Anthropology and Source Criticism: The Cannibal Controversy Revisited
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Abstract: This paper focuses on the continued wrong use of source criticism in a famous controversy about the reality of the customary practice of cannibalism. A brief survey of the anthropological literature shows the existence of very different attitudes towards source criticism, a fundamental tool for historians and for anyone dealing with historical material. By taking into account the old and deep-rooted Western fascination for cannibalism, I suggest critical standards adapted to the specificities of cannibalism-related material, while common mistakes in its treatment are highlighted. A case-study – Forsyth’s famous paper about Jesuit sources and the cannibalism of the Brazilian Tupinamba, published in the Journal of Anthropological Research in 1983 – demonstrates how proper source criticism can fundamentally change the potential readings of any ethnographic material.

Keywords: Cannibalism; Source Criticism; Scientific Controversy; William Arens; Tupinamba; Donald Forsyth.

In three recent books, the French anthropologist Georges Guille-Escuret undertook a particularly ambitious task: he intended to provide a global picture of the practice of
cannibalism in the world, in what he calls a 'comparative sociology of cannibalism' (2010, 2012 & 2013). For one unfamiliar with the study of cannibalism, it may seem surprising that such a wide scientific approach to the theme appears nearly unheard of in modern anthropology. Such is indeed the paradoxical status of cannibalism in our time: there is a striking contrast between, on the one hand, the popularity of the theme in the mass media and its astonishing ability to boost the sales of books and films, and, on the other hand, the unease of the scientific community with multiple and non-exclusive aspects of anthropophagy (or man-eating): its repulsiveness, its elusiveness, its socio-political implications for the descendants of supposed former cannibals, its absurdity in Western eyes, and, last but not least, the lack of credibility of a disturbing proportion of the testimonies upon which the 'serious' discourse about 'exotic' cannibalism is – or has to be – based.

This last difficulty ended up being at the centre of a heated debate about the reality of customary cannibalism. William Arens was the one who started the controversy in 1979, in his The Man-Eating Myth – Anthropology and Anthropophagy:

'Excluding survival conditions, I have been unable to uncover adequate documentation of cannibalism as a custom in any form for any society. Rumours, suspicions, fears and accusations abound, but no satisfactory first-hand accounts. Learned essays by professionals are unending, but the sustaining ethnography is lacking. (...) This study (...) suggests that for layman and scholar alike the idea of cannibalism exists prior to and thus independent of the evidence' (1979: 21-22).

The book, rightly criticized by many for its obvious shortcomings, nevertheless changed the way cannibalism was studied by anthropologists. It had become more than ever impossible to overlook the question of the reliability of the ethnographic material associated with the theme, be it to dismiss the problem as a whole or to address it in one way or another.
Source criticism – the fundamental methodological tool of the historian, which allows the establishing of the level of reliability of a particular document – should have been at the heart of the debate. Quite surprisingly, however, one will rarely find the words 'source criticism' in any of the anthropological works involved.

This absence is terminological as well as methodological. From an outsider’s point of view, it is rather unexpected, since most of the ethnographic material related to cannibalism that is currently available to us consists precisely of 'historical' testimonies, ranging mostly from the sixteenth to the mid-twentieth century. Indeed, cannibals have the unpleasant habit of usually 'disappearing' before professionally trained ethnographers can reach them – something that also needs to be taken into account when reading older ethnographic material. Some have already questioned the unfamiliarity of many anthropologists with source criticism (see for example Traimond 2000), but this problem assumes a particular significance in a debate where studies ignoring the rules of source criticism nonetheless use historical material to defend their point: the reality of cannibal customs. As for me, I would like to show how flawed reasoning has often been applied to the material and how it contributed to shape baseless understandings of past cannibal practices.

This paper intends to provide first a brief overview of the current state of the anthropological literature on cannibalism, based on its varying interest in source criticism. This will be followed by a contextualization of the Western attitude towards the theme before and during the time of production of the earliest testimonies. A better understanding of the context of production of the historical material will subsequently allow us to set the critical standards best adapted to cannibalism-related material. Proper source criticism will then be put to the test using in particular a famous Brazilian case-study.
POST-ARENS ANTHROPOLOGY OF CANNIBALISM: A BRIEF OVERVIEW

The Man-Eating Myth was welcomed with some moderate praise and much criticism – well-deserved and doubtless not unexpected by the author himself. Its legacy is nonetheless indisputable: by the time the book was published, anthropological studies devoted to cannibalism were still far from common, and the new debate it generated turned out to be the much needed trigger for a renewal of scientific research on the subject (see for example Lindenbaum 2004). However, the reactions and the new, original approaches went in many different directions, a fact which discourages many non-anthropologists who would like to go beyond the obvious Arens reference. For the sake of clarity, I think it is possible to identify in this scientific production a few dominant approaches to the question of cannibalism, each characterized, among other things, by different attitudes towards source criticism:

1. Some studies focus first and foremost on interpretation and symbolism. Marshall Sahlins exemplifies this with a brilliant paper about Fijian cannibalism (1983): sources are restricted to their status as purveyors of raw information, while their number and similarities are implicitly deemed sufficient in order to establish a decent level of reliability. Cannibalism is seen as cultural and therefore must be studied in its wider cultural context. Whether cannibalism is actually practised tends here to be considered an irrelevant question: meanings and understanding are what matter. Despite the indisputable interest of that focus, this lack of concern for factual reality itself seriously weakens the conclusions reached. In Merrilee H. Salmon’s words:

‘Clearly, much can be learned about the culture by trying to understand its hopes and fears on their own terms. Yet by denying the importance of the factual question, anthropologists overlook a crucial consideration for grasping the social reality they seek to understand. Evans-Pritchard, for example, knew
that witches, in the sense of persons with supernatural abilities to cause harm to others, did not exist. This knowledge significantly shaped his understanding of Zande accusations of witchcraft and their explanations involving the activity of witches. If anthropologists who study cannibalism talk do not know whether or not cannibalism occurs, their ability to understand the meaning of the talk is severely limited’ (Salmon 2000: 205).

The lack of consideration for the type and quality of the evidence involved in the intellectual process of interpretation has become highly problematic in some cases. Observed facts, myths, second-hand information, indigenous testimonies and *a posteriori* reconstructions tend to be mixed on apparently equal terms in very conceptual approaches (for example Erikson 1986; Gillison 1983; Zubrinich 1999). In their quest for a deeper understanding of cannibalism as a cultural trait, some authors thus tend to deprive their readers of the basic elements required to assess the value of their reasoning.

2. Sceptical approaches, on the contrary, aim to assess the reliability of particular sets of ethnographic material. They are about the deconstruction of testimonies and the identification of inconsistencies. Rather balanced studies (for example Archer 1980) have more radical, nearly hyper-critical counterparts (for example Pickering 1999), but all of them share some kind of focus on source criticism. Their weight in the controversy is nevertheless quite insignificant, first because of their rarity, then because they only contribute to a very local level of the controversy: indeed, any Arens-like global generalization appears unreasonable from a critical point of view.

3. Postcolonial approaches, for their part, tend to postulate that cannibalism is fundamentally a discursive tool used in the interactions between Westerners (most of the time) and natives (for example Banivanua-Mar 2010; Hulme 1998; Kirkaldy 2004 & 2005). The cannibal controversy emerged at the same time as post-colonialism, both focusing on the
potential gap between Western discourses and native realities. Yet, source criticism is often overlooked by postcolonial scholars, even if there are famous exceptions (see Obeyesekere 2005). They tend to consider that their concerns lie in matters disconnected from the factual reality of native life – the actual practice of cannibalism included. Problems of reliability are therefore mostly ignored, while in fact ethnographic discourses about cannibalism are here treated as largely unreliable material in itself – but often without applying the methods to prove it.

4. The next category of anthropological works dedicated to cannibalism that I want to highlight here is defined more by its objects than by the theoretical stances adopted by the authors. Indeed, a significant part of that scientific literature deals with cannibalism as part of beliefs about sorcery (for example Baum 2004; Behrend 2007; Strathern 1982). Sometimes sorcerers are said to be eaten, but most of the time they are the invisible cannibals threatening human society. In that case, the object itself – sorcery – reduces the need for a debate about the reality of cannibalism: anthropologists are generally convinced beforehand that actual sorcerers do not exist and non-existent beings normally do not eat people. Yet, these beliefs are deeply rooted as truths in many cultures and they shape views and discourses about cannibalism: there is no doubt that an imperfect understanding of these beliefs on the part of Western observers and others – sometimes prompted by the natives themselves for strategic or political reasons (Baum 2004) – has generated many accounts of actual cannibalism, which still need to be submitted to careful source criticism.

5. Finally – and this is my main focus here, because this is where the use of source criticism should be crucial – there is a specific 'anti-Arens' literature, whose objective, mainly or at least in part, is to prove that Arens was wrong (for example Abler 1980; Brantlinger 2006; Forsyth 1983; Gardner 1999; Obermeier 2001; Wendt 1989). The strategy is simple: ethnographic testimonies related to a supposedly cannibal human group are assembled and
questioned about their reliability. If that material is able to pass the test, the practice of customary cannibalism is demonstrated and Arens is at the same time proven wrong. While excessive critical expectations are the weak point of some sceptical approaches, critical laxity and fundamental misuses of source criticism are an all too common characteristic of these 'anti-Arens' approaches.

This categorization is undoubtedly incomplete and cannot encompass the whole anthropological literature devoted to cannibalism. It has been conceived only has a practical tool in order to provide rough representations of the dominant research trends currently available to the student interested in the subject, with source criticism in mind. In the next pages, I will have the opportunity to mention some works much more balanced in their use of that methodology, but the fact remains that most anthropological studies dedicated to cannibalism either ignore source criticism or misuse it. This is quite troubling when we remember that the whole debate about the reality of customary cannibalism was supposed to revolve around the matter of the reliability of non-professional ethnographic testimonies – and therefore precisely around source criticism, the only method available in order to deal with that kind of problem.

It is also crucial to keep in mind that this controversy is far from being only about technical disagreements about the handling of sources. Pro- and anti-Arens accuse each other of being ethnocentric, the first because they supposedly refuse to accept the reality of native customs inconceivable to the Western mind and the latter because, on the contrary, they supposedly give too much credit to prejudiced Western testimonies about native customs. This ideological tension probably contributed to the production of studies built around their expected conclusions more than around testimonies and the coherent method that could assess their reliability.
These very modern difficulties illustrate the particular place of cannibalism in the Western realities and imagination: there is an age-old – and complex – Western relationship with man-eating that needs to be taken into account when handling historical ethnographic material. This is why I think it is essential to devote some space here to a very brief outline of the context of production of early Western testimonies about cannibalism.

A WESTERN CONTEXT FOR CANNIBALISM

The history of cannibalism in the West has attracted some much needed attention in the last decades. Of particular interest for my purpose here are studies devoted to the European Middle Ages and Renaissance (for example Blurton 2007; Lestringant 1997; Nagy 2009; Niayesh 2009; Vandenberg). There, indeed, is the cradle where the preconceptions and prejudices of the early explorers of the New World were born, and the place where their testimonies about cannibalism were published, read and often written.

Man-eating is a multi-layered reality for a man of the late fifteenth century, a few years before the word 'cannibal' itself was invented. Survival cannibalism during severe food shortages has often been mentioned during the Middle Ages and later. In addition to the limited reality of this desperate food choice, rumours and tales of man-eating abound in times of famine, and biblical as well as literary models are numerous and well-known (Vandenberg). Then there is the Eucharist: the eating, during mass, of the true flesh and blood of Jesus Christ, not metaphorically but literally: subtle theology and vivid miracle stories of bleeding hosts and bread turning to flesh were there to remind the faithful that what he consumed was really a divine morsel. What’s more, Protestant polemists did not hesitate to label Catholics as cannibals because of this dogma (Lestringant 1997). Medicinal cannibalism
has benefited of much attention lately: body parts or fluids are ingested as medicine (for example Gordon-Grube 1988; Noble 2011). Mummy parts, in particular, were used as a kind of panacea in Europe well into the nineteenth century. Above all, man-eaters abound in myths, legends and in the Latin and vernacular literature (for example Blurton 2007): giants, ogres, English kings, tricked adulterous wives, crusaders, witches and many other characters filling the European imagination are sometimes said to eat humans in whole or in part.

There would be much more to say about it, but let’s leave it at that for now. It must therefore be taken into account that man-eating is a recurrent, sometimes almost obsessive, theme in medieval and early modern Europe. Cannibalism has many facets and it is never welcomed with indifference; it is associated with violent emotions: fear, disgust, hate. It challenges fundamental identity boundaries. But for our early modern travellers, man-eaters were more than this: the medieval ethnographic tradition had been constantly repeating that they were waiting on the margins of the world map. This tradition had been inherited from Ancient Greece, then reworked during the Middle Ages and was still widely circulated and influential even after it was largely proven wrong by the new discoveries. It is fundamental to take this into account when we try to understand the relationship between early modern European travellers and cannibalism.

Herodotus, in the fifth century B.C., already described a world where a string of cannibal people, some more gruesome than others, occupy the outer ring of the known world. Cannibalism, first among other customs, was depicted as a perfect inversion of Greek sociability, and therefore found its place in the most remote parts of the earth: the Far North, the Far East and the Far South (see for example Hartog 1980). After Herodotus, the fate of cannibals did not change much for nearly two thousand years: they were only pushed back a bit farther by library ethnographers to the North-East of Asia, but you could still see them,
with the same names, on late medieval illustrated world maps, chewing on human limbs (Vandenberg).

The fourteenth century marks a crucial turning point in the history of the Western representation of the cannibal. This period saw European travellers reach India, Southeast Asia and China. The written records of their adventures, mixed with centuries of traditional ethnographic knowledge (monsters and marvels included), form the bulk of the most successful 'geographical' work of the late Middle Ages: the *Travels* of the library ethnographer John Mandeville (see Deluz 1988; Higgins 2011). His work remained until the early seventeenth century an unrivalled authority (much more than Marco Polo or any authentic late medieval travellers to the East ever were).

The man-eaters of John Mandeville possess some fundamentally new characteristics compared to the traditional ones. Forget the cold and inhospitable landscapes of North-East Asia and their cannibal nomads, the new man-eaters dwell in the most remote regions of Southeast Asia, where they go naked under the tropical sun (which had been for a long time the traditional Arab view of the cannibal, see for example Tibbetts 1979). Mandeville mostly borrowed the idea from the tales of real travellers, but expanded this far beyond the realm of reality, inventing numerous islands farther East where he relocated some of the traditional cannibal customs going back to Herodotus – old customs which could no longer find their place in the North. This is ethnographic imagination on the move: man-eaters are maintained in their role as guardians of the last frontiers of humanity, wherever these frontiers are.

This brings us to the late fifteenth and sixteenth centuries and to the birth of the New World ethnographic record. When Columbus left for his first journey West, he did not believe he was leaving for the unknown: he had read Mandeville and others and was fairly sure he would reach Asia, the land of the Great Khan, and possibly before that a few islands where people with strange customs lived (see for example Gil 1989). Finally reaching a place where,
following imaginative reference books, naked man-eaters should have lived on warm and lush islands, the brave western travellers found some. Such a coincidence, while not impossible, is quite exceptional (giants, Amazons and the like were also found on occasions, and only disappeared quite late from the records). But coincidences like this one make the historian wary and force him to raise his expectations in terms of source criticism. And, in general, the western sensitivity to cannibalism – as a practice unvaryingly marking the boundary between civilization and inhumanity, between known and unknown – forces us to be particularly cautious when dealing with western testimonies about the subject. This is why critical standards applied to the ethnography of cannibalism definitely have to be rather high.

CANNIBALISM AND SOURCE CRITICISM: THEORY AND CASE-STUDY

History, as a discipline, over time underwent profound changes in its methods and aims. At the same time, a few general principles of source criticism were firmly preserved as the basis of any documentary analysis (reference works are numerous, one of their main models being Langlois & Seignobos 1898. See for example Howell & Prevenier 2001). We should see how these principles can adapt to the cannibal question.

Source criticism starts with what can be called 'external criticism': when was the source produced? Where? By whom? What were the source’s sources? Is the source preserved in its original form? We need to focus here on the author, on the production of the source and, ultimately, on its general authenticity. A few principles seem obvious: a library ethnographer is normally less reliable than a field ethnographer; field notes are normally more reliable than a published – and therefore reworked and reorganized – work; a spontaneous and contemporary letter has more value than a collection of memoirs. A good source should be
close, spatially and chronologically, to the events it recounts; the same goes for the author, who should be clearly identified.

Some aspects of external criticism overlap with what we’ll call 'internal criticism': what is inside the document? What precisely is said by the author and what did he mean? How knowledgeable is the author? Is he a competent witness, considering the context? Is he an eyewitness or is he rather borrowing information? Is he fully sincere or would it be in his interest to distort reality? Do the contents themselves seem fully coherent or could the author have made mistakes? Is there any way to confront the testimony with other independent sources? Internal criticism frequently turns out to be trickier than external criticism. Reading an historical document properly, elucidating its exact contents and identifying its potential biases: it all requires patience and caution. This has been the most difficult issue for many anthropologists who specifically took on the task of proving that Arens was wrong, as we shall see.

Difficulties specific to the cannibalism-related material are indeed numerous. First, there is never such a thing as a perfectly neutral author: this is particularly true for cannibalism, considering the burdensome historical and cultural context stated above as well as the long-standing sensitivity to the question. Therefore, if external criticism is obviously essential, internal criticism is even more fundamental in this case because a reliable author and his source may at times be unreliable, in particular when dealing with sensitive matters. This is often misinterpreted: we should remember that a source should not necessarily be accepted or rejected as a whole. Doubting the reliability of an author’s lines devoted to cannibalism does not mean this author is globally treated as an outrageous liar (some 'anti-Arens' authors wrongly left their readers with this absurd outcome: see for example Forsyth 1983: 172). An author may lie, of course, but most authors just have their own selective take on reality and their own understanding of their responsibility towards objectivity. It would be
anachronistic to expect a constant concern for objectivity on the part of an early modern (or even later) ethnographer.

Another characteristic of cannibalism is that there is an incredible number of ethnographic sources mentioning it – and this is paradoxically problematic. From the strict point of view of source criticism, this should not really change anything. This is indeed neither good nor bad: this is just another element of the wider context of a given document. And source criticism is only interested, at first, in individual sources. The only effect numbers may have on the evaluation process is to raise, once again, critical standards: when a considerable number of witnesses appear, it may be expected to find among them at least some very reliable eyewitnesses offering detailed accounts. The problem, for an historian, is precisely that this does not seem to work that well for cannibalism. But anthropologists commonly adopted another point of view, as the examples of Volhard and Evans-Pritchard will show.

*Kannibalismus* is a book published in 1939 by the German ethnologist Ewald Volhard, and it is still unparalleled today for the broad scope of its study. It also remains, despite its age, a good model of the gap between the usual anthropological treatment of the abundance of cannibalism-related sources and orthodox source criticism. For Volhard, the fascination exerted by cannibalism on travellers has mostly positive consequences: following him, it ensures that not a single case of customary cannibalism can be overlooked, since any person getting any kind of information about it anywhere in the world would feel the urge to report it (which he describes as 'ein gewaltiger Vorteil anderen Kulturerscheinungen gegenüber'; 1939: 368). But he also emphasizes the fact that, in his view, fear and disgust tend to prevent people from giving a detailed account of the practice, making it difficult to grasp meanings and motives.
Nevertheless, Volhard is also fully aware of the weaknesses of his abundant material: detailed accounts and eyewitnesses are lacking, the influence of the Western imagination is obvious and the fact that indigenous testimonies are sometimes conceived mostly to please those asking the questions is a matter of concern. Testimonies, he says, tend to be inaccurate and the best of them are not always reliable (‘Aber auch diese wenigen sind keineswegs immer zuverlässig’; 1939: 368). Volhard’s worries, however, only apply to the anthropologist’s ability to establish the meanings and motives behind the practice: the reality of the practice itself and of its extent is established without a doubt by the unending addition of its appearances in the record. The German scientist sees classification and organization of the material as the key to transcend its weaknesses and make up for the lack of information readily available for interpretation (1939: 368-370).

So, numbers first, then the amount of details and, finally, the concern for the reliability of independent sources: these are, in order of precedence, the criteria taken into consideration by Volhard for the use of the historical and ethnographic material. From the amount of material, you would be able to draw a map where all the cannibals in the world are located (Volhard 1939: 368). But, afterwards, the lack of details, and sometimes the lack of reliability – here comes finally source criticism, need to be compensated somehow by the anthropologist’s intellectual intervention. This is wrong in many ways, because source criticism is only useful when it is used on the frontline: first the whole material is criticized, and then it can be interpreted, because it is only by doing so that you can you have a good idea of its contents and reliability before doing anything else. Volhard uses à la carte source criticism, and reliability is therefore, for the most part, arbitrarily established.

The illusory weight of numbers is detrimental to a critically-informed use of sources. This is the most common mistake found in the whole literature dedicated, in one way or another, to the cannibal controversy (see below): too many authors reason as if numerous
unreliable sources could spontaneously generate a cumulated level of reliability. On the one hand, in the cannibal controversy, accumulated unreliable sources are generally used to shape an abstract reliable material (like how fractions add up in mathematics). On the other hand, source criticism, faced with the same sources but totally unaffected by their accumulation, would tend to maintain a globally low level of reliability (the mean level of the sources). It might even further lower this level because, for the sake of objectivity, unreliability could be addressed as if it was an infectious disease: the result would be more like multiplying fractions – the more unreliable sources there are, the less the whole material would seem usable.

The infamous 'no smoke without fire' rule is intimately associated with this mistaken law of accumulation. Another milestone in the history of the anthropological treatment of cannibalism will exemplify this, while also showing that the question of the reliability of the cannibalism-related material was a source of concern long before Arens released his pamphlet. In a paper first published in 1960, Edward Evans-Pritchard studied the cannibalism of the Azande (or Niam-Niam) from Central Africa. He systematically analysed the main texts supposedly establishing the reality of Zande cannibalism. None really managed to survive his careful inquiry: in his view, they rely far too much on hearsay, the authors do not know precisely which ethnic group they are referring to or do not spend enough time in the region to be of any interest. He applied the most rigorous source criticism to his whole material with great intelligence – but his conclusion comes as a surprise:

'Now, I think that no one will deny that the evidence of the travellers, each considered independently, ranges from the dubious to the worthless; but there is no smoke without fire, and taking all the evidence together we may conclude that there is a strong probability that cannibalism was practised by at any rate some Azande' (Evans-Pritchard 1960: 251).
Source criticism, in this case, was leading the anthropologist into what could seem like a dead end: as abundant as it is, an unreliable material would have to be dismissed. It would not necessarily mean that the Azande did not practise cannibalism, but it would imply that there is no convincing proof enabling one to reasonably assert that they did. This logical thought process suddenly took another direction with the help of 'common sense' in the form of the 'no smoke without fire' rule, which artificially created a new balance in the understanding of Zande cannibalism. The proof via selective accumulation was thus legitimated and Evans-Pritchard was able to establish his view of the matter at hand, which is, in short, that cannibalism had indeed been practised, but on a very limited scale, and probably not by the 'true' Azande but by foreign elements in the society (1960: 254).

Postcolonial scholars would undoubtedly have much to say about a conclusion reproducing common stereotypes about the 'foreign cannibal' – but this is not really the point here. Using the totally unscientific 'no smoke without fire' argument amounts to pure speculation. There is no point bothering to apply source criticism to the material if, in the end, only the instincts and skills of the anthropologist are deemed able to assess what is reliable and what is not. Evans-Pritchard clearly switched methods halfway through his paper, which is, once again, a form of source criticism à la carte. The basic elements of the method are applied accurately, but their consequences are denied in favour of an absurd conclusion: the material is unreliable; therefore you will have to rely solely on the anthropologist.

After devoting some attention to the essential rules of source criticism and to common uncritical reasoning in the anthropological literature, we now go back to the more technical aspects of source criticism. The best way to highlight how difficult it is to use this method for cannibalism-related material is to deconstruct a famous study dedicated to the rebuttal of Arens’s position. The Tupinamba case might be the perfect candidate for this.
The question of the reality of the cannibalism of these Brazilian natives has long been discussed, in particular on the basis of Hans Staden’s account of captivity among them, first published in German in 1557. Scholars are still engaged in an insoluble debate about the reliability of this testimony, both sides advancing reasonable arguments (Menninger 1995; Whitehead & Harbsmeier 2008). In order to get around this problem, many (see recently Duffy & Metcalf 2012) resort to a paper by Donald Forsyth (1983) supposedly establishing the reality of Tupinamba cannibalism on the basis of reliable sixteenth-century Jesuit testimonies. The significance of this particular study in the cannibal controversy should be totally dependent on the quality of its use of source criticism. However, there is much to say about it, and carefully identifying Forsyth’s mistakes could lead to a reappraisal not only of the Tupinamba case in general but also of many anthropological works sharing the same aims (proving the reality of customary cannibalism) and shortcomings.

Forsyth writes as an introduction that 'The Jesuit accounts are full of descriptions of this practice [cannibalism] (...). In view of Arens’s denial of Tupian cannibalism, the Jesuit sources are doubly significant' (1983: 150). This is a promising start, all the more so because Forsyth himself has carefully selected some extracts significant enough to be reproduced in translation in his paper: 'I have translated what I consider to be the most important passages' (1983: 150). Unfortunately, out of the twenty-five quotes available, more than ten are totally unrelated to cannibalism. The author wanted to prove that the Jesuit accounts are fundamental sources of information for other aspects of local life and customs as well. But this is confusing for a casual reader only expecting the much anticipated testimonies about cannibalism and impressed at first sight by the amount of extracts cited. Source criticism also needs to be applied to the scientific literature itself: this already leaves us with only thirteen or fourteen of the best sixteenth-century Jesuit testimonies about Brazilian cannibalism to chew on.
Forsyth tries to take into account the time each Jesuit spent there, their character and, as far as they can be identified, their prejudices. This is decent external criticism at work: the context is well taken into account and the lack of objectivity of the authors is highlighted. Nevertheless, key elements of external criticism still were forgotten along the way and, more than anything, internal criticism was ignored. It is now necessary to look at each translated extract in turn, in order to comprehend how wrong interpretation can go when the sources are not seriously questioned.

The fourteen quotations related to cannibalism come from twelve different sources produced by eight different reliable authors between 1549 and 1591: this is a substantial body of material and an acceptable chronological span for that period. External criticism then requires that we look at the kind of sources available: four are reports or treatises and eight are letters, most of them of the formal kind. Only three letters by two different authors (Anchieta and Navarro) unequivocally contain accounts of their authors’ personal experiences on the field. The aim of most of the texts making up the body of evidence assembled by Forsyth is to give people in Europe some general information about Brazilian natives and their customs, most often with a tendency to give priority to anecdotal material. Most of the time, it is impossible to establish without conjecture where the cannibalism-related information given by these sources comes from. This is a real difficulty for source criticism, but a first warning sign of that kind is not unusual with ancient material.

Internal criticism starts with questions about the nature, the quantity and the quality of the information given. This is supposed to be the best material available, so we could maybe expect a certain level of detail in these sources. Establishing what can be considered a 'substantial' level of detail in a given context can obviously be a tricky issue – that much is true. In this case, however, this task is quite easy since the authors describing Brazil to their European contemporaries in fact rarely 'describe' cannibalism. Most of the time it is only
briefly mentioned: 'They return to eat them at a great feast', 'all of them eat human flesh', 'under no circumstances do they eat human flesh' (Forsyth 1983: 155), 'all of those on the coast who have the same language eat human flesh, even though some never eat it and have a great abhorrence of it' (161), etc. These general assertions are typical of third-hand information or 'common knowledge': personal experience is imperceptible. Cannibalism being a common stereotype of otherness in the European tradition, it would be unwise to give too much credit to such testimonies lacking any reference to personal experience, whoever the author may be.

When, on the contrary, testimonies are detailed, comparisons between them could lead to a hierarchy in terms of levels of reliability. In this case, at least five different Jesuits recount a famous rite, here reduced to its most important elements: a prisoner is brought into a village, then he is fed and pampered for weeks and he can even be given a wife. Afterwards, family and allies from neighbouring villages gather for days of festivities and the prisoner is finally executed before being butchered. Some of the available sources mention cannibalism at two different stages of this process: the children that the prisoner might have with a local woman are eaten and the prisoner is himself eaten after his execution. But, if we carefully read the available accounts, some significant discrepancies appear:

1. Nobrega (1549, in a letter): the children are eaten, but no mention of the final cannibalism (Forsyth 1983: 151).
2. Nobrega (1549, in a report): final cannibalism, but no mention of the children being eaten (152).
3. Correia: final cannibalism and children eaten (the boys, always; the girls, rarely) (165).
4. Cardim: no mention of final cannibalism and no mention of the children being eaten (167).
5. Soares: final cannibalism, but no mention of the children being eaten (169).
Forsyth, unfortunately, does not seem to realize how his sources diverge in terms of contents. He even introduces Cardim’s description of the rite, which is a lengthy and incredibly detailed account of the whole process, of clothes and attitudes, as ‘one of the most detailed accounts we have of the cannibalistic rites of the Tupinamba’ (167) – and it is fascinating to note that this particular account precisely is the only one where cannibalism is not mentioned.

Moreover, these accounts, in terms of source criticism, are not on an equal standing. The two different accounts by Nobrega are just short and rough sketches mixed with other irrelevant information. Correia’s account is much more detailed, but he seems to focus on some aspects (the difference between the boys and the girls born of the prisoner) and just mentions others without any detail (final cannibalism). Cardim’s testimony is by far the most impressive: it takes two whole pages of Forsyth’s paper and is full of details. This is the only case where the last phase of the rite – the butchering – is really described. Finally, Soares, who may have read Cardim (169), adds a few original details in a much shorter account. None of these authors, alas, implies that he personally saw the whole thing happen. Yet, Cardim, even if he is not the earliest witness, is without a doubt the closest we can get to an eyewitness account since he offers detailed and coherent information often unavailable elsewhere. Of the five testimonies as they appear in Forsyth’s paper, Cardim’s should clearly have been given priority when the place of cannibalism in the Tupinamba sacrifice was questioned.

As regards the butchering, Cardim is the only one who describes precisely how the victim is cut up, how the morsels are distributed among the spectators and why some of it is cooked (in order for those coming from far away to be able to take it back home). But he does not say what is done with the flesh. If we look at Cardim’s text, there is no sign of restraint in it: the butchering as he depicts it is bloody enough. Therefore, we certainly cannot conjecture that he voluntarily avoided mentioning the ‘horror’ of cannibalism.
But is there consistency in the rite as Cardim describes it? Can this process really work if cannibalism is not there to make it whole? Hypothetically, this could be the exact question the Jesuits themselves faced, and most of them could have considered that cannibalism was a necessary ending. But maybe we can test our reading with some cross-cultural comparison: indeed, source criticism does not stop with the source itself or its immediate context; it provides a lot of general questions that, if answered, will change in return the way a source can be understood. In this case, nineteenth-century accounts of cultural practices in Burma and India contain descriptions of surprisingly similar processes (van den Bosch 2007; Means 2000); at the end of those ritual sacrifices, the executed prisoner is butchered and the morsels are distributed, like in Brazil. Some bits are even cooked for those travelling far away. But there is no cannibalism here, and the flesh is finally buried: its purpose was not to be eaten. This does not mean, of course, that the ending had to be the same in Brazil. It just proves that cannibalism is not a necessary ending for rituals of that kind and that the most detailed account for Brazil mentioned in Forsyth’s paper, which is the only one leaving out man-eating, does not suffer from any problem of internal consistency, while all the others do in one way or another.

Recently, Franz Obermeier (2001) also tried to demonstrate the reality of that 'cannibal rite'. In his attempt, careful comparisons of Portuguese, French and German material were made. Obermeier managed to demonstrate the reality of the rite itself by successfully comparing independent accounts of each phase of the process. But he did not dare – or forgot, or avoided – to compare the diverging forms, when they can be found, of the last phase of the process, where cannibalism is supposed to appear. He nonetheless concluded that his study proved that the Tupinamba were indeed cannibals – which is in fact quite surprising, to say the least, since he did not write a word about man-eating (except in the title of his paper, that is). This short digression shows how effective internal criticism can be when applied to the
modern studies themselves before being applied to their source material. It also disturbingly confirms that cannibalism can easily escape scientific rationality.

Going back to Forsyth, we have seen that, up to this point, his assemblage of sources, when subjected to source criticism, allows us to say that some Jesuits wanted to describe Brazil as a place where at least some natives practised cannibalism in varied but undetailed forms. We also noted that the Tupinamba had a particular rite involving the execution and butchering of a prisoner, the state of the sources suggesting that cannibalism, despite repeated appearances in the material, might not have been associated with that practice. This is less – by a wide margin – than what Forsyth thought he had proven already.

The eyewitness accounts could change our perception of the whole problem at hand: they should be decisive since they come from reliable authors. A Jesuit named Blasquez, for instance, describes an agreement with natives, but it is unclear if he personally witnessed it. This agreement stipulated that they could execute prisoners as long as they did not eat them (Forsyth 1983: 166); incidentally, this is a testimony about natives not practising cannibalism and about the Portuguese fearing that they might. Blasquez seems to be sure that this was a Christian victory, but it would be pure conjecture on our part to believe that the natives involved did indeed lose something: they could as well have promised not to do something they would not have done anyway.

A certain Anchieta, for his part, directly witnessed the execution of two people (a slave and a prisoner) and their subsequent butchering: 'some pierced the cut off members with sharp sticks, others smeared their hands with fat and went about smearing the faces and mouths of others, and it was such that they gathered blood in their hands and licked it, an abominable spectacle, such that they had a great slaughter on which to gorge themselves' (Forsyth 1983: 159). This is a remarkable first-hand and quite detailed account of the practice of a limited form of cannibalism: the ingestion of small amounts of human blood. This is the
kind of highly reliable testimony we were looking for. But Forsyth seems to have seen something else in it, since he introduces the quotation like this: 'One of the things [Anchieta] observed was the execution and eating of one of the Indian slaves'; once again he does not seem to have properly read the extract or maybe he made a baseless conjecture using the ambiguous last sentence.

Finally, the most personal accounts present in Forsyth’s paper all come from a Jesuit named Navarro. A first quotation is in fact a trap: Navarro recounts how, not long after his arrival in Brazil, he baptized prisoners in a village; he integrates in his account a reference to the common knowledge that prisoners of that kind were meant to be eaten, but this mention is totally out of context (Forsyth 1983: 163). This is information on the same level of reliability as those commonly given to European contemporaries (see above).

The second quotation is worthy of a lot more attention: this is Navarro’s personal experience in a village where, in his presence, people were cooking the body of a girl and other body parts. The extract must be read carefully, in order to distinguish between, on the one hand, what the natives said or did and, on the other hand, what Navarro thought about it: internal criticism is here focusing on contents and coherence. Here is the sequence of elements in Navarro’s testimony: the natives say that they have killed a girl – Navarro sees the body being cooked – he thinks they are going to eat it and wants to stop them – his interpreter does not want to translate anymore – he becomes friend with them (how?) – he goes into other houses and sees body parts being cooked in the smoke – he persuades them (how?) not to 'carry out such a great evil' – the natives say they have buried the flesh - 'and it seems to me that they reformed somewhat, at least in public one does not see them' (Forsyth 1983: 163).

This is a story, once again, about cannibalism, but without cannibalism. The natives kill a girl, cook a body and bury human flesh; Navarro fears cannibalism, fights against
cannibalism and triumphs over cannibalism; communication between the two sides depends on an unwilling interpreter. On Navarro’s own admission, cannibalism remains invisible. There is therefore no link, apart from conjectures, between the natives’ behaviour (what he saw) and Navarro’s interpretation of the situation (what he thought). This is therefore an unsatisfying testimony about cannibalism, but a good one about bodily treatments.

The third and last quotation translated from Navarro’s writings is partly a reiteration of the previous episode (Forsyth 1983: 163-164). In the original section, Navarro explains why he cannot baptize the natives:

'they are strongly rooted to [the practice] of eating human flesh, in such a manner that, when they are about to pass out of this world, they immediately ask for human flesh, saying that they have no other consolation than this, and if they don’t get it, they say that they go the most miserable of men on earth; their consolation is their vengeance. I spend most of my time censuring this vice. The answer that some give me is that only the old women eat it. Others tell me that their ancestors ate [human flesh], so that they must eat it too, that it is their custom to avenge themselves in that manner, for their enemies eat them: so why do I want to take from them their genuine delicacy?'

Internal criticism helps us realize that multiple discursive levels are mixed in this extract. The story about dying natives asking for human flesh is probably second-hand because it is followed by first-hand questions and answers showing that Navarro does in fact not really know who asks for or eats human flesh, how they do so and why. Like in previous cases, this extract is unable to prove that these Brazilian natives practised cannibalism; on the other hand, it proves that they were at least talking about it – and not without discrepancies. It also shows that Navarro strongly believed that they ate human flesh but was never able to witness it or to define it precisely.
ELUSIVE CANNIBALISM, ELUSIVE SOURCE CRITICISM

As we have seen, applying internal criticism largely amounts to carefully reading a source’s contents, without letting the reader’s expectations take the lead. Forsyth should have been more careful and should have paid attention to the texts themselves and not only to their authors. If he had done so, he would never have concluded his analysis like this: ‘considerable detail concerning the practice of killing and eating captured enemies is given in these sources’ (Forsyth 1983: 170). It is highly disturbing that these obvious weaknesses did not affect the reception of this particular work, still cited today as one of the main references proving the absurdity of Arens’s thesis.

The way Forsyth uses the 'no smoke without fire' rule is also very consequential: following him, if the Jesuits fought against cannibalism, it is necessarily because it was 'pervasive'; if not, why would they waste their time doing so (172)? This is of course highly conjectural and such reasoning could be used to demonstrate the reality of practically anything and everything. Forsyth even adds: 'if the Tupinamba did not practice cannibalism, numerous experienced and knowledgeable Jesuits lied outright (...). If we cannot believe the voluminous testimony, full of painstaking detail, that anthropophagy was indeed practised, how can we possibly believe anything these people had to say about Indian customs and behaviour (172)?'

We know of course that there is no such thing as 'painstaking detail' about the practice of cannibalism in this paper, but Forsyth was not aware of it. This happened because he did not take into account the fundamentals of source criticism and, in general, because his tools for dealing with historical material were inadequate. Furthermore, his understanding of
testimonies as either truths or lies is simplistic: both authors and sources can be reliable and unreliable at the same time, following their contexts, their aims, their internal logics. Source criticism precisely implies some fastidious but rewarding work that sheds light on the complexity of any testimony and on the wealth of information of varying quality that it contains.

Forsyth’s inventory of Jesuit testimonies allowed me to illustrate the wide range of mistakes typically made by many anthropologists dealing with ancient source material. I could extend the same disturbing analysis to most of the literature similarly aimed at the demonstration of the reality of the practice of cannibalism. American cannibals in particular have been subject to a number of unsatisfying studies of this kind (for example Abler 1980; Chambers 2007; Wendt 1989), but a short example about Fijian material will show that the problem is quite widespread. This is highly problematic because these works have contributed to support the mistaken idea that the cannibal controversy is a thing of the past, while in fact the methods used never quite adapted enough to meet renewed critical expectations.

Patrick Brantlinger published in 2006 an article focusing on what he defines as two exemplary Fijian historical sources dealing with cannibalism, both written by nineteenth-century missionaries. On a general note, he is inclined to accumulate sources without individually criticizing them and uses the ‘no smoke without fire’ rule. But his analysis is mostly based on two important testimonies which could still counterbalance these common weaknesses. Unfortunately, they only include vague second-hand accusations of cannibalism and what Brantlinger himself identifies as the empty cannibal boasting of a local king. Apart from this, these texts are full of the expectation of cannibalism or of bodily treatments that could possibly lead to an ulterior but invisible consumption. Contradictions are common, for instance between the idea that cannibalism would be a masculine and chiefly privilege, and graphic depictions of whole villages preparing collectively for a cannibal feast.
Upon reading the extracts reproduced in this article, it appears that neither of the missionaries described cannibalism or said that it happened in his presence, while both were sure it was practised. Strangely, Brantlinger, like Forsyth long before him, is not aware of this absence, and it allows him to conclude that the practice of cannibalism is a perfectly established fact thanks to these reliable eyewitnesses. This is exactly the same pattern of mistakes that we identified in the study of the Brazilian Jesuit testimonies: incomplete external criticism focusing mainly on the personality of the authors and on their proximity with the field does not suffice if sources are not read properly, if the authors’ conjectures are taken at face value, and if the existence of variable discursive levels is ignored.

A review of anthropological works dedicated to the study of cannibalism shows how complex the relationship the discipline has with source criticism is: this method is sometimes simply rejected as dangerous or pointless (see for example Gardner 1999), is often ignored or is regularly misused. The few cases where it is properly or even excessively applied do not weigh much in the literature. It is fascinating to notice that, despite Arens’s focus on sources and reliability, the related methodology – source criticism – never explicitly came to the front of the debate. Even in the few works where sources were more thoroughly studied before the probability (Isaac 2002) or reality (Conklin 2001) of cannibalism was established, one does not find a formalized view of the principles used in the treatment of the historical material.

Georges Guille-Escuret, whose ambitious 'comparative sociology of cannibalism' (2010, 2012 & 2013) was mentioned at the start of this paper, unfortunately does not stray from the methodological line established by his predecessors. He is convinced that sceptics have been focusing too much on isolated testimonies. He knows that, taken one by one, the sources are not very convincing, but this does not mean much, according to him, if you look at the bigger picture – if you look for general tendencies in the record. Absurd statistical realities thus become historical facts and the 'no smoke without fire' rule is amply used: what
is repeated most often becomes valid information. This is how rumour works but not how History or Anthropology should work. Sadly, it seems that the cannibal controversy has not yet produced its full effects on the way some anthropologists deal with their sources. We should hope that some much-needed interdisciplinarity will finally be able to renew our understanding of cannibalism as a practice, as a metaphor, as a fear, as a fantasy, as an ideological tool or as anything else it might be.

NOTES

1 - Cannibalism is here taken as synonymous with anthropophagy and man-eating. Although I would generally prefer a much wider definition of this phenomenon, it is here restricted to its most obvious aspect for the general public and for the controversy at hand: the direct consumption, in various contexts and in different forms, of human flesh by a human being.
REFERENCES


