



**Caught between Apprehension and  
Comprehension: Dilemmas of Immersion  
in a Conflict Setting**

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## **ABSTRACT**

Conducting fieldwork in areas affected by violent conflict presents a particular, yet context specific, range of challenges and opportunities for researchers. Researchers may be caught in a dilemma, between apprehension and comprehension. While they are compelled to seek an ever-deeper level of understanding, their apprehension of the perceived risks may hold them back from seeking it. The paper argues that the constraints produced by the basic dilemma of immersion are contingent on a host of variables outside researchers' control and that while they may be very difficult to overcome, they can be mitigated by the adoption of a flexible and pragmatic research strategy based on local knowledge. However, highlighting issues related to the politics of truth, competition over the rents of the research, and trust, it also shows that although this approach may mitigate dilemmas of 'immersion' in conflict settings it produces new, context specific challenges.

## INTRODUCTION

Based on fieldwork in eastern Congo, I will discuss some of the challenges and opportunities that a researcher may be confronted with when carrying out ethnographic research in a conflict setting. My fieldwork experiences in eastern Congo were marked by a fundamental and constant tension between trying to obtain empirical data and avoiding taking unnecessary risks. On the one hand my 'vocational mission' as a researcher compelled me to seek an ever-deeper level of understanding of the field of enquiry. On the other hand my partly self-induced perceptions of the risks involved in this research pulled me in the opposite direction. This basic dilemma of 'immersion' permeated the entire fieldwork experience and set serious limitations to what I permitted myself to do. Yet, while research in conflict zones presents a unique set of constraints on the collection of data, it also presents a unique set of opportunities (Vlassenroot, 2006). In particular it allows researchers to interact with people who are living through conflict and have experienced the violence associated with it, either directly or indirectly. This provides researchers with a possibility to understand the conflict from the point of view of the people involved – *a sine qua non* for describing human interaction in a conflict setting and for understanding why they do as they do. Conversely, abstaining from carrying out research in conflict zones can lead to seriously flawed depictions of the conflicts; e.g. through overreliance on theory, models or popular discourses.

The paper argues that while the basic dilemma of immersion may be very difficult to overcome, it can be mitigated by the adoption of a flexible and pragmatic research strategy based on local knowledge, but that this approach also contains certain context specific

challenges. The problem with a rigid agenda is that it approaches the field as a kind of research laboratory, that is to say it assumes ideal field circumstances for interacting with interlocutors (i.e. stability, trust, calm, security, freedom from fear) and presupposes the researcher's position of control. But in a conflict setting these conditions rarely, if ever, exist, forcing researchers to find new tactics and techniques for acquiring a solid level of insight, while at the same time minimising attendant risks to themselves and others involved with the research (Kovats-Bernat, 2002). While special ethnographic, theoretical and ethical sensitivities are required when working in dangerous areas, the hazards faced by fieldworkers engaged in this kind of fieldwork are substantially mediated through the negotiation of potentially hazardous circumstances (Sluka, 1995, p. 227).<sup>1</sup> Each social context presents a different set of challenges and opportunities, and each researcher's way of dealing with these challenges is different. This makes it difficult to make a generalisable list of complications arising out of carrying out fieldwork in a conflict setting.

During my research I quickly became aware that my collaborators and interviewees were much better equipped than I to foresee the potential hazards of the research. While the researcher may acquire insights into various local issues from afar this cannot substitute for the knowledge of the local rules of the game acquired through lived experience. For that reason working through local intermediaries may mitigate the potential risks associated with the fieldwork due their experience with the setting. Thus, rather than guide my fieldwork with a clear hierarchy between researcher and 'assistant', I carefully considered

<sup>1</sup> See Wood (2006) for a thorough treatment of the ethical challenges of conducting fieldwork in conflict settings.

the advice and recommendations of the local population and my collaborators in deciding what conversations (and silences) were important, what information was too risky to get at, the amount of exposure to risk that was considered acceptable, the questions that were potentially dangerous to ask, and the daily techniques that were important to follow for the safety and security of myself and those around me. Often I leaned towards the suggestions of my contacts (who were better at anticipating danger than I was) rather than rely on my own presumptions as to what was supposed to be best for us.

My local intermediaries were central to negotiating access to data in eastern Congo, they enabled me to set locally informed limits on my own inquiries, but also facilitated the acquisition of otherwise inaccessible data. However, this also brought its own challenges. I wish to highlight three interrelated challenges that continuously forced themselves to the forefront. They were the *politics of truth* the *competition over the 'rents' of the research* and *trust*.

The relationship with my collaborators was not just a formal relationship between employer and employee and researcher and 'assistant'. They were crucial 'brokers' between local society and myself. As such they were largely responsible for my access to the empirical data. In eastern Congo today, as elsewhere in Africa where the rise of international development has led, progressively, to the unprecedented economic importance of brokerage activities, the role of the intermediary or 'broker' is very common (Bierschenk, Chaveau & Olivier de Sardan, 2002). The massive influx of international resources into eastern Congo over the last two decades in the form of humanitarian aid, development assistance, and one of the world's largest peacekeeping missions, has increased the de-

mand for local brokers and entrenched 'brokerage' as a livelihood strategy. International aid has become a stake of vital importance for local actors and its distribution among locals can generate conflicts (Büscher & Vlassenroot, 2010). The fact that this competition over international resources occurs in a context of abject poverty only makes it more intense. Researchers are not exempt from this competition, and field research in a conflict setting which is marked by widespread poverty is particularly vulnerable to sidetracking from 'brokers' who may have their own particular interests in shaping the project.

Although I was not engaged in aid activities I still represented an external resource, and my collaborators would develop their own diverse interests, which were mostly out of my control. The project provided various interlinked socioeconomic and political opportunities for them. Socioeconomically it presented opportunities to earn some cash, establish relations with influential people, acquire recognition, professional experience, etc. Politically it provided an opportunity to present certain narratives to the outside and thereby to participate in the global politics of truth in a new way.

Ultimately, the richness of the ethnographic data depends upon the mutual trust between researchers and their local brokers and interlocutors. Trust, however, is not acquired overnight, and can easily be undermined in a conflict setting where distrust between individuals and groups is very high. If there is competition between the brokers the task becomes even more difficult. Nevertheless, in a conflict setting the importance of building trust with key interlocutors, 'brokers' and other collaborators is magnified. This is so not just because it strongly influences the quality of your data, but also because the security of everyone involved is at stake.

For locals it is paramount that they can trust researchers, especially if they are part of a conflict, if they are persecuted or in conflict with formal or informal rules of the socio-legal order. The risks for locals of being associated with a foreign researcher are potentially great. They can, for instance, be targeted by authorities, or by people motivated by economic gain. However, the issue of trust is much more complex than that. In effect it permeates all the activities of research from the most mundane activities to the most important interviews. It is therefore paramount that the researcher carefully manages his or her self-presentation, and in this respect the role of the 'brokers' is obviously crucial. This is even more important if the objective of the research involves investigating politically and emotionally charged issues, such as ethnicised conflict, corruption, and violence. Asking questions about such issues could easily be misunderstood thus rendering researchers and their collaborators suspect in the eyes of local interlocutors and authorities.

### **DILEMMAS OF 'IMMERSION'**

Although often contested, the term 'participant observation' is used to describe the process of immersion into an unfamiliar environment. Regardless of whether or not one subscribes to all the ideas and methods prescribed by it, its main message is clear enough. Researchers immerse themselves in an unfamiliar social universe for a prolonged period of time so as to observe it, while taking part in it. Researchers are thus able to observe, if not from the 'inside', in the strict sense of the word, then at least at close quarters, those who live in this universe, and to engage in direct long-term interaction with

them. Therefore participant observation, regardless of the degree of immersion, requires the researcher to be in direct contact with the group and their social universe.<sup>2</sup> It has been that way since at least the Malinowskian revolution of cultural immersion, which many believe inaugurated long-term intensive fieldwork as the defining method of modern anthropology (Stocking 1983).<sup>3</sup>

This method presents a unique opportunity to provide an *emic* understanding of what living in a conflict setting entails, but it poses a series of challenges which are difficult to overcome. In my experience it produces a very basic dilemma between the vocational mission to acquire an ever-deeper understanding of the lived experiences of the local and the real and imagined security risks that such commitment would require. Further it creates dilemmas of an ethical nature, because it confronts researchers with the question of how deeply they are willing to 'immerse' themselves in the social universe of their research subjects, without transgressing their own ethical threshold.

I became acutely aware of such dilemmas during my first period of fieldwork research in eastern Congo in 2005, which was centred exclusively on one of the numerous local armed groups: the Mäi-Mäi group of Joseph David Karendo Bulenda, known as General Padiri, from the Bunyakiri area in Kalehe territory in South Kivu. The militia had been one of the biggest local armed groups who,

<sup>2</sup> On a sliding scale Gold (1958) and Junker (1960) distinguish between 'the complete observer', 'the observer as participant', the 'participant as observer' and the 'complete participant'.

<sup>3</sup> Herbert (1991: 157–203) points out that already in the late 18th century, missionaries spent long periods of time living among far-away primitive societies, not only so as to convert them to Christianity, but also to empirically document the endless moral depravity and savagery of their manners and customs.

supported by the Congolese government, had participated in the armed struggle against the Rwandan backed *Rassemblement Congolais pour la Démocratie-Goma* RCD-G rebellion, which occupied large parts of eastern Congo. Most of the group's fighters had been reintegrated into the Congolese army following a peace agreement in 2002, which led to the creation of a transitional government. The purpose of the research was to understand the rationalities and practices of their armed struggle. The bulk of the data was to be produced through interviews with former and active members of this local militia at all levels in the military hierarchy, as well as civilian personnel associated with the group.

Even though Padiri's Maï-Maï group had been reintegrated it was not disbanded entirely as several groups had stayed behind in a stronghold in Bunyakiri for, as they continuously reminded me, "nobody has any trust in each other, everybody is watching each other, so no one wants to demobilise fully". So the group was in a state of alert, convinced that another war would break out sooner or later, and the group's members proudly proclaimed to still be guided by the Maï-Maï 'cause'. They referred to themselves as the 'real Maï-Maï, in contradistinction to the various groupings and political parties, which called themselves Maï-Maï. Through the research I was able to acquire an inside view on how the group's members perceived the conflict and the rationales and practices of their engagement in it, mainly from the leadership's point of view.

Some of Padiri's officers and their most loyal soldiers were in more or less open conflict with the regular armed forces, which they believed had sidelined them. Largely excluded from the main patronage networks of the Congolese army, they were mainly kept in reserve without men to command except for their small groups of bodyguards, waiting and

hoping for their leaders to negotiate a better deal for them. They were in a sort of limbo, which they referred to as 'dispo' (*disponible*), barely able to make ends meet and not in a position to build up or maintain any kind of patronage-based parallel command structure, which some of their former adversaries had done (cf. Ericksson Baaz & Verweijen 2013).

They lived better during the days of the 'maquis' when they partly controlled large areas of Kalehe territory and beyond, and they looked back upon on that time with a certain level of nostalgia, in spite of the fact that they were in open conflict with the RCD-G and the Rwandan army. It was in many ways a sidelined and disillusioned band of ex-fighters which I encountered. Their position on the fringes of society only confirmed what they had long suspected; namely that the new 'post-conflict' political order was in reality warfare by other means. In their eyes the Congo was the victim of an internationally orchestrated conspiracy designed to continue the illegal exploitation of the country's riches and the domination of its people by foreigners, primarily Rwanda, Uganda, and Burundi, who acted as the executants of certain Western powers, notably Great Britain and the USA.

The experience of getting to know a group of a group of active and former militia members quickly made me realise that their social world was one marked, among other things, by insecurity, severe trauma, alcohol abuse (by some), uncertainty, and the daily struggle to make ends meet. Although I felt strongly about understanding their social universe and their modes of reasoning, it was a social universe that I was only willing to become habituated to, to a certain extent. In my mind becoming too habituated to it contained too many physical, mental and ethical hazards and I experienced a power-

ful tension between my own research agenda and the ethnographic experience itself. This tension revealed in a particularly acute manner not only the limits of the relativist doctrine of culture contained within modern anthropology, but also of the method of immersion in an unfamiliar social universe through participant observation.

Participant observation presupposes a sort of doubling of consciousness, which is difficult to sustain, even in a setting that not marked by violent conflict. It presupposed that researchers are able to set aside their personal history and thereby divest themselves wholly of their values and prejudices in order to attain sympathetic understanding of their research subjects. Supposedly this should enable them to obtain non-subjective pure scientific data. But as Bourdieu (2003) points out, the idea that researchers can wipe their mental slates clean when engaged in fieldwork is largely an impossible empiricist dream. Like everyone else, researchers are shaped by the values, rules and norms inculcated in them during their lifespan.

Carrying out fieldwork in a conflict setting reveals in a very immediate and direct way the inherent dilemmas of immersion. Many ethnographers who study various aspects of violent conflict have reported on the bewilderment and disorientation experienced when studying armed conflict. Nordstrom and Robben describe this kind of tension felt by many ethnographers as an 'existential shock' (1995: 13).

Researchers respond differently to the experience of violence, and past research experiences may have a strong influence as well. One does not need to experience violence or poverty in order to feel its effects in a social universe. Listening to the narratives and rumours of war, of daily survival, of violence, of abusive officials, of expropria-

tion, of disease, of flight, of abducted children, and other tragedies is likely to have a profound impact as well.

I found that attempting to navigate in a conflict setting was unsettling because it constantly forced me to consider the attendant risks; I was thus constantly reminded of the 'dangers' of the field – imagined or real. Whereas in the beginning of the fieldwork researchers might worry about unfamiliar sounds and gestures, and attach too much significance to insignificant rumours or events, the actual experience might unsettle them even more. Acquiring the 'know-how' to navigate in a conflict setting is in other words a double-edged sword as it puts researchers in a dilemma, caught between comprehension and apprehension. While the practical experience of the field enables researchers to understand the insecurity and dangers of the field, their apprehension of it may block them from acquiring that practical experience.

For instance, at one point one of my interlocutors half-jokingly asked me if I wanted to become a *Mäi-Mäi*. The idea was intriguing. Being part of the group would certainly have provided first-hand knowledge about the point of view of the *Mäi-Mäi*, but what would the risks be? And how would it change their expectations towards me? It forced the issue of my position as a researcher, and how far I should immerse myself in their way of being. One of the ways in which I tried to deal with this issue was to ask my interlocutors only to tell me things they could accept that I would subsequently write about. The rationale was that I did not want to pass on information which might incriminate them at a later stage. At the same I wanted to maintain a good relationship with them in case I would come back to do further research.

## TRUST

During my research of the Mai-Mai mutual trust slowly increased. It had been my intention from the outset to attempt to build a relationship built on trust and mutual respect, since I wanted to obtain an *emic* point of view. In line with the relativist ethos of modern anthropology I wanted to understand their way of thinking and through that to show its inner logic. This was in sharp contrast to how they were portrayed by most commentators, apart from a few academics, who have tended to objectify them as traditional tribal warriors without clear political objectives, and in any case as a source of disorder and insecurity.

The event that was most conducive to their acceptance of me was that I travelled to meet General Padiri in Kisangani. At first he was reluctant to meet me because, as I was later informed, he and his close collaborators found my presence suspicious. According to one of my key interlocutors in the group they found it unlikely that a man of my young age could carry out such a project. So, they believed that I was working for someone else. And as they told me several times afterwards, they had encountered white people at the front who were working with their enemies. What seemed even more suspicious to them was the fact that I had arrived via Rwanda, whose regime they regarded as their arch enemy. Thus, unknown to me at the time, I was being screened by various members of the group during our conversations. As it turned out several members of the group became very enthusiastic about the research and happily assisted me in it. This proved to be a boon for the research. They engaged in our discussions in a surprisingly open manner and gladly, almost proudly, provided their precious 'point of view'. Thus, accepting a flexible approach where I partly allowed the

research to be defined by my interlocutors, rather than subject them to rigorous scientific probing and testing to 'extract' the truth about them, proved to be productive. In fact, due to their pronounced initial mistrust I believe it was the only way to acquire an inside view of the group. Once a certain level of trust was established new members of the group were introduced to me on an almost daily basis, each providing different perspectives and documentation (e.g. administrative, political and legal documents, photos, and videos).

As I became increasingly immersed in the social universe of the group the relationships between some members of the group and myself became more and more friendly. We shared stories about our lives, talked about our families, values, politics, spirituality, worldview, etc. By the end of my research I considered some of them friends. As an example of how our relationship developed I helped to facilitate a dialogue between the UN and the disgruntled Mai-Mai who were considering going back to the 'maquis', on their own request, which again raises the question, how far one should go as a researcher? I also introduced them to several influential people that I had met via my internship in Bukavu, and when one of them was appointed as the mayor of one of municipalities in Bukavu after my departure, some of them saw this as related to my agency. At my farewell party, which was organised by my host (*Université Catholique de Bukavu*), several of the Mai-Mai participated, which under normal circumstances would have been very unlikely, and we exchanged farewell gifts.

But as relationships become more familiar they also become more complicated. Being seen and treated as a friend, and even a 'frère', by a group of combatants, some of whom were on the verge of defecting from the army

and who considered themselves as still at war, is obviously a double-edged sword. It should be noted that being called a 'frère' has a specific set of connotations in eastern Congo. It implies for instance being loyal to each other and helping each other out in the daily struggle for existence. In addition it is part of a complex system of signification and mutual obligation, which revolves around the notion of the 'family'.

### THE POLITICS OF TRUTH

Several of my interlocutors rationalised my appearance as the result of God's will, and I was told more than once that a diviner had foreseen my appearance in a specific locality. While this, to a certain extent, is revelatory of their belief system, I also believe that it is indicative of the kind of relationship they were trying to build with me. It simultaneously legitimated my presence and raised their expectations of me. I had the distinct feeling that I was seen and relied upon as an agent of change. The arrival of a 'white person', a *muḥungu*, associated with a development institute in a Western university was a significant omen for them. The point here is that my fieldwork was from the outset suffused with unequal global relationships of power and truth. Without in any way wishing to inflate the importance of my work, I believe that from their point of view accepting me as their interlocutor enabled them to participate in a global politics of truth. Commonly perceived and described by mainstream media and many locals at best as superstitious tribal militias and at worst as 'negative forces', 'violent savages' or 'diabolical', the presence of a researcher willing to listen to their side of story presented an opportunity to counter these depictions and perceptions – which

they regularly did in our conversations. In the global politics of truth, the voice of someone affiliated with a university is endowed with considerably more authority than they were. At the same time, however, the validity of the narrative of fieldworker is deemed valid only insofar as it is based on the words and observed actions of his, or her interlocutors, since the touchstone of the claim to scientificity of ethnography; that is, its claim to speak the truth authoritatively, is that it is based on the point of view of the locals.

However, the underlying premise of the growing sympathy between certain members of the group and me was that it was based on mutual interest. Whereas I was interested in their narratives for scientific and ethical purposes, but also for the advancement of my career within academia, they were interested in collaborating with me because they saw me as someone who would be able to help them get their message through to the outside world, but also as someone who might be able to help them practically. Moreover, several of them asked for 'loans' while others thought that I might be able to help them emigrate. For example one officer who had a good position in the army, following a discussion on spirituality in Denmark and in the Congo asked me to facilitate his emigration to Denmark where he could help evangelise the people. But most commonly they would seek my assistance with finding collaborators or donors for various development projects, such as the construction of maternity clinics, orphanages, churches, and schools.

The juncture between the dilemma of immersion and the politics of truth also manifested itself during our daily conversations with them. I struggled to find the right balance between being critical and being engaged during the conversations. I found that in my eagerness to understand them I was inclined

to sympathise and agree with them. Robben (1995: 86) has pointed out that the intersubjective construction of knowledge between the ethnographer and his interlocutors is suffused with seduction. In my case there were times where I was too willing to accept the narrative of the *Maï-Maï*. I found their various political claims reasonable in many ways. I was able to verify with my own eyes that they were marginalised and struggling to survive. I could see with my own eyes that they had been manipulated by the government and given false promises in the post-War political order. Importantly, their re-articulation of the post-colonial critique of the neo-colonial world order to a certain extent resonated with my own view of North-South power relations, which had been formed through my readings of critical post-development and post-colonial studies.

In the narratives of the *Maï-Maï* they represented the will of the Congolese masses, and were the vanguard of Congolese resistance to foreign exploitation and domination. It was tempting to become absorbed in this self-glorifying narrative, with its divisions between heroic victims and villainous perpetrators. It had an instinctive pull and it seduced me with its air of authenticity, which accrued to it precisely because it was the 'local point of view'. I did not fully realise at first that in my eagerness to build a good rapport and in my desire to empathise, sympathise and understand them, that I had to some extent abandoned my sense of criticism.

However, my interviews with other people outside the movement revealed another side of the group. My interviews with NGO workers, demobilised child soldiers, relatives of child soldiers, and other ordinary people, made it abundantly clear that there was a very sinister side to the group, which included the systematic abduction of children to serve as

child soldiers, forced labour, torture, assassinations and authoritarian methods of rule in their zones of control. I found it hard to believe that my interlocutors had carried out such activities and I was reluctant to accept these claims to begin with.

By the same token I did not feel a strong urge to confront them with these questions. For instance, our conversations often revolved around different issues related to the conflicts in the area. During these conversations it became clear to me that the subjectivity of the observer of the conflict, and that of the participant in the conflict were unequivocally incommensurable ways of being. From the point of view of my interlocutors their own survival and that of their fellow Congolese citizens depended on the destruction of their adversaries, often represented by the broad figure of the 'foreigner' or that of the 'traitor'. I had no stake in this conflict and I could not ascribe to it, much less participate in the violent struggle. This narrative generated an intense feeling of discomfort, not least because I believed that such narratives with their reified notions of ethnicity and culture had contributed to the production of interethnic violence in the first place.

This underlines the importance of diversifying the field of inquiry to different groups so as to produce a broader spectrum of local perceptions and experiences of conflict. If not, there is a real risk of lending authority to the narratives of a single group, while minimising the importance of others. The point is not that the extractions of more knowledge through more local perspectives produce a more scientifically valid representation of the empirical reality of conflict, but rather to show that any given conflict setting is co-constituted by a myriad of contesting and contestable narratives, each of which provides a different local perspective on it: its injustices, its inse-

curities, its legitimacy, its ‘good guys’ and ‘bad guys’, its causes, its consequences, etc. These narratives are inherently performative; that is to say they are intended to generate sympathy, empathy and enmity, scandalise, shock, impress, produce indignation etc. Researchers do not overcome the issue of the performativity of conflict narratives by simply ‘gathering’ local points view; on the contrary they risk being seduced by their politics of truth, and thereby legitimating them. Thus while sympathy and empathy towards their interlocutors is important for researchers, a consistently critical approach is equally necessary in order to go beyond simple self-glorifying narratives and their familiar dichotomies between ‘victims’ and ‘perpetrators’, ‘good guys’ and ‘bad guys’ and ‘good’ and ‘evil’.

### **‘BROKERAGE’ AND THE COMPETITION OVER THE ‘RENTS’ OF THE RESEARCH**

I returned to South Kivu in 2009 and 2010 to carry out fieldwork for my PhD with a new research agenda. From the beginning many of my former contacts expressed a strong interest in facilitating the research. I quickly noticed that a rather strong competition developed between my former contacts over collaborating with me: for the position of research assistant, to that of transcriber, to facilitating my housing, my transportation and so on. This competition involved making up stories about each other, sidetracking the research. I was even told that two of my friends and colleagues from UCB had almost got into a fight because one of them believed the other had implied to me that he was a drunkard and that he was not carrying out the duties asked of him, and that was why I had opted not work with him again. Moreover, I noticed

that several people started to refer to me as ‘papa’, which was more clearly a vertical power relationship compared to the notion of ‘frère’, or ‘ami’ which are more horizontal.

This time the objective of the field research was to investigate how different actors in a specific area exercise authority. It was a much broader, more complex and comprehensive research agenda. After much deliberation I decided to carry out the research in Bunyakiri, the former stronghold of General Padiri’s Maï-Maï group, since I was already familiar with the area. In order to facilitate the research I hired a former militia member who was from the majority ‘ethnic group’ in the area: the Batembo. He was well connected, both in Bukavu and in Bunyakiri, and he was well respected among the defected militia members who controlled certain parts of the area. He also had good relations with several FDLR officers who, on and off, had been allies of Padiri’s group during the RCD-G rebellion. Moreover, he spoke French and had been extremely helpful during my first research. Lastly, it was also a way of maintaining my relationship with the Maï-Maï, of showing them that I had not forgotten them. I also hired another person with academic credentials to his name to help with the transcription of the interviews. This person was from a different ethnic group; he was a Mushi, which politically and economically is the dominant group in the provincial capital of Bukavu. As I will show, this approach to the research would prove to be full of unforeseen consequences, but it also allowed me to get a first-hand understanding of the importance of ethnicised antagonisms in eastern Congo.

They quarrelled over a great many issues but at the heart of these quarrels were two issues: distrust based on ethnic background and competition over who was to be the bro-

ker between local society and me. As another friend told me they both want to be your ‘*enfant Chéri*’. With respect to the ‘ethnic’ issue it was noticeable in several ways. For instance in the way they spoke about each other’s ‘ethnic groups’ when the other was not present. Both of them blamed the difficulties we encountered during the research on the cultural attributes of their respective groups. Whereas my Bashi ‘assistant’ would blame the problems we encountered in Bunyakiri on the ‘uncivilised’ mentality of the Batembo, my Batembo ‘assistant’ would blame the problems on the ‘deceitful’ character of the Bashi.

During a meeting with representatives of the customary authorities of Buloho chiefdom in Bunyakiri my Mushi ‘assistant’ left the room. One of them leaned towards me and whispered: ‘If I were you I would not bring that Mushi to Bunyakiri, it could bring you trouble’. Without overestimating the importance of my research, I believe that my Mutembo ‘assistant’ was acting in the interests of the Batembo community and, more specifically, those of the officially recognised customary authorities of his native chefferie Buloho, with which he was quite close. I think that just as the Maï-Maï saw me as an asset, so too did certain members of the Batembo community see me as someone who had the potential to put them on the map, so to speak, and they did not want a Mushi involved with the research.

This issue of the political and economic marginalisation of the Batembo had been at the forefront during my research with the Maï-Maï. It was clear to me that underneath the surface of nationalist discourse there was a strong ambition to use the strength they had acquired by forming the Maï-Maï group for political leverage. This sense of being unjustly marginalised was still very strong among the Batembo I encountered in 2009 and 2010,

and most people I met seemed more than willing to talk about it. It was clearly a most important political issue for them and I started thinking that it this could be turned into an opportunity and it became a focal point of my research.

### **COMPETITION OVER THE RENTS OF THE RESEARCH: THE CAR**

As a means to illustrate the competition over the economic ‘rents’ of the research I will relate some events that unfolded when I attempted to buy a car. The process of purchasing a car vaulted me into the rough and tumble world of bargaining and brokerage in the economy of the Congo. Although it was an exceedingly frustrating process, which in many ways was counterproductive to the research agenda, it provided a valuable first-hand experience of the norms operating in the Congolese economy.

Neza Bilakila (2005, pp. 20–1) has argued that an individual in need of a good or a service, or who needs to resolve a problem in Kinshasa, almost invariably becomes the client of a broker. When this happens a process known as *la coop*, which he translates as ‘bargain’, begins. The ‘bargain’ implies trickery, and wheeling and dealing. The commission, which is generally very modest, is the stake of the bargain. Given the difficulties the vast majority of the population experiences to make ends meet in Kinshasa and the rest of the Congo, people are drawn into these multi-actor bargains. This takes place in all sectors of daily life and cuts across the social spectrum, and is subject to strong social regulation. Although the purchase of the car took place in Bukavu and not in Kinshasa, the process displayed all the hallmarks of *la coop*.

As soon as I arrived my research assistant began the search for a car. The first car he found was an old Toyota belonging to a friend of his, which I thought looked too worn out and expensive. The second car he found was a Range Rover, which he had found through a mechanic from Bunyakiri. It looked to be in better shape than the previous one. This car belonged to a local merchant. We took it for a test drive and it seemed to be in good driving condition. The price was higher than the previous car, but I agreed to rent it for a few days to make further tests of it.

The next day my research assistant brought another one of his friends from Bunyakiri; a mechanic working in a monastery, to help with the testing of the Range Rover. They took it for a test drive. The test drive dragged out and eventually I called my research assistant and asked him what was going on. He sounded agitated and he said that they would soon be back. A little while later he called me and claimed that the car had a serious problem with the front axle and the steering mechanism. In the meantime the mechanic who had facilitated the sale came to my residence and said that the merchant, whom I had not yet met, would like to see me. I agreed to meet him. He arrived at my residence with a couple of 'friends'. He was visibly agitated and angry and after the initial greetings he told me: 'We have a problem! You have cancelled the deal, but you had agreed to buy it'. I told that the deal was pending on the test of the car and that I had only rented it for a few days. 'Yes, but your friend said you wanted to buy it'. I replied that since I had not signed any papers the deal was not concluded, and that since the car has a mechanical problem I did not want to buy it. He claimed that that was a lie, and that he could drive me to Uvira (a town situated around 130 km south of Bukavu) and back to prove it if I wanted to. I de-

clined the proposition, and said that I would return the car as soon as my research assistant came back with it. I apologised for the inconvenience, after which the merchant left with his friends. A couple of hours later my research assistant and his contact returned. They said that they had submitted the Range Rover to a series of practical tests, which had revealed the problem with the front axle and the steering mechanism. I also noticed that my research assistant was quite agitated and fidgety. I asked them why the merchant had been so angry, and my research assistant told me that they had threatened him with going to the police for wanting to sell a defective car, as a means to force him to accept cancelling the deal. I told them that he should have contacted me much earlier so that I might have talked to the merchant myself. I then returned the car immediately to its owner with my translator. When we arrived there the merchant refused to take back the car. He claimed that my research assistant and his friend were the ones who had damaged the car, and that he wanted an indemnification, which I refused. In the end he agreed to take it back if I agreed to pay the full three days of rent in spite of the fact that I had only had the car for two days.

The search for a new car continued and eventually my research assistant and the monastery mechanic raked up an aging Toyota jeep through a nun from Bunyakiri, that her convent was willing to sell. It was in reasonable condition and I decided to buy it. Following these events I was informed by several of my other contacts, including my transcriber that the merchant and my research assistant had got into an argument over the commission. Apparently my research assistant had been promised 500 USD in commission for selling the car, but when this 'coop' was about to be concluded there was a dispute about the

amount of the commission. Until that point I had trusted my research assistant, but that event showed me that there were limits to how much I could trust him. I considered his attempt to secure himself a commission for selling the vehicle as an attempt at embezzling the research money, and in any case as a dishonest act. The incident shook me because I had considered him a friend and I trusted him to ensure my security.

Regardless of my feelings at the time the event should be seen in light of the practice of the 'coop', so frequent in the Congo. The practice of *la coop* is a version of what is known as *la débrouillardise* ('fend-for-yourself-ism'), which dominates the everyday economic interactions in the Congo. *La débrouillardise* emerged as the Congolese economy and public institutions collapsed towards the end of the 1980s. It is an expression which is always on the tip of the tongues of the Congolese. *La débrouillardise* is an immensely powerful and pervasive form of practice (Jackson, 2001; MacGaffey, 1987, 1991; Marysse & De Herdt, 1996). As a motto, *la débrouillardise* authorises restrained dishonesty, trickery and stealing in the struggle to survive in the face of the uncertain conditions of war, poverty and the collapse of public institutions. It is however, not to be confused with unrestrained selfishness. If you attempt to circumvent the bargaining system, it is seen as an attack on the social order in which solidarity with your kin and friends is seen as paramount. As my research assistant once said to me very honestly: 'It would be good if you could meet with some of your old Maï-Maï friends, because I can be accused of keeping you away from them. They can accuse me of keeping all your money for myself or of allowing you to go home to your university without having shared anything with them'. For me this encounter with the *débrouillardise* of everyday life

revealed in a striking manner the limits of my embeddedness in local society. Not only was I exceedingly frustrated and irritated by it, but it also showed that I was not truly part of the network of solidarity of my collaborators. Instead my position was liminal, at the same time inside and outside their network of solidarity. I was allowed in and protected by it on the condition that I offered something in return in terms of material and symbolic goods. However, at the same time it provided me with the opportunity to understand the living conditions of many people in the Congo. In that sense it illustrates the dilemma of immersion in a conflict setting, marked by distrust, poverty and the daily struggle to make ends meet.

## CONCLUSION

This paper has shown that cultural 'immersion' in conflict settings is suffused with tricky dilemmas. Carrying out fieldwork in a conflict setting inevitably entails that researchers familiarise themselves with the real and perceived risks of their interlocutors. As a learning experience this can be quite unsettling, even shocking, and a hazardous undertaking. Researchers therefore often find themselves caught up in the unsolvable dilemma of wanting to obtain an ever-deeper understanding of the lived experiences of their interlocutors, but finding themselves hesitating to familiarise themselves too much with their experiences because of the real and perceived threats that are involved. In this sense it shows the limits of the ethnographic method of cultural 'immersion' in high-definition. This 'dilemma' of immersion may be partly offset by working with local 'brokers', who are often better equipped for understanding, anticipating and navigating the specific set of

risks of the local conflict setting. However, working with local brokers entails its own set of challenges and risks, which themselves are shaped by the specific conditions of the conflict itself.

Studying conflict at close quarters shows that the study of human interaction and the values and interests that drive it differs markedly from the study of physical objects carried out in research laboratories. Researchers have little influence over how their interlocutors perceive and react to them and their research agendas. To a certain extent researchers have to make their interlocutors 'interested' in their research agenda, but they are not in control of what kind of 'interest' they develop in their research. That said researchers' personal histories profoundly affect the expectations and interests that their interlocutors develop in their research in a conflict setting; that is, while the symbolic and material interests that researchers' interlocutors develop in their research is to a large extent shaped by their life situation and by their perceptions of individual researchers, they are also shaped by how researchers interact with them. Much can be done to manage the expectations and interests of their interlocutors and 'brokers', but much is also out of their control.

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