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# Learning “To Be” Kinyarwanda

Reflections on Fieldwork,  
Method and Data in the  
Study of Rwanda’s Transition

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# **Learning “To Be” Kinyarwanda**

## **Reflections on Fieldwork, Method and Data in the Study of Rwanda’s Transition**

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## **ABSTRACT**

The objective of this paper is to document the research process underlying a study on the Rwandan transition. An extensive documentation of the research process is needed (although rarely systematically undertaken) in order to understand or assess rigor (scientific and empirical) and reflexive activities deployed in the achievement of the study results. The underlying source of inspiration to do so are questions of validity that guide social science research as such. As a consequence, trustworthiness and phronesis are central concerns due to the particular epistemological intake and research strategy adopted. The paper describes the fieldwork activities, choice and use of research techniques, the reflective process guiding design and analysis, and provides an overview of the data. The paper documents five main research principles underlying and guiding the study: immersion, iteration, multi-sitedness, mixing methods and diachrony. Two main research techniques are discussed in detail: systematic observation activities and a life history approach. A detailed overview of the nature of the available data as well as a reflection on issues of epistemology and ontology concludes.

## 1. INTRODUCTION

The objective of this paper is to document the research process underlying a study on the Rwandan transition. Substantive findings are presented elsewhere (for example Ingelaere 2007a, 2008, 2009a, 2009b, 2009c, 2010b, 2011a, 2012a, 2012b). The broader questions asked were: “what does it mean to live through a transition, from ‘peace’ over violence into ‘peace’ and from one political regime into another?”, “how did the *gacaca* courts – a tradition-based transitional justice mechanism installed to deal with the legacy of the Rwandan genocide – function in the context of such a transition?” and “how did the *gacaca* practice influence this experience of transition”?

An extensive documentation of the research process is needed (although rarely systematically undertaken) in order to understand or assess the rigor (scientific and empirical) and reflexive activities deployed in the achievement of the study results. This paper is motivated by the fact that also studies adopting a qualitative or inductive drive – one of the characteristics of this study as will be explained below – need to demonstrate rigor and therefore take into account key notions in research such as reliability (are measures consistent?), replicability (is study repeatable?) and validity (are conclusions well-founded?). However, these notions are generally associated with a positivist stance, a quantitative drive, the testing of theories. Many authors have called for the adaptation (for example Mason 2002: 38-39) or a translation (for example Lincoln & Guba 1985) of these criteria.

The overview presented here is motivated by such a translation exercise centered around the idea of *trustworthiness* (Lincoln & Guba 1985: 289-331): is the research carried out according to the canons of good practice and are the research process and the research findings open for scrutiny to others (*credibility criterion*); does the research provide rich accounts and detailed information so that a database emerges that can be used to assess relevance for other milieux and settings (*transferability criterion*); is there a complete record of all phases of the research process (case and respondent selection, sampling criteria, interview procedures, decisions taken to construct and alter research design, analytic strategies adopted) (*dependability*); did the researcher act in good faith by not allowing personal values and theoretical concerns to overtly influence the research process (*confirmability*)? These criteria will not structure the paper, they are the underlying source of inspiration.

From the outset it is therefore needed to articulate the general orientation of this study on the Rwandan transition. I like to think of this study as a ‘*phronesis-like*’ approach (Flyvbjerg 2003; Schram & Caterino 2006; Flyvbjerg, Landman, Schram 2012).<sup>1</sup> Flyvbjerg (2001) introduced the Aristotelian concept of *phronesis* in a broader argument on the characteristics of the social sciences and in an attempt to make social science matter (again). By elaborating on the concept of *phronesis* he “called for social science researchers to revise their standards for acceptable research methodologies, giving up fruitless attempts to emulate the natural sciences and instead reincorporating context-sensitive research, such as case-studies, narratives and datasets that help social actors learn to appreciate the complexities of social relations and practice various social crafts, including policy and change more effectively” (Schram 2012: 2). Important in such an approach is an intimate familiarity with contextualized settings. *Phronesis-like* research is not a method, but can provide a number of methodological guidelines (Flyvbjerg 2001: 129-140): focusing on values; placing power at the core of the analysis; getting close to re-

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[1] I prefer to use the notion “*phronesis-like* research” (Flyvbjerg 2003: 129, 162) since I cannot claim to have fully taken into consideration all dimensions of a ‘*phronetic* social science’, which is however not the idea if I understand Flyvbjerg correctly. A *phronetic* social science is neither paradigm nor method and ‘*phronesis-like*’ research is practiced in many ways.

ality; emphasizing little things (local micropractices); placing practice before discourse; studying cases and contexts; asking how? (not only why?); joining agency and structure; dialoging with a polyphony of voices. The adopted design of this study and the analysis presented throughout the different publications evoked above resonates with these methodological guidelines.<sup>2</sup>

This paper explains the fieldwork activities, methods and research techniques adopted, the reflective process undertaken in design and analysis, and it also provides an overview of the data. A first section explains the five main research principles that underlie this study: *immersion*, *iteration*, *multi-sitedness*, *mixing methods* and *diachrony*. Each of them is described extensively below.<sup>3</sup> The subsequent section discusses the involvement of Rwandan “interpreters”, my research collaborators. Then the two main research techniques are discussed in detail: the systematic observation of *gacaca* proceedings and a life history approach. Another section will provide insight into the use of other research techniques such as interviews, group discussion and (participant) observation in the field. A detailed overview of the nature of the available data concludes paper as well a reflection on issues of epistemology and ontology.

## 2. STUDY DESIGN: RESEARCH PRINCIPLES

Emerson et. al. (1995: 29) state that when “first venturing into a setting, field researchers should ‘cast their nets broadly’ [...]”. I spent an extensive amount of time in the field spread over repeated visits: approximately 32 months from 2004 onwards.<sup>4</sup> Indeed, as I will argue in this paper I attempted to cast the nets wide and deep as well: I explored events, interactions and phenomena that, at first sight, are not related to the *gacaca* activities; I gathered a massive amount of ‘pieces of the real’, traces of the reference reality. The reference reality is that particular aspect (piece) of social space and time that the researcher wants to report on and that he/she aims to understand (Olivier de Sardan 2008: 8 & 47).<sup>5</sup> It is important to understand that the notion ‘reference reality’ does not refer to an objective truth to be discovered with the ‘correct’ method and techniques. Rather, ‘reference reality’ is referring to the research topic under investigation. This ‘reference reality’ under study is considered to be single and plural, subjective and objective. The research approach discussed in this paper is attuned to generate insight in these multiple dimensions. It does not mean that the fieldwork activities happened in an unstructured or unreflected manner. Five research principles characterized the research activities. The first principle is *immersion*.

### 2.1. Immersion

The notion of immersion occupies an important place in the description of ethnographic and anthropological approaches to research (Olivier de Sardan: 51; Emerson et al 2). “Immersion in ethnographic research, then, involves both being with other people to see how they respond to events as they happen and experiencing for oneself these events and the cir-

[2] This *phronesis*-like stance solicits a preliminary and continuous reflection on existing knowledge and the process of knowledge construction. The presentation of such an exercise falls beyond the scope of this paper. I have done so elsewhere regarding my ‘field’ of study, Rwanda and the *gacaca* courts (Ingelaere 2010; 2012a). Such an exercise provided guidance regarding research design, method and representational strategies. I summarize the outcome of this reflection here: it is needed for scholars and observers to reveal the social and historical context for the knowledge generated; it is warranted to physically and mentally move away from the center of society and adopt a bottom-up perspective that captures the voices of ordinary people. Second: it was needed to be aware of legal, normative and theoretical concerns imposing global models and concepts on the field of study. Third: it was needed to establish a “comprehensive empirical record” (Axinn and Pearce 2006: 2, 26, 185) of the topic under study.

[3] These major tenets of the study design were documented in an unpublished manuscript, Ingelaere (2006a).

[4] The research on transitional justice in Rwanda started in mid-2005 but I had undertaken fieldwork previously in some of the research locations on topics related to the genocide and *gacaca* courts.

[5] The English term “reference reality” is based on the translation provided by Jean-Pierre Olivier de Sardan during a workshop at Roskilde University (Olivier de Sardan n.d.)



cumstances that give rise to them” (Emerson et al 1995: 2). Immersion is closely related to the notion of (participant) observation. However, while the latter is a research technique the former is, in my interpretation, a particular approach, a guiding principle structuring a research project. Immersion implies a continued and long-term engagement with the research environment *in situ*, thus even when not in the process of doing research; when there is no intention to take notes of the experienced or observed events. It is the development of a sort of tacit knowledge, where the “interpretation of a given situation becomes almost a reflex” (Olivier de Sardan n.d.; 2008: 53) that results in a visible difference in the works of a “fieldworker who calls on lived experiences (through immersion) and an armchair researcher working on the basis of data collected by others” (Olivier de Sardan n.d.; 2008: 54). And it facilitates the development of a particular type of interpretative process that allows for “seeing the invisible, hearing silence, thinking the unthinkable” (Schatzberg 2008). It is evident that this type of knowledge can only be the result of extended and repeated stays in the field.

It is important to stipulate that although the crucial aspect of immersion does characterize my research approach, I have never attempted to become, or considered myself to have become, a Rwandan among Rwandans. I still don’t know what it feels like and how it *is* to have your family exterminated or to be in a Rwandan prison for decades and I don’t know how it is to personally appear in a *gacaca* court as a plaintiff, defendant, witness or judge. In the field, I was aware of the fact that I had an international passport, a credit card and a plane ticket in my pocket (or at hand). And I was aware that, in the worst-case scenario such as for example a situation of genocide, I would probably get a C130 ride home courtesy of the Belgian government.<sup>6</sup> But I attempted and progressively managed to bracket (not erase) these conditions and move closer to these practices and experiences. Indeed, I am confident enough to say that if required I could now emulate the logics of certain behavioural practices I studied. As a consequence, I would be able to pass “a test that some ethnographers aspire to [is] ‘if you think you understand the X then you should be able to act like the X’”(de Sardan 2008: 103).

I am confident that I have developed that type of tacit knowledge referred to by the notion of immersion: I can act in a *contextualized* manner in the Rwandan milieu. Hence the choice of the title of this paper: “learning to be Kinyarwanda”. In fact, “knowing Kinyarwanda” (*kumenya Kinyarwanda*) means two things (Nkusi 1987: 85).<sup>7</sup> On the one hand this expression refers to the ability to speak Kinyarwanda, the language of Rwanda, by being familiar with syntax and words etc. On the other hand, “knowing Kinyarwanda” also connotes familiarity with the local customs, the established practices among Rwandans and also how language is used, thus when and how to speak, remain silent etc.

First and most important: through a continued engagement with and in the Rwandan ‘field’ I learned to navigate the terrain similarly to the way Rwandans do; I tried to be as emphatic as I could but, as I will explain later on, I have no comparable experience of what it means to live as Rwandan peasant on a Rwandan hill as many Rwandans my age have (de Lame 2011; Sommers 2012). Nevertheless, I am confident enough to say that I moved much closer to such an understanding through the research activities described in this paper. I am appealing

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[6] This remark is in fact ironic since it refers to the questionable evacuation procedures of the Belgian and other governments during the 1994 genocide through which non-Rwandans were repatriated and Rwandans left to die. I understand that a nation has the intention and obligation to assist its citizens in distress but that does not exclude the, at least moral, obligation to help others in need and danger.

[7] One could make a distinction between *kumenya Kinyarwanda* and *kumenya Ikinyarwanda* to capture these two meanings. *Kumenya Ikinyarwanda* refers to “knowing the language”. *Kumenya Kinyarwanda* refers to knowing “Rwandan practices”. For example, one says *kurya Kinyarwanda* (eating à la Rwandaise) or *kubyina Kinyarwanda* (dancing in a Rwandan way).

on an “ethnographic pact” (Olivier de Sardan 2008: 28-32) with the reader of this study: “I was there”. But I was not only there, I was there in a certain way, several times, in several ways, in several locations and I was accompanied by Rwandan ‘interpreters’. All of the latter issues qualified my “being there”

For several reasons, I cannot say I speak Kinyarwanda in the linguistic or grammatical sense, although I am familiar with common expressions and words. I am also able to identify the topic of a conversation especially related to the research theme under investigation. Such an understanding was a means to follow up on the quality of the translation provided by my translators. I only have a basic understanding of Kinyarwanda since I have been working on a sequence of research projects with each of them for a limited duration and, at the time, no guarantees for future funding. For these very practical reasons I was always of the opinion that, it was not useful to invest in an in-depth study of the language.<sup>8</sup> More importantly, I realized that Kinyarwanda is such a complicated language, both grammatically and by use, that I would need (a) translator(s) anyway all the time to guarantee the quality of the understanding of my interlocutors. The latter aspect made me reluctant to invest more in my understanding of the grammar of the language. It provided the opportunity to focus more on that other dimension of speaking Kinyarwanda: how things go on among Rwandans. However, being aware of the importance and complexities of the language I was very demanding in the choice of translators as well as in the nature of the translation activities. At times I worked with two translators in the field and a third translator annotating the transcripts at a later stage. I will return to this issue later.

I am confident that the fact that I do not speak the local language does not constitute an obstacle to this study because of the aforementioned reasons, especially the importance attached to the translation process. For similar reasons, Finnström (2003: 33-34) also regrets only having limited knowledge of the local language of the people he studied. He, nevertheless, produced an insightful and widely praised ethnography.

That other dimension of *knowing Kinyarwanda* (*kumenya Kinyarwanda*) relates to the form of the verbal and non-verbal interactions between people: the way things go on between Rwandans. I can say I have developed this skill in two ways. Firstly, I acquired a certain implicit understanding over the years that allows me to interpret the significance of events, occurrences, certain utterances that might go unnoticed to others. Secondly, this type of knowledge also allowed me to navigate the field, an issue of the utmost importance considering the nature of the overall research environment (Ingelaere 2010 & 2012a). I start by discussing the latter – navigation – and will return to the former – interpretation – afterwards.

### 2.1.1. Navigation

This ‘field’ is at times riddled with a range of obstacles. Through this long-term immersion I acquired something similar to what Aristoteles calls *phronesis* in his philosophical works.<sup>9</sup> *Phronesis* is like practical wisdom but also has the connotation of prudence. Indeed, the aforementioned examples of research projects gone awry, research findings suppressed or destroyed demand prudence, it does not mean that research is impossible. Furthermore, the *ethics of dissimulation* and the *aesthetics of progress* that characterize the Rwandan field – as discussed

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[8] However, these multiple individual research projects resulted in over 30 months of time spent in the country and over eight years of engagement with the country. With hindsight, it would have been better to invest more in the language but this could not have been predicted from the start.

[9] See for example his Nichomechean Ethics (1999). I use a strict definition of *phronesis* as prudence in this section to describe fieldwork navigation. As mentioned Flyvbjerg (2001) uses and expands the Aristotelian concept of *phronesis* in a broader argument on the characteristics of the social sciences.

in Ingelaere (2010) - demand a practical understanding of navigating the terrain.<sup>10</sup> Rwandans also behave with prudence. In fact, I learned to behave in such a way by observing Rwandans' behaviour.

I provide four examples that illustrate how I navigated the Rwandan socio-political landscape with *phronesis* acquired through immersion. These examples touch with varying degrees on the issues of gaining/maintaining/deepening access to the field. In addition, these examples are meant to give a flavour to the reader regarding the practical organization of fieldwork. The examples introduce dimensions of fieldwork and methodology that will be discussed in detail in the following sections. The first two examples focus on the navigation of the overall research environment. The following two examples reveal the importance of tacit knowledge during interactions with the people inhabiting these environments.

#### **2.1.1.1. Courting A Military Commander**

A first example dates backs to the initial years of *gacaca*. On 7 March 2006, I attend an information session on *gacaca* on a hill in the countryside. In the evening I wrote down the following experience in my field diary:

I am sitting on bench with next to me my translator. As custom and politeness requires, the white man [me] was guided to the front of the meeting room. I am supposed to be seated on the front row, the row reserved for the local "notables", in general all kinds of authorities. Usually I try to avoid this type of treatment, I accept this time since it's an information session not a genuine *gacaca* hearing. Although the bench is already rather full, a corpulent man is entering the meeting area and is still looking for a seat in the front row, next to me. Although there is hardly any place, he squeezes himself into the 15 centimetres next to me. In doing so, he basically dislocates me from my spot. I am obliged, even pushed to the right of the bench, where my translator almost drops to the floor. I am irritated since I know we will have to sit squeezed like this for the next couple of hours in the heat and trying to take notes. And in fact, I am a bit puzzled, since I have never experienced such a rough behaviour before. These gatherings are generally characterized by an immense decorum and formality. And although I generally do not like it, but "white" people are treated with the greatest respect. The behaviour of that man would only become intelligible later. After a speech of the local authority, the *gacaca* co-ordinator and someone else, it is announced that the local military commander of the region will now address the crowd with a word on "security" (*umutekano*). The man next to me starts to move and gets up. I feel a thumb in my stomach. The local military leader is not someone you want to get in trouble with and I have been fighting over centimetres of bench space to be able to take notes over the last two hours or so. I look at my translator, he looks at me. We did not realize at all, he was dressed in plain clothes, but his rather authoritarian style was becoming "comprehensible" now.

After introducing some security issues and a general talk on *gacaca* etc, I hear he switches to a new topic. The word "*muzungu*" (white person) is pronounced several times. My translator stops translating (which he had been doing for the entire meeting). I look at him. The audience laughs. He too, but he grins instead of laughing. The military commander, a captain,

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[10] Navigating the terrain, adjusting research methods and exploring interpretation strategies is evidently not only relevant for the Rwandan context. Neither it is new in the postgenocide era. See for example the reflection by Vidal (1971) on ideological representations, issues of iteration and adaption of research techniques as well the issue of 'hidden populations' during fieldwork in the 1960s. Codere (1962: 46) refers to the issue of "finding and modifying research techniques" when conducting research during Rwanda's so-called social revolution in the period following the year 1959.

continues. He accentuates his words with gestures of his limbs, hands go up in the air, as well as an occasional foot. Although I ask my translator to resume translating, he refuses and says: “we’ll talk afterwards”. The military commander is the last person to speak.

When the crowd starts to leave, and the “notables” on our bench as well, I ask my translator what that was all about. It seems that the military commander was lashing out at me during his speech. And to me translator as well, hence the reason why he stopped translating. In fact, he had told the crowd, when he was addressing the procedures in *gacaca*, that they should not consider the *gacaca* as a sort of justice as the ICTR in Arusha. He called the latter “muzungu-justice”. The “muzungu”-turn in his speech brought him to me and my presence there on that hill, observing *gacaca*. And he apparently then mimicked the speeches generally produced by high-level dignitaries, that “the white did do nothing to stop the genocide, but now they are here to observe”; “that the white now even deny the genocide in Rwanda; and that I am here to observe, take notes and then write a book, misinforming the public but, nevertheless, make a lot of money as well as my translator.” Although most of these accusations were lodged against the white, I was clearly considered as their representative on the hill today.

Although this particular event was interesting in itself (it revealed something about the discourse on *gacaca*, the relation with the West and the relation between (military) authority and the population), I realized it could have serious consequences for the continuation of my research in that spot. Firstly, this military commander was clearly not fond of me and could potentially be a factor complicating my stay on the hill in the future. I had to find a way to make sure he did not. Secondly, his behaviour and speech during that particular meeting had influenced the perception the population had of me and my translator. I needed to find out if it did, how it did and what I could do about it.

Indeed, during some conversations following this incident and especially with genocide survivors in the community, I realized that some adopted the viewpoint of the military commander. Some started questioning my intentions. On the other hand, I noticed that what had happened had increased my credibility with other segments of the population. The latter people had noticed I could also be treated a bit unfairly, even be accused falsely or as the representative of others as some of them were. It deepened the connection with these people. I could use that element to strengthen rapport with them. I was, however, more worried about the relationship with the people that did follow the military commander regarding his outlook on me and my translator. I noticed there was no insurmountable problem at that point. No damage that could not be repaired. Otherwise I would not have had any other option but to leave that locality. If I had not already been frequenting the community for some time and if I had not already established important connections with some of the inhabitants, things could have been different. Nevertheless, some work needed to be done to counter a negative perception in some segments of the population.

Therefore, and in an effort to also address the first problem - namely the fact that this commander could not only continue to influence perceptions but also make my life miserable as such - I realized that I had no other choice but to approach him individually and talk to him in private. Again, through my immersion in the field I had learned that Rwandan authorities, firstly, try to excel in the preaching of the official line when speaking in public as discussed in Ingelaere (2010) (the “rehearsed consensus”) and, secondly, that they don’t want to be challenged in public. In private, however, they can be much more forthcoming. Also, I realized that it

was necessary that the population noticed that I was on good terms with him, but, also and important that I was working independently of him, that no information would reach him through me. So: I needed him and I also needed to get rid of him.

My translator and I happened to meet him the day following the incident. We<sup>11</sup> spotted him in the commercial centre of the area, a place with bars and shops. I went to greet him. I suggested to go to a bar to share a beer. He agreed. We moved to the rear of one of the bars where it was quiet without other customers. I noticed my translator was not behaving as he normally is when we are in the field: he was nervous. It was a sign for me to be cautious. I use the behaviour of my translators and collaborators, whose habits I got to know over the years, as a yardstick to assess the nature of the situation we were facing or to qualify the interaction or conversation we were having. In that way my Rwandan collaborators – coming from different backgrounds as I will explain later – were not only translators in the linguistic sense of the word but truly “interpreters” as well. I wrote down the following impressions in my field diary after the meeting with the commander:

The commander is a corpulent fellow, his eyes are bloodshot and pierce at me in the dark. His head is swollen. I speculate it is due to the overconsumption of beer. He orders two Amstels. I order a coca-cola, my translator as well. I show him all the permissions I received from institutions and ministries in Kigali such as the Ministry of the Interior (MINALOC), the National Service of the *Gacaca* Courts (SNJG). Evidently I had used these permissions to introduce myself with the local authorities at the different echelons at the local level: district, sector, cell. I know the drill. However, I was not aware of a military presence in this very spot, otherwise and to be on the safe side I would also have contacted its commander. The fact that we had not done so probably created yesterday’s incident. In the end, I know that the security forces constitute a sort of parallel network of power operating in the background of the civilian administration.

He asks me why I do research. I decide to give him an answer that I think will please him. On the other hand: the answer I give him is also genuine. It is one of the reasons that motivate my research in Rwanda. I explain him that I was 15 years old during the genocide and that I saw the images of the genocide on TV. I especially refer to that image that was shot from a rooftop: a couple of men hand out blows with machetes to people lying on the ground on a dirt road. I tell him this image stayed with me for years and that I had to come to Rwanda to find out myself what happened in Rwanda and why. Not by reading about it, but by talking to the people of Rwanda. As said, this explanation is actually true but dramatizes the motivation a bit and it leaves out the more technical issues of academic research etc. He seems to accept and appreciate my explanation. Our conversation continues. He talks now. I hear the words “information – transformation” emerging in his Kinyarwanda. Indeed, after translation it turns out he is particularly worried about how Rwanda is presented to the outside world.

Suddenly he asks the question: “do people talk about the double genocide?” I am caught by surprise. Don’t really know what to say at first. I try to answer as neutrally as possible and tell him: “nobody evoked that topic and it is not the topic of my research, so.” He grudges. I hear these same words repeated again: “information – transformation”. He starts an exposé on the difference between war crimes and genocide. He more or less acknowledges that the RPF committed war crimes

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[11] I use “we” when referring to an action/activity undertaken jointly with (a) Rwandan collaborator(s). Otherwise I use “I”.

but is vehemently against the idea that a genocide was committed by the RPF as well. Also, he says that sometimes foreigners are guided by the wrong people. He evokes the example of a foreign female researcher working on decentralization that was guided towards erroneous insights since she worked with a translator ‘implicated’ in the genocide. I understand the message, my translator as well. We both nod but avoid following up on his suggestion.

The discussion is difficult and tense, but little by little I notice some of his initial hesitation and suspicion subsides. After a long and difficult evening I leave somehow relieved since I realize I managed to ward off a potential nuisance.

Indeed, in the following days and weeks our interaction with this military man became more hearty. In fact, since he was always hanging around in the commercial area, the busiest place of the locality, I frequently went there to greet him and have some small talk with him. The idea was to develop a relationship based on courtesy with him, to avoid any further trouble and to have common ground in case more trouble arose. I also intended doing so in order for people to notice that there was no problem in our relationship and that he accepted and condoned my presence in the area and the work I was doing. However, I also made sure that I did not give the impression of being too familiar and friendly with him. The relationship needed to be characterized by courtliness. I never shared food or beer with him anymore in public for others to see; that would be a token of a familiarity that might again influence my interaction with other people in a negative way. The attempt to reflectively limit the nature of my interactions with him was not based on any prejudice however. I attempted to balance all interactions with people belonging to different social categories in an effort not to become caught in cliques or being perceived as such. Avoiding getting caught or seen to be caught in cliques and networks is an important reflective measure to manage “biases” in the field (Olivier de Sardan 2008: 93-94). In a similar vein, I monitored the nature (frequency and intensity) of my interactions with all kinds of social categories that had a stake in my ‘field’ such as released prisoners, people accused in *gacaca*, genocide survivors, *inyangamugayo*, local authority figures, etc. After all: people also do research on the researcher in their midst.

#### **2.1.1.2. Dodging The Arrival of National-Level Authorities**

The previous example shows how a continued effort was needed to govern access to the field. It is just one example to illustrate the thousands of interactions I had over the years that were aimed at gaining, maintaining and deepening access. An understanding of how to go about things in Rwanda was useful in doing so. A second example focuses on the mimicking of sensitization when navigating the research field.

As has been documented elsewhere, the Rwandan regime is very keen on educating its people (Ingelaere 2010, 2012; Purdekova 2011; Thomson 2011; Mgbako 2005). Sensitization campaigns have become a continuous effort. Not only in *ingando* or *itorero* but also during *umuganda* and in a multitude of meetings with authorities the population is “sensitized” on a multitude of topics. At times this is simply a way of divulging information, sometimes it is an education campaign, often it is also a sort of political indoctrination. What is more important given our focus here is the fact that sensitization is something Rwandans are used to and, not unimportant, it is effective. Therefore, also during my own research activities I adopted a sort of “sensitization” attitude in case I deemed it necessary, but in a subtle manner, of course, very different from the military style political indoctrination adopted by the Rwandan regime at times.

I did so since I was aware of the fact that at times I was inquiring about very sensitive topics that would most probably complicate my research if (local) state officials found out what I discussed with people. Firstly, in the beginning of a stay in a location I had not been before I would ask very neutral questions during formal and informal interviews. As was stated above, people do research on the researcher as well. I was aware of the fact that local officials and security personnel would follow up on my presence after my arrival but that they would lose interest once they realized I was actually not doing anything interesting, let alone dangerous. At that point and it was up to me to decide whether I had reached that point based on a general appreciation of how things evolved and felt in the community, I shifted towards more sensitive topics. But even then I hid the more sensitive questions in longer interviews discussing rather neutral topics. For example: I would ask many questions about the economic situation and changes in it over the years. But somewhere in the middle of this sequence of questions I would also inquire about some views on the political situation, freedom of expression and choice, the behavioural practices of state authorities. In fact, I resorted to life history interviewing as an overall indirect approach with the objective of addressing sensitive topics without tackling them directly. But in the beginning of the interview and also at the end when thanking my interlocutor I would stress the fact that we had talked about, for example, the economic changes in life and nothing else. I will return to this issue in the following example and when discussing the life history approach in detail in a later section.

While residing on a particular hill I would also walk from one respondent to the next. In passing, my translator(s) and I would meet a lot of people. As usual in the Rwandan countryside, greetings are more or less obligatory as well as a little small talk accompanying the greeting. I had learned and agreed with my translator(s) always to intentionally insert a reference to the 'neutral' and 'official' objectives of our presence as and when the opportunity arose. In doing so, we more or less had our own sensitization campaign operating at different levels; in a continued effort, we repeated the same reason why we were on this particular hill. But again: this was done in a very subtle manner with the objective of facilitating our stay on that hill and aware of the fact that such an exercise could not influence the research results.

Nevertheless, through the increasing familiarity with the Rwandan context and way of doing things, I became aware of the fact that outsiders to a community are regarded with suspicion as such and that any drastic and unexpected change of behaviour enhanced that suspicion with potentially negative consequences for any ongoing research activities. Sensitization was important here too, specifically in the form of rumour control; I will give a concrete example.

Elsewhere (Ingelaere 2009c), I documented and analysed a dramatic event on a hill called Ntabona involving the killing of a *gacaca* judge and the execution of a number of inhabitants by security forces in retaliation. I resided in Ntabona in the weeks following the killings. The atmosphere was extremely tense. Fortunately I had established a deep rapport in the community since I had resided on the hill for several months in the preceding 3 years. Together with two Rwandan assistants/collaborators I was undertaking my own investigation into the murders that had happened in the margin of the *gacaca* activities. Evidently this could create problems if local officials and security personnel were to find out what we were doing, especially since the regime had resorted to extrajudicial killings, an issue that was evidently not to be known to the outside world. Firstly, my longstanding familiarity with the hill and its inhabitants provided cover. My presence was not suspicious since I tended to return frequently and I was often around. Also, in my investigation into the killings I adopted the approach described above: I was hiding my questions on the killings in more neutral conversations with people. I researched the 'sensitive' by using research as my cover.

Nevertheless, at some point I noticed a certain agitation in the ranks of the local authority figures. They started to organize themselves in a frenzy that was unfamiliar to me. I asked around in a subtle manner through my collaborators: they would mainly contact trusted key informants on the hill in such an occasion. It turned out the local officials had just received the message that a delegation of national authorities including members of parliament and other dignitaries would come to visit the hill the next day. They wanted to do so to *sensitize* the population about the negative effects of the genocide ideology allegedly rampant in their community. I realized that this visit could potentially have negative consequences for me if these authorities from Kigali considered it undesirable to see a European researcher residing in a rural Rwandan community where people had just been killed by security forces. Therefore I decided to leave the same day to avoid being spotted. However, my sudden departure could create suspicion with the local state officials and security personnel: why would the white man leave when the authorities from Kigali arrived? Does he have anything to hide? I was not sure that this would be their way of reasoning but it could be the case, so I decided to dwell on the side of safety, again, by curbing suspicion and initiating sensitization myself. I started a campaign of counter-rumours before rumours and suspicion could arise. I told my two Rwandan collaborators/translators who were also living with me in the community at the time to start informing people in a subtle way that the arrival of the dignitaries from Kigali was going to disturb our work schedule. Since the population was expected to gather to hear the speeches of the national authorities, it would be impossible for us to make arrangements with people and organize visits and interviews. We spread the rumour that the reason why we left was purely practical. That same night, I left with one of my translators. Another one stayed behind to follow-up on and document the meeting with the authorities scheduled for the following day. As a Rwandan, he could blend in better than me.

### **2.1.1.3. Interviewing “A Random Peasant”**

As will be explained later in this paper, I used a range of research techniques to gather data. Many of these techniques involved interactions with “ordinary” Rwandans through unstructured conversations, semi-structured interviews, life story interviewing or the administration of a questionnaire. I illustrate how I navigated these interactions by evoking one particular case. I focus on a life story interview conducted with Jean in March 2007 in the Rukoma sector of South-East Rwanda. Contextual understanding through immersion was not only important in navigating the research environment as illustrated through the previous examples but also in the interaction with participants living in these research environments.

Jean is a freed prisoner. I had already interacted informally and conducted formal interviews with Jean during a previous stay in the sector in 2006. I had then stayed in the sector three times for some weeks together with two Rwandan assistants. My research had been mostly exploratory during these stays. I was attempting to understand important themes and topics in the unfolding of *gacaca* and the experience of transition in general and in the local context of Rukoma. This time I had returned to the hill with six Rwandan assistants. The objective was to undertake life story interviews with a stratified random selection (approximately 70) of Rukoma’s inhabitants. Jean was included in the sample of released prisoners living on the hill. I will discuss interview procedures in a following section that deals with the life history approach. Here I focus on the interaction with the interviewees as exemplified by Jean’s case.

Before starting research on the hill in 2007 I had introduced myself with authorities at the district, sector and cell levels as I always did once I arrived in an area or returned to a



locality after a longer period of absence. On each of these levels I had informed the authorities about the nature of the research activities and I had given them a copy of my research permits obtained at relevant institutions in Kigali.<sup>12</sup> The introduction at the district was more of a formality but was needed since it could create problems if personnel from the district were to visit the sector and find us talking to its inhabitants without them being formally informed about our presence. I had encountered such a problem in the past. The introduction at the sector level was more important, however. At this level the executive secretary needed to be informed about our presence and the reasons for our visit. It was simply impossible to spend an extended period of time in the sector without informing the executive secretary, who is in charge at the local level.<sup>13</sup> On the other hand: it is indeed more a question of informing not really asking for permission. The consent from Kigali through research permits implied that access to the population had already been granted. If I had not had such (a) permit(s) and had not informed the district first, the executive secretary would not have granted permission for our presence and activities. Since we had permits from Kigali, there was no other option for the executive secretaries but to accept our research activities. Accountability in the administration flows upwards as I discuss elsewhere (Ingelaere 2010). From a practical point of view, with respect to the organization of fieldwork, I used the awareness of this dynamic in my interaction with the local state officials.

As stated above, I had already spent some weeks on the hill in 2006. I was already familiar with the executive secretary. In addition I had brought some Belgian beers as a token of appreciation for his willingness to let us carry out our work in “his” sector: one does not return empty-handed to a friendly relationship. I wanted him to consider our relationship to be qualified as friendly which was a strategic move to strengthen ties with him. As mentioned before: the idea was to have him grant access to the population but without being involved in or obstructing any of the research activities. Again he offered to have one of his aides assist us in the research. I always declined such a proposal but in a subtle manner in order not to arouse suspicion: I would say that I was aware of the fact that they already had a high burden of tasks to execute as state officials and that I did not want to monopolize their time. In addition, in 2007, I used the argument that we had become familiar with the milieu and did not need anyone guiding us. If he had insisted on sending someone with us, I would have accepted but would have cut that individual loose after some time by using similar reasons. The introduction at the cell and *umudugudu* level – administrative levels below the sector – happened in similar vein but was less important: the executive secretary is the key actor at the local level.

Contacting the interviewees started after having secured access to the population by means of – and this is important – a public permission of the authorities but also after having ensured our autonomy, publicly as well. In the meantime, before coming to the sector, I had already identified the sample by using a list of all inhabitants of the hill. I had compiled such a list in 2006 by gathering this information in each of the cells in the sector. In addition, at the beginning of our stay in 2007 I had organized a meeting with a number of trusted informants in each of the cells to identify key demographic characteristics of the inhabitants, including who

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[12] The fieldwork was made possible through research permits received from several Rwandan ministries and governmental bodies. The National Service of the Gacaca Courts (SNJG) (Permits N°204/05/01/2006; N°209/01/02/2006; N°17/01/2007/24; N°12/04/2007; N°21/01/2008; N°16/04/2008; N°25/01/2008; N°19/04/2011. My Rwandan collaborators who carried out *gacaca* observations had personal research permits issued by the SNJG as well). The Ministry of Education, Science, Technology and Scientific Research (Permit N°11022006). The Ministry of Youth, Culture and Sports (Permits N° 202/22.34; N° 727/22.34). The Ministry of Internal Security (Permit N° 08022007). Ministry In President’s Office in Charge of Science, Technology and Scientific Research (Permit N° MINISTR/002/2007). The Ministry of Local Government (Permits N° 195/07.05; N° 123/07/04). I would like to thank these bodies for issuing these permits.

[13] See Ingelaere (2011b) for a detailed discussion of the local government structure and governance practice.

was a released prisoner, a genocide survivor, an old-caseload returnee, etc. This information was needed for the sampling procedure. Jean had been identified as a released prisoner. Within this group he had been randomly selected for a life story interview.

One of my Rwandan collaborators was in charge of identifying and contacting the selected individuals. While I and the other Rwandan collaborators were conducting interviews, he would contact the individuals in the sample and make an appointment for the next day. If the selected person was not available, another day was fixed or it was communicated that we would return at a later date to make an appointment. This collaborator had scheduled an appointment with Jean the next day at noon. When conducting life story interviews we attempted to do two interviews a day per research collaborator: one in the morning and one in the afternoon. It is useless to make an appointment by using specific hours. During that particular time of the year when we resided in the Rukoma area it was the rainy season which meant it was often difficult to make an appointment in the morning period since people were cultivating their plots of land. Jean preferred to meet us in the afternoon after he had finished his work. We always attempted to respect the preferences of the interviewees in order to create the right atmosphere for the interview: we did not want to disturb the natural flow of life of our interviewees.

On the day of the interview we arrived by car in the sector. We stayed overnight in a parish located at approximately 3 kilometres from the sector; it was impossible to find lodging for six people in the sector. We commuted daily by jeep to the sector but left our car next to the church. From there we walked to the interviewees selected for the day. At times it could take over an hour to reach the area where the interviewees were living. We always went to the houses of respondents, irrespective of the difficulties in the terrain. Meeting people on their turf was not only a sign of respect, it was equally a means of increasing trust. Moreover, it was also a way to show that we were not connected to the government; normally local authority figures summon people to their offices or another central location when someone was needed for a government related issue. All of these precautions seem futile but taken together they make the difference between interviewees speaking and interacting more freely and in any case with fewer reservations. Although it sometimes placed a heavy burden on the research team and it probably resulted in fewer interviews per day, I deemed it more important to guarantee the quality of the interactions and conversations.

I attempted to organize the daily interviews, ten per day<sup>14</sup>, in the same neighbourhood of the sector. In that way the team worked in the same area and I could easily follow up on their work and assist when problems arose. I accompanied one of the assistants during an interview in the morning and another assistant in the afternoon; I rotated between my collaborators. Although they had received a training on interview procedures and behaviour in the field, it was a way to keep an overview of their activities and provide feedback whenever necessary. Upon arrival in the sector, we had walked to the neighbourhood where Jean lives. After finalizing a previous interview, I walked to Jean's home with one of my Rwandan collaborators. There was no lunchbreak at noon. We only ate in the morning and in the evening: we never consumed anything in the sector in order to avoid creating frustration with people who did not have sufficient food themselves. Again, the objective was not to influence the perception of the inhabitants of the hill. When not working with a team but when present in a sector with only one or two translators I occasionally took a break in a "cabaret" (pub) for a Fanta or brochette in order to judge the atmosphere. But during these research periods I was working in a less structured ex-

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[14] One research collaborator was in charge of making appointments with interviewees, the other five assistants conducted interviews with me as supervisor rotating between each of them.

ploratory mode than with the life story survey I am describing now. I will return to the difference between less and more structured research modes in a later section.

Upon arrival Jean greeted us. First we talked for some time about daily issues: the work he had done that morning, the children we saw walking around his house, his livestock, the weather. After some time we shifted the conversation towards the motive of our visit and explained the nature of the conversation we would like to have with him, namely our interest in his life. We did not mention any specifics such as the *gacaca* proceedings but referred to our interest in the changes in his life in general. Although we did not lie, we remained vague regarding the specifics of the research topic in order not to influence the nature of his expectations or responses. We explained that we had the permission from authorities in Kigali and at the local level but that we were not working for the government or any other government body. We explained that I was a student from Belgium and stressed that his participation was voluntary and anonymous. I had already done so during previous interviews with Jean. I nevertheless repeated the procedure to avoid any suspicion and, in doing so, we asked for his informed consent. Jean agreed to participate. He was pleased that someone came to listen to him.

In the meantime we were sitting on small chairs next to his house. We were sitting at the back of his house guarded from view. I always made sure we could not easily be spotted by passers-by or onlookers. If possible I attempted to conduct the interview inside the house of the interviewee. At times, this was not desired by the respondent. Jean also preferred to do the interview outside his home. Jean is a very poor peasant with hardly any belongings and a house in ruins. Demanding entrance to his house would be shameful to him. Aware of that issue and without discussing it we accepted to do the interview on the spot he preferred. My Rwandan collaborator conducted the interview since he normally executed this type of interview alone. I followed the conversation through his translation and intervened whenever I deemed necessary. In doing so I gave him advice regarding his interview techniques without interrupting the flow of the conversation. The questions were open-ended and meant to facilitate Jean in telling us his story. The specifics of the procedure will be dealt with in a later section focusing on the life history approach.

At some point, as usual, one of Jean's neighbours entered the enclosure surrounding his house and found us at the back of the house. We stopped the interview and greeted the man. We exchanged some words, the usual small-talk. Also here we explained in a very gentle manner what we were doing and asked the man if he could leave us carry on with our work. I did not want anyone else to be present during the interviews since this could influence the nature of the responses of the interviewee. On the other hand, I tried to communicate that message carefully since a straightforward message to leave us alone could create suspicion about what we were doing and discussing, especially since Jean was a released prisoner. In general released prisoners were closely monitored by authorities with the help of other inhabitants. For that same reason we had started our interview sequence in Rukoma with the local genocide survivors. If we had started our interviews with the released prisoners this might have created suspicion in the group of genocide survivors. By first conducting interviews with genocide survivors they had appreciated the attention we devoted to them and they were familiar with the contents of the conversations. I had learned this insight the hard way, namely during previous attempts to navigate the field. As was stated previously, during any interaction and action in the field I was focused on maintaining and deepening access to the field and especially not to lose access; complaints by genocide survivors, especially accusations of harbouring genocide ideology, could mean the end of the research on that particular spot or in general. I did so in an attempt not to

be seen to belong to cliques.

At the end of the interview we thanked Jean for his cooperation and verified whether he had anything to add or ask us. With the objective to “sensitize”, as mentioned previously, we again stressed the reason for our visit and in doing so deflected the attention from the more sensitive topics we had discussed as well. Our visit to Jean ended with the usual *small-talk*. Following a common practice in Rwanda, Jean accompanied us when we left his house. On the pathways crossing his plots of land we continued asking questions about his agricultural activities based on whatever we noticed in the fields surrounding his house. After some hundred metres, Jean paused and shook hands. This was the end to our interaction with Jean that day and we continued to the place where our jeep was stationed. Along the way we greeted people and *small-talked* and if the opportunity arose, carried out some very subtle sensitization regarding our presence. Once all of the collaborators had returned to the jeep, we left the sector for our lodging. At night I verified all the interviews conducted by all of the collaborators, provided feedback and discussed the experiences of the day. Together with the assistant in charge of making appointments we organized the activities of the following day.

#### **2.1.1.4. Accessing A Former Mayor**

The example of the life story interview with Jean evokes the nature of my navigation of the field by making use of the tacit local knowledge I acquired during that very process of navigating. The interview sequence described above happened during a research period in which I worked with multiple research collaborators and was aimed at gathering data in a structured manner. During many other periods in the field I worked more in an exploratory mode. During these periods I worked with only one, at times two collaborators who would mainly operate as translators. During these periods I observed local practices, *gacaca* hearings or conducted individual interviews or group discussions. At times I would simply have conversations with people. During one of these types of stay in Ntabona, a hill located in the north of the former province of Gitarama, I learned that the former mayor of the area had returned from the DRC since my last stay on the hill. The man had been mayor for decades under the Habyarimana regime until the introduction of the multiparty period in the early 1990s. At that point he had been ousted and replaced by someone belonging to a party other than the MRND. Given the fact that I had been trying to understand the history of the locality in order to situate the ongoing *gacaca* practice in the *longue durée* of the locality, I considered it important to meet him.

On the other hand I realized that he was without doubt suspicious of any unannounced visit to his place to ask for a formal interview. Maybe he would accept but that would not necessarily mean he would say anything genuine. The man had lived as a refugee in the DRC for over a decade which probably meant he had been afraid to return to Rwanda. I had learned that he was not accused in the local *gacaca* trials in any of the sectors that constituted the former commune he used to govern. He had been ousted before the genocide and never returned to power. He had remained powerless during the genocide and had, apparently, not been implicated in the killings. I concluded that the reason why he had been reluctant to return to Rwanda had probably been based on fear for the reigning RPF regime not the fear of being judged for crimes committed. The fact that he returned did not necessarily mean that such a fear had subsided.

It had come to my attention that the regime did indeed keep an eye on the activities of people who had occupied important positions in the former Hutu regimes, even at the local level. Therefore, I looked for a go-between: someone who trusted me and who had a good

relationship with the former mayor. Through trusted key informants in the area I learned who the man frequently interacted with, who he had apparently warm relationships with. I was given a few names. One of them turned out to be one of the priests of the parish where I was lodging during my stay on that hill. Since I had returned several times to these locations I had developed a good relationship with the priests of the parish. One night during a dinner with the priests I mentioned I had learned that this former mayor had returned. I framed my interest in the man in the overall and rather neutral discourse I used on my research and explained that I would find it interesting to come into contact with the former mayor. The priest who frequently visited the latter suggested that he mention my presence at the parish and in the sector during an upcoming visit. He was going to mention that I had stayed many times in the parish and had become familiar with the local inhabitants of the area. I had no doubt he was going to depict a favourable image of my person and doings. Indeed, the next week I was told to go to the former mayor and pay him a visit. I did so together with a translator. During this occasion I did not use any formal, even unstructured, interview guide and I did not write anything down. I wanted the setting to be the least threatening as possible. I hid the questions I wanted to pose in the (seemingly) natural flow of the conversation. The interaction did indeed provide me with some insightful information. And I have no doubt it would have been different if I had not been first introduced by a “trusted friend.”

### **2.1.2. Interpretation**

Knowledge arising from immersion is not only useful in navigating the field, it also helps regarding interpretation of events, occurrences, interactions, utterances: it facilitates an *emic* understanding of things. I illustrate the link between such a tacit, contextualized knowledge and interpretation by giving two examples.

#### **2.1.2.1. “The Tears of Men Flow Inside”**

I was doing an interview in March 2007 with a released prisoner. The conversation was taking place in the man’s home. I was listening to his life story while I was sitting on a bench next to my translator. We were facing our interlocutor. At some point during the interview we broached the topic of his incarceration in the aftermath of the genocide. He had been held in captivity in the local *cachot* for several years. He explained the horrendous situation he had lived through in detail: prisoners packed together in a small room without nourishment etc. The combination of both resulted in some dying while standing. Standing since they were supported by the bodies of the other men in captivity also standing upright in an attempt to manage too little space.<sup>15</sup> The man continued with the description of his plight and gave many details of torture he endured until he suddenly started crying. It might seem understandable that this man started crying given the fact that he was recollecting the torment of the past. However, emotions are not often publicly expressed in Rwandan custom, let alone among men. A man does not cry: “the tears of men flow inside” (*amalira y’umugabo atemba ajya mu nda*) as a Rwandan proverb knows. And especially not in front of other men. Knowing this proverb and the overall habits governing emotions in the Rwandan socio-cultural universe I realized that the fact that this man started crying in front of two other men (me and my translator) was exceptional and thus telling. Even more, the expression of emotions regarding crimes that are not considered as crimes, such as for example the treatment of prisoners in the aftermath of the genocide, goes against an overall climate of suppressing these types of recollections and the emotions that accompany

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[15] See Tertsakian (2008) on the prison conditions in the years following the genocide.

them. I will return to this issue below. For the moment, I want to evoke the fact that this particular emotional outburst carried a deeper significance which might have gone unnoticed if I had not been familiar with certain expressions and behavioural habits acquired during my previous stays in the field.

### **2.1.2.2. “The Lance Will Decapitate You”**

A second example illustrating the connection between immersion and interpretation dates back to 2010. In the months preceding the 2010 presidential elections the vice-president of the Democratic Green Party, André Kagwa Rwisereka, was killed in the South of Rwanda on 13 July. International media reported about the fact that he was decapitated and that “a big knife, sort of machete” was found near the body (The Guardian 2010; De Telegraaf 2010). I resided in Rwanda at the time. When I heard about the details of the murder, the decapitation and the “long knife” involved, I did not simply interpret this murder as a “potentially-politically-motivated-murder” but as a “potentially-politically-motivated-murder-conveying-a-clear-message” as many Rwandans interpreted the news without openly expressing the nature of their particular understanding. Like most Rwandans, in the meantime and because I had navigated “the field” for a long time *in situ*, I had become familiar with certain dimensions of the way the RPF recruited new members. For example, during interviews people had sometimes referred to the ways the RPF proceeded. An excerpt from a group discussion in 2007 is a case in point<sup>16</sup>:

**Q : Are you obliged to join [the RPF]?**

1. Yes, we need to adhere the RPF because it is said that the RPF is a “family”. But it is a political party. If one is asked to do so, one has to do so.

**Q : How does one go about it?**

1. It is said: “you see, we have a “family”. Do you not like the president? So, you need to pledge allegiance.”

**Q : How to pledge allegiance ?**

6. (Laughing) You raise your hand and say that you, member of the « family », will work for the country.

1. I swear to be member of the RPF and I will do whatever I am asked to do.

8. A financial contribution of 100 Rwf is demanded every year by every household.

**Q : What do you think about that ?**

4. We are not supposed to think. There is no one we can recount those types of things to. One accepts and tries to calm oneself.

In fact, not only in formal interviews but by interacting with Rwandan friends I had learned that, firstly, it was rather difficult not to become member of the RPF and, secondly, that one had to swear an oath to the party with the words, and I paraphrase: “Me [name], I swear to be loyal to the principles of the party [the RPF] and in case of violation of my oath I may be punished as any other criminal”. More telling, however, was the fact that I had learned earlier, through friends who had been a soldier during the rebellion, that when the RPF was still a rebellion in the beginning of the nineties, this oath was slightly different. At that time reference was

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[16] Group discussion (FGD), Rukoma, 18 March 2007, anonymous.

made to decapitation by a lance if one betrayed the party.<sup>17</sup> This murder and especially the nature of its execution immediately rang a bell, especially in the tense atmosphere at the time and due to the fact that Rwisereka had indeed defected from the RPF. Later I learned that Rwisereka originated from Eastern Congo where he had been recruited by the RPF in the early nineties and thus probably used the “earlier” oath of allegiance.<sup>18</sup> For many Rwandans and to me, the death of Rwisereka was no surprise given the nature of his execution.

## 2.2. An Iterative Research Process

*Iteration* is another principle underlying this study. I use the notion in three ways. Firstly it refers to a successive movement to and from the physical setting of the field, namely Rwanda and the Rwandan hills. Secondly it also means rotating through multiple locations in the field. And thirdly *iteration* also refers to a psychological movement: the intellectual reflection on the “field”, namely the topic under study. Other publications (Ingelaere 2010, 2012) are the result of such a continuous reflection on “the field of knowledge”. Both dynamics, physical and reflective, had the objective - through the very nature of the process of *iteration* - to deepen the quality of the study.

Firstly, regarding the physical dimension of *iteration*, the current study is based on approximately 32 months that were spent in Rwanda since 2004. I made over 10 return visits to Rwanda. The bulk of that time was spent in rural areas. When in Rwanda, often for extended periods of six months, I also moved frequently between these rural localities. Danielle de Lame (1996: 25), studying one hill in the period preceding the genocide asserts that in-depth research will enable the researcher to understand the specificities of the site under investigation, but that a nearby site, another hill, will always remain strange and unapprehended. *Iteration*, therefore, also refers to the physical movement between different research locations. This will be further discussed in the next section that focuses on the breadth-depth principle, another characteristic of the research activities. I will later discuss the characteristics of these sites in detail.

Although I initially started doing research on the unfolding of the local genocide in a local hill in Rwanda in 2004 and subsequently focused on the dynamics of the movement out of poverty in a World Bank study in 2005, most of my research time since mid-2005 was dedicated to understanding the *gacaca* process in Rwanda. As documented elsewhere (Ingelaere 2008; Clark 2010): the modernized *gacaca* court is a dynamic and lived institution that functioned for several years. Through my repeated visits to the same locations over the years and by continued observation activities throughout the years I was able to grasp the process as it evolved over time.

By going back and forth between Rwanda and Belgium as well as by going back between Kigali and the rural research locations under study I not only managed to deepen my understanding of the topic under investigation through a continued follow-up on the changing dynamics but I also managed to increase trust between myself and the inhabitants living in these locations. *Iteration* was important here too, as the previous examples demonstrated. I returned many times to the same research locations and to the same people to conduct inter-

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[17] The only place, to my knowledge where information can be found on this issue is a reference made by Ruzibiza (2005: 65) in his book on the RPF campaign: “I solemnly swear before the men that I will work for the RPF family (“*umuryango wa RPF*”), I will always defend its interests, and, if I divulge its secrets, I will be decapitated like any other traitor”.

[18] Ruzibiza (2005: 65) is rather straightforward regarding the implications of this oath: “It is obvious that leaving the RPF equals breaking this oath and that whoever did so was ready to have his head decapitated as announced in the oath. In other words, whoever becomes member of the RPF family is obliged to stay there until he is put aside by the RPF, otherwise one stays until one dies.” (my translation)

views, have conversations or simply spend some time. In doing so, I was deepening my relationship with these locations and these people. The suspicion prevalent in Rwandan communities evoked previously does not only subside by adopting sensitisation procedures or by being very prudent – the Aristotelian *phronesis* evoked above - while present in the field. Multiple return visits to the same location showed that there was no harm in sharing thoughts and time with me. It also showed that I was allowed to return to the people I frequented. The examples of maintaining and deepening access referred to previously illustrate that I never considered this process established or taken for granted: I was always prudent, even worried, to disturb my relationship with these localities and its inhabitants.

Evidently, I not only increased trust through *iteration*. Whenever possible I resided on the spot or very near to the hill, most often in parishes. A parish is often the best option to stay in rural areas: it offers accommodation that is minimally needed to keep going for longer periods during research. I never considered the option of staying with one of the inhabitants let alone state officials at the local level: everyone has a stake in the dynamics of the past and the present on a Rwandan hill, a dynamic one does not always understand initially but that might complicate things tremendously. Although parishes and churches had an important stake in the genocide as well,<sup>19</sup> the positive side of parishes lies in the fact that parish priests tend to rotate every couple of years; they are generally not part of longstanding and entrenched dynamics in the community (hence the reason why they rotate in the first place).

At times I therefore had to commute daily to the nearest site where housing was available. While residing on site or nearby I took part in local life to the extent it was possible. Most importantly: I continuously walked around, interacting and engaging in *small-talk*. But I also went to the market, frequented local bars (*cabarets*), I played soccer and volleyball with young people in the local playgrounds, etc. I brought volleyballs or footballs with me several times. I did so in an effort to give something to “the community” since I was always clear not to give any reward, financial or other, to individuals. Giving to individuals could potentially create envy in the population and could pollute the voluntary character of the interactions. In order to give back to the community as a whole, I started a small library on one of the hills where I lived for several months. I collected French and English books at home and shipped them to Rwanda during my next return visit. A library service was initiated together with the local school teachers.

While “being there” and “returning to being there”, I was evidently very conscious to reach out to these local communities as a whole. Aware that I was not the only one doing research, but that “the locals” were doing research about me as well, I avoided being perceived as inclined to hang out more with person X or Y or social category Z or Q.

These undertakings were not only genuine commitments from my side but I also hoped that these efforts would be considered as tokens of genuine interest in the people of these localities. Indeed, after a while I was considered “a son of the hill” in the locality where I spent most of my time, as two widow genocide survivors started to refer me to. My translator was, symbolically, offered a piece of land on that hill. Sometimes my efforts to increase trust were also purely strategic, namely when I brought Belgian beers for local the most important state official when *iterating* back to the field, a strategy that resorted the desired effects I have to say. Sharing (“exotic”) beers is, after all, an important token of “knowing Kinyarwanda” in the second meaning of the term, as Danielle de Lame aptly described in her ethnography of a rural

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[19] See for example Longman (2010).



hill (de Lame 2005: 303-340).

Evidently, every formal interaction (interview, group discussion, life history interview, survey, etc.) started with a formal explanation about the nature of the research activities (without being too specific, however) and asked for consent on the part of the interviewee. Hardly anyone ever refused to participate. But, in fact, many of the people that were willing to share their time, life story, sorrows and joys asked me what was in it for them, what I was going to do for them. I always explained that the results of the research were not directly going to influence their individual lives (in a positive or negative way). Sometimes I referred to my efforts evoked above (library, volleyballs, etc) to show that I did something for the whole of the community and that I could not give to individuals. I was, however, very clear to point out that their participation might make a difference for Rwandans as a whole since the results of the study would probably also, at least in an indirect way, reach decision-makers. Almost everyone accepted and appreciated this explanation. Many even rejoiced the fact that someone came to listen to them, that a *muzungu* (me) had actually taken the time and effort to walk all the way to their home and returned every couple of months. Often these homes were situated in a remote part of a hill and I, indeed, always walked to their homesteads since it was another way of showing respect for the participants and thus increase trust. As I was told several times over the past years: in several localities I went where no white man had ever been before. I continuously walked the hills under study.

*Iteration* does not only refer to the physical movement of continuous return and rotations within the field, it also evokes the reflective and adaptive nature of the research process. As mentioned before, this study was not initiated with the objective to test theory, for example. If it had been, the research activities would have been much more focused. Also, the net was intentionally casted very wide: data gathering was intense and voluminous. Focus, method and research techniques were shaped and sharpened while the study progressed. For example: the choice to resort to systematic life story interviewing in 2007, a method explained in detail below, was the result of insights in strengths and weaknesses of research techniques I was using in the period 2004 - 2006. The research process was divided into several phases of which some were more open-ended and exploratory. During these phases I operated in what can be called an “unstructured” research mode. Other phases were more focused and characterized by the use of more structured research techniques. Also, as will be explained in the section discussing the genesis and format of the life story format, I *bricolaged* my own approach based on previous assessments of the use of research techniques. As mentioned elsewhere (Ingelaere 2010a: 59): “attempts to generate insights into post-genocide Rwanda require innovative approaches if they are to produce useful results.” Olivier de Sardan (2008: 68) states that a researcher needs “to tinker and to invent appropriate techniques for his own use in accordance with the novelty of his object and approach.”

### **2.3. Mixing Methods**

As mentioned previously, this study was not initiated to test a hypothesis or with the objective of making theory. The idea was to render a phenomenon – the experience of participation in Rwanda’s transition through the *gacaca* courts – intelligible. Mixing methods was needed to reach the objective of producing “a comprehensive empirical record” (Axinn and Pearce 2006: 2, 26, 185) on the topic.

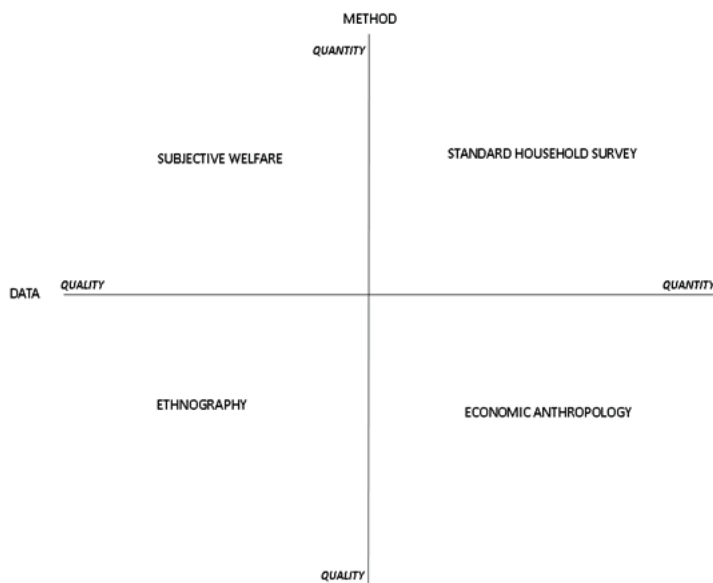
Taking into account the research question, the existing knowledge and the research environment, I adopted an ‘inductive theoretical drive’. Morse (2003: 190) defines a ‘theoreti-

cal drive' as "the overall direction of the project as determined from the original questions or purpose and is primarily inductive or deductive." When the theoretical drive is inductive, the research is *discovering* answers to initial questions and *exploring* the concepts that underlie the research topic. In contrast: working deductively is more suited to *test* a hypothesis. An inductive stance solicits a qualitative approach while the use of quantitative methods is best suited for testing. An inductive theoretical drive and the use of primarily qualitative methods and data assured that the research remained open to unexpected findings.

The fact that the theoretical drive is inductive and thus qualitative does not mean that a quantitative inquiry is not allowed. On the contrary, considering the complexity of the research question and environment I complemented an overall qualitative drive with quantitative strategies. This strategy was used to enlighten and further guide the overall theoretical drive, especially as a background for the analysis of 'qualitative' data.

The social sciences are characterized by a long-lasting debate on the difference and the (in)compatibility of quantitative and qualitative approaches. The vision of allegedly mutually exclusive approaches originates in the fact that researchers prefer to use the methodological approach according to their skills. Social and cultural anthropology has a particular place in this debate. While the social and behavioural sciences in general aim at reducing the complexity of reality, anthropological and ethnographic approaches generally aim at depicting the complexity of the social and cultural reality. Nevertheless, researchers of all kinds are increasingly expected to adopt mixed methods approaches to answer complex research questions (Teddle & Tashakorri 2003).

**Figure 1.: Quantitative and qualitative methods and data**



Source: Adapted from Hentschel (1999) and Rao & Woolcock (2003)

The crux of the quantitative-qualitative problem can be located in the fact that it is not clear what we can understand under the labels 'quantitative' and 'qualitative'. Methods as well as the type of data can be either qualitative or quantitative (Hentschel 1999, Rao & Woolcock 2003). A combination of both – for example qualitative data gathered with a quantita-

tive instrument – is also possible. A polarization across two methodological lines is not correct if we consider figure 1. above: there are four possibilities. In addition, it can be argued that every method contains qualitative as well as quantitative aspects and that other characteristics such as level of structure, interviewer involvement and researcher involvement are as relevant in the characterization of techniques (Axinn and Pearce 2006: 2-15).

Table 1. compares different methods according to these criteria while table 2. challenges reigning conventions regarding the connection between specific research techniques, sample size and coding. This study is based on such a continued effort to develop hybrid methods surpassing the qualitative-quantitative distinction not for the sake of originality or avant-gardism but in light of an effort to develop the “empirically comprehensive record” referred to above. Such a record needs to guarantee the empirical adequacy between the representation and reference reality as evoked earlier.

To overcome the problems evoked earlier – a complex reality in the aftermath of violence, social and political pressure – both quantitative/qualitative or structured/unstructured research strategies were used to collect data. At times this happened by tinkering both into one technique as will be discussed shortly regarding the life story approach. At times I alternated periods of focused qualitative/unstructured research (mainly informal/formal interviews and observations in the field) with periods using more structured/quantitative techniques such as survey questionnaires. Finally, I also quantified (coded) qualitative data to bring more structure to apparently unstructured data with the objective of allowing more systematic analysis. The treatment of the transcripts of *gacaca* observations is a case in point. This procedure will be discussed in detail below.

**Table 1. Structure, interviewer and researcher involvement among data collection methods**

Data Collection Method	Level of Structure	Interviewer Involvement	Researcher Involvement with Study Population
<i>Surveys</i>	High	Usually	Low
<i>Less Structured Interviews</i>	Low	Always	High
<i>Focus Groups</i>	Low	Always	Medium
<i>Observation</i>	Low	Usually	High
<i>Historical/Archival Methods</i>	Out of researcher's control	Out of researcher's control	Low

Source : Axinn & Pearce (2006: 10)

**Table 2. Comparing sample size and coding possibilities among data collection methods**

Data Collection Method	Data on Large Numbers of People	Could be Coded as Numbers	Could be Analyzed as Text
<i>Surveys</i>	Usual approach	X	X
<i>Less Structured Interviews</i>	Possible	X	X
<i>Focus Groups</i>	Possible	X	X
<i>Observation</i>	Possible	X	X
<i>Historical/Archival Methods</i>	Possible	X	X

Source : Axinn & Pearce (2006: 12)

By casting the net wide, mixing methods and through the triangulation of data I could layer the data over one another to identify recurring themes, overlaps as well as blind spots and inconsistencies. Indistinct elements were filtered out in such a way to gradually discover *grounded* patterns and returning themes in the social reality – more in particular the reference reality of the *gacaca* practice - faced and lived by the inhabitants of communities at the local level. An overview is provided in the second part of this paper for a further draw up of this mixed method approach, the different sorts of data collection tools used, the themes explored through the tools, the selection criteria for respondents and the number of respondents for each tool.

I conclude this section that discussed the mixed method principle underlying the study by briefly addressing the issue of the worldview or paradigm underlying this study. With worldview I mean “how we view the world and, thus, go about conducting research” (Creswell 2007: 21). This worldview refers to a set of beliefs or assumptions about the reality of phenomena and how they can be explored and understood. As mentioned already in the introduction, I will not dwell on the crisis of representation (Marcus and Fischer 1986) and I avoid any “epistemological hypochondria” (Geertz 1988: 71) that paralyzes any genuine research activity. But that does not mean that I am unaware or ignorant of the epistemological and ontological assumptions that guide the design of this study. In fact, and following Creswell (2003; 2007: 21-27), Maxcy (2003) and Flyvbjerg (2001), the mixed methods principle underlying this study and the ‘phronesis-like’ character of the research process stresses a *pragmatic stance* – which I use in the common sense meaning - in that respect. A *pragmatic stance* or worldview places the research question in the centre of the study and values both subjective and objective knowledge. It considers reality (ontology) to be both singular and plural and results in a “pragmatically governed interpretation of studies practices” (Flyvbjerg 2001: 140).

#### **2.4. Breadth and Depth (Multi-Sited)**

This sequencing of more qualitative/unstructured and more quantitative/structured techniques can also be considered as an *iterating* aspect of the research approach, a key principle discussed earlier. In addition, combining these two approaches resonates with another key principle underlying this study: guaranteeing “breadth” and “depth”.

To assess the underlying research topic it was necessary not only to mix methods but also to diversify the overall nature of the observations made. The study aimed at understanding both the breadth and depth of processes (Barron et al 2004, 2008). Any researcher is faced with two extremes in methodological approaches. On the one hand large *n-studies*, typical nationwide surveys, can establish the breadth of a study. By collecting data through survey questionnaires distributed to randomly selected respondents in randomly selected communities, one can infer statistically sound conclusions based on a significant part of the population and, therefore, representative for the population as a whole. Every social setting is marked by idiosyncrasies. The use of large-scale surveys avoids those idiosyncrasies by reducing the complexity of reality and producing universally valid predictions and statements. But predictions are no explanations and although predictions and statements are valid for a large population, the data are not rich in detail because mostly quantified and collected on the basis of concepts drawn up beforehand.

On the other hand, ethnographic research generates information very rich in detail, *emic* conceptions and gives insights into the reason *why* and *how* events happen and processes take place. Ethnographic approaches are able to identify underlying patterns and themes that will not easily surface by using questionnaires: they are well-suited to understand issues of pro-

cess. This approach can also identify social categories that remain invisible and themes that 'fall through' the tight grid of preconceived questions and already coded answering possibilities. On the other hand the question of representativeness remains. Why should findings gathered in one place, albeit rich in detail, be valid for a larger population or even another, similar, nearby place?

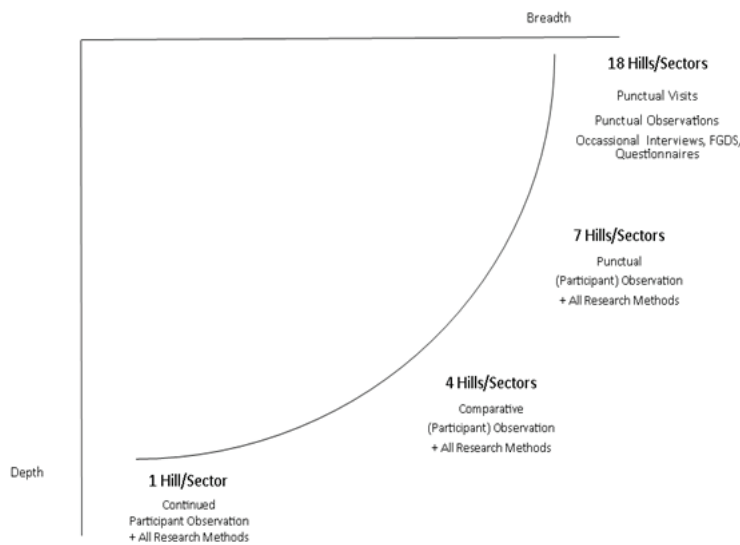
As I also argued regarding the alleged irreconcilable dichotomy between quantitative and qualitative, these two approaches – broadly summarized as survey versus ethnographic research - do not have to be as mutually exclusive as presented above. Integration of both viewpoints is possible and results in a better understanding of the topic under question. The idea is to use the best of both worlds and avoid the weaknesses of each of them. *Iterating* through a multitude of research locations inside Rwanda helped to establish both breadth and depth of this study.

Figure 2. below gives an overview of the interlocking aspects of the depth and the breadth of our research approach. As highlighted in the graph below, I used a sort of pyramidal approach in the oscillation between breadth and depth. I have a truly ethnographic understanding of one site since I lived there for several months. The understanding of three additional sites is similar; they were studied in-depth on the basis of longstanding presence in the field, although less extensive than the one referred to previously. In these four sites I used multiple research techniques and followed all the *gacaca* activities. In addition, however, I collected a substantive amount of data, including two rounds of life story interviewing with a representative sample of the population and repeated *gacaca* observations. Sporadic *gacaca* observations took place in a wider range of 'sites' (11 additional locations in different regions of Rwanda). In these sites I used multiple research techniques ranging from surveys over formal/informal interviews, group discussions and observations.

insight into the dynamics taking place in the periphery of the *gacaca* proceedings in order to fully understand the nature of the actual *gacaca* activities taking place. This required a continuous observation of the *gacaca* proceedings within the same communities as well as an in-depth exploration of the social dynamics in the context of the *gacaca* activities in these locations. Four sites were therefore selected for in-depth study. But punctual observations were also made in other locations for comparative reasons and to verify the breadth of the insights.

These research locations were purposively chosen but this choice aimed at maximizing variance. Maximizing variance (Rao and Woolcock 2001) on specific variables (regional, conflict dynamic, historical bases of power, etc.) was the criterion for the selection of provinces and communities: it helped to sharpen patterns, made recurring themes emerge and established findings relevant for a wide range of environments.

**Figure 2. Study Breadth and Depth**



Source: Adapted from Barron et al (2004)

**2.4.1. Overview Research Sites**

Map 1. provides an overview of the research sites. The differences in size and shape of the indications reflect the balance in breadth and depth discussed earlier. The dots indicate locations where a significant amount of time was spent. More time was spent in the sites with bigger dots. Life histories were systematically recorded in these sites. Areas marked with a star refer to the locations where the *gacaca* proceedings at the sector level were systematically observed and recorded. All of the *gacaca* activities during the trial phase at the sector level were recorded in these locations. In the other locations, *gacaca* activities were also observed but not systematically, only punctually.

I defined a research location to be a sector as they existed during the period of the genocide and until the administrative reform in January 2006. I focused on a sector since the *gacaca* tribunals were operating at this level (or below as I will explain in a following section). Moreover, a sector as it existed until the administrative reform in 2006 corresponds with the natural horizon, the common action radius of an inhabitant of the Rwandan hills. People interact with other inhabitants, institutions and authorities at the sector level on a daily basis. Administrative units higher up, such as districts lie beyond the daily horizon of the inhabitants of Rwanda’s hills. Also, as can be seen on map 3.1., I will mostly refer to the provincial demarcations as they existed at the time of the genocide and until January 2006 since the *gacaca* process dealt with events and crimes that happened when these demarcations were in use.

A hill in central Rwanda, in the north of the former province of Gitarama, is the location where my research in Rwanda started. In 2004, in the context of a research project focusing on the unfolding of genocide at the local level, I lived for approximately three months on that

hill.<sup>20</sup> The research focus at the time allowed me to acquire information on the local genocide and the prevailing community dynamics. It also meant that I already had access and established rapport with many of the sector's inhabitants. Both issues – having lived in the community before and the information on the genocide dynamics – would prove fruitful regarding the research activities focusing on the *gacaca* practice and the experience of transition in the years to follow.

In 2005, with the start of my research on *gacaca* courts I adopted the breadth-depth principle in the selection and frequentation of locations. In 2005, at the start of the *gacaca* activities nationwide, I applied the following principles to select the research locations where all the *gacaca* activities at sector level would be observed. First, I selected a number of provinces (at that time the old administrative structures were still in place). The selection of provinces was guided by the principle of *attaining maximum variance*. The idea was to select as widely diverging contexts as possible on different levels: demographic and ethnic composition, historical bases of power, conflict history and intensity of violence.

### Map 1. Overview research locations



The selection of regions was based on an extensive literature review and the expertise of informed observers. The (former) provinces Ruhengeri, Kigali-Ngali, Gitarama and Kibungo were chosen. Based on the information gathered, I purposively selected three additional sectors for in-depth study, apart from the sector I had already established access to in 2004. In later stages more research locations were added especially also in different regions of the country, including in the former provinces of Gisenyi, Butare, Gikongoro and Buyumba. For comparative reasons I applied different research methods in different clusters of research locations as shown in figure 2. The objective was to compare the findings across locations in order to establish trends and variations. Also, the systematic observations were broadened with punctual observations in other areas. These additional locations were randomly selected but always with the intention to maximize variance on the above mentioned indicators.

[20] I was part of a research team in which several researchers each explored the unfolding of genocide in one sector (hill). The findings are discussed in: Boersma, J. & Brone, A. & Chaplier, J. & Ingelaere, B. & Meyer, C. & Pinchotti, S. & Thiry, I. & Spiesschaert, M. & Verwimp, P. (2013).

I provide some basic information on the four sites where the *gacaca* process was systematically observed and the three additional sites where *gacaca* was not systematically observed but in which lengthy periods of time were spent, where two rounds of life story interviews were conducted and where a multitude of additional data were gathered.

Jali is situated in the north of Rwanda.<sup>21</sup> The sector is located in the heartland of the former regime. Several dignitaries and military personnel in the former regime hailed from this region. Not only during the period of the genocide between April and July 1994 but also during the years of civil war starting in October 1990 many Tutsi inhabitants were killed. The sector was also severely affected by the civil war – generally referred to as an insurgency – that ravaged especially Northern Rwanda between 1996 and 2000.

Ntabona and Runyoni are situated in the former province of Gitarama in central Rwanda. Runyoni is located on the main road between Kigali and Gitarama. Although Runyoni used to be a rural community it is slowly becoming semi-urban in the last couple of years with the expansion of the city of Kigali. Runyoni is situated approximately 10 kilometres from the centre of Kigali. Runyoni was also during the period of the genocide influenced by developments in Kigali.

Ntabona, on the other hand, is distinctively rural. It takes approximately an hour and a half by car to get from Kigali to Ntabona. Sector Ntabona is a large hill in the north of the province of Gitarama surrounded by three rivers demarcating the sector. The river Nyabarongo is the natural frontier between the former province of Gitarama and the former province of Kigali-Ngali. During the three months of genocide in 1994 periods of killing and looting alternated with days of resistance to the killings.

Lastly, Rukoma is situated in the marches and along the lakes to the south-east of the Bugesera region. Before the construction of the tar road through Bugesera, it took approximately 5 hours by car to go from Kigali to Rukoma. In the sixties a so-called *paysannat* (state scheme regrouping peasants) had been constructed in Rukoma. Tutsi from Gikongoro and Butare were relocated to this inhospitable environment. During the period of the genocide thousands of Tutsi died in Rukoma.

A significant amount of time was spent in three additional sectors: Nyakabanda, Marangara and Rambura. In these locations, the *gacaca* practice was punctually observed, life stories were collected twice with a stratified random selection of inhabitants and multiple interviews and group discussions were organised on different occasions. Nyakabanda is situated north of the former provincial town of Ruhengeri (Musanze currently) on the slopes of the volcanoes. The sector borders the national park with mountain gorillas and is only a few kilometres away from both the borders with the DRC and Uganda. In the early 1990s the sector was twice crossed by the RPF during attacks on the city of Ruhengeri. Part of the sector was occupied by the RPF in that period and part was a demilitarized zone. Marangara is an extremely vast sector located in the centre of the Bugesera region. The region is inhospitable due to the climate and soil conditions. The area was home to thousands of Tutsi during the period of the genocide. Currently the sector contains a very high number of so-called old-case load returnees, Tutsi who returned to Rwanda after the genocide. Lastly, Rambura is located south-east of the former provincial town of Butare. It takes approximately two hours driving from Butare to reach the sector. The sector borders Burundi. Inhabitants frequently cross into Burundi and Burundians

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[21] In order to guarantee anonymity and to protect research participants, all names of locations and individuals have been changed. The actual names can be provided upon request.



often come to the Rwandan side. The events in Burundi in the 1990s influenced the dynamics in Rambura through cross-border contacts. Rambura is located in the region where thousands of people fled the start of the *gacaca* activities in 2005-2006.

## **2.5. Diachrony – A Longitudinal Approach**

A fifth and last research principle relates to a dimension that characterized most of the research techniques: the ability to capture *dynamics over time*. The research wanted to understand the experience and perception of a transitional practice – the modern *gacaca* process – in the context of a political transition. Therefore, important points of reference are the pre-, during and post-genocide periods, the different phases of the transition as well as the different phases in the *gacaca* activities: pre-, during, and after. The data collection tools, therefore, needed to be designed to grasp the community, household and individual situation and the overall local-level institutional context at different moments in time. All of the research instruments were designed to be able to grasp dynamics of the *longue durée* of institutional changes, value transformation, changing attitudes, perceptions and experiences in order to assess the situation at different moments in the transitional process. The central importance of a continued observation of the evolving *gacaca* proceedings as well as the life history approach structurally embedded this principle into the research activities. I will discuss these key research techniques (and others) in a following section.

### 3. RESEARCH COLLABORATORS

The principles discussed above could not be followed nor could the objectives for the study be reached without the assistance of Rwandan collaborators. Between 2006 and 2011, I worked with 21 Rwandan researchers who assisted me with various aspects of the study. In 2006, 2007 and 2011 when lengthy periods were spent in Rwanda to gather data through a range of research techniques I worked with a team of 10 Rwandans on average (11-12-9). In 2008, 2009 and 2010, when mainly *gacaca* trials were observed, I worked with on average five collaborators (7, 4, 4).

I had no institutional connection in Rwanda. I worked independently. I organized my fieldwork and managed my research team alone. In 2005, while working for the World Bank, I had been involved in the selection procedure looking for Rwandan field assistants to participate in a research project that aimed to collect both quantitative and qualitative data on a range of topics. I was part of the selection committee that identified approximately 70 research collaborators with various skills out of a group of over 500 candidates. Moreover, I was co-responsible for supervising the researchers and their research activities in the field. Based on that experience I was able to identify collaborators with the skills, motivation and background that I needed.

Throughout the different phases of the research I worked together with four types of collaborators. I had two lead researchers who assisted me almost full-time for the duration of five years. They were primarily chosen based on their ability to interact with the rural population and their skill in the navigation of “the field”, as explained above. I valued these skills as well as their capacity to reside in rural communities over any formal education. I also insisted on using collaborators who had lived in Rwanda for their entire life to ensure familiarity with the diachronic dimension of the research principles. For similar reasons I wanted my research collaborators to be able to speak French. English is new to Rwanda since the genocide and carries a specific connotation. Only in recent years is English becoming less exotic in the countryside.

Evidently, these two main collaborators had ample experience with participation in survey and qualitative research. These two were also my principle translators in the field. Often and always during group discussions I used both of them during interviews. One of them would translate and the other would take notes in Kinyarwanda or French, depending on the type of interview. These two principle investigators were also responsible for the continued *gacaca* observation in four locations that will be discussed shortly. It is evident that their continued engagement with the research project and, as a consequence, their *iteration* to the same research locations and the interaction with the inhabitants was a factor that contributed to an increase in familiarity and trust as well. It also ensured that they were able to understand dynamics that would remain obscure to others who had not been engaged with these communities for such a long period of time.

I expanded my group of research collaborators when shifting from relatively unstructured to more structured research modes. The differences in these types of research modes can be seen when comparing the examples of interviewing Jean, the released prisoner, and the former mayor as discussed previously. The first happened in a more structured mode with several collaborators, the latter took place in the relatively unstructured mode. During the structured research mode, the idea was to collect data with larger numbers of people through systematic data collection. These procedures will be discussed in a later section. During these phases I added three or four collaborators to the two lead collaborators. These additional collaborators were identified in the pool of researchers evoked above. Nevertheless, I always invited multiple

people to a sort of job interview to verify which people had the skills and motivation I needed. Also, during these phases I would organize several days' training on the principles and methods of data gathering activities we were going to undertake, for example fielding a survey (2006) or life story interviews (2007 and 2011). I would always train more collaborators than needed and make a final selection based on an assessment of the training period. This also allowed me to replace collaborators if needed while the research activities were ongoing. I had a pool of reserves that had been trained and could easily function as replacements.

During these training sessions I not only focused on the primary technique to be used (life story interviewing or survey research) but also discussed and practiced through role play issues regarding fieldwork in general (interacting with respondents, behaviour in the field, non-verbal interaction, .....). During each of the research phases (with a larger group or with one or two of the principle collaborators) I was always present in the field with the research team. When in the breadth/structured mode of research I mainly operated as a research supervisor, assisting the Rwandan collaborators and verifying their work. The research activities happened in collaboration. My Rwandan assistants were evidently more knowledgeable than me regarding certain dimension of Rwanda's history, culture, customs and other particularities. As mentioned, they were not only translators or collaborators but also "interpreters" in the broadest meaning of the word. I always asked their advice and discussed ways how to go about things. Not only in the interaction with local inhabitants of Rwanda's hills but also in the interaction with Rwandan collaborators and "interpreters of the field" I avoided being caught in "cliques" or "networks" (Olivier de Sardan 2008: 93-94) by working with multiple assistants with different backgrounds while valuing and comparing each of their suggestions.

Indeed, although skills and motivation were the primary criteria in the selection of assistants, I attempted to include collaborators from different backgrounds in my team. I observed a balance with respect to gender and ethnicity. And I took into account these characteristics of my assistants when I considered it necessary to guarantee or even increase the quality of tasks in the field, e.g.: when I noticed it would be better to have a genocide survivor that was part of my team to conduct interviews with genocide survivors. At the same time, however, I made sure that the same collaborator did not only do all of the interviews with the same category of people. This could have created additional biases in the research results.

The collaborators had copies of my research permission that they could use in the field if needed. During the introduction by myself or my collaborators, it was always stressed that we were not connected or working for the government although we had the permission from central authorities in Kigali and the local officials. All interviews apart from group discussions were administered in the house of the respondent. Depending on the interview format, the interviews were translated on the spot from Kinyarwanda to French by a field-assistant/translator. In these cases the interviewers/enumerators wrote down expressions in Kinyarwanda with a specific meaning surpassing immediate possibility of translation. These were discussed afterwards and compared with the translated statements. During other occasions interviews and group discussion were written down in Kinyarwanda and translated later. We did not use recording devices since respondents may not be familiar with them and they might have aroused suspicion, affecting responses.

Apart from the principle collaborators and the punctual collaborators in certain research phases, I also made use of a number of collaborators responsible for data capture. I used two in 2006 and 2007 and four in 2011. All interviews and group discussions were later typed

out by these assistants who would also annotate the interviews when faced with particularities related to translation of statements from Kinyarwanda. Between 2008 and 2010, one of the principle collaborators also took care of the data capture activities mainly related to ongoing *gacaca* observations at the time. These collaborators were selected based on their skills as well. In addition, I had selected collaborators who were also able to annotate interviews: they would provide information on specific word use, expressions or potential problems with translation. Apart from using skilled and at times two translators in the field, this was an additional measure to guarantee the veracity of the data collected.

During moments of intense data capture I had provided these assistants with laptops in a rented office in Kigali. I had rented an office to avoid them working at home with this sometimes sensitive material. These collaborators had also received training tailored to the nature of the data capture. They were given new data to be captured electronically during every returning *iteration* of myself and the team to Kigali when coming back from the rural areas. At these moments I also supervised and verified their work and discussed any matters and problems arising.

In addition, I also worked together with a number of research assistants living in the localities under study.<sup>22</sup> I shall discuss their involvement in the following section since they were only involved in the observation of the *gacaca* process in their sector.

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[22] The other collaborators were not inhabitants of the research localities to avoid being part of community dynamics.

## 4. METHOD: KEY RESEARCH TECHNIQUES

Two main research techniques were used to collect data: observation of *gacaca* proceedings and life history interviews. Both of these techniques were adapted to the Rwandan environment and to the research question by taking into account the research principles discussed in the previous section. I discuss both of these techniques in detail, starting with the *gacaca* observations.

### 4.1. GACACA OBSERVATIONS

#### 4.1.1. DATA GATHERING

In 2005 and in the first half of 2006 punctual observations were made during the preliminary phase of the *gacaca* process, the so-called information gathering phase. Subsequently from mid 2006 onwards and with the start of the trial phase, all trials taking place at the sector level were observed in the four communities selected for in-depth study. When I was residing in Rwanda the observations were made by myself assisted by a translator so I would be able to follow the proceedings. Another field assistant would record the proceedings verbatim in Kinyarwanda.

When I was not present in Rwanda a Rwandan field assistant went to the research locations on the day the *gacaca* trials were taking place. These assistants resided in Kigali but travelled to the areas under observation each time *gacaca* activities were scheduled to take place. They wrote down every word spoken during the trials in Kinyarwanda. In addition they took note of important non-verbal interactions during the trials. They also added a field report to every observation of a *gacaca* session that detailed relevant information on events, rumours, social dynamics observed or established through informal interactions with the inhabitants in the community during that particular day of observation.

In addition to the two Rwandan field assistants that travelled each time to one of the research sites I had personally contacted one or multiple trusted inhabitant(s) of the research sites with the request to write extensive summaries of the ongoing *gacaca* activities. Following the *immersion* and *iteration* principles discussed previously, I had already spent several months in each of the research locations before the systematic observations started. I was thus able to identify trusted and able assistants belonging to the community. Most were young educated people and thus not implicated in the genocide. None were members of the local administration. They wrote similar observation and research reports (they had no ‘official’ permissions from government bodies in Kigali in order not to draw attention) of the *gacaca* activities as well as information on the dynamics surrounding the *gacaca* trials. None of these people were personally known to the research assistants travelling to the research sites from Kigali. On the one hand this established a control mechanism through which the observation activities of the research assistants could be verified. On the other hand it was a security measure to guarantee the quality of the observations when I was not present on site. Also, by using an “insider” and an “outsider” to the community the research reports could provide complementary information.

Initially, the outside researchers did not know the inside researchers although they were aware of the fact that they existed; they were thus aware of the control mechanism. This awareness increased the quality of the reporting and curbed potential fraud. Due to the long distance that needed to be travelled to the Jali sector, it was practically impossible to send an outside observer weekly or even twice a week to the community to observe every *gacaca* activity. In Jali, therefore, two local inhabitants monitored the *gacaca* proceedings. They were, however, not

aware of each other's identity. Also here this anonymity had the objective of installing a control mechanism that guaranteed trustworthy observations. In addition a research assistant residing in Kigali used to travel to Jali from time to time to monitor the *gacaca* proceedings as well.

As I will explain later multiple *gacaca* courts were introduced per sector in mid 2007. In addition the appeal courts started working as well. Multiple *gacaca* trials were conducted simultaneously. It therefore became impossible to record all of the *gacaca* activities verbatim with the same number of researchers. I consequently adapted the monitoring scheme. Since it turned out that all of the field activities by the research assistants were trustworthy as established through the oversight principles in the previous months and with the need to be able to make more observations simultaneously, the inside and outside observers were introduced to each other. From that moment on they jointly organized the monitoring activities. Every one focused on one of the *gacaca* courts during each *gacaca* session. They rotated courts in later sessions in order not to follow activities of the same courts all the time.

In case there were more courts operating than could be monitored it was decided to make an inventory of all the verdicts in the trials that had not been recorded verbatim. In doing so we still managed to have data on all the *gacaca* activities at sector level taking place in these research locations. For some trials, however, we only recorded basic information on the act of accusation, the persons involved in the trial and the verdict. There was no specific principle to choose the trials to be monitored in their entirety, they were simply selected at random.

Table 3. and 4. provide an overview of the trials monitored in each of the research sites (table 3.) as well as the distribution of the observations over the years (table 4.).

**Table 3. Overview *gacaca* data – systematic observations**

		Ntabona		Runyoni		Rukoma		Jali		All	
		N	%	N	%	N	%	N	%	N	%
Total		307	100%	686	100%	226	100%	508	100%	1727	100%
Nature of observation 1	Complete	141	45,9%	115	16,8%	106	46,9%	56	11,0%	418	24,2%
	Extensive	132	43,0%	65	9,5%	0	0,0%	438	86,2%	635	36,8%
	Summary Verdict	34	11,1%	506	73,8%	120	53,1%	14	2,8%	674	39,0%
Nature of observation 2	Almost All Data	273	88,9%	180	26,2%	106	46,9%	494	97,2%	1053	61,0%
	Limited Data	34	11,1%	506	73,8%	120	53,1%	14	2,8%	674	39,0%
Level of observation	Sector	282	91,9%	575	83,8%	160	70,8%	279	54,9%	1296	75,0%
	Sector - Appeal	24	7,8%	102	14,9%	60	26,5%	175	34,4%	361	20,9%
	Sector - Revision	1	0,3%	9	1,3%	6	2,7%	54	10,6%	70	4,1%

**Table 4. Overview *gacaca* data – systematic observation - year**

		2006		2007		2008		2009		2010		Total	
		N	%	N	%	N	%	N	%	N	%	N	%
<b>Total</b>		45	2,6%	1241	71,9%	67	3,9%	272	15,7%	102	5,9%	1727	100,0%
Nature of observation 1	Complete	0	0,0%	349	83,5%	16	3,8%	53	12,7%	0	0,0%	418	100,0%
	Extensive Summary	44	6,9%	233	36,7%	43	6,8%	215	33,9%	100	15,7%	635	100,0%
	Verdict	1	0,1%	659	97,8%	8	1,2%	4	0,6%	2	0,3%	674	100,0%
Nature of observation 2	Almost All Data	44	4,2%	582	55,3%	59	5,6%	268	25,5%	100	9,5%	1053	100,0%
	Limited Data	1	0,1%	659	97,8%	8	1,2%	4	0,6%	2	0,3%	674	100,0%
Level of observation	Sector <sup>a</sup>	45	3,5%	1009	77,9%	32	2,5%	131	10,1%	79	6,1%	1296	100,0%
	Sector - Appeal	0	0,0%	220	60,9%	11	3,0%	115	31,9%	15	4,2%	361	100,0%
	Sector - Revision	0	0,0%	12	17,1%	24	34,3%	26	37,1%	8	11,4%	70	100,0%

A distinction is made between the nature of the observation as well as the level of observation. “Complete” refers to the trials that were recorded verbatim by the outside observers who travelled to the research locations. “Extensive summary” refers to the trials monitored from mid 2007 onwards made by the research assistants, inhabitants of the communities. The latter were also instructed to record the trial activities in as much detail and as exhaustively as possible but due to the specific nature of their observing activities and their skills some of the interventions in a trial were not recorded. Therefore, I do not consider them as “complete” observations. They are, however, “almost complete”. In addition there are the “verdicts”. These are the trials of which the proceedings were not observed but where some basic information as well as the outcome of the trial was recorded.

The level of observation refers to the nature of the sector level trials. A trial could take place in first instance, in appeal or in revision. In all of the research locations as in all other sectors in Rwanda *gacaca* proceedings were also taking place at the cell level. The cell is like a neighbourhood in a sector. Every sector contains multiple cells. These cell level *gacaca* activities dealt with property cases. Since there are multiple cells in each sector and since the *gacaca* activities at these levels were generally also organized simultaneously during the same day of the week, it was impossible to monitor all of these cell level activities as well. We only monitored these cell level activities punctually. Also here, there was no specific principle to select the trial activities to be recorded at the cell level: it happened at random.

In doing so an additional 103 trials at cell level (property related cases) were observed (throughout the four locations). Forty-five trials dealing with property related cases at sector level were also observed (throughout the four locations). These trials were a sort of appeal trials to the *gacaca* activities that took place at the cell level. An additional 42 *gacaca* trials were observed in other locations, thus locations throughout Rwanda not systematically monitored (see map 1.). These observations took place punctually and were organized for comparative reasons, to live up to the breadth-depth principle, namely by situating the systematic observations in the four research sites in the context of ongoing *gacaca* activities in other areas. In total 1,917 trials were observed dealing with allegations against 2,573 individuals.<sup>23</sup> 1,338 of

[23] For comparative reasons and to accentuate the profile of our data set, reference can be made to three international NGOs that systematically monitored the *gacaca* activities. Penal Reform International (PRI) observed 1240 trials (806 during the information collection period however and these are not actual trials) (PRI 2009: 59), Avocats Sans Frontières (ASF) monitored 1455 trials

these individuals stood accused at the sector level and 1,235 of them were involved in cell level proceedings dealing with property offences.<sup>24</sup>

#### 4.1.2. CODING AND DATA ANALYSIS

The *gacaca* proceedings were recorded verbatim or summarized in Kinyarwanda. We did not use any recording devices since this was not permitted through the permission received by the National Service of the Gacaca Courts (SNJG). As mentioned, I preferred not to use such devices since they can create suspicion in the population. The Kinyarwanda version of the observations was subsequently translated into French and electronically captured.<sup>25</sup> The observations in the four research sites resulted in a total of 2,898 electronic pages (roughly based on an estimated 5,800 pages originally written in Kinyarwanda in the field). That is 1,097,172 words.

Time was needed to digest and analyse this amount of information. With the observations ongoing, I read all of the recorded observations that were given or sent to me on a monthly basis. With the assistance of a Rwandan collaborator not involved in the monitoring activities I also compared at times the Kinyarwanda version of the recordings with the translation in order to guarantee both the quality of the observation as well as the quality of the translation. This continuous reading of incoming reports as well as my own regular observations in the field made me familiar with the overall logic and the main features of the *gacaca* activities in practice.

In 2008, taking the mixed methods principle characterizing the study design into account, it was decided to code all of the observation reports according to a number of variables. The objective was to bring more structure to relatively unstructured data in order to systematically analyze the *gacaca* activities. Based on the familiarity with the *gacaca* process I constructed a set of 171 variables.<sup>26</sup> Two Rwandan field assistants started coding the observed trials in 2008 under my supervision. This process lasted until mid 2011. Only the systematically monitored trials at sector level observed in the four research locations were coded. Trials observed in other locations as well as cell level trials (even in the four locations systematically monitored) are not considered in this process of quantification/structuration.<sup>27</sup>

An observation is not a questionnaire. A questionnaire is usually completed exhaustively, an observation cannot be manipulated in order to reach all the info needed (see also table 1. discussed earlier). *Gacaca* trials are not duplications of a preset form. Although they evidently follow a similar pattern not all trials contain the same information. The trials did not contain information on all of the variables. Therefore, some information on some variables was simply not visible in the *gacaca* transcript and could, therefore, not be coded. In addition, as we have explained, some *gacaca* trials were transcribed in their entirety, some were extensively summa-

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(ASF 2010: 11) and Human Rights Watch (HRW 2011: 11) followed 350 cases throughout the entire period that the *gacaca* process was operational. It remains unclear however how they define 'a trial' or 'a case'. This might be different from the definition adopted in this study. Nevertheless, none has attempted to establish a full oversight in the activities in a single location, neither did they quantify their observations on a large number of variables to systematically analyze the observations. To no surprise, since it is not their mandate or objective to do so. This study, inspired by the diachrony-principle underlying the design did establish such a record over time. See the reports on the website of Penal Reform International <http://www.penalreform.org/publications/gacaca-research-reports> and the reports on the website of Avocats Sans Frontières: <http://www.asf.be/fr/publications>. (Websites last accessed on 10 July 2011).

[24] The logic of trials at the cell level was different from sector level trials. Multiple defendants were tried jointly during trials at the cell level. Sector level trials primarily dealt with defendants sequentially. Hence the reason why the number of trials observed at the cell level involved many more defendants.

[25] A limited number of trials were translated *in situ* during the observation process.

[26] A number of these variables, approximately 50, focus on specific information on the genocide. These data have less to do with the actual *gacaca* process and will, therefore, not be considered in the current analysis.

[27] We did, however, quantify some aspects of the cell level proceedings on property cases observed in the four research locations.



alized, for others only basic info was recorded on accusations, people involved and the verdict.<sup>28</sup>

Some operations – for example the analysis of the nature of interventions during trials – could only be performed with respect to trials that had been entirely transcribed. The analysis of some variables is therefore based on a total of 418 observations. The latter is the total number of trials that were recorded verbatim (with a guarantee that no information is missing). A prerequisite to make a decent analysis of all interventions during trials is, for example, the need to have recorded all interventions during these trials.

Other aspects are analyzed based on a total number of 1,053 trials which is the combination of trials recorded verbatim and trials recorded as extensive summaries. For example, information regarding the fact whether activities of information gathering supplementary to the actual trial proceedings had taken place is based on the analysis of these 1,053 trials. The field observers had been instructed to record information on a certain number of variables from the start of their observation activities. Therefore, this information was always available and could be coded for each case.

The number of observations fluctuates for a number of other variables. For some variables – such as the number of confessions made by a defendant – we depended upon the fact whether information on confessions had been divulged during the trial proceedings. If this was not the case, the trial in question could evidently not be considered in the analysis. The total N is thus lower. Or the analysis of the total number of people attending the trial for example was not systematically written down by the field assistants. It was recorded in 927 trials, so the analysis is based on the observation of 927 trials. Although the field observers had been instructed from the start of the observation activities to record these numbers, human error made this not happen in some cases. The number of cases used for this variable is thus lower than 1,053.

The quantification/structuration of these qualitative/unstructured data was undertaken in order to establish trends. I used these data as descriptive statistics to gain a systematic insight into the *gacaca* process. Although these findings are statistically representative for each of the research locations, I cannot claim representativeness in the statistical sense of the word for the *gacaca* process as a whole. Considering the limitations in budget and time this was simply not possible and it was never the objective. The objective of this study is to establish an empirical comprehensive record of the *gacaca* process, to proceed with an analysis that is empirically grounded. The nature of the selection of the research sites, the completeness of the observations in these sites as well as the comparative embedding of these systematic observations in those undertaken in other locations guarantees the veracity of the findings. It guarantees *trustworthiness*, a key concept underlying qualitative research strategies, as explained in the introduction of this paper

Although it is necessary to rely on a considerable number of observations, quantity is, however, not the main issue. The quality of the analysis is more important, of course. Therefore, the approach to the monitoring and analysis of the *gacaca* activities was - taking into account one of the study principles - considered to be an *iterative* research process. The quantification/structuration was done based on the qualitative/unstructured familiarity with the *gacaca* process. The quantified/structured findings were subsequently used to further explore and analyse these trends by exploring the qualitative/unstructured data.

I conclude with some definitional issues. A “*gacaca* session” is – in our research

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[28] Where the analysis of the *gacaca* proceedings is presented in publications that make use of the *gacaca* data described here, there might thus be a changing number of total observations (N) depending on the variable, which is a consequence of the above.

design, not by law or regulations - defined as the gathering of a *gacaca* court. A session can deal with multiple accused. If there were more accused they were normally dealt with sequentially, at times in a joint sub-session. Each individual case was considered as “a trial” (thus a trial is related to the individual). If the cases of two individuals were treated jointly in one (sub-) session, these cases were considered as two trials. The total number of observations that are used in the systematic analysis – 1,727 – thus deal with the trial of 1,727 individuals.<sup>29</sup>

#### 4.2. Life History Approach

As explained in the introduction, the objective of my study on Rwanda’s transition was (and is) to understand the experience of transition as *lived from below* in general. This was attempted first and foremost through the observation of the *gacaca* activities as described in the previous section. In addition, it was needed to explore also the experience and perception of the people practising *gacaca*. During the *iterating* process of reflecting on the research topic described earlier I decided to adopt life history interviewing as an additional important research technique for two reasons: content and context.

A life history approach has proven to be a method *par excellence* when social change and social mobility are under investigation (Thompson 1982: 289-306, Bertaux & Thompson 1997). The impact of political and economic change or development as such on the lives of individuals and social categories can be studied by adopting a life history approach (Slater 2000, Delcore 2004), the changes brought about by industrialization or livelihood diversification are explored through the use of life stories (Lie 2000, Slater 2002), waves of migration over time can be adequately understood by making use of life stories (Balan et al 1973, Rogaly & Coppard 2003, Panos Institute 2006) and life history research is increasingly being used to research poverty dynamics over time (Davis 2006 & 2009, Bird & Shinyekwa 2003, Kothari & Hulme 2004, Narayan et al 2009; Narayan 2009, Narayan et al 2010). Historians have often resorted to a life history approach since a life story is told with a constant reference to historical change (Plummer 2001: 39-40, Thompson 1981).

I mentioned previously the need to use innovative approaches aimed at capturing the voices of ordinary people in the Rwandan socio-political context. From this perspective too, the life history approach was considered to be an asset. Blee (1996), for example, adopted a life history approach to explore racial movements since “racist activists tend to be disingenuous, secretive, intimidating to researchers, and prone to give evasive or dishonest answers” (Blee 1996: 687). Life story interviews proved to be more productive because the topic under investigation was explored indirectly. The technique proved to be an excellent approach to deal with the *aesthetics of progress* and *ethics of dissimulation* phenomena (Ingelaere 2010).

By collecting life stories the respondents not only told their own stories but also – indirectly – the story of regime change, differences in perceptions according to varying social categories in the population. Without directly inquiring about that topic they told the story of *participating in gacaca from below*.

There is a huge diversity of life stories and a multitude of approaches to conduct life history research. The specific approach is referred to by a number of terms: life histories, life stories, biographies, oral histories, life narratives, testimonials etc. In its most basic sense, a life story is “the story a person chooses to tell about the life he or she has lived, told as completely and honestly as possible” (Atkinson 1998: 8). Life stories differ from life histories to the extent

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[29] The situation was slightly different with respect to cell level trials

that the latter term takes into account the aspect of collection, editing, interpretation and (re) presentation by the researcher (Roberts 2002). The idea here is not to present a comprehensive overview of the history of life history research (Bertaux & Kohli 1984), neither to give a comprehensive insight into life story research. Extensive discussions of a life story/oral history approach can be found in Vansina (1996; 2006), Thompson (1978), Bertaux (1981), Denzin (1989), Tonkin (1992), Perks & Thomson (1998), Atkinson (1998), Miller (2000), Plummer (2001), Roberts (2002) and Ojermark (2007). I, nevertheless, refer to some trends in the multitude of approaches in the following sections. The objective is to demonstrate that informed choices were made when tinkering a variant of life history researching. The construction of this approach had to take into account the previously explained research principles underlying the study itself: *iteration*, *multi-sitedness*, *mixing methods*. *Diachrony* is evidently a key characteristic of a life history approach as such; while *immersion* is more an encompassing principle of the study.

#### **4.2.1. Surveying Life Stories: Stratified Random Sampling in Two Waves**

Life stories can be long or short and research using life stories can make use of one, a few or many stories. Extensive life stories generally aim to present the “full account” of the subjective realm of one individual. Classic examples are the lengthy story of a Polish migrant to the United States, Wladek Wisniewski (Thomas & Znaniecki 1920/2007), the story of Nisa, a !Kung! woman (Shostak 1981), the life of Rigoberta Menchu, the nobel prize laureate (Burgos-Debray 1984), the story of a South African sharecropper, Kas Maine (Van Onselen 1996), the life of an African bar girl in two volumes (Chernoff 2003 & 2005). Shorter life stories are often truncated versions of the book-length stories. As a consequence however, shorter stories are often “more likely to be edited down into the researcher’s ‘story’” (Plummer 2001: 25). The shorter stories are more focused as the objective is to explore a topical issue instead of the whole of the life experience. Jane Fry (Bogdan 1974) is a classic study of one individual but with a limited focus on the theme of trans-sexuality, the life story of a Thai farmer is used to explore the topic of development (Delcore 2004), the analysis of the life story of a female member of a neo-Nazi group needs to facilitate the understanding of racism (Blee 1996), the narrative analysis of a Brazilian woman’s life story is used to depict the complexity of identity and the incoherence of life (Patai 1998), two life stories are used to understand poverty (Hulme 2003).

The length of the story and the number of stories collected is important with respect to representativeness, validity and reliability, although several authors argue that this evaluation mode is inappropriate with respect to life history research (Atkinson 1998: 59; Plummer 2001: 153-154). As with the *gacaca* observations, it seems more important to consider the completeness, carefulness, veracity and plausibility of the data produced and the empirical record established. To reach these goals an informed choice needed to be made considering the breadth-depth principle underlying this study.

Reducing the length of the stories makes an increase in the number of individuals under scrutiny possible and thus establishes the breadth of the study (findings). Twenty-four life histories of rural Ugandans need to shed light on the movement in and out of poverty in Uganda (Bird & Shinyekwa 2003). A life history project collected 54 life stories of internally displaced Colombians out of which 19 are documented in a book (Panos 2006).<sup>30</sup> Davis (2009) collected 242 life stories and uses 90 to analyze poverty dynamics in Bangladesh. Balan et al (1973) make use of the life cycle information of 2020 men to analyze geographical and social mobility in Mexico.

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[30] Panos London uses life stories and oral history to give voice to poor and marginalized people from all over the world. The Panos website presents the findings of a range of research projects using a life history approach. See: <http://www.panos.org.uk/>

A World Bank study used thousands of life stories from 14 countries to study determinants of movement out of poverty (Narayan et al 2009); the study makes use of 2900 life stories in India alone, one of the study countries (Narayan 2009).

As these examples of life history researching demonstrate, doing systematic and structured life interviews in a number of localities and with multiple respondents selected through a sampling scheme is thus an option when designing a life history approach. It is noted that those who collect oral histories rarely sample their respondents, while those who sample rarely collect oral histories (Varshney 2002: 20). But Varshney has shown in his study on ethnic conflict in India that combining both yields innovative findings. Following the research principles underlying the study I adopted such an approach.

As explained in the section dealing with research sites, the selection of communities (sectors) was guided by the principle of establishing both breadth and depth of the study. The research locations where life histories were collected were chosen based on the idea of ‘attaining maximum variance’<sup>31</sup> which allowed for an indicative apprehension of life experiences incorporating various dynamics of historical events and state or societal practices. Field sites are highlighted in the map (map 3.1.): large dots are sites where life stories were collected.

Given the topic of study ethnicity is evidently an important variable to take into account with respect to a “structured” selection of respondents. Although Rwanda continues to be ethnically bi-polar, with Hutu and Tutsi as the main ethnic categories<sup>32</sup>, it is no longer permitted to identify people through these ethnic markers; instead, ‘new’ social categories with new forms of markers have emerged.<sup>33</sup>

The few studies that have focused on identity suggest that “ethnicity remains a central factor for Rwandan social identity” (Longman and Rutagengwa 2004: 176) and that “today (ethnic) group identity is meaningful (arguably even more than before the genocide)” (Buckley-Zistel 2006: 112) or that “the Hutu/Tutsi distinctions are more rigid than ever” (Zorbas 2004: 42). Penal Reform International, an NGO undertaking research on the *gacaca* process and reconciliation, observes that Rwandans do not contest that they are all Rwandans but that this does not necessarily mean that the feeling of ethnic belonging does not persist as well: “the racist and discriminatory dimension of ethnic belonging does not appear diminished” (PRI 2004: 38). Waldorf (2009: 118) state that government policy “reinscribes ethnic divisions” in an analysis of the social dynamics resulting from laws against divisionism and genocide ideology. Mclean-Hilker (2009: 96) concludes a study with Rwandan youth with the observation that the Rwandan policy on reconciliation “has been to emphasize rather than de-emphasize ethnicity and reproduce the “ethnic” logic that underpinned the genocide”. Although these studies are very well researched and argued, an understanding of the breadth of such arguments and its relation to the *gacaca* process remains absent.

A danger of reification exists when focusing on crude ethnic categories. As Nigel Eltringham (2004) rightly remarks: one has to avoid an “absolutist schema of social distinction that they [genocide perpetrators] project on to society and the absolutist version of history to which they appeal”. The analysis that will be presented in the following section does not place ethnicity central stage. However, both during data gathering activities – in general and not

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[31] This procedure was modelled on the work of, for example, Varshney (2002) and Gibson and Woolcock (2005).

[32] Information on the situation of Twa in relation to the policy of unity and reconciliation in Rwanda can be found in Thomson (2009) and Adamczyk (2011).

[33] See Eltringham (2002) for a discussion of these new identities. See also Helen Hintjens (2001; 2008) insightful discussion of ‘identity politics’ in Rwanda.

only regarding life story interviewing – and the analysis and interpretation of the data attention was given to ethnic categories, categorization and awareness. Attention to ethnicity does not imply that other dimensions of Rwandan identities – such as socio-economic class, professional status, regional affiliation, gender or age – are ignored or considered to be irrelevant. On the contrary, a comparative micro-analysis of the genocide demonstrates, for example, that the violence unleashed at the macro level was appropriated and fundamentally shaped by the micro-political matrices and social formations in which it took hold (Ingelaere 2006b). Genocide, although shaped from above, was significantly reshaped in a highly differentiated terrain of local social tensions and cleavages, regional differences, and communal or individual particularities. The genocidal violence reflected both the goals of the supra-local forces and factors : mainly the Hutu–Tutsi cleavage mobilized by political actors for political purposes and their local shadows: struggles for power, fear, (intra-group) coercion, the quest for economic resources and personal gain, vendettas and the settling of old scores.

Ethnicity was not a structuring device in the design, analysis and representation of this study on Rwanda’s transition, although it plays an important role in a number of publications (Ingelaere 2007b, 2009a, 2010b). Ethnicity is taken into account because Rwanda is a bipolar society with Hutu (approximately 84 percent) and Tutsi (approximately 14 percent) as the main identity groups marking the social and political landscape but also because of the fact that the master narratives of the 1994 genocide and other periods of violence and war in Rwandan history were structured along ethnic lines, even if the mobilizing momentum was (partly) derived from other motivating forces. The *gacaca* process deals with (a clearly demarcated set of) these crimes: it is inevitable to pay attention to ethnicity. And – most importantly – because ethnicity has officially been banned from public life it has become an unobservable variable in most (empirical) studies of post-genocide Rwanda.

The selection of respondents for life story interviewing was based on markers that – to a certain extent – underlie ethnic categories. Local inhabitants distinguish five social categories to identify themselves and others, especially also in the context of the *gacaca* activities. These social markers were used in the selection and sampling procedure during the life history surveys since it was evidently important to understand differences in experience and interpretation of transition and *practising gacaca*.

Tutsi inhabitants are divided into ‘genocide survivors’ and ‘old caseload returnees’. In the latter case they, their parents or even their grandparents fled Rwanda after the so-called Hutu Revolution of 1959 and returned to Rwanda after the end of the genocide and the RPF take-over in 1994. Hutu inhabitants are described as ‘released prisoners’, those ‘accused in *gacaca*’ and those who are ‘not accused and have never been imprisoned’.<sup>34</sup> Lists were compiled with the names of all the household heads on the selected hills and several groups of key informants were asked to identify every household according to one of these five groups. Subsequently households within each group were selected through a stratified random sampling scheme and their heads or another adult member were interviewed resulting in around 70 respondents in each locality.<sup>35</sup> Those selected were all over 30 years old, since respondents needed to have lived through the transition and regime changes and be aware of the period since 1990 and 1994. Interviewees needed to have had agency in the genocide and the period of war in the nineties, otherwise their experiences would have been less relevant to understanding the *gacaca*

[34] I did not use the category of so-called ‘new caseload returnees’ (people returning to Rwanda after fleeing Rwandan in the wake of the genocide in 1994) since this category is no longer salient in social life in the Rwandan hills.

[35] Some communities did not have old caseload returnees, others didn’t have released prisoners.

practice.<sup>36</sup> Table 5. gives an overview of the number of respondents and the social category they belong to.

**Table 5. Overview life story interviews**

	2007 Wave			2011 Wave			Total
	Male	Female	Total	Male	Female	Total	
<b>Hutu - not accused /no prison</b>	86	65	151	74	58	132	<b>283</b>
<b>Hutu - accused in <i>gacaca</i></b>	81	11	92	52	11	63	<b>155</b>
<b>Hutu - released prisoner</b>	72	1	73	54	0	54	<b>127</b>
<b>Total Hutu</b>	<b>239</b>	<b>77</b>	<b>316</b>	<b>180</b>	<b>69</b>	<b>249</b>	<b>565</b>
<b>Tutsi – survivor</b>	39	61	100	34	54	88	<b>188</b>
<b>Tutsi – old-caseload returnee</b>	28	25	53	20	20	40	<b>93</b>
<b>Total Tutsi</b>	<b>67</b>	<b>86</b>	<b>153</b>	<b>54</b>	<b>74</b>	<b>128</b>	<b>281</b>
<b>All respondents</b>	<b>306</b>	<b>163</b>	<b>469</b>	<b>234</b>	<b>143</b>	<b>377</b>	<b>846</b>

As will be explained in detail in the following section where the contents of the interviewing activity are described a pilot study with life story interviewing took place between January and July 2006 whereas other research techniques were used as well during that period. A first wave of life story interviews was conducted between January and April 2007 in the above mentioned localities.<sup>37</sup> A second wave was organized between January and June 2011.<sup>38</sup> The objective was to re-trace the same people in order to complete their life stories. The number of respondents was then lower due to attrition (decease, migration, imprisonment, etc.).<sup>39</sup> The organization of this second wave also made a comparison between two “objective” points in time possible.

#### 4.2.1.1. **A Nested Approach: Realist Tales, Narratives and Subjective Rankings**

Anthropologists have often used a life history approach to address one of its main concerns: subjectivity and meaning: “the life history reveals, like nothing else can, the subjective realm” (Plummer 2001: 20). Famous examples can be cited of anthropological publications where the main device to explore these topics is the life history (Smith 1954, Crapanzano 1985, Shostak 1981, Burgos-Debray 1984, Werbner 1991, Bozzoli 1991, Fernandez & Gutiérrez 1996, Caplan 1997, Chernoff 2003 & 2005). Miller (2000: 10-17) further distinguishes three common approaches available to analyze life histories: narrative, realist and neo-positivist. The narrative approach emphasizes the mediating function of the context, which refers to the position of the individual telling the story as well as to the research context that elicits the tale and produces

[36] Some authors (Sommers 2012; Mclean-Hilker 2009) have focused on youth in post-genocide Rwanda.

[37] Due to practical circumstances, the life story interviewing in one location took place in December 2007-January 2008. The findings from this location are currently integrated in the study but they were not part of previous publications that made use of the life story data (Ingelaere 2007b, 2009a, 2010b).

[38] The lists identifying people according to these categories were initially compiled in 2007. By 2011, the *gacaca* process had continued. More people were accused in *gacaca*. A person was recategorized in case he or she was accused in *gacaca* after 2007. The 2011 life story survey focused on the period 2000-2011 in order to complete and complement the 2007 survey results.

[39] I recorded the reasons why people where not available in order to assess what influence attrition might have in the follow-up survey. The reasons were diverse without a clear pattern that might skew the comparison between the two waves. On the other hand, it became noted that a number of people were not included in the second wave since they were imprisoned or executing community service.

the story. The realist approach is essentially inductive; it is a grounded-theory perspective that uses the empirical material that arises from the data. A point of saturation in the data-gathering process allows for the construction of theory or analytical concepts. On the other hand, the neo-positivist position is deductive: theory is tested by gathering empirical material. It is evident that all of the afore-mentioned perspectives overlap to a certain extent. Moreover, several other frameworks can be deployed to analyze “the layers of life story meanings” (Plummer 2001: 39, Agar 1980, Luborsky 1987).

Given the objective of the study, the overall research design and a worldview of “pragmatism” underlying this design – i.e. centrality of the research questions and the existence of singular and multiple realities – the life history approach was used inductively to gather factual (realist) information on the reference reality under scrutiny *and* to explore the subjective realm (narrative) of individuals experiencing transition and participating in the *gacaca* practice. The life story material is primarily considered as a resource not as a topic.<sup>40</sup> It means that life story data provide social commentary. This is what this research project attempted to do: “what does it mean to live through a transition?”

In doing so an analysis of these numerous life story elements in the narratives enabled us to understand – in recollection and in perception - what it means to live through political transition and *in* transitional justice: to shift from a period of violence towards peace, to move from one regime into another and most importantly: what it means to participate in the modernized *gacaca* courts.

However, such an intake on life story research is often combined with other quantitative and qualitative methods. The information can be structured to allow more focus. The involvement of the researcher becomes more important than a direct narration (Ojermark 2007:15). Following the *breadth-depth* and *mixing methods* principles underlying the study I added such a structuring/quantitative element to the exercise: this quantitative element operated as a structuring device in the data gathering activity. This structuring element resided firstly in the use of a semi-structured interview guide and secondly in the systematic use of a visual to elicit rankings over time.

First, regarding the semi-structured interview guide: during a pilot for the life story interviews in 2006, I conducted 50 full life story interviews with 30 Hutu and 20 Tutsi respondents from several villages (sectors). These interviews were conducted during several sessions and through open-ended questions touching on almost every aspect of the interviewee’s life. These initial life story interviews lasted in total between 7 and 14 hours per interview (spread over several sessions). Based on an analysis of these narratives I identified important recurring themes in the narratives and I derived a set of questions to be used during shorter interview sessions. Five themes corresponded with the different dimensions that matter in life for ordinary people: the socio-economic situation, the feeling of security, the level of confidence (trust) in others (with a subsection for one’s own ethnic category and that of others) and the feeling of

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[40] With respect to the analysis the nature of life history research is marked by a distinction between the use of life stories as a resource or as a topic (Plummer 2001: 36-45). Life stories are resources when they are used to shed light on (a dimension of) social life. The account of the individual experience is the inside perspective used to generate insights with respect to subjectivity and meaning, history, ambiguity, process and change. Life stories become a topic as such when the production of the story, the telling of the tale and the construction of the individual life are under investigation. From this angle, life history research is an instance of narrative inquiry used to explore the interplay between interviewee and interviewer (Behar 1990) or the construction of the self and the nature of identity (McAdams 1988 & 1993, Patai 1988, Hammack 2006). Narrative approaches focus on the life story construction by the subject. The life story is (also) a topic when, for example, identity is theorized as a narrative (Ezzy 1998) or the narrative construction of the self is explored through a so-called “cartography” (Sermijn et al. 2008, Roets et al 2009).

political representation and personal prospects for the future.<sup>41</sup> Important to note that these concepts were not imposed on the interviewees as *etic* conceptions. By contrast: I made sure that also the structuring of the interview procedure was based on an *emic* understanding that had emerged from the open-ended and exploratory pilot phase.

But the developed interview guide was also in this procedure semi-structured containing mainly open-ended and facilitating questions that structured the interview around these five themes. The structured interviews lasted between 1.5 and 3.5 hours and enabled us to cover a larger sample of respondents.

Secondly a visual was used during each life story interview to help the respondents in assessing these key themes (economic situation, feeling of security, etcetera) in the different periods in their lifespan (Figure 3.). I refer to this visual as a “ladder of life”.<sup>42</sup> In the life story interviews a value between –5 and +5 was given (by the respondent) through pointing to the appropriate step on the ladder for each year during the adult life cycle.

The nature of the image was explained: on top of the ladder are those people who feel the most, secure, politically represented etc. in ‘the community of the respondent’ as to avoid comparison with residents of Kigali, which would oblige them (in their perception) always to choose the bottom steps. The spatial reference is their own community and the field sites in this study were always rural with predominantly peasant inhabitants. When the geographical area for comparison was defined and the people on the top step characterized (step +5), the nature of the bottom step was further explained for every theme by stating, for example, that that step was occupied by people who’s economic situation is worst in the community, who are ‘the worst off in political representation in the community, etcetera (step –5).<sup>43</sup> The enumerators and my translator were trained (and supervised) always to use exactly the same phrasings to explain the nature of the ladder and its steps in order to avoid a heterogeneous interpretation by the respondents. Equally important, and as mentioned previously, is the fact that all respondents have a similar understanding of the concept of these five structuring themes. The respondent was always first asked to describe in his/her own words how he/she interpreted this notion and the analysis of these responses indicated a shared understanding (see for example Ingelaere 2007, 2008, 2010).

Subsequently, the respondents were asked to place themselves on the ladder. The question asked for every period or year was: ‘At this point in your life, where do you situate the experience of security, political representation, etc. compared to the other inhabitants of your community (sector)?’ Subsequently a move back in time was made to the year of marriage or the first year of adult life (if single), repeating the question for that point.<sup>44</sup> The same questions

[41] The above-mentioned elements can be considered as the dimensions of life that matter for ordinary people. They are also reflected in the findings from large-scale research that aimed to establish the different dimensions of “well-being” and the “good life.” Those identified included: material, physical and social well-being; security; and freedom of choice and action (see Narayan, 2000). They refer equally to the different dimensions of the concept of “human security” that shifted the attention from the territorial security of nation-states towards the security of people. The main characteristics of “human security” were summarized in the 1994 World Development Report (UNDP 1994) as economic, food, health, environmental, personal, community and political security.

[42] Inspired by Cantril (1965). The World Bank research on movement out of poverty makes use of a similar ‘ladder of life’ tool in order to understand perceived changes over time. This approach was an inspiration in the development of design and methodology of this study. See: Narayan et al (2009), Narayan et al (2009), Narayan and Petesch (2010). Krishna et al (2004, 2005, 2006a, 2006b, 2006c, 2009, 2010) make use of a similar “stages of progress” method. This “stages of progress” method was apparently an inspiration for the studies undertaken by Narayan.

[43] During the life story interviews with ‘old caseload returnees’ the questions about political representation in the 1980s (when they did not live in Rwanda) was not further qualified but asked as prescribed in the standard procedure. It implies that respondents in their narratives and rankings predominantly took into account both the feeling of representation in the countries where they resided at the time and in relation to their home country, Rwanda.

[44] Interviewers were instructed to start the ranking exercise from the first year of adult life or marriage – or, if necessary, always



were then asked with reference to the past asking a rating for every year. The findings from the life story narrative were used to help people recall their situation at a certain moment in time. For example, when someone had told us he or she had a firstborn child in 1986, reference would be made to 1986 as ‘the year when your first child was born’.

It needs to be noted that the scale (ladder) itself remains fixed throughout the different periods in time. The scale functions as a mental map and background against which the personal movement up and down the ladder of life – the imaginary but stable situations/levels of security, confidence, political representation, ... – is assessed ‘in time’ and ‘in comparison’ with the surrounding environment: the fellow community members also moving on the ladder of life.

It is important to keep in mind that these rankings are indicators of perceptions and portray changes in rankings over time: events and periods in the past are re-interpreted through the lens of events happening during subsequent life periods. Since no baseline data are available recall is the best means to get at these issues. Moreover, since the objective is to understand the experience and perceptions of transition, recollection reflects how perceptions work: they are influenced by individual experiences in the past and mediated by discourses produced by the government, media and other institutions, past and present.

I have previously portrayed the results of the subjective ranking exercise by making use of graphs (Ingelaere 2007b, 2009a, 2010b). Sometimes life history research makes use of graphical representation techniques. Balan (1973) constructs “path diagrams” based on information available in the biographies of men. Goldthorpe (1980) developed a technique to visualize life trajectories. Hammack (2006) uses “life-line drawings” where Israeli and Palestinian youth draws a line that represents the events of their lives accompanied by the story of the life of the individual. Davis (2009) plotted the life trajectories on a vertical time axis and horizontally with respect to critical life events influencing the dynamics of social mobility during the life course. A similar approach was used during the World Bank “Moving Out of Poverty”-study (Narayan et al 2009; Narayan 2009).<sup>45</sup> Graphical representations are not only a powerful tool to visually depict the crucial stages in the life course, they are also heuristic devices in the analysis.<sup>46</sup> I followed a similar strategy in the analysis and interpretation of the life story material.

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in the year 1990, even if the respondent was not yet adult or not yet married then. This implies that very few respondents were below adult age in 1990. It also implies that for the 1980s (2007 wave) there are fewer observations (rankings). Weighting was applied in order to account for research design and sampling procedure. The figures picture the weighted ‘average’ ranking of all respondents. For the years 1980–9, these weighted averages are based on the rankings of fewer than all respondents, since some respondents were not of adult age or were not yet married.

[45] Especially the country reports available on the website present findings of the life story interviews, including the graphical representations. See: <http://web.worldbank.org/WBSITE/EXTERNAL/TOPICS/EXTPOVERTY/EXTMOVOUTPOV/0,,menuPK:2104414~pagePK:149018~piPK:149093~theSitePK:2104396,00.html>

[46] For example Davis (2006 & 2009) identified eight typologies or trajectory patterns in his study on poverty dynamics in Bangladesh based on the graphical representations. These patterns subsequently allow for a focused analysis of the narratives and information available in the life story interviews.

## 5. OTHER RESEARCH TECHNIQUES

### 5.1. Key Informant Interviews and Individual Interviews

I evidently had many conversations with inhabitants of the locations under study while residing in these areas. These conversations were not formally recorded, at times I made jottings after the conversation in case these verbal interactions had been important in some respect. The example of the former mayor discussed previously is a case in point. Many times, however, I organized formal interviews with individuals. With “formal interview” I mean that the conversation was documented: name, time and date were identified and a transcript is available. The motivations to conduct an individual interview as well as the choice of the topic varied depending on the research phase and the context at hand. For example, I conducted more individual interviews in the exploratory phase of the research in 2006. Or, when organising group discussion I would contact a particular participant in the group discussion in case I thought this person could have an added value for the exploration of the research objectives or since I had observed a particular behaviour (silent, outspoken, fear, ...) of that participant in and potentially due to the group setting. The person would be individually contacted to do a follow-up interview on a specific topic; these interviews were mostly informal and aimed at clarifying certain issues.

Throughout the research locations I also established contacts with key informants. I consider a “key informant” to be someone who played an important role in the facilitation of the research or who had important knowledge to clarify a dimension of the topic under scrutiny or simply someone I had developed a trusted relationship with. In every location I had multiple key informants. These key informants were people able to provide important information on history, social and political organization, economic life and ongoing community dynamics in the margins of the *gacaca* activities. In an attempt to avoid getting caught in cliques I would always crosscheck the information or opinions with multiple key informants. These key informants were also solicited to identify the individuals and households living in the area on the basis of their different social positions in the community, for example to organize the sampling of the life story respondents or to identify the demographic information of people testifying in *gacaca* in case that info was unknown to us.

Leaving aside these numerous informal conversations, I have a record of 77 formal interviews conducted throughout the research period and locations.

### 5.2. Archival Research

When possible local archival records were consulted. These could be archives of local authorities, local mediators or administrative archives at district or provincial level. I did not consult the *gacaca* records of *inyangamugayo* since I had no permission to do so and more importantly this could create the perception of interference in the unfolding of the *gacaca* process. I wanted to avoid such a perception at all costs since it could create animosity and complicate the research activities. With one exception: I consulted the *inyangamugayo* at the local level in 2006 regarding basic numeric information on the genocide and *gacaca* process collected during the information collection phase.<sup>47</sup> But also at that point, I made sure not to consult the archives myself but only through the intermediary of multiple *gacaca* judges of the respective bench of the courts at the cell level of the sector. Also here the reluctance to consult the records myself (or through a Rwandan collaborator) and the subsequent decision to do so with a minimum of two members of the bench present was informed by my continued awareness to curb suspicion

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[47] I refer to that information regarding 6 research locations in Ingelaere (2007b: 56-57)

and maintain access to the field.

I consulted other archival records in order to get information on a variety of topics, ranging from the history of the community to the nature of conflicts and conflict resolution capacities of the locality. The consultation of archival records was especially important regarding the latter aspect. I used an insight in the type and incidence of conflicts to understand the impact of the *gacaca* process (Ingelaere 2011). The key informants and especially the *abunzi*<sup>48</sup> committee members were often able to give an overview of the nature of conflicts in the area. The latter is the institution specifically tasked to deal with local conflicts. Specially designed forms were handed out for this objective. This information was compared with findings from the group discussions.

### 5-3. Group discussions/interviews

I made use of group discussions (FGDs) during several phases of the research process even in the awareness of their limitations. The group setting – the identity of the participants or the nature of their behaviour – can imply a dominance in the verbal interaction or guide the direction of the discussion. The latter can (or should be part of) the nature of focus group data analysis. In general, however, a group setting does not necessarily have to be problematic in case one is aware of the strengths and limitations of (focus) group interviewing. For example, the presence of multiple knowledgeable people can guarantee the veracity of the information when eliciting very factual information such as the nature and incidence of conflicts in a community. And the group setting is at times also a means to gain an insight into the opinions of certain categories of the population. It is important to manage the group discussions well, regarding selection of respondents, location of the encounter and the interaction between the participants. A group setting and the nature of a collective discussion can also reveal important non-verbal information simply through the behaviour of participants. As mentioned previously, I would also contact people individually at times to do a follow-up interview based on the nature of their participation in the group setting.

Respondents for the group discussions were selected based on the principal of ‘snowball sampling’. Snowball sampling is a non-probability sampling scheme through which one begins by (purposively) sampling one person and then, through this person, obtains a list of persons who have the same characteristics as the initial person selected and so on. For example: for the FGDs I initially selected one person who belonged to the specific group I wanted to interview – for example genocide survivors – and this person would then provide a list of names of other genocide survivors to be invited for a group discussion.

I strove to have 6 participants in a discussion group, at times there were 4 or up to 10. Given the nature of the group setting and its dynamics, I always made use of the assistance of two Rwandan collaborators when organizing group discussions: one collaborator translated the discussion in order for me to follow and facilitate it while the other recorded the statements of the respondents.<sup>49</sup> Expressions in Kinyarwanda with a specific meaning were separately recorded, discussed afterwards and compared with the translated statements. All interviews were later typed out and annotated when faced with particularities related to the translation of statements. As with *gacaca* observations, interviews and life history interviews I did not use record-

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[48] The *abunzi* are committees of locally elected inhabitants tasked with resolving local level conflicts in the population. As mentioned in the introduction, they have replaced the task of the “older” *gacaca* but have been formalized as well.

[49] During one phase of the research two of my Rwandan field assistants organized group discussions without my presence. In that case one researcher facilitated the group discussion and a second one recorded it. These group discussions were organized in 2009 and are discussed in the following sub-section.

ing devices since respondents are not familiarized with them and they might arouse suspicion and possibly a reservation (in response).

**Table 6. Overview identity respondents group discussions – 2006-2007**

		N
inyangamugayo	groups	12
	respondents	76
released prisoners	groups	6
	respondents	33
survivors	groups	14
	respondents	81
no prison <sup>1</sup>	groups	15
	respondents	81
youth	groups	11
	respondents	68
members abunzi/ubudehe <sup>2</sup> committees	groups	10
	respondents	52
total	groups	57
	respondents	391

1 In 2006 with the information collection phase in a final stage and the judges preparing files, it was impossible to identify people accused in *gacaca* with certainty. Such an identification became possible in a later stage and was used from 2007 onwards, e.g. during the life story interviews.

2 *Ubudehe* is a decentralized community development scheme.

While group discussions were mainly organized sporadically, whenever I deemed it necessary or opportune, I made abundant, even systematic use of a group discussion at two specific moments in the research cycle. First in 2006<sup>50</sup> during the exploratory phase of the research activities where I mainly organized group discussions with participants belonging to the social categories evoked earlier: freed prisoners, accused in *gacaca*, genocide survivors but also *inyangamugayo*, *abunzi* or youth not implicated in the genocide (table 6.). Being aware of prevailing gender norms, I always organised separate discussions for men and women (despite a few exceptions due to particular circumstances). I used a semi-structured interview guide with open-ended questions to solicit views on the ongoing *gacaca* process and its contextual environment, the socio-political context at the national and local level. Table 6. provides an overview of these discussions.

Secondly I systematically used group discussion in 2009 to explore the impact of the *gacaca* activities that had been going on for several years at that point.<sup>51</sup> The following themes were systematically explored in 2009: the nature of life at the local level, the nature of

[50] A limited number of group discussions (12) were organized in 2007 while primarily doing life story interviewing at that time.

[51] The fieldwork activities in 2009 when group discussions were used generating an insight into the impact of the *gacaca* process took place in the context of the evaluation of a reconciliation grassroots programme implemented by an NGO. The research instruments were designed to assess the implementation, functioning and impact of the programme. However, the research was designed not only to focus on the programme sites and activities as such but also on non-programme sites as well as the broader social tissue. In doing so, the research activities established an insight in the texture of social life in general and captured the type and incidence of conflicts since one of the objectives of the programme was the foster conflict resolution at the local level. Although the research activities were thus taking place in the context of a programme evaluation, due to the design of the research activities it was possible to use the findings to gain an insight into social processes at work in Rwandan society at large, irrespective of the functioning of the programme. The author would like to thank La Benevolencija Humanitarian Tools Foundation for the financial support to carry out these fieldwork activities. The findings and interpretations of these data do not necessarily reflect the point of view or the opinions of La Benevolencija or its employees. More information on this evaluation and the research methodology can be found in: Ingelaere, B., Havugimana J.-B., Ndushabandi, S. (2009).

conflicts and their resolution, the nature of and changes in the level of social cohesion.<sup>52</sup> Also here the discussions were systematically and verbatim recorded during the sessions. At a later stage a code book was constructed and the interviews were systematically coded based on a number of variables. This procedure allowed for a quantification/structuration of a selection of the qualitative data. I had not adopted such a structuration for the group discussions conducted in 2006-2007.

**Table 7. Overview identity respondents group discussions – 2009 impact study**

		N
authorities & opinion leaders	<i>groups</i>	14
	respondents	73
released prisoners	<i>groups</i>	9
	respondents	44
survivors	<i>groups</i>	10
	respondents	58
destitute & problematical	<i>groups</i>	11
	respondents	64
general population	<i>groups</i>	10
	respondents	59
total	<i>groups</i>	54
	respondents	298

Table 7. gives an overview of the groups selected for discussions during the 2009 impact study. Five categories were established. Authorities and opinion leaders included locally elected or appointed authority figures as well as teachers, priests, heads of associations, influential merchants etc. The group of released prisoners contained people that had been incarcerated for some years on the presumption of participation in the genocide. Survivors are people that survived the 1994 genocide. The destitute and problematical contained local inhabitants generally considered as extremely poor and with particular social problems functioning as a counterpoint to the group of authorities and opinion leaders generally considered to be the local elite. A last group was labelled as “general population”, containing any other person not belonging to one of the other categories.

#### 5-4- Questionnaires

During the exploratory phase of the research activities in 2006 I also made use of questionnaires in order to verify the feasibility and usefulness of survey research and, evidently, to thicken the empirical record already established through the other research instruments deployed at the time. The questionnaires were probing perceptions and changes in perceptions on topical issues underlying the experience and perception of transition and the unfolding of the *gacaca* activities. Following the principle of *diachrony* underlying the study, these issues were focusing on changes over a period of 15 years. The questionnaires also probed the factors that helped or hindered socio-political mobility of individuals and the social interactions between individuals within the larger context of their households and community in the margins of the *gacaca* activity. In total I collected 160 questionnaires distributed to 16 respondents in 10 purposively selected sectors. The respondents were randomly selected by using lists of households

[52] In addition, since these discussions were also focused on understanding the impact of a particular programme as explained in the previous note, questions regarding the experience and perception of the implementation and overall organisation of the programme activities and their content were asked in the sites where the programme operated.

per cell. As stated previously, 2006 was an exploratory phase in the research cycle. The experience of doing survey research on opinions and budgetary limits made me decide to rely more on other research techniques. I demonstrated elsewhere the problems with doing research in Rwanda, i.e. the influence of the *aesthetics of progress* and the *ethics of dissimulation* in an authoritarian environment (Ingelaere 2010). An opinion survey is not the best tool to be used, although the results do not necessarily need to be discarded. Instead, I resorted to the life story interviewing since such an approach was more indirect.

Nevertheless, during the second wave of the life story interviewing (2011) I added a very brief questionnaire to the life story interviewing format. I considered a questionnaire a convenient way to gather truncated information that could easily be coded (quantified). Every respondent interviewed during the life story survey – and already interviewed in 2007 since it was a panel survey – was also asked a very limited number of questions on the appraisal of the *gacaca* courts as well as the type and incidence of conflicts experienced over a longer period of time. These questions were asked in an effort to follow up on the research conducted in 2009. This time the questions were asked of individuals whereas they had been posed in a group setting in 2009. At this stage, I also used the incidence and type of conflicts experienced at the individual level (compared to the community level in 2011) to assess the impact of the *gacaca* process. The approach is thus similar but not identical; the information is complementary. The selection procedure and the demographics of respondents is similar to life story survey characteristics described in section 4.3. in this paper and presented in table 5..

### 5-5- Observation

Observation is a final research technique that needs to be mentioned. Although it does not need extensive discussion since it is closely associated with the notion of *immersion* described extensively in the first section of this paper. The continuous *gacaca* observation activities as discussed in detail previously constituted the bulk of the observational activity. Observation needs to be mentioned, nevertheless, since *immersion* referred primarily to participant observation in the Rwandan environment as such and the *gacaca* observations were evidently an activity focused on one institution. (Participant) observation as a research technique also allowed for an understanding of life in the particular community by simply being there. Non-verbal communication and observation gave additional information that enabled to *contextualize* the data collected through other techniques and it provided important information and insights as such. The interactions with the military commander described in the section on *immersion* (2.1.1.) in this paper are illustrations of the nature of these types of observations and how they were recorded. There are multiple ways to record and present field notes (Van Maanen 1988; Emerson et al 1995). I had no particular strategy for recording observations and interactions in the field. Sometimes I made notes in a diary every day, sometimes I did not write anything for many consecutive days (but was using other techniques): writing notes was mostly resulting from a particular observation or interaction that had struck me. For example: the observation of the interactions between Sévérine and the killer of her children evoked in the introduction.

In addition to my own unstructured writing of field notes, I asked my Rwandan research assistants to write a comprehensive observation report when residing in the field sites to observe *gacaca* hearings. I asked them to do so when I was not accompanying them in the field (as discussed above). Apart from providing me with the trial minutes of the day, they were also instructed to give me field notes based on their experiences that day in that location, the ongoing dynamics, any important interactions they had with the inhabitants or anyone else (state

officials, other observers, ....) and to identify any important changes compared to the previous visit. I used these reports to contextualize the trial minutes. They were particularly useful to understand the localized practices shaping the *gacaca* process (see for example Ingelaere 2009c).

## 6. CONCLUSION

The overview of the fieldwork, method and data showed how I casted the net wide and deep in order to understand the research topic. The research principles – *immersion, iteration, breadth-depth, mixing methods* and *diachrony* – aimed at establishing a comprehensive empirical record of the *gacaca* process and the experience of transition in Rwanda.

In conclusion an overview of the formal record of individuals contacted and interviews conducted as well as *gacaca* observations is presented in table 8.. “Formal” refers to the fact that I can produce a written and electronic version of the interview / questionnaire / minutes / observation report and to the recorded time, date, place and identity of interviewees<sup>53</sup> or to the information on place, date, time and nature of the *gacaca* proceedings or practice observed.

**Table 8. Formal record of individuals contacted/interviews conducted /observation (2006-2011)<sup>54</sup>**

Interviews		
Instrument	Individuals	Interviews
Life History Interview	469	846
Group Discussion	689	111
Individual Interview	77	77
Questionnaire	160	537
Informal Conversation	N/A	N/A
Total	1359	1571
Observations		
	Trials	Individuals
Gacaca Observations	1917 <sup>1</sup>	2573
Other Observations		N/A

<sup>1</sup> As mentioned, a differentiation needs to be made between trial hearings recorded as “complete”, “almost complete” or as “verdict” as explained in the section dealing with the *gacaca* observations. Publications that make use of these data use pseudonyms for respondents and localities in order to guarantee anonymity. Data can be consulted upon request (although issues of anonymity and the protection of respondents will guide data release).

This overview on fieldwork, method and data also revealed important characteristics regarding the ‘identity’ of this study. This identity has its consequences in terms of knowledge and validity claims. Firstly, the mixed methods approach and ‘*phronesis-like*’ character of this study – two issues intimately connected as stated by Schram (2012: 24) – shape the identity of this study. As stipulated: it means that the research question or topic is central to the study. The approach is primarily problem- and data-driven. Furthermore: the design of the study facilitates bringing and keeping into focus context, power and the dynamics of structure and agency. Also, it means that the study is non-paradigmatic (Schram 2012: 24).

This study does not claim to be an ‘ethnography’, let alone a self-reflexive one or one occupied with the poetics of writing as tends to be the case in the postmodernist paradigm. Nevertheless, the underlying research strategy is mainly inductive and pays significant attention to the emergence of *emic* conceptions. Nor is this study a ‘classic’ statistical-comparative work in the sociological or political science tradition. An anti-foundational as well as an anti-relativist position underlies the design and ‘logic’ of this study. And this both regarding the position of the researcher in the research process as well as the ‘nature’ of the object under

[53] Interviewees had the option to participate on the condition of anonymity in all of the interview formats. In that case only demographic information was recorded.

[54] I do not take into account a number of group discussions organized to understand the nature of socio-economic mobility over longer periods of time since outside the scope of this study. These findings will be discussed elsewhere.



study. The situational position of the researcher is inevitable but should not be denied (positivist attitude), neither should it be exalted (subjectivist attitude) (Olivier de Sardan 2008: 97). It is important in that respect that the notion 'reference reality' evoked in this paper does not refer to an objective truth to be discovered with the 'correct' method and techniques. Rather, 'reference reality' is referring to the research topic under investigation. This 'reference reality' under study is, as discussed, considered to be single and plural, subjective and objective. The research approach discussed in this paper is attuned to generate insight in these multiple dimensions. Moreover, this paper guarantees transparency and insight on the nature of the research process and the reflective strategies adopted during this process. In doing so, this paper engages with the important notion of *trustworthiness* guiding the assessment of research activities as explained in the introduction. As a consequence, it is needed to suspend a number of the reigning conventions that guide other research perspectives and paradigms in order to take the validity and knowledge claims made in and by this study serious.<sup>55</sup>

More in particular does this imply from a statistical-comparative point of view – an approach that is often (tacitly) foundational – that this study is primarily based on a number of (carefully selected) extended case-studies. The knowledge emerging from this study on the Rwandan transition does not claim statistical representativeness or validity for the *gacaca* process or the Rwandan population as a whole.<sup>56</sup> Similarly, a methodological reflection on narrative analysis in the context of Truth Commissions argues for strong method – an argument I underscore – and “systematic evidence that obeys rules of inquiry relating to the fairness of a sample population from which narratives are drawn. The greater representiveness [sic] of such a sample, the greater the ability to make generalizations, and when combined with the expert’s [researcher’s] additional capacity for intuitive and holistic interpretations, it seems to me the fuller the account that is provided” (Landmann 2012: 36-37). The language adopted in the previous quote – ‘fairness’, ‘rules of inquiry’, ‘greater representiveness’ – reveals the need to alter some conventions in assessing and understanding the validity claims made by qualitative, mixed method and *phronesis*-like research.<sup>57</sup> I have referred to the fact that, for example, a life story approach solicits an evaluation mode that pays more attention to ‘completeness’, ‘carefulness’, ‘veracity’ or ‘plausibility’ over statistical validity, representativeness or generalizability. These rules of inquiry evidently also imply respect for a number of reigning conventions attached to instruments and analysis adopted from ‘other’ paradigms.

Similarly, but from the vantage point of an ethnographic or anthropological inquiry – an approach that tends to be postmodernist and relativist at times – , it is important to accept a move beyond the mere self-reflection of the researcher as well as the move beyond the single site or case. Single or limited cases or self-reflection can be rich but not necessarily relevant and insightful. Second, it is simply wrong to assume that the numeric would not be part or relevant for ethnography or anthropology. In fact, there is an entire anthropological tradition – especially originating in the ‘Manchester school’ – built on inquiry attempting to construct comprehensive empirical records using extended cases and valuing ‘quantification’ (Epstein 1969).

Lastly, the knowledge and validity claims resulting from this *phronesis*-like approach imply that the findings and interpretations presented in this study are “no final truth” (Flyvbjerg 2001: 139). That would be a foundational position that is, as mentioned, rejected. Ingelaere (2010a) already analyzed the production of knowledge in and on Rwanda. And Ingelaere (2012a)

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[55] See in that respect the debate between Laitin, Flyvbjerg and other commentators in Schram and Caterino (2006).

[56] This does not exclude that they are ‘statistically’ representative or valid at another level, namely the sector or village level.

[57] See also the remarks by Olivier de Sardan (2008: 96-97) on representativeness.

focused on the construction of representations on *gacaca*. This study and the research activities underlying this study are not external to the knowledge processes and representational strategies that were scrutinized in these articles. In fact, the awareness of being situated in knowledge-producing power relations (and power-producing knowledge relations) in the field, the academic community and society as such informed the *phronesis*-like stance adopted and the research principles and techniques that flowed from it. It solicited an overall “heightened awareness in data collection” (Flyvbjerg 2001: 158). On the other hand this does not mean that the interpretation presented here is only as valid as any other interpretation of the *gacaca* process and Rwanda’s transition. That would imply a relativist stance that is equally rejected. I consider the interpretation and representations emerging from this study on the Rwandan transition a necessary piece of the puzzle. I make such a claim due to the research approach and principles discussed in this paper and the fact that I documented this research process here. The latter is rarely done but paramount to assess the extent of the empirical basis, the technicalities of data-gathering and treatment, decisions made to deal with ‘practical’ obstacles during the course of fieldwork and an explicit discussion of nature of the reflective process on values at work in the research process. It does not imply that the interpretations and representations emerging from this study cannot and will not be complemented or challenged by other knowledge and validity claims.<sup>58</sup>

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[58] Flyvbjerg (2001: 130–131) states: “As regards validity, phronetic research is based on interpretation and is open for testing in relation to other interpretations and other research. But one interpretation is not just as good as another, which would be the case for relativism. Every interpretation must be built upon claims of validity, and the procedures ensuring validity are as demanding for phronetic research as for any other activity in the social and political sciences. [...] If a better interpretation demonstrates the previous interpretation to be “merely” interpretation, this new interpretation remains valid until another, still better interpretation is produced which can reduce the previous interpretation to “merely” interpretation. [...] The procedure describes not an interpretive or relativistic approach. Rather, it sets forth the basic ground rules for any social or political inquiry, inasmuch as social science and philosophy have not yet identified criteria by which an ultimate interpretation and a final grounding of values and facts can be made.”

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