Militarized youths in western Côte d'Ivoire: local processes of mobilization, demobilization, and related humanitarian interventions (2002-2007)

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Photograph 1: Patchwork of pro-governmental militia elements

Photograph 2: Female recruits, Guiglo
Methodology

For a number of reasons – mainly linked to length of stay and instruments used for collecting the data – this work is not a classical ethnography. It was nonetheless largely inspired by anthropological work and by actor-oriented approaches.\(^1\) With some exceptions, I did not observe respondents in situ (nor when engaged or when carrying out routine tasks); I focused instead on reconstructing their perceptions of past events by providing an artificial platform of exchange and by taking their life story as point of departure. This methodological choice was mainly guided by the object of study: after all, I was studying processes of mobilization \textit{a posteriori} and the bulk of the youths I interviewed had been mobilized in 2002-2003, five years before the start of data collection. The primary data I gathered foremost stems from 237 semi-structured one-on-one interviews\(^2\) I conveyed with male and female low-ranking recruits between 14

\(^1\) Actor-oriented approaches stress the interplay of internal and external factors when exploring social change. At the same time, they take the stand that human action and consciousness play a central role in that interplay. The concept of agency is central in this perspective and social actors are assumed to have a genuine capacity to process social experience, to devise ways of coping with life, even under the most extreme circumstances. A core feature pointed out by Giddens (1979) is that at any point in time, anyone has the capacity to act otherwise.

\(^2\) The 237 respondents consisted of 200 men, 16 women and 21 adolescents enrolled in a reinsertion project. I also interviewed 5 former militia recruits who were not involved in any project and a dozen local entrepreneurs who had agreed to take on some of these youths as apprentices in the workshops they owned. The detailed interview guidelines are presented in appendix 2. The guidelines were usually used as a checklist and interviews took the form of a friendly conversation.
and 35 years old. At the time of the first meeting, half of them had already left their respective armed groups (the pro-government militias) and nearly all of them were involved in an NGO-led reinsertion project. Systematic information was collected on their pre-war lives (educational and professional trajectories, relationships with close family, social networks), their motivations to join armed groups, their actions during the war (or perhaps better said, whatever they wanted to tell me about that period), and how they saw their future and options outside armed groups, including their own reflections on the reinsertion project in which they were participating. The relatively large number of cases from both belligerent sides had the merit of providing a great variety of patterns. The bulk of the data was collected in Man, Guiglo and Blolequin, the respective strongholds of rebel and pro-government armed groups in western Côte d’Ivoire. Perhaps I was too cautious, but I chose to conduct most interviews within the premises of three reinsertion centres ran by humanitarian agencies and used at the time of the data collection for project purposes. A few interviews were done in a village near Guiglo (Zouan) and in Abidjan, with former recruits who had not received any reinsertion assistance. The fieldwork periods were November-December 2006, April-May 2007, June 2007, and follow-up interviews were conducted in November-December 2007. I also continued to contact several respondents by phone until the summer of 2008, to get a sense of how they were getting on as the years passed.

Prior and during the data collection phase, I had to make a certain number of choices, weighing what I wanted to do against what at the time seemed realistic to accomplish. How should I empirically define ‘youth’ in this study? Which characteristics should I use to choose my sample? Where do I do fieldwork? How should I deal with child respondents? In surroundings extremely ‘humanitarian-ized’, how can I – if not avoid – at least minimize being confused with development practitioners? Additional dilemmas emerged while undertaking the analysis, not the least related to how to deal with issues surrounding categorization. And as I was largely drawing on respondents’ narratives, how to avoid falling into the trap of taking a too individualistic approach? I reflect on those points below.

Methodological choices

First dilemma: how best to contextualize individual narratives?

When drawing on personal narratives, the obvious risk is to fall into an extreme form of methodological individualism that partly distorts social phenomena be-

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3 I was helped by a research assistant in that process. We split the interviews among each other.
cause it fails to relate individual experiences with the wider structure. The analyst in fact has to be particularly careful not to exaggerate the instrumental rationality of individuals too strongly, or there is a real danger of remaining confined in a deterministic approach that bounds human choices and decisions by motives and previously existing causes, regardless of the structural aspects. To avoid such trap, I engaged in a process of document reconstitution to get a good grasp of the local context (by ‘local’ I mean the immediate environment of the people under study). I was particularly interested in developing a coherent version of the recent history of the western region by generating a detailed timeline of violent events since 2002, in order to get a sense of the general atmosphere in which respondents had evolved. After all, in many cases, among the driving rationales for explaining military engagement were the circumstances. I therefore extensively tapped into journalistic sources, particularly Ivoirian newspapers archives (Fraternité Matin, 24 Heures, Notre Voie, L’inter, Le Front, Soir Info) and those of national and international press agencies (Agence Ivoirienne de Presse, Agence France Presse, BBC, PANA Press, Reuters). Luckily, Côte d’Ivoire is home to a plethora of media and hosts at least a dozen daily newspapers with wide circulation throughout the country and easy internet access. Far from denying the partiality of certain sources and the politicized nature of some documents (Ivoirian newspapers are well-known for their political engagement and aggressive tone), my goal was to extract the most ‘factual’ information; hence, I treated the various articles as valuable primary documents that accounted for a particularly violent period. I completed this documentation with UN and INGO situation reports on western Côte d’Ivoire, impartial forces updates, and secondary sources (International Crisis Group analysis, IRINNews, and UNOCHA Bulletins). This reconstruction work had the merit of clarifying the different conflict phases and of introducing a certain temporality to the analysis of the conflict in the west of the country. It was a necessary step to understand, \textit{a posteriori}, local processes of mobilization.

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4 Long (2001) in fact warns us that many ‘micro’ studies fall short because of a tendency to adopt a voluntaristic view of decision-making, by highlighting too much the transactional nature of actors’ strategies while not examining enough how these individual choices were shaped by larger frames of meaning and action and by the distribution of power and resources. One acts in a certain way not only because of individual characteristics. The ability of people to build up room to manoeuvre only takes on full meaning when it is related to structural aspects and specific historical patterns. These actually partly explain how such room to manoeuvre is framed (Abbink, 2005; Carney, 1999; Giddens, 1979).

5 A summary of this chronology is presented in Chapter 5, the detailed version in appendix 1.
Second dilemma: doing research in militarized settings

Man and Guiglo were both extremely ‘militarized’ and ‘humanitarianized’ in 2006 and 2007 – the period when I was doing fieldwork – which raised several dilemmas. How could I best approach such a messy field without unnecessarily putting myself and others at risk? How could I avoid being taken for a humanitarian practitioner while using the premises they used to conduct my first interviews? In Man, at the time of doing fieldwork, the local administration was completely managed by rebel officers. No recruits had yet been officially demobilized and none had received financial compensation. The mere prospect of releasing low-ranking recruits was not even debatable with the highest in command five years after the start of the conflict. In Guiglo, 981 militia elements had gone through a demobilization process and had received some kind of financial compensation. The local administration was also fully military and pro-government militias had an extremely bad reputation. They were particularly prone to hostile demonstrations against the UN and the French impartial forces, a characteristic I could fully observe in early 2006, when I was not yet doing fieldwork for this study but was nonetheless in the area for consulting activities.6

It was in such settings that I started my research at the end of 2006. Needless to say, timing was quite crucial for the success of my data collection and I could probably not have approached the youths the way I did if I had conducted the interviews earlier (at least in Guiglo). If I was regularly monitoring the changes taking place in terms of local security, my fieldwork occurred during a relative period of calm and I was never caught in the wrong place at the wrong time.

Because of the sensitivity of the topics I wanted to talk about and the risk that my actions could be misinterpreted by the military hierarchy if I went into too much detail, I adopted a rather low profile and opted, for this particular project, not to seek formal approval of the local authorities when carrying out the research.

When I met the military Préfet in Guiglo and the Com’Zone in Man, I introduced myself as a researcher from the University of Amsterdam interested in studying the impact of war on local youth. I purposely kept the definition broad to avoid

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6 On 18 January 2006, following a peaceful demonstration in front of the UNOCI base to protest against a controversial communiqué (the International Working Group on Côte d’Ivoire had just announced that the mandate of the Ivorian National Assembly due to expire on 16 December 16 2005 would not be extended), the event degenerated into a violent confrontation between the UN Bangladeshi peacekeepers and an angry mob. It resulted in the deaths of five protesters and the wounding of thirty-nine others (Human Rights Watch, 2006). In response to the shootings, a militia leader called everyone on the radio ‘to come into town to avenge the death of those struck down by the assassins’ bullets.’ Shortly after, militia leaders once again used the radio to incite violence against all UN and humanitarian organizations. Several offices were burned, twenty cars tagged humanitarian were extensively damaged, office equipment was looted (computers, electric generators), as well as food and medical supplies stocks (the WFP warehouse was completely emptied).
giving the impression that I was only focusing on militarized youth. As an extra precaution, I temporarily downplayed my French identity, overemphasizing my Dutch background instead. This discourse was generally well received and in Guiglo, the Préfet was particularly keen on sharing his thoughts on the subject. As I did not feel comfortable undertaking such research without the quasi certainty that an umbrella organization would help me reach a safer place elsewhere should the local situation abruptly deteriorate, I approached two international development agencies and asked them to include me in their evacuation plan. One of them was the German NGO GTZ-IS. I had heard they had just begun a pilot project in Man and Guiglo for reinserting young recruits and I used them as gatekeeper to get access to 216 respondents.

Third dilemma: doing research tapping into humanitarian practice

There is an inherent tension in doing research while tapping into practice and in combining an approach that critically examines actions of development by using external interventions as means of accessing the bulk of respondents. There seems to be some kind of irreconcilable contradiction between the two and a real danger of bias for the analysis. I would argue that such a tension can yield productive results if the researcher pays sufficient attention to a range of things. To begin with, planned interventions offer a relatively easily accessible tank of potential respondents. While some would argue that it biases the selection of cases, not selecting respondents who are engaged in projects would probably also lead to bias as humanitarianism has become so much part of the local environment that it would be a mistake to systematically discard anyone who participates in an NGO-driven intervention. Now indeed, critics point more to the question of balance. The problem is not so much framed in terms of whether or not project participants should be excluded from a selection of cases, but to keep a fair proportion of those who are benefiting from some kind of assistance and those who are not, to overcome the risk of encountering the same type of profile among the respondents. I would argue that such precaution is perhaps not necessary in certain contexts where external interventions have very limited impact on people’s lives. Also, it tends to conceive ‘project participants’ or ‘target populations’ as a homogeneous group, with ‘post-project’ lives drastically different (and better) from the ‘pre-project’ ones. In reality, there is a great variety of patterns and different individual responses to similar structural and circumstantial conditions. In western Côte d’Ivoire, the reinsertion projects under study had very limited impact on project participants’ lives. I nuance that point more in Chapter

7 It could be hypothesized that those who benefit from programme interventions have a better social capital than those who do not, so that the socially marginalized are left out.
9, but the general impression was that the project fulfilled more a function of networking (with project participants adding ‘fellow participants’ and ‘project staff’ to their social network and range of opportunities) than a function of support per se, although this indeed varied depending on individuals.

The fact that my initial encounters took place in centres run by NGOs for reinsertion purposes was nonetheless an issue I needed to mitigate. During my first visit to the centres, NGO staff – with approval of their hierarchy – organized an informal meeting with the youths present that day, where I was given the opportunity to introduce myself and to explain why I was there. For the sake of clarity, I explained that I was interested in hearing the life stories of young people who had spent some time in the militias/rebellion in order to compile such testimonies in a book. I emphasized that I was not interested in names, but in understanding from their points of view what drove them in and out armed groups and why they acted the way they did. I also emphasized that nobody had to meet with me if they did not want to. Interviews were not compulsory. I stated several times that I was not part of the project staff, that I could not materially help (so as not to raise undue expectations), and that any information shared with me would be kept confidential. While it is unlikely that everyone present at the meetings understood clearly what I intended to do, several youths volunteered to talk to me on my next visits. In terms of order, as I came to realize afterwards, the first persons I met in Guiglo were close to the militia leaders, some were even related to them. When I interviewed adolescent recruits in Man (they were hosted in a separate centre), the first person I met was the ‘President’ of the youths, followed by main members of his ‘bureau’. In order to help basic project management, the creation of a certain hierarchy amongst the children had been encouraged by the local NGO running the centre to facilitate collective interactions with project staff. A ‘President’, a ‘Treasurer’, a ‘Secretary’, a ‘Chief of Hygiene’ had therefore been named by their peers and were mediating collective demands. During my next visits, more and more adolescents registered their names to schedule an interview with me, probably reassured by what early interviewees had reported to them and by my frequent visits to the centre. If one message had been clearly passed on, it was that I was not a threat.

In Man, Guiglo and Blolequin, most interviews were done inside, in a quiet room, and it was usually not a problem to conduct them in French (communication was difficult with ten respondents in Man and interviews had to be cut short as I did not have enough knowledge of their respective local language to be able to carry on in-depth interviews without the help of a translator). Some interviews were filmed, some were taped and I took notes of the rest. With the
youngest respondents, nearly all exchanges were filmed, unless they did not want this to be done. I was more cautious with older recruits, as I feared – perhaps too much – that there was a greater risk my intentions would be misinterpreted. I, for instance, intentionally avoided asking from the start if I could tape interviews. It was only if I felt a conducive climate in the one-on-one exchange that I would ask the respondent if he or she would not mind if our exchanges were recorded. Although I had my camcorder with me nearly all the time, I was determined to only use it if I was completely sure that my intentions would not be misunderstood.

Fourth dilemma: how to empirically define youth for the scope of this study?
Since this work focuses on militarized youths, there is the need to define what is understood to be a ‘youth’ in this particular study. To be young is not a matter of biological age, and many scholars would agree with such a statement. Chauveau defines youth in terms of relational position. To him (and I share his view), being young is socially and culturally constructed, in relation to other generations, and in relation to access to relevant assets and resources that confer a certain social status (Chauveau, 2005a). If the notion of youth is a heuristic concept, there is also no universal definition of childhood, and the concept remains locally defined. Conceptualizing youth and childhood this way, in terms of local categories of perception, is in sharp contrast with the ‘target group’ categories built to meet the needs of external interventions. Those categories are in fact often constructed in ad hoc ways and according to age benchmarks defined by international standards, which are quite far from the lived realities of the people they attempt to define. In the normative approach promoted by planned interventions, a child is considered a child until the age of 18 (the age after which he/she is no longer eligible to receive child benefits), and a youth usually ceases to be young at 25 (the age limit is sometimes extended to 30). Defining childhood and youth along these lines inevitably suffers from a lack of solid grounds at the local level. Rosen has been among the fervent critics of such ‘politics of age’, a term he himself coined. When reflecting on the definition of childhood (Rosen, 2007), he argued that one effect of the ‘straight-18’ focus widely promoted in international law has been to shape the concept of childhood in a very strict way (bounded by numerical age) at the expense of more interesting reflections, discounting the more varied and complex local understandings of children and childhood and using age categories as instruments to advance specific agendas. Rosen especially pointed out that the mainstream discourse has tended to stifle the fact that

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8 It never happened. I forgot my camera twice during the period I did interviews, and each time, children expressed some disappointment.
older teenagers are likely to differ from younger children in many ways and that there is a tendency to infantilize 16 and 17-year-old recruits in contexts where adolescence and military life are not necessarily seen as antinomic. He has also stressed that by focusing too much on the older teenagers (the recruitment age debate focused on the ages 15 to 18), the youngest ones have been forgotten, despite the fact that many child recruits are in their early teens when they are recruited (which this case study also illustrates).

But if youth and childhood are conceived as relational positions locally defined, does this thus mean that there are no limits to calling someone ‘young’ or a ‘child’? Abbink (2005) strongly argues in favour of such a benchmark, arguing that having no strict definition for youth does not mean that middle-aged people should be categorized as young. Even if some of the ‘middle-aged’ share common characteristics with younger persons (not yet having secured stable work, not yet having been in a position to raise a family, etc.), many of them miss the transition to adulthood because of poverty and deprivation (ibid) – I would add bad luck. As years pass by, their lives take a tragic turn and they eventually lose their youth. Their future no longer lies before them.

The line between ‘young’ and ‘child’ is a more blurred one and needs to be framed along cognitive development terms. Recent research has shown that children in different cultures are likely to engage in complex moral reasoning at a much younger age than expected (Boyden, 2007; Rosen, 2005; Rosen, 2007). In developing countries in particular, where most people are used to fending for themselves from an early age, context and experience have proved to play a

In the late 1990s, there was considerable debate on which minimum age for recruitment to set in international law and a range of actors actively lobbied for abolishing the then marker of 15 as the minimum tolerated age for recruitment, and for raising this age limit to 18 in official documents (Harvey, 2000). INGOs were particularly active in pushing this view, along with the International Committee of the Red Cross, the Swedish government and the Quaker UN Office in Geneva (Brett, 2005; Rosen, 2007). Those in favour of the change were mainly arguing that the 1989 UN Convention of the Rights of the Child (UNCRC) had failed to completely prohibit child recruitment and were particularly keen on underlining a contradiction in the Convention since on the one hand, a child was defined in the text as anyone under 18 in need of special protection, and on the other hand, recruitment of 16 and 17-year-olds was tolerated. Proponents were therefore keen to have such an ‘anomaly’ corrected in international law by raising the benchmark to 18. On the opposite side, people feared that too much focus on age would distract international attention from more fundamental issues such as forced recruitment. Ryle (1999) notably argued that what eventually mattered was the way conscription took place, not chronological age. Whether recruits are 16, 18 or 21 is of lesser importance, as long as these people willingly enter the force. After several stalls in the negotiation process, an optional protocol to the Convention of the Rights of the Child on involvement of children in armed conflict was adopted in 2000 (United Nations, 2000). It called on states ‘to take all feasible measures to ensure that members of their armed forces who have not yet attained the age of 18 years do not take a direct part in hostilities’ (Article 3). With respect to state armies, it forbade compulsory recruitment before the age of 18 while tolerating voluntary enlistment of 16 and 17-year olds, provided states maintain minimum safeguards to protect the minors in their ranks. With respect to non-state armed groups, it unconditionally forbade recruitment under the age of 18 and criminalized the practice (Article 4).
major role in the development of human cognition and in influencing modes of action. Age and maturity are no longer conceived as necessarily going hand in hand, and adulthood is no longer overestimated by assuming grown-ups’ immunity to being influenced.

For the scope of this study, I gave up trying not to bracket ‘youth’ into two figures, as I kept meeting 14-year-old mothers who were already fending for themselves for several years and 42-year-old ‘Présidents des Jeunes’ in the areas I toured (which appeared somewhat odd since they generally did not resemble adolescents or twenty-year-olds or people in their early thirties). If I tried in the beginning to look for some kind of remnants of an intricate age system in the societies I visited (a system in which generational issues are mitigated by the assigning of a social role to age groups, the maintenance of clear boundaries between them, and the existence of strict codes of behaviour), it became clear that such a system was long gone in western Côte d’Ivoire, and that the civil war had challenged it even more (despite the fact that some underlying ideas continued to persist in terms of reciprocity and mutual obligations). So when is one young in this book and who was included in the sample of militarized civilians? I pragmatically opted for men and women between 14 and 35 years old.

Ethical considerations

Beyond methodological challenges, studying militarized youths is an ethical minefield. The ‘do no harm’ imperative is trickier to reach in conflict zones due to exacerbated political polarization, the presence of armed groups, and the general unpredictability of events. I tend to agree with scholars who point out that research cannot be ethically conducted everywhere and should not even be attempted in certain settings. One difficulty is to find satisfactory ways to address consent. If tackling the issue is often used as an example of good practice leading to an ‘ethically correct’ research, how best to do so is a major point of debate in culturally diverse settings. Another difficulty is to mitigate the potential stress respondents might experience as they recall disturbing events. But doing research on armed groups in war-affected areas is also disturbing because the researcher is fully confronted with certain practices and ideas about violence, whatever his/her own views are on deontological ethics. To what extent can one suspend judgment? How to minimize emotional shakes? Can one keep a fresh mind after hearing about the same atrocities for the umpteenth time? Researchers are not immune to feelings, and some discourses can be hard to swallow, even for the most open-minded. Wood (2006) mentions these emotional dynamics because she is persuaded inadequate attention to them may lead field researchers to make errors in judgment.
The ‘do no harm’ imperative

How to define ‘harmful’? Drawing on his experience as a member of a Canadian Research Ethics Board, Haggerty (2004) puts it this way: ‘In the eventuality that a research project poses a greater risk than what a person might encounter in his/her daily life, these risks must be managed by the researcher or the research cannot be conducted.’ Since they are related to a broader context than the research project alone, ethical concerns are put in some kind of perspective and the impact of social science research projects on people’s lives is not overestimated. Haggerty in fact specifically warns about the current tendency to overrate the potential harm of research, mentioning a certain form of ‘ethics creep’ that invades social science research in the name of ethics.

Did I put research participants at a greater risk than otherwise in their daily lives? I doubt it. It would be largely overestimating the impact of this research. Notwithstanding a few exceptions, I genuinely believe the bulk of respondents were smart enough to decide for themselves whether they wanted to speak with me and what information they wanted to share. They also usually had a much more developed sense of risk than I did. I was indeed a bit nervous at checkpoints when I had on me transcripts that could prove the implication of a youth with an armed group ‘from the other side’. But how likely was it that my bag would be searched? And if it was, how likely was it that respondents would be traced back or blacklisted on the basis of first names and villages of origin? Would a pile of paper raise a soldier’s attention? There was surely a greater chance of my CFA francs being ripped off. If particular biographical details make my respondents recognizable – and I know an obstinate reader would be able to trace them back should he or she be patient enough to crosscheck all interview fragments in this book – it was highly unlikely in the field that a person would take time to do that with the aim of confronting someone.

But since a method I used was to ask young people to describe the distressing events they experienced during the war (thereby obliging them to recall disturbing memories as they were presenting themselves as either victim or perpetrator of violence), I had to minimize the psychological harm that might derive from such a recollection. It is always tricky to put yourself in the place of someone else and to evaluate the psychological damage certain questions can trigger. Different individuals have different reactions to the same experiences, and events that appear to me (and others) to be terrible do not necessarily affect the people who experienced them the same way. While doing fieldwork, I finally opted for inquiring about harrowing events without pushing my respondents if they did not want to elaborate much. If they wanted to talk and to describe what happened to them, I took the stand that they were willingly doing so and that they were conscious of the consequences; it was not unethical of me to listen. Some res-
pondents mentioned having experienced nightmares for a while after having fought in the front line, others felt that they had to take some distance before returning to their former environment, to make a ‘fresh’ start. With regard to the ‘do no harm’ imperative, Haggerty (ibid) makes a nice parallel with social scientists and journalists, basically arguing that while both conduct interviews, videotape people and undertake some forms of participant observation, journalists are much less bounded by ethical protocols and it is expected that a story be told, unless the interviewee clearly mentioned that certain information has to be off record. The assumption is also that respondents get quoted, unless special reservations are made. In academia, the initial assumption is the opposite: research participants remain anonymous unless they provide explicit permission to be identified, and the content of information (the nature of what to write) is heavily weighed, especially when some information appears sensitive and subject to an interpretation that might endanger the respondent or distort an ongoing process. When I mentioned that I would not use real names in the book, many respondents told me that they would not have any objections if their real name was used. For the sake of precaution, I eventually adopted the scholarly attitude and opted for standard anonymity.

**Consent**

It would be overestimating my explanatory skills to assert that interviewees came to me in full understanding of the potential risks and benefits of participating in this research. I also cannot ignore the power imbalance between myself and the youths I interviewed, or the differences of perception and interpretation of some words and concepts across cultures. If, when presenting the topic of my research to potential participants, I was careful to say that individual interviews were not compulsory, I cannot discount the fact that the mere notion of what is mandatory and what is voluntary differs across contexts, and I could be easily challenged if I were only to base my argument for informed consent on the basis of this distinction.

Power imbalance is an inherent part of society. It is present at the local level (based on race, sex, wealth, age, gender and ethnicity) and it is also present in the Researcher-Subject relationship in research. In Man and Guiglo, I was by far better off than the youths I interviewed. I was staying in accommodations that exceeded local standards (in Man, I was hosted in the guest house of an INGO; in Guiglo, I was hosted in the guest house of a local timber company), I was sometimes eating in fancy *maquis*,\(^\text{10}\) and – perhaps the most peculiar aspect – I

\(^{10}\) A *maquis* is a local bar, where you can also eat. It is an Ivoirian term.
was only passing through. Everyone I met knew I would be leaving after a while. On top of that, I was a young white woman in her early thirties – an outsider, clearly – and while this had its advantages in the sense that some people shared information with me that they probably would not have shared with someone else (a compatriot from their close social network, a younger or an older ‘passer-by’), I know it also had drawbacks and it was sometimes difficult to grasp certain subtleties. Was I seen as ‘being too outside’ to be a good listener? I don’t think so. I may have been the only person to have shown a genuine interest in listening to these youths’ life stories, and I was probably the only one to whom they narrated their life, from their first memories of elementary school to their support network over time, and the range of paid activities they had engaged in since they started working. The way I was perceived was not necessarily linked to people’s level of education. I had very good discussions with people who had dropped out of school at an early age and some exchanges were far less rich with people more educated. The quality of the discussions mostly depended on the one-on-one interaction between interviewee and interviewer, and on the interviewer’s capacity to follow the thread of discussion, allowing interesting digressions and shifting back when needed.

As research protocol, I opted for oral consent procedures as I did not want to formalize the exchanges too much; it also seemed to be the most culturally appropriate method. Respondents were briefed on their right to withdraw at any time and on their possibility not to answer certain questions. Although no-one cut an interview short, some remained evasive on certain points and a few refused to answer a particular question. When this was the case – usually while talking about war experiences – I stopped probing for more details and usually changed the subject. What strikingly comes forward though from this study, in line with other ethnographies of civil war (Weinstein, 2007; Wood, 2006), is that someone who has experienced war at first hand is usually genuinely willing to discuss his or her story with an outside researcher, foremost to ventilate personal feelings and to correct what has been told. There are so many negative stereotypes associated with combatants.

Am I being lied to?

If I compare my experience as a humanitarian practitioner with my experience as a researcher, the main difference in approach lies in one fundamental assumption. As service provider, my main conviction was that people around me were in general lying. Or to perhaps put it better: I was genuinely convinced that they were overstating their miseries in order to receive more assistance (mixing structural issues with the effects of war is the common trap of humanitarianism). As researcher, I took the opposite stand and the more incredible a story was, the
more likely I was inclined to give it credit. When I reflect on this 180-degree turn, the best argument I can think of is that – in my new position – people had much less reason to lie to me or exaggerate stories. After all, what I was offering them was no more than a listening ear and a vague opportunity to anonymously appear in a book. The diversity of stories I heard comforted me in this idea. Some of my respondents clearly presented themselves as fighters, some stated during the interview that they only occupied backstage positions. And while some initially introduced themselves as combatants, the rest of their story nuanced their position to a great degree. Interestingly, I was sometimes tipped by some respondents about their peers: that one had stayed behind, that one had gone to combat, that one told lies (recall here that I mainly drew from stories of recruits who were enrolled in a reinsertion project so they all knew each other, at least by sight). This was indeed an additional layer of information to weigh. But if there might be some kind of stake associated with being known for having fought in battle (perhaps the feeling of being more entitled than others to receive some form of compensation), individual deeds during war were not particularly over-rated during interviews.

But there is still a main caveat when tapping into someone’s memories. When relying on testimony that describes events that happened several years ago, respondents run the risk of mixing individual experiences with constructed or imagined narratives. In most cases this is quite unintentional. Yet it occurs rather frequently and the challenge for the analyst is to try to disenmesh the two. Another tricky point is to address the complexity of assessing the credibility of a story properly. Several studies have looked at the relationship between memory, narrative and credibility, especially drawing on refugee testimonies (Cohen, 2001, Hegel 2002, Kalin, 1986 cited in Lammers (2006) mainly arguing that incoherence and inconsistencies in someone’s testimony are not necessarily proof of forgery. What I experienced in the field and what at first resembled inconsistent storytelling was in fact a genuine difficulty respondents experienced when asked to follow some kind of chronological thread. They were constantly navigating between different periods and themes and had tangible difficulties in projecting themselves in a particular moment during the war. It was a real challenge to follow that up in the right manner and probing was constantly needed to get a story straight.

Emotional shakes

I was told many things. Someone regretted he had had to kill prisoners, another confessed he had enjoyed torturing them, a very young female respondent ended up as sex slave of a local commander, and one will be scarred for life by all the beating up he underwent. Some testimonies speak for themselves:
'It is only when I remember the fightings, the death, when I think about what I did that I start to feel remorse. Killing someone is not an easy thing. We could not kill the prisoners of war, but sometimes, when we had caught 4 or 5 people, we had no other choice. We could not guard them and stay behind, we needed to move on and continue to fight. So sometimes, our chief commanded us to go with them, he used to say “accompany them”. That meant we had to kill them. So if you’re not in a strong state, you cannot do it. When you look a man in the eyes, you cannot shoot him. Never. When I think about it (...) But since I was given an order, I had to. Afterwards, it takes hours to raise your spirits, to be strong enough. You’ll smoke a lot, you’ll drink a lot, until you fall to sleep. It’s not easy.’

Apart from being a good illustration of a reality that lies miles away from any of mine, this story is disturbing because one cannot avoid experiencing empathy, even for the cruelest acts. Perhaps placed in the same circumstances, I would have done the same things. These reflections led me to read about the ethics of war and peace, and specifically on what responsibilities are at stake when obeying orders. Is there some kind of inalienable moral duty that always prevails, regardless of context? And if so, what is permissible in times of war and what is absolutely not? If there are several approaches to these dilemmas (from Huntington’s moral obligation to obey to Cramer’s moral objection argument), I tend to be more comfortable with approaches that take the stance that in extreme circumstances, the direct perpetrators of violence – the ones who follow orders to kill – somehow enter into a process of ‘dehumanizing’ their enemy. The effect is to undermine their authority as moral agents and their sense of judgment (Dower, 2009).

**Self-censorship**

I did not approach all respondents the same manner and my data suffers from a conscious gender/age bias. I did not use video with female respondents, for instance, I did not even ask if they would bother. I was afraid to get dramatic confessions of violent sex scenes and I did not want to get that on tape. It seemed to be indecent to do so. Looking back on this self-censorship, I realize the precautions I took may have been unnecessary. With me as interviewer, women had no less agency than men in deciding what they wanted to tell and what they preferred to keep to themselves. I had no intention to push them. With children,

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11 Huntington’s position is that as long as an order is lawful, soldiers have a moral obligation to obey. In his view, soldiers carry no moral responsibility for war crimes committed in response to a superior’s orders and the principles of obedience isolates the culpability for war crimes with those commanders who initiate such orders. Huntington’s argument is mainly based on the assumption that without complete and unquestioning obedience, military units will be unable to perform their functions. Cramer instead points out the *moral objection argument*, which designates a situation when an order received dictates an action that is morally wrong. In Cramer’s view, the duty to follow orders is not unlimited and actions that would be illegal or immoral for individuals to engage in normal times (such as acts of torture, mutilation or intentional harming of innocent) are not permissible for a person based on a ‘blanket of immunity’ (Cramer, 2006).
curiously, I had fewer reservations. They were between 14 and 18 when I first met them, and as they had already been old enough to carry a Kalashnikov rifle, I assumed they would also be old enough to know how to handle me. I therefore took the stand that they were able to engage in complex moral reasoning from an early age, in line with other critical scholars’ views who argue that in places where people are used to fending for themselves from a very young age, context and experience play a major role in influencing modes of action (Boyden, 2007; Hart, 2009; Rosen, 2005).

I would have liked to personally interview mid-level and high-ranking officers to get their inside view on the mode of functioning of their group. It would have brought quite a comprehensive level of detail. But, given the circumstances, I did not feel comfortable approaching them with such a topic. If I did not seek a lot of contact with local commanders, I nonetheless had the opportunity to meet some of them. During my stay in Guiglo, I interviewed a former militia leader who had converted himself into a development broker through a local NGO aiming at reintegrating ‘deviant young men’ into society. Our conversation was more focused on his current work than on the armed group he formerly led, yet it yielded interesting contextual information and helped put things into perspective. I also attended a workshop on social cohesion organized by two consultants commissioned by the European Union. The workshop had the advantage of providing a setting where key local actors were gathered in one place at the same time: the military Préfets of all the western departments under government control, the village chiefs, the chefs de cantons, the representatives of the non-autochthonous communities, various local representatives of several committees (the committee of the allophiles, the committee of the returnees, the committee of the displaced, at both regional and micro-local level). The sous-Préfet of Pêhé was particularly open to discussion and we exchanged emails. In Man, discussions with high-ranking officers were couched in much more wooden language. With the Com’Zone, exchanges were primarily protocolar and I was particularly concerned to use neutral language, fearing — again, maybe too much — that I could jeopardize my research if the highest in command would become suspicious about my presence in the area. Talks about reinsertion prospects were out of place as releasing recruits on the rebel side was not an option at the time, and I found it inappropriate to discuss other affairs. When I had the opportunity to informally speak with a rebel sergeant in a local maquis — he was actually partly detached to serve as day guard in one of the centres used for reinsertion purposes — our conversation remained focused on his life story and on his current extra-military activities. He was mainly making his living by managing the real estate he had in Abidjan (rental stores and houses) and he also told me that he was getting a weekly incentive for his participation in the rebellion. We did not talk
much about his military involvement as I did not have the impression he wanted to elaborate on it.