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THE FISH PEOPLE

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The Fish People

Linguistic Exogamy and Tukanoan Identity in Northwest Amazonia

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For my father and in memory of my mother

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Preface

In November 1968 I arrived in the Vaupés territory of Colombia planning to study beliefs and practices related to disease and curing in a Northwest Amazon tribe. I originally intended to work with the Tikuna, south of the Vaupés, but conversations with anthropologists Gerardo Reichel-Dolmatoff and Alicia Dussan de Reichel after my arrival in Colombia convinced me that the Tikuna were too acculturated for the study I really wanted to do. I turned to the Vaupés region, which had the additional advantage of being the focus of a number of ongoing research projects, including Reichel-Dolmatoff's own work with the Barasana and Desana.

After arriving in Mitú, the airstrip town on the Vaupés River that is the administrative seat of the Vaupés territory, I spent about three weeks making canoe trips and flights in small airplanes to various settlements. Realizing I had to locate myself quite far away from Mitú and the Vaupés River, I then took a missionary plane south to Monfort on the Papurí River and soon after went on a ten-day visit with some Desana Indians to a small, nucleated village on Caño Virarí, which, however, was still too acculturated for my purposes. It became clear during this frustrating but valuable period of orientation that I really did not want to compromise in terms of acculturation level, and I reconciled myself to the inevitability of settling in an extremely isolated community reachable only by a long canoe trip.

During my stay at Monfort I met Samuel, a Desana who spoke Spanish and understood why I wanted to stay in the most traditional settlement possible. Samuel agreed that a longhouse community was the only answer to my requirements and even listed a number of characteristics still applicable to longhouse communities but absent in nucleated villages. He said that later I could live in a mission town, once I understood where Tukanoans had started from in their journey toward the Colombian–Brazilian and Christian worlds. He suggested I live with his Bará in-laws on the Inambú River, assuring me they would be delighted to have me, and I decided to trust this indirect invitation. First, however, I returned to Bogotá to buy such necessary equipment as an outboard motor and gasoline. I then returned to the Vaupés, meeting Samuel in Mitú and

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traveling with him to his wife's parents' longhouse, Púmanaka buro ('hill of many leaves'), where I was to spend the next eighteen months.

The longhouse was the home of about twenty Tukanoans: Bará men, their non-Bará wives, and their children.¹ These people had heard via the Vaupés grapevine that I was coming, and undoubtedly were as anxious about my liking them as I was about their liking me. (They later told me some of the worries they had had during the beginning of our acquaintanceship.) For my part, I desperately hoped things would work out because it seemed as though I had already wasted an inordinate amount of time.

My first two days in the longhouse were a confused jumble of talking to people via Samuel and making attempts to begin learning Bará; a puppy I had acquired in Mitú provided a conversation topic of sorts. Disoriented and anxious, I was also extremely excited, reactions shared by virtually all anthropologists upon entering the field but perhaps heightened by several factors in my situation.² One, I was completely alone. Two, by then I had spent almost three months and was extremely impatient to begin "real" fieldwork. And three, living in a Tukanoan longhouse involves an intensity and exclusivity of contact with one's fellow residents unmatched in the vast majority of residence arrangements found in the world. For periods of a week or more a settlement's residents see only one another. Púmanaka buro, like all longhouses (at least at present), is remote from other settlements; a two-hour canoe trip separates it from its nearest neighbors downstream, a Tuyuka longhouse community. Still, although I felt terribly cut off from virtually everything familiar to me, I adjusted, and over time a very deep attachment formed, both on my part and, I believe, on theirs. The intensity of feeling has been only partly diminished with time.

Over the months I would come to realize just how dependent I was on these people, a dependency that made me euphoric at times and depressed at others. At the beginning, I only dimly realized some of the psychological adjustments I would have to make; these demanded more of me than any other aspect of fieldwork, far more than physical discomfort. It is one thing to learn to cope with new routines, to eat strange foods, to survive wasp massacres and fungal invasions. Much of it, in fact, I enjoyed, and the challenge posed by the rest suited my romantic side very nicely. But my feelings of loneliness and incompetence were hard to deal with, heightened as they were by my belief that anthropologists should not become "too involved." I tried to remain objective, not to take sides in arguments, and in general to monitor my behavior so as to avoid permanently alienating people. Straddling the fence of emotional involvement and expression (saying and feeling neither too little nor too much) was perhaps the most difficult of requirements in a setting like the longhouse, because these people, how they felt about me and I about them, affected my life and work in the most profound way. Of course I failed to keep my supposed objectivity. I did take sides in disputes, and I have come to accept the inevitability of this

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involvement far more than I did then. Certainly at times my role as anthropologist conflicted with that of coresident, but I believe that in general whatever value my research has derives to a considerable extent from the relationships I shared with my fellow members of Púmanaka buro. I am *not* making the obvious observation that the longer one lives and interacts with a group of people, the better one understands them. True as that is, I mean to suggest here that a process of emotional attachment must occur in order really to understand a culture but that at times it works against one's desire to be detached, objective, scientific. The best emotional stance to take when carrying out fieldwork must, of course, be an individual decision, but to believe in one's total objectivity and detachment is naive and self-deceptive: A great deal can be gained in crossing emotional boundaries along with the cultural ones.

These rather lengthy remarks are intended not only to express my deep feelings of appreciation toward the people of Púmanaka buro but also to call attention to the fact that I did spend much of my time with one particular group of people in a rather isolated location, a fact that bears directly on the avowedly regional perspective taken by this book in portraying Tukanoan social identity. How I came to reorient my research plan from a local to a regional perspective is important information for a number of reasons, and thus a discussion of it follows.

Soon after arriving at Púmanaka buro I began a study of the Bará language, in preparation for carrying out the cognitive ethnosemantic part of my research. It quickly became apparent that the proposed research could not be done, because it depended on an implicit research assumption that the members of the subject group all spoke the same language. I found myself in a region with more than sixteen languages, several of them represented in each longhouse. Although I had been aware of this fact before, its implications did not strike me until I was in the field. The rule of exogamy required that all inmarrying women at a settlement be from other language groups. Every Tukanoan I talked to was at least trilingual, and children began acquiring two languages almost from the beginning of language learning. Most conversation was in Bará, but the women with whom I spent much of my time also spoke in Tuyuka and occasionally in Tukano ("just for a change").

Furthermore, the same problem arose when I contemplated research on decision making about disease and curing and the degree of fit between normative pronouncements and actual behavior. Other settlements of the region were at least half an hour by canoe from each other. Worse, there were no other Bará longhouses on the river or in the immediate vicinity of Púmanaka buro, all its neighbors being Tuyuka, Tukano, and Desana. How could I, then, by working with one language, claim to say anything about how these people think about and classify disease terms and behaviors, or anything else, for that matter? This situation, which fortunately confronts few fieldworkers, seemed to discourage every possible avenue of research until I decided to capitalize on it and begin an

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investigation of the social structure of Tukanoans, in particular those of the Papurí drainage region, concentrating on the relation between multilingualism, kinship, and marriage.

If I had begun with a well-thought-out research design, complete with hypotheses to be tested and so forth, my fieldwork would have conformed to current standards more closely than it did. My progress toward understanding the various linguistic and marital mysteries of the Vaupés has in fact been much more serendipitous, full of dead ends and interesting side paths that, unfortunately, incomplete data have not allowed me to explore thoroughly. I offer these remarks to explain some of the gaps in the data; I simply was not aware of the complexity and reach of the system while researching it. The conclusions given here have evolved during a ten-year period and are the result of many struggles with the data and many conversations with fellow Vaupés specialists and other anthropologists.

Although I spent much of the field period at a single site, I traveled frequently; sometimes I accompanied the residents of Púmanaka buro when they went to rituals at other settlements but more often I traveled in order to get in and out of the region. The canoe trip from Púmanaka buro to Mitú never took less than six days, and one memorable trip during the dry season took ten days. As I came and went I changed boat crews frequently and slept at a different settlement each night. I gathered as much information as I could at each stop, especially on demography and settlement locations; by the end of my fieldwork, when I had grasped at least in part the extent of the marriage system, I came to see this information as extremely valuable. In every settlement some of the unmarried affines came from distant places, making it possible to find out about communities well off my route. Furthermore, Tukanoans displayed a great interest in the physical and social geography of the Vaupés, and they seemed to see it as a single region. Their interest in answering my questions or finding others to answer them gradually convinced me that I was investigating a true cultural focus, equivalent to the interest of highland Chiapas Indians in corn or Dobuans in sorcery.

Thus, I eventually broadened my study as far as possible, because neither I nor Tukanoans could see any clear-cut boundaries between Tukanoans and "other people." Even my goal of a complete census of marriages of living individuals of the upper and mid-Papurí and its tributaries, which I eventually achieved, did not mark off a socially bounded unit. During my trips I drew sketch maps, which, crude though they were, demonstrated in combination with the census data the extent to which marriage unified subsections of and, less so, the entire Vaupés region. Thus those canoe trips that I often dreaded, greatly envying other anthropologists who were able to fly into and out of their sites, proved to be the source of much essential data.

It must be borne in mind that my research strategy evolved during fieldwork and that I spent most of my time in a single longhouse community, circumstances

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that have led to my taking the Bará language group and, more specifically, the residents of Púmanaka buro as representative of Tukanoan society. Although I have been careful throughout the book to point out all the discrepancies and exceptions of which I am aware, it should be remembered that in general we shall be looking at Tukanoan society through the spectacles of the Bará living in the Papurí drainage region. My approach has also necessitated striking a balance between ethnographic specificity and systemic overview. The balance here seems to me a good one, but because the work deals with both local and regional data I have had to make many decisions regarding the amount of material to include from either perspective. I obviously cannot, when describing an open-ended system like the Vaupés, describe and analyze how every person, every local group, every language-affiliated group differs from and resembles every other one. A serious attempt to combine both a regional overview with a comprehensive ethnography of every group encompassed within the overview would require more years of research and more volumes than even the most forgiving granting agency or publisher could conceivably tolerate.

Thus, this book represents a point – I hope a high but not a final one – in a long process of investigation and analysis of Tukanoan social identity. I hope to return to the Vaupés and continue research; indeed, this monograph was completed before another sojourn only when it became evident that one planned for 1976 would be impossible.³ Many of my intentions for future research will become apparent in the following pages. I am confident in the essential accuracy of my portrayal of the form of the Vaupés social system; by adding content, admittedly largely from Bará/Papurí sources, I believe I have sketched a reasonable portrait. It is hoped that this book will inspire others to comment on the picture I have painted or attempt their own interpretation of this fascinating system and the people who have created it.

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Juanico, María, Lois Paul, Michelle Rosaldo, and my mother, Mary Elizabeth Gaines Jackson, have all died since the research began. I regret they cannot see its final form, because they were all instrumental – in very different ways – in bringing it about.

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The orthography used in this book is a phonetic transcription of Bará, simplified to make it accessible to and convenient for English-speaking readers. The kinds of simplifications chosen do not confuse the particular lexical items dealt with in the text, that is, they would not reduce phonetically distinct forms to homonyms. For a more comprehensive treatment of Bará (as spoken in the Pirá-paraná, a separate dialect from Inambú Bará), see Stolte and Stolte, 1971. For example, tone (Bará has two) is not indicated in my transcription, and stress has been simplified (high tone and stress co-occur): Stress falls on the penultimate syllable unless shown elsewhere. Aspiration is not phonemic; I have indicated preaspiration where it occurs with the voiceless stops /p/, /t/, and /k/ as an aid to pronunciation (e.g., *mehkó*, ‘father’s sister’).

Vowels

<i>Unnasalized</i>		<i>Nasalized</i>	
a	as in <i>father</i>	ã	
e	as in <i>eight</i>	ẽ	
i	as in <i>pediatrics</i>	ĩ	
o	as in <i>oval</i>	õ	
u	as in <i>food</i>	ũ	
ü	similar to German ü	ü	

Consonants

p	as in <i>put</i>	ñ	as in <i>onion</i>
b	as in <i>but</i>	m	as in <i>man</i>
t	as in <i>tub</i>	r	alveolar flap, as in Spanish <i>pero</i> (between <i>r</i> and <i>l</i> in English)
d	as in <i>dub</i>	w	as in <i>water</i>
k	as in <i>kit</i>	y	as in <i>yawn</i>
g	as in <i>got</i>	h	as in <i>hat</i>
n	as in <i>not</i>	ng	as in <i>sing</i>

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In order to distinguish the linguistic stocks from which cited forms derive, I have used *italics* at the first occurrence of Bará and other Eastern Tukanoan words (as well as for scientific names, emphasis, and metalinguistic references) and **bold face** for the first occurrence of Spanish, Portuguese, and Tupian loan words.