, including word lists derived from the Swadesh list protocol (Swadesh, 1952), often used in Central Africa in order to, among many other things, characterize linguistic affiliations and the local multilingual context. Finally, a small saliva sample would be collected, by letting the participant spit in a cup specially designed for DNA extraction once back in a molecular-genetics laboratory environment.

The informed consent procurement itself, a priori ...

We tailored an informed consent procedure conveying our scientific aims and data collection protocol, following the recommendations and guidelines from the Declaration of Helsinki.¹ This declaration mainly guides medical research involving human subjects and, as such, might not at first call the attention of cultural anthropologists. However, from my point of view, its recommendations can extend far past the field of medical research to draw researchers' attention to ethical concerns involved in any type of socio-cultural or anthropological questionnaire or interview. Indeed, by replacing, where relevant, the words "the physician" with "the anthropologist" in the Declaration of Helsinki, I obtained an accessible, relevant, and efficient toolbox for building informed consent procedures in our multidisciplinary project in a way that university institutional review boards (IRBs) and national ethics committees could easily evaluate and review.

To build our informed consent procedures, the first and most obvious thing was to make crystal clear our research project, its goals, and its means, in general and in particular, to the people we would try to recruit in Central Africa. However, it was not the only thing we had to make explicit for people halfway around the world to obtain their informed consent, in the meaning of western university IRBs and national ethics committees. Indeed, we needed to be explicit about potential risks, discomforts, costs, benefits, and compensations involved in volunteer participation with our research study. We also had to describe all the measures undertaken by the researcher in order to: (1) ensure confidentiality and privacy of the data collected; (2) secure data access and non-commercial use of the data; (3) justify the eligibility of participants based only on the primary research finality and technical protocol limitations; (4) potentially transfer

some data to third parties for research purposes; (5) allow the participant to retract from the study for any reason, at any time; and (6) allow the participant to complain about the researcher's behaviour to relevant independent organizations.

I know, this sounds like a researcher's worst bureaucratic nightmare. Let alone the incredible cultural gap and seemingly complete inadequacy of addressing these lawful procedures to any person (not just foreigners) outside the realm of technocratic administration. Therefore, cultural anthropologists often rightly opposed such procedures with very reasonable arguments. For instance, they have pointed out that, historically in numerous places around the world, some westerners have tricked entire populations into signing western procedures and documents cruelly lacking information and consent, which has been proven much too often to be highly detrimental to the "volunteer signers" in the short, middle, and long term. Consequently, this has led, justifiably so, to strong reluctance on the part of numerous communities worldwide up to this day to acquiesce to any tedious administrative procedures presented by any westerner, including researchers. Such dynamics can very well bias and damage further relationships and therefore undermine the foundations of cultural anthropology work before it even begins.

Nevertheless, looking more closely into the ethical bases underlying each item of this daunting list, I believe that they may raise pertinent questions about how scientific protocols, including mine, could and should be improved. Moreover, these informed consent items are, explicitly or implicitly, already obtained from participants in the vast majority of modern anthropological ethnographic work grounded in extended periods of fieldwork. Indeed, it would be almost impossible for an outsider such as an ethnographer to conduct long-term research, spend months in the same village, and come back year after year, if the consent of the people was not largely acquired, largely explicated, and regularly updated. Therefore, I think that these procedures ought not to fundamentally affect the nature of anthropological practices in the field (the practices that are ethical and deontological of course), but rather help young researchers to build their protocols and improve reflexivity upon one's scientific methodologies.

Moreover, in my experience, they prove empirically useful for a certain type of fieldwork: the rather short ones. These procedures will take more time to explain and

discuss with participants than what is necessary for a simple saliva sampling. Therefore, it slows down the collection of relatively straightforward biological data, but can trigger conversations that may rapidly and unexpectedly reveal information and subjects intrinsically interesting to anthropologists. In other words, in the field, my experience was that the infamous informed consent procedure could be used as an efficient icebreaker – a means to build trust and rapport between the researcher and the communities they work in. To some extent, explaining the informed consent procedure, and discussing it with possible participants, works almost like an indirect anthropological questionnaire. In fact, this procedure can potentially provide some preliminary information about social context, about personal inter-relationships between neighbours and outsiders, and about previous experiences with other research groups while giving local participants a voice right from the beginning in the data collection protocol.

Informed consent has to be procured in a written form? Really?

Ethical committees and IRBs alike usually recommend if not mandate the procurement of volunteer informed consent from participants “preferably in a written form” – in other words, a rather lengthy piece of paper with a signature indicating informed consent at the bottom. Since I was to work with mostly non-literate people, living in a largely paper-free world apart from school notebooks in some situations, such a request seemed incredibly surreal and naive. Therefore, I decided to adapt the written consent procedure with a video recording to be used only for the purpose of gaining informed consent. Thus, I translated my western bureaucratic informed consent form into an oral script, which I would then record myself reciting to general audiences in the target communities and to each participant individually, asking and allowing for questions to ensure comprehension and address concerns, and finally providing participants an opportunity for free commentary. In addition, I would leave the corresponding written form for anyone interested.

Although I was contacted by several ethics committees for a more detailed explanation – the unusual request for the change to a paper-free protocol had initially provoked interest, but now proposing such ad hoc procedures outside the classical paper

form no longer represents an issue for them. Furthermore, I found out that the difficult exercise of translating, in detail and with precision, the scientific project and protocol into an oral script, was directly useful in preparations for fieldwork. Indeed, it was highly efficient for explaining the project to jurists and fellow scientists from other disciplines involved in ethics committees, easing discussions considerably and also enabling me to better understand and formalize the various ethical concerns my project raised. Finally, it proved as useful when describing the project to people educated in the West as to those who were not, whether potential participants in the project or simply curious.

The last element I was surprised with was the status of the written form I was giving to the Central African participants. These pieces of paper contain the oral script and all my contact information as well as that of various institutions involved in the project. I assumed they would be completely meaningless for the participants I would face. I was very wrong for numerous reasons.

Although most of the people I worked with in Central Africa during this project were non-literate, they would always easily find someone literate in the vicinity ready to read them the text and handle the contact information it contained. I know this for a fact, having received a number a random phone calls (asking if I was doing well and when I would be back), from the famous “arbre à réseau, parfois” or “rocher à réseau, parfois”. In French this literally means, the “network tree, from time to time” or the “network-rock, from time to time”. The idea is that even in regions very isolated from western technology, such as the Central African countryside, all villages have a tree or a rock, where, somewhat magically, the GSM network is erratically available. Thus, people form a line, just like in front of the sole public phone box in the vicinity, to call whomever they would need to call, including me for petty things in this case.

Furthermore, going back to villages I had previously visited, I realized that these documents were often carefully preserved in improbable plastic sheets, in the living environment of the rainforest where no piece of paper is easy to preserve. After a series of questions and observations, I realized they would use these documents on certain occasions as evidence, often very ineffective in my observations, of being part of a project conducted by important western foreigners. The most common occasions to my

knowledge were when seeking western medical treatment at the local dispensary or hospital, and when being visited by other teams of researchers or non-governmental organization programmes. However, further investigation should be conducted to verify the probably wide array of occasions when these papers might be used. Finally, some would use these papers as top-grade ... cigarette paper! In other words, what these pieces of paper represent symbolically for the participants receiving them, their status, how they are preserved, as well as how they may be re-used, could be the topic of an interesting reflexive research in cultural anthropology, in my view at least.

Are you informed and voluntarily consenting to participate?

I was finally ready to go to the field after having obtained all institutional green lights for this supposedly bulletproof protocol. Do not worry; most things went extremely well and according to plan – due entirely to the extensive experience, commitment, and enormous amounts of patience demonstrated by my cultural anthropology colleagues. Nevertheless, I faced a number of situations that can, I think, provide useful anecdotal examples of the ethical concerns raised during the deontological practice of any anthropological fieldwork; concerns that are often, but not always, addressed in formal informed consent procedures, however buried under layers of technical phrasing.

How voluntarily are you participating?

In March 2006, we inaugurated the project with ethnomusicologist Sylvie Le Bomin (see Chapter 5, this volume) in one of her hard-won field sites among the Teke and Mbahouin Akele agriculturalists and their Bongo hunter-gatherer neighbours in the Haut-Ogooué region of southeast Gabon. After fine-tuning the oral script through a few trials and a few errors at the outset of our fieldwork, we had developed a detailed description of our scientific project, its ambitions, and its tasks – all included in the protocol along with the whole detailed list of informed consent items. We piloted the procedure with a few communities and it seemed to be well understood, as evidenced by the number of directly relevant questions asked by the audience in every village.

Two important factors, while not unique to this region of the world, substantially eased my work to procure the informed consent of participants. First, there is a

historically low population density in Central Africa and the Congo Basin, shown to be due mainly to secondary fertility issues in a challenging parasitological and climatic tropical forest environment (Retel-Laurentin, 1974). Therefore, villages in the countryside are small, often between thirty and 200 individuals, and it was easy to describe publicly our project to a significant portion of the villagers in a timely manner.

The second crucial fact about hunter-gatherer populations in the region (the so-called “Pygmies”, a word that frequently carries derogatory connotations) is that socially they are collectivists (Hewlett, 2014). For my informed consent procedure, this has delicious consequences that I had not expected at first. These communities have a very complex horizontal organization, traditionally absent any sort of leaders by birth-right. However, neighbouring non-Pygmy populations, who are vastly more powerful politically and socio-economically, had installed official administrative chiefs in the Bongo hunter-gatherer villages, with all the attached administrative, judicial, but mostly symbolic prerogatives. Thus, we presented ourselves and our project to the appointed Bongo chief to ask if he would allow us to try to work with people in his village. It was rather late in the afternoon; we had spent half a day reaching the village in the Teke plateau on one of the trails away from the Congo border. Therefore, the chief told us we would have to re-explain everything first thing next morning at dawn, before everyone left for the day’s work.

Then, for whatever reason, the chief felt it necessary to make the agreement public; he came out of his “corps de garde” and addressed whoever might be listening in the dozen small tin-foil or adobe houses comprising the village in a loud, authoritative voice. He told everyone that it was of utmost importance that they be there to listen to us the next morning and participate in our project, which would bring good things to the village. Convinced that such an ally would ensure the success of the data collection in this village, I went to bed reassured; ready to start working right away the following morning.

I did not, however, sleep well at all: wasn’t it coercion and an abuse of our position to have the local chief force his people to participate in our study? Were there not potentially pressures and threats that would remain invisible to me, including possible payback once we left the village? How could my institutionally validated

protocol mandate me to comply with local laws and obtain the agreement of local authorities to conduct my work when the authorities themselves had little regard for whether people participated willingly or not? The colourful array of reasons for such coercion (at best), could range from the most trivial (money and ego), to the most complex political and social chess game at play in the village. I do not think the ethical abyss dug by this basic contradiction will ever be resolved by any rigid, systematic procedure enacted in the field. Ultimately, it calls into question what volunteer participation actually is in any given social context, though I believe it is better left to philosophers, psychologists, and sociologists to answer.

Facing such an issue in the field, one has two options: pack up and leave, considering the village lost to research with volunteer participants; or triple the amount of time one initially thought to spend in the village. The extra time could be used to lengthen the information part of our protocol, to rebuild a relationship of trust with at least some of the fifty souls in the village, and to make sure to invent harmless scientific reasons to refuse people who though obviously not willing to participate were nevertheless forced to do so by their chief. We had the good fortune not to encounter that problem and ended up never having to spend triple the amount of time.

Indeed, at five in the morning the next day, we were up with Sylvie and fully ready to start explaining our protocol and stimulate critical questioning and expression of free will among a village we thought had been subjugated by the display of their powerful chief. Well, I had forgotten about the “collectivists” part of our textbook: the village was completely empty, and no one but the very unhappy chief and a pair of sick elderly people were there to listen to us. We learned later in the day that, in fact, everyone had agreed during the night to leave the village before dawn, whether because they had better things to do, or because some people in the village thought that the chief’s speech the previous day was arrogant and obnoxious. If they were to participate, they would do so because they wanted to, not because they were told to. One can imagine my relief at that point, and how thankful I was for their choices in social organization! Data collection in this village went extremely well and I believe we interviewed nearly all the adults in the course of our week’s stay – all but the chief. Indeed, he had had his pride hurt by this popular rebellion against his authority, and he

spent the whole week sitting on his chief chair, turned towards the outside of the camp, sulking, while everyone from the village would laugh at the joke and try to get him to participate. He never did so.

This feature of social organization in numerous Central African hunter-gatherer populations has thus always helped me in conducting the informed consent procedures. No matter how many times I would explain publicly the project and answer the same questions, I would inevitably explain it again to all those who had declared themselves willing to participate the previous evening, I was always asked first to start everything from the beginning, again.

While in this specific context the researcher is easily convinced that they have obtained truly voluntary participants, I do not forget that researchers are more often confronted with the problematic ethical contradiction mentioned above. I have yet to find any way to circumvent the problem other than to devote more time to building a trusting relationship and to openly answer people's legitimate questions. I often consider that 70 per cent of my time in a village is spent acquiring this informed consent in a satisfactory way, and only 30 per cent effectively collecting data. From another perspective though, the hours, and sometimes days, spent explaining the project, allowing time for discussion in the community, and answering questions seems remarkably efficient in light of the situation of total awkwardness one must overcome. Imagine: someone totally unknown comes from far away to tell you that his life's work will be to answer a question about you; a question that you yourself probably never considered important to ask in the first place. Hours are but a blink of an eye in bridging that divide.

Why, in this village, do only men volunteer to participate?

In another instance, things did not work so well. In April 2007, I was in the Ngunié region in central Gabon. Mimongo is a medium-size town with a small Bongo (same ethnonym but a different ethnic group from the Bongo from Haut-Ogooué) hunter-gatherer village only two kilometres away. It was the end of my fieldwork and weather conditions had been horrific. Equatorial rain had turned small rivers into raging torrents, flooding the bridges and cutting communications and commercial routes between

villages. As a consequence, malaria took its toll, due in part to the even more limited access to medical care than usual in these conditions. In this last village of my trip, I was welcomed with indifference, as expected in these hard times, but welcomed nonetheless. After explaining my project and offering an unexpected tiny change from the everyday routine, a number of middle-aged men came forward to participate and try the experience. Everything went well, and the next day I had another group of males, coming forward and eager to participate in a joyful atmosphere, laughing at the funny behaviour of another one of these weird western scientists. This continued until the fifth day when I realized that I had interviewed almost all the adult males in the village, but only one elderly woman.

I became, rather slowly I reckon, intrigued by this and began asking around as to why, without ever obtaining a reasonable answer when given an answer at all. Later in the evening of this fifth day, I received a very official letter from the local traditional medicine practitioner and divination specialist, summoning me to his practice somewhere outside town for a formal interview. Of course, I went obediently, and discovered an imposing man surrounded by an army of devoted apprentices of various age classes, each intently focused on the numerous tasks the doctor had assigned them. After very formal presentations, I was asked to describe what work I was doing with “his” Bongo village. Thus, I did so once more, and although he obviously understood I was not conducting medical research of any kind, he ended our discussion by telling me he had convinced the women in the village not to participate in my research, as otherwise bad things might happen to them – in one form or another.

When I asked why he had done so, he told me he thought it suspicious that I went to see all local authorities, including the appointed chief of this small community, to ask permission to work, but did not come to see him, head of the traditional doctor association of Mimongo. I admitted that I did not, because I was unaware that I had to in the first place, but that now that I was there, we could sort the misunderstanding out. He told me that only a substantial amount of money in his pocket might help him to conduct a small ritual to eliminate the bad omen (which he, by his own admission, had fabricated) and convince the women of the village that it was now safe to work with me.

Facing this maddening situation, I decided to cease fieldwork in the village immediately, give all collected material back to the participants, say goodbye to the people who had rented me a room and prepared my food, and leave the following day. Only a single elderly man raised his voice publicly to complain about the inhospitality and lack of foresight of the villagers, telling them I was here to reconstruct their past at little cost and that working with me was in the best interests of the people, especially the younger generations. A tiny victory for informed consent amid the ocean of defeat in which I found myself drowning. But ultimately nothing came of it; people seemed indifferent, if not relieved, by my decision to quit the field in these conditions. It was pointless and more probably just plain stupid, to try to oppose the traditional doctor, whose symbolic power could so efficiently induce people to fear my presence like some terrible taboo. I had obviously failed to secure volunteer participation in the village, my bulletproof informed consent procedure notwithstanding.

Each item in the dreaded informed consent list could be the subject of a number of such anecdotes, illustrating only some of the myriad unresolvable ethical, or even worse, deontological dilemmas that can and sometimes do arise in the field. I wish to present just two further such examples, which touch on what appears to be a far more prosaic matter that future volunteer participants ought to be informed of, namely, “benefits and compensations”.

Troubled benefits and compensations

Aimed at reconstructing population histories through genetic data, my project would obviously not provide any directly appreciable benefit to any of the participants. We warn possible participants extensively that we would not disclose individual genealogical results, but only results at the population level, and with wide levels of uncertainty. Furthermore, we were not conducting meaningful genomic investigations orientated towards disease, the identification of susceptible genetic variants, or any individual diagnostic of a medical relevance. Indeed, such approaches require vastly different sorts of methods, based in medical genetics, as well as the collection of types of data that were beyond the capabilities of our modest scientific project, even if we had suddenly decided to change our ideas and address such questions, which we did not.

We also repeated over and over that we were not medical doctors and could not provide treatments. Therefore, we warned that understanding how biological diversity came to be in the region and where people might have migrated from in the remote past, was the only thing that our project would produce.

In reality, I was surprised to see how rare it was that any more than a handful of individuals would decide, legitimately, that they had better things to do than participate. Indeed, it may have been that all the people I visited imagined I had some secret thing of great direct benefit to give them, but if so they never complained about not receiving it. Alternatively, people seemed to think that the stated aim was of sufficient benefit to merit spending a bit of time doing exotic questionnaires and spitting in a cup.

For a research protocol like mine (requiring one or two hours per participant, plus collective time) the general recommendation is strongly against compensating participants directly. The deontological arguments for such compensation are shaky and as learned the hard way in previous experiences mainly in medical research fieldwork, monetary incentives can promote over-enthusiasm among participants and therefore affect the reliability of collected data. Finally, in poor areas, giving out money often created a waiting line beyond the financial means of our research project. However, not giving out money also proved to be tricky in numerous villages in Gabon and Cameroon, where appreciation and gratefulness is often symbolized with a public gift of money. We decided to cut this Gordian knot by instead giving large amounts of edible goods and primary use items to the community rather than smaller amounts to individuals. This included goods such as cooking oil, rice and beans, matches, soaps, school books and writing items, or other types of utilitarian goods that the community would ask for and that were within our means to provide.

We also warned possible participants this gift compensation would be offered in public to the group as a whole and was to be shared among them following their rules, without our intervention in any way. This choice was based on the previous experience of our team members having raised animosity within communities by giving compensations individually to participants. Conflict can indeed arise when compensation shares are not perceived as fairly divided among participants who otherwise recognized that they had been involved to the same extent in the project. In

our case, it is likely that, from the participants' perspective, a fair compensation for participating does not necessarily mean that everyone should receive an equal share, but results instead from a complex collective calculation of, among others things, social status, household size, immediate need, and differences in access to the particular resource.

This method of compensation nonetheless carries potential drawbacks as all others, no matter how suitable it may sound in theory. In 2007, we went with Marie-France Mifune, an ethnomusicologist involved in the project, to western Uganda on the Nord-Kivu/Semiliki border with the Democratic Republic of Congo. Our main task was to update ethnographic data obtained in the 1980s by colleagues who never had the chance to return during and after the numerous bouts of civil war that had chronically devastated the region since. If the ethnographic data gathered were promising, and if the opportunity presented itself, we were also going to conduct DNA sampling as we did in Gabon and Cameroon in the years prior. Indeed, the Ugandan Scientific Research and Technology Ministry had recently released their ethical procedure to obtain research authorization for projects involving human biological sampling and ethnographic interviews. After submitting to them the same informed consent and research protocol we had designed for Gabon and Cameroon, we rapidly obtained our authorizations and started our fieldwork a few months later.

Working with the Bamba and Konjo agriculturalist communities of the Semiliki valley, we provided our usual compensation after two weeks of extremely interesting work in a village. The first hint of a problem should have been when we had to firmly refuse to deliver the goods to the chief's backyard instead of in the public space as initially planned. At the end of our stay, though, everyone seemed happy and we went on to work in the neighbouring Nsua hunter-gatherer village for the following two weeks. After a few days, we each received a similar letter from a local boy hired to carry them to us. The letter addressed to me, written in English (a lingua franca in the region) is reproduced in Figure 14.1. It relates a disagreement among a group of participants upon the sharing of "the prize I brought". The fault is clearly laid upon the chief, while Marie-France, our guides, and our translators were unanimously declared free of any possible wrong-doing. We immediately tried to gather more information and to see if a

reasonable improvement could be made on our initial agreement with these people who clearly felt they had been cheated. However, they refused any intervention from us, and strongly emphasized that it was their business and that they just wanted to inform us of these regrettable facts concerning our compensation gifts. With that, the debate was closed and we finished a remarkable scientific adventure in the region without further difficulties. Nonetheless, I had to confront the fact that dissatisfaction clearly arose because of my project, my protocol, and the way I had conducted the work. On a cerebral level, I've told myself that I followed, to the letter, the protocol I had obtained ethical approval for, and that as imperfect as my protocol had been, I was cleared and thanked by the community. To this day, I am still not convinced this has been of any consolation.

TAKE IN FIGURE 14.1

The last anecdote I want to relate also concerns an unforeseen compensation-related issue, and highlights how fine the line is concerning this aspect of anthropological fieldwork. In 2010, I was going to Cameroon and the Tikar country to conduct a project, very similar to that in Uganda, among the Bezan hunter-gatherers and the Tikar agriculturalist neighbours (Figure 14.2). Towards the end of my field trip in this small region of central Cameroon, I made a mistake that jeopardized my work in one of the Bezan villages. In fact, when presenting what I had in mind to compensate them for their participation, I mistakenly called the compensation a “salary”. For the first time ever, voices raised, people were unnerved, and some immediately shouted at me. Not understanding what this was about, I kept on building my own demise, and insisted that I could unfortunately not do anything more, that other villages had received the exact same amounts and everyone there had gladly agreed to participate, etc. A total failure. Everyone basically turned on me, telling me I was nothing but the usual lying westerner, that the salary was, in fact, much smaller than what I offered (*sic*), but had to be given in cash, not goods. Finally, they declared I was suspected of trying to trick them, and so on and so forth. Confused, I offered them a substantial fraction of the

compensation that I had brought and retreated quietly without trying to recruit participants in that village.

TAKE IN FIGURE 14.2

Back in the car I was completely baffled. My guide and my translator laughed at me, telling me I had made a stupid, unforgivable mistake. In misery, I asked them to reveal to me which mistake I had made, one that I had never made in the dozens of other villages I had visited. It was simple; I should not have ever have mentioned the words “work” and “salary”. For the local people, these two words come from a very different realm than the words “research project” and “time-compensation”. My colleagues told me that once I had said the words, it was like I was trying to play tennis during a soccer game! I had no chance of making myself understood, even if I had been aware of the nature of my mistake. Here again, my bulletproof informed consent procedure could not have done anything to save me. From my colleagues’ cheerful point of view, the fault was entirely mine.

Conclusion

Through these examples, I have aimed to illustrate how research projects and all their implications (at least from the standpoint of western ethics committees) can be efficiently and comprehensively conveyed to non-western audiences. However, these experiences also emphasize that any given individual’s decision to participate or not at any given time is influenced by multiple, shifting, often unknowable and therefore unpredictable factors, which no pre-fabricated protocol can ever account for fully. As researchers, we must become comfortable, but not complacent with regard to this fact. Informed consent procedures are imperfect guidelines, bound to evolve as researchers do their best to adapt them to one unique situation after another, a.k.a. the daily life of an anthropologist in the field (Figure 14.3).

TAKE IN FIGURE 14.3

There is no question that formal consent procedures eat up far more of the researcher's valuable and limited time in the field than anyone would like, due mainly to tedious administrative formats and the obtuse and specialized language used in them. Nevertheless, they can also be utilized and appreciated as a powerful tool, introducing a healthy degree of reflexivity into one's research, which can help sharpen research goals and methods by leading one to consider their possible ramifications, both intended and unintended. Such considerations can clarify a research programme, enabling the researcher to provide more lucid and accessible explanations of their projects to critical members of ethical review boards and potential participants alike.

Indeed, these are the researcher's two most crucial audiences, and the most effective informed consent protocols will involve some artistry in balancing the demands of both. Preparing scientific fieldwork involves complex negotiations of logistic, ethical, scientific, and administrative components. It requires higher levels of coordination than does lab research and demands shared commitment among numerous parties towards a distant and difficult goal (*stricto sensu* for fieldwork abroad). Research ambitions must be scaled to the realities and obstacles foreseen in the field, while successful programmes must be at once highly structured yet flexible enough to accommodate unexpected contingencies. Frustratingly, the complexities, realities, and demands of fieldwork are nonetheless often not fully understood, which can lead to the perception that fieldwork is not adequately productive of worthwhile research by many academic institutions and even many researchers.

Altogether, it is therefore crucial to develop procedures that ensure research aims and methods are transparent and replicable, and in agreement with fundamental deontological requirements associated with the scientific study of humans. However, it is important to remember that ultimately our ethical responsibility is to the people we hope to learn from and about. To this end, I have benefitted from a certain degree of creative resistance against obtuse, technical administrative language that can be so counter-productive to the attainment of truly informed consent, so essential to the collection of accurate and reliable data, especially when working with non-western populations. In other words, careful consideration of the ethical and deontological

dilemmas of fieldwork is not only fundamental to successful fieldwork; it is the key to successful research.

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Questions for reflection

1. When does the researcher need official research authorizations?
2. Who do you think the researcher should ask and obtain research authorizations from?
3. Are there field-anthropology disciplines or methodologies that do not need ethical approvals to be conducted?

Note

1. The latest (November 2013) update of the Declaration of Helsinki on Ethical Principles for Medical Research Involving Human Subject can be found at: <https://sites.jamanetwork.com/research-ethics/index.html>.

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