Rethinking Fieldwork and Ethnographic Writing

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Colombian anthropologist Luis Guillermo Vasco participated in pathbreaking collaborative research with the History Committee of the traditional authorities (or cabildo) of the indigenous community of Guambía, in southwestern highland Colombia. The collaboration resulted in a series of publications, including the book, Guambianos: Hijos del aroiris y del agua *(Bogotá: Los Cuatro Elementos, 1998), co-authored with Guambiano elder Abelino Dagua Hurtado and Guambiano researcher Misael Aranda. The research also made far-reaching contributions to the historical self-consciousness and the political agenda of the Guambiano community itself. In this article Vasco reflects on the nature of collaborative research methods.*[1]

During the 1970s a broad-based questioning of ethnography and its purpose unfolded in Colombia. In part, the origins of this discussion came out of a group of anthropologists with whom I was affiliated, a current that has been somewhat inappropriately called the "anthropology of debate" (Arocha 1984: 90, 97–99). I see the label as inappropriate because there never was a true debate, and those who disagreed with our critique preferred, for the most part, to remain silent.

Other academics emphasized in particular an important—but not fundamental—issue: our need to achieve wider dissemination of our research results and to ensure that a broader readership understood our writings, given that the language we used at the time was overly specialized, comprehensible exclusively to "initiates," as well as heavy, flat, lifeless, and tiresome.

This debate was also on the rise in other locations, especially in North America, but there are significant differences between what was occurring in Colombia and in the North. While in the United States the central thrust involved writing as a means of communicating research results, the growing presence of a strong indigenous movement in Colombia led us to question the very ways we engaged in research, above all in the field. For us, the key question was: How can we achieve a complete transformation of the anthropologist's craft?

We felt that writing was a secondary issue, although it did come up in our reflections. We focused on a broader and more important set of problems, given the conditions of our country: Why and for whom should we pursue anthropological research? We did not believe that rethinking the literary forms of communication of our research results was of the essence; instead we proposed a reconsideration of the very forms taken by our research as well as the objectives we hoped to fulfill through our work, a reconsideration that in itself would determine the final results, including the nature of our writing.

Moreover, the overwhelming majority of the indigenous people, blacks, and peasants to whom our work was directed were illiterate. Many Native people were monolingual in their own languages, which at the time lacked alphabets that could have made literacy possible; the few alphabets that existed were the products of the Summer Institute of Linguistics (SIL), an unmistakable enemy of the indigenous struggle, and for this reason many indigenous people refused to use these writing systems.[2] Our general position was to refuse to put the results of our work in writing.

We believed that it was not possible to transform ethnographic writing in a substantial way, except by modifying field methodologies. That is to say, changes in writing only impact form, as we note in the postmodernist current that has been most closely concerned with this issue. It is clear today that the postmodernist rethinking of writing has basically remained at a theoretical level, without achieving the objec-

tives it proposed. Hence some authors' affirmation that postmodernists cannot move beyond a declaration of objectives; very few works have emerged from their central proposals.[3]

In contrast, we focused on the central principle that ethnographic work should support the interests of those social sectors who have traditionally constituted the objects of anthropological study, particularly indigenous peoples, who at that moment made up the most politically dynamic popular sector in Colombia. We wanted to support them through our research.

This was not an entirely new affirmation. At the end of the 1960s a group of social scientists came together in the Rosca de Investigación y Acción Social (Circle of Research and Social Action), later known simply as La Rosca, a group led by Orlando Fals Borda and Victor Daniel Bonilla. La Rosca's publications strongly criticized the existing orientation of social research in the Colombian intellectual environment and called for a social science at the service of Colombians, including peasants, indigenous people, and blacks.

This group developed a new approach called participatory action research (PAR). Later, its members moved toward what they called activist research, an approach that was more committed to transforming social relations. They established research links with various social sectors in engaged in struggle, particularly with black groups on the Pacific coast, the indigenous people of Tolima and Cauca, and peasants on the Atlantic coast.[4]

On the Relationship between Theory and Practice

Such a proposition required that priority be given to the relationship between theory and practice, since La Rosca and our own collective saw practice as the fundamental objective of social research. We based ourselves on statements by Karl Marx, especially his thesis that "philosophers have only *interpreted* the world, in various ways; the point is to *change* it" (Marx 2001: 170) which we saw as applicable to the social sciences, among them anthropology. Second, concerning how to achieve this change, we agreed that "the coincidence of the changing of circumstances and of human activity or self-changing can be conceived and rationally understood only as *revolutionary practice*" (Marx 2001: 168). In the same text, practice is called a sensory human activity. It was from there

that the members of La Rosca derived the notion of activist research.

It was evident to me that for the ethnographer, the space of such practice could not be located in the scholar's office at academic headquarters but must be in the field, where the various social groups, in this case indigenous people, pursued their struggles to transform their basic living conditions. If, according to Marx, the field of practice was the only space in which it was possible to validate the knowledge emanating from research, then it would be essential for us to rethink fieldwork, a central facet of ethnography, and to examine the subject-object relationship, keeping in mind the fact that relations of power are established through ethnographic fieldwork. Although such relations develop between the researcher and those studied in the specific context of each investigation, they are framed and determined by much broader relations of power and domination between the national society and indigenous nationalities in Colombia.

The members of La Rosca also posed the need to abandon the closed world of theory that reigned in academic spaces and move instead toward privileging practice. Some of them even abandoned the university for many years to live among the groups with whom they were working. This was the case with Fals Borda, who left the National University and established himself for years on the Atlantic coast, forging relations with peasants whose struggle was led by the National Association of Peasant Users (ANUC).

For many others, in contrast, practice is still understood simply as a set of material activities, conceived to some degree in isolation from theory. Alternatively, practice is viewed as a collection of purely individual actions, the transformative potential of which is almost nil. These notions are quite distinct from that of transformative practice in the Marxist sense—from what some call "praxis."

Based on an erroneous notion of practice, the problem of space is frequently hidden, inexplicit, and peculiarly managed. A specific form of territoriality is created for the purposes of ethnographic research, in which there is a space reserved for practice and a different one for theory. But this is not just a case of conceptual differentiation: it is a spatial and temporal separation between the two, with one following the other in time, reinforced by a mutual exteriority. One is the world of the "objects of study," and the other is that of the researcher, the "subject."

The ethnographer moves in metropolitan urban space. The "other,"

in classical anthropology, is a rural being who belongs to the colonial world; the colonized. A relationship between the two develops to facilitate the entry of the ethnographer into the process of knowing. This encounter begins when the anthropologist begins a journey to that other world, preceded by conquerors, colonizers, missionaries, traders, and travelers (all eyewitnesses), whose vast stores of information played a preliminary but essential role in the development of anthropology by constituting the raw material for the writings of many of the first anthropologists, those whom we call "armchair anthropologists."

Exteriority obliges the ethnographer to abandon the accustomed space of academic activity to travel to the space where the objects of interest reside. There is no other possible way to enter into contact with them through one's sensory organs, which are the only mechanism for acquiring all the information needed for the work. It is not by accident that this mode of working in the field is known by the generic term "observation": that is, the prioritizing of the visual to obtain sensory knowledge in a direct way, although surveys and interviews incorporate at a secondary level the work of the other senses, especially hearing, to acquire indirect information about things the ethnographer cannot witness personally.

Thus there are differentiation and separation in the knowledge process, in both its spatial and its temporal dimensions, as a result of accepting knowledge as being merely sensory and of giving the highest priority to this form of knowledge. This may be a consequence of perceiving the relationship between sensory knowledge and rational knowledge as that of two successive and cumulative stages that unfold at different points in time and in different spaces, instead of paying attention to the dialectical relationship that unfolds, through practice, at each moment of the production of knowledge.

If, on the contrary, both forms of knowledge are considered to interact dialectically, they should take place simultaneously in the field. In this way, fieldwork is transformed, its epistemological status altered: instead of being simply a technique for collecting information, it becomes a method of knowing, of "producing" knowledge.

It is useful to remember that until the first decades of the twentieth century, observation meant, above all, direct observation, the ethnographer as an eyewitness. Participant observation only emerged and acquired significance later on, some time after Malinowski; in his view,

participant observation is secondary and consists of what he calls "submerging" oneself in native life.

In this way fieldwork was "invented" as an approach that has characterized anthropology during the last century and has become the framework defining anthropology and differentiating it from the other social sciences.

The Foundations of Empiricism

In this sense field research emerged out of the growing dominance of positivism in the social sciences, giving primacy to sensory knowledge or, in extreme cases, to the notion that this is the only kind of knowledge that exists and consequently the only kind of knowledge that can be acquired. Such an approach gave rise to an empiricism in which observation was the only source of facts, constituting the totality of knowledge. For knowledge to be complete, we had only to organize the data once we were back home, employing concepts derived from various theories that functioned, for the most part, not as categories of analysis but as principles, the role of which was merely to organize and classify information. The use of categories generally sought to establish the commonalities among the various elements of a society or between societies, with an emphasis on categories derived from empirical observation. Later fieldwork would be oriented exclusively toward verifying the presence of these same aspects in other societies, also through observation. This is how "participant-observation guides" and "guides for classifying cultural data" functioned and operated.

Despite his declared intentions, Malinowski was not able to abstract himself from concrete reality or transcend it. His concepts are empirical, mere generalizations. Even his theoretical essay "A Scientific Theory of Culture" (Malinowski 1990) is not truly theoretical, since he limits himself to a handful of generalizations regarding the human compulsion to satisfy biological needs. For Malinowski, at one level data must be constructed through observation. At a second level, the anthropologist must scrutinize and seek out those realities that are invisible to simple observation, those that are impossible to grasp directly in the field through the use of the senses; his tools included charts and diagrams. Then there is a third level of penetration: the rigorous synthesis that seeks out broader sets of relationships in order to evaluate their

weight within the cultural context. At this level, which has nothing to do with fieldwork, Malinowski appears to distance himself from empiricism. But this is not really the case, since his work advances by establishing relationships among empirical data so that he can establish empirical relationships and generalizations of a similar nature.

At the first level, one obtains immediate factual information. At the second, one generalizes, drawing broader assertions through the comparison of distinct cases; these assertions derive directly from fieldwork. At the third level, greater generalization is achieved by establishing correlations among earlier assertions. In this way a network of relationships emerges, organized at the empirical level through the immediate and tangible manipulation of information, a network in which and for which writing serves to shape knowledge into a totality.

According to Malinowski, the observer constructs the facts, which are "invisible" realities. Malinowski does just this in his study of land tenure in the Trobriand Islands, a system in which significance derives from the relationship of tenure to agriculture. He begins with statistical documentation based on concrete cases, a method that consists of collecting information on all those instances relevant to the topic, real or hypothetical; he is not using statistics in a literal sense, since he does not work with samples but with the totality of the cultural universe, nor does he treat his information quantitatively.

Malinowski did not believe in reducing the process of synthesis to a single occasion at the end of the research project but considered that there should be intermediate steps at which partial synthesis was carried out. However, he always maintained that in order to synthesize, the researcher had to distance himself from the field, in spite of the fact that these were the moments when theory and material reality should confront one another. One of the unintended results was that at the moment that the definitive synthesis pointed out gaps in the data, the gaps could not be filled because the researcher was once again at home with scant possibility of interacting directly with the group under study. We can only emphasize the fact that Malinowski never achieved this last level of synthesis in his work on the inhabitants of the Trobriand Islands.

In truth, Malinowski's synthesis does not involve abstraction but generalization. To abstract is to rise to a level above the concrete facts; it is the capacity to compare and relate that which is not comparable because it is different. Marx accomplishes this in his analysis of com-

modities. He compares all the different forms that commodities assume in the capitalist system, placed before his eyes by the market, each empirically distinct. Such a methodological comparison is possible only when it is founded on the assumption that the everyday process of exchange involves an implicit comparison of distinct commodities, since no one exchanges one good for another similar one. What commodities have in common cannot be perceived by the senses but only by the mind, through theoretical reflection. Thus Marx carries out a reductive process, moving from one reality to another, since in order to acquire knowledge of the essence of commodities—an essence that is invisible to the senses—it is necessary to go beyond empirical facts, beyond visible form, transcending it to move toward the higher level of value, the abstract quantity of labor that is necessary to produce commodities. Nevertheless, the validity of this knowledge is always confirmed by concrete empirical reality.

In contrast, Malinowski inquired in immediate and practical terms into the function of those institutions that formed the focus of his research, institutions for which the basis was biological and not social; here, the social is secondary in relation to the biological. Marx wrote that to speak of a person's need for food, clothing, etc., was a truism, and this is precisely what Malinowski offers in his theory. Leach (1974) believes he discovered the "epistemological basis of Malinowski's empiricism" in William James's pragmatism more than in C. S. Peirce.

But Malinowski is not the only scholar to have developed an empiricist approach to anthropology. So has Lévi-Strauss, who might be thought of as the antithesis of empiricism. His work is essentially formalist, in permanent movement, his attention centered on phenomena and not essences. For this reason, for Lévi-Strauss the senses do not matter, given that they only describe and do not explain. He illustrates how structures undergo transformations, according to formal theoretical laws, but he cannot explain the real, material, causes of these transformations. He can confirm that any given element is present in different societies, but he cannot explain why.

Are Ethnographic Methods Neutral?

One of the central dilemmas La Rosca faced was how to return the results of the process of acquiring knowledge to the groups to whom it

should have belonged, who were its beneficiaries. The fact that this was a fundamental problem, the solution to which absorbed a significant share of La Rosca's efforts, indicates that in spite of the important transformations that participatory action research injected into research methodologies—despite the group's creation of relationships with members of the social sectors with whom they were working and the high level of participation that these sectors achieved in the research process—in the final analysis the results still remained in the hands of the researchers, just as had always been the case. This provoked unease and concern among La Rosca members, who felt it was necessary to engage in extensive reflection and to give priority to the search for new ways of returning knowledge to those who should control and use it in their struggle to transform reality. In my opinion, the members of La Rosca were unable to solve this problem satisfactorily, despite their well-known achievements in creation of tools for communicating results to social sectors that were, for the most part, illiterate.

An analysis of La Rosca's practice, a close critical reading of their publications, and my own experience with indigenous peoples lead me to conclude that the contradictory results La Rosca obtained derive from the fact that they did not engage in a deep and critical evaluation of the research methods used by social scientists, something that they did accomplish with respect to theory and research agendas. As a consequence, they recognized the anti-popular character of theory and agendas but considered that existing research methods were neutral, without class character, and for this reason could be employed by anyone without generating any negative effects, although in their writings La Rosca did affirm the need to create new tools that complemented their theoretical and political proposals.

I concluded the opposite. These research techniques had been developed by social scientists working in the service of the enemies of the people, to reinforce domination and exploitation. In addition, these methods were intimately associated with the theories underlying the work of these scholars, which is why they fit together. It was through the use of such methods that relations of power between the subject and object of research were reproduced in the field. Despite the fact that they introduced new methodologies, when participatory action researchers grounded their work in these standard techniques they became, against their better judgment, the true subjects of the research process. For this reason, in the end the knowledge that had been gen-

erated remained in their control, in their heads, and not in the hands of the peasants, blacks, and indigenous peoples with whom and for whom they had worked. In reality, these groups never surrendered their status as objects of research, remaining just as they had always been.

Consequently, if it was essential for La Rosca to abandon the social theory that was in vogue at the time in favor of participatory action research, then it was also necessary for them to apply the same approach to their research methodology: engage in a critique that exposed its character, how it operated in the field, the mark it left on the process of generating knowledge and in the final research results, and, of course, its consequences in terms of the usefulness of their research for achieving the objectives of grassroots organizations. It was necessary to create new methodologies, in accordance with the theories that had been adopted to replace those of the social sciences.

The authoritarian character of methods like interviewing is clearly manifested in the fact that it is the ethnographers who ask questions about issues that are of interest to them, without leaving openings for the informants, in turn, to inquire into those topics of interest to them. We can also add the fact that researchers choose their informants according to their own selection criteria; it is generally unthinkable for the authorities of the group being studied to designate which people should work with an ethnographer. There is the additional issue of the nature of the questions being asked and the conditions and place and time of fieldwork, which are freely decided by the subject of the research. Clearly, such arrangements are an imposition that places informants in an unmistakable relation of subordination. Furthermore, this mode of operating inevitably introduces an ethnocentric quality to the research results, negating the basic criteria and the perspectives of the society being studied concerning the research topic.

The Separation of Intellectual from Material Labor

A second element in this discussion is the very character of knowledge and its sources. In spite of the fact that La Rosca's members broke with earlier social scientific tradition to validate the central position of the learning and experience of the people, they did not go far enough, and because they thought it was common sense, they relegated this knowledge to a subordinated position, theoretically and practically. While not

all popular knowledge and experience is true, and not all of it must be accepted, La Rosca erred when members used their own criteria as a filter to evaluate the validity of popular learning. That is why when the time came to return the results, as occurred in *Historia doble de la Costa* (Fals Borda 1979: 11), the book had an analytical, theoretical, and methodological channel that was generalizing and written in an academic language meant for researchers and advanced grassroots cadres, and another descriptive channel, which was concrete and anecdotal, composed in the language of storytelling, with many photographs, meant for common folk to read.[5]

There is yet an even more important backdrop to this story, one that reveals the real, objective, and decisive architecture of the division between the subject and the object of knowledge. I am referring to a form of the social division of labor characteristic of class-based societies: the separation of intellectual labor from manual or material labor. In these societies intellectual work is a process reserved for one class sector, the intellectuals, who belong to the petty bourgeoisie. That is what we must try to rupture, instead of accepting it as valid or unchangeable and adapting ourselves to it. Deep down, most of the researchers who subscribe to participatory action research presuppose that popular sectors are incapable of creating valid scientific knowledge, despite the fact that many PAR practitioners would consciously reject such an assertion. It is for this reason that they do not appropriate theoretical constructs or methodologies from popular sectors. And this is why it is not strange that in the end, knowledge winds up in the hands of the researchers. Furthermore, many PAR attempts at "returning information" are no more than simple processes of vulgarizing knowledge produced by others without breaking down the division between manual and intellectual labor.

Confrontation and Knowledge

In my opinion, to break with this state of affairs, which impedes the possibility of making room for the knowledge and ways of knowing of popular sectors, we must build upon the act of confrontation (which can also be called dialogue) in the creation of new research techniques and methodologies.[6] The development of these methods must be nourished by the forms of knowledge and theorizing of popular sectors. It is in confron-

tation with people that both our knowledge and theirs will be validated, refined, and combined to produce concepts, methods, and procedures for activist research (*investigación-acción*), ways of knowing and doing that are novel, creative, and, above all, transformative of reality.

From the moment I began to participate in the struggles of indigenous peoples of the Colombian departments of Cauca and Nariño in mid-1972, my attention was drawn to how Native people functioned in their assemblies, encounters, and other large meetings. They operated according to a system of "break-out groups" (*comisiones*), an obvious borrowing of terminology used to designate procedures that are in vogue among many sectors of national society, particularly among academics, trade unionists, and students.

But on closer examination I soon realized that there were significant differences from these other contexts. Usually, the activity begins with a general meeting in which all the participants present the necessary reports and establish the central agenda items to be considered. Then participants are distributed in "comisiones," the nature of which depends on who is participating; in some instances, people join one break-out group or another for linguistic or ethnic reasons: Guambianos, Nasas, Pastos, solidarity workers (*solidarios*), etc.[7]

There are no secretaries or note takers, although frequently one of the members is chosen to moderate. During all the time that the group meets, the different participants intervene to present their ideas and points of view, leading frequently to broad and sometimes very heated arguments; some speak over and over again, others intervene less frequently, and there are very few who do not participate. At the end of the time allotted to the group meetings—which in my experience may go on for two or three days—all members rejoin the general meeting, without having come up with explicit conclusions or a prepared report for the plenary session.

Once everyone comes together, the topics are revisited, and the various participants intervene afresh with their ideas, generating an ongoing discussion. Finally, the group analyzes what has to be done and how, and the meeting ends after agreement is reached.

The fact that the break-out group meetings I observed did not reach conclusions, and did not present reports to the plenary, and that everyone took part in the follow-up general meeting as though they had never worked in comisiones, was inexplicable to me. I tried to com-

pare it to what occurred in a text by Mao Tse-tung, "Oppose Book Worship," which proposes a research technique that involves "hold[ing] fact-finding meetings and undertak[ing] investigation through discussions." Mao adds that "this is the only way to get near the truth, the only way to draw conclusions" (Mao 1930). This means we must begin with the premise that it is the people of a place who know, and for this reason, the leadership must meet with groups of twenty to thirty people to discuss problems; each one contributes, and the solution is constructed collectively.

What is different is that for Mao, the organization itself and its leadership analyze the results of discussion and select the appropriate conclusions, in order to arrive at decisions and return them to the people in the form of directives for action. In the indigenous meetings, decisions are made by all, based on an exchange of ideas that takes place through conversation; governors, *cabildos*, and leaders do not make the decisions, but they do put them into action.[8]

I came to understand that the break-out groups of indigenous meetings were, clearly, research encounters, where knowledge of a problem was generated through discussion. In the course of this dialogue each participant confronted the knowledge of others, in order to arrive ultimately at a broad conclusion. Later, one of the members of the History Committee with whom I worked told me that everyone has the right to participate in the generation of knowledge, everyone knows his or her part, and collective discussion can lead to a group conclusion.

I was wrong when I surmised that these meetings did not reach conclusions. They did, although the conclusions did not take the same form as those with which I was familiar, nor were they recorded in writing. Later it became clear to me that at the small-group meetings and in the multiple discussions that developed there, conclusions emerged in the minds of all the participants: they came to acquire a greater knowledge of the problem than they had had before the meeting, since it was no longer personal knowledge but that of the entire group and validated through debate. This knowledge was expanded yet further in plenary sessions, where shared conclusions emerged out of an even larger group. Final decisions were thus supported by the shared conviction that they had been arrived at through collective discussion. It became evident to me that this was a clearly intellectual activity and that what people were engaged in was "research *mingas*," "knowledge mingas,"

the result of which transformed individual knowledge into collective knowledge, although it continued to exist individually in the minds of all of the participants.[9]

Our field methodology in Guambía was structured around the centrality of such research meetings: we understood that knowledge could not be generated individually, through the use of informants who recount their experiences to a researcher, but needed to happen in collective form, with the intervention of the greatest possible number of members of Guambiano society.

Ethnography and Power

Some ethnographers, including the postmodernists, identify the problem in terms of the power relations inherent in ethnographic research and propose to resolve it by identifying the written text as the space in which these relationships unfold, ignoring their presence in the material realities of fieldwork.

In contrast, my objective was to transcend these power relations in the field. To achieve this, it would be necessary to comprehend and reconsider the field as a space of practice, a space dedicated to the production of knowledge and the use of this knowledge to transform reality, thus validating this knowledge. But two indispensible conditions were necessary: research activities must be guided by a theory that is developed simultaneously with the fieldwork, and research should be a collective practice, not individual and isolated. This was made possible in our case by an organic relationship to the struggles of the indigenous movement. It ensured that our research objectives would be defined in light of the needs of the struggle, guided by a broad discussion organized by the cabildo council.

Intent upon modifying the relations of power inherent in the ethnographic text, various anthropologists have identified the problem as one of ethnographic authority, which, they observe, has been squarely in the ethnographer's hands. It is time to transfer authority to those who are the subject of the writing. But how can we truly transform power relations in the text if they have not been modified in the field, where they originate (not to mention in social conditions that the ethnographer is powerless to transform)?

Geertz (1989) conceptualizes the author in two senses: as the author

of a text and as the authority who makes decisions about the text. But it is possible to add a third sense, one who does something oneself; in other words, we can consider the problem of autonomy in ethnographic writing. By questioning authority in this final instance, we not only consider the author as an authority but also raise the possibility that writing can fill the role of a spokesperson, as something that speaks for another.

Geertz himself (1989) has characterized ethnographic research in terms of "being there and writing here." That is, the information is collected "there," but "here" is where it is worked and analyzed, knowledge is produced, and writing takes place. This statement implies that the author accepts traditional ethnographic writing practice, and it assumes that researchers and their own society, on the one hand, and the societies they study, on the other, are completely separate. Consequently, fieldwork is understood strictly as a stage of collecting information, as sensory knowledge "out there," in the space of those who are studied. Analysis and organization of this information happens in the space of the researchers, which is found "here."

In the face of this, the authority and power of the ethnographer are only broken when certain central assumptions are accepted and acted upon in the field:

- local participants assert control over the research;
- the ethnographer's opinion is but one of many;
- the ethnographer's opinion must be discussed with the local participants;
- local participants formulate their own research proposals;
- oral narratives are accepted as truth, and not mere discourses.[10]

As a result, the authority of the ethnographer should be equal—and on some occasions even subordinated—to that of local people. In this way joint action and a true dialogue characterized by the confrontation of perspectives will transform previous relations of domination. An example of this might be what occurred during the first six months of my work in the indigenous community of Guambía. We had adopted as our primary strategy a methodology centered on picture-maps (*mapas parlantes*), but we abandoned it following a decision by the cabildo.[11] The council felt the method would create problems, given the political situation at the time (the permanent presence of the M-19 guerrillas

and the Colombian army in Guambiano territory) and cabildo relationships with religious institutions (pressures by nuns to influence the new governor, who was closely allied with the convent). It was precisely the dimensions of armed conflict and religion that were emphasized during this phase of research.

But it is not only when local authorities set research objectives and guidelines that ethnographic authority is decentered, nor does it occur exclusively through the introduction of local knowledge forms, like the reflexive-investigative meetings that were central to our research technique. It comes with a new recognition of the status of the wisdom and knowledge of Native peoples. Much of this knowledge is contained in historical narratives, which anthropologists have called "myths," and which give concrete form to personages, relationships among them, activities, and events. Many ethnographers—Lévi-Strauss among them—have erroneously proposed these should be considered metaphors or symbols that ethnographers must interpret in order to determine what they are really expressing. Such an approach implies that there is a separation between the material world and the world of ideas that does not exist in these societies or, at least, is not completely developed there.

In my opinion, this owes to the fact that we are not dealing with contemplative knowledge but with knowledge that seeks to produce acts, as Marx suggests: a knowledge-act (*un saber-hacer*) embedded in daily life. In Guambiano thought the world of ideas and the material world are one, so that what might be called "material culture" is also part of an ideological constellation, as I have argued elsewhere.[12] This is the inverse of what occurs in structuralism, which argues for the independence of symbols from everyday life and defines humans as the producers of symbols and not as beings who work.

The Guambianos do not make this separation between object and idea, since none of them lives without both. To know is to travel the topography, because culture is imprinted in territory. To know is to grasp not only in the mind but also in the body: knowledge is not only thought but felt. Therefore theory and practice are not separate, and it is possible to think through things. As a result, their conceptual forms are different from ours. For Guambianos, abstractions are expressed through concrete forms, through concept-things (*cosas-conceptos*). For example, time is a snail that walks, as the Guambianos say. These con-

cept-things are built out of concrete material elements that exist in everyday life. Out of this arose our methodology of seeking concepts in everyday life.

Marx argued that the relationship between material reality and forms of consciousness among the earliest forms of society is different from that which exists in class society:

> The production of ideas, of conceptions, of consciousness, is at first directly interwoven with the material activity and the material intercourse of men, the language of real life. Conceiving, thinking, the mental intercourse of men, appear at this stage as the direct efflux of their material behaviour. (Marx 2001: 68)

That is to say, given that material work and intellectual work are not separate, material and ideas are not separate either; this does not mean that they are not differentiated or that it is impossible to locate material activities, on the one hand, and the constellation of ideas, on the other. Put another way, in this type of society ideas remain amply loaded with material things, and at the same time material activity is loaded with ideas. This, of course, radically contradicts the arguments of symbolic anthropology and even those of the broader current of thought from which they derive: structuralism, particularly in its Lévi-Straussian variety. Consequently, when we refer to these societies it is impossible to argue for the existence of possible worlds that are not at the same time lived worlds.

This notion of things was noted by Lévy-Bruhl (1966: 113), who thought that "the antithesis of matter and spirit, so familiar to us as to appear almost natural, does not exist in primitive mentality—or, at any rate, it interprets it differently from ourselves." As would be supposed, Lévy-Bruhl, who was not a materialist, interpreted mentality differently from Marx, especially in regard to how it originates. Elsdon Best (Best 1924: I, in Lévy-Bruhl 1966: 114) clearly expressed his thoughts on "primitive" concepts and the effects they produce in the minds of ethnographers: "Confusion is caused in our minds by the native terms denoting both material representations of immaterial qualities and immaterial representations of material objects." This helps us to understand why, for the Guambianos, knowledge exists objectively, outside of individuals. Consequently, what we must do is to observe knowledge and relate to it through the senses and the mind. This is what leads

shamans to use sight or vision as an essential tool for knowing, while in our society knowledge is a human creation.[13]

Nonetheless, not all Western theories of knowledge are contrary to indigenous ones. The Marxist thought of Frederick Engels (1940) argues for the existence of a dialectics of nature. Others, however, affirm that the dialectic is a creation of human thought and that Engels's vision is mechanistic; so, they would argue, we impose the dialectic on nature through our thought, given that it is not otherwise present there: nature in itself would not be dialectic.

In any event, if knowledge is embedded in things, it exists objectively there, in material reality, and can only be verified through practice—which is precisely what Marxism argues. This is contrary to the theories produced by the social sciences, which leave the application of knowledge to public servants, despite the fact that anthropology, particularly North American anthropology since the 1950s, has emphasized this in development programs that the United States introduces in the countries it controls or seeks to control, generating currents like that of directed cultural change, proposed by Willems (1964), and Foster's applied anthropology (1969).

Posing the problem as one of the relationship between the subject and object of investigation obscures the crux of the problem: the existence and exercise of social relations, not just those that occur between the dominant society to which the researcher belongs and the dominated or colonized societies under study but those that operate within the dominant society itself, and which involve, shape, and determine the behavior of the ethnographer. In reality, since people are social beings, the individual is not in reality individual but is a space in which multiple social relations intersect; the individual is the collection of social relations that converge in and determine that person.

Malinowski is credited with the creation of the ethnographic subject and, consequently, of the subject-object relationship.[14] The ethnographic subject is the materialization of the individual in the concrete field of ethnography and, as such, is a creation of bourgeois society, the product of private property and capitalism. Marx considered that the individual did not exist in primitive society, although of course individual people did, differing one from the other. That is to say, the individual did not exist as a social category, as a social subject counterposed to society.[15]

A person is a physical being and a social being, the product of society. As a social category, the individual is that person plus the constellation of social relations to which the person belongs. Hence it is possible to say that one is not one but all of the others. That is why Malinowski's notion of the ethnographer or the native as a personal subject is a fiction. To the contrary, Marx considers capitalists as the incarnation, the materialization, of social relations, in some cases to their detriment. The fact that they are so says nothing about individuals, nor does it imply that from this point of view they are monsters.

As a consequence, it is impossible to sustain the notion of the ethnographer as an individual; that is, as the personal subject of research. What I am as an individual is determined outside of me, by society. That is why the process of transformation can and should only begin outside. Going against popular notions in contemporary anthropology and in modern society, my subjectivity is objectively given outside of me. This breaks with the false contrast between subjectivity and objectivity posed by those systems of thought that seek to create—in order to manipulate—the fiction of an identity between the person and the individual, beginning with the incorrect assertion that the individual is natural and has always existed. To the contrary, the individual is a historical and social construct that develops alongside the appearance of the first class societies, during the process of decomposition of the primitive community; in that community the individual did not exist, only the collectivity, although it was made up of people, the raw material from which the community was made.

Posing an opposition between subject and object becomes a problem that is constructed on the foundations of the fiction of the identity between the person and the individual, and in this way it is transformed into a tool for domination. According to these criteria, the unequal relation between researcher and researched, between subject and object, appears as a voluntary question and, consequently, one that can disappear thanks to a voluntary act of the ethnographer, as if it were not founded in objective reality.

Malinowski himself does not deny subjectivity but isolates it by giving it a channel of expression in personal everyday life; he distinguishes this from science, where free reign cannot be given to the imagination or the sentiments of native people, and there is no place for the emotions. His methodology acknowledges the duality of human nature, of

sentiment and reason. Thus, both aspects must be taken into account in the process of data collection. In order to approach the natives' sentiments, Malinowski proposes use of the *corpus inscriptionem* as a tool. That is, it is necessary to record people's sentiments, to grasp their point of view and their explanations of things, as he did with the Trobriand Islanders when they navigated the Bay of Kiriwina. But for him, this is not part of knowledge; it is only the raw material with which the ethnographer will produce knowledge.

A distinct way of approaching this would transcend individuality, break with the fiction of the subject, and eliminate the notion that the subject of knowledge is the ethnographer. In reality the subject is the group, made up of both the ethnographer—who carries as baggage the identity-determining relations of another society—and the natives.

The field should become the space of encounter between subjectivity and objectivity, producing conditions under which their confrontation contributes to constructing a true ethnographic subject. As this practice unfolds it comes to demonstrate its validity, becoming the real and not simply the declared space for the encounter between objectivity and subjectivity. This involves generating knowledge during the process of change itself, since practice is the principal generator of knowledge. It does not involve first knowing and later applying this knowledge.

Life and Knowledge

In order to comprehend the epistemological proposal regarding fieldwork specified in my methodology of "collecting concepts from life," I must now examine the relationship between life and knowledge. I call attention to two central elements. The first is that knowledge exists objectively in things; it is not a human construct, as is believed in the West. The second is that knowledge is expressed through concept-things, material elements that are part of everyday life. For this reason, gaining access to them—"recognizing" them—can only occur in the course of everyday life, which entails living with the societies one studies as an essential element in the process of creating knowledge.

Anthropology envisions the contrary. For Lévi-Strauss, for example, everyday life has no explanatory value, and one needs to transcend it to be able to know, to gain access to a hidden reality; for Lévi-Strauss one must move from everyday life to reality—which is invisible—through a

reductive process. This involves reducing life to reality, forms to content, in a process built upon theoretical models or matrices. Although everyday life is his point of departure, it plays no further role in Lévi-Straussian thought. From that point on, theoretical thought is the only instrument that can generate knowledge. Fieldwork provides the entryway into knowing reality, at the same time that it is an obstacle to that knowledge. This is why everyday life and the elements it comprises are set aside, having no meaning in and of themselves but only when encompassed within a structure. This is what constitutes the relationship among elements, which are generally constant; what is modified are the relations among them, their combinations, although this occurs according to universal laws.

Another important aspect in Lévi-Straussian thought is the attribution to human beings of the characteristic of working at the unconscious level to produce the social world. For this reason, it is clear that humankind is not conceived of as a subject of our own history but rather as a passive object of circumstance, the determining factors being fundamentally biological; this makes this theory anti-human.

At first, disillusioned as he was with his own society, Lévi-Strauss sought indigenous people as sources of an alternative way of life. But he concluded that there is no perfect society and that there was no reason to tolerate among indigenous peoples that which he would not tolerate in his own society. His idea was that the anthropologist would construct a model of society, but it would be the task of others to use this model to achieve a better life in his society. He constructed his model by taking elements from all societies, decontextualizing them in time and in space; that is, uprooting them from the everyday life in which they were embedded. This allowed the model to be applicable to any society in any time and place, a clear process of "dehistorization." That is to say, since structuralism does not transform, it does not return to reality. This is what happens with Lévi-Strauss himself, who never returned to ethnographic fieldwork of the sort that produced his book, *Tristes Tropiques* (1992).

In contrast, for Marx, life both conceals reality and at the same time manifests it, although not presenting reality as it actually is. In his method one moves from the real and concrete to the "thought-of concrete" through a reductive process that is entirely different from that of Lévi-Strauss. If for Marx real life is not an explanation, it is what

must be explained. There is thus a formal similarity between the two authors, but not a methodological one, despite Lévi-Strauss's affirmation that one of the sources of his thought, together with geology and Freudian psychoanalysis, is Marx.

The Guambianos lend importance to life by affirming that knowing is walking (*recorrer*). But there are two ways to do this: first, as in the walking needed to complete the tasks of everyday life; second, as a research methodology. Walking the territory is a form of knowledge, whether this be accomplished bodily or through thought, although the two cannot be separated. This is what occurs when the Arhuacos of the Sierra Nevada de Santa Marta weave: men sit to weave at their looms, and while their fingers weave their lives, their minds travel in order to learn the sources of life.

Walking as a way of knowing among the Guambianos is also based on the fact that history is imprinted in space and is a way of collecting concepts in life. As Marx argued, there is no separation between the process of living and that of thinking or knowing life, although there are differences between them. When the ethnographer is the knowing subject but does not live the life of the society being studied, the process of research and knowledge building is isolated from life, when it should be part of life. In Guambía, research and knowledge are part of life and they both serve the struggle. In general, it is possible to affirm that in indigenous societies learning occurs while living in the everyday world. Knowledge is a creation; you learn to think about things by doing them.

Postmodernist anthropology, especially Tyler (1986), takes the antivitalist position of structuralist formalism to an extreme and speaks not of knowledge, but of evocation, arguing that there is no objective referent in a text's past because it is a mere creation. For him, we are speaking of a kind of nostalgia; this is a process of creation in which what matters are the ethnographer's sentiments—the facts are only a pretext. For this reason, he refers to the proximity of ethnography to poetry. Under such conditions, ethnography is more an art than the production of scientific knowledge.

Going against the pretentions of a Malinowskian ethnography that proposes to recreate reality in a text by positing its truth-value, Tyler emphatically declares that evocation does not bring reality to the here and now. For him, reality is, at best, a representation, not a presenta-

tion, and no representation poses problems of trustworthiness or reliability (*confiabilidad*). Thus ethnography does not allow us to know, nor is that its objective: what ethnography makes possible is the construction of an image of its object. Tyler quite rightly rejects the possibility that this representation can have political objectives; that is, that its objective is transformative.

To the contrary, in my work I am guided by the notion that there is no discontinuity between life and reality, although they are not the same; reality only exists as life and can only be experienced legitimately by living it. Life is experienced only through the senses: sight, smell, touch, etc., although if we stay at this level we will not comprehend it. In order to know what moves life, it is necessary to think it as well as to live it. Otherwise it is impossible to explain why it is how it is.

Lévi-Strauss is correct when he states that life itself does not contain its own explanation, something he shares with Marx. Life is incomprehensible in and of itself, and for this reason we must go further, in search of what reveals at the same time that it conceals. On this basis it is possible to begin to see life through other eyes, illuminated by knowledge, but we must return again to life to transform it, since if we only have thought, we change nothing.

This implies that the process of fetishizing reality is not merely a mental process or, as some have argued, a false consciousness. Instead, the process means a fact exists in everyday life and has its counterpart in consciousness, in the universe of ideas. For this reason traditional ethnography's principle of observing "over there" and knowing "here" is invalid, because life itself is the beginning, middle, and end of knowledge.

Indigenous Struggle, Territory, and Knowing

In my book on the *jaibanás* (shamans) among the Embera people (Vasco 1985), I proposed that there was an indissoluble relationship between space and time in this indigenous nationality, in which the category of time does not have its own independent expression, being formed instead in relation to the category of space, which occupies the preponderant position.

Later, when I joined the indigenous movement of southwestern Colombia, I realized that this association of space and time also existed

there and that it was expressed in the notion that history is imprinted in the territory: this is why you must walk territory to gain knowledge of it. In order to accumulate the knowledge necessary to participate effectively in support of the land-claims struggles in the *resguardos*, I had to walk through different spaces continuously, in particular those located along community boundaries.[16] This also happened when I visited the Pastos in the department of Nariño, where we made a long voyage that began in the resguardo of Males and extended to the warm lands that border on the Kwaiker. This occurred in Guambía, where I experienced exhausting treks along the mountaintops of the *páramo* and climbs to the highest peaks.[17]

When I arrived in Guambía to participate in a project of revitalizing historical memory (*recuperación de la historia*), an elderly woman asked about the objectives of my stay and felt sorry for me because she said I would surely tire myself.[18] When I asked her what she meant, she responded, "Knowing involves a lot of walking." As the project developed, it was clear that she had hit the nail on the head, pinpointing the crux of what is involved in a Guambiano method of acquiring knowledge, and something that is surely also present in other indigenous groups. In Guambía, walking not only allows one to know—to listen to—the territory, but at the same time provides the central axis of how territory is constructed.

Some time later some Guambiano elders explained to me in further depth how I should understand this process. They concluded by affirming that the loss of traditional knowledge and of Guambiano lifeways was due to the fact that people no longer walk but prefer to move around in vehicles. This breaks the lines of communication with the land, one of the essential sources of knowledge.

A central facet of our research in Guambía, introduced at the beginning of the project, was the active recollection (recuperación) of the toponymy, the place-names in the Wam (Guambiano) language. When I arrived in August 1987 this work had already begun with activities coordinated by the History Committee in resguardo schools, where teachers, students, and parents walked the lands of their districts (*veredas*) and drew maps, recording place-names in Guambiano. These maps were presented to the public at assemblies and, later, were exhibited at the Museum-House of Culture (Museo-Casa de la Cultura).

I noticed the maps when we began our task of historical reconstruc-

tion in the "office" the cabildo assigned us next to the museum. During a coffee break on one of my first days there, I looked at the maps and noted the name given in Spanish to a hill: Los Tres Jóvenes (the Three Youngsters), *Maatseretun* in Wam. I asked the committee members about the history of this mountain, and they said there was no history. I assured them—an oddity of my profession?—that there had to be one. One day, some three weeks later, one of the committee members arrived with the news that there was a story explaining the mountain's name. He told us that the night before, on his way home, he had met an elder on the road and they had continued together, talking. They were well on their way when in the distance, a bright sunset exposed the profile of Los Tres Jóvenes against the sky, and the elder told him that it had a history.

From there on in, we discovered that it was not sufficient to record place-names in the vernacular, although this was important because many of them had fallen into disuse and were no longer remembered, particularly by the youth. We also had to learn the histories associated with these place-names, stories that were at the same time part of Guambiano history and of its construction of territory. But there was more: these stories established the foundations of the role of each place in Guambiano life. Thus Maatseretun was traditionally a site that young people visited when they reached a certain age in order to learn what lay before them in life; this was a custom that few now followed, which some elders felt contributed to the disorientation experienced by today's youth.

On the basis of this information, we planned a variety of walks covering all of the resguardo lands, one of our objectives being the recovery (recuperación) of the names associated with various places. The members of the research group participated in these walks, accompanied by elders who lived on or worked those lands and knew them well; sometimes we were accompanied by other Guambianos and Guambianas who wanted to get to know their territory. As we walked, the elders recounted the place-names and their histories, sometimes differing on them, because not all elders agreed on the names or on the stories behind the names. All of this was recorded in maps and notes so that it could later be taken up in larger groups in order to produce, ultimately, a map of the entire resguardo, with its associated stories. The map would focus on the creation of Guambiano territory through

the history of the actions of its inhabitants and the ways that they have used the territory in their daily lives, historically and in the present.

Collecting Concepts in Everyday Life

On one occasion I asked some Guambianos with whom I was talking what would be the best route to arrive at the house of a *compañero* in another district. One of them explained to me that I should take the road down to the "Virgin's Fork" (La Horqueta de la Virgen) and then take the path up to the right. I remembered a crossroads not far away, and where the roads met there was a statue of the Virgin Mary, probably put there by the Missionary Sisters of Mother Laura. I thought I understood the directions. But to my surprise, the other Guambianos found the instructions hilarious: over and over they repeated, in Wam and in Spanish, "la horqueta de la virgen," and each time their laughter grew stronger and more prolonged. I could not understand what was so funny about the expression.

When I recounted my experience to the committee members, they likewise could not contain their laughter. They finally explained to me the meaning of *utik*, or "fork," which signifies generation or procreation. When an unmarried daughter leaves her home and returns with a child on her back, her parents say *utikmisra arrinkon*, that she left by one branch of the fork and returned by the other: she left on her own and she branched out, she lost her virginity. When the nuns placed a statue of the Virgin at the meeting of these two roads, they probably did not know they were making a joke, not through speech but with things.

Some time later on one of our walks, we crossed the lands reclaimed (*recuperadas*) from the large landowner Suszman, on our way to La Clara. Standing on level ground, we noted a number of snails in the black and humid soil. One of my Guambiano companions told me that this place was called Srurrapu, "snail," which I attributed to the presence of land snails there. But as we proceeded up the plain we turned onto a road that ran through a fallow lot that was covered with grass. The compañeros cut down the grasses with their machetes and a large and almost flat rock appeared, its surface completely covered with spiral petroglyphs: "Srurrapu," they said, pointing them out. And one person added: "This is history: a snail that walks."

It all became even clearer when we met an elderly woman on the road,

her head covered with a traditional Guambiano hat, a *kuarimpøtø*, woven in a spiral. The Guambianos call the hat a snail, a long braided ribbon made of reeds. The same compañero who had designated the snail petroglyph as history now explained that in this hat "you can read history."

These elements kept appearing in everyday life and in relationships. I have called them "concept-things," through which the Guambianos think and talk about their world, as they do in stories: through abstractions that condense their thoughts, their theories.

In the process of detecting, or "discovering" and appropriating this concept, we investigated and discussed the other contexts in Guambiano life in which it appeared and their different meanings, in order to comprehend what knowledge they contained.

We found that we required a conceptual framework to guide our analysis and to structure the knowledge emerging in the course of our research. The Guambiano concept-things—the utik, srurrapu, and kuarimpøtø already mentioned as well as others that were discovered in similar circumstances, such as *mayaelø*, *lata-lata*, *linchap*, and *kantø*—furnished the theoretical foundations of our analysis.[19] This led to an arduous process in which these concepts were compared with my own theoretical ideas and with others that came out of our readings.

When we were well advanced in the process, we held a workshop with government employees who work with the Guambianos. Some of them asked about relations between Guambía and Colombian society and about how Guambianos understood the concept of community. The next day one of the Guambianos asked to respond to the questions posed the night before. He expanded upon the principles that orient Guambiano life and what has happened to them under the system of exploitation by the dominant classes in Colombia. He took the example of the Gran Flota Mercante o Flota Mercante Grancolombiana (the Colombian merchant fleet), which had been in the news the night before, and he converted it into a theoretical tool that allowed him to analyze the relationship between Guambiano society and Colombian society. He used the concept of market as the foundation of the relations of control and exploitation of the Guambianos by Colombian society. Thus the Flota Mercante Grancolombiana took form as the exemplification of merchant capital, which is predominant in a region like Cauca, where the level of industrial development is extremely low and, of course, so is the movement of financial capital.

The compañero explained, moreover, that his analysis had taken form the night before in his kitchen, as he talked with his family about what had transpired that day in the workshop.[20] To our surprise, the compañero created and used one of these concept-things that characterize Guambiano thought, as they do among other indigenous people.

Everyday life is highly variable, but its structural elements are not invisible. There are things that are repeated, over and over, with little variation, and they provide us with a key. We must search out those elements in which they are lodged, in everyday life, where they can be experienced. They are the true "processors" of social life.

The collection of concepts from life does not refer to thoughts encapsulated in language but instead to practical thought that can only be partially grasped through words, as in surveys, interviews, and the like. It is necessary to live everyday life, to share activities and work, because that is where thought is lodged (that which some mistakenly call "ethnic thought"), and it is there that we can complement it with observation. Thought emerges as actions and objects in everyday activities, tied to them by "uses and customs" that lend them permanence and continuity. The word is not separate from thought.

When you live this life, with all its difficulties and problems, and work side by side with people in search of a solution to their problems, you note that they recognize these concepts and compare them to Western ones. We carry the latter in our heads and we cannot leave them behind when we go to the field, even though ethnographers have recommended that we arrive with a "blank slate." In the process, however, one's own notions are modified, one's mode of thought is transformed and, of course, so is one's mode of action. In other words, as you collect these concepts from everyday life, you begin to live differently and you begin, deliberately, to think differently, experiencing a methodological process in which many elements of indigenous thought become your own. This implies that you become more like the people involved, and it could not be any other way, but those with whom you live and work also become more like you. Without fear of exaggerating, you can state that after working with people, if you leave just as you arrived, you have missed the most important point in your research.

It is obvious that this process of conceptualization and abstraction springs from and is nourished directly by community lifeways. This means that in the course of a project focused on knowledge creation it

is necessary to participate in daily life and, consequently, to experience its difficulties and hopes so that they become one's own. This is different from the participant observation of ethnographers, which is only a tactic for collecting information more efficiently, for winning people's confidence, and for verifying in the most tangible sense the material that has been collected.

It is also different from what has been called "accompaniment" (Andrade 1993: 2–3), because that involves accompanying the lives of others during a discrete period of time, without their problems becoming those of the researcher.

Collecting concepts embedded in everyday life refers not only to how Guambiano knowledge might be described from an external perspective. It is also related to other facets of this knowledge. For example, what we call nature, of which the Guambianos feel they are a part, was explained on one occasion by *taita* Lorenzo Muelas as follows: "Nature doesn't belong to us. To the contrary, we belong to nature."[21]

For the Guambianos all of the elements that make up nature are animate beings who are alive, as are people. That is why one must relate directly to these beings—to the páramo, to rain, to the wind, etc.—in order to understand Guambiano lifeways. For this reason, a short time after we began our project we went to the páramo so that it could become acquainted with us. That day the páramo, clouds, and wind received us with great force, lashing at us during the trip. When we returned that night the compañeros decided that "the páramo didn't recognize us," although they promised that once it knew us, the next time it would treat us better. We had similar experiences with lakes, mountains, and other beings, whom we had to consult and interview several times, and from whom we received "signs" in distinct forms in different parts of our bodies, each one meaning that they were telling us something.

For this reason, when the Guambianos refer to certain important mountains in their territory, they say that "those mountains speak about many things." The highest mountains, with their tremors and strong quakes, announce what is coming. It is necessary to learn to listen to all of this, sometimes with the body and not with the ears, although on other occasions these beings connect directly with our minds by way of dreams and other forms of communication.

Writing Ethnography

I have already argued that for the postmodernists the essence of ethnographic practice is in writing. Postmodernism overvalues the text in comparison to fieldwork and even analysis, to the point of proposing that societies or cultures can be read like texts. In general, postmodernism reconsiders writing without raising anew the question of its contents or of power relations. That is to say, this is a kind of formalism that is not transformative, nor does it seek to be so.

For postmodernists, authority is decentered in the text but not in the world. This implies that the authors use their authority to deauthorize themselves in a clearly ideological move that is, basically, a ploy. The dialogue the postmodernists proposed is only a monologue of the ethnographer with himself, in which the "native" is an intermediary, a pretext.

Duvignaud (1973) has shown how guerrilla struggles that commenced in the 1950s in indigenous and peasant societies—once seen as traditional and conservative societies impervious to change—allowed them to become the subjects of their own history. He argues that these struggles called into question the notion of culture and demonstrated the fictive quality of the objects of anthropological knowledge, obliterating their semblance as exotic and strange. Since then, anthropology can no longer base itself in the stability of the culture concept but instead is based in change. Decolonization obliges us to reconsider the relationship between the observer and the observed. Even if it has always been valid (although not explicitly accepted or taken into account), we now face the reality that our objects of study cannot be separated from our own reality, nor should they be viewed as standing outside it, even though they are described without giving them a voice.

In my opinion, the postmodernist ethnographers (and even some who are not postmodernist or not entirely postmodernist) have sought refuge in writing in the face of their discomfort over the mutiny of the colonized, their objects of study, who have stood up and begun to walk by themselves, becoming subjects. Now, as in *The Leopard* by Giuseppe Thomasi di Lampedusa, they want to change everything so that nothing changes.

"Being there," in the field, has been transformed by decolonization. The very basis of doing ethnography is eroded by the unprecedented question, "Who do we think we are to seek to describe them?" The very

possibility of being in the field is placed in doubt, and even when this is possible, it is no longer feasible to continue working under the conditions and according to the criteria and interests of the ethnographer.

But the stage that comes after fieldwork has also been clouded by doubts over the validity of written representation, with the very epistemological foundations of ethnography being challenged. If for Malinowski knowing the other was knowing the self, then culture is a conscious product to the extent that consciousness is achieved when one comes to know the other. Today, some postmodernists argue, anthropology does not involve knowledge of the real Other but only of a creation of ethnographers, who remake the other according to the terms of the society to which they belong. An earlier anthropology says more about ethnographers and their society than about the other. This might possibly be true if one accepts that rather than being scientific knowledge, all previous ethnography is the alienated thought of the West about the non-Western other, just as Marx argues happens with religious discourse about the gods, which is really a discourse about capitalism.

Writing, Image, and Society

In effect, writing is socially determined, in terms of both what is said and how it is said. In anthropology, Western society has produced an image of the Other that satisfies its own needs. The ethnographer's Indian is not a real Indian but the image of an Indian that capital needs to infuse and spread at a particular moment in order to exert dominion. For this reason it is ever-changing.

Some ethnographers recognize this, and for them the Other is not the Other but an image, a representation of ourselves. These are pragmatic virtual images that have real effects. Even today, there are ethnographic descriptions produced by indigenous people with the aim of capturing resources, such as that of the "perfect ecological Indian."

From this point of view ethnography is a system of production of images, of representations of reality; consequently, its products are not reality but images of it, not simple portraits. But postmodernists go further and assure us that representation is a free and subjective discourse, with no relationship to reality: it is a simple pretext. In contrast, for most Native societies there is no separation between representation and reality; discourse and image are reality, since, as we have

seen, there is no separation between reality and thought, between reality and discourse. Scientific knowledge, for its part, seeks a concordance between knowledge and reality.

Thus distinct criteria for the epistemological character of ethnographic representation appear to confront one another, questioning whether one creates knowledge about reality and, if so, what the relationship between them is.

For the most radical of the postmodernists, however, this dilemma is meaningless, as there is no pretense of doing science. When they postulate that we are not dealing with texts, but with discourses, interpretation abandons all intent of explication, detaching reality from knowledge by arguing that all knowledge is valid, since knowing is completely subjective. That is to say, the objectivity that for years was pursued by anthropologists is abandoned. For this very reason, some now place anthropology within the humanities, given that it does not seek to show things as they are but instead uses reality as a starting point for creating a subjective image of how anthropologist-artists see the world, each one with his or her own style. In short, the images that are produced are not meant to be representations.

One of the strongest arguments in favor of this position derives from the fact that reality is chaotic, that there are no structural relations and all elements are unmoored, all congruence among parts merely invented, a construction; this occurred with the classical anthropological concept of culture. What exists is not a community or a society but chaos. It is useful to remember that Marx (1973) had already argued that such chaotic visions of reality are the result of a focus on the superficial relationships among external elements, a conception that surely characterizes formalism.

Supposing that postmodernist ethnography is correct in that it only creates images of reality and not knowledge, we must still consider the problem of what kinds of images it creates and what their effects are. Various authors, including Vine Deloria (1988) and Stanislav Andreski (1972), have extensively analyzed the power of manipulation inherent in ethnographic description, which creates images that are not in accord with reality. But it does produce change in communities, who seek to accommodate to these descriptions in order to benefit from them, with the aim of being recognized as indigenous. All of this is the consequence of the relations of power and domination exerted over them.

The dominant classes only recognize the Native who resembles the existing imaginary created by anthropologists.

In Colombia the indigenous struggle has included the demand to be recognized as Indians according to their own criteria and not those of the office of Indigenous Affairs, which are based on the images and concepts of anthropologists; in recent years this office established new teams made up of anthropologists and lawyers, who would certify the nature of Indianness in some communities of the Putumayo. There is a close relationship between this and the processes of reindianization and cultural recovery, which sometimes follow external criteria, but which the state seeks to impede or reverse because, as they say, "there are too many Indians in Colombia."

Such images are, nevertheless, neither pure nor isolated. To the contrary, there is a mutual contamination between internal and external images through the mass media and the direct action of ethnographers. There is also the possibility that the ethnographic image does not coincide with reality in the moment it is produced—but that it will in the future, thanks to its effects on communities. These external visions coexist with internal ones. For example, at a celebration in Guambía children under the guidance of their teachers (some of them nuns) abandoned their traditional costume to don feathers and loincloths so that they looked like Indians.

But the written image also has effects on the dominant party making the description. This occurred in the nineteenth century, with implications that continue to the present, with the discourse of Western charitable civilizing actions directed at savages. It functioned as a moral argument among the colonizers to justify the colonization of aboriginal populations.

What Should We Do with the Research Results?

In my opinion the divorce between reality and knowledge arises out of the great spatial and temporal gaps that anthropology has placed between fieldwork, analysis, and writing. Indeed, the extreme postmodernists reduce ethnography to the moment of writing, deleting the analysis of data, given that it is no longer necessary since they no longer think of this as a process of knowledge production; others confine themselves to presenting their data directly, with only a slight ordering

of it, if any, to allow readers—they allege—to form their own representations or evocations.

According to my analysis and a rethinking of the epistemology of fieldwork that converts it from a simple technique to a methodology that joins my research experience to that of the Guambianos, what I propose is to bring together the two levels (no longer stages) in a single plane, at the moment of fieldwork. This does not imply that there will be no distinction between these activities, since each has its specific characteristics. Of course, this contradicts the arguments of the postmodernists, since for me ethnography is not writing: writing is not fundamental. What is essential is fieldwork, with which both analysis and writing are inextricably entwined.

This implies that the questions of why, for whom, and how to conduct fieldwork must also be asked of writing, which would then play a role in the knowledge process and would not be limited to reporting research results. In this sense, writing would no longer be a communicative tool but, instead, would become part of the research methodology; it would also be epistemologically transformed.

In Guambía our research was based on a fundamental question: How should the Guambianos administer reclaimed lands in a specifically Guambiano manner, different from that of private property under capitalism? This question contained two other questions or problems: how to distribute reclaimed land, and how to work the land. The distribution solution proposed at the time by external agents, particularly by the Colombian agrarian reform agency then called INCORA, was to award individual plots or to create cooperatives or community enterprises. The Guambiano perspective was based on the idea that "everyone has a right" and that it was necessary to grant land rights even to children.

Initially, the Guambianos proposed a way of solving both problems together. First, to walk the land to see how the Guambianos cultivated it, participating in agricultural labor, and to see how they worked on the haciendas, by looking at material remains of the past (the remnants of ancient furrows, for example). Second, to talk with people, especially with elders, about how they remembered the relationships Guambianos had with the soil and with the territory, including their ways of cultivating it.

But there remained the problem of what we were to do with this material. Would we convert orality into writing by means of our field dia-

ries and notes? At the outset we made posters, because the original idea was to create picture-maps. Finally, as explained earlier, the cabildo did not allow us to continue working in this way and requested a series of pamphlets for the general public and for teachers and schools, some written in Guambiano and others in Spanish. This was a difficult problem for us to resolve.

The passage from orality to writing involves a process of translation from the local person to the ethnographer, which is mediated by the field diary. When one tapes, one obtains a comprehensive record, but transcription is also writing. Thematically organized field notes are an intermediate step between the diary and writing, breaking the groupings of information in everyday life that are partially recorded in the field diary and introducing a new set of associations based on the ethnographer's conceptual scheme or on interpretation of the contents of the diary. This is not necessarily or exclusively a translation of meaning; it is a translation from one code to another, something that implies elaboration.

Orality contains a series of elements that are not directly related to words: intonation, posture, facial and corporal expression, etc. Signs like quotation marks, question marks, and exclamation points that are employed in writing are an attempt at addressing such missing elements but can never completely achieve it. Orality also involves a cumulative process that requires paying attention in ways that differ between societies that privilege orality and those that privilege writing; the listener and the reader differ, in this sense.

There is a certain flexibility in orality that contrasts with the fixity and permanence of writing. This gives orality a character marked by a permanent process of updating, modification, and change of contents within the dynamics of each context. In contrast, writing persists without alteration while reality changes, although of course how readers receive and interpret texts does change. Journal articles are somewhat more flexible in this sense, because there is a brief temporal distance between issues of the periodical, allowing us to recognize to some extent the changes taking place.

Written texts do not contain the background information that is implicit in orality. Who is being addressed? When? These elements are known, for example, in the case of a life experience shared between a speaker and a listener. In contrast, writing must explicitly contain all the elements needed for it to be comprehended, since it does not know

who its readers are and assumes that the author has no previous relationship with them, even though generally we write for those whom we want to reach; but when a text is prepared for a specific public, it is very difficult for others to understand it.

The Field Diary

The confrontation between orality and literacy is part of a wider divergence founded on relations of domination between societies and, consequently, dominance of writing over orality. In ethnography such conditions created the need to establish mechanisms for converting orality into writing, accomplished through the mediation of the field diary, through which indigenous lives are made into texts that are halfway between orality and writing. A second moment of writing—of rewriting—is then required to produce a definitive written product. This process occurs "here," distant from indigenous lives, in the ethnographer's own society.

Given that the movement from oral societies to ethnographic writing is mediated by the field diary, the following questions must be asked: What kind of writing is contained in the field diary? What is its relationship to fieldwork?

One generally thinks of the diary as an indispensible tool for collecting, saving, and remembering information arising out of fieldwork. But in reality, it is a much more complex part of the process of objectification that leads from orality to literacy. The field diary constitutes the first step in this process, because it shapes and fixes ideas, discourses, customs, and actions in the material space of the page.

It is assumed that in the process of writing a diary, ethnographers set aside their own subjectivity to objectively record information that has passed through two filters, the sensory organs and the brain; that is, they engage in interpretation. The difference between a field diary and a personal diary is that the ethnographer can separate objectivity from subjectivity, although in either case this involves a process of elaboration, given that the two coexist in the ethnographer's personal life, where feeling and knowledge or reason are never separated.

Writing fosters that separation; it enables a critical revision of the influence of subjectivity over objective knowledge. For this reason personal diaries are rarely published by their authors unless in exceptional

cases, as occurred with the personal diaries of Malinowski. On occasion an author may quote some excerpts from the contents of a personal diary, but usually as an anecdote or illustration.

All of this has been neglected. The questioning of ethnographic writing has been limited to the final written text and not the field diary. This may be due to the fact that the field diary is never complete, but we can ask ourselves why not. Its peculiar ambiguous status derives from that fact that it is not considered a written work, although it is written. Its purpose is to indicate the personal relationship of the ethnographer with the moment, because it is closer to the subjects, to orality; its structure is not analytical. It is organized according to the flow of everyday life, which is not textual but lived, unconnected or incongruent if read in this way. It does not contain explicit theorizing, although the ethnographer's ideas guide the pen. Nevertheless, some recommend that it be read at night to encourage reflection and, building upon it, to create an explicitly written ordering.

Nor has the essential character of the field diary been discussed. That is, we have not reflected on the question of whether the movement from orality to writing requires this "semi-written" text. It may be that poor memory makes it necessary, although the fleeting nature of memory is one of the consequences of writing. Sound recordings, which some use in place of the diary, are themselves a form of writing because in and of themselves they are not usable and require transcription. Without the diary, it would be necessary to develop other tools of memory, like those that exist in oral societies who remember through lived actions and events. Living with people helps to activate these mechanisms, in which case we cannot properly speak of memories but need to speak of tools employed in the course of living; when we leave, these tools begin to recede, until they eventually disappear.

If we accept the viewpoint of extreme postmodernism, the only truly ethnographic document would be the personal diary, or even better, the personal response of ethnographers to their relationship with the Other, their feelings about this relationship. Geertz (1989) thinks Malinowski was the first to achieve objectivity by separating knowledge from subjective impressions, which he obtained by using two separate diaries.

Thus it might appear that there is a simple process of translation from an oral to a written code, but it is also a process of individualization, of the predominance of the individual over the communal. To that we might add a process of objectification, because ethnographic writing is

supposed to lend an objective character to this knowledge, which, once written, achieves an objective existence external to that of its author. Orality, in and of itself, is never objective but becomes so when recorded in writing and displaced from the self, independent of the subject.

The oral code is more subjective and corresponds to societies that are more communitarian in nature. The written code has a more individualist character, accommodated to class societies where writing sets in motion objectifying processes by separating words from their authors and lending them permanence. The materiality of the oral text is more transitory: it lasts only so long as the air is in motion, although it can endure imprinted on the brain, in memory.

The objective character of the written text in relation to its author has been taken up by the postmodernists as the basis of their act of deconstruction. Each reader can rewrite the text according to his or her own interests, independently of the person who wrote it. According to the postmodernists, the text only says what it says according to the personal criteria of the reader. Nevertheless, this is a condition of *all* writing; the process of interpretation mediates between author and reader. But orality also involves interpretation, for example, in relation to the context.

The materiality of writing creates a permanent process of interpretation of the text, while interpretation of orality occurs more immediately. Texts have a broader and more global character because their interpretation can also be converted into a text, as occurs with reviews and critiques, and it is possible to form discussion groups to interpret them.

Museums: The Path to Writing

The transition from orality to writing in the process of generating knowledge has been up to now the task of anthropologists, which has required that they experience through fieldwork the lives of the societies studied.

In the early years of anthropology the relationship with the object of study was indirect, mediated by colonial administrators, missionaries, and traders, who lacked preparation and training; an example of this is the work of someone like Frazer. Later, informants became intermediaries. One would not plunge into native life but would be led by the hand by informants, complemented by direct observation. But it was with the introduction of participant observation that it became possible

to move from orality and everyday life to writing, with life obscured by the decontextualization of information, which is abstracted out by way of field notes. This is how the notion of "living there and writing here" emerged. Orality, in contrast, cannot abstract out of daily life, which furnishes it a context; without daily life, orality would make no sense.

In the move from orality to ethnographic writing, museums played an intermediary role. At first, museum collections were accumulated directly, through colonial looting and later by ethnographers. Meaning was lodged in the objects, which were neither contextualized nor accompanied by discourse or writing. Today museum exhibits are based upon previously composed scripts, and their exhibit cases are filled with writing. When the Guambianos assembled their museum, the guide to how to locate and exhibit objects was provided by the structure of the Guambiano house, the central axis of social life; the script was only written much later, to be able to recount what was on exhibit, why, and how it was organized.

In the early period, research results were presented exclusively through objects, but the reordering of the museum made the known unrecognizable, producing the effect of alienation, what is now called the "museum effect," in which the exotic appears unreal. Native people do not recognize themselves in such museums, just as later they would not recognize themselves in texts. Ethnography makes the self strange, abstracting and decontextualizing it. Later, relationships began to be drawn between museum objects, something that also became a characteristic of written ethnography. In this order of ideas the first museums constitute a form of ethnography prior to the ethnographic monograph. They were very close to aboriginality, because the objects directly communicated their contents. That is to say, in the process of movement from abstraction to writing there was an initial inability to separate the material world of the societies studied, their objects, from what was plucked from them by the anthropologist, who took these things away to place in museums. In this way the results of knowledge were presented through things, as among indigenous peoples.

Power and Writing

But written expression has other consequences. Until now it has been a historical coincidence that oral societies are those that are dominat-

ed, while societies characterized by writing have been the dominators, despite the fact that we cannot draw a direct cause-effect relationship between one condition and the other. Such relations of domination have meant that orality is translated into writing and not the reverse, a fact that is strengthened by the ethnographer's belonging to the dominant society.

But if ownership of writing was not the cause of domination, it has played a role in the process of subordinating oral societies. Some authors have noted the coincidence between the introduction of writing and that of class society and have observed that writing arises among the dominant classes. The first things that were written, as far as we know, were inventories of goods from the stores or warehouses of kings; that is, inventories of products accumulated through exploitation.

I have already argued that the introduction of intellectual work, as a specialized activity separate from manual labor, corresponds to the social formation of class society. Writing played an important role in this process, making possible a distancing between materiality and ideas when the ideas were put in writing.

The confrontation between orality and writing is not abstract but driven by politics and presented in the broader context of social relations. In indigenous societies, as also occurred in the dominant classes of our society, many adhere to the belief that learning to read and write creates, in and of itself, the conditions for demolishing relations of domination by unmasking deception. Thus the appropriation of foreign forms of knowledge is accepted as plausible.

The focus, contents, and even the form of the written text are not entirely the product of the author. This is a tenet of trendy currents of anthropology, which in turn arise out of specific social conditions; if this is not the case, the monologic postmodernists must respond. This state of affairs fragments, dilutes, and deauthorizes the author's practice. The author is, then, not an author(ity), as is illustrated by the fact that diverse authors coincide in drawing a common image of Indians, depending upon the era in which they are writing, in spite of differences of personality, historical period, and other such things.

The ethnographic monograph, for example, is a standardized model of writing that has been maintained for decades and is still the rule in academic circles. If it really had an author, why is it always written in

the same style? Perhaps authors are not very authorial and are limited by narrow social frameworks. Models of scientific writing, including the ethnography, create certain limits and adhere to them by asserting a prior definition of contents and objectives, by defining the principal theme and reorganizing social relations around a broad thematic plan, with a set order and a preestablished set of relations among topics. This implies that before writing begins it is known what the text will say.

If ethnography is ideologically premised on the existence of the ethnographic subject in the field, the mechanisms I have described will convince ethnographers of their own importance and obscure their role as social agents, just as they are invested with an authorial identity that they really do not have. The vision of the "anthropologist as author" is the fundamental axis that reinforces their importance. That is to say, there is a consistency in the double illusion of the ethnographer: the illusion of the field and that of the text. This impacts directly on the possibility of knowing by learning from indigenous people. The Yaqui shaman Don Juan realized, as Carlos Castañeda's books convey (1991 and especially 1998), that the loss of personal importance is a requisite for beginning to learn.

Maintaining Concepts in Everyday Life

In order to confront all that goes into the writing of ethnography, one of our tasks is to attempt to recover, in writing, that which has been annulled, excluded: in other words, daily life. One path points toward challenging the abrupt separation between daily life and the field diary as well as between the diary and the definitive text. This is made possible through co-writing with indigenous people, something that cannot arise out of the goodwill of the ethnographer but must emerge out of a particular framework of social relations and solidarity, as part of their struggle.

In terms of translation, the field diary is not univocal but can take two forms: one in which facts are translated, according to my ideas and vocabulary, into something for my ends; and another in which there is almost no translation, but instead the Native vocabulary, context, intonation, body language, and emphases of utterances are all recorded. In order for the field diary—which is a first instance of writing—to fulfill its role in this second diary format, one must be able to hear in one's

head as one reads the voice of the speaker and to see in the mind's eye the image and actions of the actor.

The word is more than a spoken discourse in oral societies; more fundamentally, it is life. When the Guambianos say that "the words of the elders were silenced," they refer to the fact that in past generations people no longer lived as Guambianos, not that the elders were mute. Autonomous thought (*pensamiento propio*) had ceased; it was no longer in motion because people began to lead their lives without heeding the elders' advice, without tradition.

For this reason, indigenous processes of cultural recovery (recuperación) require something new, the making explicit of thought in a novel format, such as concept-words instead of concept-things, in order to promote a critical appreciation of the new form. In order to achieve this, it was necessary for the Guambianos to write, making way for a new and needed critical perspective. Earlier forms were oriented toward lending permanence to Guambiano thought, to conserve it as a way of life, but they did not contribute to its recovery, which at the moment was their primary necessity.

When thought is life and is lived, recovery implies beginning to live it again after it has been lost. But the reappropriation of past lifeways, recuperated in their former state, is impossible, because these uses and customs, this tradition, no longer conforms to the conditions of the present; no one wants to live that way; it is impossible. Guambiano lifeways as they once existed are no longer; today it is necessary to create new lifeways built on the foundations provided by a search for the roots of culture and thought. One of the ways of accomplishing this is to work with elders, but also with mediating institutions like the Museum-House of Culture, the role of which, as defined by the Guambianos, is precisely to give life. That is why cultural heritage is life, is alive, and is not a dead past, as it is for us.

This need for explicitness also arises from the fact that many aspects of Guambiano material culture, where thought was once lodged, are no longer a part of everyday life; they have been lost. It was necessary to remember them through alternate strategies, such as through the words and memories of the elders and through walking the territory.

Here archaeology, ethnohistory, and the search for objects that are no longer in use all played a role. The elders recounted their uses and customs. Guambianos began to make objects that were no longer uti-

lized, as samples for archaeological research. But memories recuperated in this manner still remained in the past: this is how it was, and it is over. They did not attempt to return to ancient lifeways. What they wanted was to resolve today's problems in their own way, as Guambianos. For this reason they needed to be explicit about cultural forms.

And it was necessary to accomplish this through a body of abstract ideas, and through expressing these ideas—which had always existed as activities, as lifeways, as objects—in words, developing new forms of thought that had not existed before among the Guambianos. For example, traditionally the Guambiano hat embodied a complex conceptual model, transmitted through its manufacture and use. Today, however, this is insufficient, although of course it is possible to begin to remake these hats based on surviving examples and the memory of the elders. But it has now, instead, become necessary to talk about hats, to write and to abstract the conceptual contents from this object of everyday use. The act of explicating produces a conceptualization that did not exist before, it creates knowledge and does not simply communicate it, and it makes possible the drawing of new types of relationships between concepts and an array of forms of thought. This obliged us to write texts—in Spanish for the moment, as writing in the vernacular develops—in which the words expressing key concepts appear in Wam. Even so, we sought whenever possible to maintain concept-things and concepts in their practical dimensions—lodged in the activities of everyday life—and to construct the organization of the written text upon the foundations of Guambiano thought and oral discourse.

We found support in a form that is intermediate between orality and writing, but which many Guambianos—including those with whom I was working—consider to be an indigenous form of writing: petroglyphs, in which snails, hats, and spirals appear, referencing the form of expression as well as the existence of conceptual categories. The presence in the text of more abstract concepts, such as *mayelɸ*, *linchap*, and *lata-lata*, was never abstracted from *mingas* and communal meals, the activities and lifeways in which they function. In Guambiano writings, words in Wam are always attached to the realities to which they refer. Picture-maps also played a role in mediating between the word and the text.

The second step in the recovery process was to conserve these concepts as everyday lifeways. For that reason there was a need to discover

ways to create new activities and lifeways that were in accord with these "recovered" concepts but that also accommodated to current conditions. People did not want to become living museums. The Museum-House of Culture was built to embody the word of the elders, a space in which this word can live as though it were a house. But children do not live there; they visit it. The objective is to allow thought to speak once again through objects.

There are, nevertheless, many difficulties inherent in the task of creating new lifeways. This cannot be a spontaneous process: it requires orientation and authority. This is the role assumed by the cabildo, whose members nowadays are functionally literate and whose command of literacy is growing, to the point that it is impossible to exercise authority without writing. For this reason the cabildo has a new role, which it did not have before, in the process of converting orality into writing, into literate thought. Continually forced to turn outward as a consequence of the process of national integration that was fortified by the 1991 constitution, the cabildo has in its practice become a basic obstacle to the work of forging new Guambiano ways of resolving problems and of living. Little by little they have abandoned the role of *tata* (elder), of someone who advises the community and orients it in daily life. As a Guambiano woman so clearly expressed it to me: "Counsel is like food; without it you die."

Writing as a Form of Knowledge

In any event, when writing is reconsidered as it was in Guambía, it becomes an essential component of a process of generating knowledge by fulfilling the dual goals of separating and conjoining. In this sense it is possible to consider, explicate, and systematically use writing as part of a methodology for generating knowledge.

As I have already stated, one of the fundamental characteristics of writing is that it necessarily abstracts. Oral discourses are essentially concrete, while by its very nature writing is an abstract form. To the extent that it generalizes, writing at the same time decontextualizes, replacing the context it eliminates with specific discursive forms that serve as referents for the reader. Given that oral language implies a direct and personal relationship between speaker and interlocutors, they all share a series of contextual elements: they live at the same point in time and

they probably belong to the same social group or to related groups and consequently share worldviews, all elements that orality assumes as given; for this reason, there is no need for them to be foregrounded or even explicitly noted. In this sense orality restricts itself to a spatial, temporal, and social context that makes it specifically concrete.

Writing, with its possibility of broader coverage, eliminates such concrete contexts, creating abstractions out of the spacial, temporal, cultural, and situational contexts. Consequently, it permits access and comprehension to people of different conditions by adopting discursive forms that replace these contexts with other referents. Precisely for this reason, writing developed as a specific form of language, corresponding to other social conditions and situations of development.

There are moments during fieldwork or research when one cannot write up a report because "one has not been able to clarify things." In reality, I have never understood the clarifying role of writing, nor which things are clarified only at the moment of writing. For that reason the introduction to a book is the last thing you write, since you do not know beforehand what you are going to write, even if we sometimes believe we do. We might have a certain idea of what we will do, but when we begin to write, things begin to change, to be transformed, clarified, entering into new relationships, and therefore the final result is never what we intended at the outset.

Empirical reality cannot be transcended simply by thinking. Thinking does not help us to go beyond the information we have collected, to comprehend it conceptually. Comprehension is not limited to forming a theoretical framework out of a body of concepts, which can be borrowed, or stolen, from books. Instead, it implies understanding the conceptual relationships among all of the elements collected during field research or in interviews and surveys. None of this is possible unless you write. Some people experience writer's block and never complete their monograph or report, because they are waiting for everything to be clarified; they hope to analyze and comprehend things before writing. That is impossible, unless it consists of a simple repetition of the factual material, organized according to different formats or grouped in a new way but without advancing in the process of understanding, in discovering the essence of things. One has to compel the brain to think; and writing pressures the brain to think abstractly. To abstract is a form of thought, but writing forces or facilitates it. Nevertheless, written language is not the only way to achieve abstraction.

Abstraction is a mental operation. When one writes, the process of abstracting is not achieved by writing. To the contrary, one must abstract in order to write, although it is also possible to develop the capacity of abstraction by exercising the mind. This means that at times we can achieve abstractions for which there are no words to say or to write, which obliges us to use archaic terms, transforming their meaning.

Another peculiarity of writing is its linear, progressive character, which also tends toward abstraction. If one does not write in a linear format the reader will experience problems of comprehension. As a result of its linear nature, writing requires the abstract organization of ideas. In social sectors such as ours, this effect is so strong that even orality has a written support in our minds, where we have a written outline that provides oral discourse with a linear organization: statement of argument, development of points, conclusions, for example. The opposite occurs when writing is introduced into oral societies. At first, writing is orality in writing. In order to be able to write texts that have been thought through, one must pass through an intermediate stage.

This was the path we followed in Guambía, to conduct research together and for writing to play an integral role in processes of creating knowledge. This occurred in the field, with the participation of the Guambianos, based on two criteria: everyone had the right to participate in writing, but everything is not equal, and for this reason, each one would participate according to that individual's own capacities and conditions.

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Notes

1. We thank Professor Vasco for giving *Collaborative Anthropologies* permission to translate and publish his work.

2. The Summer Institute of Linguistics, also known as the Wycliffe Bible Transla-

tors, is an evangelical missionary organization dedicated to translating the Bible into native vernaculars to spearhead religious conversion. For years there has been broad-based opposition to the SIL among indigenous organizations, because of both its missionary agenda and its support of conservative politics.—Trans.

3. Reynoso (1992), for example.

4. For an extensive explanation of La Rosca's principles, see Bonilla et al. (1972).

5. *Historia doble de la Costa* (Double History of the Coast) resulted from the work done by La Rosca with peasant organizations on the Atlantic coast. It is a two-channeled publication that caters to a distinct readership for each channel, as Vasco describes, or invites readers to move back and forth between channels in a kind of dialogic movement between theory and ethnography.—Trans.

6. See Vasco (2002: 434–41.

7. These are the major indigenous groups of southwestern highland Colombia. *Solidarios* were non-Native activists organized in support of indigenous struggles.—Trans.

8. Colombian indigenous communities are led by annually elected councils called *cabildos*, headed by a governor.—Trans.

9. A *minga* is an Andean form of labor exchange, a collective work party. The notion has been extended by the indigenous movement to encompass political activities as well as subsistence labor.—Trans.

10. In this and the following section, Vasco uses the term *indígena* or "indigenous person" in statements where in English it would be more appropriate to use "local" or to identify groups by their ethnonym, like Guambiano.—Trans.

11. Introduced in the late 1970s by researcher-activists working in solidarity with neighboring Nasa cabildos, picture-maps provided a context for remembering indigenous history by condensing historical referents drawn from oral narratives into the local topography, fostering a participatory recounting of the past that privileged the indigenous memory and modes of storytelling; see Bonilla (1982).

12. See "The Notion of Indigenous Cultural Production," in Vasco (2002: 402–10).

13. A broader explication of this indigenous concept and its implications for processes of knowledge creation can be found in Vasco (1985: 131–37).

14. For example, see Clifford (1988: chap. 3) and, from a different perspective, Geertz (1989: chap. 4).

15. I take this up in "Indigenous Telluric Thought" (Vasco 2002: 196–202).

16. A *resguardo* is an indigenous corporation, frequently validated by colonial title, that administers communal landholdings in Colombia. It is governed by an elected council, the cabildo.—Trans.

17. The *páramo* is the humid plain at the top of the cordillera in the northern Andes.—Trans.

18. Vasco uses the term *recuperación* to describe the history project. This is the word used to refer to the process of occupying usurped lands to reintegrate them into the communal resguardo structure. When the study of oral history is referred to as a process of recuperación, it is linked into this political struggle.—Trans.

19. Vasco defines some of these concepts earlier in the book in which this article appears (2002: 297). He glosses *mayelø* using Guambiano quotations: "There is enough for everyone" and "All of the members of this household, this great Guambiano household [that is] our territory, we share everything." Mayelø provides the foundations for the others, including *lata-lata*, which denotes "equality," in terms of access to rights and

to things. *Linchap*, which means "accompaniment," is the basis for Guambiano unity. *Kantø* is a communal work party common throughout the Andes (known in Spanish by the Quechua term, *minga*).—Trans.

20. See Dagua (1991).

21. Lorenzo Muelas is called *taita* as a term of respect, related to the word *tata* (elder). He was governor of Guambía on several occasions. He was a member of the Colombian Constituent Assembly that wrote the 1991 constitution recognizing Colombia as a multicultural and pluriethnic nation, and he subsequently served in the Colombian Senate. He is also a well-known environmental activist.—Trans.

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