P. Price
Real toads in imaginary gardens; Aspelin vs. Lévi-Strauss on Nambiquara nomadism

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Aspelin's recent article in *Bijdragen* on the question of alleged economic dualism among the Nambiquara (1976) would do much to clarify the place of this people in ethnological perspective, were it not for Lévi-Strauss' appended comments, which tend to revive the doubts that Aspelin has laid to rest. Under the circumstances, it may be of interest to the comparativist as well as the student of structuralism for a third Nambiquarist to take a stand on the matter.

Reduced to essentials, Aspelin claims that the Nambiquara are not seasonally nomadic, and in consequence, he casts doubt on the whole structure of associated oppositions in terms of which Lévi-Strauss comprehends Nambiquara life. In rejoinder, Lévi-Strauss defends his ethnography, asserting that differences between his and Aspelin's accounts must be owing to either 1) a decrease in nomadism between 1938 and 1968, or 2) unusual "political unrest" in 1938, or 3) a difference in the groups studied by the two ethnographers. I will examine these three possibilities in reverse order.

The Nambiquara linguistic family may be divided, as Lévi-Strauss perceived, into three languages, which he called *a*, *b* and *c* (1948: 8-13), and which I have called Southern Nambiquara, Northern Nambiquara and Sabanê (Price and Cook 1969: 688-93). Aspelin worked with the Mamaindé, who are speakers of the Northern language, and I have worked with all of the Southern groups, although most extensively with the *kithaulhú*. Lévi-Strauss also worked mainly among the

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Northern and Southern Nambiquara, being unable to communicate adequately with the Sabanê (1948:53). It is difficult to know which was the Northern group he worked with most closely, as the name he gives, *taründe*, appears nowhere else in the literature and is unknown to the Indians. The Southern group he knew best was the *w'akalitésú*, and it is rather unfortunate that he characterizes it as one of the "more northerly bands" (1976:32), which is accurate geographically, but not culturally. The group lives reasonably close to the *kithaulhú*, has maintained alliances with them, and appears to differ from them in very little. Thus, Aspelin and I, between us, have studied the same people as Lévi-Strauss, and the contention that he worked with a different group will not stand. In any case, I agree with Aspelin that the Northern and Southern Nambiquara differ very little in their economic practices, so that results should not vary significantly with the specific group studied.

The suggestion that there may have been unusual "political unrest", accounting for increased nomadism at the time of Lévi-Strauss' visit, is unsubstantiated. In the 1920s, there was an attempt on the part of the Indian Protection Service to establish a Post among the Nambiquara. With the ascent to power of Getúlio Vargas in 1930, the Indian Protection Service became moribund, and the Post ceased to function. The telegraph line which crossed the Nambiquara region was rendered obsolescent by the advent of radio, and missionaries, hampered by a lack of funds during the Great Depression, were absent from the region during most of the 1930s. In other words, at the time of Lévi-Strauss' visit in 1938, the Nambiquara were enjoying a hiatus in the process of Western expansion into their region. It is difficult to see what factors might have been responsible for the unusual degree of political unrest which, Lévi-Strauss says, "obtained both between the Indians and the Brazilians and between the Indian bands themselves" at that time (1976:32). The Nambiquara today are highly factious and tend to mistrust the *civilizados*, and it is probable that this was true in 1938. But there is no reason to suppose that it was more true in 1938 than it is now.

The proposition that the Nambiquara were more nomadic at that time bears careful examination. But the use of the terms "nomadic" and "settled" reflects an unfortunate dualism in anthropological thinking, and the notion that a group might be "nomadic" in one season and "settled" in another represents no improvement, as the utility of the basic opposition continues to be accepted without reflection. What is
needed in place of these generalities is relevant information on Nambiquara agriculture, house-building and villages.

The practice of agriculture is extremely important to the Nambiquara, and has been so for a long time. A preference test conducted with three Mamaindê men showed, to Aspelin’s own surprise, that they would rather fell trees to make gardens than go hunting or spend time in any of four activities, including “stay home and rest” (1975: 186-88). The savannah Nambiquara grow 17 different kinds of manioc, and three others have been lost within living memory.

Agriculture figures prominently in many myths. In particular, the myth of the Flute Boy accounts for the origin of agriculture, describing how the body of the protagonist was transformed into twelve traditional cultigens (see Table I). It is noteworthy that ten of these cultigens are each derived from a single part of his body, while manioc and corn, which are dietary staples, are derived from more than one part.

### TABLE I

**Traditional Cultigens**

| Cultigen    | Description                                                                 |
|-------------|------------------------------------------------------------------------------|
| walins’ú    | manioc                                                                       |
|             | leg bones (which resemble manioc stems)                                      |
|             | fingers (which resemble manioc leaves)                                       |
|             | flesh (which gave rise to manioc dough)                                      |
|             | brains (which gave rise to polvilho)                                        |
| qatyantisú  | black bean                                                                   |
|             | ribs                                                                         |
| qâtsú       | fava bean                                                                    |
|             | ears                                                                         |
| pêlhú       | squash                                                                       |
|             | eyes (which resemble squash seeds)                                           |
| yaldúsú     | arrowroot                                                                    |
|             | spine (which resembles the root)                                            |
| hakkisú     | cará                                                                         |
|             | testicles                                                                    |
| yap’ãnsú    | tãá                                                                          |
|             | liver                                                                        |
| kayâtsú     | corn                                                                          |
|             | teeth (which resemble the kernels)                                          |
|             | hair (which resembles the floss)                                            |
| sanêsú      | pepper                                                                       |
|             | gall bladder                                                                  |
| êtsú        | tobacco                                                                       |
|             | louse eggs (which resemble the seeds)                                        |
| walutsú     | gourd                                                                         |
|             | skull                                                                         |
| t’uhs’ú     | urucu                                                                        |
|             | blood                                                                         |

While the informants who recited and interpreted this myth also grow sweet potatoes, peanuts, bananas and cotton, whose use is well integrated into their material culture, they insist that these cultigens
are not parts of the Flute Boy, but were acquired from the first Brazilians who entered their area. These Brazilians, they say, were black, and robbed Indian women. These statements are supported by historical evidence. There were colonies of escaped slaves in the Nambiquara area during the Eighteenth Century, and in 1795 Ensign of Dragoons Francisco Pedro de Mello led an expedition against one of them, established near the Rio Piolho. He reported that the escaped slaves “waged a perpetual war” with the “heathen Cabixês” (Nambiquara), “in order to kidnap women, by whom they had their Caboré children”. He found “huge plantations” that included peanuts, sweet potatoes, bananas and cotton (cited in Roquette-Pinto 1935: 38).

Thus, it would appear that the Nambiquara had a developed agriculture prior to their first contact with Western society in the Eighteenth Century. Certainly João Leme do Prado encountered gardens of manioc in 1770 (cited in Castelnau 1851: 165-66), and Von Martius, working from the notes of the Austrian naturalist, Johann Natterer, who lived in the southern tip of the Nambiquara region during the years 1826-29 (Pelzeln 1871: X, XI), characterizes them as having a “bountiful agriculture” (1867: 244).

Nambiquara houses, as opposed to windscreens, require a reasonable amount of labor to build. However, they are thatched with sapé grass (in the savannah) or with the leaves of the açai palm (in the Guaporé valley), which last only a few years. They rapidly become infested with crickets, cockroaches and fleas, which begin at some point to try the patience of even the most stolid individual. At these times, the simplest effective expedient is to abandon the house and build a new one. Houses also fall into disrepair because of the practice of pulling out handfuls of thatch to light fires, and when an inhabitant dies, the house is burned. All of these factors imply that houses must frequently be repaired or rebuilt. These activities are most commonly undertaken at the beginning of the rainy season, when the protection of a house becomes more necessary. In the Alto Sararé, houses burned because of deaths in May 1968 (end of the rainy season) had still not been completely replaced by November. In many other villages, I have seen families live contentedly in a windscreen during the dry season, and only build a house at the beginning of the rains. In fact, I have seen some Nambiquara partially destroy their houses at the beginning of the dry season, taking down one wall, for example, to let the breeze enter. At the beginning of the rains, these damages must be repaired. However, despite this tendency to deteriorate, well-built houses, such as those
described at the beginning of this century by Rondon (n.d.: 118, 121, 136, 138, 145, 259), Pyreneus de Souza (1920: 403) and Roquette-Pinto (1935: 219-20), may last for three years or more.

Houses are generally built in the savannah, but not too far from the gardens, which are in the forest. However, a place that has houses is considered a village only if someone is buried there. When a person dies near home, he is buried in the cleared space between the houses, but when he dies far from an established village, he may be carried to a potential gardening area, where a new village can subsequently be established. Informants have explained this matter as a general principle, and I know of several actual cases in which a corpse was transported to such an area for burial. In an extreme instance, the body of a former leader was disinterred several days after burial and carried to an area that seemed more promising. In Nambiquara thinking, a place where no one is buried is not a village, no matter how many people live there; and a place where someone is buried is a village, even if no one lives there. I once collected a list of names of former villages and began to ask informants who used to live in each of them, only to learn in a few instances that the “village” in question was merely a camping place where a child was buried. In other instances, the village had been occupied by several families.

Nambiquara villages are named, usually for a salient geographic feature or for a prominent ancestor who is buried in them. The word for village, *s‘ih-yen-*, is composed of the morphemes ‘house’ and ‘open, flat area’, which would suggest the gloss ‘a place for houses’ rather than ‘a place where houses are’. When a house is destroyed, for whatever reason, the inhabitants generally rebuild on another site in the same village. Villages are occupied continuously for a period that seems to average between 10 and 15 years, and once abandoned, may be reoccupied after a lapse of several years. This pattern of residence was apparently operative at the beginning of this century, for Roquette-Pinto says, “Following the trail that took us to the village on the Juina [apparently *olakiyensú*] ... we passed through different places where the village had been” (1935: 223). Von Martius, following Natterer, would suggest that it was operative early in the last century as well, for he says that the Cabixi (Nambiquara) had “feste Wohnplätze” (1867: 244). The village visited by Lévi-Strauss in 1938 (apparently ‘uhsáyensú’) had no houses at that time (1955: 339; 1976: 32). This does not mean that it did not have houses previously, nor that it did not have houses subsequently. Lévi-Strauss reported accurately his informants’ intention
to rebuild at the beginning of the rains, but he erred in supposing that
the houses would be built only for the duration of the rains. They would
last as long as conditions permitted.

Even though I have shown a developed agriculture associated with
reasonably permanent villages to be of long standing, it might still be
supposed that the Nambiquara formerly left their villages for longer
periods during times of agricultural inactivity than is now the case.
There is, however, no evidence that they ever engaged in trekking, as
did the Shavante (Maybury-Lewis 1974: 53-59). In 1968, I had the
good fortune to live for seven months in the Alto Sararé, with a group
that was only then coming into peaceful contact with Brazilian society.
This period included the entire dry season, and there was no apparent
nomadism. Rather, individuals came and went every few days, exactly as
do Aspelin’s Mamaindê.

In sum, the Nambiquara house, though well-constructed and seem-
ingly permanent when first built, is not very durable, and damaged or
destroyed houses are generally not repaired or replaced until the begin-
n ing of the rains. However, a Nambiquara village is not defined by its
houses, but by its burials. Houses are rendered ephemeral by the vagaries
of the material world, but villages are as permanent as memory and
tradition can make them. It is unfortunate, but not surprising, that
anthropologists, as representatives of a highly materialistic society, have
confused Nambiquara houses with Nambiquara villages.

The evidence, then, does not suggest a rudimentary agriculture asso-
ciated with flimsy, temporary houses and complemented by seasonal
nomadism, as Lévi-Strauss would have it, either 30 or 100 years ago.
His insistence on defending his ethnography does a disservice both to
the Nambiquara and to ethnology. His fieldwork was very brief, and
conducted under difficult circumstances; clearly he cannot have obtained
the same depth of vision as Aspelin who has spent a much greater
period living with the Indians. Why does he not admit that his reports
on the Nambiquara may be in error? Surely his reputation does not, at
this point, depend on his ethnography.

The fact that he does attempt a defense, in the face of a mountain
of contrary evidence, is significant. It would appear that Lévi-Strauss,
like many other anthropologists, does not understand Lévi-Strauss.

The crux of the matter is his attitude toward evidence. The essence
of scientific thinking, as Russell has pointed out (1975: 243), is an
attitude of systematic doubt. One must continually re-examine what
one believes to be the case. One must balance all the evidence before
arriving at a conclusion, and if new evidence tips the scales in the other
direction, one must be ready to reverse one’s conclusion. In this context,
Aspelin’s epistemological stance, that facts are what everyone agrees on
(1976: 26-27), is highly suspect. If we allow facts by consensus, a racist
studying racists will report his prejudices as facts. It is not important
whether other people agree with the observer’s conclusions; what matters
is whether the observer has been honestly unable to doubt his own
conclusions.

Opposed to the attitude of systematic doubt is the attitude of af-
firmation, which characterizes dreams, art and religion. The person
who has such an attitude does not weigh his evidence, but uses it
selectively to build another reality. This artistic vision is not a scientific
model, but it is not therefore “wrong”. If works of art are successful,
they say something transcendent about the human condition in general.
They become, in Marianne Moore's phrase, “imaginary gardens with
real toads in them”. Because we recognize that the premises of art are
different from those of science, we are not accustomed to criticize works
of art for descriptive errors. Shakespeare’s profound comprehension
of human nature is not invalidated if Lear never really existed, nor is
Keats’ response to Chapman's Homer less meaningful because Balboa,
and not Cortez, discovered the Pacific.

The same may be said for Lévi-Strauss. Those of us who have studied
the Nambiquara know that the ethnographic situation is in many
respects different from that which he presents. It is also widely held
that he has, in Aspelin’s phrase, “taken the liberty of reorganizing”
other, published, data (1976: 27). But this is not relevant criticism
unless we make the mistake, as Lévi-Strauss himself apparently does,
of viewing his work as science. We should take our cue from those who
considered Tristes Tropiques for the Prix Goncourt, judging it not as
science, but as art. The Nambiquara did not provide Lévi-Strauss with
information, but with inspiration. Recognizing this, we should neither
read Lévi-Strauss to know about the Nambiquara, nor should we criticize
him because his portrait is not faithful to the original.

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**COMMENTS BY LÉVI-STRAUSS**

Nothing could be farther from my mind than to try to defend my ethnography at all costs in the face of conflicting evidence. I am too aware of the limitations in time, accessibility of the natives, linguistic communication and mere security to which I was subjected in a difficult region, forty years ago. This is why in my comment on Professor Aspelin’s paper, I have been content to state the facts that were available to me at the time, and to point out that, on the basis of those facts, “it did not seem unreasonable to assume ... that the northern bands were mostly
nomadic during the dry season, [etc].": a very modest claim indeed, and carefully phrased so as not to be taken for a pronouncement on the objective validity of my interpretation.

Should it be proven that the rhythm of Nambikwara economic life was more complex than it then looked to be, that they were more agriculturalist and less nomadic than they appeared during the single dry season that I spent among them, I would have nothing to object. And all the less so, since I have myself insisted that the Nambikwara, as I saw them, illustrated a case of pseudo-archaism, and that the cruder aspects of their material culture could not completely conceal a former and probably higher cultural level (*Anthropologie structurale*, ch. VI). Therefore it would not be disappointing to learn that the bands studied over a much longer period by Drs. Price and Aspelin were actually still close to that former level which I could only postulate, and mostly from certain peculiarities in the way of life of the band I knew best. Upon my return to Utarity in December 1938, I found that particular band still wandering; they certainly had not led a settled life during the dry season.

On the other hand, there are three points in Dr. Price's valuable paper with which I cannot agree.

1) The first concerns the liberty of reorganizing my data which I am supposed to have taken in previous circumstances. This indictment already appeared in Professor Aspelin's paper, and I did not challenge it then because it seemed to lie outside the scope of the main discussion. May I recall that this accusation originated with respect to a paper published in this periodical, and in which I had stated that a north-south axis cross-cutting the moieties was operative in the structure of the Bororo village. As this feature had not been mentioned previously, it was said that I had invented it. However, since the publication of my paper (*Bijdragen*, Deel 112, 2e Aflevering, 1956, pp. 99-128), the existence of the north-south axis in that particular area has been confirmed by subsequent observers (J. C. Crocker, "Reciprocity and Hierarchy among the Eastern Bororo", *Man*, n.s., vol. 4, no. 1, March 1969, pp. 57-58, n. 9; Z. D. Levak, Kinship System and Social Structure of the Bororo of Pobojari, A Dissertation Presented to the Faculty of the Graduate School of Yale University for the degree of Doctor of Philosophy, 1971, p. 125, n. 1 (mimeogr.)). Therefore the vicious and gratuitous indictment that I have "manipulated" the data can once and for all be dismissed.

2) Dr. Price claims that my suggestion of political unrest in the
Nambikwara area is unsubstantiated. Just before my stay, the Parecis telegraph station had been under attack several times by the so-called Beígos de Pau, close neighbors of the Nambikwara, and the general state of alarm still extended to Utarity and Juruena, the reason alleged being that if the Beígos de Pau were on the warpath, the Nambikwara could well be too. Furthermore, the Nambikwara band whose daily life I was sharing in the Utarity region, was the same one that, some years earlier, had slaughtered a group of Protestant missionaries. Not unreasonably, whenever those natives appeared near the telegraph, they were looked upon as potential murderers. During my stay in Campos Novos, an open battle almost broke out between two Nambikwara bands. Finally, in Vilhena, smoke from the campfires of an unknown band was seen regularly a few kilometers away, causing unrest not only among the Nambikwara sharing camp with me, but also among the telegraph people. These were certainly not peaceful conditions!

3) *Tristes Tropiques* was not honored with the Prix Goncourt. Believe it or not, it was considered too scholarly.

**COMMENTS BY ASPELIN**

I would like to thank both Professors Lévi-Strauss and Price for their comments on my article on “Nambicuara Economic Dualism . . .”, as well as the editors of *Bijdragen* for the opportunity to respond to those remarks at this time. I very much regret, however, that Professor Lévi-Strauss did not have a copy of the final version of my article on which to base his remarks. Rather, he had only a preliminary version which unfortunately did not include my detailed consideration (on pages 17-28) of some of the possible reasons for the apparent differences between our two ethnographies. Although some of his comments may for this reason not really have been directed at the argument that was ultimately published, after all, he has raised some valuable questions for our consideration as we attempt to further clarify these issues.

In addition to providing some very useful supplementary information (both from his own observations and from those in earlier accounts of the Nambicuara) regarding these points, Professor Price has also considerably complicated the problem by the direction which he has taken in his closing remarks, although I am not surprised that he would take this tack. He has essentially raised once again, although in a somewhat different form, the question of “intent” which I had previously
discussed (pp. 27 f.). He contends that Lévi-Strauss' account of the Nambicuara should be judged as art, rather than as "science". Although I had previously suggested that some of Lévi-Strauss' statements regarding the Nambicuara might be considered more theoretical than descriptive, this is quite different from what Price has accused him of doing. If Price is right, there is no way that any reader who has no first-hand knowledge of the Nambicuara (or of the other societies about whom Lévi-Strauss has written) would ever be able to accept any of his material as "accurate", or whatever other empirical criterion one might wish to employ in this sense. Price seems to have forgotten that this same question of intersubjective reliability has been raised many times before regarding the ethnographic enterprise in general, and I would refer him to the literature on that issue before he again attempts to single out any particular ethnographer to chastise in this regard.

More importantly, the specific issues which Professor Lévi-Strauss has raised in his comments on my paper are each worthy of a much more careful and complete consideration than that provided by Professor Price here.

Certainly, in discussing differences and similarities among groups, one must pay close attention to the factor of time and the changes which may have occurred, for whatever reason, over the particular time span involved. Pages 18 through 25 of my original article are devoted to exactly this question. There, I found four major forces which might have affected Nambicuara residence and subsistence in the last seventy years: 1) the introduction of new cultigens; 2) the introduction of steel tools; 3) the introduction of non-indigenous diseases and consequent depopulation; and 4) political restrictions by the intrusive Brazilians on Nambicuara mobility. I discussed each of these in some detail and found them to have been either a) already effective by the time of Lévi-Strauss' research; b) so recent as not yet to have induced changes of the sort we are looking for; c) non-productive of such changes regardless of when they occurred; or d) counter-balanced in some manner so that any changes which were so induced could not affect the overall situation under discussion. I would certainly be the last, therefore, to deny that any changes had taken place in Nambicuara culture in the last thirty years. Those parameters of residence and subsistence which I described in my original article were not sufficiently affected by these various forces of change, however, to account for the degree of difference which exists between Lévi-Strauss' ethnography and mine.

Price is quite right in pointing out that Lévi-Strauss' taxonomy of
the northern Nambicuara bands is rather confusing and that it might therefore be difficult for the reader to assess the exact degree to which Lévi-Strauss and I are actually comparing notes on the same or similar people. Indeed, in his comments on my article, it seems quite likely that Lévi-Strauss might have confused the Maminde of the Cabixi River whom I studied with the so-called “Cabixi”/Nambicuara near Vila Bela, far to the south, thus attributing hundreds of kilometers to the distance between the two groups he and I studied. This is not at all the case: they were really quite close both culturally and geographically. Most of Lévi-Strauss’ information on “the northern Nambicuara” dialect area (as shown in the map on page 6 of my earlier article) was obtained from a group which Lévi-Strauss has called the “Tarunde”. He met this particular group, as a matter of fact, at the telegraph station of Vilhena which has been the northeastern boundary of Maminde territory since at least 1910 (as shown in the maps on pages 6 and 20 of my earlier article, as well). Although “Tarunde” is no longer used as a band label by the northern Nambicuara themselves, it is fair to say that the Maminde and the “Tarunde” were extremely close neighbors and shared the basic parameters of their culture in common. Were my earlier article to have been written about the so-called “Cabixi”, on the other hand, Lévi-Strauss’ point would have been much more appropriate, for their dependence on corn as a basic staple would decrease the comparability of their ways of life to those of the other Nambicuara groups such as the Maminde or “Tarunde” who are dependent primarily on manioc, in terms of such things as seasonality of harvest, storability of product, allocation of labor time by season and by sex, and the like (as pointed out on page 18 of my original article), even though Price, who has worked with one of these southwestern “Cabixi” groups on the Sararé River, finds that even there the bulk of my analysis also holds true. Since the “Tarunde” appear to have provided nearly half of all of Lévi-Strauss’ detailed information on Nambicuara daily life, their cultural and geographic proximity, if not contiguity, to the Maminde render my comparative analysis even more telling. Our ethnographic differences simply cannot be this simply explained away.

On the other hand, it is only fair to point out that the particular Nambicuara farm and village site which Lévi-Strauss visited were actually those of the other Nambicuara group which provided most of the rest of his information on Nambicuara daily life. This band, the wakaletosu, lived on the far eastern edge of Nambicuara territory, in a somewhat different ecozone than either the “Tarunde” or the Maminde
(to say nothing of the "Cabixi"). Thus, some of the differences between his observations of "Nambicuara" agriculture and residence based on the wakaletosu and mine based on the Mamaindê may or may not have been consequent upon the sub-cultural and geographical distance separating these two Nambicuara (dialect) areas, although I think these would have been minimal, in any case. To argue the point in terms of vague geographic referents such as "northern" or "almost northern" or whatever, as Price and Lévi-Strauss have done, is only to confuse the issue hopelessly. If Lévi-Strauss' comments on "Nambicuara" agriculture and village residence are only specific to either the "Tarunde" or the wakaletosu, and are not even applicable to the other, as his comments on my article and my analysis here seem to suggest, and can be generalized even less to the rest of the Nambicuara as a whole, then he should have told us so thirty years ago. His discussions of Nambicuara subsistence and residence speak of "the Nambicuara" in general, however. The question then really becomes whether or not Lévi-Strauss' ethnography of the Nambicuara is adequate, rather than whether it is accurate in any absolute sense.

Simply to suggest, as Price has done in conclusion, that Lévi-Strauss' account of the Nambicuara in *Tristes Tropiques* is more a work of art than one of ethnography, and that my argument with Lévi-Strauss on the basis of the passages I have quoted from *Tristes Tropiques* regarding Nambicuara subsistence and residence is therefore equivalent to looking for "real toads in imaginary gardens", is to do those who read our arguments a great disservice and to indicate an ignorance of the extant Nambicuara ethnography on Price's part and a carelessness in his reading of my original article which I am sorry to see. I had thought I had made it quite clear, on pages 2 through 5 of that article, that not only did Lévi-Strauss' account agree very closely with that provided ten years later by Kalervo Oberg, but that the passages which I had cited from *Tristes Tropiques* were also to be found in Lévi-Strauss' more scholarly work on the Nambicuara, which, together with Oberg's material, forms the bulk of the Nambicuara ethnography in the Human Relations Area Files and other cross-cultural data sources, for example. Although I suppose that it is possible, I have some difficulty in accepting the view that the whole Human Relations Area Files should be regarded as merely an aesthetic creation. The fact that *Tristes Tropiques* itself received the Prix Goncourt as a work of art (or, more appropriately, literature) is therefore just as irrelevant for our investigation of Nambicuara ethnography as are most of Price's remarks regarding it.