

# **On the geographical origins and dispersion of Tupian Languages**

*Greg Urban*  
*University of Pennsylvania*

Where did the Tupian languages originate? How did they come to occupy their historical homelands? José Brochado (1984), filling in a major lacuna in Lathrap's (1970) scheme, has added a distinctive voice to the long-standing debates surrounding these questions. I am grateful to Francisco Noelli for bringing him to my attention. As Noelli indicates, Brochado's work provides the foundations for dialog and cooperation between linguistics, cultural anthropology, and archaeology. It is in the spirit of cooperation and dialogue that I'll make some friendly criticisms of his research, as well as of the linguistic and cultural work pertinent to the question of Tupian origins. My purpose will be to pinpoint areas for further research that might provide clues for solving the continuing mystery surrounding the Tupi.

I'll be concerned primarily with two principal hypotheses put forth by Brochado, the first deriving from Donald Lathrap, the second the novel contribution of Brochado himself:

1- Displaced Persons Hypothesis<sup>1</sup>: that the Tupian stock originated along the main course of the Amazon river. The hypothesis is based on the observation that the families of the Tupian stock (other than the Tupí-Guaraní family) occupy tributaries of “the upper course of the Xingu, Tapajós, and Madeira... far from the main course of the Amazon... Because all of these rivers are southern tributaries of the Central Amazon or of the Madeira, the only rational explanation is that they have radiated from Central Amazonia up these rivers, which further suggests that we should look to this area [the central Amazon] as the center of origin of the Tupi stock” (Brochado 1984:36)<sup>2</sup>.

2- Two-Pronged Hypothesis: that the proto-language from which both Tupinambá and Guaraní derived was located along the main course of the Amazon, and that “the proto-Guarani must have started moving up the Madeira, out of Central Amazonia, by at least 200 B.C., or perhaps even earlier” (Brochado 1984:265). They would have entered the Paraná-Paraguay drainage and southern Brazil from the west and north by 100 A.D. The proto-Tupinambá, in contrast, would have moved eastward down the Amazon, and then along the coast of Brazil and into eastern Brazil by 800 A.D. “The combined thrusts of the Guaraní and Tupinambá described an immense two-pronged movement encircling the eastern section of the Brazilian Uplands. The final result of that encircling movement was that the Guaraní and the Tupinambá eventually clashed in Southern Brazil, along a front running generally parallel to the course of the Tieté River” (1984:371)<sup>3</sup>.

Regarding this second, two-pronged hypothesis, Brochado (1984: 352) states: “It is the fact that, in terms of shared [linguistic] innovation, the Tupinambá and Cocama-Omágua form one natural unit, while Guaraní and Sirionó form a contrasting natural unit, that establishes an Amazonian hearth for the differentiation of Proto-Tupinambá from Proto-Guarani.” And he apparently regards this Amazonian origin – by which I take it he means the banks of the main course of the central Amazon river – as “fact and not hypothesis” (Brochado 1984:352).

My conclusion, having reviewed the arguments, is that it is hypothesis and not fact. It is an interesting hypothesis, but one that is by no means firmly established.

**1. We need archaeological investigation of the areas adjacent to the Chapada dos Parecis, as well as of the watercourses flowing from it into tributaries of the Amazon**

The Lathrap-Brochado displaced persons hypothesis regarding the origins of the Tupian stock conflicts with the linguistic hypothesis put forth by Rodrigues (1964:103):

Digno de nota é o fato de quase todas as famílias lingüísticas do tronco Tupí até agora reconhecidas se concentrarem na região do Guaporé, isto é, do alto Madeira, particularmente entre os rios Guaporé e Jiparaná (ou Machado)... Este fato sugere que talvez o centro de difusão do Proto-Tupí deva ser procurado na área do Guaporé.

Migliazza (1982:500-502) and Urban (1992:92) followed Rodrigues regarding the Tupian stock, but saw it as one instance of a larger pattern. Migliazza looked at the Tupian case alongside that of the Arawak, Carib, and Pano-Tacanan, and observed correlations between the postulated homelands of these families and forest refuge sites, as well as dispersal centers for terrestrial vertebrates. I included the same families, plus the Jê, some smaller language families, and linguistic isolates. The peripheral hypothesis holds that the areas of linguistic dispersion (between 3,000 and 1,000 B.C.) were arrayed in the elevated regions forming the periphery of the main course of the Amazon. Presumably, the language families originated in cultures adapted to these regions, with mixed subsistence bases – gathering, hunting, fishing, and part-time agriculture.

What strikes me today, reviewing Brochado's argument, is that there are few if any archaeological data directly supporting the displaced persons hypothesis. The argument is made largely from the antiquity

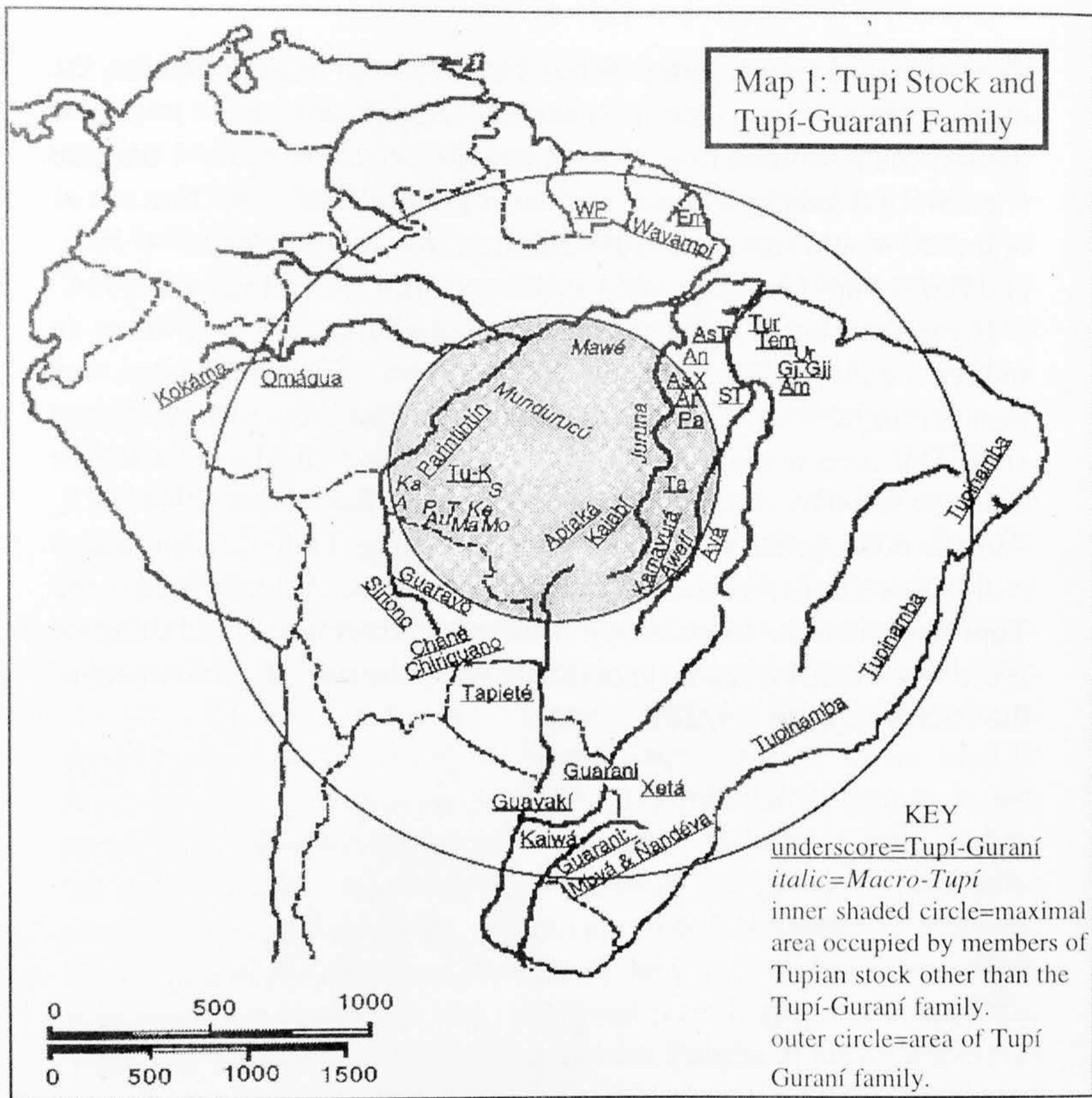
of pottery in the Amazon basin. The only stylistic arguments concern Tupinambá and Guaraní and their relationships, respectively, to the Miracanguera and Guarita traditions of the Amazon. No actual linkages have been established with the Macro-Tupí living in the uplands adjacent to the Guaporé valley. The antiquity of ceramic traditions along the Amazon does not tell us that the Tupian language families originated there. Languages and ceramics do not necessarily travel together, and, in any case, Brochado has not established a positive connection between the Macro-Tupí families and these pottery traditions.

What we need are archaeological investigations of the Chapada dos Parecis and of the drainages into the Guaporé-Madeira, as well as the Tapajós<sup>4</sup>. We need to document the movement of Tupí-speaking peoples into this region from the Amazon, if the displaced persons hypothesis is to be corroborated, or, contrarily, the movement from the Chapada down into the Amazon, if the upland or peripheral hypothesis is to be verified. The evidence ought to be readily interpretable. Lathrap argued (1970: 129) that "Groups pushed even temporarily onto the uplands of old alluvium could be expected to lose the more complex aspects of their social and religious life, and there would be far less time for non-functional embellishments of ceramics." Hence, the Macro-Tupians living near the Chapada dos Parecis must represent degenerate cultures, devolved from higher cultural forms. What we ought to find in the Chapada dos Parecis, if his hypothesis is correct, is an early (1,000-3,000 B.C.) intrusion of a highly developed ceramic tradition, which would show signs of internal degeneration over time into simpler forms.

Let me now make some observations about the problem, based on the documented spatial distributions of the languages (see Map 1), as well as on a rough outline of the classification of the Tupian stock (Figure 1). I'll have more to say about the internal classification of the

Tupí-Guaraní family later. What I want to say here concerns the displacement of languages in space with respect to time. In linguistic theory, the tree branching correlates with time, the base of the tree (Proto-Tupí) taking us back to possibly 3,000 B.C. The first set of branches would shoot out from the trunk sometime after 3,000 B.C., and Proto-Tupí-Guaraní would itself have been branching by 500 B.C.

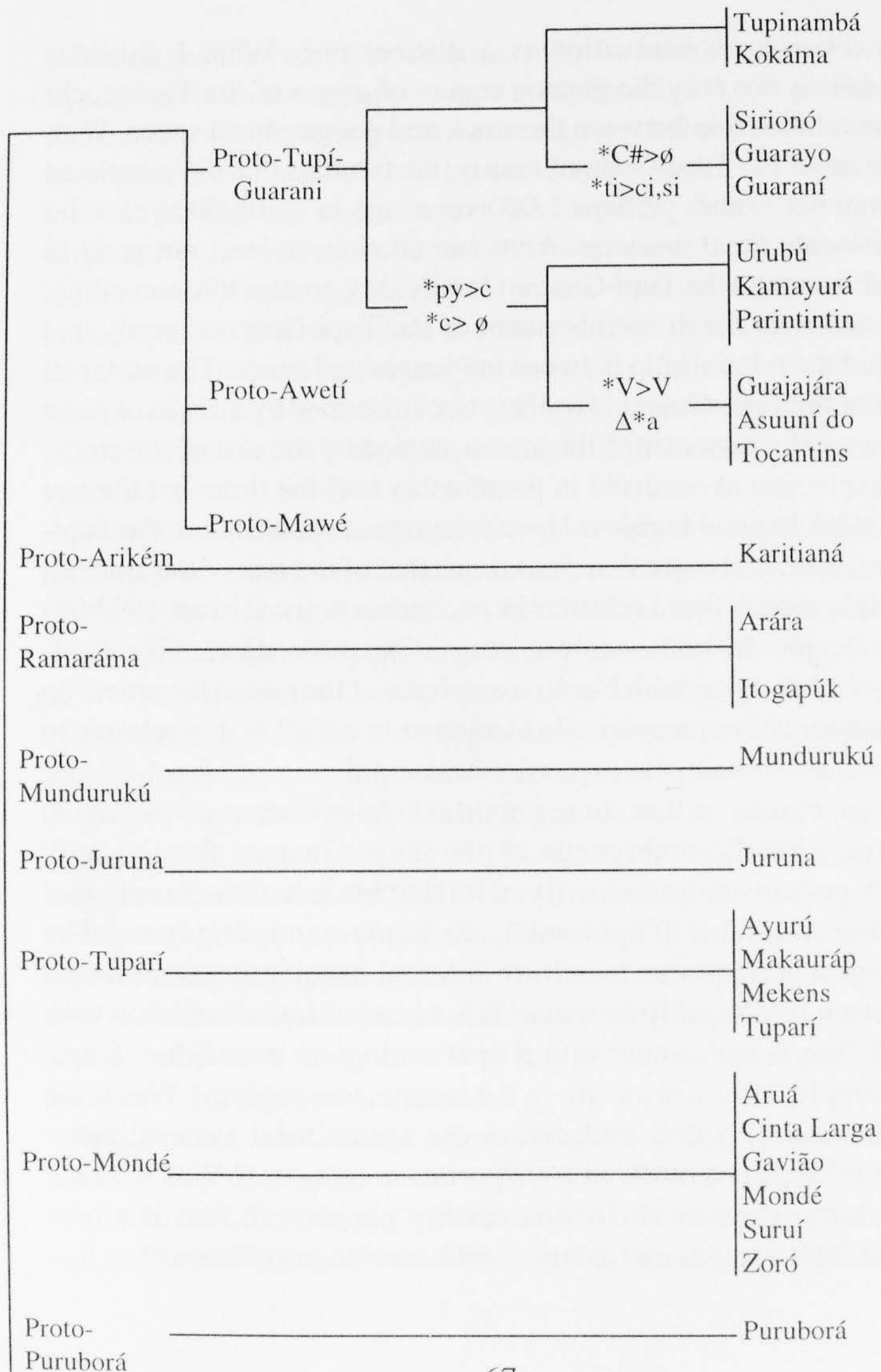
If you correlate this branching with spatial distribution, you see an interesting thing. The languages that branched off first did not travel very far, or, at least, they ended up within a relatively circumscribed area. This area is indicated by the shaded circle on Map 1. The fact has been noted by various authors, at least since Rodrigues (1964: 103), that the concentration of nodes (other than the Tupí-Guaraní node) within this smaller circle, indicates that the probable homeland of the Tupí stock is to be found within this area. I therefore couldn't agree more with Noelli when he generically circumscribes the homeland of the Proto-Tupí (ms. p. 28).



Abbreviations for Tupí-Guaraní Family	
Abbreviations for Tupí Stock Languages	Languages/dialects
A=Arara	Am=Amanajé
Au=Aruá	An=Anambé
Ka=Kepkiriwat	Ar=Araweté
Ma=Makurap	AsT=Assuriní do Tocantins
Mo=Mondé	AsX=Assuriní do Xingú
P=Puruborá	Em=Emérillon
S=Surui	Gj=Guajá
T=Tuparí	Gjj=Gujajára
	ST=Suruí do Tocantins

Not listed on map:  
 Horá=southernmost dialect of Sirionó and adjacent to it.  
 Kokamáya=almost identical to Kokáma and adjacent to it on map.

upi



However, my observation is a distinct one. What I consider important is not only the generic region of origin of the Tupí stock, but the relationship between the stock and geographical space. With exception of the Tupí-Guaraní family, the families that differentiated out from each other perhaps 5,000 years ago in that 5,000 years did not dislocate great distances from one another, at least not great in comparison with the Tupí-Guaraní family. My point is that something happened with the dismemberment of the Tupí-Guaraní family that changed the relationship between languages and space. The maximal extent of the Tupí-Guaraní family is circumscribed by a circle of more than twice the diameter of the area occupied by the rest of the stock. The displacement occurred in presumably half the time that the rest of the stock has had to move. Hence, the rate of expansion of the Tupí-Guaraní family is more than four times that of the rest of the stock. It is for this reason that I referred in my earlier work (Urban 1992:92) to the “explosão que ocorreu com a expansão da família Tupí-Guaraní.” I disagree with Noelli’s emphasis at the end of his article on the slowness of expansion. Slow relative to what? If it is relative to the stock as a whole, the expansion was rapid.

My conclusion is that some important change occurred in Tupian cultures with the emergence of the Tupí-Guaraní family, and, indeed, perhaps more narrowly, with that branch of the family that includes Guaraní, Tupinambá, Kokáma, and Omágua. The languages, perhaps due to culture-internal developments, assumed a different relationship to space. What kind of transformation took place? Was it movement into a new ecological zone (the várzea, for example, after a prior life in the headwaters region)? Was it the introduction of a new cultigen in the agricultural system (bitter manioc)? Was it a mode of transportation (canoes)? Was it a new cosmology (the search for the earthly paradise)? Was it a new orientation to travel and contact with remote populations?



Whatever the case, the “displacement” of the displaced persons hypothesis does not mean the same thing for all of the Tupi. For most of the Macro-Tupians, it would have meant a relatively short migration upstream, into the uplands. Why did they not seek out lowlands and várzea-like ecology elsewhere, as, presumably, did the Guaraní and Tupinambá, according to Brochado? Why were the Tupinambá and Guaraní not displaced into the uplands like their Macro-Tupian counterparts? If Brochado’s model accounts ecologically for the distribution of the Guaraní and Tupinambá, it fails to account for the Macro-Tupians. The models we are using presume that all of these cultures were basically alike. But the spatial distribution of languages suggests that this was not the case.

In addition to this principal confusion within the displaced persons model, there is the lack of clarity about precisely when displacement might have occurred. Brochado (1984:308) writes: “Lathrap’s model of the spread of the Tupian languages places the first differentiation of Proto-Tupian in Central Amazonia around 2000 B.C. This corresponds to the node at the top of the phylogenetic chart of *Macro-Tupian languages*, as it has been reconstructed from Lemle (1971) (Figure 3)” (my emphasis). But his Figure 3 does not show the Macro-Tupí level. It shows only the Tupí-Guaraní family. The slippage is important because of the difficulty of bringing the archaeological evidence together with the Macro-Tupí level. Brochado (1984: 316) himself notes that “prior to 500 B.C., data from within the Central to Lower Amazon Basin is scant and from widely separated locations.” The correlation of the Macro-Tupí level with an ancient polychrome tradition is, at this point, a guess.

Furthermore, what exactly happened back in 2000 B.C., or whenever the first Tupian displacement took place? Did the community that displaced the others displace them one at a time, in a kind of series, so that we have first one Macro-Tupian group heading up the Madeira,

then another, then another? Why did so many of them get displaced to the same area? Was it because this area was unoccupied? The lack of conceptual clarity is in part what makes the model so appealing, if one only studies pottery stylistics. But it is hard to pin the model down with real-world situations of displacement that might result in so many of the Macro-Tupian families ending up in one place.

The alternative, of course, is to imagine that the Macro-Tupian languages began their dispersal in the uplands, and that some of them moved downstream, coming in contact with cultural developments along the Amazon river and possibly participating in them. Certainly, it is easier to imagine the Tupí-Guaraní family having its origins in an encounter between a Macro-Tupian group and várzean cultural developments, than it is to imagine that the Tupian stock itself arose in this way, relocating through displacement. On this part of Brochado's work, and Noelli's exposition, I am still placing my bets on the upland origin of the Macro-Tupians. However, archaeological investigation of the region is needed to provide more evidence about what might have actually happened here from 3,000 B.C. or so until somewhere around 1,000-500 B.C. My guess would be that the early Tupians were adapted to an upland environment, but with frequent forays into regions of lower elevation, and possibly even into the várzea.

**2. We need a new internal classification of the Tupí-Guaraní family, an arrangement of sound shifts into a historically meaningful sequence, and lexical sets that establish subgroupings of the Tupí-Guaraní family<sup>5</sup>**

Crucial to Brochado's two-pronged hypothesis is the relationship between Tupinambá, spoken along the coast of Brazil in the sixteenth and seventeenth centuries, and old Guaraní, spoken at about the same time in the areas that are today southern Brazil and Paraguay. How

closely are they related? Are they dialects of a single language, or are they distinct languages? How long ago might they have diverged, judging from linguistic criteria? Brochado (1984:365) has already guessed “at least 200 B.C., or perhaps even earlier,” based on archaeological evidence. Can that guess – a separation of at least 1800 years to the time of classical Tupinambá – be supported linguistically?

Over three decades ago, Rodrigues (1964:103) opined that: “Segundo o critério aqui adoptado, a relação entre o Tupinambá ou Tupí Antigo e o Guaraní Antigo é a de ‘dialetos,’ e dialetos muito próximos (90% de cognatos), mas não a de ‘linguas.’”<sup>6</sup> This statement was based on the cognate rates he observed – that is, on the percentage of words from a basic word list that are shared between the two languages and that can be shown to derive from a proto-language ancestral to the two. Dietrich (1990) has also recently argued for their close grammatical similarity.

Can the differences support a claimed separation of 1,800-2,000 years? I attempted to confirm Rodrigues’s (1964:103) glottochronological estimate, using Lemle’s (1971) cognates and a small subset from Swadesh’s word list. My sample – too small to be taken seriously, but a good check on Rodrigues’s results – showed a greater than 85% cognate rate between Guaraní and Tupinambá. Using the same word selection, Spanish and Portuguese showed a 90% cognate rate, while French showed a rate of about 75% with both Spanish and Portuguese. So the relationship between Tupinambá and Guaraní, based on these criteria, would be closer to that between Spanish and Portuguese than between French and either Spanish or Portuguese<sup>7</sup>. That is, Tupinambá and Guaraní would be distinct languages, but very closely related. It is not unreasonable to think of them as like Portuguese and Spanish. The 1,800-2,000 year minimum separation proposed by Brochado and Noelli seems probably too long. Something much closer to a 1,000 rather than 2,000 years would seem reasonable. And 1,000 years fits the earliest

radiocarbon dates of Tupinambá ceramics in the Northeast – approximately 800 A.D. (Brochado 1984: 342).

An important revision of this relationship occurred less than a decade later, with the publication of Lemle's (1971) reconstruction of the Tupí-Guaraní family, demonstrating certain interesting sound correspondences. Her work established that Kokáma, a language of the western Amazon basin, was closer to Tupinambá than to Guaraní, at least insofar as sound correspondences were concerned. Rodrigues (1964:102) had placed Kokáma in a separate subfamily of the Tupí-Guaraní family, which he had divided into six subfamilies. Moreover, Sirionó, which Rodrigues had listed as part of a separate subfamily, but put a question mark by, was placed by Lemle together with Guaraní.

Lemle's work was crucial to Brochado, who, however, drew an unwarranted conclusion from it: "the fact that Tupinambá is in one group and Guaraní in another is crucial for my thesis. This means that the speech communities we shall call, respectively, Proto-Tupinambá and Proto-Guaraní had split long ago" (Brochado 1984:38). Lemle's work does not mean that at all. She established some sound shift differences between Tupinambá and Guaraní. Most importantly, she noted that what had been consonants in word final position in the proto-Tupí-Guaraní language get dropped in Guaraní but are retained in Tupinambá. So, for example, the reconstructed proto-Tupí-Guaraní word for 'tighten' is \**momik*. In Guaraní it is *momĩ*, but in Tupinambá it is *momik*.

Now that is not a great difference. From the fact of a sound shift alone, we cannot determine the date of separation of two speech communities. We would need other information to reach conclusions about time depth. Two dialects of the same language might also be distinguished by sound shifts. This is certainly true, for example, of class-based dialects in the American English of New York City, where the lower classes tend to drop post-vocalic /r/, whereas the upper clas-

ses tend to retain it (Labov 1972). So the word for 'four' is for the upper classes /f r/ but for the lower classes /f /.

I should add that, while Lemle distinguished Guaraní from Tupinambá, she put them together in one branch of the Tupí-Guaraní family. The other branch of the family included all of the other *Amazonian languages* Lemle studied (Asuriní and Guajajára grouped together, along with, possibly, Tapirapé; and Kagwahiv [or Parintintin] and Kamayurá and Urubú grouped together, along with, probably, Kayabí). So Guaraní and Tupinambá continue to be closely related in Lemle, pace Brochado, with Kokáma being the only Amazonian language that forms a grouping together with them.

Lemle's (1971:128) classificatory tree forms part of Figure 1, which includes as well the Macro-Tupian families and languages proposed by Rodrigues (1985b, 1986). The tree shows the sound shifts that would have led to differentiation at the different nodes. Thus, the earliest sound shift would have differentiated an Amazonian or northern group from an extra Amazonian (with the exception of Kokáma) or southern group. This is not the thesis so convincingly put forth by Brochado, admittedly, but it is the one suggested by the distributions, were it not for Kokáma. Let me exempt the latter language for the moment. I'll come back to it later, as it is crucial Brochado's hypothesis.

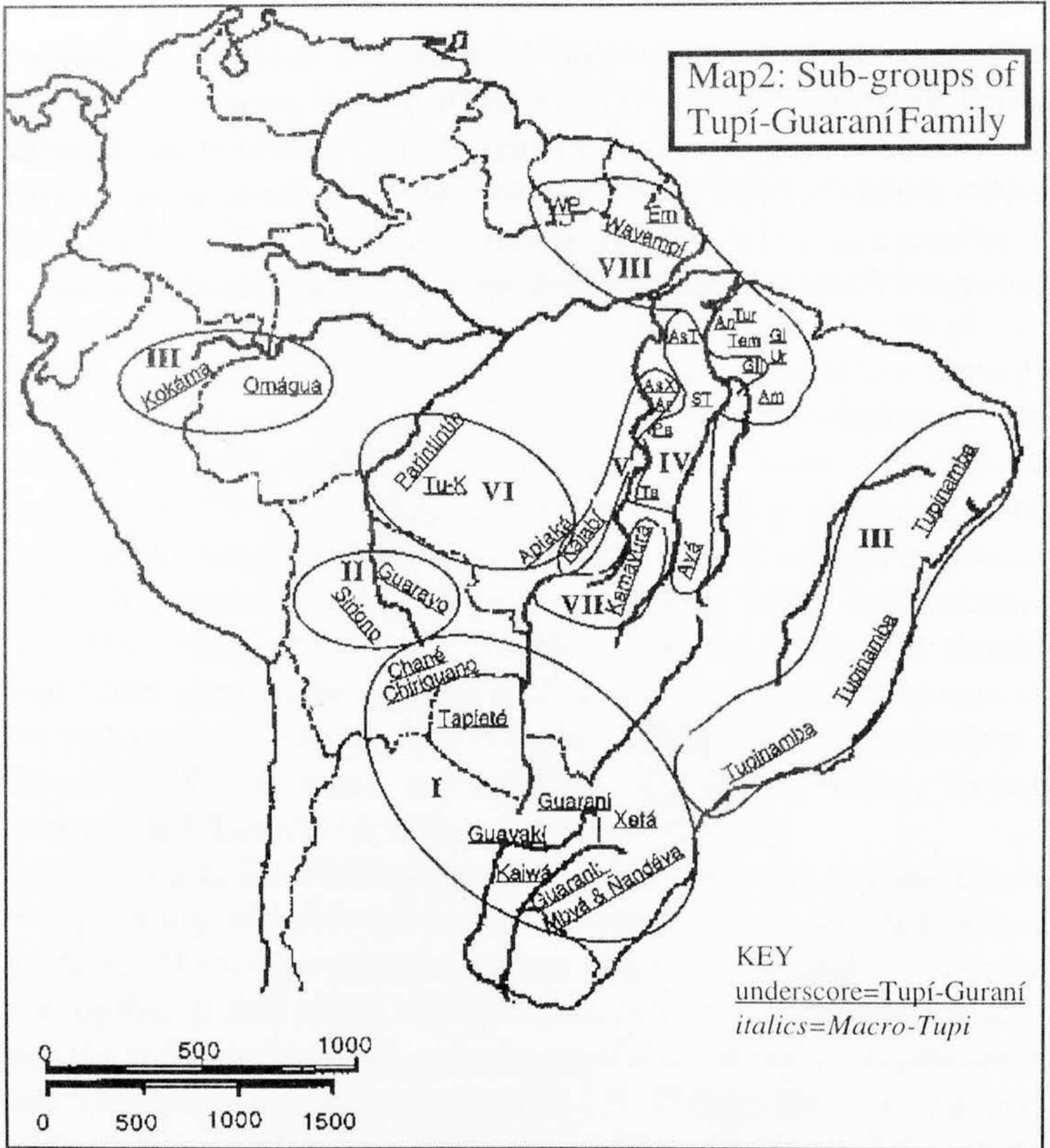
In the Lemle classificatory tree, the southern or extra-Amazonian branch of the Tupí-Guaraní family would have maintained itself, at this early phase, closer to the Proto-Tupí-Guaraní phonology. The Amazonian group, in contrast, would have changed some of the proto-sounds. In particular, \*py [or \*pj]<sup>8</sup> in the proto-language and in the southern or extra-Amazonian branch would have continued unchanged into the historical languages. However, the Amazonian branch would have changed \*py [or \*pj] to c [or ts]. In a later differentiation, the ancestors of the Tupinambá and Kokáma, forming

one branch, would have continued the proto-forms, and the ancestors of the Guaraní, Guarayo, and Sirionó, forming another branch, would have changed some of the proto-sounds. In particular, they would have dropped final consonants, as mentioned earlier, and they would have changed \*ti to ci [tsi] or si.

The model is very neat, but I have often wondered whether it was not too neat. Would Tupinambá so perfectly carry on the proto-forms? My doubts about this stemmed from the apparent bias towards Tupinambá, which is excessively well-represented on the list. Of the 221 Proto-Tupí-Guaraní lexical items, 206 have reflexes in Tupinambá. The next highest number of cognates is Guarayo (164), followed by Sirionó (153), Guaraní and Guajajára (150), Parintintin (146), Kamayurá (143), Asuriní (140), Urubú (126), and Kokáma (94). The average is 141 or 68% of the Tupinambá items. Why should Tupinambá be so well-represented? Is it because, as a language of the sixteenth and seventeenth century, it was closer to Proto-Tupí-Guaraní? Was it more conservative? Or was it simply better documented than the other languages?

In any case, Rodrigues's (1985b) made a subsequent attempt at sub-grouping, which I have organized into Figure 2, and represented in Map 2. His subgroupings are more conservative, less historically intelligible. It will not be immediately apparent to the casual reader that the sub-groupings are actually related to Lemle's classificatory tree. The representation in Figure 2 is an attempt to bring that relationship out. However, Rodrigues (1985b: 48) himself was quick to note that his work on the eight "sub-conjuntos" did not constitute an internal classification of the family: "mas antes um ensaio de discriminação de seções dessa família caracterizadas pelo compartilhamento de algumas propriedades lingüísticas, as quais podem servir para diagnosticar o desmembramento de todo o conjunto de línguas Tupí-Guaraní visto como resultante histórico de uma proto-língua pré-histórica."

In any case, the first line in Figure 2 relates to Lemle's transformation of \*c [or \*ts] into Ø. Of his sub-groupings, IV-VIII show a clustering, as in Lemle's tree. In all of these languages, the proto-sounds \*tx and \*ts merge. However, in some cases the merger is reflected as h (IV and VI), in others as h or Ø. The relationship between IV and VI does not show up in any other transformation.



Abbreviations for Tupí-Guaraní Family

Languages/dialects

- |                          |                   |
|--------------------------|-------------------|
| Am=Amanajé               | Pa=Parakanā       |
| An=Anambé                | Ta=Tapirape       |
| Ar=Araweté               | Tem=Tembé         |
| AsT=Asuriní do Tocantins | Tu-k=Tupí-Kawahib |
| AsX=Asuriní do Xingú     | Tur=Turiwára      |
| Em=Emérillon             | Ur=Urubu-Kaapor   |
| Gj=Guajá                 | WP=Waympipukú     |
| Gjj=Guajajára            |                   |
| ST=Suruí do Tocantins    |                   |

Not listed on map:

- Horá=southernmost dialect of Siriono and adjacent to it
- Kokamáya=almost identical to Kokáma and adjacent to it on map



	I	II	III	IV	V	VI	VII	VIII
1a	*tx>ts,s or tx	*tx+*ts>ts,s	*tx+*ts>ts,s	*tx+ts>h	*tx+*ts>h,ø	*tx+*ts>h	*tx+*ts>,h,ø	*tx+*ts>h,ø
1b.	*ts>h,ø							
2.	*pj>tx,x			*pj>tx,ts	*pj>s		*pj>ts	*pj>s
3.	*pw>kw,k	*pw>kw,k		*pw>kw	*pw>f	*pw>kw,fw,f	*pw>hw,h	*pw>kw
4.	*C#>ø	*C#>ø						*C#>C,ø
5.				*j>tx,ts,s,z	*j>dy			
6.					3rd person pro. marking	3rd person pro. marking		
7.		*accent ult> accent pen						
L	Guarani Antigo	Guarayo	Tupinambá	Tapirapé	Kayabí	Parantintin	Kamayurá	Takunyapé
A	Mbyá	Sirionó	Língua Geral Paulista	Avá	Asurini do Xingu	Tupi-Kawahib	Kamayurá	Wayampi
N	Xetá	Hora= southern dialect of Sirionó	Língua Geral Amazônica	Assurini do Tocantins	raweté(?)	Apiaká		Wayampipukú
G	Ñandevá		Kokáma	Suruí do Tocantins				Emérillon
U	Kaiwá		Kokamíya	Parakanã				Amanayé
A	Guarani Paraguaio		Omágua	Guajajára				Anambé
G	Tapieté			Tembé				Turiwára
E	Chiriguáno							Guajá
S	Chané							Urubú

Figure 2: Rodrigues' Sub-grouping of the Tupi-gurani Family

As regards the characteristics in lines 1b. and 2. of Figure 2, sub-group I (with Guaraní) diverges from subgroups II (with Guaryó and Sirionó) and III (with Tupinambá and Kokáma). This might suggest a historical node in which sub-group I diverged from sub-groups II and III, which then later diverged from each other. However, look at the transformations under numbers 3 and 4. In this case, sub-group III seems to have diverged from sub-groups I and II, which form a natural grouping. What could account for this? Well, one possibility is that there were periods of isolation between ancestral communities followed by periods of intensive contact, in which the changes affecting one community carried over into the next. Proto-Guaraní might have hived off from Proto-Tupinambá. Guarayo could be, in this scheme, an off-shoot of Proto-Tupinambá, but then Guarayo would have later become isolated from Tupinambá, coming into contact, however, with Guaraní, so that both were affected by the transformation indicated in 3 and 4. However, we need additional research to determine what has gone on here.

My suggestion is that a major attempt be made to organize the Rodrigues transformations into a historically intelligible sequence. If that does not prove readily doable, then we may need a major new reconstruction of Proto-Tupí-Guaraní itself<sup>9</sup>. In particular, my recommendation would be not only that we redo the proto-lexicon, but that we search for cognate sets that establish or confirm sub-groupings, but that themselves do not derive from the proto-lexicon. In other words, what we want to show is that some lexical innovations took place after the original dismemberment of the family, and that those lexical innovations characterize certain sub-groups of Tupí-Guaraní languages. The nature of those lexical items may actually tell us something about the cultural transformations that took place, in the spirit of Benveniste (1973), and, perhaps also, the environments that the ancestors of the sub-groups might have inhabited when the innovation took place (see Rodrigues (1988) quoted in Balee (1994:

2) regarding linguistic evidence for agriculture in Proto-Tupi).

An examination of Map 2 will show that Rodrigues's sub-groupings do make considerable geographical sense. It is difficult to draw lines perfectly around the sub-groupings, because, for example, the areas occupied by sub-groups IV and VIII overlap. But it is clear in this case that IV has the more southerly, VIII the more northerly distribution. The pronominal marking systems and the transformation of \*pw into f do align V and VI as more westerly, within this northern or Amazonian branch. However, the transformation of \*pw into kw in sub-group VI is part of a broader set of transformations that sweeps across the family, and that may have occurred independently or reflected contact. And this alignment does not jive with the reflexes of \*pj, where sub-groupings V and VI are radically distinct – in V \*pj becomes s, whereas in VI \*pj is retained as pj.

The transformation of \*pj [or \*py] into ts [or c] was a major feature of Lemle's differentiation of the Tupinambá-Guaraní node from the Amazonian node. However, the transformation did not take place in Rodrigues's subgroup VI, the westerly sub-grouping. Moreover, \*pj did undergo change in Guaraní, and that change, as well as \*ts into h or Ø, may indicate later contacts between the Guaraní branch and speakers of sub-group IV, the upper Tocantins group.

Without Brochado's archaeological arguments, and ignoring Kokáma again, for the moment, we might be inclined to infer from this evidence that the Tupí-Guaraní family began its dismemberment somewhere in central Brazil, perhaps, along the Xingu river, with the north/south split taking place first, albeit with later contacts. The vector would have been southward, with perhaps another north/south split differentiating the Tupinambá (north) from Guaraní (south), going along with an east-west opposition. This is not an entirely ridiculous proposition, given the distribution of Guaraní sites and their radiocarbon dates in Brochado (1984: Figure 15), where the oldest site

is in the Alto Paraná. In this scenario, the ancestors of the Tupinambá and Guaraní would have crossed over the central Brazilian highlands, from the headwaters of the Xingu and Araguaia rivers, into the Paraná drainage.

**3. We need archaeological investigations of: (i) the Brazilian coast from the mouth of the Amazon to Rio Grande do Norte, documenting the proposed entry of the Tupinambá into eastern Brazil, and (ii) the Madeira-Guaporé into the Paraná-Paraguay system, documenting the proposed entry of the Guaraní into southern Brazil.**

I must confess that on first blush Brochado's two pronged hypothesis struck me as wildly implausible. Could we seriously imagine a community living along the Amazon between 500 and 200 B.C., splitting in two, with one half forming a compact traveling group that moved up the Madeira and Guaporé, with no groups hiving off along the way, until the entry into the Paraná-Paraguay headwater? Similarly, the other group would have descended the Amazon, reaching the mouth, and worked its way down along the coast, again without small groups hiving off, until it reached the area around Rio Grande do Norte. Can we imagine this kind of scenario in an ethnographically real world?

My initial response was, No, but Noelli's paper, followed by a close reading of Brochado's work, has led me to conclude, Maybe, although Probably Not. The archaeological evidence Brochado mounts and the argument he spins around it are intriguing. However, the argument is entirely built up around ceramics and stylistic interpretation. The direct archaeological evidence at this point does not extend beyond the areas of known inhabitation of the Tupinambá and Guaraní. We do not have evidence of a continuous movement along the proposed pathways based on direct archaeological finds.

Let's look briefly at the question of the Guaraní migration. According to Brochado (1984: 365), by "at least 200 B.C." the ancestors of the Guaraní would have split from the ancestors of Tupinambá somewhere along the middle course of the Amazon. The early Guaraní would have headed up the Madeira and around the central plateau of Brazil, entering the Paraná-Paraguay drainage area and arriving in southern Brazil by 100 A.D. In what would have been a remarkable journey, they would have moved several thousand kilometers in the space of 300 years or so, and then become relatively sedentary for the next two thousand years.

What is the evidence that they made this journey? Brochado (1984: 365) acknowledges that "there is almost no archaeological data" on the area covered by the proposed migration route. His argument rests upon a set of inferences drawn from ceramic styles. The Guaraní pottery shares some of its characteristic shapes (particularly, the conoidal jars) with traditions farther west, which Brochado (1984: 324-328) ultimately associates with the Panoan peoples of Peru and Bolivia. However, the actual archaeological finds connected with this Cumancaya style date from a period later (350-550 A.D. at the earliest) than the arrival of Guaraní in southern Brazil. So Brochado hypothesizes that the early Guaraní must have acquired their characteristic pottery shapes from speakers of Proto-Panoan living on the Bolivian side of the Guaporé valley.

My point is that there is no direct archaeological evidence for this part of the two-pronged hypothesis. At this point, we cannot rule out areal diffusion of style, and we cannot even demonstrate that the Panoans did not acquire the conoidal jar shapes from the Guaraní, rather than vice versa. After all, DeBoer (1990) argued that the later Panoan style represented an acculturation to the Kokáma-Omágua pottery tradition. The foundation of evidence upon which Brochado has erected his bold hypothetical construct is remarkably rickety.

Nor is the evidence much better for the descent of the Tupinambá from the mouth of the Amazon down the coast of Brazil. Brochado (1984: 343) states that “the criterion of spatial continuity is totally fulfilled with regard to the expansion of the Tupinambá. There is no embarrassing gap, such as the one that complicates my discussing of Guaraní expansion.” In fact, however, he goes on to note that “very little archaeological investigation has been done in the area between Marajó and Northeastern Brazil.” The few archaeological pieces he does mention are not dated. Furthermore, as Fausto (1992: 382) notes, the radiocarbon evidence for north-to-south movement along the eastern coast is not convincing: “não há uma diferença substantiva entre as datações mais antigas no Rio de Janeiro ( $980 \pm 100$  d.C.), e as do extremo nordeste da costa ( $800 \pm 65$  d.C.).”

In my opinion, direct archaeological evidence for the Tupinambá prong of the hypothesis is as shaky as is that for the Guaraní prong. We need further investigation of these two crucial areas. Because the stylistic evidence presented by Brochado centers on the differentiation of Guaraní and Tupinambá ceramics, aligning the latter with Miracanguera and Marajoara, but neglecting a full comparison with Kokáma and Omágua ceramics, the two-pronged hypothesis seems to me to depend principally on linguistic evidence, and, in particular, on the relationship between the Tupinambá and Kokáma-Omágua languages. Consequently, it is to this question that I now turn.

#### **4. We need new ethnohistorical, linguistic, and archaeological research on the Kokáma and Omágua, focused on their relationship to the Tupinambá of the Brazilian coast**

Brochado (1984: 352) wrote: “It is the fact that, in terms of shared innovation, the Tupinambá and Cocama-Omágua form one natural unit, while Guaraní and Sirionó form a contrasting natural unit, that establishes an Amazonian hearth for the differentiation of Proto-

Tupinmabá from Proto-Guarani.” At the same time, the archaeological relationship between the Kokáma-Omágua and the Tupinambá is not satisfactorily investigated in his work, nor, to my knowledge, has the problem since been well-studied from an archaeological point of view.

As a consequence, after reading Noelli and Brochado, I took a closer look at the Kokáma and Omágua linguistic materials. Rodrigues (1985b: 43) had already remarked that: “Como o Kokáma apresenta certas propriedades importantes não Tupí, dá a impressão de tratar-se de mais um caso de língua Tupí-Guaraní adotada por um povo não Tupi.” A key factor he mentioned was the sharp divergence of the grammars (and, I would add, the lexicons) of Kokáma-Omágua from Tupinambá, despite the direct derivability of many Kokáma lexical forms from those of Tupinambá. This is a crucial fact.

By my count, Kokáma shows only 94 cognates among Lemle’s (1971) list of 221, that is, 42%, as opposed to Tupinambá, with 206, or 93%. Part of this discrepancy can be explained by the paucity of Kokáma data. There are some cognates that did not show up on Lemle’s list. However, it does seem that, for many lexical items, the Kokáma language has words that are not recognizably Tupian. Because of the proximity of Kokáma to Tupinambá in a classification based on sound correspondences, the divergence in the lexicon, coupled with grammatical differences, signals language contact.

What I now believe is that Kokáma is an early variant of the Língua Geral Amazônica that was imperfectly taken up (perhaps 400 years ago) by a people who formerly spoke a non-Tupian language. Because of Kokáma’s propinquity to Tupinambá as regards sound correspondences, and its distance as regards lexicon, we would expect that, if Kokáma were an old Tupian language, the lexical divergence would be explicable in terms of recent borrowings. Hence, the sources of those borrowings ought to be readily ascertainable. Apparently, this is not the case, although further study is called for.

At the same time, an examination of the Tupian forms in Kokáma suggests their close affinity with the Língua Geral Amazônica (LGA), itself the descendant of classical Tupinambá. My guess is that Kokáma and Omágua were very early (perhaps even 16th century) offshoots of LGA.

Rodrigues (1986:104) notes four major phonological transformations that characterize the differences between classical Tupinambá and Língua Geral Amazônica. Each of these also applies to the differences between Tupinambá and Kokáma, as can be verified by a study of the data in Lemle (1971):

1- the *b* in Tupinambá merges with the *w* in LGA and also in Kokáma (except in final position, where it disappears (see Lemle (1971:112);

2- the sound *o* in Tupinambá merged with *u* in LGA and also in Kokáma (Lemle 1971:114). Kokáma is the only Tupí-Guaraní language investigated by Lemle in which this merger occurred;

3- LGA and also Kokáma add a vowel to verbs that in Tupinambá ended in a consonant, e.g., ‘sleep’ = *ker* (T), *ukiri* (K); ‘push’ = *moayan* (T), *yumuyani* (K); ‘stand’ = *puɁam* (T), *ipamawa* (K); ‘pierce’ = *kutuk* (T), *kitika* (K);

4- LGA and also Kokáma lose the nasal consonant  $\eta$ , although in Kokáma it becomes an *n* rather than nasalizing the preceding vowel (Lemle 1971:111); however, Faust and Pike (1959: 18) observe that the [ $\eta$ ] allophone of /n/ that occurs word finally appears optionally as nasalization of the preceding vowel; Kokáma is the only Tupí-Guaraní language studied by Lemle that, like LGA, lacks a phonemic velar nasal consonant.

There are also differences between Kokáma and LGA, which should not be ignored. The former, for example, does not have phonemic nasalized vowels, and it also lacks an *e*.

Still, the evidence of similarity between Kokáma and LGA found in the lexical forms is striking. In many cases, Kokáma and LGA exhibit more similarity with one another than either does with Tupinambá. The similarities are too great to be due to chance. An obvious hypothesis



to account for them is that Kokáma and LGA are both recent (< 500 years ago) offshoots of Tupinambá. In Table 2, I have given a few examples of these similarities:

Table 2: Comparison of Kokáma, Língua Geral Amazônica, and Tupinambá

	Kokáma	LGA <sup>a</sup>	Tupinambá	No. <sup>b</sup>
1.ashes	tanimuka	tanimuka	tanimuk	55
2.blood	tsuĩ	tui	uwĩ	190
3.bone	kanwara	kanwera	kaŋ	150
4.egg	tsupya	supia	upiʒa	152
5.fall	u'ari	wari	ʒar	35
6.fat	ikawa	ikawasawa	kab	19
7.hair	tša	sawa	ʒab	34
8.hot	tsaku	saku	akub	178
9.house	uka	oka	ok	41
10.husband	müna	mena	man	123
11.louse	kiwa	kiwa	kib	171
12.rain	amana	amana	aman	54
13.root	tsapwa	sapu	apo	181
14.smooth	ĩtsiman	isima	sim	113
15.tobacco	putima	pitima	petim	96

<sup>a</sup> The Língua Geral Amazônica forms are taken from Stradelli (1929). The phonemicization is my own. Stradelli very often lists multiple variants of a given word. In each case, I have chosen the variant closest to the Kokáma form. Here are the Stradelli forms in the original orthography: 'ashes': tanimúca, 'blood': tuí, 'bone': can-uera, 'egg': supiá, 'fall': uaari, 'fat': icáuasáua, 'hair': sauá, 'hot': sacú, 'house': oca, 'husband': ména, 'louse': kyua, 'rain': amana, 'root': sapú, 'smooth': iciyma, 'tobacco': pytyma.

<sup>b</sup> The numbers in this column are from Lemle (1971).

As regards the rules discussed above: Tupinambá *b* goes to *w* or  $\emptyset$  in Kokáma and LGA (6,7,8,11). Also, Tupinambá *o* becomes *u* (9, 13), although in 13 Kokáma *u* followed by an additional *a* evidently led it to its reanalysis as *w*, and in 13 LGA seems to have retained the *o*<sup>10</sup>. Also, we see, in these examples, that the addition of final vowels occurs

not only in verbs, but also in nouns (1,5,6,7,9,10,11,12,14,15). Finally,  $\eta$  becomes  $n$  in one case (3).

In addition to similarities owing to the rules discussed above, both Kokáma and LGA also sometimes add a vowel in front of a Tupinambá word beginning with a consonant (6, 14), and, more importantly, both also sometimes add consonants before Tupinambá words beginning in vowels, especially LGA  $s$  corresponding with Kokáma  $ts$  (4,7,8,13).

At the same time, LGA has probably itself undergone considerable change over time and appeared in different forms in different places. It should not be surprising that LGA and Kokáma also sharply diverge from each other in many lexical forms. As Moore (1993: 22) observes of contemporary LGA (or Nheengatú):

The language called today Nheengatu has changed at a rapid rate: the contemporary form would not be mutually intelligible with its form of 400 years ago. Other Tupí-Guaraní languages have not shown the same changes or the same rate of change. More than natural language change was at work to produce the changes in Nheengatu.

The general picture is consistent with the view that Kokáma and Omágua have resulted from the absorption of an early variant of LGA by non-Tupian-speaking populations.

If Kokáma is indeed a variant of LGA deriving from Tupinambá in the post-Columbian period, then all of its Tupian vocabulary should have Tupinambá cognates, i.e., it should not have Tupian words that do not have a Tupinambá origin, unless the latter could be shown to be recent borrowings. By my count, Lemle's (1971) Proto-Tupí-Guaraní word list contains 94 Kokáma cognates. Of these, 92 appear to have Tupinambá counterparts. Of the remaining two, I am able to account for one – 'swell':  $i'ruru$ . This is the Kokáma word given by Faust and Pike (1959: 74), although not by Espinosa 1989: 264), who give *titata*. However, the form *ruru* does appear in Tatevin (1910: 185) with the related meaning 'wet'. However, I am still not able to explain

the absence of Tupinambá cognate for Kokáma *tsüwüka*: ‘stomach’, though this may be due to inadequate data<sup>11</sup>.

One task ahead of us is to examine the non-Tupian lexical items in Kokáma in an attempt to determine their origin, which may reveal the original language on which LGA was superimposed. My own brief study turned up a number of items that may be of Arawakan origin<sup>12</sup>. In itself, this would not be surprising, since the historical Kokáma and Omágua lived near Arawakans – the Chamicuro and Morique, most immediately, and the other Peruvian Arawakans (Amuesha, Asheninca, Machiguenga, and Nomatsiguenga), as well as Arawakans further down the Amazon. Perhaps these are recent borrowings. However, the lexical forms diverge from those of their immediate neighbors, showing closer similarities to more far-flung Arawakan languages. The possibility therefore exists that Kokáma may have once been an Arawakan language.

We now have a very good, though by no means exhaustive, reconstruction of Proto-Arawakan (Payne 1991), so that questions about the provenance of words can be more systematically pursued. Some possible cognates are listed in Table 3:

Table 3: Some Possible Arawakan Cognates in Kokáma<sup>13</sup>

	Kokáma	Proto-Maipuran-Arawakan
‘bee’	mapa	maba
‘water’	uni	uni
‘salt’	tüwü	idiwi
‘wash’ <sup>14</sup>	tsukuta	šuku
‘pain’ <sup>15</sup>	tsači	kači [wi]
‘ant’ <sup>15</sup>	tsatsiwa	kaši

If these words are Arawakan loan words, the question is: to what languages are they most akin? Let's look at the first three. Simple variants of the form *uni* ('water') are widely disseminated throughout Maipuran Arawakan. Two neighbors of the Kokáma have variants of the form: Chamicuro *uníhsa* and Morique *on*, though neither has the simple form *uni*. The word *mapa* ('bee') is also widely distributed. However, it is not clear whether any immediate neighbors possess the form<sup>15</sup>. Asheninca and Machiguenga, for example, have unrelated words. Payne (1991: 395) does not report cognates for the neighboring Chamicuro or Amuesha, but the word given by Parker (1994: 265) for Chamicuro is the divergent form *maʔšiʔto*, and the Amuesha word is divergent (David Payne, personal communication). As regards the word *tiiwü* ('salt'), the most closely related forms come from Asheninca (*tiwi*), Machiguenga (*tibi*), Piro (*tiwti*)<sup>16</sup>, and Wapishana (*thiwti*), i.e., groups from the south and also east. In addition, none of the languages reported by Payne has a spirant in place of the initial *k* in 'ant' and 'pain', although spirantization of *k* occurs in other cases.

In short, the sources of the Arawakan words in Kokáma are not obvious. Hence, it is possible (but only *possible*) that they reflect an older substratum, over which a Tupian language was laid through a process of language replacement. At present this is largely speculation. A detailed study of the non-Tupian vocabulary in Kokáma needs to be undertaken to ascertain its provenance.

Two interesting regularities in the above list should be remarked: *\*k>ts/\_V[spirant]* ('pain', 'ant'); and *\*š>ts* ('wash', 'ant'). Given the minute size of the above sample, and the phonological processes that might have operated on loan words, these regularities are, at present, not impressive.

Other evidence suggestive of an Arawakan base for Kokáma and Omágua comes from phonology. The Kokáma phonemic inventory reported in Faust and Pike (1959) diverges in one important respect from all other Tupí-Guaraní languages. It has four spirants – two

fricatives (*s* and *β*) and two affricates (*ts* and *c*). No other Tupí-Guaraní language studied by Lemle (1971) has more than two spirants, and none shows a contrast between a fricative and an affricate spirant. Moreover, Lemle reconstructs only one spirant (*ts*) for Proto-Tupí-Guaraní. Interestingly, however, Payne (1991:444) reconstructs the same set of four spirants for Proto-Maipuran-Arawakan. If Kokáma and Omágua resulted from the superposition of LGA on an Arawakan substrate, the spirantal contrasts might reflect the retention of ancient Arawakan feature. At the same time, there are possible areal sources for this set of contrasts, since the neighboring Chamicuro actually distinguish six spirants (Parker 1994: 261), and Shell (1965) reconstructs four spirants for Proto-Panoan.

Another bit of evidence is found in the vowels. Kokáma lacks phonemic nasalization, as, apparently, did Proto-Maipuran-Arawakan (Payne 1991:457). The absence of phonemic nasalization in the substrate language might have resulted in the failure of the Arawakan-speaking Kokáma and Omágua populations to faithfully assimilate the Tupian distinctions.

At the level of grammar, Rodrigues (1985:43) has pointed to the radically divergent pronominal system. It is interesting to observe that at least one of the non-Tupian pronouns may be of Maipuran-Arawakan origin, albeit probably not a direct loan from any present-day language. The third person form used by male speakers is *uri*, which resembles the Proto-Maipuran-Arawakan *\*li*. Kokáma does not make a phonemic distinction between *l* and *r*, and *i* is a frequent reflex of *ɨ* in many Arawakan languages, and also possibly in the Kokáma word for 'ant'. Furthermore, some languages have a vowel preceding the *r*. In Asheninca Campa, for example, the three third person forms are *irirori* (m.), *iroori* (f.), and *iroo* (n.) (Payne, 1978). The other three non-Tupian pronouns in Kokáma do not appear to be obvious reflexes of the Proto-Maipuran-Arawakan forms, but first person singular male

speaking (*ta*) resembles the analogous form in Piro (*hita*), and the first person plural inclusive male speaking form (*tanu*) in Kokáma looks like the first person singular (*ta*) with the addition of a form identical to Proto-Maipuran-Arawakan first person singular (*\*nu*). In any case, it is possible (but, again, only possible) that the people speaking Kokáma today originally spoke an Arawakan language, traces of which remain in their grammar and lexicon.

At the same time, contemporary linguistic evidence alone cannot resolve this question. We need a detailed ethnohistorical reconstruction that focuses on this question. As far as I have been able to ascertain from the literature, with exception of two words recorded in 1542, we do not have linguistic evidence on Omágua and Kokáma prior to the eighteenth century (see Loukotka's (1968:116-117) references)<sup>17</sup>. Métraux (1963c:689) states that the two 1542 words "recorded by Carvajal in Aparia's village are Guaraní (*coniupuyara*, 'women', and *chise*, 'stars' not 'sun')."

The word *coniupuyara* ("women") might be linked to the Proto-Tupí-Guaraní word *\*kuyã* ("woman"). However, and this seems to have been overlooked by commentators, the word bears no relationship to those reported for Kokáma and Omágua as meaning "woman," which are *wáina* and *uainú*, respectively, in Loukotka's list (1968:118). The word "woman" is one in which both of these languages differ from Proto-Tupí-Guaraní, as well as from Guaraní and Tupinambá in particular. At the same time, *kunia* is mentioned by early sources as a feminine gender marker (Rivet 1910:171), so that the possibility cannot be completely ruled out that the word *coniupuyara* is of Omágua origin.

The word for 'stars', *chise*, bears scant resemblance to the Proto-Tupí-Guaraní word for "star" reconstructed by Lemle (1971: 120), *\*yatsitata*, which is nearly identical in form to the Guaraní or Tupinambá cognates she lists – *yacitata* and *yasi-tata*, respectively. It

shows perhaps a little more resemblance to the Kokáma word *tsiitsu*, but the connection is by no means apparent.

Another aspect of the problem is that there is doubt as to whether “Aparia’s village” was actually that of the Omágua, doubt that Métraux (1963c:689) himself expressed, because of the discrepancy as regards location. Porro (1992:182) notes the striking absence of any mention by the early chronicler of head deformation. Aparia’s village may have been that of the Tupinambarána – Tupinambá displaced by Portuguese from the coast, who would have crossed overland to the Madeira, then descended downward, arriving in the Amazon by 1538, and in Chachapoyas, far into Peru, by 1549. Tupinambá speakers from the coast were, therefore, as the result of post-Columbian migrations, in the area of the Kokáma and Omágua by the mid-sixteenth century, well before there is substantial record of the Kokáma or Omágua language. The latter peoples may have acquired the Tupinambá language from them.

During the 17th century, the Kokáma and Omágua were missionized and the entire region devastated by slavers. Since the first word lists are not reported until the mid-eighteenth century, much might have transpired in this turbulent period that could have resulted in language replacement. A detailed ethnohistory is needed that would focus on the linguistic issue, supplementing Myers’ (1992) reconstruction of the Omágua collapse, which does not scrutinize the linguistic issue.

Looking at the problem today, the evidence on Kokáma and Omágua leads me to doubt the Brochado-Noelli proposal, if not finally to reject it entirely. At the same time, the linguistic issue is only half of the matter. The other half is the question of ceramics. Surprisingly, Brochado did not give serious attention to Kokáma and Omágua ceramics in his work, nor is this problem seized upon by Noelli.

The question put most directly is this: how closely related are the ceramics in the Kokáma-Omágua region to the coastal Tupinambá ceramics? There seems to be a consensus that urns of the former

resemble Marajoara urns, mainly in design, but also, in some measure, in shape. The Napo phase urn depicted in Lathrap (1970:152, Fig. 48), for example, bears some resemblance to the Marajoara urns shown by Meggers (1963:Plate 16: a, c, d) and Roosevelt (1991:46 ff.). Both tend to bulge toward the base, rather than exhibiting the characteristic conoidal shape of the Guaraní urns<sup>18</sup>. This is true also of the nineteenth century “Miracanguera subtradition” pot shown by Myers (1992:152, Figure 2b), which looks strikingly Panoan in shape, and also, in some measure at least, decoration. However, the Panoan shapes would have resulted, according to DeBoer (1990), from Panoan acculturation to the Miracanguera style. This pottery contrasts with the Guaraní and Tupinambá forms depicted by Brochado (1984: Figures 14 and 16).

Brochado, however, has proposed that Tupinambá (but not Guaraní) ceramics can also be identified with Miracanguera, thus creating a linkage with the Kokáma-Omágua. Yet the relationship is by no means obvious. It is important to remember that Brochado himself, according to Noelli (ms.p.11), in 1969 proposed the term “Tupí-Guaraní” to designate a single, integrated ceramic tradition. And, by his own account, Brochado (1984: 312-313) formerly viewed Tupinambá pottery “as derived from, or as an offshoot of, the Guaraní.” How truly distinct can the two be?

Brochado’s radically new 1984 conclusion followed upon his graduate training under Donald Lathrap, the originator of the Amazonian hearth model, and the purveyor, at times, of an almost mystical vision of the Amazon basin as a Garden of Eden. He argues that Tupinambá shapes are a subset of the Marajoara shapes. However, Tupinambá ceramics seem quite distinct from those of the Kokáma and Omágua.

There are differences between Tupinambá and Guaraní ceramics. However, it is not clear how significant those differences are<sup>19</sup> – Brochado (1984: 299-303) spends just four brief pages discussing them. Moreover, the discussion downplays the significance of the similarities,



such as corrugation and shared shapes. And the central empirical question of the stylistic relationship between Kokáma-Omágua, Tupinambá, and Guaraní ceramics goes unexamined.

Lathrap (1970:156) claimed that it was out of the Barrancoid tradition – which he considered to be of Arawakan invention – “that the Guarita tradition evolved without major discontinuities and without major external influences.” Could it have been Arawakans themselves who developed the polychrome tradition originally? One feature present in Marajoara and Kokáma-area archaeological ceramics is modelling. Modelling of animal and human figures is not ethnographically documented for the historically known Tupians, or archaeologically for the Tupinambá or Guaraní. Yet modelling is a feature of historical Arawakan pottery, even into the presentday in the Xingu Park: “Von den Steinen’s statement, so widely commented upon, that the *Arawakan*-speaking tribes were the only ceramists in all the upper Xingú (where Tupi, Jê, Carib, and Arawaks came together) was probably true as recently as 1938” (Lévi-Strauss 1963:332), though more recently Awetí and Kamayurá had modelled hemispherical bowls with animal shapes, “perhaps made by *Arawak* women who married into these tribes” (Lévi-Strauss 1963:333).

Need we equate the polychrome tradition with a single language group and its peregrinations? Tupinambá and Guaraní ceramics styles are simply too discontinuous with Marajoara and Kokáma-area styles to imagine that they must have been produced by a linguistically unified population. We cannot explain the spatial distribution of ceramics exclusively by migration. Diffusion through contacts must additionally have been at work, and the question is: how did the polychrome tradition spread between eastern and southern Brazil and Amazonia – the former appearing (pace Brochado) more similar to one another than either is to the Amazonian tradition?

## 5. Conclusion

I worry that my critical remarks may discourage readers from a careful study of Brochado's major work. Nothing could be further from my intentions. Brochado's work deserves to be more widely known, and Noelli is right to champion it. Updated and with some revision, his master work – a still unpublished Ph.D. dissertation! – should appear in monograph form in both English and Portuguese. Brochado has achieved a major new synthesis of eastern Brazilian materials, and his work should be studied by all specialists in the area.

At the same time, I am not prepared to accept his principal substantive claims, although neither can I at this time definitively reject them. I continue to think that the Tupian stock probably originated in a headwaters area between the Madeira and Xingu. It is more likely that the Tupí-Guaraní family had a várzea origin, but, if so, that origin may have been along a tributary of the Amazon, such as the Xingu, rather than along the banks of the Amazon itself.

The two-pronged hypothesis regarding the Tupinambá and Guaraní is fascinating and wonderfully argued, but, as of now, without direct archaeological substantiation; and it is ethnographically implausible. We need serious research of the kinds I have outlined to even begin to reach a firm conclusion. Without direct evidence, too much devolves upon the linguistic relatedness of Kokáma-Omágua to Tupinambá, and, as I have argued, there are reasons to suspect that Kokáma-Omágua may be the result of a post-Columbian adoption of Tupinambá or *Lingua Geral* by peoples who formerly spoke a wholly different language (possibly Arawakan?), traces of which remain in the non-Tupian portions of the vocabulary and grammar.

Tupian pre-history remains enveloped in fog. However, it is impressive that so much ingenious work has, of late, gone into guessing the shapes of otherwise fog-shrouded objects. Thanks to new

cooperation between archaeologists, linguists, and ethnohistorians, we can now dimly make out the lineaments of the past 5,000 years, even if some of the shapes still lend themselves to multiple interpretations – including the startlingly new spatial configuration of Tupinambá-Guaraní relations proposed by Brochado. Will this new configuration prove, as the fog lifts under the sunlight of additional research, to be the one that is really out there? Or will it be like those fanciful shapes we fool ourselves into seeing when perception leaves too much to the imagination? Even if the latter proves to be the case, Brochado and Noelli will still have made an important contribution to Tupian studies by forcing us to look at old facts in new ways, and, hopefully, by stimulating new empirical research, which is what we truly need.

## Notes

- 1 This is not Brochado's label, but the hypothesis derives from Donald Lathrap's work on the Upper Amazon, and Lathrap (1970:128) explicitly uses the expression "displaced persons," albeit it not for this case.
- 2 Lathrap (1970:78) located "the home of the Proto-Tupí-Guaraní speech community on the south bank of the Amazon slightly down stream from the mouth of the Rio Madeira."
- 3 Lathrap (1970:153-154) used the notion of a "two-pronged migration pattern" to describe the movement of the Kokáma and Omágua into the Napo and Ucayali region of the upper Amazon, but he did not, to my knowledge, use it, as Brochado does, for the Tupinambá and Guaraní expansion into eastern Brazil.
- 4 Becker-Donner's work (1956), which Brochado (1984:331) himself mentioned, is a survey of one section of the right bank of the Guaporé. The survey does not extend up into the Chapada, but neither did it provide evidence of an early Guaraní presence. There may be other investigations already underway, though I have not seen the results. Clark Erickson has

begun an examination on the other side of the Guaporé in Bolivia. P.I. Schmitz (1987) and Irmhild Wurst (1994) have done work to the east of the area. Michael Heckenberger has been undertaking some interesting new archaeological investigation in the Xingu Park, in the headwaters of the Xingu river. None of this research, however, directly focuses on the area in question.

- 5 I am relying, for my comparative understanding of Tupian languages, principally on the work of Aryon Dall’Igna Rodrigues (1958, 1964, 1985a,b, 1986) and Miriam Lemle (1971), as well as that of Soares and Leite (1991) and Priest (1987). I have also examined Dietrich’s (1990) attempt to include grammar into the comparative picture. Major new work on families within the Tupian stock other than Tupí-Guaraní is being undertaken by Denny Moore (Moore and Galucio 1993; Moore in press).
- 6 For this reason, incidentally, I am puzzled by Noelli’s statement (manuscript p. 22): “Urban ao citar que teria havido uma lingua chamada Tupí-Guaraní, fez emergir uma antiga discussão de nomenclatura já resolvida no final da década 40.” Surely, Noelli is mistaken. Rodrigues (1964: 101) himself uses the term “Tupí-Guaraní” for both the family *and the language*.
- 7 I note also that, of the 221 proto-lexical items on Lemle’s (1971) list, Guaraní has 150 cognates (or 64%). Of those 150, 146 or 97% have counterparts in Tupinambá.
- 8 This is just an orthographic difference between Lemle (1971), who uses \*py, and Rodrigues (1985b), who uses \*pj. The same is true for \*c (Lemle) and \*ts (Rodrigues), which represent the same sound.
- 9 Dietrich’s (1990) study is not a new reconstruction. Rather, it is one that looks at grammar in addition to phonology and lexicon. Dietrich is concerned especially with the conservatism versus innovativeness of Tupí-Guaraní languages, and, in this regard, he places Tupinambá among the Guaraní dialects, with Kokáma divergent and innovative.

- 10 The change of *o* into *u* in LGA took place between the 18th and 20th centuries. The change may not have taken place yet in this word, and, indeed, there are many similar examples in the Stradelli dictionary, much of it dating from the 19th century.
- 11 A cognate does appear in Guaraní (see Ruiz de Montoya 1876[1640]:207) – *ebe*.
- 12 There are also a few Quechua words, and one or two Panoan forms, but these seem to have readily identifiable sources. Many Kokáma-speakers are today also fluent in Quechua.
- 13 The Tupinambá form for ‘bee’ as reported in Tatevin (1910:116) is *eirena*. Lemle (1971) did not reconstruct the Proto-Tupí-Guaraní word. The Tupinambá forms given by Lemle for ‘water’, ‘salt’, and ‘wash’ are, respectively, *ĩ*, *yukĩr*, and *yosey*. There is another Tupinambá form for ‘wash’ that may be cognate with the Kokáma word. The *Vocabulário na Língua Brasileira* (Ayrosa 1938:273) gives the word *ajaçuc*, which would be *ayasuk* in contemporary orthography, *suk* being possibly cognate with *tsukuta*. Nevertheless, I’ll include the Kokáma word here. Kokáma distinguishes the verb ‘bathe’: *yatsuka* from the verb ‘wash’: *tsukuta*, the former being obviously cognate with the Tupinamba word. This distinction is apparently not found in Tupinambá. The Tupinambá forms for ‘pain’ and ‘ant’ show similarities to the Kokáma forms, but they do not appear to be cognates according to the derivational rules in Lemle (1971). There is no generic word for ‘ant’ in Tupinambá, but the *Vocabulário na Língua Brasileira* (Ayrosa 1938:241) gives as one species term *ygçaubá*, which would probably be *isauba*, from which one would expect *itsawa* in Kokáma. Similarly, the word for pain is *baêrasig* (Ayrosa 1938:195), the relevant portion of which would be *-rasin̄*, from which one would expect *ratsin*. However, the Kokáma forms for ‘pain’ and ‘ant’ may have resulted from the interaction between LGA and Arawakan languages, as the former spread throughout the Amazon basin. The Kokáma words are from Faust and Pike (1959) and Faust (1972), with exception of the words for ‘ant’ and ‘wash’, which come from Espinosa (1989).

- 14 Because 'wash', 'pain', and 'ant' have possible cognates in LGA, if not in Tupinambá, I consider them at best weak evidence for an Arawakan connection. At worst, they are illusions. The first three, however, 'bee,', 'water', and 'salt', show no resemblance to Tupí-Guaraní forms.
  
- 15 Payne (1991:295) does not list cognates for the western group of Maipuran-Arawakan. The Chamicuro word given by Parker (1994:265) is maʔʂʔito.
  
- 16 Matteson (1965:364) states that the Piro word is "borrowed," "source not known." She does not clarify her reasons for asserting this.
  
- 17 Métraux (1963:688) mentions that the Mission of Santa Maria de Huallaga was at one time in the "charge of Father Raimundo Cruz, who composed a *Cocama* grammar." He does not give a reference, however, and I have been unable to track down this source.
  
- 18 The University of Pennsylvania Museum of Archaeology and Anthropology actually includes some large Marajoara urns that show a Guaraní-like conoidal shape.
  
- 19 I won't pick apart Brochado's contrast, but an example of the problems with it is the following statement: "Ethnohistoric accounts state that most drinking cups [among the Tupinambá, as opposed to the Guaraní] were beautifully painted gourds, not pottery bowls, and there are a few examples of these exquisitely decorated gourds in very early ethnographic collections made along the Central Amazon" (Brochado 1984:302). Brochado here seems to suggest that the use of painted gourds is one way in which the Tupinambá differed from the Guaraní. However, it is ethnographically well-known that the Kaiwá (Guaraní) incised and burned patterns on Gourds (Métraux 1963a: 88), and that "painted, incised, or fire-engraved gourds (*Lagenaria siceraria*) used as cups are, after pottery, the best expressions of Chiriguano art (Métraux 1963b: 477). The Tupinambá and Guaraní may not have been, in this regard, as different as Brochado contends.

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