P. Aspelin
Nambicuara economic dualism: Lévi-Strauss in the garden, once again

In: Bijdragen tot de Taal-, Land- en Volkenkunde 132 (1976), no: 1, Leiden, 1-32

This PDF-file was downloaded from http://www.kitlv-journals.nl
PAUL L. ASPELIN

NAMBUICUARA ECONOMIC DUALISM: LÉVI-STRAUSS IN THE GARDEN, ONCE AGAIN

The Nambicuara-speaking peoples, who live in the northwestern part of the state of Mato Grosso, Brazil, seem to have been one of the important data-sources from which Claude Lévi-Strauss has derived many of his principal ideas (such as that of the dualistic nature of some cultures) as well as many of the examples involved in the generation or presentation of those ideas. This is not surprising in light of the fact that they were one of the first non-Western cultures among which he did field research. They thus appear to have made a strong impression on the development of his thinking during this early stage in his career, as that is portrayed in the autobiographical portions of his *Tristes Tropiques* (1964, 1974). Recent research has shown, however, that some of the important data reported by Lévi-Strauss regarding the Nambicuara are apparently erroneous or misleading. The nature and seasonality of food production activities and their relationship to group mobility are specific concerns of Lévi-Strauss' original accounts; they are thus also the specific concern of this paper.¹

The Nambicuara are probably regarded, by most people who are familiar with them through Lévi-Strauss as the primary source, as a semi-nomadic people who depend primarily on gathering, secondarily on hunting, and thirdly and rather unimportantly on some few crops which they plant in fields cleared with standard slash-and-burn methods but which will not produce enough to tide them through most of the year. Since this impression is not very accurate, it is unfortunate that it seems to have become rather strongly entrenched in the secondary

¹ An earlier version of this paper was presented at the Southern Anthropological Society Meetings in Blacksburg, Virginia, in April, 1974. I am grateful to Professor Ronald Reminick of the Cleveland State University, Dr. P. David Price of the Fundação Nacional do Índio, and to that audience in Blacksburg for their comments on the earlier versions. Of course, however, all responsibility for this present article still resides solely with me. Field research for this article was generously supported by the National Institutes of Mental Health and the Cornell University Latin American Studies Program.
literature as well (e.g. Textor, 1967: 73 f. and “paragraph” FC-51; Udy, 1959: 152 et passim).

Lévi-Strauss is not the only anthropologist to have visited and written about the Nambicuara since their first major peaceful contact with the outside world, in 1909 (Rondon, 1922: 56, 103), although he is certainly the best known of those who have done so. Beginning with Edgar Roquette-Pinto (1919), these anthropologists include Kalervo Oberg (1953), Desidério Aytai (1964), Lajos Boglár (1969), Cecil E. Cook, Jr. (Price and Cook, 1969), René Fuerst (1968), P. David Price (1972), and myself (Aspelin, 1975). Next to Lévi-Strauss', the second-most widely-read account of the Nambicuara is probably that of Oberg, who visited them in 1949. At that time, access to the area inhabited by the Nambicuara was somewhat difficult; it was even more difficult in 1938, when Lévi-Strauss visited them.

Because of these difficulties of access, neither of the authors of these two best-known accounts of the Nambicuara (Oberg, 1953; Lévi-Strauss, 1958, 1964, 1974) appears to actually have seen either a functioning Nambicuara village or its concomitant farmland or garden (which is called a roça in Portuguese). The importance of this fact, which is probably not widely recognized by most readers of their reports, is that a serious distortion has thereby been introduced in the ethnography regarding these villages and roças and the nature, timing, and importance of the activities associated with them. The reader of Lévi-Strauss' popular Tristes Tropiques (1964, 1974), for example, will undoubtedly learn from the lyrical prose of this work that the Nambicuara-speakers are a highly mobile, essentially semi-nomadic, people who, for only a small part of their time, maintain a short-lived residence in their villages while preparing the small roças which supplement their primary gathering and hunting activities. Nambicuara residence is portrayed as so unimportant and so brief as to make their villages almost unworthy of the name. A careful reading of Oberg (1953) also reinforces this impression.

From 1968 through 1971, I had the opportunity to undertake nearly three years of field research among the Nambicuara-speaking peoples. On the basis of that research, I suggest that the exact opposite of the

---

2 Although these dates refer specifically to publications cited in the bibliography to this article, they also give an idea of the approximate dates of each person’s respective researches among the Nambicuara.

3 Most of the material on the Nambicuara in the Human Relations Area Files, for example, comes from these two authors.
impression formed by Lévi-Strauss' and Oberg's accounts is, and was, more the case: that is, I have found, first, that the Nambicuara are not seasonally nomadic at all, and, second, that they actually depend much more on horticulture than these accounts report. I have found that the Nambicuara maintain substantial agricultural plantings, upon which they rely for a good deal of their annual food consumption. They do, certainly, rely on hunting and gathering for certain parts of their food supply, notably protein and fresh fruits; but the basic staple of their food supply is the manioc which they plant in their farm plots.

Although the Nambicuara-speaking peoples are divided into several mutually-unintelligible dialect groups, as Price and Cook (1969) have shown, these dialect groups share an essentially common culture. Unfortunately, in many of Lévi-Strauss' writings regarding the Nambicuara, and most especially in *Tristes Tropiques* (1964, 1974), it is often very difficult to tell which of these dialect groups he is actually writing about. A careful reading and comparison of the examples he uses, however, will show that quite often he is speaking of, or using as an example, the northwestern dialect group which includes the Mamaindé among whom I conducted my own field research. (The location of the Mamaindé, and of these various dialect groups, is shown in figure 1).

What, exactly, did these two authors, Lévi-Strauss and Oberg, have to say about Nambicuara residence and horticulture? Oberg has stated quite clearly his opinion on this subject:

> Although the Nambicuara practice a shifting agriculture, their dependence upon the wild animal and plant life of the region is so extensive that they might well be classed as nomadic hunters and collectors... The dependence upon food crops in relation to game, fish, and wild food plants is difficult to gauge. The statements of missionaries, however, appear to indicate that cultivated crops do not provide a year-round food supply and may actually play a minor role as compared with dependence upon noncultivated resources.

In late August, when the cicada begins to sing, and the first thunder is heard, the Nambicuara return to undertake their planting activities. ... After the planting is finished and the field is growing well, the people leave on their continuous rounds of hunting, fishing and collecting (1953: 87, 90f.; emphases mine).

Lévi-Strauss elaborates on this theme in his *Tristes Tropiques*, where he says also that Nambicuara life is divided into two distinct halves, so that for part of the year the Nambicuara are sedentary and rely on the produce of their roças while during the other part of the year they are nomadic and rely exclusively on hunting and (primarily) gathering:

> The Nambikwara year is divided into two distinct periods. During the rainy season, from October to March, each group lives on a hillock above a stream, where they build crude huts with branches or palm-fronds.
They burn clearings in the gallery-forest growing in the damp valleys, and plant and cultivate gardens. They grow mainly manioc (sweet and bitter), several varieties of maize, tobacco, occasionally beans, cotton, ground-nuts, and gourds. The women grate the manioc... and squeeze out the juice... Gardening provides enough food to last during part of their sedentary life. The Nambikwara even preserve cakes of manioc by burying them in the ground; they dig them up, half rotten, a few weeks or months later. At the beginning of the dry season, they leave the village and each group breaks up into several nomadic bands. For seven months, these bands wander through the savannah looking for game, especially small creatures such as larvae, spiders, crickets, rodents, snakes and lizards, as well as fruit, seeds, roots or wild honey; in short, anything which will prevent them dying of hunger. Their encampments, which they set up for one or several days, or sometimes even for a few weeks, consist of crudely constructed shelters... During this period, the search for food is an all-absorbing activity. The women are equipped with digging sticks...; the men hunt with large palm-wood bows.... Nambicuara economy has two aspects: hunting and gardening, which is done by the men, and collecting and gathering which is the concern of the women. While the male group is away hunting all day with bows and arrows, or is busy gardening during the rainy season, the women, armed with their digging sticks, wander across the savannah with the children, gathering, uprooting, clubbing, capturing, or seizing anything they come across that can be used as food: seeds, fruit, berries, roots, tubers, eggs and small animals of every kind. At the end of the day, husband and wife come together again around the fire. When the manioc is ripe and while there is still a supply, the man brings back a load of roots, which the woman grates and kneads into flat cakes, and if the hunt has been successful, the pieces of game are quickly cooked under the red-hot ashes of the family fire. But during seven months of the year, manioc is scarce; as for hunting, it is largely a matter of luck in these sandy wastes, where animals are few and far between and hardly ever leave the thickets and grazing grounds near streams or water-holes, which are scattered over vast tracts of semi-desert scrub. It follows that the family has to rely mainly on female gathering and collection [which] during half the year represent the Nambikwara's only hope of survival.

...although the sexual division of labour assigns an essential role to the women (since the family's food supply to a very large extent depends on female gathering and collecting), their occupations are looked upon as an inferior form of activity; the ideal form of life is considered to be agriculture or hunting... For the Nambikwara the relationships between men and women are connected with the two poles around which their existence is organized: the sedentary agricultural life based on the twofold male activity of hut-building and gardening, and the nomadic period during which food is chiefly supplied by female gathering and collecting — the first representing security and gastronomic well-being, the second uncertainty and famine.

[As opposed to the gathering life of the dry season,] they have quite a different conception of their sedentary life (the specific and ancient character of which is nevertheless proved by the original species they cultivate), on which the unchanging sequence of agricultural operations confers the same durable quality as that possessed by the reincarnated masculine souls, the stable rainy season dwelling, and the plantation which will once more spring to life and yield crops 'when the death of the previous cultivator has been forgotten' (1974: 275, 287-290; emphases mine).

In the English translation of his earlier, more scholarly, publication on
the Nambicuara, which essentially parallels on pages 14-17 and 61 the text quoted above, Lévi-Strauss' opinion of the transitory nature of Nambicuara residence is even more clearly shown:

At the arrival of the dry season, they abandon the village, or rather the site of temporary residence, and each group "breaks up" if one can say that, into several nomadic bands (1958: 14; emphasis mine).

These accounts thus clearly present a picture of the Nambicuara as being well-fed and entirely sedentary during the rainy season (which occurs from October through March) and hungry and entirely nomadic during the dry period (from April through September). Let us at this time abstract the main points they have made regarding Nambicuara activities during each season of the year, in order to deal more carefully with each one.

During the rainy period, we are led to believe from these reports of the Nambicuara:

1. They build new, temporary villages, in which they remain only for the duration of the rains.
2. They burn their rogas (having already cut them at the beginning of the dry season in May and June).
3. They plant their rogas.
4. They have ample food (from the rogas).
5. They completely harvest their rogas, storing whatever manioc cakes are left in holes in the ground.
6. They rely largely on the labor of the males in producing food.
7. Little, if any, hunting and gathering is done, this being mostly done in the dry season.

During the dry season (from April to September), these ethnographies indicate:

8. They leave that village for good (since it was only "temporary"), after cutting their gardens somewhere in the gallery-forest.
9. They have little food.
10. They rely entirely on the products of the hunt and of gathering, with the slight addition of the manioc cakes which were buried in a hole earlier (as indicated in number 5, above).
11. They rely largely on the labor of the females in producing food.
12. Little, if anything, is done regarding roça activities, these being mostly done in the wet season.

Certainly, this all seems both clear and quite logical. Unfortunately, however, Lévi-Strauss, Oberg and the missionaries referred to by Oberg
were all quite wrong regarding the seasonal production activities of these people. How could this be?

Although it usually difficult to tell from either Lévi-Strauss' or Oberg’s accounts which of the data they present was obtained through first-hand observation and which was obtained at a distance through informants, a careful reading of the texts indicates that neither of them ever visited an independent Nambicuara village or its roça, although they certainly wrote much about such roças. Oberg never got farther in his travels than the telegraph station and mission outpost of Utarity, on the eastern boundaries of the Nambicuara-speakers' territory (as shown in the map in figure 1). There, he obtained all of his information from

![Figure 1. Nambicuara territory](After Price and Cook, 1969: 689; and Price, 1972: 64)
some members of the southeastern dialect group of the Nambicuara who happened to be visiting the mission station during the dry season.\textsuperscript{4} Although the territory of this dialect group was not far from Utiarity, Oberg did not visit their village nor their garden. His information is all second-hand, either from those southeastern Nambicuara or from the missionaries resident at Utiarity, who may have visited the villages (but, I suspect, did so only for short visits, if at all).

Lévi-Strauss was following the path of the telegraph line which had been built from Cuiabá (the capital of the state of Mato Grosso) to Porto Velho (the capital of the territory of Rondônia) by the famous Brazilian Indianist, General Rondon, during the construction of which, in 1909, the Nambicuara were first pacified. This was the easiest path for Lévi-Strauss to use in order to be able to cover this immense distance during the dry season, when travel is easiest in the region. In August, when he was with the Nambicuara, no major work is usually done in the gardens, in the sense of cutting (done in May and June) or burning and planting (done in September) although garden food is harvested at that time. Though many people do go on hunting and gathering expeditions in August, many also remain at home then, living off the production of the roças. Whether there was anyone in the Nambicuara villages at that time or not, however, Lévi-Strauss would not have seen them, since the Nambicuara had long since learned not to build their villages along the line. It was a fine place to visit and it provided an easy path for them to follow through the forest for hunting, but it was no place to live, since diseases and onerous tasks awaited those who tarried there. Thus, Lévi-Strauss did not see any Nambicuara villages nor any of their roças, although he did see a lot of Nambicuara territory. One of his companions on this trip, Professor Luiz de Castro Faria, of the Museu Nacional in Rio de Janeiro, has informed me (personal communication) that the only group of Mamaindê which they encountered, for example, was one which came to visit them as they were encamped at the telegraph station in Vilhena, so that they never actually visited a Mamaindê village.

An investigation of the old telegraph line's archives (now in the archives of the Fundação Nacional do Índio in Cuiabá, Mato Grosso), undertaken by David Price and myself, suggests that the employees of the telegraph line were themselves strong proponents of the idea that

\textsuperscript{4} The difference between the southeastern and southwestern dialect areas is not as great as that which separates these two from the others. Sabanê is most divergent of all (see Price, 1972: 62-73).
the Nambicuara speakers were nomadic. Since these employees rarely strayed from the line into Indian territory with all its real and imaginary dangers, they only saw the Indians when they were on the move, when they came to visit the employees at the telegraph stations. Price (1972: 134) is therefore probably correct in suggesting that Lévi-Strauss may himself have been influenced somewhat in the formulation of some of these ideas by the telegraph employees that he encountered.

While Oberg and Lévi-Strauss were only able to visit the Nambicuara-speakers for short periods, during the dry season, in locations other than their home village and roça areas, I was fortunate to be able to spend a considerable amount of time during both seasons actually resident in one or more of their villages. In the course of that residence, it became quite obvious to me that their accounts did not accurately portray the reality of Nambicuara residence and food-production activities today. Upon further ethnohistorical investigation, as discussed below, I found that those accounts didn’t accurately portray these activities in the 1930’s or 1940’s either.

This idea had first been suggested to me in 1968 by Cecil Cook, Jr., then of Harvard University, who was investigating the ethnomedicine of the southeastern Nambicuara dialect group. Cecil had invited me to visit the southeastern, and some of the other, groups with him in order that he, I, and David Price (then of the University of Chicago) could develop a joint research project on different aspects of Nambicuara culture as seen from the different Nambicuara dialect groups. Price was at that time studying the parameters of kinship among the southwestern dialect group, although he later came to also study this for the southeastern group as well. I then decided to investigate economic phenomena from the perspective of the northwestern dialect group, although we all agreed that economic phenomena would probably be quite similar among all the dialect groups. (Kinship and ethnomedicine vary slightly more, the latter primarily for the Sabanê dialect group, which also speaks a most dissimilar dialect.) Cook found the differences between what Lévi-Strauss had reported regarding economic behavior for the Nambicuara and what he had observed (especially among the southeastern dialect group, but also among the others which he had visited) to be worthy of a more detailed investigation. David Price also suggested to me that he thought that Lévi-Strauss’ account was misleading in this regard, and suggested that if I were interested in carefully researching the phenomenon that I should do so initially without a detailed rereading of Lévi-Strauss at that point (having read the material prior to leaving for Brazil) so as not to bias my own perceptions until I had ascertained in general the important parameters of Nambicuara economic life and the relative dimensions of these parameters. Then, prior to proceeding to the immense amount of work necessary to quantify parameters, so as to be able to actually make some concrete statements about them, to reread Lévi-Strauss’ work to see if indeed there were differences to be accounted for. This proved to be good advice.

I set about, in the three years of my field research, to first ascertain the
In comparing the information which I obtained on subsistence activities and residential mobility for the Mamaindê with that reported by Oberg and Lévi-Strauss (shown again here, in parentheses) as characteristic of the wet season for "the Nambicuara", I have found that:

1. (They build new, temporary villages, in which they remain only for the duration of the rains.) Actually, the building of houses near the roças is staggered throughout the year. This set of houses is permanently enough occupied to be termed a village, not merely a "temporary village". These villages are and were (according to ethnohistorical general parameters of Mamaindê life, broadly conceived to include kinship, marriage and residence, technology, production and economics, religion and mythology, as much as possible of their language, and so forth. After having done so, in order to put into proper perspective the specific things I wished to focus on, I began to quantify the patterns and processes of behavior that I had determined to operate. At this point, I reread Lévi-Strauss' account and found that, indeed, there were differences to document and analyze. For this, I kept detailed records of all possible village residents in terms of where they went, what they were to do, who went along; how long it took, what else they did, what the results were, what people had to say about it, and how my presence may have affected these things. I timed carefully-chosen samples of each of these types of activities, under different circumstances for different types of individuals, to develop a quantified model of the Nambicuara day. Records were kept which allowed this to be developed on an annual basis in order to show any variation by time of year (whether conceived of as season or micro-season) or in response to any other factors, whatsoever. Quantitative records were kept of the amount of time necessary to make all domestic items and carry out all of the activities in which the Nambicuara engaged (including burying their dead, participating in puberty festivals, and making manioc soup, for example).

Quantitative records were also kept of the exact amount of all of the food produced and consumed by each producer or consumer unit (usually, but not always, the nuclear family), as well as the exact amount of all other products such as houses, baskets, necklaces, gourds, bows and arrows, flutes, etc.) produced by Mamaindê, including all services performed (such as babysitting, leadership, shamanism, fetching firewood, etc.) and all of the time spent in and income derived from any work for the Brazilian colonists moving into the area. All of this production data was collected thoroughly, so as to illustrate any seasonal or other variations which might occur for any season of the year or in response to any other factors.

The details of the methodology used in the gathering of this information are certainly important for an understanding of its accuracy and I have, thus, documented them with great care in the larger work (Aspelin, 1975) which reports in much greater detail all of the research findings summarized, out of necessity, so briefly here. I am certainly indebted to Cecil Cook and to David Price for suggesting these subjects for my inquiry and for aiding me in my entry into the field of Nambicuara studies. As mentioned above, however, all responsibility for these particular results still resides with me.
accounts discussed below) occupied for periods of from three to over ten years. The actual houses are usually replaced about every 3.5 years since the posts and thatch are usually rotten by then. This disagrees with the data presented by Lévi-Strauss.

2. (They burn their roças.) They do burn their roças just before the impending rains, usually in mid-September. This agrees with the data presented by Lévi-Strauss.

3. (They plant their roças.) They do then plant their roças following the burning, although they may actually plant some crops, such as corn, before the rains come. This then partially agrees with Oberg's and Lévi-Strauss' data.

4. (They have ample food from the roça.) Although they do have ample food during the rainy season as a whole, their food supply is actually at its lowest during the initial part of the rainy season (and the last part of the dry season) before the crops just planted have begun to produce and while last year's plantings are beginning to taper off in production. It is also at this time that the fruits of the forest have not yet matured so there is less fruit to eat and less game available, feeding on the fruit. This happens to be exactly the month of August, when Lévi-Strauss was there. Thus, this agrees with Oberg's and Lévi-Strauss' accounts only insofar as those accounts are specifically restricted to the particular time period of the latter part of the dry season and the initial part of the wet season and are not extended to either season as a whole. This period of relative shortage is soon alleviated by the production of forest fruit and game products and, later, by the maturing of the corn crop in January. It is further reduced throughout by the fact that the manioc planted in the preceding year is just at this time beginning to come into its prime.

5. (They completely harvest their roças, storing whatever manioc cakes are left in holes in the ground.) The Nambicuara do not harvest the entire crop of their gardens at any one time, since they can't eat it all at once and since storage facilities are poor, so that the products won't keep for very long. Rather, they harvest each crop over as long a period as possible. This harvest actually continues throughout the following dry season, when some of the major crops (peanuts, lima beans, yams, etc.) are best for harvesting. As shown in figure 2, the harvest of the manioc planted in September only begins in May (although the roots are still very small then). The harvest of that crop thus continues throughout the dry season and the next rainy season until the following March, a year and a half after the crop was planted. Also, it should
be noted that manioc cakes are not buried in the soil but are rather left to dry over a fire so that they will keep. They are then stored in the rafters of the house as a hard, dry cake which few animals will bother. This storage is done, however, only to reduce the necessity to return to the roça every day, not in order to store up large amounts of processed food from one season to another. There is no need to store processed manioc cakes since the roots themselves can remain growing in the ground until needed, as natural storage of the raw product. This information, then, strongly disagrees with that presented by Lévi-Strauss.

![Diagram showing growing and bearing seasons of manioc](image-url)

**Figure 2.** Growing and bearing seasons of manioc  
(Cross-hatching indicates time of reduced manioc supplies)

6. (They rely largely on the labor of the males in producing food.) As is shown in table 1, the exact opposite of this actually occurs. Rather than males contributing more effort than females to all food-production activities in the wet season, and females more than males in the dry season (as also expected from point 11), males actually contribute more effort in the dry season and females more in the wet season. An analysis of the allocation of time specifically to roça-food production activities, out of the larger set of all food production activities, provides an explanation of why this occurs. Table 2 (presented and discussed further below), from which the summary figures in table 1 were derived, shows that males and females both contribute labor in both seasons to the production of roça foods but that females contribute more than males in each season, rather than in either one of them alone. For the production of roça foods the roça must be cut, the dry bush and felled logs burned, the area planted (and unburnt branches piled out of the way), growing crops cultivated (although little of this is done) and
harvested (when most of the small amount of cultivating actually takes place), and the resultant raw foodstuffs processed into edible food products. Although males are responsible for cutting, burning, and helping to plant the roças, half of the labour of planting, almost all of the harvesting, and all of the processing of the roça products are done by the women. The women’s harvesting and processing activities require by far the largest part (80%) of all of the roça food production activities listed above for either males or females. Since roça crops are available and consumed in both seasons (as discussed in point 5, above), the large amount of female roça-food production labor required to harvest and process them is approximately equally required in each season (although it clearly varies somewhat depending on the relative proportions of the different crops available each season, the amount of time each different crop requires for its processing, and the number of females available at different times to do the job). Because there are fewer women resident in the village in the wet season than in the dry season (as shown later in table 3) to share in the work of harvesting and processing roça foods, the amount of work necessary per resident woman is thus greater in the wet season than in the dry season, as shown in table 2. For these reasons, then, females contribute more labor per person to food-production tasks in the wet season than do males. This clearly differs from the picture presented by Lévi-Strauss.

6 Lévi-Strauss appears not to recognize or give to the processing of raw foodstuffs its necessary and proper importance. Perhaps, since he didn’t visit an operational Nambicuara roça, he also didn’t have the opportunity to actually observe the processing of very many roça food products. Of these, manioc requires the largest amount of time and labor. Most other foodstuffs, from whatever source, require comparatively little processing, or manipulation, of the raw substance to render it edible. It would seem, however, that he might have been aware of the labor requirements for manioc from what he observed among the other tribes that he visited in Brazil.

When one speaks of food-production activities, one must necessarily include all of the tasks necessary both to obtain and to prepare the food until it reaches its final (in the economists’ sense) or edible form, ready for the consumer. I doubt that the Nambicuara women would appreciate hearing that someone considered that part of their labor which is devoted to processing roça foodstuffs to be unnecessary, unimportant, or easy. Processing is especially important for manioc products, of course, since eating unprocessed “bitter” manioc (the Nambicuara have very little “sweet” manioc) leads usually to death from prussic-acid poisoning, as the Nambicuara well know. It seems, then, that the processing of raw foodstuffs must be included for both logical as well as practical reasons with the obtaining of those raw foodstuffs in order to totally describe and analyze food-production activities; reality is otherwise either simply overlooked or else seriously distorted.
Table 1. Total daily food production labor

<table>
<thead>
<tr>
<th>Sex of Worker</th>
<th>Average daily hours/person</th>
<th>Wet Season</th>
<th>Dry Season</th>
</tr>
</thead>
<tbody>
<tr>
<td>male</td>
<td></td>
<td>2.01</td>
<td>2.49</td>
</tr>
<tr>
<td>female</td>
<td></td>
<td>2.59</td>
<td>1.56</td>
</tr>
</tbody>
</table>

7. (Little, if any, hunting and gathering is done, this being mostly done in the dry season.) Actually, during the rainy season, a great deal of hunting and gathering activities take place, both in the form of short excursions from the village, lasting for only a day or a part of a day, and in the form of longer-term expeditions which may last several weeks. This is possible since there is really no extended period in which labor must be concentrated in garden activities, with the exception of the cutting and planting seasons, which occur only at the very end and beginning of the rainy season, respectively, as shown in figure 3. Of

Figure 3. Mamaindê seasons
these two major roça activities, only the latter, planting, actually requires or usually involves the labor of most or all of the adults, and this only lasts for approximately one week, anyway. As a matter of fact, the time devoted to hunting and gathering in the wet season, when these two activities are considered together, is almost exactly equal to that which occurs in the dry season. If the proper categories of information presented in table 2 are thus summed, it will be seen that hunting (for the men) and gathering (for the women) together require in the wet season 2.75 hours/man-woman versus 2.77 hours/man-woman in the dry season. Slightly more gathering is done by each woman in the wet season than in the dry season (0.95 versus 0.74 hours/day) to take advantage of the increased abundance and variety of wild plant products available at that time. Conversely, slightly less hunting is done by the men in the wet season than in the dry season (1.8 versus 2.03 hours/day) because game is very much more available in the wet season so that less effort is necessary to procure considerably more meat. It is not fair to say, however, that either one of the seasons per se is more devoted to both hunting and gathering as opposed to agricultural production (discussed below). This information, therefore, disagrees substantially with that of Lévi-Strauss.

Table 2. Daily food-production labor

<table>
<thead>
<tr>
<th>Activity</th>
<th>Average daily hours/male</th>
<th>Average daily hours/female</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Wet Season</td>
<td>Dry Season</td>
</tr>
<tr>
<td>roça-food production</td>
<td>0.21</td>
<td>0.46</td>
</tr>
<tr>
<td>hunting</td>
<td>1.8</td>
<td>2.03</td>
</tr>
<tr>
<td>gathering</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Totals</td>
<td>2.01</td>
<td>2.49</td>
</tr>
</tbody>
</table>

Regarding the activities which Lévi-Strauss and Oberg present as dry season activities for the Nambicuara, I have found that:

8. (They leave that village for good.) Many people remain resident in the village near the roças for considerable periods of time during the dry season, while others leave on hunting and gathering expeditions and then return to spend several weeks in the village to harvest crops.
from the gardens and to eat the meat which has been hunted (and
smoked to keep for a few days) and brought back to the village. As a
matter of fact, people spend more of their days residing in the village
during the dry season (65 % for the men; 76 % for the women) than
during the rainy season (46 % for the men; 39 % for the women), as
is shown in table 3. This is largely accounted for by the fact that hunting
and gathering are better in the wet season, so more people leave the
village to pursue these activities then. Again, this information directly
contradicts that of Lévi-Strauss and Oberg.

Table 3. Average daily residence (adults)

<table>
<thead>
<tr>
<th>Period</th>
<th>Male</th>
<th>Female</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. wet season average</td>
<td>46 %</td>
<td>39 %</td>
</tr>
<tr>
<td>2. dry season average</td>
<td>65 %</td>
<td>76 %</td>
</tr>
<tr>
<td>3. annual average</td>
<td>55 %</td>
<td>56 %</td>
</tr>
</tbody>
</table>

9. (They have little food.) As indicated for point 5, above, supplies
of certain kinds of food are somewhat reduced toward the end of the
dry season. This occurs in August, when Lévi-Strauss was passing
through, after the non-manioc root crops (such as yams) and lima bean
and peanut crops have all been harvested to eat or to store as seed for
September planting. However, it is also at this time that the new manioc
production begins to climb, if one is in the village to harvest it, as
shown in figure 2. The Nambicuara which Lévi-Strauss saw were not
home to harvest this because they had come to see him where they
expected him: along the telegraph line. During the dry season as a
whole, food supplies are not only available; they are, as shown in table 4,
actually slightly more abundant than during the wet season as a whole,
although their composition varies just as does the relative amounts of
different labor activities necessary for their production (as was shown
above in table 2). Thus, insofar as Lévi-Strauss' account is supposedly
describing food production activities for the dry season as a whole, this
information largely disagrees with his.

10. (They rely entirely on the products of the hunt and gathering.)
During the dry season, roça products continue to be important. As
mentioned in points 5 and 9, above, some (such as lima beans and
brown beans and peanuts) are only available during the dry season
while others (such as yams) are actually at their peak production during
Table 4. Food consumption per person per day

<table>
<thead>
<tr>
<th>Item</th>
<th>Dry Season</th>
<th>Wet Season</th>
</tr>
</thead>
<tbody>
<tr>
<td>all manioc</td>
<td>1.19 kg.</td>
<td>1.26 kg.</td>
</tr>
<tr>
<td>yams</td>
<td>0.6 kg.</td>
<td>0.1 kg.</td>
</tr>
<tr>
<td>all meat</td>
<td>0.18 kg.</td>
<td>0.4 kg.</td>
</tr>
<tr>
<td>Totals</td>
<td>1.97 kg.</td>
<td>1.76 kg.</td>
</tr>
</tbody>
</table>

the early dry season and still others (manioc) are increasing in production during that season. Table 4 clearly indicates that total manioc production/consumption per person is almost the same during both seasons (1.19 kg for the dry season versus 1.26 kg for the wet season) while that of yams is actually much greater during the dry season than during the wet season (0.6 kg versus 0.1 kg). Again, this information essentially contradicts that of Lévi-Strauss.

11. (They rely largely on the labor of the females in producing food.) As was discussed above, regarding point 6, table 1 clearly shows that the food production labor of the males in the dry season is greater, per person, than that of the females. The reasons for this are delimited in table 2, which shows that the females do less gathering and less roça-related work in the dry season than in the wet season while the men do both more hunting (since game is harder to obtain at this time) and more roça work (especially the cutting and burning) in the dry season. Thus, the relative proportions of male versus female labor do shift by season, but in the opposite manner from that described by Lévi-Strauss.

12. (Little, if anything, is done regarding roça activities, these being mostly done in the wet season.) Many significant roça activities actually occur during the dry season, including the cutting of the roças, the important ceremonies which accompany that cutting, and the continuing harvesting and processing for use of the crops from the previous year’s roças. The information shown in table 2 indicates that, although the combined average male and female labor per day in roça-food production (which includes processing the harvested crops as well as cutting, burning, planting, and harvesting the roça) in the wet season is 1.85 hours, that for dry season roça activities still requires 1.28 hours, or two-thirds of the figure for the wet season. Once, again, this information substantially disagrees with that presented by Lévi-Strauss.
Conclusions

In this paper, I have attempted to show that there is a considerable degree of divergence between what Lévi-Strauss (and Oberg) and I have reported regarding the seasonality of food production, food-productive activities, residence, and the division of labor by sex of the Nambicuara-speaking peoples. Whereas Lévi-Strauss and Oberg either state or strongly imply that: a) the Nambicuara are sedentary during the rainy period and nomadic during the dry season, I have found that they are not seasonally nomadic; they are not entirely nomadic in one season and entirely sedentary in the other. Although in terms of annual percentages, only slightly over 50% of the people are resident in the village on any average day, as shown in table 3 (so that it is fair to say that individuals do not occupy their village on a full-time basis), the situation for the group as a whole is both more stable and more complex than Lévi-Strauss and Oberg report. b) The Nambicuara rely primarily on male food-production activities during the rainy season and primarily on female food production activities in the dry season, I have found that the association between season and primary labor contribution by sex is exactly the opposite, as shown in table 1. In addition to the seasonality of such labor being questioned, I have shown that both female roça activities and male hunting activities are considerably more important than Lévi-Strauss and Oberg give them credit for being. Furthermore, no activity by either sex is entirely restricted to, nor absolutely more important in, either season. c) The Nambicuara rely primarily on roça-foods during the rainy period and on food obtained by hunting and gathering during the dry season, I have found that, while manioc is slightly more abundant in the wet season (as shown in table 4), the difference is really very slight and is more than compensated for by the greater availability of yams in the dry season, so that the dry season as a whole is not a time of roça shortage nor is the wet season a time of roça super-abundance. Micro-seasonal variations in food supplies are more important than any simple differences between the two seasons as wholes.

These differences between my report and Lévi-Strauss' might be accounted for in several ways. First, we might each be dealing with a different group, either a different culture or sub-culture, and not actually talking about the same people and their (therefore) same culture at all. Second, we might each be dealing with the same people but at significantly different periods of time, so that their culture might have changed from what Lévi-Strauss reported it to be to what I have found
it to be. Third, the differences between the two accounts may be due
to the fact that no two observers see the same event in the same way,
so that all “objective” accounts are inherently “subjective”. Fourth, one
or the other of the two different accounts might be factually in error;
one might be wrong. And, fifth, one or the other of the two accounts
might involve facts which have been altered through theoretical analysis
and rearranged into an explanatory model which relates to but does
not replicate the empirical facts from which it was built; that is to say,
one may involve ethnology as well as ethnography. Each of these possible
reasons needs brief consideration here.

1) On the basis of my observations when visiting villages and roças
of all of the other dialect groups of the Nambicuara-speakers, it seems
quite clear that there is little variation among them in terms of food
production activities except regarding the members of the southwestern
dialect group, which includes the Galera and Sararé (as shown in
figure 1), for example. Although all of the Nambicuara-speakers grow
both manioc and corn, the southwestern group depends primarily on
corn while the rest depend primarily on manioc. Thus, many general-
izations based upon the Mamaindê are probably (with the proper
caveats) applicable to the rest of the Nambicuara-speakers, with the
exception of these southwestern people. Since the accounts of Oberg
and Lévi-Strauss are, as a matter of fact, little concerned with the south-
western group, we are at least all generalizing, in effect, about the same
unit: the Nambicuara-speakers exclusive of the southwestern dialect
group.

2) Certainly, some time has elapsed since Lévi-Strauss made his
observations among the Nambicuara and the time when I made mine:
30 years, to be exact. During that time, some changes have occurred
in Nambicuara culture. Some of these were internally stimulated, others
were brought about by the interaction of the Nambicuara peoples and
their culture with the varying segments of the outside world which
approached them at different times, for different reasons, in different
ways (Aspelin, 1975: 21-26; Price, 1972: ch. 1). In order to investigate
the nature and magnitude of these changes, I attempted to find out

There are also some minor differences among the dialect groups in terms of
the division of labor (such as which sex makes baskets or brings water; cf. Price, 1972: 272 f.) and in the specific sub-species of manioc or yams
grown, but these differences, although worthy of further investigation in terms
of variation in cognitive structures or the history of trade among the Nam-
bicuara dialect groups, are not sufficient to make my generalizations (as
restricted in the text at this point) any less valid.
as much as possible about the history and ethnohistory of the Mamaindê. I interviewed those who had known them in the past: SPI and FUNAI personnel, rubber and ipecac gatherers, road construction personnel, missionaries, army and air force personnel, explorers, shopkeepers, anthropologists, doctors, and colonists. I conducted (together with David Price) a thorough search of the appropriate archives of Rondon’s telegraph organization, the SPI, and FUNAI in Cuiabá, Mato Grosso, for all relevant information; I collected life histories, myths, and interviews from the Mamaindê (and some of the other remaining northern dialect groups), and I visited many of the old Mamaindê villages which had been inhabited since Rondon’s time (as shown in figure 4) and asked the informants who accompanied me there appropriate, non-directive questions to eventually elicit a picture of Mamaindê life and its changes over the last half century.8

8 Of the old villages shown in figure 4 of the text, I visited and investigated those labelled A, B, C, D, E, F, H, K, L, and S. Many of the others (especially G) were visited and investigated by the Summer Institute of Linguistics missionary to the Mamaindê, Mr. Peter Kingston. These researches made it possible, for example, to establish the following residential history for the present residents of village A and their antecedents:

<table>
<thead>
<tr>
<th>Time Period</th>
<th>Village</th>
<th>Time Period</th>
<th>Village</th>
</tr>
</thead>
<tbody>
<tr>
<td>1945/50—1955:</td>
<td>Village C</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

For each of those sites, approximate dates and lengths of residence were obtained through genealogical methods and by comparing Mamaindê accounts of happenings at the time with accounts obtained from the other historical sources mentioned in the text. It was also determined why each village was established where it was, approximately how large it was, and why it was eventually abandoned.

The old roças used in conjunction with each old village were also visited and information obtained regarding the crops which were grown there, the origin of the planting stock used for each crop, the tools used and their origins, the length and relative quantity of yield, and the organization of the production process for each type of foodstuff grown, hunted, or gathered.

In addition to the old villages shown in figure 4, there were others located along both sides of the Cabixí River, but little is known of them since most of their people died of epidemics in the early 1920’s, following the establishment, in 1921, of a transshipment station for telegraph-line supplies near village J (which dates to that time). This is the place known in Rondon’s reports as Porto Amarante (Botelho de Magalhães, 1930: 441).

A complete report on these and other old Mamaindê village and roça sites (Aspelin and Kingston, 1971) was prepared by Peter Kingston and myself at the request of the Fundação Nacional do Indio (FUNAI) for their use in considering how and where to establish a reservation for the Mamaindê. In June of 1973, the Mamaindê were moved by FUNAI to new villages within the existent Nambicuara reservation, east of BR-364 (Aspelin, 1975: 270).
Out of this research, only four changes with much significance for the questions under consideration in this paper emerged: first, some new cultigens had been introduced into their agricultural repertoire; second, the efficiency of many food-production activities had been

Their position in their traditional homeland had become politically and economically more and more untenable as the Gleba Padronal encroached further and further on their roça and hunting lands, as mentioned in the text. Figure 4 thus represents the state of affairs in the Mamaindê world in 1971, when the Aspelin and Kingston report was prepared.
changed to some degree by the introduction and increasing availability and use of steel tools; third, population had declined drastically with the introduction of non-indigenous diseases; and, fourth, roça food production activities had been restricted at the time I studied them from what they had been in the past.

The new cultigens had very little effect on the overall picture of Mamaindê food production activities since they were never very successful nor important in the Mamaindê diet. Thus, their production did not alter the amount or organization of labor required or the output of roça foods in any particular time period. Most of these new cultigens were introduced or became most easily available to the Mamaindê, and to the rest of the Nambicuara-speakers as well, following the opening of the Cuiabá-to-Pêrto Velho highway (BR-364) in the early 1960's.\(^8\)

Land speculators began selling Indian lands to improverished farmers from the overcrowded Brazilian south and northeast who began moving into the area with ever-increasing frequency. In 1965, for example, a Brazilian land development company opened a new colony, the "Gleba Padronal", in the middle of traditional Mamaindê territory. Primarily from the new colonists in "the Gleba", and to a lesser extent from Brazilians passing up and down the road, the Mamaindê (and, similarly, the other Nambicuara) have obtained some new varieties of beans, corn, bananas, pineapple, and “sweet” manioc, together with some new crops previously unknown to them, such as sugar cane and rice. They have had little success with either of the two new crops which might seem most likely to alter their subsistence base: rice and beans. These have not yielded at all well and have often ceased to be planted soon after their introduction. Similar bad luck has afflicted the Nambicuara’s efforts to raise domesticated animals, such as chickens and pigs (and also goats and guinea pigs) which they have obtained at various times, since the days of Rondon, from missionaries and government agents with whom

---

\(^8\) Rondon had attempted to introduce new farming methods, in addition to new crops, to several of the Nambicuara dialect groups. These attempts left no lasting mark, however, and are only vaguely mentioned in historical sources (Rondon, 1922, passim). In 1942, the SPI established an Indian Post at Espirro, some twenty kilometers to the north-northeast of the Mamaindê. This post was responsible for the introduction of many new crops and tools and other changes into the lives of the Nambicuara in its vicinity, but, since it was in the area of another dialect group which was on unfriendly terms with the Mamaindê, it had little effect on them (Aspelin, 1971: 24). The agent in charge of this post from 1942 until 1968 never even went to visit a Mamaindê village in all of that time; even as close as they were geographically, so slight was the contact between them (Affonso da França, personal communication).
they have been in contact. Any surviving descendants of these experiments are of negligible importance in most Nambicuara diets. The other cultigens introduced recently are actually only new types of cultigens (such as corn or pineapple) already familiar to the Mamaindê, and have thus to some degree only supplanted, rather than augmented, the overall production of that crop.

The introduction of steel tools altered the efficiency of many Mamaindê food-production activities, to different degrees. Steel axes made it much easier to fell trees in order to clear roças, make bows and arrows, build houses, and gather certain types of fruits for which the tree must be felled, for example. Informants who had themselves in the past worked entirely with stone axes indicated that it was necessary to begin cutting roça earlier than at present and that the cutting period also extended somewhat later than at present, with the net result being that the amount of labor required approximately doubled but that the increase was apportioned approximately equally to each season.

Additionally, women used to help their husbands or sons in the cutting process in the past, because it was so onerous, by helping cut the smaller brush with large hardwood “swords” or by pulling small plants out by the roots. The stone axes used by the Nambicuara at that time were used mostly for cutting large trees for they tended to ricochet off of the more-supple smaller growth. The net effect of this on the arguments made above concerning the amount and seasonality of male labor inputs in the roça-food production process as compared to those for females would thus not be very great. In any one year, the roça-food production labor of the males would increase in both seasons (since the cutting extended both earlier and later) and that of the females would increase somewhat in the (late) wet season (when the clearing of the underbrush is done prior to cutting the larger trees). In the wet season, therefore, the labor requirements per person would be greater for the females than for the males, as is the case today. In the dry season, however, the males might contribute slightly more labor per person than the females, or (since this estimate of twice as much cutting labor required without steel axes, machetes, and sickles is only approximate) at least closely approximate their contribution. A further factor must be considered, however. Since it took so much more work to clear a roça in the first place, without steel tools, more labor was invested in the past in keeping existing roças free of the encroachment of weeds which, rather than declining soil fertility, seems to be the most important limiting factor for the production of manioc (the most important Mamaindê crop)
in this area, according to both the Mamaindê and the local colonists interviewed on this question. The Mamaindê today, as a matter of fact, regularly harvest manioc from roças that are three or four years old, replanting the stems of the harvested plants as they go and thus ensuring future yields from the same cleared space as long as weed competition is kept under control. In other words, since it took more very hard labor to make roças in the past, fewer were cut in any one year, or, to put it another way, new roças were cut less frequently. Thus, the short-run changes in the relationship of male to female roça food production labor due to the lack of steel tools which were discussed above were not actually realized in the long run. Female roça-food production labor, per person, would be approximately the same then as now, since most of it is involved with the harvesting and processing of the same types of roça products in approximately the same quantities, per person. Male roça-food production labor would have been somewhat, but not substantially, greater than at present, in order to produce the same amount of food per capita as is produced today. Thus, the situation has not changed significantly, for our purposes here, since the time of Lévi-Strauss, when some steel tools were actually already to be found among the Nambicuara.¹⁰

Just as Rondon's telegraph employees and SPI personnel were

¹⁰ Steel tools had been introduced first by Rondon's pacification teams, during the construction of the telegraph line, twenty-five to thirty years before Lévi-Strauss' visit. Subsequently, employees of the telegraph line and the SPI continued to provide the Nambicuara with large amounts of steel tools. Missionaries at Utiarity, and, after 1942, the SPI personnel at Espirro, also provided a large number of steel tools which were widely traded among the various Nambicuara dialect groups, even to those which had not yet actually come into contact with the Brazilians. (Stone axes had previously been traded extensively from group to group, in much the same manner.)

There were other consequences for the production of food, or of other products, of the introduction of steel tools, of course, but it would require more space than available here to discuss them all as fully as they might warrant. The most important is certainly that concerning the cutting of roças, discussed in the text at this point. Other important effects which should be mentioned in this context are those on the production of bows and arrows, the making of any domestic artifact (including houses), the planting, harvesting, and processing of foods, and hunting and gathering.

It became much easier to cut down the hardwood trees used for making bows and arrow points with the introduction of the steel axe, and to shape and work the wood with the introduction of steel knives. It essentially became much easier to make almost all domestic items, such as houses, necklaces, gourd cups, etc., with steel tools. Relatively few of these items are produced in any given year, however, and the additional amount of time required to do so without steel tools would not have infringed greatly on the Mamaindê day
primarily responsible for the introduction of steel tools into Nambicuara culture so also it must be remembered at this point that the primary disease-related depopulation of many of the Nambicuara groups had also occurred before Lévi-Strauss' visit, due to diseases introduced in the construction and maintenance of the telegraph line twenty-five to thirty years earlier. (See, for example, Lévi-Strauss, 1974: 294; Price and Cook, *passim*). The primary effects of this depopulation, in terms of subsistence activities, would have been to reduce the overall or total volume of production of Nambicuara products, including food, from all sources and in all seasons (since food production is an individual or nuclear-family activity and does not depend on larger groups, the

(production requirements for these items are discussed more fully in Aspelin, 1975: chapter 4) or greatly affected the seasonal allocation of labor in these tasks. One effect of using less efficient tools in these production activities would probably be to reduce the number of them produced, as was the case with roças, as that was discussed in the text. This would then act to make villages even more stable or temporally permanent, since individuals would be even more inclined than they are now to repair existent houses rather than build new ones elsewhere.

Steel tools are used now in the planting, cultivating, harvesting, and processing of roça foods, as well as in the cutting of the roça. Hoes (and traditional digging sticks of wood) are used for planting, but they are not much more efficient than the stone axes and digging sticks used in the past. to make the holes in which manioc stems or corn seeds, or whatever, are inserted. The same can be said for cultivating and harvesting (where now machetes and digging sticks are used instead of the previous stone axes, digging sticks, and the pulling of plants by hand). Steel knives are now used to peel and cut the foodstuffs in processing them to be cooked, but the bamboo knives used for this in the past were remarkably sharp and efficient in these tasks.

In hunting and gathering, also, the overall effects of the introduction of steel tools have not been great. Axes and machetes are used in gathering to fell trees or cut off branches. These are somewhat more efficient but they also result in plants being cut down which would previously have been left alive to produce again in a following season (for, previously, only fallen fruits would have been gathered in some of these cases). Thus, while it is easier to get the fruit in one year with steel tools, one must go farther and look longer in each subsequent year, so that the efficiency of steel tools soon returns to the efficiency level of the pre-steel-tool technology, or perhaps even falls below it. The same may be said for hunting with steel shotguns, of which the Mamaindê had a few in 1968-71, although before that they had been quite scarce. With guns, a man can kill at greater distance with less chance of missing an animal. The animals become more wary, however, and, especially animals which travel in groups, such as savannah deer and the peccaries of the forest, will break and run when the first shot is fired. With a bow and arrow, however, several animals may be shot before all of them become frightened. Thus, here too, the increase in efficiency of steel tools is only short-lived and would have little effect in the long run on the allocation of labor time among these people.
disruption would not have been exponentially great). Hunting and gathering activities might have become slightly more productive, given less competition for the available resources per hunter or per gatherer, but this effect would be negligible. Furthermore, these effects would essentially already have had their primary impact, and would be of even less importance from 1938 to 1968 than they were from 1908 to 1938.

At the time when I studied the Mamaindê, the Gleba Padronal had been operating in their area for three or four years. In this time, the Gleba had appropriated and sold to colonists almost all of the land around village A (shown in figure 4) from which most of the production data quoted here were obtained. The Gleba had left the Mamaindê little land in this area, and what they left was certainly not the best of the lot. Thus, the people of village A were not able to cut as many nor as large rogas as they would have liked to, nor did those rogas which were actually cut produce as well as they would have were they located in more productive areas.

In terms of the division of labor, little would be changed by this for, although in the past the men could and did cut more roça, the women also had more to harvest and process. Thus, the primary effect of this change would be to make the roça an even more dependable and important food source in the past than it was when my study was made, in 1968-71. Since it was still shown to be an important part of Mamaindê subsistence at that time, it could not have been of only little importance in the past, as Lévi-Strauss' account indicates.

This consideration, then, of such changes as have affected Mamaindê subsistence activities in the last fifty years, including the introduction of new cultigens and new tools, depopulation due to disease, and a restriction in the amount of good roça land available to them, clearly shows that the situation did not change sufficiently from Lévi-Strauss' time to the time of my research to account for the type and degree of differences noted between our two accounts.

3) Although it is true that two observers may have different perceptions of the same event at the time of its occurrence and different recollections of it thereafter, based on either cultural, subcultural, or idiocultural variations in the way each of them has been taught to view and interpret the world around them, this would not seem to be a satisfactory reason for the differences recorded here. Both Lévi-Strauss and myself come from what is essentially the same macro-cultural tradition of "Western culture". Furthermore, each of us was trained,
as an anthropologist, to take into account our own cultural baggage when considering that of other people. The differences in our perceptual orientations to the world should therefore be slight (although nonetheless existent) and should not account for differences of the type and magnitude discussed here. As a matter of fact, we do substantially agree in many of our other observations on the Nambicuara (such as on the nature of the relationship between male cross-cousins or on the raising of children and pets, for example). The coincidence of vantage point for those data strengthens my contention that we also share a common vantage point for the phenomena here at issue.

4) If two people may have different perceptions of the same event or situation (of the same “reality”), then they may also hold two different sets of “facts” to be “true” regarding that event or situation. Thus, reality, truth, and facts all become relative; they pragmatically exist only in the dialectical interaction which takes place between them as absolutes and the observer who perceives them. It is this process of perception and the perceptions thus formed which are the operational reality, facts, and truth for humanity. We may say that we have reached a stage of “empirical” or descriptive accuracy when we can generate a statement representative of a set of very similar perceptions shared by all (or almost all) of those people involved in any particular event or situation, regarding that event or situation; we may say that we have reached at least a partial understanding of this empirical world when we can generate a statement about it which also accounts for those few who don’t share those similar perceptions with the “almost all” who do. A consensus among the people involved in any particular situation, a widely-shared state of subjectivism or relative reality is, by virtue of the amount of refinement and successive approximations to “actual reality” entailed in rendering it acceptable to an audience of this scope, appropriately labeled an “empirical fact”. Factual accuracy, for our purposes here, then, involves the degree to which any statement represents a widely-shared set of perceptions of “reality”. Factual error involves the degree to which statements do not represent a widely-shared set of perceptions of “reality”; these may or may not actually be held by any members of the culture, event, or situation in question.

Most of the data which I have presented here regarding Nambicuara subsistence activities has been presented as “empirical fact”. All of the Mamaindê whom I observed fishing said they were fishing. All of the Mamaindê who seemed to me to be hunting, planting crops, leaving to go gathering, or eating manioc cakes said, when asked what they were
doing, that they were hunting, planting crops, leaving to go gathering,
or eating manioc cakes, respectively. On that basis, I believe the in-
formation presented here to be “factually accurate”. Since, then, both
Lévi-Strauss and I were dealing with essentially the same group, which
hasn’t significantly changed in these aspects of its culture between
the time of his visit and mine, since we are both equally capable of
observing and reporting on such phenomena from a common vantage
point, and since our two reports differ considerably, I can only conclude
that his account is factually in error.

5) The discussion above is based on my assumption that Lévi-Strauss
intended this information to be taken as “fact”. Sometimes, that may be
a dangerous assumption, for (since theories are derived from facts and
models are applied to facts to evaluate their explanatory power) facts,
models, and theories may all appear in the same discussion and (since
they are usually to some degree permutations of one another) be easily
confused with one another if they are not clearly labeled as to which
they are. In a theoretical article entitled, “Do dual organizations exist?”,
Lévi-Strauss (1963) neglected to such an extent to label his facts,
theories, and models that he succeeded in convincing so perceptive a
critic as David Maybury-Lewis that he (Lévi-Strauss) couldn’t keep
these levels straight in his own mind, himself (Maybury-Lewis, 1960).
Lévi-Strauss (1960) replied that he had clearly in mind which of his
statements involved empirical data and which involved an analytical
reorganization of that data into new, more insightful, and more power-
fully-explanatory arrangements than those in which they originally
appeared. He was concerned, though, with more than the mere “actual
social segments” of empirical existence; he was concerned with arranging
the facts of that existence so as to demonstrate

that behind the bewildering diversity of empirical data, there may exist a small
number of recurrent identical properties, appearing over and over again, although
combined differently (Lévi-Strauss, 1960: 52).

Maybury-Lewis (1967: 293) subsequently also came to see the usefulness
of considering social structure as merely a construct in the mind of the
anthropologist (as Lévi-Strauss had done in “Do dual organizations
exist?”), as a model of which the various observed patterns of social
relationships are only different expressions. His original contention,
however, that one must of necessity clearly label the various levels of
one’s presentation is still very much in order here. How else would
a reader know that Lévi-Strauss (1963) had taken the liberty of
reorganizing Radin’s data regarding the spatial organization of Winne-
bago villages and Albisetti and Colbacchini’s (and his own) data on the axes of symmetry of Bororo villages? Unless this is known to be the liberty of analysis, it must otherwise appear as either fact or fabrication.

In a comparative article entitled, “Do dual organizations exist?”, a reader might well expect analysis as well as description. In Tristes Tropiques (Lévi-Strauss, 1964, 1974) and “Family and social life of the Nambicuara Indians” (Lévi-Strauss, 1958), however, this would be much less expected, if at all, and, whether expected or not, it should have been clearly labeled as such were it to appear. If analysis is involved, however unclearly labeled, it serves here more to mislead than to deepen insight. Thus, whether Lévi-Strauss’ data are wrong or his theories are mixed together with his data, the overall picture he presents actually loses rather than gains in explanatory power, for one or both of these reasons.

Although somehow neat and satisfying of our frequent desire as social scientists to reduce to some simple organizational scheme the complexities of human life, much of Lévi-Strauss’ account of the Nambicuara (since it is apparently not intended to be a model of, but rather a description of, Nambicuara culture) can only be said to be factually erroneous. For this reason, the pattern of duality presented by Lévi-Strauss (as quoted above and also in Tristes Tropiques, 1974: 276, 280, 288, 290, 292) as characteristic of Nambicuara culture is unfortunately not characteristic of them at all:

1. wet season
2. male
3. ideal (desirable or positive)
4. sedentary life
5. roça subsistence
6. solid houses
7. plenty
8. certainty
9. durability (stability)
10. reincarnation
11. life
12. friendliness
13. knowledge
14. Meso American tradition
15. etc.

dry season
female
inferior (undesirable or negative)
omadic life
gatheringsubsistence
frail shelters
scarcity
uncertainty
transitoriness (instability)
terminal existence
death
hostility
ignorance
local adaptation tradition
etc.

If, however, the information I have presented here will no longer support this neat dichotomy, if a principle of duality of patterning is not to be found in these aspects of Nambicuara culture, if all of Nambicuara culture (and, thus, by extension, perhaps, all cultures) cannot
be neatly described and analyzed in terms of this principle, which might ultimately be extendable to or reducible to only two major dyads:

16. change (history)       unchanging (ahistoric)
17. culture              nature

are we to say that Nambicuara culture (or perhaps all cultures) remain things of "shreds and patches", unanalyzable congeries of unrelated bits and pieces? Obviously not. This exercise has shown that Nambicuara culture is clearly amenable to careful description and interpretive analysis, in which various behaviors and beliefs can be shown to be clearly related to others, though not in terms of Lévi-Strauss' ideas of the duality of cultural patterns which appear to be too simple for the complexity of existence even among the Nambicuara. This argument does not necessarily mean, however, that such a principle may not operate elsewhere, nor, moreover, that it may not actually be found to operate in Nambicuara culture, were the Nambicuara data to be reconsidered in still another form. At present, however, it is fair to say that the data presented here on the Nambicuara do not accord with Lévi-Strauss' idea of the duality of the patterning of their culture. Although Lévi-Strauss' own data do agree with his conception of this dual pattern, the conclusion of this paper is that much of that data appears to be wrong.

Two primary conclusions thus emerge from this effort. First, it suggests that more concern needs to be given to restudies of the remaining subjects of earlier anthropological researches, with an eye toward confirming the actual data base reported from those cultures while this is still possible (cf. Garbett, 1967). It indicates that anthropologists cannot be too careful regarding the accuracy and clarity of their presentation of the data for which they are often the sole source. Although Lévi-Strauss was quite right, in 1946, in pointing out that anthropologists needed to get on with the business of analysis "which better deserve[s] the scholar's care and attention (1946: 140)" than does continuing to argue over small details of fact, we obviously still must have accurate, complete, and clearly presented factual material to analyze. Good data are a necessary prerequisite for legitimate analysis and theorizing, since, without accurate data, theories remain of necessity specious. Morris Opler once remarked to me that far more journal space was now being devoted to publishing theoretical articles than the available data base might actually warrant. Perhaps it is now time to devote somewhat more effort to obtaining, refining, confirming, and
publishing more of that data and somewhat less to building our theories on ground which is otherwise less firm than necessary.

The second major conclusion to emerge from this effort is that we need to be most careful in utilizing in our work the theories and ideas of others as in any way explanatory without actually deriving those theories from our own data by itself. Otherwise, in addition to possibly applying valid theories to cases where they don’t fit, we would also continually admit the possibility of applying invalid theories as well, theories which might not even be applicable to the original case from which they were supposedly derived. The usefulness of having these theories to suggest to us some of the questions which we might ourselves want to ask of our own data cannot, of course, be underestimated. For this, if for nothing else, Lévi-Strauss’ work will long retain its importance: as a fount of facts and ideas for others to reconsider and explore as I have done here.

Department of Anthropology
The Cleveland State University, Cleveland, Ohio 44115

REFERENCES CITED

Aspelin, Paul L.

Aspelin, Paul L. and Peter K. E. Kingston.

Aytai, Desidério.

Boglár, Lajos.

Botelho de Magalhães, A. A.

Fuerst, René.

Garbett, G. K.
Lévi-Strauss, Claude.

Maybury-Lewis, David.

Oberg, Kalervo.

Price, P. David.

Price, P. David and Cecil E. Cook, Jr.

Rondon, Cândido M. da Silva.

Roquette-Pinto, Edgar.

Textor, Robert B.

Udy, Stanley H., Jr.

**COMMENTS BY LÉVI-STRAUSS**

I do not intend to question Professor Aspelin's well documented and competent account. However, what has been observed at a given date does not necessarily invalidate previous reports by two independent observers — Oberg and myself — on conditions prevailing respectively
twenty and thirty years earlier; and all the more so as, in Oberg's case, the relevant data were corroborated by the missionaries who, having lived for ten years in contact with the natives, had ample time to get acquainted with their seasonal moves.

To explain the discrepancies, several facts must be taken into account. First, this region which, in my time, was practically virgin land on both sides of the path followed by the telegraph line, underwent later a tremendous upheaval, with several airfields opened in the heart of Nambikwara territory, a major highway cutting across it, new settlements, a land corporation ruthlessly operating all over the area, and several devastating epidemics. Taken together, these changes may have compelled the Indians to modify their way of life. On the other hand, the political unrest which, at the time of my visit, obtained both between the Indians and the Brazilians and between the Indian bands themselves, could be held temporarily responsible for more wandering than may have been the custom.

Above all, it should be kept in mind that the Nambikwara were by no means a homogeneous lot. It was common knowledge in 1938 that the Southern Nambikwara led a far more sedentary life than the Northern groups. Even between neighbouring bands who, sometimes, spoke mutually unintelligible dialects if not languages, there were obvious differences in behaviour and material culture (such as the presence or lack of pottery) which may well have corresponded to different types of economic activities. While Professor Aspelin worked with the Mamaindê whom I have met briefly, most of my dealings were with more northerly bands.

When at the peak of the dry season, one of these bands took us to a village site two days' march north of Utiariti, we reached together a round, cleared sandy area devoid of habitations of any kind. However, this was undoubtedly a village site as the nearby gardens were well kept. The natives told us that they would rebuild their huts at the time of the rainy season. When I returned to Utiariti in December 1938, I observed a few kilometers away natives busily building a beehive hut of the type later described by Oberg, but which had been nowhere in evidence during the dry season. Therefore it did not seem unreasonable to assume, as Oberg did ten years later, relying upon the missionaries' testimony, that in our time at least the Northern bands were mostly nomadic during the dry season, and that they settled in freshly rebuilt huts in order to spend the rainy season.

Collège de France, Paris.