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“In this context”:

The many histories of anthropology

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My thanks go first to Fernanda Peixoto, Heloísa Pontes and Lilia Schwarcz for the invitation to participate in the Anthropology of Anthropology Seminar, at the University of São Paulo. The subject is dear to me for many reasons, the most obvious of them being the coincidence of names, since the seminar's title is the same as the one I gave to the doctoral thesis I defended two decades ago.[1] The organizers may be running a risk by calling me into this debate, because it will be unavoidable to bring up a major part of my training in anthropology and the context in which it took place. I have divided my presentation into three parts. In the first part, I try to recover what it meant a study on the “anthropology of anthropology” in the late 1970s. In the second, I examine at least two kinds of histories in anthropology. In the third, I raise an alert about a certain blur between history and theory, using an example from anthropological literature.

I

The Anthropology of Anthropology: The Brazilian Case was a Ph.D. dissertation that I presented in the United States in 1980. It arose from a basically Durkheimian concern with inquiring into science just as anthropologists had done with religion from the discipline's beginnings. Part of my project was to ask how social scientists experienced and reproduced their *own* “system of beliefs.” As I came to perceive that they shared some relatively

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Link to this issue: http://www.unb.br/ics/dan/Serie352empdf.pdf

[1] See http://www.unb.br/ics/dan/Serie110empdf.pdf.

similar objectives and central values, I raised a series of questions. What were these values? Who were these people who became anthropologists? What was the efficacy of their knowledge? How have they reproduced socially? And, above all, how were they recognized? As in Mauss, magic always depends on the social approval that legitimizes it.

My project was quite orthodox, and inspired by classical authors. By following the tracks of social recognition, I defined the period to research and the actors involved. It was after 1930 that the social sciences in Brazil — broadly labeled as sociology — came to be seen as relevant to the country's development, and to be institutionalized as academic knowledge. This took place in São Paulo, particularly at the Universidade de São Paulo (USP), and also at the Escola Livre de Sociologia e Política. Having achieved legitimacy over a period of a few decades, a gradual process of branching out, bricolage and individualization ended up distinguishing sociology from anthropology, political science and history.

While the thesis had a Maussian orientation, I organized it under the inspiration of Roger Bastide, who proposed to attack a phenomenon from several different angles. To this end, one chapter examines the career of Florestan Fernandes from the Tupinambá to black studies to the bourgeois revolution, revealing tensions and dilemmas of a social scientist who helped forge sociology intellectually and institutionally in Brazil, suggesting patterns that are still with us today. Another chapter looks at how anthropology since the 1960s has combined its object (indigenous groups) with theory (Florestan's dialectical sociology), thus allowing Roberto Cardoso de Oliveira to coin the notion of inter-ethnic friction. This became the groundwork for the project of a "sociology of indigenous Brazil." From there, the incorporation of peasant issues — i.e., the regional populations making contact — was just a step away. The last chapter finally shows that not only anthropologists do anthropology. In his research on the formation of Brazilian literature, Antonio Candido (1964) reveals the process through which it became a national project. In contrast, decades later, Roberto DaMatta (1980) picks out popular expressions — carnival and other daily-life rituals — to examine "what makes brazil, Brazil." Both of those authors, one a sociologist and the other an anthropologist, study aspects of an ideology intended, or desiring, to be national. Running throughout the thesis is a dialogue with Norbert Elias' proposal that in the 20th century one must consider national ideals in order to understand ideological aspects of sociological theories, in confrontation with Louis Dumont's suggestion that anthropology only develops in modern individualistic contexts (see Peirano 1981).

Two more comments. While Brazil was the privileged case, my project also aimed at putting the discipline itself to the test. Following the best anthropological tradition, the cases of France, Germany and, to a lesser extent at the time, India provided a comparative outlook. As for the thesis title, at the time I hardly considered it inspired, overwhelmed as I was by a shortfall of imagination at the end of my writing. Back then, an "anthropology of anthropology" sounded, at best, obscure.

The general context and the options. A certain uneasiness about the role of anthropology and anthropologists was germinating in the United States in the late 1970s. The first signs broke out of that colonial guilt which, over the next decade, would lay siege upon North American academia. In this general setting, I could envision two possibilities for research: one directly linked to the malaise of the center, consubstantiated in the inversion of the anthropological "out"-look. In other words, coming from the periphery, my path would be to make the United States my object of research. Many Brazilian colleagues did follow this line, at the time. I saw this as someone else's problem, though, and was not attracted to the idea. Another possibility came from David Maybury-Lewis, my thesis advisor, who suggested a research project on dual organizations in Ethiopia. I was flattered by the proposal, since accepting it would place me in the midst of still recent debates in the field of structuralism. The decision was all the harder for me, because it was a challenging proposal. At the end, however, I turned this option down because I did not see myself fitting into that line of investigation, nor would I be able to follow it up in Brazil. In this context, doing fieldwork in the Cape Verde Islands would have been a more promising project, but I had to give up that option too, for family reasons after a short trip there.

It was then that George Stocking Jr. spent a semester at Harvard. It was 1977 and Stocking had already won notoriety as a historian of anthropology. His classes were fascinating both for the past he opened up to us and for his erudition and intellectual refinement. It was during his classes that I raised the fateful question that would lead me to the thesis I finally wrote: If the German ethnographers who went to North America left behind a Franz Boas, why had we not gained an equivalent legacy from the ethnologists who came to Brazil as part of the same project? Why do our intellectual lineages so rarely go back to the German ethnographers from the late 19th century (except for Baldus, and Schaden, for example)? Why, after all, did the long-term ethnographic style never "catch on" in Brazil as it did in the United States? (A contemporary version of the same question might lead us to query why some currents never make it here, while others catch *and stick*, as obligatory "musts".) Moreover, why did literary critics and sociologists at certain times do anthropology just as good or even better than anthropologists? Precisely because I planned to find answers in the broader ideas and values (or cosmology, in this case a political cosmology) of different social contexts, I found myself doing "an anthropology of anthropology."

Stocking was not impressed at first. In reaction to a "trial paper" I handed in to him, he suggested that I research the institutional history of the University of São Paulo (USP), for instance. My proposal looked very unorthodox to him. A couple of years later, I sent the draft of the thesis to Chicago and felt gratified that "the anthropology of anthropology" had not displeased him. I still owe that study on the USP, though.

II

Until the 1960s, only the ethnologists themselves told the history of anthropology, and only at the end of their careers. The successful career of authors and the fact that they had been contemporaries to events and publications gave their narratives legitimacy and credibility. A few examples come to mind: six years before his death, Alfred Haddon (1855-1940) published *History of Anthropology* (London: Watts & Co.). The first edition of *The History of Ethnological Theory*, by Robert Lowie (1883-1957), came out in 1937, when the author was already well known. *Developments in the Field of Anthropology in the Twentieth Century*, by Clyde Kluckhohn (1905-1960), is dated 1955.[2] Other indicators: André Singer published *A History of Anthropological Thought*, by

[2] Among more contemporary anthropologists, this practice can be seen in *The Expansive Moment. Anthropology in Britain and Africa 1918-1970,* published in 1995 by Jack Goody, in which the anthropologist reflects on a period in which he participated in the history he recounts. The writing of introduction-to-anthropology books at the end of their careers has also been common (see for example, *Social Anthropology*, by Leach, 1982). Finally, several articles with autobiographical reminiscences have been published by renowned anthropologists in *Annual Review of Anthropology* (for example, Firth 1975, Leach 1984, Srinivas 1997, Geertz 2002, Goodenough 2003).

Evans-Pritchard (1902-1973), after the author's death (New York: Basic Books, 1981); in the mid-20th century, studies and biographies on “classical” anthropologists were published, such as Goldschmidt's 1959 study on Boas.

That all changed with George Stocking. In 1968, Stocking published his first book, *Race, Culture, and Evolution. Essays in the History of Anthropology*, which had become an obligatory reference by the following decade. His line of work has greatly expanded with researchers looking at anthropology at different historical periods in many diverse contexts. Besides the volumes published as part of the “History of Anthropology” (*HOA*) collection, directed by Stocking from 1983 on, the best source for information on contemporary studies on the history of anthropology is the *HOA Newsletter*, put out by Stocking himself since 1973, which lists work in progress, commentaries and recommendations. Despite today's large production, no other historian of anthropology has surpassed Stocking in terms of either the relevance or the continuity of his work.[3]

Authors, however, have no control over the appropriation of their work, and Stocking's writings are no exception. There is a particularly curious phenomenon in Brazil in this regard. The history of anthropology developed by Stocking often becomes anthropological theory, as both professors and students fail to separate historiography from theory. This situation provokes serious consequences in the formation of new generations as students avoid plowing through long classical monographs in favor of Stocking's more interesting accounts.

It is time therefore to distinguish between different kinds of histories of the discipline. Although intertwined, their differentiated objectives give them specific strategies as they bring an author out of the past. I will discuss two of these approaches. One is the history of the discipline in the historiographical style consecrated by Stocking (which includes, as a sub-type, the anthropology of anthropology). More on this, later. The other is the “history of theory,” a history which is internal to the very practice of anthropology and which indicates the orientation and the central issues for the discipline, the refinements it has undergone and particularly certain insights which, not fully appreciated when first published, now inspire the renewal of both empirical and theoretical questions.

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[3] By Stocking see, just as an illustration, his well-known studies on Franz Boas (Stocking 1974a, b, d).

The history of anthropology or "In this context ..." It is interesting that Stocking himself never entertained doubts as to the difference in approach between history and theory. He never assumed the role of a theoretician of anthropology, but rather defined his orientation as a historian. He distinguished between two distinct perspectives (Stocking 1968, cap. 1): one, he calls presentist, the other, historicist. The first perspective is normative, based on the notion of progress, and focuses on the rationality of thought in a process leading up to the present. Stocking's is the second option, of a commitment to understanding the past for its own sake. He is more concerned with thinking than with thought, with understanding than with judging, and with plausibility than with rationality. His interest thus lies in approaches that focus on context, process, plausibility and viability. It is from this perspective that Stocking speaks about how the social sciences are rather insensitive to the fact that predecessors often asked questions (and provided answers) about problems that remain relevant to this very day.

A few years later, Stocking (1971) set out another distinction: the "traditional" current of historiography, whose main objective is to classify scientists of the past to the extent that they anticipated the present state of the discipline, *versus* the "new historiography of the sciences," located at the intersection between history, epistemology, and the sciences. The "new historiography" focuses above all on questioning the option between (i) concentrating on specific works — that is, the theoretical and experimental problems as defined by the scientific community — and (ii) investigating the influence of technological, socio-economic, institutional and political factors. Also of interest is the question of whether there is a continuous development of knowledge from common sense to science, or whether we should look at science as an epistemological eruption in a particular historical period.

Here I include an episode I remember from Stocking's lecture course, which I (and several other graduate students) attended. Before each class, Stocking passed out a mimeographed sheet to each student, with a short list of five or six topics, a bibliography and several unfamiliar names, identified by their dates of birth and death, and a minor biographical indication. These were the markings for a map, the context. During the class, Stocking would provide the plot that brought all these names, books and characters together. This was when historical data revealed social links and networks, when heroes became human beings, when neglected authors took on unexpected roles. This was not the history of works but the history of people thinking. In this regard, Stocking one day confessed in a graduate seminar that when he re-read his papers before publication, he was impressed by the number of paragraphs beginning with the phrase "In this context …." He would correct this repetition (which, although tamed, is still present in his writings), but it corroborates one fundamental aspect: events, characters and works must always be situated in the social and historical context of their time.

Hallowell. Here, however, we ask, is "in this context" not a common expression in ethnographic texts as well? Do we not invariably observe events, beliefs, language, etc. *in context*? It is then worth our while to look at the period during which Stocking graduated as a historian at the University of Pennsylvania. Stocking had in A. Irving Hallowell (1892-1974) his mentor in anthropology during the late 1950s. It was Hallowell in 1965 (prior, therefore, to the first edition of *Race, Culture, and Evolution*) who proposed that the history of anthropology should be "an anthropological problem." Stocking (1976, 2004) recognizes this influence in his work.[4]

To examine the history of anthropology, Hallowell (1965) argued that it is best to follow anthropology's inquiries than to follow the conventionally-defined discipline, thus allowing for a direct attention to the cultural context and historical circumstances out of which formulations of anthropological questions must have developed. This perspective would avoid any chance of arbitrarily isolating the development of anthropology from its roots in a wider cultural context.

Hallowell goes further. Anthropological questions are not exclusive to modern times. If we look for them in non-Western societies, we will find them inserted into peoples' cognitive orientation, in their cosmologies, from which they have not been separated, abstracted and articulated as they are today in our milieu. With that perspective, Hallowell opens room for us to examine not only chronological history but also the conditions for the emergence of anthropology

[4] Upon reading the first draft of this paper, George Stocking kindly sent me a copy of a paper he just published, "A. I. Hallowell´s Boasian Evolutionism" (Stocking 2004), a study about Hallowell's trajectory through his disciplinary, cultural, and personal contexts. The study is dedicated to the memory of Hallowell, and in the introductory comments Stocking mentions that Hallowell was actually a member of his own thesis committee at Penn.

outside of its modern institutional field. Hallowell in other words allows us to look at Western culture as distinctive as "the theatre of a continuing and accelerating effort by man to obtain increasingly reliable knowledge about his own nature, behavior, his history and varying modes of life, as well as his place in the universe" (1974:305). In other times and places, this was a goal of priests, theologians, philosophers, etc. By questioning how some lines of inquiry gain legitimacy as a discipline, Hallowell opens up the line of investigation that nowadays we may call an "anthropology of anthropology."[5]

Hallowell thus represents a fundamental link in our discussion, as he both brings together and differentiates between various kinds of inquiries. First, Hallowell promotes the convergence between the history of anthropology and the anthropology of anthropology, thanks to the elements we identified above.[6] This convergence, however, has two major exceptions: (i) while the former looks at the past as "another place," the latter seeks to question both past and present in an ongoing quest for conditions that might legitimize certain questions as anthropological in nature; and (ii) an "anthropology of anthropology" requires a theoretical orientation that is itself anthropological, in order to provide a groundwork for the investigation (which in my case I found in Durkheim and Mauss[7]). Second, since Hallowell was a fieldworker who also examined the conditions for the history of anthropology (Hallowell 1976), we are fortunate to find a subtle permeability between the lines of investigation present in Stocking's historiography, in the anthropology of anthropology, and

[5] It is not entirely surprising, therefore, to see that while the history of anthropology for Hallowell was "an anthropological problem" in 1965, a few decades later anthropology could question itself and put itself to its own test.

[6] My first contact with Hallowell's work, of course, came during Stocking's courses.

[7] I digress to mention that the anthropology of anthropology in the 1970s had a by-product that, for lack of a better name, I call "the politics of theory." As I surveyed the values that legitimized anthropology in Brazil, I empirically identified the ubiquity of the ideology of nation building as a project of social scientists. While formulated in different ways, this issue comes up in biographies, underlies decision making, establishes academic careers, informs choices between courses, etc. The political dimension was, and still is, a clear-cut ethnographic presence. Later, as I researched the case of India, I found a similar trait. In that case, however, there was a double dialogue: both with a national project and with a civilizational project self-defined in confrontation with the West, cf. Peirano 1987. (This perspective finds a parallel in Ahmad 1995, who builds on a Marxist vision of intellectual production.) In due time, I realized that to term these processes as "nation building" or "civilization building" was impoverishing, as is always the case when we use labels. To avoid this, I began raising empirical questions. For example: what can parallel publications by authors of the same generation show us (cf Peirano 1997, in which I compare books by Geertz, Madan, Rabinow and Veena Das)? When the metropolitan centers set out to develop an anthropology "at home," what meaning does their project have, for example, in Brazil? (cf. Peirano 1998). I identify these questions as related to the "politics of theory."

in what I call theoretical history. Stocking remarks: "'The History of Anthropology as an Anthropological Problem' provides a model for an approach that is in the best sense both historical and anthropological" (1976:19).

Permeability is not identity, though. In the third approach, the lines of separation must be clearer, as they involve the reproduction of the discipline.

Theoretical history. In contrast with the approaches taken by the history of anthropology and the anthropology of anthropology, we have theoretical history, a term I use to indicate the *sui generis* combination of history + theory, and which consists of an internal vision into the practice of anthropology. Theoretical history informs and guides the refinement and the expansion of anthropology based on our predecessors' work and our own field research. By means of theoretical history we perceive the questions that have marked the development of monographs that we consider to be cornerstones of the discipline, its canonical (or mythical, as some prefer) body. Whenever we set out to train students in anthropological theory through the sequential reading of authors, and by means of the examination of the unfolding questions we perceive as relevant, we are putting the history + theory duo into action. Theoretical history thus examines problems that have become deserving of inquiry, and also the dialogues held by anthropologists — dialogues that have become part of the open and continually renewed repertoire of anthropological questions. The final movement is thus spiraling and dynamic, with earlier questions taking on new life — far from a linear or progressive course.

Since students of anthropology do not "learn," but rather are "trained" in anthropology (Duarte 1995), a major share of their initiation leads to the creation of individual or collective lineages, through the patch-working and bricolage of specific theoretical orientations. As an internal dimension of anthropological practice, theoretical history informs and guides new research. Here we must emphasize that ethnographic knowledge is based on the surprise that constantly awaits the ethnographer — i.e., a surprise both to our common-sense truths and to anthropological theory, and which allows us to contest, correct and/or enrich them. This (Weberian) "eternal youth" of anthropology has been under way ever since Malinowski established the *kula* as a new agency in the Western world, in contrast with theories then in vogue about primitive economy. It is to be expected, therefore, that the revisiting of the classics be a fundamental practice in the discipline. Theoretically necessary, but also indispensable for their sociological role in creating inter-generational links, the classical monographs provide us with an intellectual framework, a theoretical legacy, a map of relevant questions and a repertoire of still unsolved problems. For the practitioner, the theoretical promise they raise oftentimes may be more significant than the context in which they were produced.[8]

Recapitulating, then, Stocking is an author who deserves his esteem. As a *sui generis* historian of science — inspired by Hallowell while a student, and active for decades as a professor of anthropology at Chicago — he maintains his identity as a historian for some central reasons: his interest more in the past than in the present, and the fact that he never went through the fieldwork rite of passage. "The historian's archive is not the ethnographer's field" (Stocking 1992b:13). He also adds that he was not trained as an anthropologist, but as an historian — albeit as an historian of a non-traditional sort at the American Civilization Program at the University of Pennsylvania. As an anthropologist, therefore, he recognizes his status as an outsider, regarded with some suspicion by "legitimate" ethnologists (who oftentimes fear being turned into natives). As a historian of science, he also feels out on the margins, since prestige in that area is dominated by the hard sciences. Nevertheless, although Stocking is not an anthropologist/ethnologist, his warning against anachronism is priceless — a lucid, clarifying and balanced lesson, particularly when we anthropologists begin considering the past as disposable. His overview of the "historicist" and "presentist" perspectives is also precious: if the past is another place, it survives in contemporary discussions and theoretical dialogues. Again, history is not anthropological theory — although this confusion between internal and external approaches often obscures more than it enlightens the training of our students.[9]

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[8] Both anthropology of anthropology and historiographical research build on theoretical history when, for example, they indicate how some debates within the discipline are doomed to failure when we consider the political-theoretical cosmology of the authors. (See, for example, Peirano 1987, which examines how the 20-year debate between Dumont and Srivinas could not come to a harmonic outcome, due to the Dumont's civilizational vision and Srinivas' national outlook.)

[9] I add two examples just for the sake of emphasis. The first has to do with Charles Peirce. For an anthropologist, his lessons on iconic, indexic and symbolic signs are unrelated to his being considered an eccentric by his intellectual peers in the 19th-century United States, and his never having attained — partially for that reason — an academic post. The extent to which his ideas on the nature of signs grew out of his outsider status will continue to be a question that it is not up to us to solve (see Borges 2004 for a successful example of ethnography inspired by Peirce). Durkheim provides us with a second case. Our ongoing appropriation of his lessons about the nature of society is not prejudiced by knowing that the author may have had a doubtful personality as a self-proclaimed guardian of the truth, with domineering characteristics, and a virtuous supporter of the patronage system (Lepenies 1985). Heloisa Pontes argues quite correctly that research on the position of sociology in the French university system at the time is helpful for us to achieve a broader

III

Historiographical readings (which are external) and theoretical readings (which are internal) each have their own questions and projects. To conclude, I return to our perennial starting point, Malinowski. I focus on two articles that discuss the transformation of fieldwork into a legitimate model for anthropological experience. Their authors are George Stocking Jr. (1992b) and Edmund Leach (1957). My underlying argument is that the two articles give rise to two different Malinowski(s).

Stocking on Malinowski. Stocking's paper was written in the early 1980s. With the intent of clarifying "the magic of the ethnographer," the article focuses on Malinowski's research in the context of anthropological discussions on method and field experiences since the mid 19th century. With the historical minutia and erudition we know so well, Stocking moves down the path from McLennan and Tylor to the missionaries and natural scientists, then to the expeditions to the Torres Straits, Haddon's and Spencer's papers and the various versions of *Notes and Queries* until the most famous revision of 1912, with the classical study by Rivers. Stocking sets out the script by saying, "Let us begin with the state of anthropological method before the culture hero came upon the scene — for this, too, is part of the myth we seek to historicize" (1992:17). Stocking wants to historicize the myth of fieldwork inaugurated by Malinowski. Throughout the fascinating history laid out in the article, Stocking shows us, for instance, how Spencer & Gillen's *The Native Tribes of Central Australia*, published in 1899, already used a recognizably "modern" style of ethnography — well before Malinowski. Frazer, on the other hand, whom we consider the prototype of the armchair anthropologist, not at all into "savages," did much to stimulate field research among young practitioners. From ethnology "off the verandah" to surveys, to Radcliffe-Brown's "intensive research", to Rivers' "concrete method," by 1914 the idea of fieldwork was a decade old, in what might be recognized as a "modern" form. A number of anthropologists had come out of English universities to spend a year or two in the field (Radcliffe-

understanding of the substantive theoretical issues faced by Durkheim and his disciples. Even so, while those factors are clearly fundamental for a history of sociology *in that context*, the theoretical apprehension of Durkheim today is entirely independent from that history. (For the timelessness of his inspiration, see Chaves 2000.)

Brown, Diamond Jennes, Gunnar Landtman, Rafael Karsten, Barbara Freire-Marreco, Marie Czaplicka, John Layard), and before the war, Seligman already had it that field work was for anthropology "what the blood of martyrs was for the Catholic church" (*apud* Stocking 1992b:30).

Stocking thus deconstructs the myth with historical evidence. In fact, within that group of researchers, Malinowski was the last to go to the field. Yet he still takes credit for the fundamental institution of ethnographic research. How this was possible is what Stocking discusses in the second part of his article, in which he shows how "the Ethnographer" (an expression taken from *The Argonauts* with capital letter and all) did not only follow Rivers' program but also changed the main focus of the investigation, from the ship deck or the mission porch to the middle of the village, and at the same time modified the concept of the role of the ethnographer, from one who simply studies a society to an observer participating in village life.[10] Alongside the kind of fieldwork, there is also a shift in the theoretical orientation, since the objective of anthropology becomes more than simply to reveal the history of humanity, as Rivers would have it.

Here Stocking's evidence and arguments refute the ideas that Malinowski behaved in an egalitarian manner (as American anthropologists would later attempt, without success) — indeed, Trobriand society was extremely stratified itself; that Malinowski traveled on a *kula* expedition (only the attentive reader concludes that he did not); that he was a mere participant observer, when on the contrary, often as a strongly interactive researcher, Malinowski questioned deep-held beliefs, pushed contradictions and forced the natives "up against the metaphysical wall" (and was placed by them in the same situation himself). Malinowski himself actually explained this perspective, but in Baloma (Malinowski 1916) rather than in *The Argonauts* (1922). Stocking thus sets out after an understanding of the success of the Malinowskian research recipe. Instigated by one of Malinowski's objectives ("how to convince my readers"), Stocking argues that, by adopting a Frazerian style — in which the scene/act relationship places the reader imaginatively within the actual setting of events —, Malinowski included an "author/reader equation," which has led to the belief ever since (Stocking 1992b:54) that the story in *The Argonauts* is a sequence of events actually experienced by the author himself.

[10] Malinowski also uses capital letters to distinguish the different positions of "the Ethnographer" and "the Philologist" in relation to language (Malinowski 1930), which may suggest more of a conventional use than a personal rhetorical device.

One high point in Stocking's article comes as he shows how Malinowski built three types of characters in *The Argonauts*: the natives (and here the question of the meaning of calling them “niggers” in his field diary comes back), those who did not understand the natives (administrators, missionaries, merchants, etc.) and finally the Ethnographer — a conception reinforced in the photographs that show “the Ethnographer's tent” placed strategically at the beginning and the end of the book. At this point, the article's tone becomes more evocative (“Considered in this light, *Argonauts* is itself a kind of euhemerist myth — divinizing, however, not its ostensible Trobriand heroes, but the European Jason who brings back the Golden Fleece of ethnographic knowledge” - cf. Stocking 1992b:56). Malinowski had created the role of the hero for himself. If this model caught on, if his “methodological charisma” carried the day, if he made himself the archetypical fieldworker, then the “ethnographer´s magic” won legitimacy because it filled the gap between methodological prescriptions and the vaguely-defined objectives of ethnographic knowledge. Stocking concludes by suggesting an anthropological interpretation derived itself from Malinowski: “And just as in primitive psychology myth functioned ‘especially where there is a sociological strain’, in anthropological psychology it functioned especially where there was an epistemological strain” (1992b:59).

Leach on Malinowski. Leach also insists on Malinowski's charismatic role, calling our attention to the fact that he had several predecessors in fieldwork (including Boas). He also points out that Malinowski was a son of his time, stuck in 19th-century orthodoxy. The ambiguity of the term “savage” is one eloquent example — even as he emphatically denied that the Trobrianders were “survivals” of a remote past, Malinowski need to presume “an age-long historical development” (cf. Leach 1957:126) to justify the state of equilibrium of the populations studied by anthropologists. Leach also identified Malinowski's personality as a prophet and a leader, who saw himself as a missionary, a revolutionary innovator of ethnographic fieldwork. As revolutionaries normally do, Leach adds, he tended to diminish his more conservative contemporaries and his immediate predecessors. Thus, an entire generation of students was trained believing that social anthropology had its beginnings on the Trobriand islands in 1914.

These observations are akin to Stocking's, but Leach presents them circum-stantially, as if taking for granted Malinowski's role in the discipline's cosmo logy. Leach's main proposal is that Malinowski's ethnographic style does not come down to an artistic device, but that it is a true *theoretical* innovation (italics in the original). For Leach, Malinowski transformed ethnography from the museum study of items of custom into the sociological study of systems of action (1957:119). Recognizing that Malinowski represented a unique and paradoxical phenomenon — "a fanatical theoretical empiricist" — two features set aside his style: first, the end of the professional informant and, second, the *theoretical* assumption that the data under the observation of the fieldworker must somehow fit together and make sense (once again, Leach's italics). Malinowski is thus a stimulating genius as he speaks of the Trobrianders, yet irremediably outdated (a "platitudinous bore," in Leach's words) when he explicitly wants to theorize, as in *A Scientific Theory of Culture* (1944) — indeed, as I recall, a posthumous book.[11] Leach thus contradicts Malinowski's self-evaluation regarding his life work, indicating that his best theoretical insights are found implicitly in his monographs rather than in the writings he personally had judged to be "theoretical." To clear up this point, Leach wonders what kind of pragmatism had guided the author.

Leach finds an answer in the philosophers William James e Charles Peirce, and indicates that Malinowski adopted the former more than the latter. Like William James, he was suspicious of any abstraction that did not derive from or refer to directly observable facts. (The Peirce alternative would have led him to consider that ideas and knowledge, and above all the life inherent to symbols, are just as real as the individuals who use them.) The rationality of savages and the proposal that primitive man does distinguish between fact and fiction are arguments that are implicit in his writing, particularly as developed in *Coral Gardens and their Magic* (1935). Malinowski, however, was unable to carry out this basic division, since he could not always judge where rational procedure ended and magic and aesthetic procedures began (*apud* Leach 1957:128).[12] Leach intervenes here to suggest that, instead of insisting that primitives were just as able as Europeans to tell the difference between work and magic, he would have made a better case had he argued that Europeans are ordinarily just

[11] With a posthumous publication, we inevitably ask why the author did not bring it out while alive. In 1957, however, all indications are that the book was taken more seriously as "Malinowski's theory" than it is today.

[12] Stocking mentions the same passage to suggest that "despite his professed methodological candor, it is clear that Malinowski himself sometimes blurred … [the distinction] 'between what is mere mythopoetic fiction and what is … drawn from actual experience`" (1992b:54).

as incapable as Trobrianders of telling them apart. (This was a task Leach himself took on in his essays in the 1960s.)

A further point relates to language. While Leach considers Malinowski brilliant for having stressed that the meaning of words depends on the context in which they are enunciated, thus revealing their pragmatic nature, he disregarded the symbolic aspect of the spoken word. It was Mauss, based on Malinowski's ethnography, who conceived the *kula* as symbolizing the ambivalent aspects of friendship and hostility, two constituent elements of the social structure. *Kula* rituals "say things" that the Trobrianders cannot put into words.[13]

Leach's article reads thus as an active dialogue with Malinowski. He criticizes, points out mistakes and failures, refutes interpretations and praises contributions, some of which even Malinowski had never imagined.[14] It is in this perspective that, towards the end of the article, he sees in the notion of "institution" a legacy for his successors. As a medium-range concept — not too abstract to appear only a verbal speculation nor too concrete to impede a comparison — it could perhaps be a bridge between the vulgar functionalism that predominated in the 1930s and the more sophisticated structural analysis of Leach's time. Finally, Leach asks whether it was not precisely because Malinowski owed so much to his predecessors that he resented their ideas, a phenomenon which might be repeating itself in the 1950s with Malinowski himself.[15]

In conclusion. In juxtaposing these two pieces, a word is necessary about the conditions under which each was produced. Stocking is determined to historicize myths, Malinowski's included, and his article (1992b) is the result of a new look at the history of anthropology, marked by a rupture he detects in the self-image of anthropology following the publication of Malinowski's diaries in 1967.[16] One may notice that, in his more recent writing, Stocking began

[13] See Tambiah (1985) for re-analyses of the Trobriand ethnographic material, in articles that materialize Leach's proposal.

[14] Leach's dialogue with Malinowski can be appreciated from another angle in his re-analysis of the Trobriand material. See for example, Leach (1958, 1966).

[15] Leach himself confesses in his own regard to Malinowski: "There was […] a point in my anthropological development when Malinowski could do no wrong. In the next phase, Malinowski could do no right. But with maturity I came to see that there was merit on both sides" (cf. back cover of Leach 2000).

[16] See Stocking (1974c) for a most insightful analysis of the moments of frustration when Malinowski would write in his diary, as opposed to the favorable moments of productive

considering the issues of ethnographic authority, the creation of texts, and the "poetics and politics" of ethnography (Stocking 1992b:15). Readers may also recognize a somewhat post-modernist playful humor in the emphasis on Malinowski's taste for metaphors — for example, the highlighting of his statement that "if Rivers was the Rider Haggard of anthropology, I shall be the Conrad."[17] In a different vein, Leach's article came out in the context of a discussion on Malinowski's work, supposedly to pay homage to the author, although at a moment not all that favorable to him.[18] Like Stocking, Leach identifies Malinowski's prophetic personality, but his central concern is to evaluate both the contribution and the weaknesses of Malinowski's perspective, to bring to light the inspiration of his proposal, to take a position on that proposal, and to recognize — all things considered — the positive *theoretical* legacy of his (methodological) approach.[19]

In sum, while we can read Leach engaged in a lively debate with his predecessor, Stocking on the other hand takes no open position on Malinowski. He sees him thinking, acting, and building his career. There are thus at least two characters answering to the name "Malinowski." For Leach, he is the author of a body of ethnographies, and the subject of theory — who therefore remains a living and fundamental interlocutor. For Stocking, he is a historical subject, the Ethnographer, the researcher in the first half of the 20th century who marked anthropology, a man who has become a myth. Is one of these approaches more valid? Which "Malinowski" do we choose? The answer is simple, of course. A

fieldwork, when he would skip the diary. The tone of the article is understanding towards Malinowski: "… [I]n the overall context of both his diary and his ethnography, one is perhaps justified in assuming that Malinowski's admittedly ambivalent and sometimes antipathetic feelings toward the Trobrianders were the basis for an interaction which, however emotionally complex, involved, in varying degrees, tolerance, sympathy, empathy and even identification" (1974c:286).

[17] We might think that in this article Stocking is invading the territory of an "anthropology of anthropology," by means of the post-modernist (theoretical) inspiration.

[18] Most articles published in Firth (1957) are, to say the least, ambivalent. In the late 1950s, the dominant discussion in anthropology did not favor discussions on the theme of fieldwork.

[19] The theoretical dialogue of anthropologists with predecessors, even when for biographical purposes, can be exemplified in Tambiah's (2002) volume on Leach. The author explains his position on the subject of the biography: "My interactions with Leach, and my own understanding and interpretation of what he wrote and said are an integral part of the text. Leach speaks, writes, and narrates—but these representations are filtered, selected, arranged and mediated by my own activity as narrator, commentator and friend. *Throughout much of the text, I am in dialogue with Leach, who cannot speak back now.*" (:xiv, my italics).

text is more elucidating to the extent that it answers the questions one puts to it. Both Stocking's *history of anthropology* and Leach's *theoretical history* help us, convince us, and either stimulate and/or inspire us, but one should not be taken for the other.

Translated by David Hathaway

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