

Who is the native?

Memories of fieldwork in the USA

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I

From 1972 to 1975 I took my Ph D in Anthropology with Charles Wagley at the University of Florida, where I did fieldwork and wrote my dissertation about an American community. This paper is a memoir of my fieldwork in the USA.

The US had for some time been the object of incidental interest of Brazilian intellectuals such as historian Oliveira Lima or sociologist Oracy Nogueira.

The latter wrote a remarkable paper on the compared pattern of race and color prejudice in Brazil and in the US. Writer Vianna Moog, in his well known book *Bandeirantes e Pioneiros*, sought to explain the development of the US and the underdevelopment of Brazil by the settlement pattern of both countries. But it was the first time that anthropological work - implying intensive field work and resulting in a monograph - had been carried out by a Brazilian anthropologist on American society and culture. Later, in Brazil, I published a book in Portuguese about this study. It was a new project in Brazilian Anthropology.

At that time, formal graduate training in Brazil had just begun. The exception was the University of São Paulo which had a loose, dissertation-only, graduate training in different fields of knowledge, but not specifically in Anthropology². With some few exceptions, like Gilberto Freyre, who took a MA degree in Columbia, or my father, Jorge Zarur, who took a MA in Geography in Wisconsin in the forties, Brazil had no tradition of graduate training in the US. Today, thousands of Brazilian graduate students are in the US and elsewhere.³

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2 Brazil has, at present, a large graduate university system.

3 Wagley's student Eduardo Galvão was, in the fifties, the first Brazilian to get a Ph D in Anthropology. I played a role in the expansion of graduation training abroad, when coordinating the Human and Social Area in the Brazilian National Research Council - CNPq. Before moving to the US, I

When I went to the US in 1972, the world was sharply divided: the ideological debate in the Brazilian social sciences was centered on the distinction between the quantitative and the qualitative approaches. The quantitative approach, associated with American Sociology, would disguise class relationships and class exploitation. The social sciences were understood as ideologies bearing the moral obligation of denouncing the living conditions of the oppressed. American Sociology and Political Science were considered as representatives of the ideology of domination, a side-effect of US backing for the military dictatorships that plagued Latin America. Influenced by the São Paulo sociological school, one of the problems facing the social sciences was how to abide by rigorous academic standards guiding empirical research, while conciliating these standards with ideological and moral imperatives.

Thus, studying in the USA represented a major problem of conscience implying a moral decision. For sociologists, France was a fairer alternative because French Sociology and Political Science emphasized qualitative methodology and French Sociology was in permanent dialogue with Marxism. French intellectualism held a strong appeal for the Brazilian university tradition. On the other hand, young Brazilian scholars felt increasingly attracted by the excellent standards at American universities.

But American Anthropology was the reverse of Sociology and Political Science. Anthropological methodology has, of course, always been qualitative. Besides, American Anthropology's most distinctive trend of the period, "cultural materialism", led by Wagley's student Marvin Harris, based its explanatory nexus on the material infrastructure of society. It was a sort of translation of Marxism to Anthropology, especially when associated with large-scale cultural change, such as in Gordon Childe's, Leslie White's and Julian Steward's versions of cultural evolutionism. By that time, structuralism had already found its way into Anthropology, and France had become the center of "conservative" thought in Anthropology. Lévy-Strauss's key notion of social structure was static, did not emphasize socio-cultural change and its explanatory nexus did not start with the material basis of society.

Working with Charles Wagley was not only politically acceptable but also desirable. Wagley came to Brazil during the Second World War to found

had just finished my MA in Anthropology at the Museu Nacional do Rio de Janeiro, a program founded in 1969 and the only one, at that time, in Brazil.

SESP (Serviço Especial de Saúde Pública) and to contribute to the Brazilian effort in rubber production for the allied troops. Wagley became a good friend of the most important Brazilian intellectuals of that time, including several considered to be left-wing. The alliance against the common Nazi-fascist enemy placed Wagley at the center of the Brazilian intellectual community. Later, in 1965, when the University of Brasilia was closed by the military government after the collective dismissal of its faculty (including his student Eduardo Galvão), Wagley led a movement of American intellectuals in solidarity with the Brasília faculty and against the Brazilian regime. While in Columbia, he invited Celso Furtado, Florestan Fernandes and Darcy Ribeiro, among others, to join the institution. His loyalty to his Brazilian friends was considered in Brazil to be one of the reasons for his removal from a leading position at Columbia University. Anthropologist Darcy Ribeiro and several others considered Wagley one of the founding fathers of Brazilian Anthropology.⁴

When I had just arrived in the US, Wagley told me that he had thought about having a Brazilian studying “race relations” in American society. Immediately, I seized the project.

My reasons were sound: *first*, it was an original initiative in Brazilian Anthropology. Later I was to learn, from the relativist school in the Sociology of Science, that too much originality may be punished. But at the time I was thinking as an idealistic “Mertonian scholar”⁵. *Second*, I felt a lot of excitement about studying the US: I was going to know the monster from inside! Years later, during a trip to the Kayapó Indians, I recognized a similar excitement when they woke up at dawn to go out hunting; *third*, it could be politically useful to Brazil, just as American studies of Brazil were useful to the US. They oriented state policies, economic investments, etc. The knowledge of the US would be essential to interact with the US; *fourth*, identity.

4 My colleagues in Gainesville were astonished because I preferred studying with Wagley instead of at Harvard. However I was looking towards Brazil and expressing Brazilian values. Later, David Maybury-Lewis was generous enough to accept me as Visiting Scholar in the Harvard Department of Anthropology.

5 Much later, my apprenticeship about the consequences of originality was reinforced by my incursions in unexplored fields in Brazil such as Cultural Ecology (see my 1979 article in *Current Anthropology*) and the Anthropology of Science (see my 1994 book). Ecology and the anthropological study of science are established fields in today’s diversified and plural Brazilian Anthropology.

The issue of identity was crucial. Anthropology is the science of Europeans (and of Americans) studying natives. However, Anthropology is not a straight “bwana science”. Long fieldwork may imply the anthropologist becoming personally involved in the population he works with. Anthropological information may be sympathetic towards the studied populations. Relativism may “humanize” natives, making strange behavior a rational answer to different conditions. Explanation may replace moral condemnation and tolerance may replace prejudice. Anthropology sides with Father Bartolomé de Las Casas, who, in the XVI century, argued that Native Americans did possess a human soul and thus should not be killed or treated like beasts. This attitude may bring real gains for anthropological natives, usually on the weaker side. But the anthropologist has no control over the information that he yields. In spite of the personal feelings of some anthropologists, Anthropology organically integrates the terms of power between nations, ethnic groups and social classes.

Even fieldwork expresses power asymmetries. Good fieldwork needs intimacy, which can be very unpleasant for both sides. The stranger may need help in carrying out the most elementary tasks of daily life and may be ignorant of basic local rules of etiquette, hygiene and morals. He is a specialist in gaffes. Worse, asking questions in some cultural areas invades the sacred sphere. In several societies anthropologists have been suspected of witchcraft, with good reason.

Thus, natives must have strong motives for tolerating their anthropologist. Those motives stem from power and not from the personal charm that several anthropologists believe they possess, and that they like to display to ‘their’ natives. In societies with extreme social distance like traditional Brazil, the formal educated anthropologist is placed in the echelons of the upper elite. He may represent a potential ‘compadre’ or sponsor, who may someday help his informants to get a job or an appointment in a hospital. Among some Indian tribes the anthropologist may be an important ally for their defense. Nowadays Indians manipulate the media quite well, but even today, reporting infractions of human or land rights may be a major contribution to be made by anthropologists working in the Amazon or in Africa. Finally, in some very poor peasant areas, the anthropologist may be a source of income, paying for the services that he receives.

In the US, I realized how we, Latin Americans, were typical anthropologi-

cal natives, historical objects of colonization and of studies. Brazilianism had created a Brazilian identity, as had Orientalism for the near East, as described by Edward Said.

The study of American society and Culture was an inversion of the colonial logic. If he had been a member of my doctoral committee, Don Quixote would have enthusiastically approved my proposal, because the power factor of the ethnographic relationship was balanced against the anthropologist. But the inversion of a colonial logic is not the inversion of concrete power relationships. Therefore, it may not be sheer coincidence that my work had been initially directed to the weaker and powerless side of the American cities, that is, to the Black ghetto.

II

My fieldwork in the US started in 1973, when under the coordination of Wagley and Solon T. Kimball, I joined the research project "School and Community". The main question addressed by the research was busing in Black and White schools. To understand why busing did not succeed, the project considered the whole race relations system in Gainesville. I was appointed to work with Blacks because it was supposed that a foreigner would be better accepted than an American student. I made it very clear to my Black informants that I was a visitor, an external observer not involved with local disputes, even if my light skin was somehow an embarrassment when walking in the ghetto streets. A Black research assistant introduced me to his family and some friends. For three months I interviewed parents irritated by busing and I tried to understand the essentials of American Black culture.

North Florida was Deep South. Three dialects were spoken in Gainesville, in 1973: the Black, the university mainstream White and the Southern regional dialect, spoken by Rednecks easily identified by their shotguns in the pick-up truck. The Ku Klux Klan was active in the region at that time. The NAACP published frequent reports of killings of Blacks along the highways, without witnesses. The police was usually the main suspect.

My research among Blacks had to be interrupted after an incident. Visiting a family in the Black ghetto, I found a young man called Frank drinking wine with the owner of the house. Wine finished, Frank and I went to a nearby small market to restock. Inside the market, located in the streets

which divided the Black ghetto from a poor White neighborhood, an old White man pushed Frank and shouted “Get out of my way, boy!” Frank reacted, punching the man. In seconds, the market was divided: Blacks to one side, Whites to the other. Between the two groups, not knowing exactly what to do, there was a Brazilian anthropologist, who finally decided to run away. The fight was separated, but the White Man who had started the conflict said that he was going to kill Frank.

My wife got extremely worried about the situation. My friend, the Black research assistant involved in the project, strongly urged me to interrupt my work in the ghetto. Wagley forbade me to continue working there. Drug dealing among some of my new informants was also a concern. Then Kimball and Wagley sent me to a rural Black neighborhood some miles from Gainesville. There I marveled at the spirituals sung at the First Black Baptist Church Sunday services. However, I felt somewhat uncomfortable when, probably in reference to my presence, the Baptist congregation debated the Black Muslim question of whether the White Man was the devil. I realized that I was absolved because these people were very kind. The sense of the universality of Christian theology had won.

Even after limiting my work to the church-going rural community I had to give up working with Blacks. Frank, the young man who was involved in the market fight, was found drowned in a motel swimming pool. The police decided it was an accident. Blacks were convinced that he was murdered in connection with the fight I had witnessed. I well remembered the White man’s threat to kill Frank.

My Black friends recommended I should abandon working with the Black community. Wagley accepted the recommendation. I felt the ambiguity of my situation: I was a sort of White for Blacks and a sort of Non White for Whites. I suppose, however, that a White American would not have gone as far as I went. It was not a surprise for me when I knew, decades later, that Gainesville was the scene of fierce racial riots.

I did not give up studying American society and culture after this setback. Thus, I turned to the other side of the ethnic coin, the “rednecks”, also called “Florida crackers”. The suggestion of working in the fishing village of Cedar Key (population a little over 700) in the Gulf of Mexico came from my friend David Fleischer, professor of Political Science at the University of Brasilia.

My experience with Blacks was essential for the success of the new initiative. I had learned how to move in a society sharply divided along ethnic and neighborhood lines. I had understood, the hard way, how strong the category “race” was, and, within “race”, the category “community” in the United States. It was difficult to accept a Latino studying a WASP population, even a poor WASP population. My “natural” study object in the US was supposed to be the Miami Cubans or Catholics in Gainesville.

I could overcome these obstacles knowing the correct communication channels between the local community and the external world. To knock on a door and try to interview someone I did not know - as I would do in Brazil - were not good tactics. To receive an uninvited stranger with a foreign accent in your own home and then have him ask personal questions is unusual in the US. In Portuguese the word for the main room in a house is Visiting Room (Sala de Visitas), while in English the family Living Room is not designed to receive strangers. My experience among Blacks had taught me that the first contacts with strangers should be through the church. Instead of inviting me to their homes, people invited me to visit their churches - the way into the community and the place to control ambiguity. I entered Cedar Key through the First Baptist Church, but I also interacted with the Episcopal, the Presbyterian and the Methodist Churches.

I carefully planned my entrance in a community known as “closed”. I met some geography students (Whites) who had tried to begin a research project in Cedar Key. The Mayor had told them that he “did not want anyone sticking their nose in”. Later one of them got involved in a fight in a bar, which made their work in the town impossible. When interviewing those students, for the first time I heard the word “clannish” to refer to the town. I then asked everybody I knew at the University of Florida who knew somebody that knew somebody in Cedar Key to introduce me. In March, 1974, I finally moved to the village, where I had been going daily since January. The first month I concentrated on a few informants mainly from the Baptist Church. I spent ten days as a worker in one of the fish processing facilities of the town (“fish-house”) and joined the volunteer fire corps.

Cedar Key people were very curious about my family and me. There were different hypotheses to explain my presence in town: One theory was that I wanted to learn how to gather seafood in order to establish myself as a new gatherer. Another was that I was in some way connected to the Peace Corps

and wanted to learn how to gather seafood, so that I could teach the skills to Brazilian gatherers. The third hypothesis was that I was tied to Cuban interests in Florida. A few miles south of Cedar Key, Cuban interests, rumored to have been backed by ex-dictator Fulgencio Batista, had bought a beautiful piece of land along the Gulf Coast. They were said to be planning a huge development there, and the county people were seriously worried about a Cuban “invasion” of the area.

After two months, more or less, it became clear to almost everyone that I was not a Mexican farm worker trying to become a fisherman, or one of “those damn Cubans taking jobs away from Americans”. People seemed reassured that I was what I said that I was: a foreign anthropologist trying to get to know an American town. The fact that my family and I speak Portuguese and not Spanish helped to make the difference. The fact that I would be leaving Cedar Key by the end of year and also that I would be leaving the US to return to my country after the end of my research gave me a new status. From the category “potential competitor” I passed to the category “visitor”. This is a privileged situation in an American town. My family and I began to receive almost weekly invitations to dinner. The Cedar Key children came to play with my daughter. People began to call upon me spontaneously to give information. From that time on I established a friendly relationship with almost everybody in town. However, I never went to drink in the local bar because I would surely have got involved in a fight.

The city was an important Gulf Coast port during the XIX century, with a population of more than 5000. When I lived there, in 1974, there were growing numbers of retirees, mainly from the North of the country, who moved to the town because of its cheaper real estate prices. The Cedar Key people’s concept of Florida was limited to North and Central Florida, and did not include South Florida “Yankee country”. They considered the US South to be exploited and dominated by the North. Impositions concerning race integration were cited as an everyday example of this fact. Several of the residents talked about the Civil War as if it had happened only a few years ago. The burning of Atlanta, for example, was sometimes described with such emotion that one might think that the teller must have been present. There was a tendency to see the town as confederated to thousands of other towns, counties and states, voluntarily forming the political unity of the United States. The city charter placed the protection of its “sovereignty” as one of the first duties of

the town, even exaggerating the model drawn up by Alexis de Tocqueville. In the same charter it was written that “jurisdictions vested by Federal or State Constitutional law on towns, cities or municipalities are hereby declared vested upon said city”. Thus, to be American was considered to be a matter of free choice. Some years before, the city had had a local judge who even condemned people to forced labor.

Cedar Key was different from other small towns in Florida. It did not have a Black neighborhood, which was abandoned years ago. There were twelve scattered old Black men living in town, who offered their places in lines and opened doors deferentially for Whites. The non-existence of a Black neighborhood in town was an open question for me. Once, walking to do my job of apprentice fisherman, I found the answer: I heard a “pst! pst! George come here” in a low conspiratorial voice call from someone who wanted to tell me a Cedar Key secret. They were an aged couple from Pennsylvania who lived in a trailer. They were absolutely terrified, afraid of the revenge of the Cedar Key natives, and with good reason, because outsiders were barely tolerated and violence was always present in the town’s daily life. They were activists of a liberal organization called “Common Cause” and felt they had the duty to tell me about the Rosewood massacre. For their protection, I did not write their names in my dissertation. Now that Mr and Mrs Ford have long been dead, I believe their role in uncovering an important American historical event must be recalled. They also must be not forgotten because of their moral integrity.

My friend David Fleischer had heard rumors about the Rosewood massacre, but I did not pay much attention to the vague information he got about the episode. With the Fords’ revelation I had discovered a hidden fact in American history, which is today well documented. In 1923, the mere suspicion of a sexual attack on a White woman by a Black man led to a pogrom against the Black community of Rosewood, which was abandoned by the residents. Hundreds of attackers came from Jacksonville, Gainesville and other North Florida cities. Although some Cedar Key residents – deceased when I lived there - did participate, several of them refused to do so. One of my informants told me that he had been asked to lend his shotgun. He said that he would not “because (he) did not want to have (his) hands wet with blood.” Not only did the attackers kill the supposed rapist, they also attacked the Rosewood community. My informants estimated between 30 and 60 deaths, but after a later investigation, these numbers were reduced to eight (six

blacks and two whites). The survivors escaped into swampland where, it was said, some of them remained in hiding for three days. It was claimed that one man cut off the fingers of those he killed and came to the town bar with them hanging from his belt.

I wrote this information in one of the drafts of my dissertation presented to members of my doctoral committee, thinking that it was a lucky strike. But my data about Rosewood was placed under suspicion. I felt I had reached the limits of academic liberty. An inquiry was set up to know if the Rosewood incident was real or an invention. I gave a recorded testimony, but I asked them to turn off the tape recorder when I mentioned the Fords; I also mixed theirs with other names. The conclusion of the History Professor who was checking the information was that I had stumbled on an incident like others that occurred in the twenties in the US South.

Once it had been confirmed that the Rosewood massacre was not a fraud and that the credibility of the researcher was intact, information drifted from my dissertation into official American history. In 1983, a journalist of the St. Petersburg Times knew about the case and published a piece about the subject. As I learned in Google when writing this article, one year later CBS ran a story about it. In 1993, historians of the three Florida state universities published a report called “Documented History of the Incident Occurred at Rosewood, Florida in January 1923”. Google’s source “Wikipedia” informed that this “report provided support for a compensation bill...Lobbyists began to receive hate mail, some from the Ku Klux Klan...”. It also says that “In 1994, Florida Governor Lawton Chiles signed a \$ 2.1 million package ...that allowed for \$150,000.00 for each survivor and \$ 500,000.00 to set up a scholarship.” A book was written in 1996 about the incident I uncovered. An awfully bad film called “The Rosewood Massacre” (despite good actors) was made in 1997. I had the bad luck to watch this piece of art on a calm Sunday afternoon.

The unveiling of the Rosewood incident was an unexpected outcome of my research. It had impact on the memory of race relations in the US. It was proved, drifted to the press, came back to the universities (now in the History Department), became certified American history and finally law.

These tensions I went through during my research reflected not only my Non White Non Black foreign underpowered status, but American life as well. There is in the US an undeclared slow-motion race war which has to be apprehended in events like Rosewood in 1923, the civil rights movement in the six-

ties, the killing of my informant and several other Black men in the seventies, as well as the race riots that now and then occur in different American cities. Its existence is realized when a time scale of decades is considered.

There is a debate going on in today's Brazil about importing the American pattern of race relations and replacing the Brazilian pattern, which is based on mixing and on skin grade classification instead of the North American absolute racial opposition and prejudice directed towards skin color rather than genealogy. This movement for an isolated Black identity in Brazil results from the action of several non-governmental organizations funded by American foundations, chiefly by the Ford Foundation. Shocked by the racial hatred and racial violence that I personally witnessed in the US, I joined the opposition movement led by anthropologists Peter Fry and Yvonne Maggie and by Black leaders like Jose Carlos Miranda.

Race is a basic concept in understanding the US, but it is a common mistake to try to project the American race concept on other national situations. Historians and Social scientists naturalize "race" as if the US race system would be normal and desirable everywhere. Race systems which deviate from the American pattern are considered anti-democratic and racist when the opposite may be true. The Brazilian "race situation" is far from ideal, but the Brazilian ethnic system has been able to diffuse violence and avoid conflict between Blacks and Whites.

III

The main focus of my research was no longer directly on race relations, but on the community of Cedar Key as a sample American community. There were no more concrete "race relations" in town as there were not many Blacks around. But the values which engendered racism were there in an almost crystalline form.

Community studies were criticized in Brazil for underplaying the role of social classes. However, a Marxist-influenced sociological approach would not be a useful tool to understand a small community. Thus, I changed to Weber's concept of social class, which was easily associated with the concept of "community", becoming a useful empirical research tool. My previous concerns about social class were kept, but the influence of Brazilian Sociology disappeared from the approach as the American situation on the

ground imposed itself. Community and race are the basic concepts for understanding the US.

As an economist graduating in Anthropology, my research in Cedar Key was an attempt to explain local economics by examining its place in the wider socio-cultural system. It was a Karl Polanyi approach to an American community, a study in economic Anthropology. I related social classes defined by their position in the economic market with different levels of economic rationality. My main assumptions were that there was a “local upper class” related to the regional economic system at the time that the poorer did not have access to external markets. The members of the “local upper class” acted “rationally” in their economic behavior. The poorer displayed the tendency to act “irrationally” in their economic behavior. Such an economic relationship was embedded in cultural values and correlated forms of social organization. Those different degrees of economic rationality set the essential rationality of the whole socio-cultural system. Thus, traditional anthropological holism was considered a condition for the understanding of the economic system and its different patterns of economic rationality.

Fishing activities in Cedar Key were concentrated on the catching of fish, crabs and oysters. Most of the production was marketed through an institution called the “fishhouse”. Most of the fishermen were permanently linked to one of the two fishhouses of the town. It was in the fishhouse that they obtained credit to buy nets or traps or, simply, to pay the grocery bill. Often the loans were not paid back and the seafood gatherers lived in constant debt to the fishhouse owner. These characteristics gave to the relationship established in the fishhouse the stamp of patron-client relationships, which were common in the old American South.

I identified three categories of economic agents in the local fishing industry: individual producers, who sold their production to the fishhouse; independent producers, who marketed their own production; and fishhouse owners. The former worked alone, which was a very inefficient (“irrational”) way to organize production. The others worked with relatives or hired hands, which was the way to maximize the available capital resources such as boats, nets and traps. In each case I measured the time, the physical output and the monetary output of each class of fishermen. Thus the access to external markets was related to “economic rationality” if by such was understood cost reduction and profit maximization.

The lack of cooperation among most Cedar Key fishermen was associated with basic values, and here I was back again exploring Southern and American values. Two categories synthesized local values. These were the words “smart” and “proud”. To be “smart” meant to be able to mislead and not be misled. It was the essence of individualism. A “smart” person did not trust anybody and, by definition, was a good businessman. “Proud” meant to be independent, especially in an economic sense. It was also an expression of individualism because people did not like to receive instructions or suggestions which could be interpreted as orders. Businesses did not succeed in Cedar Key because the owner “kept telling people what to do” and the employees would always, therefore, abandon the job.

“Pride” was the code word to describe group participation and set hierarchies. A person was “proud” to be a man (women were considered to be less “proud”), to be a Cedar Key native, to be an American and to be White. The minimal church attendance of men compared with women’s attendance was explained by the “pride of life”: “men do not accept even God above them”, as one Pastor said. White women were classified as prouder than Blacks. These criteria influenced the division of labor. Any form of organized, cooperative, repetitive work in production lines was considered to be a woman’s job.

Such values affected all the forms of organization found in town. There was an evident tendency for non-hierarchical voluntary associations or for no voluntary organizations at all. The weakness of Cedar Key voluntary associations ran against the American middle class pattern, whose members are always joining clubs and all kind of voluntary associations. The rejection of voluntary associations was exemplified by failed attempts to organize fishermen’s cooperatives to run fishhouses.

In the same way, the village was not classified as integrating a political hierarchical model like the Brazilian, which has the municipality at the bottom and the Federal government at the top. As has already been seen, despite the real American political hierarchy, Cedar Key people considered the US a loose confederation of towns, counties and states. Thus the lack of hierarchies and the difficulties in cooperation were a major explanatory factor. To work in a group was not “smart”. Conflict could arise from the division of the workload and from the division of production. Besides, working together might imply some kind of hierarchy. Thus, I had to explain why some fishermen (“fishhouse owners” and “independent producers”) worked in a group

and why other fishermen (“individual producers”) accepted the fishhouse hierarchies. The first answer was the distance between rule and behavior. The other was kinship: those categories of fishermen were able to overcome the contradiction between cooperation and local values by the use of kinship. The kinship system replaced voluntary associations, weakening the rejection of functional hierarchies.

Kinsmen of the wealthiest fishermen (fishhouse owners and independent producers) had the lowest emigration rates in town. Cooperation among close kinsmen was desirable. Thus, the “pride” explanation was that they were “helping” a relative. In the same way, fishermen linked to the fishhouses were “helping a childhood friend”. The more prosperous the person, the larger his kinship network in town. Fishhouse owners had a larger kinship group than did independent producers, and the latter had a larger kinship group than individual fishermen, usually limited to their nuclear family. Thus, there was an association between the extension of kinship-based solidarity of the local upper class and the disorganization of the poorer fishermen. This was a key factor explaining the longevity of the local class system.

A striking aspect of my research was that the conceptual scheme, comprising ethnography and holistic description, was the very same, along general lines, as the one I had applied before to study an isolated Amazon Indian tribe. I treated an American town as I had treated South American Indians applying the genealogical model of inquiry and other traditional field techniques. The model worked so well because the town of Cedar Key, despite its economic and political integration in a large country, was rather isolated, as were the Indian tribes I had lived with. But the same methodology applied to isolated Indians and to twentieth-century Americans also reflects the work of generations of anthropologists refining the concepts of their discipline, as well as what Franz Boas called the “psychic unity of mankind”.

IV

In 1984, I finished the translation into Portuguese and the adaptation of my Cedar Key text to a book format. The book was a publishing success. I wrote for a wider public than the small Brazilian anthropological community and it was read by anthropologists, university and high school students and by several readers drawn from the category known as “educated public”.

Brazil has today a diversified and complex graduate university system. As a consequence, several research projects have been carried out by Brazilian anthropologists in the United States. These projects, with two exceptions, study Brazilian immigrants. In 2004, Maxine Margolies presented a bibliography about Brazilians in the US, which comprised almost 200 titles almost all authored by Brazilians⁶. After my 1975 work, one exception is Luis Roberto de Oliveira's dissertation on the Small Claims Courts system of Cambridge, Massachusetts. Small Claims Courts were a recent revolution in the Brazilian judiciary system and I suspect that Oliveira's dissertation has to do with it. The other exception is Rosane Manhães Prado 1993 study on small towns in the United States, a doctoral dissertation presented to Museu Nacional of Rio de Janeiro.

Studies of Brazilians immigrants are relevant because they investigate a dramatic new situation. Brazil was an immigrant country and there is, today, a Brazilian diaspora towards the United States, Japan and other countries. Studies by Brazilians of Brazilian immigrants in the US bear the additional interest of indirectly studying American society. However, these studies do not reach – and this was not their intention – the political nerve of the identity question, that is, the inversion of the colonial logic of Anthropology. I would prefer to see more Brazilian anthropologists studying WASP society. It would enrich Anthropology's cross-cultural experience and stress the non-acceptance of the premise of segregation, because the idea that Brazilians should study Brazilians, Blacks should study Blacks and so on is plain methodological segregation.

The construction and deconstruction of identities may be of major interest to Anthropology. My study of American culture was designed to have impact on Brazilian identity and not on American identity. It had, however, an unexpected impact on American race relations when it unveiled the memory of the Rosewood massacre. Even when anthropologists want to be faceless in politics, theirs is an essentially political task. This is true for the study a tribal population or for a whole nation. Thus, the choice of the research theme should always take into account its presumable effects on the identity of the studied society and on the identity of the anthropologist's society. This is an ethical and a methodological question.

6 In <http://sitemason.vanderbilt.edu/files/>

Thus, a basic problem is how Anthropology conceives itself. Anthropology may think of itself, at the same time, as an academic discipline and as Social Thought. Therefore, Anthropologists should make their research choices and write their texts aiming for the potential social impact of their work, rather than only thinking of their academic impact. Sensitivity towards the social impact of anthropological text associated with insights that only fieldwork can bring, results in a much more interesting and relevant body of knowledge.

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