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ETHNOBIOLOGY IN FOUR PHASES

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ABSTRACT.—I recognize four phases of ethnobiology: I, II, III, and IV. Ethnobiology I begins well before the formal naming of ethnobiology as a scholarly endeavor at the end of the 19th century. This initial phase has been widely characterized, albeit over simply, as essentially utilitarian. Ethnobiology II was elaborated in the cognitive/linguistic anthropology of the 1960s. Ethnobiology III integrates knowledge with practice, stressing the ecological consequences of knowledge applied to make a living. Ethnobiology IV emphasizes the rights of indigenous peoples to control their traditional knowledge. I elaborate this framework here and consider how these diverse perspectives might be integrated more effectively in the future.

Key words: ethnobiology, history of ethnobiology, cognitive anthropology, ethnoecology, indigenous rights.

RESUMEN.—Se pueden reconocer cuatro etapas dentro de la etnobiología: I, II, III, y IV. La fase etnobiología I comenzó mucho antes de que se acuñara el término etnobiología a finales del siglo 19. Esta etapa inicial se caracterizó por su enfoque simple y esencialmente utilitario. La segunda fase, etnobiología II, se desarrolló durante los años sesenta del siglo veinte bajo la influencia de la antropología cognitiva/lingüística. Posteriormente, la etnobiología III se interesó por la integración de conocimientos y prácticas, haciendo hincapié en las consecuencias ecológicas que tiene la aplicación de los conocimientos para la subsistencia. Por último, la fase etnobiología IV pone énfasis en el derecho que tienen los pueblos indígenas de controlar sus propios conocimientos tradicionales. El artículo desarrolla estas ideas y analiza la manera de integrar estas perspectivas más efectivamente en el futuro.

RÉSUMÉ.—Je reconnais en ethnobiologie quatre phases: I, II, III, et IV. L'ethnobiologie I commence bien avant l'attribution formelle des noms et tire ses racines de l'ethnobiologie en tant qu'effort scientifique de la fin du 19e siècle. Cette phase initiale a été largement décrite, souvent de façon trop simple, comme étant essentiellement utilitaire. L'ethnobiologie II a été élaborée pendant la période d'anthropologie cognitive et linguistique des années 1960. L'ethnobiologie III intègre savoir et pratique tout en soulignant les conséquences écologiques des savoirs lorsque ceux-ci sont utilisés au sein des activités économiques. L'ethnobiologie IV souligne les droits des peuples indigènes à contrôler leur propre savoir traditionnel. Dans cet article, je présente ce schéma et discute de la façon dont les différentes perspectives pourraient être mieux intégrées à l'avenir.

INTRODUCTION

I have been a practicing ethnobiologist for 35 years and trust I will continue to pursue the many remaining ethnobiological mysteries for some years yet. I have witnessed the field evolve through several significant transitions. As a green graduate student, I saw ethnobiology emerge in the early 1970s as a favorite, if esoteric, domain of ethnoscientific theoretical analysis. I joined colleagues in the early 1980s in defense of indigenous knowledge and rights in land under the rubric of Traditional Ecological Knowledge. I watched bemused as ethnobiology captured the public imagination in the 1990s as a heroic search for miracle cures amongst the vanishing cultures of the tropical rain forests. Finally, at the close of the 20th century, I shuddered as firebrand preachers of a postmodern morality castigated ethnobiology as bordering on biopiracy. I would like to pause here to reflect on these changes with an eye to the future of our ethnobiological project.

First, I will review and elaborate somewhat upon historical periodizations recognized by colleagues who have contemplated the history of the ethnobiological enterprise. This review will be quite selective, certainly less than comprehensive. It has been standard to recognize a two-fold division between ethnobiology before and after Conklin, that is, from an essentially descriptive approach in the half-century since Harshberger coined the term ''ethnobotany'' (1896) to a self-consciously comparative and theoretical stance inspired by Conklin's exemplary study of Hanunóo botanical nomenclature and classification (1954). Conklin delineated basic lexeme types in Hanunóo plant names and sketched the outlines of taxonomic structures that—subsequently elaborated by Bulmer and Berlin—now provide a consistent, if controversial, analytic framework for a comparative science of ethnobiology (Berlin 1992; cf. Ellen 1986).

This historical watershed has been characterized as pitting an initial utilitarian bias in which ethnobiology was dedicated to the discovery of useful plant and animal products—an approach institutionalized in the Society for and journal *Economic Botany*—against an abstract intellectual emphasis on ethnobiological knowledge valued for its own sake. Claude Lévi-Strauss (1966) powerfully enunciated this in his critique of Malinowski's utilitarian functionalism, the notion that, in Malinowski's words, ''The road from the wilderness to the savage's belly and consequently to his mind is very short. For him the world is an indiscriminate background against which there stands out the useful, primarily the edible, species of animals and plants'' (Malinowski 1974:44).

However, a careful reading of early ethnobiological programmatic proposals and ethnographic accounts—such as Castetter (1944), Jones (1941), and Robbins et al. (1916)—shows that these early students of ethnobiology appreciated indigenous knowledge of plants and animals from diverse angles. They included in their prescriptions and ethnographies much of linguistic, symbolic, ecological, political, and aesthetic interest. Castetter argued emphatically that ethnobiology was distinct from the newly coined field of economic botany (Vestal and Schultes 1939; cf. Robbins et al. 1916) by virtue of the fact that ethnobotany, as opposed to economic botany:

> consisted of far more than collecting, identifying and ascertaining the uses of plants by primitive peoples; that it must strike more deeply into

> the thought and life of the peoples studied, gaining from them their impressions of total environment involving such matters as primitive concepts of plant life; the effect of a given plant environment on the lives, customs, thoughts and everyday practical affairs of the people studied; and the extent of the people's knowledge of plant parts, functions and activities (Castetter 1944:159).

Moreover, the intellectualist emphasis that takes center stage in ethnoscientific analyses in the Berlinian style is now routinely complemented by detailed ecological and historical material and analysis, as it was in Berlin's own work (e.g., Berlin et al. 1974).

Clément stretched this dichotomy of early versus late, ideographic versus nomothetic, utilitarian versus intellectual ethnobiology to include a third stage. He termed his three stages ''pre-classical,'' ''classical,'' and ''post-classical'' ethnobiology, borrowing the Mediterranean periodization from Murray's historical analysis of the rise and fall of ''classical ethnoscience'' (1983). Clément characterized pre-classical ethnobiology as essentially *etic*, that is, dominated by the perspective and interests of the scholar, which were often utilitarian, a search for plant or animal products that might have economic value and thus justify the funding provided for the research. Classical ethnobiology pursued indigenous knowledge as a means to understand how humans make sense of their living environment, an explicitly *emic* perspective inspired by Conklin and elaborated in the 1960s as ethnoscience, subsequently more generally known as cognitive anthropology (Sturtevant 1964).

Clément linked the pre-classic etic orientation with a pervasive ethnocentrism, apparent in much of the ethnobiological literature of the late 19th century. Our ancestors of this period might have granted that ''primitive peoples'' occasionally, whether by accident or out of dire necessity, stumble on some useful ethnobiological insight, which civilized scientists might profitably take note of. By contrast, classical ethnobiologists recognized indigenous knowledge as an exemplary manifestation of a universal scientific inclination.

Clément characterized post-classic ethnobiology as involving the ''emergence of marked cooperation between Western scientific researchers and Native peoples.'' We may see this as a natural development of classical ethnobiology, since, if we truly value indigenous knowledge as science, should we not also value the indigenous scientist as a colleague, not simply as a subject of our own empirical investigations and theoretical ruminations?

I would like to see Clément and raise him one by recognizing *four* phases in the history of ethnobiology.

ETHNOBIOLOGY IN FOUR PHASES

Ethnobiology I: First Steps.—From its inception as a formally named academic specialty, beginning with ''ethnobotany'' in 1895 and ''ethnobiology'' in 1935 (or as a recognizable scholarly activity, at least as early as the 16th century studies of Aztec ethnobiology by Sahagún, de la Cruz, and Hernández) until the 1950s. The focus of Ethnobiology I is to document plant (or occasionally animal) uses,

particularly uses that might prove profitable to the ''Western'' scientist and his readers. Nevertheless, some excellent, careful work was done in this spirit.

Ethnobiology II: Cognitive Ethnobiology, a.k.a. ''Ethnoscience.''—Attributed first to Harold C. Conklin, Yale University anthropologist, whose 1954 dissertation on ''The Relation of Hanunóo Culture to the Plant World'' set a very high standard for subsequent ethnobiological research, especially with respect to the meticulous documentation of the ''emic'' or ''indigenous'' perspective by careful attention to local linguistic usage (e.g., names, descriptive conventions, etc.). Ralph N.H. Bulmer and Brent Berlin deserve credit for defining the major theoretical issues of Ethnobiology II. The central focus of Ethnobiology II is ''cognitive,'' with strong links to cognitive psychology and linguistics.

Ethnobiology III: Ethnoecology.—An ecological focus developed during the 1970s and 1980s out of widely expressed discomfort with the limitations of the perspectives of Ethnobiology I and II due to, in particular, the lack of emphasis on the broader ecological context of folk biological knowledge. Victor Toledo, a Mexican anthropologist and ecologist, has promoted the concept of ''ethnoecology,'' editing an occasional journal *Etnoecología* (Toledo 1992). The key issues for Ethnobiology III are the links between knowledge and action, in particular, with respect to resource/habitat management. The concept of TEK (Traditional Ecological/Environmental Knowledge) and its cousins, IK (Indigenous Knowledge), TKW (Traditional Knowledge and Wisdom), and LEK (Local Ecological/Environmental Knowledge), encompass this broader perspective, not being limited to an inventory of named plants and animals and their uses, but investigating also knowledge of soils, climates, plant and animal communities, successional stages, etc.

Ethnobiology IV: Indigenous Ethnobiology.—During the 1990s the study of ethnobiology (of whatever type) has come increasingly to be seen as linked to the exploitation of indigenous communities by global powers, whose agents the ethnobiological researchers are presumed to be, wittingly or unwittingly. ''Knowledge'' is increasingly defined as an instrument of ''power,'' following the philosophical lead of, to name just the most prominent example, Michel Foucault. Thus documenting TEK in the public domain is interpreted as ''stealing'' the intellectual property of indigenous or local communities. From this perspective, the only legitimate ethnobiology is that conducted by and for the community. Darrell Posey, founder of the International Society of Ethnobiology in 1988, crafted a powerful statement—''The Declaration of Belen''—of the moral responsibility of ethnobiologists to the peoples we study, often marginalized indigenous communities struggling to hold on to their ancestral lands and identities. I will briefly consider the implications of this development for the future of ethnobiology.

ETHNOBIOLOGY I

Ethnobiology I is the equivalent of the pre-modern or pre-classical ethnobiology of others, with the caveat that it was neither purely utilitarian

nor thoroughly ethnocentric. It was most often etic, as Clément has argued, but in a pre-Pikean era before the emic-etic distinction was clearly enunciated. I believe the motives and perspectives of this earlier generation of ethnobiologists were complex, though they no doubt lacked our present appreciation of the theoretical potential of cross-cultural comparisons.

Clément begins his account of ''The Historical Foundation of Ethnobiology'' (1998) in the waning decades of the 19th century, though with a nod to the great antiquity of human interest in learning from other peoples new ways to see and to interact with their living environment. I prefer to start three centuries earlier, taking as a prototype of early ethnobiology the Franciscan friar Bernardino de Sahagún, who organized a collaborative ethnographic project with students at the first New World college, the Colegio de la Santa Cruz at Tlatelolco—students whose parents were Aztec nobles and witnesses to the destruction by the Spanish conquistadors and their Native allies of their ancient world (Ortiz de Montellano 1990:16–29). These students mastered the art of writing Latin, Spanish, and Nahuatl using the Latinate orthography the friars taught. Under Sahagún's direction they recorded in Nahuatl, often with lively illustrations, the testimony of elders on a wide range of cultural matters, including not least of all extensive essays on the knowledge and use of plants and animals for medicine, food, artesanal production, and ritual (cf. Book 11, *Historia General de las Cosas de la Nueva España*; the Nahuatl version with modern English translation is known as the Florentine Codex [1950–1969]). Contemporaneously, a Spanish doctor, Francisco Hernández, dispatched to the New World by King Philip II of Spain to document local medical knowledge—generally regarded in Europe at that time as in many ways superior to their own medical practice, at least with regard to the Mexican flora—and an Aztec scholar, Martín de la Cruz, recorded in detail Aztec knowledge and use of curative plants. The motives of these ethnographers—and I believe that is an appropriate title—were in part utilitarian, especially in Hernández's case, although Sahagún's motives were clearly far more complex.

Sahagún pursued his life's work under the threat of the Inquisition. The justification for permanently recording the beliefs and practices of people judged to have fallen under the evil influence of Satan was deeply suspect to his superiors. Meanwhile, his colleague, Diego de Landa, was busy burning every Mayan book he could find. Sahagún defended his work by arguing that a meticulously accurate record of local knowledge and belief was essential to effectively eradicate beliefs inspired by the forces of the anti-Christ, providing those entrusted with the enlightenment of the heathen an effective understanding of the enemy. However, the meticulous care and immense effort apparent in Sahagún's intellectual collaboration belies any simple utilitarian or evangelical rationale. Sahagún came to deeply respect the knowledge of his Aztec interlocutors. He thus developed a quite modern ethnographic sensibility, for which the primary motivation is to understand and thus more fully appreciate our common humanity (Todorov 1999).

I have discussed the Mexican case in detail as it is the colonial encounter most directly relevant to my own fieldwork with Native American peoples. However, it is certain that similar intellectual exchanges of ''traditional

environmental knowledge'' have characterized cultural encounters throughout human history. No cultural tradition has emerged *sui generis*; all are derivative and hybrid. The ''Columbian Exchange'' (Crosby 1972), nevertheless, may qualify as the most revolutionary cultural encounter in human history.

In short, it would be unfortunate if we were to dismiss the admirable efforts of our predecessors on the grounds that they lacked a fully contemporary appreciation of the theoretical potential and moral complexity of the ethnobiological enterprise.

ETHNOBIOLOGY II & III

My Ethnobiological phases II and III are equivalent to Clément's classical ethnobiology. However, I divide Clément's classical period in two to recognize that the elaboration of Berlin's general principles of folk biological classification and nomenclature (Berlin et al. 1974) inspired criticism and reaction from those who faulted Berlin's perceptual/taxonomic theory for dismissing the practical, social, and broader cultural and ecological context in which folk biological knowledge is embedded (Balée 1994:1; Ellen 1982, 1986; Randall 1976). I myself suggested that Berlin forced our data to lie in the ''procrustean bed'' of formal taxonomy (Hunn 1982), which caused some lingering hard feelings.

In the late 1970s resistance to reducing ethnobiological analysis to terms and taxonomies grew. That Berlin's principles, warts and all, remain a signal theoretical achievement of ethnobiology and provide the standard analytical vocabulary for comparative analysis cannot be denied. Furthermore, a detailed descriptive account of local nomenclature and classification provides an essential foundation from which to move on to address questions of wider interest and more pressing relevance, questions about how this foundation of systematic knowledge of plants and animals is put to effective use by real people living real lives, not only in the past but in the present and future: *ethnoecology*, in a word.

In the early 1980s interest in environmental issues derivative of cultural ecology and concern for the survival of indigenous communities joined forces. In 1984 a Working Group on Traditional Ecological Knowledge was inaugurated by Gregory Baines under the auspices of the International Conservation Union at the United Nations (Williams and Baines 1993), while nearly simultaneously Michael Warren and his colleagues at CIKARD (Center for Indigenous Knowledge for Agricultural and Rural Development) initiated a network of Indigenous Knowledge Resource Centers around the world (Berkes 1999:18–19). Both initiatives valorized indigenous environmental knowledge as offering sophisticated alternatives to the ecological destruction of capitalist development paths.

Before I proceed I must issue a disclaimer. While some of my good friends are paleoethnobotanists and zooarchaeologists, my musings here are limited by my experience as an ethnographer. I thus describe ethnobiology as an ethnographic project, saying little or nothing about the parallel history of paleoethnobiology, the work of my archaeologist colleagues. I believe what sets paleoethnobiology apart is methodological, the fact that archaeological inferences about ''the dynamic relationships among peoples, biota, and environments'' (Salick 2003:1) —a definition of ethnobiology adopted at the NSF Biocomplexity

workshop on ''Intellectual Imperatives in Ethnobiology'' held at the Missouri Botanical Garden in April 2002—must be inferred from ''the 'black burned bits,'… the remains of yesterday's dinners, discarded fuel wood, collapsed building timbers, clothing, tools, and ritual and medicinal needs'' (Adams 2001:49). Paleoethnobiology has made great strides in recent decades by developing creative and highly systematic methods of preservation of both the macroscopic and microscopic traces of past human activities, analyzing them within an increasingly sophisticated ecological theoretical framework. Paleoethnobiology and ethnographic ethnobiology converged strongly in the 1970s, as questions concerning the past, present, and future of the ''human footprint'' on the environment took center stage (Fowler 2001; Minnis 2001).

This marriage of environmentalism and indigenous activism had scarcely been consummated when the fundamental premise of indigenous ecological wisdom was attacked by a political ecology that viewed ''nature'' as a modernist hegemonic ploy (Escobar 1999) and the ''Ecological Indian'' as a pernicious myth (Krech 1999) exploited by neo-colonial environmentalists intent upon co-opting indigenous peoples while commandeering their traditional homelands for the preservation of a non-existent ''pristine'' wilderness. Undeterred, ethnoecologists continued to pursue carefully documented analyses of traditional resource management and continue to provide sensitive accounts of how indigenous communities are adapting to contemporary realities while maintaining a core of traditional subsistence practice (Berkes 1999).

ETHNOBIOLOGY IV

Our ethnographic mission to record for posterity the rainbow diversity of human ways of life, duly published in the ''public domain'' for all to see, appreciate, understand, and admire, turned sour before the critical gaze of those who, following Foucault, exposed knowledge as ''power,'' and thus the pursuit thereof as morally suspect. Indigenous activists became increasingly wary of the consequences of freely sharing their understanding of the world with outsiders.

I believe we cannot go back to an earlier era when as academic scholars and scientists we felt free to indulge our intellectual curiosity however and wherever we pleased. We have no right to demand that others share their knowledge with us, not even in the interest of human enlightenment. Rather our science and our scholarship—involving as it does human subjects—must be truly collaborative. However, I have argued elsewhere that I believe such intellectual collaboration between indigenous or traditional communities and academic scientists and scholars should be of substantial mutual benefit (Hunn 2002).

Ethnobiologists have been notable pioneers in collaborative research and publication. Ian Saem Majnep and Ralph Bulmer jointly published *Birds of My Kalam Country* (1977), in which Saem Majnep, Bulmer's long time Kalam colleague, dictated text in his native language describing each bird species of his home turf, while Bulmer wrote parallel passages commenting on Saem Majnep's Kalam accounts from the perspective of a professional ethnobiologist, some years before such innovative ethnographic writing became a *cause celébre* for critical anthropology (Clifford and Marcus 1986).

With all due respect to Darryll Posey (1990), I believe we may have harmed our cause by exaggerating the dollar value of TEK. While some few highly profitable drugs may be confidently attributed to indigenous inspiration, such cases are in fact rare and intellectual attribution often complex and ambiguous (Brush 1996). Efforts to protect indigenous knowledge as ''intellectual property,'' in order to demand that multinational corporations share profits with indigenous communities or to devise legal restrictions to prevent the unauthorized appropriation of indigenous knowledge, may have the unintended consequence of defining indigenous knowledge as a commodity. I believe this belies the nature and obscures the true value of indigenous knowledge. I have argued elsewhere (1999, 2002) for the view that Traditional Environmental Knowledge is primarily of value as a living tradition, adapting to changing realities while sustaining a people in their distinctive way of life. Certainly we should decry the unprincipled exploitation of indigenous knowledge, but we may assist threatened indigenous communities most effectively, I believe, by supporting their claims to land and control of subsistence resources while providing our audience carefully documented ethnographic analyses of TEK in action.

CONCLUSIONS

To conclude I would like to offer a revisionist comment on Castetter's definition of ethnobiology as the study of the role of plants and animals in the lives of ''primitive peoples.'' The truth of the matter is that ethnobiological accounts are particularly telling and exceptionally rich to the extent that they describe the knowledge and livelihoods of people living ''close to nature.'' By way of contrast, Terence Hays has quoted ''that Everyman, Alexander Portnoy,'' as follows:

> Greenery I leave to the birds and the bees, they have their worries, I have mine. At home who knows the name of what grows from the pavement at the front of our house? It's a tree—and that's it…. The kind [of tree] is of no consequence, who cares what kind, just as long as it doesn't fall down on your head (Roth 1971:251).

This sketch of modern urban perspectives on biodiversity, unfortunately, is not that broad a caricature, if your have ever questioned undergraduate anthropology students on their knowledge of their local flora and fauna (cf. Dougherty 1979). One could make a case, in fact, that the detailed environmental knowledge so characteristic of the pre-modern world is largely irrelevant for urban youth, whose natural environment is the shopping mall and whose successful adaptation to contemporary realities depends far more on a mastery of *commodity diversity* than of biodiversity.

There is a great historical divide between communities rooted in the land and those caught up in our contemporary global flows of capital, labor, and commodities (cf. Balée 1994:164–165). Traditional Environmental Knowledge is special because it grows out of a radically different relationship between people, their communities, their labor, and their land, than is characteristic of modern societies (cf. Marx 1964). Perhaps it is true that ethnobiology is doomed to dwell

nostalgically on what has been lost. Perhaps for that reason ethnobiology has remained resolutely marginalized by the emerging fascination with political ecology among the present generation of environmental anthropology students. Thus, perhaps it is ethnobiology's mission to document ''primitive peoples'' in their tight environmental embrace while there is still time, but only if we understand ''primitive'' as neither a pejorative epithet for the resolutely backward nor our fascination with ''primitive peoples'' as implying a naïve romanticism for a vanished past, but to the contrary as indicative of our fierce commitment to resist the final triumph of global capitalism.

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