Introduction

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This introduction to the volume argues for the central and integrating role of the subject matter of ethnobiological research in anthropology understood in its widest sense: in its biological, archaeo-historical, and socio-cultural dimensions. The background and current status of ethnobiology are assessed, and its contribution to anthropological issues considered under the following headings: the foundational paradigm of taxonomic orthodoxy; language and the translation of knowledge systems; cognition and culture; the social organization and transmission of knowledge; medical ethnobiology; the applied practice of ethnobiology; and – the meta-theory which binds all this together – the co-evolutionary paradigm as part of a wider 'biocultural synthesis'. The way in which the collected papers exemplify these themes is discussed.

Background

This volume explores the contribution of recent work in ethnobiology[1] to anthropological insights in the widest sense. As a project, it arises from the observation that, increasingly, the subject matter and methodologies of ethnobiological research address core questions about the character of culture, language, cognition, knowledge, and human subsistence, and how these interact through, for example, long-term processes of co-evolution. We seek to provide here some kind of qualitative test of the assertion that ethnobiology stands at an important intellectual junction between biology, culture, and sociality; a view which the authors of this collection share, but which is not necessarily or always apparent in the practice of individual exponents. That ethnobiology has not always been seen to occupy such a critical position also needs explaining. In order to meet such a requirement, this introduction supplies some historical background to developments that have taken place over the last fifty years or thereabouts. We are, however, mostly concerned with current work, and indeed with the prospects for future research in this field of study. We offer, therefore, a retrospective certainly, but also and most importantly, a tentative prospective.

Richard Ford (1978) once memorably said that ethnobotany – but we can extend the point to ethnobiology as a whole – represented a common discourse but lacked a unifying theory. Since that observation was made things have changed. Certainly, ethnobotany, at least, is now replete with methods manuals (Alexiades 1996; Martin

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1995), readers (Minnis 2000), and textbooks (Cotton 1996), as well as taught programmes around the world. While methods, textbooks, and courses are not in themselves evidence of theory, they do imply the attainment of a critical level of agreement as to the issues which ethnobiology should address, which must surely be a precondition for the development of theory. And there are some excellent recent overviews, such as those of Miguel Alexiades (2003), Gary Martin (2002), Daniel Clément (1998*a*; 1998*b*), Doug Medin and Scott Atran (1999), Stanford Zent (n.d.), and others, which have identified a degree of coherence and a defined trajectory over time which those in the pioneer phase might now find surprising. It is certainly a more visible subject. Whether there is now something which we might describe as distinctive ethnobiological theory is another question, but the subject is certainly more theorized, from a number of perspectives.

The history of ethnobiology as a discourse has been documented elsewhere (Brown 1984: 2; Bulmer 1975; Clément 1998*a*; Ellen 1986; Hays 1974; Porteres 1977; Sturtevant 1964; Zent n.d.). In histories and general accounts of both ethnobiology and ethnobotany it has become conventional to distinguish two dimensions, orientations or levels (Bulmer 1975: 10), or phases (Hays 1974: 100-3), of inquiry (also Davis 1995: 43; Ellen 1996: 457-8). The first phase was typified by the determination of culturally, or more specifically economically, significant species and their assessment in terms of 'the objective biological dimension'. At its earliest and most rudimentary, this comprised the listing of names and uses of plants and animals in native non-Western or 'traditional' populations, often in the context of salvage ethnography; or, more crudely put, *ethno*-biology as the descriptive biological knowledge of 'primitive' peoples (e.g. Castetter 1944; Harrington 1947; Stevenson 1915). In the second phase or dimension, which emerged historically and logically from the first, the focus was on the study of human conceptualization and classification of the natural world, a development in retrospect, and iconically, marked by the appearance of Harold Conklin's doctorate in 1954. But in both of its senses, and for most of its history, ethnobiology has usually been a secondary adjunct to other studies, and the importance of such a service should not be understated. In this ancillary role the distinction between the two orientations is perhaps understandable, even necessary, but increasingly these have become aspects of a single problematic. Thus, in the British tradition, anthropological ethnobiology (meaning largely, but not exclusively, studies of folk biological classification) acquired what legitimation it has in its liberation by Claude Lévi-Strauss (1966) from being merely a branch of linguistics and folklore (Bulmer 1975: 10), but it has subsequently become much more than this. In France, ethnobiology needed no such legitimation, with independent roots in the linguistic work of André Haudricourt (e.g. 1973; Haudricourt & Hédin 1987 [1943]) and in a vigorous local tradition of economic biology (Clément 1998*a*; 1998*b*; Porteres 1977).

Today, ethnobiology is, first and foremost, the study of how people of all, and of any, cultural tradition interpret, conceptualize, represent, cope with, utilize, and generally manage their knowledge of those domains of environmental experience which encompass living organisms, and whose scientific study we demarcate as botany, zoology, and ecology. (For an alternative definition see Clément 1998*a*: 19.) But ethnobiology – like anthropology more broadly – seeks to go beyond the local, to compare such knowledge and its consequences between different human populations, and to establish generalizations that are valid at the regional, global, and species level. In order to do this it must inevitably translate into the special-purpose

categories, language, and intellectual issues of global (no longer purely Occidental) scholarship.

Ethnobiology is now as much analytic as it is descriptive, and has begun to develop conspicuously its own theory. Like ethnography, it has made a virtue of its practice, and like social anthropology, it is as much defined by its methods as by its theory. Areas where ethnobiological methods and theory have become particularly distinctive include: resource pool approaches; quantitative plot studies; the links between biological and cultural diversity (including agrobiodiversity); the historicizing of global biological exchange; resource sustainability; the problematizing of non-timber forest product issues; knowledge transmission and erosion; valuation theory; the comparative study of the bioactivity of useful species in relation to taxonomy and cultural convergence; the concept of ethnobiological keystone species – to say nothing of all that theory which came with the examination of ethnobiological classification, and which Brent Berlin has done so much to advance. Despite all this, ethnobiology has conventionally been seen by its practitioners as only an interstitial subject, and has been so perceived by outsiders: peripheral, interdisciplinary, and derivative, an importer rather than an exporter of ideas and techniques. The present volume seeks to explore the contention that, in fact, there has been an equally significant counter-flow of ideas and practices from ethnobiology, and is concerned with that outflow of contributions into anthropology more generally. It additionally seeks to make the case for ethnobiology as a distinctive interdisciplinary subject which is especially suited to developing synergies which go beyond the 'mixing' of ideas from adjacent and overlapping subjects. There is, of course, a persuasive case to be argued in favour of the influence of ethnobiology on other subjects: ecology, for example (Balée 1998; Plotkin 1995; Schultes & von Reis 1995: part 6), ethnopharmacology and ethnopharmacy (Etkin 1986; 1988; 1993; Schultes & von Reis 1995: part 8), conservation biology (Cunningham 2001; Johannes 1989; Laird 2002; Tuxill & Nabhan 2001), development studies (Warren, Slikkerveer & Brokensha 1995), political ecology (Zerner 2000), and economic botany,[2] being those that most readily come to mind; and even for its intrinsic proto-disciplinarity (Alexiades 2003); but here we concentrate on the contribution to anthropology, where connections at the interface have perhaps been best articulated.

This introduction attempts to provide, as it were, an empirical account of the kinds of theories which ethnobiologists employ, where they come from, and the extent to which they might constitute shared, and recognizably anthropological, theory. The conjunctural qualities that I attribute to ethnobiological research arise in part because, like many other fields of 'the science of humankind', it seeks to produce knowledge of the relationship of categories to behaviour, and of culture to social action; but it is additionally unusual in having tangible material referents which are simultaneously the units of analysis in natural science discourse. A comparable simultaneity of reference has been argued for studies of material culture, which Sillitoe suggests also provide us with 'a sure observational baseline' (1988: 5); and in both cases the intersection between the discourses provides opportunities to test the rigour of our methods through repeated and systematic cross-reference. The introductory remarks which follow are restricted to just seven headings, are in no sense either exhaustive or mutually exclusive,[3] but do have a certain salience in the literature: the foundational paradigm of taxonomic orthodoxy – what I call 'the Linnean grid'; language and the translation of knowledge systems; cognition and culture; the social organization and transmission of knowledge; medical ethnobiology; the applied practice

of ethnobiology; and – the meta-theory which binds all this together – the co-evolutionary paradigm as part of a 'biocultural synthesis'.

The Linnean grid

What links the work of most ethnobiologists, whatever other theoretical orientations they may subscribe to, is at least passing acknowledgement of the relevance of contemporary biological (and specifically taxonomic) orthodoxy. Empirical work which has paid no attention to this can be of no lasting value as its phylogenetic reference will always be in doubt. If descriptions of the natural world are rendered entirely in terms of local words and categories, they will be of virtually no use to other scientists, other local folk, and policy advisers who must work within a world of firm identifications and frameworks. Of course, the distinction between indigenous (or local, or traditional) and modern (or scientific) knowledge is now much discussed and critiqued, and there is no settled agreement on the terms we use to express the differences which such a debate encodes (Ellen & Harris 2000), but science itself and the pragmatics of intercultural communication and action require that provisional working assumptions be made.

For some ethnobiologists the Linnean scheme provides no more than a grid on which to map folk categories, a way of pinning them down to some more widely shared representations; for others that grid is a crucial part of an argument, either by demonstrating the degree to which folk classifications might match or deviate from their scientific counterparts in terms of category boundaries or representations of diversity, or by demonstrating the cultural significance of biological information. But some people who call themselves ethnobiologists also operate wholly within a biological paradigm, being concerned with the uses of a particular species, biological family, or functional group of species, rather than with their local cultural representation and social correlates. Some of these individuals would regard themselves as anthropologists, but others have a primary allegiance to a different discipline. Consult any issue of *Economic Botany*, *Journal of Ethnopharmacology*, *Human Ecology*, or *Journal d'Agriculture Tropicale et Botanique Appliquée* for insight into work of this kind. At a more analytical and inclusive level such an approach informs those who seek to place the understanding of human subsistence strategies generally in some overarching biological paradigm: say, diet breadth studies, evolutionary ecology, optimal foraging theory, or pharmacognosy.

If anthropologists wish to pride themselves on their ability to apprehend and translate the ethnographic 'other', then they should perhaps with equal commitment take on board translation between the categories of international science and folk knowledge. There is a parallel case here with all anthropology, of course, in that we write in English – or in some other widely spoken tongue of a nation in which global science is instituted – and so translate between technical and folk categories all the time, even if they are anthropological ones (e.g. clan, shaman, taboo). In his contribution to this volume, Eugene Hunn – while acknowledging the pragmatic necessity of 'Latinate names' – attempts precisely this. He invites us to speculate reflexively on the apparent arbitrariness of the Linnean grid. This, especially in the context of the 'new systematics' based on cladistics, phylocodes, molecular evidence, and fear of species overload, is increasingly generating patterns which fail to match in convenient ways the kinds of resemblances enshrined in a taxonomic orthodoxy based on herbarium sheets, spirit specimens, skins, and osteology. We can see this in botany (Hollingsworth, Bateman

& Gornall 1999), zoology (Pennisi 2001), and indeed in primatology and evolutionary anthropology (Relethford 2003: 248-53). Hunn suggests that we might consider scientific Latin as just one 'special-purpose' language amongst many capable of grasping the reality of biodiversity, and in examining the often technically profound way in which many unwritten languages of local peoples encode biological knowledge the evidence is plain to see. That Latin and not Tzeltal has become the global tool for accurately describing organisms is due to the vagaries of history: the adoption of Latin as the language of the Roman church, and therefore of European scholarship, the expansion of Europe into the rest of the world rather than the other way around, and the eighteenth-century scientific enlightenment (Stearn 1973: 6-50).

Language and the translation of knowledge systems

Early work in ethnobiology often addressed the problems of lexicographers and philologists. This was especially the case in France (e.g. Haudricourt 1973; see also Bulmer 1974: 79). But the approach derived from philology, and indeed from modern historical linguistics, is still reflected in the use of ethnobiological language data to track the introduction, erosion, extinction, diffusion, domestication, and change in significance of useful species (Balée 1994; Nabhan & Rea 1987; Whistler 1991), and to correlate biodiversity with cultural diversity using language as a proxy (Maffi 2001). Moreover, modern ethnobiology (which, if we take the longer view, begins seriously in about 1950 with the emergence of what I earlier called 'the second phase') found its first theoretical stimulus in linguistics, either in the anthropological linguistics of Franz Boas, Edward Sapir, and Benjamin Lee Whorf or (later) in the structuralism of Roman Jakobson and Ferdinand de Saussure. The linguistic methods employed during this formative phase are sometimes labelled ethnosemantic (or at least were in its first wave), and in the United States owed much to work published in the late 1950s and early 1960s in the ethnoscience tradition (Sturtevant 1964), the guiding methodological principle of which was to yield sufficient data to understand those rules which might permit an ethnographer successfully to replicate the language behaviour of a native (e.g. Frake 1980).

The combined methodologies of ethnoscience and componential analysis proved to be a productive paradigm in terms of the studies they inspired, and influenced the analytical terminologies of a later generation. But while they served well as schemata for yielding basic data, and their elicitation techniques are still valuable today, their fatal drawbacks included over-reliance on formal methods of interview, a preoccupation with nomenclature and distinctive feature analysis to the exclusion of much else, accompanied by an over-simplified sociology, and a tendency ultimately to play down the dynamics of sharing knowledge and cross-cutting rules of classifying behaviour in favour of eliciting taxonomic schemes of often bewildering complexity. In addition to its techniques, its lasting value also lay in its concern with the relationship between category and word; and an ability to show that the correspondence between the two was seldom straightforward, for example that not all words imply the existence of separate categories, and that categories could exist independent of lexical labels. This was made possible in part by the mapping of folk categories on to their phylogenetic denotata: thus theorizing the relationship between emic and etic based on reproducible empirical data. Such a linguistic approach is perhaps best exemplified in the work of Harold Conklin (1954) and continues, for example, in the work of Paul Taylor (1990). It is grounded in the assertion that since biological classification is a part of language,

then it must necessarily be understood primarily as a linguistic phenomenon, employing the techniques of linguists.

Hunn emphasizes here the importance of understanding local languages for a full appreciation of local ethnobiology. Names of fauna and flora have also been crucial in the study of onomatopoeia, metaphor, and sound symbolism, which both he and Berlin refer to in their respective papers. But Hunn also reminds us that language, and especially written language, has its limits. Not only is the written word often inadequate to grasp the precise way in which local peoples perceive their environment, but the vocal and verbal dimension itself is insufficient. Despite the attempts of anthropologists such as Steven Feld (1996) and Paul Stoller (1989), the synaesthetic reality of sensory perception of the environment can be reduced to written texts only with difficulty, and this is partly the reason why it is so hard to reduce practical or embodied ethnobiological knowledge to a written text. Hunn suggests that some of this reality is captured in the often dazzling illustrations which accompany ethnobiological texts, but he also points to a future in which multimedia ethnographies (see, e.g., Hesse-Biber, Dupuis & Scott Kinder 1997) will allow us to extend the realms of what is possible to publish as scientific and scholarly communications. Moreover, recent cognitive anthropology, as reflected below, and in the paper here by Steven Mithen, has now effectively demonstrated the ability of the mind to make sense of much ecological knowledge, and indeed culturally to transmit such data, without constantly converting it into language (Ellen 2003*a*: 62-3; 2003*b*: 47-8).

Translating the biological knowledge of the cultural other into the categories and theories of global science has arguably been the mission of ethnobiology since the 1950s. This has been so not only in the sense of measuring and comparing local constructs against the Linnean grid, but also in terms of attempting to comprehend local conceptual systems as they pertain to understanding the natural world. Increasingly, that mission has also provided a voice for local people, both technical experts and ordinary folk, as in the role described for indigenous knowledge generally in current development work (Sillitoe, this volume). But since the knowledge which this reflects is articulated orally, or even devolved in non-linguistically coded tacit experience, it often poses major problems for effective conversion into the literate mode, inviting serious over-simplification and straining the limits of ordinary language. Consider, for example, how you would explain to a child how to tie a shoelace – over the telephone. Ethnobiology connects, therefore, with the writing of ethnography,

Until his death in 1988, Ralph Bulmer spent an enormous amount of time in the field working with Kalam subjects in both Papua New Guinea and New Zealand. It is perhaps significant that increasingly he shifted to the publication of verbatim texts, mainly authored by his principal informant, Saem Majnep (Majnep & Bulmer 1977; 1990), which moved him away from an interest in the structure of classifications. In so doing he produced what is perhaps the first postmodern ethnobiology. In contemporary ethnobiology we are more aware of the individual conveyor of knowledge (the Ton Alonsos and the Saem Majneps) than ever before (B. Berlin 2003; Marcus 1991), and it is now difficult to see knowledge simply as disembodied abstract diagrams on paper. In this volume Eugene Hunn explores the notion of 'writing ethnobiology', and provocatively suggests that ethnobiology was producing what James Clifford might call the dialogic ethnographic narrative that allows 'the "subaltern" voice of the Other to be heard' almost a decade before the celebrated volume edited by Clifford and George Marcus (1986). Ethnobiology – like other branches of anthropology – has contributed

powerful prose and gifted writing, and has been a prime site for pioneering experimental ethnography and for experimental writing, for example in its innovative use of illustration. Hunn suggests that it is useful to distinguish between popular master narratives, designed to convince the reader (and to his selection we might add, for example, Nabhan 1998), technical narratives, which legitimate the argument with reference to good science, and reflexive monographic narratives, which explore and interpret the data in a self-critical and nuanced way. Amongst the authors of master narratives are some ethnobiologists, such as Richard Schultes (Davis 1996) or Darrell Posey, the charismatic founder of the International Society of Ethnobiology, who are cast in a truly heroic role (see also Plotkin 1993). Such writing is a two-edged sword, for while it advertises the significance of what ethnobiologists do (and may recruit us students and bring us fame, if not fortune), and while there is a clear link between writing, rhetoric, and a kind of advocacy which is central to much ethnobiological research (e.g. Hunn 1990; Posey 1999), it is in danger (like other kinds of popular writing in anthropology) of trivializing and exoticizing.

Cognition and culture

In cognitive anthropology issues raised by ethnobiological classification have been central to debates about category formation, classification, knowledge transmission, and the evolution of the mind. The works of Brent Berlin (1992), Atran (1990), Brown (1984), Hunn (1977), and Boster (1996) all bear testimony to the fertile synergy between research in ethnobiology and cognitive studies more generally. There are important 'inner connections', as Clifford Geertz (1966) might put it, between, say, Berlin's work with Kay (B. Berlin & Kay 1969) on colour, or Conklin's (1964) work on kinship categories, and their work on ethnobiological classification. Ethnobiology has also provided a crucial empirical link connecting anthropology with psychology and cognitive science (Atran 1998), and, in the work of Steve Mithen (1996), with archaeology as well. In this volume, Berlin discusses the striking role of verbal mimesis in the evolution of human cognition, especially in relationship to the semantically opaque names and the physical qualities of certain organisms. Mithen, in his paper, reiterates the idea that cross-cultural similarities in ethnobiological classification are a legacy of a universal 'evolved predisposition' in *Homo sapiens*. He suggests that a new ability to cognize the natural world accounts, in part, for the success of early species of *Homo*, such as *ergaster*, during the period of Old World colonization about two million years BP, and *Homo sapiens* in the Arctic and the Americas later, and also as some kind of preadaptation to the transitions to agriculture of the early Holocene. Mithen presents us with a challenging account of how natural history intelligence might have evolved. Indeed, there can be little doubt that approaches from our understanding of ethnobiological systems will, over the next decade, help to provide answers concerning the extent to which knowledge is constrained by the evolved architecture of the brain, the extent to which that architecture is itself the product of neural enculturation in the development of each biological individual (where the similarities reflect common ecological stimuli), and the extent to which shared knowledge structures reflect parallel socially devolved cultural processes (Ellen 2005).

Many anthropologists reserve their scrutiny for categories which divide up social and cultural space at several removes from unsocialized perception, with so-called 'complex' categories and more abstract schemes. Though such schemes are no less real for those in whose representations they feature, by themselves they serve only as a

partial basis for understanding category formation. Ethnobiology has provided much of the evidence through which to test and elaborate theories of the category, in both cognitive anthropology and cognitive psychology. These began by being mainly linguistic in character, and early attempts derive almost entirely from the distinctive feature model developed in componential analysis. Such a model emphasizes the boundaries of categories, and this general approach found favour in the underlying semantics of the Anglo-structuralist analyses of Edmund Leach (1964) and Mary Douglas (1966). What such theories lacked was dedicated research in natural and laboratory settings, and it soon became clear that categories are much more fuzzy in their construction, something which could be modelled by modifying componential analysis as a kind of polythesis, or in terms of core-periphery models which assume the preeminence of cognitive prototypes, depending on whether linguistics or psychology is the preferred reference discipline (Ellen 2005: 4-5). At the same time, more attention was being paid to the relationship between cognition as a mental activity and the learned bodily routines which act on and in the world but are not necessarily simply the enaction of mental processes, and between knowledge and enskilment (Ellen 2003*a*: 48). In other words, ethnobiological ethnography enabled more accurate modelling of real-world categories.

A concern with theories of folk classification, in the sense of attempting to work out the relationship between categories at all degrees of inclusiveness in a domain, is relatively recent, its formative phase being associated largely with Conklin (1954) and Berlin (B. Berlin, Breedlove & Raven 1974). To some extent Émile Durkheim and Marcel Mauss (1963 [1901-2]) had prefigured a sociological theory, but this had never directly addressed mundane or technical categories, though it was adopted by Leach and Douglas as if this had been so. The main impetus, rather, came from work influenced by ethnoscience, adopting the idiom of taxonomy borrowed from the Linnean tradition of Western thought. As with the linguistic paradigm, the taxonomic model proved to be an elegant device for generating data, though its usefulness as an exclusive paradigm has been widely contested (Ellen 1993; Friedberg 1990; Hunn 1977; Sillitoe 2003), in particular its assumptions concerning rank, level, and contrast, together with its weak engagement with the issue of knowledge variation, flexibility in the use of constructs, and the social context of classifying behaviour.

Theories of the change and evolution of lexical and semantic fields relating to environmental sense data are a relatively recent focus of interest (B. Berlin 1972; Brown 1984). We can see an anthropological precursor in Morgan's (1871) theory of the evolution of kinship terminologies. In immediate terms the stimulus can be traced to the aforementioned early work of Berlin and Paul Kay (1969) on the evolution of basic colour terms. Such theories seek to show the order in which terms and ranks are added to languages when the evidence is aggregated at a global level. Clearly, these theories owe little to ethnographic practice; indeed it has been argued that their limitations lie in an over-dependency on inadequate cross-language data, a method of aggregate abstraction which eliminates reference to culture history, the a priori establishment of 'levels', arbitrary assumptions in distinguishing terms as members or otherwise of a particular and exclusive domain, and the inference of psychological reality from nomenclature. However, one lasting impact of this body of work has been the irrefutable conclusion that all cultures encode a concept of basic category, and that they repeatedly divide up the natural world in particular and similar ways.

The social organization and transmission of knowledge

In cognitive anthropology ethnobiology has provided studies that have enlivened and empirically substantiated the more arid debates about knowledge and its transmission. By contrast, social theories have played a relatively peripheral and relatively late role. Durkheim and Mauss gave us a model for the study of social classification, and as we have seen, this was adopted by the Anglo-structuralists and social constructionists. The first sociologically oriented work of any significance was that of Ralph Bulmer (e.g. 1967; 1970), taking his cue from Lévi-Strauss but working very firmly in the British tradition of social anthropology. What is important about Bulmer's work, apart from an impressive thoroughness, is its dedication to dissecting the relationship between the mundane and the symbolic, between what Berlin describes as special-purpose classifications and general-purpose ones. Meanwhile, in North America, Whorfian semantics and the Nida-Conklin hypothesis were instrumental in drawing attention to the complexity of certain terminologies by explaining why we should find certain phylogenetic biases in the distribution of folk knowledge (B. Berlin, Breedlove & Raven 1966).

The last few decades have seen various analyses of the connections between cognition and collective representations, mind and culture, and between 'mundane' and 'symbolic' classifications (e.g. Bloch 1977; 1985; Ellen 2003*a*: 50-1; 2005). Much of this, including debates around the role of metaphor, totemism, animism, and the construction of 'nature', has supported the view that the interrelationships between symbolic and mundane are often far from clear (Ellen 1993; Fox 1971; Healey 1993; Rival 1998; Rosaldo 1972). Having said as much, some confusion has arisen from a failure to distinguish clearly instruments (means or agents) of cognitive process from the medium of belief and cultural representation, that is, to explore how the invariant possibilities of mind (concretization, polarization, analogy, taxonomy even) act on and through existing sets of beliefs and representations, and influence the formation of new ones. A generation of anthropologists have tended to conflate cognition with collective representations, but as Bloch (1985: 301) has insisted, we cannot treat cognition as some arbitrarily imposed scheme. The kinds of cognitive device which I have just briefly listed are apparent in the social construction of categories across the complete range of human experience, 'mundane' no less than 'symbolic'; and if we wish to understand the processes which underlie classifying activity in general, and which connect the instruments of cognition with ethnographic appearances, we would do well to begin with those processes through which we categorize (as far as this is ever possible) the discontinuities of the physical environment. Despite feedbacks from pre-existing classifications and representations, and their inextricable social contextualization, animals and plants provide us with some of the simplest possible, in a word 'elementary', relationships between objects and their representations that are accessible to researchers producing data in natural settings. Categories of natural kinds are about as rooted in the empirical world as categories can ever be, and in a way that those applied to the world of people and social phenomena can never be.

The kind of work typified by Bulmer, Leach, Douglas, and Bloch, while firmly part of British social anthropology, still did not address empirically the way in which ethnobiological knowledge is distributed, organized, transmitted, or valued. The corrective to the kind of generalization upon which such analyses relied arose, initially, as a critique of the 'omniscient speaker-hearer' model that the ethnoscience exponents had articulated. Increasingly, ethnographic practice began actually to measure the variable

distribution of knowledge within a population (e.g. Gardner 1976; Hays 1974), or variation in the significance of particular species (Stoffle, Evans & Olmsted 1990; Turner 1988). But once it became empirically evident that fundamental knowledge might vary within a population, the data raised important issues concerning: the extent of 'cultural consensus' (Ellen 2003*a*; Romney, Weller & Batchelder 1986; Sillitoe 2003: 109-16); constraints on transmission of knowledge networks deriving from structured bias and stochasticity (Casagrande 2002); knowledge exchange and flow; and the information upon which subsistence decision-making might be based. Here again ethnobiological knowledge provided convenient data with which to explore a new methodology (Boster 1984; 1986). And although much had been assumed and asserted about gender as a variable in knowledge distribution, it was not really until the appearance of the crucial work of Howard (2003) on women and plants that this became a serious matter for empirical investigation. Again, the social divide in knowledge between specialists (e.g. healers) and generalists had much been speculated about, but though crucial to addressing issues of cultural consensus, it has even now proved intractable to effective empirical study. Much more attention has been paid in recent years to knowledge transmission, and here too, much of the impetus has arisen from fears of ethnobiological knowledge erosion. One outcome of a focus on transmission, naturally, was data on the distribution of knowledge by age and generation (Stross 1973). Models for analysing transmission remain an area for current debate central to current anthropological concerns, a debate in which the influence of Luigi Cavalli-Sforza (Hewlett & Cavalli-Sforza 1986; Ohmagari & Berkes 1997) looms large.

Medical ethnobiology

One field where the social organization and distribution of knowledge is well reported is medical anthropology. But although we would like to acknowledge here that medical anthropology has gained much from work conducted by medical ethnobiologists (and vice versa), the case is not a clear one. This is a source of some puzzlement. Textbooks in medical anthropology, on the whole, pay scant attention to the work of medical ethnobiology. We might reasonably expect that medical ethnobotany or ethnopharmacology or ethnopharmacy would be providing the baseline data for much of the content of medical ethnographies, but what medical anthropology has seemed hitherto to lack is full engagement with phytomedical reality, and an acceptance that the healthcare practices of most people on the planet depend on plants and animals. At the same time many accounts of folk phytomedicinal uses still lack serious consideration of local ethnographic context. Here, it seems to us, is an enormous opportunity and challenge for research.

Of course, there are significant exceptions, the work of Nina Etkin (1986; 1988; 1993) perhaps being the most visible, but there is also the path-breaking work of Elois Ann and Brent Berlin (1996) and (in France) Francis Zimmerman (1989). Work in medical ethnobiology has not only permitted the interrogation of the food/medicine (Etkin 1986; 1994) and medicine/poison (Bisset 1995) conceptual divides, but also the exploration of the interface between the great scholarly systems of knowledge and local folk practice, in, for example, Ayurvedic, Tibetan, and Chinese medicine (Anderson, Salick, Moseley & Ou Xiaokun 2005; Hsu 1999; Zimmerman 1989). The organization of medical and biological knowledge in these traditions is subtly different from much folk medicine, given its literary presentation and institutional base – what Anna Waldstein and Cameron Adams call here 'medical schools'. Its study, therefore, requires different

methodological tools and skills from researchers, but it is no less relevant to the broader mission of medical anthropology.

The contribution by Waldstein and Adams to this volume addresses directly the interface between medical anthropology and ethnobiology. It is written as a review – almost an annotated bibliography – precisely because it seeks to build intellectual bridges between medical anthropology and ethnobiology, and precisely because such an infrastructural service to the sub-discipline is what is needed at this time. For there is a curious lack of connection not only between studies of the socio-cultural presentation, treatment, and context of disease and studies of the use of *materia medica* (though see, e.g., Telban 1988), but also between studies of conceptions of the body and bodily experience and studies of the empirical knowledge of anatomy and physiology, excepting, for example, Frake (1980) and Lewis (1974). Coincidentally, work in this second area has been much informed by the same cognitive and empirical approaches which gave rise to the systematic analysis of ethnobiological knowledge and naturalistic ethnomedicine in the 1960s. Moreover, people's folk knowledge of human bodies, their processes and pathologies is closely allied to their experience and understanding of the same dimensions of the animals with which they come into contact, especially if they are domesticated. It is no wonder, therefore, that there is a significant overlap in the herbal treatments reported for humans and those found in the ethnoveterinary literature (Mathias-Mundy & McCorkle 1995). Similarly, just as different medical systems often, and increasingly, coexist in the same place and, indeed, are combined in the therapies of some practitioners and in the minds of consumers (e.g. Golomb 1985), the same has happened historically in terms of the movement of medicinal plants and the ways in which they have been subject to processes of ethnopharmacalogical and therapeutic hybridization (Bennett & Prance 2000).

The applied practice of ethnobiology

Applied ethnobiology did not emerge from within the conventional arena of applied anthropology at all. In one sense it had always been integral to how ethnobotanists at least have conceived their project, as the study of useful (meaning economic) plants. It also existed in a forensic sense, as the specialized techniques by which culturally modified and transformed organic materials can be identified, for whatever applied purpose. As *ethno*-biology rather than economic biology, and specifically as the study of the knowledge of local peoples, it was much rejuvenated in the 1970s through the failure of science-driven top-down development projects, and through the activism of environmental NGOs and indigenous peoples' movements. Because such issues have become intensely political, their 'applied' character has been at times controversial. Its original sense, if we consider for example the practice of 'economic botany' developed at Kew Gardens (Brockway 1979), or what is reflected in the early editorial policy of *Economic Botany*, was the application of biological knowledge to commerce and industry. This is still important, and has its highest, and most controversial, profile in the role of ethnobotany in drug discovery (Chadwick & Marsh 1994). However, the crisis in top-down development policy orientated it firmly from the 1970s onwards towards 'valuation studies' (Brush & Stabinsky 1996; Peters, Gentry & Mendelsohn 1989), and to a role in elucidating farmer knowledge (e.g. Cleveland & Soleri 2002; Richards 1985), knowledge of non-domesticated species, and folk medicinal knowledge, all in the interests of participatory approaches to development (Alcorn 1995; Sillitoe, Bicker & Pottier 2002). From the 1980s, it was being routinely employed as a strategy for ensuring

favourable biodiversity outcomes and as a way to integrate the interests of local people with wildlife conservation objectives (Cunningham 2001); it also exercised an influence in agricultural development contexts equal to that of farming systems approaches (Sillitoe 1998: 210). Ethnobiology had become absorbed into the rhetoric of 'indigenous knowledge' and 'indigenous rights'. Indeed, the paradigm of ethnoecology (e.g. Brush 1992; Nazarea 1998; Posey 1984) so massively re-configured studies of agriculture and ecology in small-scale societies that in environmental and ecological anthropology it has virtually obscured and replaced other kinds of approaches.

Unlike conventional applied anthropology, applied ethnobiology at its birth was innocent and unencumbered by the angst and dilemmas accumulated by 100 years of history of anthropology as the purported handmaiden of colonialism. Without this baggage it has managed to achieve recognition for the value of traditional ways of life and affirmed the value of local knowledge systems – as Eugene Hunn puts it in his paper – without being either romantic or patronizing. Not only has ethnobiology made an enormous impact on the development field and the politics of less developed countries over the last decade – as Paul Sillitoe demonstrates in his paper – it has also – despite the biopiracy controversies – pioneered protocols for the conduct of responsible research and development of natural products in the face of national and commercial interests (Posey 2000), and been at the forefront of initiatives to promote genuinely participatory approaches, and the interests of local and indigenous populations, especially with respect to the ownership of knowledge and genetic resources. In a workshop at which Darrell Posey and I were once present, I had the temerity to suggest that ethnobiologists ought to be 'dispassionate' in the way they interpreted knowledge systems. As those who knew Darrell might have expected, he demurred, strongly, asserting that, on the contrary, we should be passionate. I like to think we were both right. We should be passionate about knowledge rights and about ethnobiology as a proper subject of study, but dispassionate in our methodologies and in the evaluation of our data.

The co-evolutionary paradigm and biocultural synthesis

As the previous section demonstrates, ethnobiology has provided an ideal site for the convergence of academically driven pure research (largely to do with classification and cognition in anthropology and linguistics) and practice-driven issues to do with subsistence regimes, valuation, and the management of natural resources, more usually associated with biology, agriculture, and forestry. But if ever it was strictly true, it is now no longer possible to describe yourself as an ethnobiologist and operate wholly within a biological paradigm, innocent of a distinctive and emergent dynamic at work in human social and cultural systems. This dynamic simultaneously influences and provides a context for biological change, while biology provides the ultimate conditions for social and cultural continuity. Biological and cultural history (which I take to include processes of natural selection, cultural adaptation, and social continuity) both depend on existing ecological and social contexts for their perpetuation, while these contexts themselves define their existence as outcomes of earlier biological and cultural changes (Figure 1). This is reminiscent of Giddens's notion of structuration in the reproduction of social life (see discussion in Ellen 2003*b*), and of Ingold's (1986) interpretation of the emergent dynamic between biological and social levels of organization. What is developing as one of the main guiding principles of ethnobiology, therefore, is what we might call the biocultural synthesis, and the failure to see that

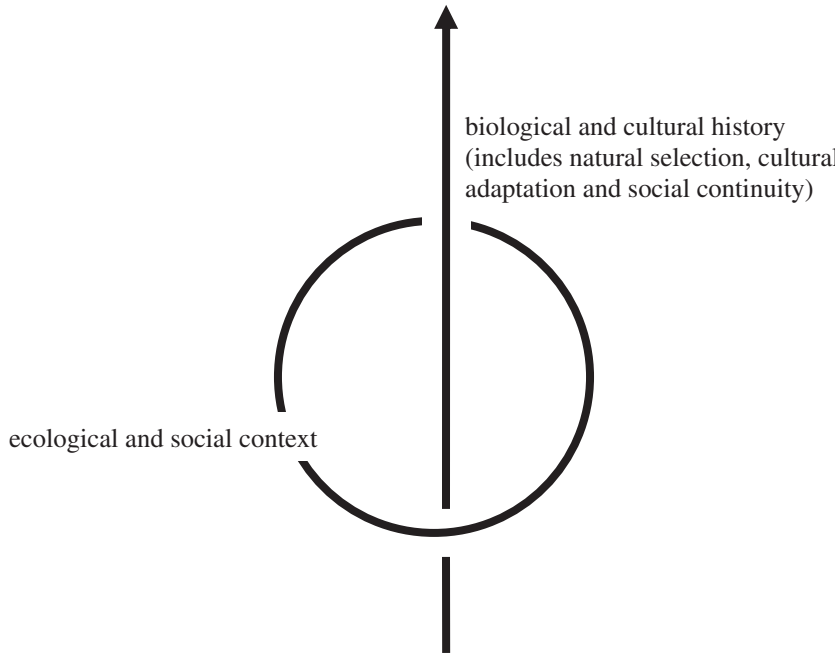

Figure 1. The implicate and recursive relationship between process and context which drives biocultural synthesis.

this has been a core global dynamic for over 10,000 years is an indication of the damage that disciplinary boundaries can do. How could we have ever thought that socio-cultural and biological domains were not inextricably linked, mutually embedded, implicate in each other, if not in simple unicausal relations of determinism?

The key explanatory concept underlying the biocultural synthesis, and one actively fostered by ethnobiological approaches, has been co-evolution. This is reflected, at the highest level of abstraction, in models of gene-culture co-evolution: specifically in work on ethnobiological classification and the idea that cognition of the natural world evolves through interaction with the knowing subject (Boster 1996; Fukui 1996; Mithen 1996). Thus, in what it tells us about the development of human sound symbolism in relation to visual cues in the environment, Berlin provides us here with a strong echo of the relevance of the co-evolutionary concept for understanding synaesthesia. Similarly, we see the influence and power of the co-evolutionary idea in work on the interactions between domestication, nutrition, and the evolution of the human dietary system (Rindos 1984), and on the cultural predisposition to use certain plants in a way we describe as medicinal (Johns 1990; Moerman, Pemberton, Kiefer & Berlin 1999), as discussed here by Waldstein and Adams; in work on forest use and traditional agriculture (Dove 1994; Padoch, Harwell & Susanto 1998); and in work on the history of landscape which traces intricate temporal patterns of interaction between environmental components and cultural practices, or what we now call historical ecology (Balée 1994; Rival in this volume). Historical ecology, with its reliance on ethnobiological data, has become, with ethnoecology, one of the dominant paradigms of

the new environmental and ecological anthropology, and is sufficiently robust to also serve as a vehicle for analyses of germplasm exchange and diffusion (e.g. Crosby 1972; Lebot 1991). Similarly, the concept of co-evolution is proving to have wider policy impacts in terms of the way we think about processes of global development in general (Norgaard 1994). But not only have classificatory studies been a primary tool yielding evidence for reconstructing anthropically induced ecological change, historical ecology has provided the context for understanding ethnobiological classification.

Given its emphasis on long-term change, it is hardly surprising that co-evolution has also become a key concept linking the contemporary paradigms of ecological and environmental anthropology to studies of palaeoenvironments and early human development (Crumley 1994; Harris 1989). More so in North America than elsewhere, ethnobiology as an organizing framework has influenced work in archaeology, in the form of archaeobotany (or palaeoethnobotany) and zooarchaeology (e.g. Ford 2001; Willey 1995). While it is true that ethnobiological insights, as with other ethnographic analogies, have for a long time played an important role in interpreting past subsistence systems and ecologies (e.g. Harris 1969; 1977; and in this volume), the conceptual apparatus of historical ecology provides a new and particularly sensitive and methodologically explicit way in which to understand how ecosystem dynamics and human agency intertwine. Indeed, historical ecology and co-evolutionary approaches provide the theoretical context in which Harris's work on multiple alternative pathways to agriculture makes most sense, together with evidence from different regions of the world that challenge the one route to farming approach. Of course, the conceptual and practical difficulties of linking the ethnographic present with the prehistoric past are not to be underestimated. Laura Rival, for example, describes in her paper the difficulties of assessing the archaeological data from the Amazon, and Harris more generally notes the disparity between the picture emerging from ethnography and that constituted by archaeology. Correlating ethnographic and archaeological evidence presents severe limitations, and Harris meticulously, and in a characteristically measured review, illustrates the strengths and weaknesses in relation to work he has conducted on food procurement using both kinds of data. This disparity is reflected at the methodological and organizational levels of work on the ethnobiology of living peoples and in palaeoethnobiology, the latter often strong on empirical techniques (Hastorf & Popper 1988; Pearsall 1989; Renfrew 1991) but weak on the theoretical linkage it permits with the cultural organization of biological knowledge more generally. Ethnobiology and historical ecology offer new ways in which archaeology and ethnography, and, more pertinently perhaps, archaeologists and ethnographers, can be brought together.

In her contribution to this volume, Rival demonstrates the theoretical power of historical ecology in the context of human ecology more generally. Following Balée (1998: 4), she defines it as the conduct of diachronic analysis of living ecological systems in order to account fully for their structural and functional properties, such that it becomes a 'dialectic of an inalienable link between nature and culture', reconceptualizing the 'problematic distinction between the wild and the domesticated' so evident in the Western cultural tradition. Though she critiques Balée's concept of post-contact cultural regression, she illustrates powerfully how botanical and ethnobotanical data have come to play an important role in Amazonian anthropology in particular, how tropical forest is being reinterpreted as a mosaic of anthropogenic as well as independent ecological forces (e.g. Ellen in press), and how hunting and

gathering in tropical forests therefore need to be re-conceptualized as forms of biological resource management (Posey 1984; Sillitoe 2003). Rival argues that peoples such as the Huaorani 'are active agents in the concentration of useful species', and more. Ethnobiology, therefore, becomes an intrinsic part of historical ecology, which in turn undermines the credibility of the more simplistic, adaptionist, and functional models of human ecology.

The centrality of ethnobiology to anthropology

In anthropology, Wolf (1980), Ortner (1984), and Hart (1990: 14) have all argued that there is no longer a shared discourse, that we are 'over-fragmented, overspecialized', or, to put it more strongly, that we have abandoned the study of humankind (Ingold 1985: 15). This is a widely shared view and one that is hardly new (Needham 1970). Nevertheless, it merits much closer attention than it has hitherto received, especially given its strange inverted echo in Ford's (1978) estimation of the position of ethnobiology in the 1970s with which this introductory essay began. In one sense there never has been a completely shared discourse in anthropology, only a series of overlapping ones; but there is now a resurgent demand within the subject that anthropology concern itself once again with the 'big' issues: with the relationship between naturalism and humanism, with the renegotiation of relations between biology, social life, and culture (Ingold 1985: 15). Ethnobiology, given its subject matter, if not necessarily the practice of individual exponents, can be said to stand at an important intersection between nature, culture, and sociality. For Ford, thirty years on, ethnobiology is now 'at a *crossroads*' (2001: 1, emphasis added). That, I suppose, is some measure of the progress which has been made. I would put it even more strongly: that we have negotiated the crossroads, and are now travelling along a road which has routinized the objectives of ethnobiological study and which continues to yield at an accelerated rate immensely rich and diverse insights which address core anthropological questions. I have attempted here to show how ethnobiology has acquired this conjunctural character, but the message is spelled out in detail and most effectively in the essays which follow.

While, as these essays demonstrate, ethnobiological knowledge is unavoidably affected by cultural relativities, it provides us with a convenient benchmark when examining the perception and use of the environment more widely. Because ethnobiology so obviously focuses on the simultaneity of physical experience, category, and sociality through language, and because it constitutes a domain where the articulation of collective representations with cognitive process, of belief with thought, material with mental, is at its most accessible, it seems to me as good a place as any to begin an inquiry into what we should understand anthropological theory properly to be. Its implications for the study of subsistence behaviour, health, ecology, categorization, and belief may be its most obvious relevancy, but it has also, like kinship in the 1960s (Ortner 1984), provided a convenient body of data on which to experiment using methods involving explicit rigour and formalism, where the search for universals and the idiographic mode engage with rare clarity, and where the relationship between middle-range theory, ethnographic special pleading, and meta-theory is well demonstrated. I think its interdisciplinarity has proved a key to its methodological strength. Interdisciplinary encounters challenge us to make sense of the methods of juxtaposed disciplines, and in ethnobiology the biologists have had to accept the necessity of qualitative methods and of a more critical approach to social and cultural data (Etkin 1993),

just as anthropologists have had to develop serious quantitative protocols and measurements to meet the methodological expectations of biologists, and the latter's insistence on sound basic utilitarian procedures, such as voucher specimens, natural resource inventories, and ecological techniques (e.g. Alexiades 1996; Fowler 2001; Martin 1995; Sillitoe 1996; Vogl, Vogl-Lukasser & Puri 2004). Thus, we should not be surprised that ethnobiology has made significant contributions towards the statistical study of cultural consensus, of knowledge variation and transmission. It is methodologically nodal and provides an instructive arena in which to explore the unresolved contradictions inherent in an unusually wide range of theories and orientations. In other words, it is no peripheral frippery but is rather at the heart of that problematic encompassed and scrutinized through the indiscipline of 'anthropology'.

NOTES

[1] I am using the term 'ethnobiology' as shorthand to include ethnobotany, ethnozoology, and ethnoecology, together with any subsidiary designations, such as 'ethnomycology'. Of these, 'ethnobotany' has undoubtedly the greatest presence and intellectual coherence as a subject, but research in any of these fields cannot really proceed far without the other, and in folk knowledge systems there is, anyway, so much empirical overlap and interconnection that it hardly makes sense to distinguish among them. Consider, for example, ethnoveterinary studies, studies of plant insecticides, ethnoentomology, pollination, plant disease, and the analysis of human agricultural and extractive systems and diet. It is for this reason that Darrell Posey was increasingly drawn to 'ethnoecology' as a more encompassing and appropriate term for the kind of work that he undertook.

[2] An inspection of back issues of the journal *Economic Botany* between, say, 1960 and 2004 demonstrates a striking shift towards a focus on ethnobotany and its associated anthropological underpinnings and procedures.

[3] This introduction is to a subject which is accompanied by a large and exponentially growing bibliography. The references cited are, therefore, of selected, indicative and exemplary work only.

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Introduction

Résumé

L'introduction au présent volume plaide pour un rôle central et intégrateur de l'objet de la recherche ethnobiologique dans l'anthropologie au sens le plus large, à savoir dans ses dimensions biologiques, archéohistoriques et socioculturelles. Nous étudions ici l'évolution passée et la situation présente de l'ethnobiologie et examinons sa contribution au débat anthropologique sous les rubriques suivantes : le paradigme fondateur de l'orthodoxie taxonomique, langage et traduction des systèmes de connaissances, cognition et culture, organisation sociale et transmission des connaissances, ethnobiologie médicale, pratique appliquée de l'ethnobiologie et, en tant que méta-théorie faisant le lien entre tous ces aspects, le paradigme coévolutionniste dans le cadre d'une « synthèse bioculturelle » plus large. Nous présentons également la manière dont les différents articles illustrent ces thèmes.

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