v. 1, n. 6, 2021

International Journal of Human Sciences Research

THE NATIVE EXPERT: AN ESSAY ON EPISTEMOLOGICAL AUTHORITY IN ANTHROPOLOGY

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Abstract: What happens to the anthropologist's work when the natives of his research are the most important and recognized experts in the scientific/academic world on the topic he is investigating? What changes in our conception of the process of knowledge production in anthropology when this process includes not only an analysis of native ideas and practices, but also a theoretical clash with natives, invested with this condition of specialists? How to conduct such a process when this clash – which in itself already destabilizes the conventional image of the anthropological profession – is, furthermore, aggravated by the fact that the native finds himself in a situation of political and epistemological prevalence over the anthropologist?
Keywords: Anthropologist.

INTRODUCTION

What happens to the anthropologist's work when the natives of his research are the most important and recognized experts in the scientific/academic world on the topic he is investigating? What changes in our conception of the process of knowledge production in anthropology when this process includes not only an analysis of native ideas and practices, but also a theoretical clash with natives, invested with this condition of specialists? How to conduct such a process when this clash – which in itself already destabilizes the conventional image of the anthropological profession – is, furthermore, aggravated by the fact that the native finds himself in a situation of political and epistemological prevalence over the anthropologist?

These questions portray a situation presumably increasingly common today, as anthropology has often devoted itself to the study of scientists, experts, and specialists of various kinds. This occurs not only because of the interest that science and expertise arouse by themselves, but because in the so-called knowledge society, these actors play an increasingly relevant role in the construction of social practices, institutions and culture itself (Knorr-Cetina, 1999).

Indeed, this situation emerged very forcefully in my doctoral research. In this one, the theme was science and technology policy in Brazil. Specifically, I analyzed the history of the creation of policies to support technological innovation in the country, which took place in the 1990s and 2000s. This analysis included (i) a reconstruction of S&T policies since the 1960s, when they began to be structured nationally; (ii) a political and economic contextualization of Brazil in the 1990s, when the concept of innovation, specifically, was introduced in the field of S&T; (iii) an analysis of several historical documents, which record the public debate that took place at the time on this topic (Brasil, 2001, 2002); and (iv) an exegesis of the legislation and government programs that implemented these policies in the country (Pereira, 2016).

Science, technology and innovation policies are not a conventional theme in anthropology, but they provide an interesting interface with an important area of the discipline, which is the studies of science and technology, or the anthropology of science. In these studies, a central issue is the relationship of science with its social and political environment (Stengers, 2002) and the need to understand this relationship in a symmetrical way (Latour, 1994, 2012). Thus, I have been trying to suggest that the use of theoretical frameworks from the anthropology of science to analyze S&T public policies can be enriching for both parties, as it allows bringing to anthropology a central theme in the field of science, which is public policy; and, at the same time, take the look of anthropology to the arenas where this theme has been discussed (Pereira, 2020).

Methodologically, my research was mostly documentary: historical description, through bibliographic review; analysis of institutional publications, various documents, texts of laws and government programs, etc. However, even so, there were natives. First, because, through the documents, I analyzed ideas, practices, knowledge, ways of acting, being in the world and doing politics for different groups, members of this "tribe" located in the field of ST&I policies in Brazil. In this sense, there was a meeting with "others" and an experience with the difference. Furthermore, the document analysis was complemented by lengthy and crucial interviews with public managers from various state institutions linked to these policies. Some occupied their positions at the time of the research, others had done so at the time of the implementation of the innovation policies. Thus, there was direct contact with these "others", raising some of the more traditional problems of anthropological fieldwork.

A basic characteristic of these "natives" was the fact that they were all, to some extent, scientists. The interviews were carried out mainly with employees of Finep (Financing Agency for Studies and Projects), the main innovation financing agency in Brazil. His areas of training ranged from economics to engineering (always with a postgraduate degree in administration, public policy, innovation management, etc.). Many were professors/researchers at top universities; almost all masters or doctors, with publications in specialized journals. One of them became part of my qualifying panel, as, in addition to being a director of Finep, he was a professor at the Institute of Economics at the same university where I was studying for a doctorate and had published reference articles on the history of Finep itself and the policies of ST&I in Brazil.

Therefore, my relationship with these people was marked by a profound ambivalence: on the one hand, they were native to the research, as they were part of the group taken as the object of analysis; on the other hand, they were theoretical interlocutors, that is, scientists/specialists with whom I could (and eventually must) debate my ideas, my theoretical assumptions and my research conclusions. At the time, this ambivalence seemed to me just a good opportunity to engage in a deeper dialogue with the natives/specialists in the theme of the thesis. Later, however, I realized that this would raise very concrete reflections, questions and problems, which I will try to outline in this work. I understand that this exercise is not only of interest to me, but to the discipline as a whole, as, as mentioned, this situation tends to be common nowadays.

Anthropology has discussed a lot (since it was founded) its relationship with the natives. But, our discipline is also made in the relationship with theoretical interlocutors, who, as scientists/specialists, in the so-called knowledge society, are inserted not only in the world of science itself, but in different sectors of society, such as the State, the market, social movements (Knorr-Cetina, 1999). It's important to discuss this relationship, not least because eventually, as I'm saying, they can become our natives.

It is important to point out that the possibility of natives becoming theoretical interlocutors, that is, people who would traditionally be studied by anthropology insert themselves in the world of science and occupy the same epistemological place as the supposedly higher anthropologist, is not under discussion here. This is a phenomenon that has occurred with some frequency and was well regarded by the parties involved, even though it has generated heated debates. The discussion here is about the opposite: theoretical interlocutors become natives, that is, people inserted in

the world of science, who occupy the same epistemological place as anthropologists (or even higher, as we shall see) and with whom we would traditionally discuss our theories, be studied by anthropology and occupy the same epistemological place as the supposedly less elevated natives.

A theme that crosses these questions diagonally, and therefore will be the focus of this article, is that of epistemological authority, that is, the problem of knowing who has (or who has more) legitimacy to construct a discourse and define it as knowledge, a truth, a knowledge. Authority here has both meanings of the word competence, that is, permission granted and ability recognized to someone to do something. Eventually, I will use the term scientific authority, referring specifically to the legitimacy to construct knowledge and define it as scientific. This does not mean to assert a superiority or autonomy of science in relation to other forms of knowledge, but rather its specificity, or uniqueness, to use Stengers (2002) term.

Since its foundation, anthropology has always involved not only a subject and an object (a subject that knows and an object that is known), but, above all, relations between subjects, endowed with different knowledge. James Clifford says that anthropology produces interpretations of interpretations, that is, anthropological knowledge about the knowledge of the natives (Clifford & Marcus, 1986). With varied nuances, there are many images that reiterate this vision. It must only be noted here that, in this sense, anthropological research involves at least three sets of knowledge: that previously acquired by the researcher, deriving from his disciplinary and cultural tradition; the knowledge of the natives, which the anthropologist comes into contact with during research; and the scientific (and cultural) knowledge that the anthropologist produces at the end of this process.

In this tangle of knowledge and cultures, it is natural that the problem of epistemological and scientific authority emerges in different ways, amidst the power relations between natives and anthropologists. Also since its foundation, anthropology has been debating this theme, albeit in heterogeneous and occasionally implicit ways.

Indeed, the general objective of this article is to show what the problem of epistemological authority consists of, how it emerges in the history of anthropology and how it is connected to the problem of relations between anthropologists, natives and theoretical interlocutors. Three distinct moments of the discipline will be contrasted: (i) the works of classical authors, such as Malinowski, Marcel Mauss and Evans-Pritchard; and (ii) the theoretical-methodological turn provided by the so-called: *studies up* and, in general, by the research carried out in the anthropologist's own society; (iii) a current moment, based on the views of authors such as Viveiros de Castro and Tim Ingold. It is not claiming that the entire history of anthropology fits, even briefly, into these three moments. It is only intended to take them as references to contrast different modes of emergence of the problem of epistemological authority.

The specific objective of this work is to show how this problem reappears and reconfigures itself today, in which, as mentioned, the traditional theoretical interlocutors of anthropologists, once inserted in different sectors of society, may come to be studied by anthropology itself, occupying, thus, the place of the natives. The reference, in this case, will be my own research, whose elucidation constitutes an underlying objective in the reflections that follow.

THE PROBLEM OF EPISTEMOLOGICAL AUTHORITY IN CLASSICAL ANTHROPOLOGY

As mentioned, the question of epistemological authority has permeated anthropology, more or less explicitly, since its foundation. In a way, this was what was at stake when Malinowski, in a European context, and Franz Boas, in the United States, proposed an anthropology based on ethnography, on the analysis of first-hand data, and not on cabinet analysis and theorizing without empirical foundation. The presupposition of this ethnographic turn of so-called modern anthropology towards the native point of view was, among other things, the admission that knowledge about the natives must be learned from the natives themselves.

It can be said that ethnographic authority, carefully constructed by anthropologists in fieldwork (Clifford, 2011), is preceded logically and chronologically by a native epistemological authority, that is, by the assumption that anthropological knowledge has as its raw material the knowledge that the natives already have about themselves. As much as, according to various criticisms, ethnographies objectify the natives, frame their knowledge in the observer's references and obscure the dialogical nature of work itself, anthropology imposes on us, as a condition for knowledge, a process of learning about the the other with the other.

As mentioned, anthropological knowledge is not defined as the result only of a relationship between subject and object, but also of an encounter between subjects with different knowledge. After all, Malinowski would not have made so much effort to remain with the Trobriands if he had not admitted that, at least in principle, the greatest experts in the Kula exchange system were the Trobriands themselves, just as Evans-Pritchard would not have made a similar effort with the Trobriands. Azande if he hadn't admitted that, at least *at first*, the greatest experts in Zande witchcraft were the Azande themselves. It must be noted that these were not individual efforts: there was an entire science project around this recognition of native epistemological authority (Clifford, 2011).

Evans-Pritchard, in particular, went beyond his teacher and predecessor Malinowski: in a reflection that became a kind of anthropological jargon, he admitted that the authority of the natives gives them the prerogative to define even the topic on which they will be recognized as experts. It seems to me that this is the meaning of the classic passage in which he explains that, in carrying out his research among the Nuer, his initial intention was to carry out a study of social organization; however, as the natives spoke repeatedly about cows, he was forced to undertake a study of cows (Evans-Pritchard, 1969).

However, in the stratagem of classical anthropology, this recognition of native epistemological authority is only the first part of the process. Next, what necessarily happens is the construction of the anthropologist's knowledge. This knowledge derives from procedures proper to the anthropological profession, that is, articulations between different research data, comparisons and generalizations, built in the confrontation between their analyzes and those of their colleagues who studied other (or the same) peoples; correlations between field data and anthropological theories; debates between these theories and those of other disciplines, etc.

Therefore, if the assumption of ethnography is that the natives possess knowledge about the subjects to be dealt with, at the end of the process, after returning to their society and completing the work he started in the field, the anthropologist will necessarily

have transformed this native knowledge in something new, necessarily different, which is your own knowledge. This way he also constructs his own epistemological authority. In this sense, natives and anthropologists alike produce knowledge, but they do not produce the same knowledge, because, although one serves as raw material for the other, it is knowledge that derives from different methods, objectives, perspectives and even ontologies.

Indeed, native knowledge will always be fundamental for the anthropologist. This will need that native ideas, practices, reflections and definitions are always "fresh", so that they can be mixed and articulated with their own ideas, practices, reflections and definitions and thus become anthropological knowledge. It is Mariza Peirano who uses the metaphor of the freshness of data to critically contrast the valuable analyzes produced in the youth works of Clifford Geertz, when he was still close enough to his ethnographic experiences, and his dull and uninspired mature works, when these experiences no longer provided the necessary support for their ideas (Peirano, 1995).

However, this native knowledge, being the raw material of anthropological knowledge, needs to be manufactured, handcrafted transformed into anthropological concepts. In other words, the anthropologist always needs to introduce some difference between what he looks and hears in the field, with the natives, and what he writes, as an anthropologist, for his interlocutors when he returns home.

The highlighted verbs refer to the classic definition of the anthropologist's job, proposed by Roberto Cardoso de Oliveira (2006). According to the author, the acts of looking and listening, carried out in the research, are influenced by the theoretical background that the anthropologist takes with him, but mediated by contact with the natives, who show him and say almost everything he needs to look and hear. Therefore, these acts are associated by the author with the faculties of perception and learning. In this, according to him, they differ from writing, which is associated with thought and creation, with the anthropologist's ability to introduce new elements into what he learned in the field.

These reflections, elementary as they are, help us to understand the drama experienced by almost every young anthropologist who returns from the countryside and begins to outline his analyses. Still energized by the ethnographic experience, he presents this sketch to his supervisor or to the qualifying panel and almost always hears the criticism that his ideas are still too mixed with those of the natives. It is precisely this theoretical difference, derived from a creative capacity, between what the anthropologist says and what the native has told him that he is being charged.

It is possible that the young researcher is overly influenced by Evans-Pritchard jargon, actually intending to become an expert on the same subjects as the natives, just as the British author was forced to become an expert on cows. It's as if he wants to know the same thing, the same ontology, from the same perspective as the native. In this case, perhaps the advisor can help explaining that the outcome of the aforementioned jargon, not always remembered, is that the work Os Nuer entered the history of anthropology for unveiling the system of lineages in African societies.

So, in the end, Evans-Pritchard accomplished exactly what his initial aim was, namely, a study of social organization. It is clear that cows were central to his analysis, but it is precisely in this transformation of native knowledge into anthropological knowledge that lies his extraordinary originality and theoretical merit. And because, after all, the

theme of the work is social organization, not cows, Evans-Pritchard, not the Nuers, became one of the great specialists in our discipline, endowed with recognized epistemological authority.

Therefore, in the process of knowledge production in anthropology, the epistemological authority of the natives must become the epistemological authority of the anthropologist. This allows us to understand another classic situation in the history of the discipline, namely, the fact that Malinowski is recognized as a great ethnographer, but a terrible theorist. It can be said that he was not as successful as his student Evans-Pritchard in building his own knowledge. His importance for anthropology is due to the work he carried out in learning native knowledge, and in the formalization of ethnography as: *modus operandi*, which came to be seen as a necessary, but not sufficient, condition for anthropological knowledge.

At this point, Ingold's assertion that anthropology is not ethnography is pertinent, as ethnography is just the description of the life of a people, while anthropology goes beyond, albeit based on it (Ingold, 2011). Malinowski's work, he said, in an ironic but true statement, was so well done that it allowed Marcel Mauss to come up with better theories than his own. In doing so, the French author became a reference in anthropology (Mauss, 2003). Therefore, we can say that he completed the cycle of production of anthropological knowledge that Malinowski left unfinished.

It is worth noting that Mauss' authority is epistemological without being ethnographic. He could never proudly say that he "was there", in the sense that Clifford (2011) tells us. Like many others, he was able to benefit from the work carried out by fellow ethnographers. In this sense, ethnographic authority, although it results from a characteristically individual/monographic work, is mandatorily transferable to other individuals, being able (and sometimes must) circulate among the members of the ethnographer's community and serving as a basis for the construction of more importante authority, which is the epistemological, that is, the knowledge itself.

The fact that several important names in twentieth-century anthropology were not great ethnographers suggests precisely that, although ethnography is an essential and defining category of our discipline, mainly because of the fusion it promotes between theory and research, this fusion does not necessarily occur in *persona* of the ethnographer, as it is often said, but in a work cycle, which culminates in the production of knowledge and may or may not be carried out in its entirety by the same individual.

It is also important to point out that the construction of the anthropologist's epistemological authority does not come at the expense of native authority: the Nuer continued to be considered great specialists in cows, even after Evans-Pritchard returned to England, presented his findings and became a great specialist in social organization; in the same way, Trobriandes continued to be considered great merchants, even after Malinowski and Mauss transformed the history of anthropology with their analyzes of symbolic exchanges.

In other words, epistemological authority in this context of classical anthropology did not imply the abrogation of native authority, mainly because, as stated, these two authorities were based on different knowledge. A decisive contribution to this peaceful coexistence was the fact that, at the turn of the 20th century, anthropologists and natives were not only inserted in different cultures, but geographically separated, without any risk of epistemological competition between them.

STUDIES UP: THE NATIVES IN POSITIONS OF POWER

In the 20th century, one of the great issues of anthropology was the displacement of ethnography towards the society of the anthropologist himself, that is, the discipline stopped studying only the "other", in a radical sense, and started to study groups and social phenomena as very close to the researcher. From urban anthropology, to science and technology studies, through to the anthropology of peasant societies and post-colonial studies, this question crossed different areas of anthropology. Although an anthropology of the center has never been consolidated, as Bruno Latour (2004) suggested, perhaps because it has never been possible to discover exactly where the center of our society is, this issue has raised important reflections.

Particularly interesting, for the purposes of this article, is the reflection that Laura Nader offers us when she proposes what she calls *study up*, that is, the study of groups in high positions in society, opposing the historically predominant tendency to study lower groups (*study down*), that is, the poor, peripheral, minorities, excluded, etc. (Nader, 1972). In fact, Nader tried to show, through examples, that the *studies up* were already proliferating in North American anthropology.

According to the author, there are several theoretical and methodological benefits associated with these studies, arising both from the opening of new fields of investigation, as well as from the sophistication of the look we cast on already established fields. Thus, her work sought to recognize the importance and draw lessons from this new direction taken by the discipline.

The emergence of *studies up* is directly related to the redirection of the anthropological gaze towards the society of the observer himself, because, still according to Nader, this occurred as anthropology students began to formulate research projects on concrete social and political issues, which immediately affected their lives and generated in them some kind of indignation. The expectation of these students was to discuss these issues through the study of public and private institutions, such as, in the example mentioned by her, a government agency responsible for the control of air pollution caused by industries. In this study, the authors concluded that the agency provided a more useful service to the industries themselves than to the citizens, as it worked closely with the former to ensure that their activities met the requirements of the legislation.

In this sense, the *studies up* promote a double movement in anthropology: not just upwards but also inwards – they are studies of groups that occupy high positions in society and, at the same time, closer to the anthropologist than the natives of distant islands. Nader seems slightly to despise this second sense of movement by minimizing practical difficulties related to negotiations for entry into the field and ethical issues that may arise in these studies, as if, in this, they are not very different from studies of societies located in others. continents. Hugh Gusterson, commenting on the resumption of *studies up* in the 1990s, on the contrary, it shows that proximity to the natives increases these difficulties, and may even make the anthropologist's work unfeasible or dissuade him from the idea of conducting essentially ethnographic research (Gusterson, 1997).

In my view, the most important thing, however, is that, by definition, ranking a group as high or low depends on the criteria we use to locate it in the social structure and compare it with other actors and groups. Nader does not bother to rigorously establish these criteria, as he is contrasting typical categories: politicians, businessmen and

bureaucrats, on the one hand; peasants, poor workers, residents of peripheral areas and colonized societies, on the other. On virtually any criterion, the former would be considered socially above the rest.

Furthermore, she compares the positions of different social groups that we take as natives, but never of the latter in relation to anthropologists. This is an interesting exercise, as it forces us to think about our own position in the social structure, or at least contrast it with that of the natives. In other words, we are faced with the following question: specifically considering the anthropologist-native relationship, which allows us to say that we are facing a *study up* or *study down*? Studies of peasant societies, poor workers, peripheral groups, etc. would probably be considered: *studies down*, for, in the United States, as in Brazil, university professors/researchers generally find themselves in higher social positions than these groups, taking into account practically any criterion of social stratification.

As for the studies of politicians, businessmen and bureaucrats, which criteria would allow us to say that they are: *studies up* or *studies down*? If it is political and economic power, it is possible that it is *studies up*, for presumably they are closer to the apparatuses of state and capital than anthropologists are. However, if the criterion is the theme with which I started this article, namely, the epistemological authority (the competence to produce knowledge), perhaps we can say that they are *studies down*, for, presumably, anthropologists are closest to the field of science, which, in modern Western societies, is the quintessential place for the production of knowledge, though not the only one.

Mais adiante, veremos que esses questionamentos são fundamentais para entendermos o caso de pesquisas como minha tese de doutorado, em que os nativos ocupavam uma posição elevada na estrutura social não apenas em comparação a outros grupos tradicionalmente estudados pela antropologia, mas em relação ao próprio antropólogo, sendo essa posição derivada não simplesmente de seu poder político ou econômico, mas de seu lugar no mundo da ciência.

Before, however, let us ask ourselves: how does the construction of anthropological knowledge (and of the epistemological authority of the anthropologist) occur in the *studies up*? What is the difference from classical ethnographies? At first, the procedure is the same: first, the anthropologist recognizes the authority of the natives and admits that he needs to learn from them about their ways of acting and thinking.

This is, it seems to me, the meaning of the fact, described by Nader, that the authors of the research on the air pollution control agency proposed to enter the field as *if they were laymen*, that is, recognizing that, in a way, they were not lay people, as the agency served them as citizens, but that they would need to put themselves in the position of apprentices, as this is an ethnographic presupposition. Then they discovered that they were, in fact, laymen and that they knew next to nothing about the universe studied (Nader, 1972).

In a second moment, the anthropologist, through his craft, that is, his reflections, generalizations, interpretations, abstractions, etc., transforms native knowledge into anthropological knowledge, then getting to know other things and under a different perspective than the natives know. And, as has been said, this transformation does not automatically imply the revocation of the authority given to the natives.

The difference (or the problem) is that, unlike classical ethnographies, in *studies up*, the native's society is the same as the anthropologist's. Socially, culturally and

geographically they are very close. The *lócus* of validity of the epistemological authority of both is, in a way, the same. The world to which their knowledge refers is also, to some extent, the same. Thus, when the anthropologist constructs and proclaims his knowledge, having as a base (or raw material) the knowledge of the natives, there is an overlap between them. In other words, these knowledges meet and potentially confront each other.

At this point, the issue arises of the reception of the anthropologist's knowledge by the natives and how this creates new relationships between them. Of course, already in the context of classical ethnographies, natives could (and eventually did) come into contact with the works of anthropologists. But, undeniably, this theme emerges with much more force when ethnographies start to be carried out in the anthropologist's own society.

Here, the same, real possibilities for cooperation or conflict between anthropologist and native arise. Cooperation can occur when the anthropologist's epistemological authority does not necessarily imply the abrogation of native authority, but rather the construction of another regime of authority, which may even be critical, but possibly supplementary, perhaps validating the natives' authority. In these cases, reports on the expectations created by the natives (often frustrated, unfortunately) of possible contributions that an anthropological research could bring to their activities are common.

Conflict can occur when the anthropologist's epistemological authority partially or totally implies the revocation of native authority, that is, when a dispute between this knowledge is actually established. This is the case, for example, of anthropological analyzes that aim to unveil aspects of the natives' lives in which they themselves do not recognize themselves or to address criticisms that seem inadmissible to them.

This is also the case for research on social phenomena against which there is a consolidated militancy among anthropologists themselves, such as a feminist study on the culture of patriarchy or a Marxist analysis of labor relations in a neoliberal context. In this case, what is at stake is precisely, as a premise, the objective of subverting the knowledge constituted in these worlds about these worlds.

Nader herself explains that the *studies up* resume a critically and politically engaged vocation that, according to her, has marked anthropology since its foundation (at least in its North American trajectory). By engaging in certain causes, anthropologists admit the possibility of coming into confrontation with the natives. In this sense, the revocation of native epistemological authority is not for the occasional or contingent anthropologist, but rather a constituent of his work.

Gusterson (1997) reiterates this view explaining that the resumption of *studies up*, in the 1990s, it was associated with the return of Marxism and the strengthening of feminism and Foucault's theories of power, that is, intellectual movements linked to some form of political militancy.

Of course, to paraphrase Clifford (1986), everything depends on the interpretation that the natives make of the interpretations that the anthropologists make of the interpretations that the natives had already made of themselves.

However, this possibility of cooperation or conflict between the anthropologist and the native may never take place in practice or be a deaf dialogue, as the natives, even socially, culturally and geographically very close to the anthropologist, often do not even take notice of the published works. about them, either because they are not interested or because anthropologists fail to give them feedback on

the research. At other times, with some reason, they find the analyzes obscure and tedious.

It is even possible that even the most severe criticisms or the most promising theoretical-political convergences do not come to directly impact the natives' lives; or that conventional means of disseminating the anthropologist's work – articles, conferences, lectures – are not part of the routine of the studied groups. Thus, although increasingly frequent, the engagement between natives and anthropologists in close relations of cooperation or conflict is still a rare development in anthropological research.

So, in *studies up* and in research carried out in the anthropologist's own society, although there is an overlap between the knowledge of the native and the anthropologist, the construction of the latter does not depend on the relationship he establishes with the native, nor on how he deals with this situation of overlap and reception of your knowledge. Inserted in the academic world, the researcher has the prerogative of creating his theories, defending his thesis, criticizing or appropriating the ideas and practices of the natives without having to negotiate with them, nor be confronted about what he says or writes. Of course, the anthropologist always needs to make his entry into the field viable. But, once this stage has been overcome, he will only return to relating to the natives if he deliberately chooses to get involved in their activities, either to collaborate with them or to confront them politically.

In the case of my doctoral research, it could be said that it was a study up, because, as mentioned, the people studied were part of a power group, which occupied a high position in our social structure. Furthermore, as has also been said, this power was not only political and economic, but, so to speak, scientific, as the knowledge they held on the topics covered came from both their personal and professional experiences in the field of ST&I policies (that is, from their condition as natives), as well as from their academic/ scientific production as scholars of the subject (that is, from their condition as specialists). In other words, they were natives endowed with an epistemological/scientific authority.

This peculiar position occupied by the natives in my research had numerous consequences, among which I highlight the following. First of all, the academic environment, of which I was a part, was not foreign to them: on the contrary, it was a place to which they also belonged. The means of disseminating my knowledge – articles, conferences, etc. – were not alien to them: on the contrary, they were means in which they also divulged theirs. If they read my thesis, they would not find it exotic: on the contrary, they would debate it with me. The main scientific journals on science, technology and innovation policies in Brazil have, in their editorial boards, some of the natives/ specialists with whom I came in contact, directly or indirectly, during my research.

So, unlike what usually happens in *studies up*, I did not have, even in the academic environment, the possibility of writing about the natives without worrying about their opinion. There was no safe place to talk about them. even if it was my will, it would be practically impossible to establish a deaf dialogue with them or to evade the responsibility of presenting the results of my research to them. On the contrary, I would be practically obliged to establish a frank and direct dialogue with them.

It is true that I could insist on the escape. I could take shelter, not in my culture, as in classical ethnographies, nor in the academic environment, as in studies up and in research carried out in the anthropologist's society, but specifically in the cultural and epistemological universe of anthropology,

that is, to publish articles in anthropology journals, which economists and engineers probably do not read, and present papers at conferences in which they do not participate. I could, after all, cling to the difference that in fact always exists between the knowledge of the anthropologist and the native; claiming that we operate in different epistemological regimes and that, therefore, our knowledge does not necessarily need to confront each other. Like some science studies, which claim not to analyze science itself, but rather its social aspects (Latour, 1997), I could say that my thesis was not about ST&I policies, but about some specific dimension that was not of interest from them, which would protect at least some boundary between me and them.

However, in addition to being doubtful, this strategy would compromise the development of my work as a researcher, as one of the central debates in my research is precisely with the bibliography on ST&I policies in Brazil, whose authors are often the natives themselves. At first, this bibliography served me as field material. Like any anthropologist, I needed to read what my natives wrote. In a second moment, it seemed inevitable to make this reading part of my theoretical framework, either to incorporate their concepts or to refute them. Therefore, my option was not to confine those people to the place of natives, nor to avoid the theoretical debate to be held with them. On the contrary, I assumed and took to the last consequences the ambivalence of the relationship established between us[1].

In other words, the theoretical dialogue and confrontation with the natives became essential not only for the construction of my knowledge, but also for my own epistemological authority as a researcher. Unlike classical ethnographies, in which Evans-Pritchard was able to know lineage systems and social organization, after studying the Nuer, who knew cows, it was not possible, in my research, to know anything other than what the natives already knew. Unlike the *studies up* and from the research carried out in the anthropologist's society, it was not possible to build knowledge and authority on the subject through innocuous criticism or a deaf dialogue with the natives, as the academic world did not guarantee this unilateral rupture.

Therefore, Ingold's (2019) definition of anthropology as "doing together" or "doing with" people applied perfectly to my research. That's exactly what I found myself doing, but not because of a deliberate decision on my part, but because of the relationship that was forcibly established between me and the natives. What I intended to "do" as an anthropologist, the natives/experts had been doing for decades, and I didn't have the authority to do without them.

THE NATIVES AS THEORETICAL INTERLOCUTORS

The second consequence of the peculiar position occupied by the natives of my research is that they found themselves in a situation that I will call epistemological prevalence or advantage over me: I was just a scientist/researcher, trying to describe and analyze their world; they, in addition to scientists/researchers, dedicated to this same task, were also members of that same world. Their knowledge derived not only from their insertion in the field of ST&I policies, that is, from their condition as natives, but also from their insertion in the academic/scientific world, as researchers on the subject. Therefore, they had the epistemological authority to talk about the themes studied not only because they were natives, inserted

1. Nevertheless, when defending my doctoral thesis, I chose not to invite the professor from the Institute of Economics who had participated in the qualifying panel. Otherwise, I would give the native the prerogative to decide whether my research was worthy of conferring on me the title of Doctor of Anthropology. That was the only barrier I kept erected between them and me.

in that universe, but also because they were scientists/researchers of the theme.

The ideas of advantage and epistemological level refer us to Viveiros de Castro (2002), whose reflection begins with an explanation of what would be, conventionally, the anthropological "rules of the game". According to these rules, according to the author, the anthropologist-native relationship would be mediated by the anthropological concept of culture, which, due to its characteristics, convinces us that both share the condition of members of some culture. This can even be the same. Consequently, the discourses and knowledge of both must be understood as culturally constituted. In this sense, the idea of culture establishes equality, at least in principle, between the two characters involved in the anthropological game. It is this, according to him, that allows the anthropologist to talk about the native, as it is this shared condition of cultural being that allows the former to establish a relationship between his knowledge and that of the latter.

However, even according to these rules, the ways in which anthropologists and natives are inserted in their cultures are different. The native is understood as a product of their culture. in the words of the author: "*What makes the native a native is the presupposition, on the part of the anthropologist, that the relation of the former to his culture is natural, that is, intrinsic and spontaneous, and, if possible, not reflexive; even better if it's unconscious*" (Viveiros de Castro, 2002, p. 1). The anthropologist, on the other hand, inserts himself into his culture in a conscious, reflexive, active way, that is, mediated by processes he knows and controls.

According to Viveiros de Castro, this differentiated insertion on the part of the anthropologist in their culture is due to the fact that, by definition, it is anthropology itself that conceives and dominates the concept of culture. So, in the words of the author, "*The anthropologist necessarily uses his culture; the native is sufficiently used by your*" (same same). In this sense, he concludes that the anthropologist's discourse enjoys a certain epistemological advantage over the native's discourse, as only the former knows and dominates the concept that mediates the relationship established between them in the knowledge production process.

Faced with this preliminary conclusion, Viveiros de Castro presents and takes to its ultimate consequences the proposal for an anthropology that refuses this advantage and places the anthropologist's and the native's discourses on the same epistemological level or plane. With this, the idea is, among other things, to allow the speech of the latter to actively produce a series of effects on the speech of the former. Critically paraphrasing Geertz, he states, in a classic passage, that it is necessary to go beyond the recognition that "we are all natives" and decree, after all, that "we are all anthropologists", that is, that it is possible, albeit on an experimental basis, establish a relationship of knowledge with the natives, in which the speeches of one and the other are, in fact and in law, on equal terms.

This proposal has a series of theoretical and political implications, which shape a particular and powerful way of thinking about anthropology and knowledge itself. For the purposes of this article, the image that the author offers us of the relationships established by the anthropologist with the natives during field research and how he acts in the midst of these relationships is particularly interesting for the purposes of this article.

First of all, it is noteworthy that the author was able to decode the rules of the anthropological game, characterizing only the relations between anthropologists and natives, and not the relations of anthropologists among themselves and with other theorists.

This is perhaps unnecessary for the author's objectives, but for the reflections conducted here, it is crucial, as what I am calling epistemological authority is built on the relationship between anthropologists and natives and also with their theoretical interlocutors.

Historically, such interlocutors have been not only fellow anthropologists, but also sociologists, political scientists, jurists, economists, biologists, psychologists, historians, in short, all those scientists/specialists with whom we debated our concepts and confronted our theses on a wide range of subjects. Our interlocutors have also been representatives of various sectors of society, such as the State, the market, social movements, whose knowledge directly or indirectly dialogues with properly scientific knowledge.

At the end of: *Ensaio sobre a dádiva (Gift essay)*, for example, Marcel Mauss presents several conclusions, addressed to several different interlocutors: sociologists, economists, moralists (their presence illustrates precisely the scope of the theoretical dialogue of anthropology beyond the scientific/academic universe). Cited earlier as an example of epistemological authority constructed through the ethnographic authority of others, Mauss would not have produced his work had it not been for his fellow ethnographers. Therefore, as important as the anthropologist's relationship with the native is his relationship with theoretical interlocutors – and it is precisely the intersection of these groups that constitutes the problem discussed here.

This brings us back to Ingold (2019). In the midst of the analysis of the anthropologist-native relationship, he also presents his view on the relationship between anthropologists and other scientists/specialists, and their own performance (or not) as such. He says:

> No other discipline is more strategically positioned to apply the weight of human experience, in all spheres of life, to questions about how to forge a dignified world for future generations to inhabit. However, in public debates on these questions, anthropologists are, to a large extent, notable for their absence. Experts from various disciplines strut on stage, offering their partial assessments of our place in the world and their predictions for the future. But where are the anthropologists? (...). The public, understandably, looks to academic researchers to provide answers to their questions. But the likely reaction of anthropologists is to censure their interlocutors, expose their implicit premises, observe that other peoples – who do not make these assumptions – would pose the questions differently. There is no easy answer. Anthropology doesn't tell you what you want to know; it shakes the foundations of what you thought you already knew. (...). This can be uncomfortable. And the commitment to taking others seriously makes it inconceivable for anthropologists to pursue the strategy – adopted by so many authors of science – of exploring the prior interests of their readers and providing the data and ideas, flavored with novelty, to satisfy them. them. (P.).

For Ingold, objectively answering the same questions as our interlocutors and actively participating in the same public debates that they constitute acts of intellectual dishonesty or epistemological suicide. Thus, if the anthropologist's relationship with the natives is, in his view, one of total engagement and collaboration, with other scientists/specialists, any positive relationship seems unfeasible. Supposedly, he defends the dialogue between anthropology and the sciences, but this would require a radical change in the ways of acting and thinking of scientists.

Apart from the possibility of such a change, the question that arises is: and when the natives of the research are precisely those scientists/experts whom he criticizes so severely? Is it

possible, in that case, to take them seriously, as he proposes that we do in relation to natives in general? Does this proposal apply to natives inserted in positions of political power and epistemological authority? Will anthropology still consist of "doing together and with" them when "they" are scientists/specialists? I believe so, and that's what I tried to do when I found myself in this situation.

For this, however, our attitude towards them cannot be such that it distances us and silences us even more in the public debate in which they participate. Using a metaphor opposite to Ingold's, Stengers (2002) calls this stance that distances, silences and makes the debate unfeasible as irony.

In contrast, the author defends an attitude of humor towards the sciences, which, for her, brings together and allows for dialogue, even if in a critical and confrontational tone.

In my view, a fundamental condition for adopting this posture is to recognize that we are also scientists and that we speak from the same place or epistemological level as them, even though we do not speak the same thing as them. This does not mean defending the superiority or autonomy of science in relation to other sectors of society, but rather admitting that science constitutes a specific place of knowledge production and participation in public debate.

Viveiros de Castro states that "the native certainly thinks like the anthropologist", but "very likely, he does not think like the anthropologist" (2002, p. 119, emphasis in the original). Similarly, we must recognize that we anthropologists are scientists like everyone else, even if we are not scientists like everyone else. This means that, when natives are specialists, it is still necessary, as the author proposes, to place themselves on the same epistemological level as them. The problem is that, in this case, such equivalence results from a different movement. It is not a question of refusing an epistemological superiority or advantage, as it has simply not been granted to us.

Studying anthropological scientists/specialists, endowed with not only epistemological authority, in a generic sense, but scientific, in that particular sense, implies dealing with natives in relation to whom it is not possible to forge a place of epistemological superiority, whether through the concept of culture or any other the other, because they are not simply inserted in this "cultural world", and are unconsciously determined by it. It is they themselves, not anthropologists, who legitimately construct, dominate and control the concepts used to describe and analyze this world. To imagine this place, in the case of these natives, would sound like colonial arrogance, but like a delusion. This was once possible and with other groups that anthropology studied; today, or at least with these natives, it is not.

"Culture, for me, is Caetano Veloso and Chico Buarque", a native – a notable researcher, director of an important area at Finep – told me untimely when I asked him if he agreed with the widespread thesis, according to which, in Brazil, lack of "culture of innovation". This thesis, regardless of its merit, represents a possibility of building an anthropological perspective on the theme of policies to support technological innovation in Brazil. Consequently, it allows the anthropologist to claim his epistemological/scientific authority on this topic. There are many other possibilities. What I have been trying to explore is the identification of an epistemological change in Brazilian science as an effect of policies to support innovation, a fact to which the natives/specialists on the subject have not paid attention and which opens a channel of dialogue with the anthropology of science (Pereira, 2020).

Whatever the path, what distinguishes the situation described in this article from classical anthropology and even from most research carried out in the researcher's own society is that the construction of anthropological knowledge and the epistemological authority of the anthropologist depend, albeit indirectly, on of a direct confrontation with the natives.

Therefore, reiterating Gusterson's conclusion, I understand that it is necessary to insist on dialogue with natives positioned in places of power – not a deaf and innocuous dialogue, but an active and frank one. The difference, as I tried to show, is that this commitment may not be configured as a deliberate act on the part of the anthropologist, but as an unavoidable stage in the process of production of knowledge in our discipline. In this situation, the anthropologist's effort to place himself on the same epistemological level as the natives is the opposite of that described by Viveiros de Castro: it is about making our speeches and knowledge produce effects on their speeches and knowledge.

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