

Vassar College Interlibrary Loan



ILLiad TN: 45715

**Borrower:** GDC

**Lending String:** \*VXW, TYC, YHM, TMA, VLW

**Patron:** ;dept; ;type; Amith, Jonathan

**Journal Title:** Mind & language.

**Volume: 2 Issue:**  
**Month/Year: 1987 Pages:** 27-63

**Article Author:**

**Article Title:** Atran, Scott; Ordinary constraints on the semantics of living kinds; A commonsense alternative to recent treatments of natural-object terms. (Volume 2

**Imprint:** Oxford ; B. Blackwell, c1986-

**ILL Number:** 13384690



**Call #:** BF455.A1 M553 v.2 1987

**Location:** PERIODICAL RM  
AVAILABLE

**ARIEL**

**Charge**

**Maxcost:** \$25IFM

**Shipping Address:**

Gettysburg College Library  
Interlibrary Loan  
Gettysburg College  
Gettysburg, PA 17325-1493

**Fax:** 717-337-7001

**Ariel:** ariel.cc.gettysburg.edu

Vassar College Library  
VXW

# VASSAR COLLEGE LIBRARIES

124 Raymond Avenue, Box 20  
Poughkeepsie, NY 12604-0020

Contact:  
Andrea Serra  
Interlibrary Loan Lending Specialist  
Telephone: 845-437-5764  
Email: [anserra@vassar.edu](mailto:anserra@vassar.edu)

Fax: 845-437-5795  
Ariel: [ariel.vassar.edu](http://ariel.vassar.edu)

**IF THERE ARE PROBLEMS WITH THIS DOCUMENT, PLEASE LET US KNOW  
WITHIN TWO (2) BUSINESS DAYS.**

ILL Number: \_\_\_\_\_

Borrower Symbol: \_\_\_\_\_

Borrower Ariel Address/Fax Number \_\_\_\_\_

Missing Page #(s) \_\_\_\_\_

Edges Cut Off Page #(s) \_\_\_\_\_

Illegible Page #(s) \_\_\_\_\_

Other: \_\_\_\_\_

**Please return this page to us with marked corrections via Ariel or fax.  
We will correct the problem as soon as possible. Thank you.**

ntics, Vol. 7). New York: Aca-

resentation Theory. In J.  
*course Representation and the*  
Foris.

argalit (ed.) *Meaning and Use*.

ce of a Proper Name. *Mind*,

f Quantification in Ordinary  
*ny*. Yale: University Press.

phora. *Linguistics and Philos-*

gic. New York: Free Press.

Blackwell.

*Syntax and Semantics*, Vol. 9:

nd a Multiplicity of Events.  
(Geneva).

out Next? In M. Brady and  
*course*. Cambridge: MIT.

nces in Discourse Represent-

hof (eds) *Studies in Discourse*

*Quantifiers* Dordrecht: Foris.

ation Categorical Grammar.

science).

## Article

# Ordinary Constraints on the Semantics of Living Kinds: A Commonsense Alternative to Recent Treatments of Natural-Object Terms\*

---

SCOTT ATRAN

Socrates: If I asked you what a bee really is, and you answered that there are many kinds of bees, what would you answer me if I asked you then: 'Do you say there are many kinds of bees, differing from each other in being bees more or less? Or do they differ in some other respect, for example in size, beauty, and so forth?' Tell me, how would you answer that question?

Menon: I should say that they are not different at all from one another in beehood.

Socrates: Suppose I went on to ask: 'Tell me this, then - what do you say exactly is that in which they are all the same, and not different?' Could you answer anything to that?

Menon: Oh, yes.

Plato, *Meno*

## I Introduction

This paper defends two general claims about the nature of knowledge and concept formation by focusing specifically on the semantic component of our understanding of living kinds. The first claim is that our various concepts of things are not constructed or processed in the same way. Accordingly, one would expect the semantics to be different for terms denoting distinct domains of objects. To justify this expectation is to indicate the special constraints that various cognitive domains impose on the meanings of various sorts of nominal categories. In particular, this

---

\* This paper was completed with the assistance of NSF Grant No. SES-8507896. I am indebted to numerous readers for careful criticism of earlier versions, but especially to Dan Sperber.

paper makes certain proposals about the singular character of lay taxonomy for living kinds and the conditions that such a taxonomy necessarily levies on the understanding and meanings associated with names denoting such ordinary sorts of living things as clover, tree, cat, robin, bird, whale, bat, fish and the like. A key argument is that in work concerned with folk categorization, researchers should clearly distinguish between the conceptual structure of living kinds and living kind terms as opposed to that of, say, artifacts and artifact terms.

The second claim is that ordinary understanding of the everyday world can be considered genuine knowledge whether or not it accords with science. Even if the same terms are used for the concepts of the layperson and the scientist in a given society, commonsense understanding may still be significantly independent of scientific interpretation. A case in point is that folkbiological taxa are not demarcated like the taxa of modern botany and zoology nor are they intended to be, even ideally. It is not that lay and scientific classifications differ ideally; for both are predicated on the idea that the constituent taxa are natural kinds with underlying natures. But the notion of 'natural kind' in the philosophical and cognitive literature generally carries with it the presupposition that the underlying natures involved are all and only those properly investigated by scientists. Ordinary folk, however, may well have a different conception of what constitutes an underlying nature, namely, one consistent with everyday experience. In fact, there could be an irresolvable disagreement between science and commonsense as to what constitutes an underlying nature in any given case without the commonsense account necessarily giving way to the scientific one. It is for this reason I refer to the natural kinds of folkbiology as 'phenomenal kinds' in contradistinction to the 'nomic kinds' of modern botany and zoology.

These considerations signal problems both with current views of conceptual structure in cognitive psychology, especially prototype theory, and with the psychological and anthropological implications of causal theories of meaning and reference in recent philosophy of language.

## II *Meaning as a Motley*

Until the 1970s most empirical studies of concept learning regarded the formation of phenomenal concepts, that is, concepts with clearly perceptible correlates, in terms of rather simple conjunctions of sensory invariants. Typically, the experimenter presented subjects a small set of blocks, or other like objects, artificially constituted so as to exhibit some arbitrary array of distinctly different perceptual stimuli: e.g. focal colours, large versus small sizes, simple circular, triangular and rectangular shapes. The subject's task, then, was to learn the 'true' concepts the experimenter had in mind by sorting objects into groups 'defined' as boolean functions,

that is, as severally necessary and jointly sufficient conjunctions of such clearly perceived attributes: e.g. ALL LARGE BLACK SQUARES versus ALL SMALL RED CIRCLES. Any attention the subject gave to such task-irrelevant properties as texture, weight, contiguity with other component objects, episodic or semantic associations to things and events in memory, etc., was interpreted as 'childish', 'simple', 'primitive', 'savage', 'complexive', 'concrete', 'iconic', 'graphic', 'pre-logical', 'unadult', or 'unscientific' (cf. Inhelder & Piaget 1964; Vygotsky 1965; Bruner, Olver & Greenfield 1966).

Much current work purporting to show the inadequacy of the 'classical view' of necessary and sufficient conditions is summarized and assessed by Smith and Medin 1981. They challenge the classical view of concept identity in the following ways: (i) few people, be they scientists or laypeople, can provide uniformly explicit criteria of conceptual identity; (ii) there are unclear cases, e.g. as to whether or not a tomato is a fruit; (iii) concept identification, verification and recognition is often expressed in terms of typicalities, rather than by means of atypical or absolute instances or features: e.g. sparrows are more typically birds than hawks because they contribute more to the feature-set of 'family resemblances' 'best characterizing most, but not necessarily all, birds.

In a recent review of Smith and Medin's attack on the classical view, Rey 1983 argues that both the standard version of the classical view and this latest opposition confound metaphysical issues of conceptual identity (truth and definition) with epistemological issues of conceptual access (classificatory and recognitory knowledge). Thus, metaphysical claims about, e.g. facts in virtue of which something is (or is not) a bird, may diverge from epistemological claims about facts pertaining to how we tell something is (or is not) a bird. Concepts would then appear:

to be about the world and how it divides up; not about how we might divide up our methods of investigating it. The interest is in the ducks themselves, not in our ways of knowing them.

Although mental stereotypes (be they prototypes or standard intensions) may carry more weight than 'accessory' means of verification when people are called upon to make judgments of category participation, people can still share a concept of, say, BIRD, whether or not they have stereotypical representations of birds in mind. It suffices that each person who shares the concept has some (possibly different) inferential procedures associated with the concept, and all persons are committed to the concept having a certain (perhaps unknown) metaphysical import. The correct 'definition' of concept is thus provided by an optimal, expert account of it. This implies that laypeople will defer to experts in matters of meaning and reference, although it is quite possible that even the experts never fully grasp the true sense and extent of the concept.

From the perspective of this paper, the major flaws in both overviews are two: First, semantics for distinct domains of objects are confounded, most tellingly for artifacts and living kinds. For example, in a series of seminal papers on the critical value of family resemblances and prototypes to which Smith and Medin pay homage, it is taken as a 'working assumption ... that in the domains of both man-made and biological objects, there occur information rich bundles of attributes that form natural discontinuities [and] these bundles are both perceptual and functional' (Rosch & Mervis 1975, p. 586; also Anglin 1977). In a study of family resemblances, Rosch and Mervis claim for both domains 'empirical confirmation of Wittgenstein's argument that formal criteria are neither a logical nor psychological necessity'. Yet no experiment in this study dealt with living kinds as such. And apart from obvious conditions like 'is an artifact' and 'is a living kind', it seems all artifacts tested here and in Rosch's other studies have necessary functional, if not perceptual,<sup>1</sup> properties,<sup>2</sup> whereas none of the basic or superordinate living kinds in the other studies have necessary functions.<sup>3</sup>

<sup>1</sup> An item may be called by a given artifactual term though it bears little (perceptual) resemblance to the prototype and exceeds the normal (perceptual) boundaries associated with the extension of that term: e.g. a varnished mahogany stump located in a living room may be a perfectly respectable table because it could well function as one. Conversely, a given object may bear a close perceptual resemblance to a prototype, yet fail to literally qualify as an instance of the artifactual term under which the prototype falls: e.g. featherweight ceremonial shields and souvenir boats may be intended only to represent defining functions, not to serve them. Thus, while artistic artifacts are still artifacts they may not fall under the terms whose items they are intended to represent. There are degrees, though: e.g. a 'perfect' prototype of a given object can have functional as well as representational value.

<sup>2</sup> Such artifactual terms as 'turkey dinner' and 'toy tree' are lexically characterized by perceptual as well as functional features. Note also that to count as being an artifact by reason of function served, an item need not itself be physically fabricated according to a plan. In some circumstances it need only be *displayed* consequent to a plan. This holds for natural objects served as food or displayed as art (cf. Wieland 1980).

<sup>3</sup> Consider in this light the following supposition as to the ways people's movements are allegedly relevant to a discrimination of 'natural objects': How do 'pursue, look up, squint, blink' distinguish 'bird' as Rosch et al. (1976) imply? Presumably, subject-initiated motor attributes are related to the space of human function and use and this space orients our apprehension of living kinds (Rosch 1978, p. 29):

What attributes will be perceived given the ability to perceive them is undoubtedly determined by the many factors having to do with the functional needs of the knower interacting with the physical and social environment... Thus, our segmentation of a bird's body such that there is an attribute called 'wings' may be influenced not only by perceptual factors such as gestalt laws of form that would lead us to consider wings as a separate part but also by the fact that at present we already have a cultural and linguistic category called 'birds'.

What little ethnolinguistic evidence there is, though, indicates that the discrimination of animal kinds, as well as their parts, occurs long before any folktales such as 'bird'

That results for artifact terms are extended without warrant to living kind terms (and vice versa) is not peculiar to recent cognitive psychology<sup>4</sup> or linguistic philosophy. Reputable philosophers of biology (Gilmour & Walters 1964), biologists (Sneath & Sokal 1973) and anthropologists (Brown et al. 1976) also reject inherent differences between logical and conceptual processes that determine the taxonomic ordering of living kinds and those governing conceptualization of artifacts. As a result, interesting findings for one domain, inconsiderably applied to another, risk being trivialized. Until independently assessed semantic domains can be *shown* similar, meaning should be assumed a motley, not a monolith.

A second misreckoning in both overviews is the assumption that analyses pertaining to the (non-phenomenal) nature of scientific concepts are necessary to a proper appreciation of our ordinary, commonsense conception of the everyday (phenomenal) world. Thus, on the view known as 'the theory of historico-scientific determinism' or 'the causal theory', to which Rey generally subscribes, the term's (unknown, possibly unknowable) nomic extension (as determined by a true scientific theory) establishes its (unknown, possibly unknowable) true meaning. But as shown below, it is pretty clearly *not* right that since 'the expert's ideal of understanding is the layman's, though the layperson may make less use of it', it follows that ordinary 'in the head' meanings which the layperson attaches to living kind terms provide no necessary truth conditions (Macanara 1982, p. 204 ff.). Neither is it the case that, commonsensically, 'there is no meaning dimension' for living kind terms; that is, 'the only dimension is empirical' in the sense that 'we should not expect to discover the

emerges (see Berlin 1972; Brown & Chase 1981). It is, moreover, plain that consideration of whether a given surface constitutes, say, a 'table-top' or a 'seat' depends upon context of human function in ways that judgments over what constitutes a 'head' or 'wings' does not. Discriminations of living kind parts are linked to the movement of organisms and the relational context of sympatric species, that is, species coexisting at the same locality. But this has little, if anything, to do with the *observer's* movements oriented by the context of human function and use. Quite plausibly humans, like pigeons and frogs, have innate species-pattern recognition schema that can be affected by the observer's situational environment. But there isn't the slightest evidence as to the influence of social context.

<sup>4</sup> To give a recent example: According to Markman and Hutchinson 1984, when children were asked to choose an object similar to a target (See this [blue]jay, birthday cake?) Find another one, they tended to choose a 'thematic' associate (nest, birthday present) rather than a 'taxonomic' associate (duck, chocolate cake). In contrast, when the instructions included an unknown word for the target (See this fep? Find another fep?), children preferred a taxonomic associate. The conclusion is that 'linguistic input may serve more generally to shape the conceptual structure of the child in the direction of greater taxonomic organization' p. 25. Yet, there was no control over differences in results for artifactual and living kind terms. But if, as Wierzbicka (1984) suggests, thematic relations are intrinsically more salient to artifactual than to living kinds, then Markman and Hutchinson's analysis is not fine-grained enough to support their conclusion as it stands.

necessary characteristics' of a living kind by analyzing the ordinary use of the term for that kind (Schwartz 1979, p. 304).

Science cannot simply replace or confute commonsense understanding, although it can elaborate novel understanding. To anyone but a scientist, it would appear odd indeed that the power of ordinary human meaning and reference should be attributed to science, rather than to 'popularly conceived conditions', even though it is science, rather than common sense, which is really quite peripheral to the immensely rich and varied conception of the world in which we and everyone else (including scientists) usually live their lives.

Let me scrutinize these two shortcomings a bit more closely. For Smith and Medin (1981, p. 5), artifacts and living kinds explicitly appear as a single conceptual domain: 'in almost all cases we will be concerned with object concepts — animals, plants, human artifacts.... This domain is a particularly interesting test case.' As with Rosch and company, and like many studies in 'semantic memory', VEGETABLE and FRUIT, which actually fall within the conceptual realm of artifacts, are equated with living kinds *per se* (see also Smith *et al* 1974; McCloskey & Glucksberg 1978; Loftus 1977). In general, when living kinds enter the space of concern with human function and use, such as eating, gardening (weeds and flowers), farming (beasts of burden), entertainment (pets, circus and fair animals), they cease to be of taxonomic importance (in the sense of 'taxonomy' to be discussed below).<sup>5</sup> For items that pertain to the conceptual space of human function and use then, there may well be 'unclear cases' of category affiliation, but this has no direct relevance to folkbiological classification.

According to Rey (1983, p. 248): 'tomatoes ... may be metaphysically clear cases of fruit (check the dictionary!); even though people may be (epistemologically) confused about them.' But the dictionary tells us that, in fact, 'fruit' has several senses. One sense is related to scientific botany, viz. 'the mature ovary of a plant'. Yet this is most certainly *not* the sense of 'fruit' that studies by Rosch, Smith or workers in semantic memory tested for, as any pragmatic disambiguation of sense in these studies would readily show. Rather, the sense of 'fruit', with which the literature on tomatoes and so forth deals pertains to the (artifactual) distinction between fruits and vegetables. This distinction is culturally, not metaphysically, determined; for, nothing falls under conjectured scientific laws by virtue of

<sup>5</sup> Take the concept FRUIT. Most mature speakers of English would likely contrast FRUIT and VEGETABLE. They would also probably agree that some examples of FRUIT, like apples and oranges, are more representative of FRUIT than are, say, tomatoes and avocados. In fact, in some contexts, such as being served as entrées rather than as dessert, tomatoes and avocados might be considered instances of VEGETABLE (Bright & Bright 1965, p. 258 n. 6). Thus, whether or not a given item instantiates the concept depends on the extent to which that item perceptually resembles focal types and on the degree to which it fulfils their usual sort of function.

satisfying the predicate 'fruit' as opposed to (or in conjunction with) the predicate 'vegetable'.

Moreover, insofar as cultural context is prescribed by what people think, there cannot be a 'metaphysical essence' for such kinds determined independently of our ways of (mentally) dividing up the world. Rey (p. 254), though, suggests otherwise: 'proper definitions of natural kind terms, on this view, are provided by the portion of science that deals with the respective natural kind.... Similar intuitions are readily available for artifactual terms ("carburetor", "viola de gamba").' Laypeople may well defer to expert mechanics, musicians or chefs in regard to artifactual affiliations; but whatever the decisions rendered, they depend on what experts actually *think* about such matters. For artifacts, metaphysics is thus indistinguishable from (expert) epistemology.

Concerning the confusion between science and commonsense, Smith and Medin (pp. 31–2) argue that modern biology has proven Linnaeus's 'classical-view' mistaken. This is supposedly because in science it is occasionally unclear to which taxon a given organism belongs, and there is no assurance that defining nomic features of structural homology or genetic constitution can be ascribed to all and only members of a taxon:

These developments in biological classification are relevant to a psychology of concepts.... The most likely place to look for classical definitions of flora and fauna is the language of biology, and to the extent that the classical view fails here, it will likely fail as a psychological theory as well.

But the fact that *scientific* data do not wholly conform to *commonsense* classification is hardly a compelling criticism of the classical view as it applies to ordinary language.<sup>6</sup>

Still, Rey notes: 'So far as the Classical View is concerned, the situation in biology seems to be even worse than [Smith & Medin] describe, given the on-going competition between [theoretically rival] taxonomies' (p. 239n). Yet this ongoing competition among theories in biological systematics may be no more pertinent to our understanding of everyday living kind terms than the ongoing controversy over the nature of various subatomic particles and forces is to our ordinary concept of BILLIARD BALL. This is not to deny that science and commonsense may interact. It is only to deny that our customary ontological commitment is necessarily bound to the scientist's. True, the open universe of science consumes the

<sup>6</sup> According to Linnaeus (1751 sec. 259): 'No man with any sense would ever say that ... the Maltese Dog, the Spaniel, the Short-haired Dog, the Mastiff, the Turkish Dog, the Barbet are [not] the same kind.' But the 'classical' view advocated by Linnaeus must not be confused with linguistic essentialism. For Linnaeus, essences were not, as Smith and Medin suppose, 'what we have called defining features', that is, nominal essences; rather, they were possibly unknown real essences.

closed world of commonsense, and a truly impersonal cosmic metaphysics which neglected to mention ordinary humankind's peculiar standpoint would suffer only a trifling irregularity. But when we inquire not into what there is in the universe at large, but into what sort of world we ordinarily conceive of, then we cannot wholly divorce epistemology from (ordinary) metaphysics.

Both overviews thus err on two crucial points: the relation between such diverse natural object domains as artifacts and living kinds, and that between science and commonsense. But there is a more elaborate misreckoning of the first point in Smith and Medin and of the second point in Rey. Actually, nothing in prototype or family resemblance theory requires a position on the second point, that is, the supposedly rigid bond between science and commonsense, and nothing in causal theory compels adherence to the first point, namely, the alleged conceptual similarity between artifacts and living kinds. In fact, eminent proponents of prototype and family resemblance theory, including Rosch and her associates, take no position on the relation between science and commonsense; whereas staunch advocates of the psychological reality of causal theory, such as Schwartz 1978 or Macnamara 1982 deny that artifact terms are extensionally anchored by presumed essences or that living kind terms are intensionally characterized by family resemblances.

Combining the insights of prototype and causal theories, Carey 1985 has recently argued for a model of conceptual development in children that is intended to take account of both the 'family resemblance' (pp. 19–20) structure and the 'domain-specific' (pp. 190–91) nature of ordinary biological concepts, 'which must be identified by the roles they play in theories' (p. 198). On her account, although pre-schoolers do appear to have a distinct ontological category of ANIMAL (cf. Gelman *et al* 1983) and perhaps PLANT (cf. Keil 1979), children before the age of nine or ten have no distinct category of LIVING KIND, that is, 'until conceptualization of the body as a machine supporting life allows the child to see animals as fundamentally alike and to conceive of plants as being like animals.' Only then do children supposedly come to view all and only living kinds as possessing underlying natures that make, e.g. tadpoles and frogs a natural sort, but not a cactus and a porcupine made to look like one (Keil in press).<sup>7</sup>

Like such causal theorists as Putnam 1975, Carey (1985, p. 19; cf. Keil in press) claims that 'even for scientific concepts like *living thing* ... no definition is immune from empirically driven revision'. So, even if one

<sup>7</sup> According to Carey, the category ANIMAL seems to constrain inductive projections of animal properties to only or mostly animals whatever the age of the children tested. But for preschoolers the extent to which a property is projected to other animals depends upon whether the children believe humans to have it. More specifically, the 'naive biology' of the animal domain is initially a prototypically based extension of the child's 'naive psychology' about why people organically function the way they do. Children

acknowledges, as most causal theorists do, that the conceptual organization of living kinds is radically different from that of artifacts, the belief that an ordinary 'natural kind' term is essentially a stand-in for the corresponding scientific term implies that any conceptual features associated with such a term are not necessary to it. This is presumably because (i) the definitional components of such a concept cannot be distinguished in principle from the empirical components, and (ii) no empirical component is safe from alteration under a change of theory 'characterized by the phenomena in its domain, its laws and other explanatory mechanisms' (Carey 1985, p. 201).

learn to correctly discriminate appropriate differences in the 'projectibility' of biological properties to other animals, and eventually to plants as 'the result of instruction, either formal or informal ... nature programs on television' (p. 199), etc. But how comes it that children ever acquire a sensibility to such instruction, which eventually limits the biological extension of naive psychology to animals and plants and *not* also, e.g., to plastic flowers or automated machines? Suppose the plausible candidates for (innate) a priori constraints on conceptual acquisition in this domain are restricted to a 'naive psychology' and perhaps the induction-limiting category ANIMAL: how, then, is a biology – naive or otherwise – that further extends to all and only plants even conceivable?

In fact, the evidence presented against there being a distinct ontological category of LIVING KIND before the age of nine or ten is hardly decisive. Consider the matter of inductive projection. What the experiments show is (i) that given new knowledge of an unknown property of flowers, young children 'were not inclined to consider either animals or inanimate objects relevantly similar to flowers' (p. 152), (ii) that given knowledge that an animal and flower had the property, there was no more likelihood of attributing that property to only living kinds than to inanimate objects as well (p. 155), and (iii) that young children have 'a great difficulty in rationalizing the inclusion of an animal and a plant into a single category' (p. 158). Concerning (i) and (ii), note that flower was the only representative of the category PLANT. But flower as such is not a plant *kind* in the sense that dogs and bees are kinds of animals. Flowers do not constitute a biological taxa either in lay or scientific botany. Moreover, when these children were told that the property belonged to a central (dog) and peripheral (bee) animal they were just as likely as adults to project the property to 'all of the new animals and the flower' (p. 158). Keil's (1983) study of semantic attribution also indicates that young children may restrict the span of some predicates (e.g. 'grows up') to living things. As for (iii), it may well be that young children are unable to *rationalize* the grouping of plants and animals in a single category, but this does not entail that they cannot *include* them in one. What the experimental evidence reported by Carey (also Keil in press) does indicate is that young children have not elaborated a notion of underlying nature that is invariant under morphological transformation, or a theory that integrates all specific natures into an overall view of biological Nature. But this does not mean they do not believe all and only living kinds have underlying natures, or that all and only living kinds are ranked into taxonomies. What evidence there is suggests that ranked plant taxonomies do arise early in the conceptual repertory of children, whether American (Dougherty 1979) or Mayan (Stross 1973). Furthermore, biological 'essences' may be presumed to underlie, e.g. the animal taxonomy of the Rotaflo of New Guinea (Dwyer 1976a), even though such presumptions do not always override morphological concerns that may group marsupial with placental mammals and separate juveniles from adults (when differences in appearance correspond to distinct behavioral and ecological proclivities) (Dwyer 1976b).

But ordinary folk, especially children, cannot wait on a fully developed understanding of biological theory to deal with important matters of everyday experience. That is why they supposedly rely on prototype judgments as conceptual heuristics for 'more or less' determining category membership. Ordinary biological 'stereotypes' (Putnam 1975), then, would differ from artifactual prototypes only inasmuch as it is the unintended and unforeseen discoveries of science, rather than the intended contrivances of human context, that motivate choices and changes in family resemblance structure.

As we shall see, there are striking cross-cultural uniformities in the structure of folkbiological classification. These uniformities are not family resemblance structures and do not arise from judgements of similarity or difference with respect to prototypes. It seems plausible that such uniform taxonomic knowledge, under socio-cultural learning conditions so diverse, results from certain regular and domain-specific cognitive constraints, though local circumstances undoubtedly trigger and condition the stable forms of knowledge attained. This knowledge, in fact, *constitutes* (the metaphysical setting of) our everyday world. But this setting need not commit one to belief in, or deferral to, a fully integrated theory of the phenomena conceptually represented in its domain. In numerous pre-literate cultures there is no evidence of any integrated theory of biological organization that encompasses the significant properties of all and only plants and animals. Yet, in these societies all and only living things are invariably organized into ranked taxonomies. It is not biological theory – be it 'naive' or scientific – that is responsible for the peculiar conceptual nature of the cognitive domain of ordinary living kinds. Rather, it is the taxonomic structure of living kinds, with its presumed structure of essential natures, that allows for the possibility of its integration into scientific theory, into a variously organized lay explanation of underlying regularity, or into no principled account at all.

### III Folkbiological taxonomy

Two decades of intensive empirical and theoretical work in ethnobiology seems to reveal that folkbiological classification is taxonomic, being composed of a rigid hierarchy of inclusive classes of organisms, or taxons. At each level of the hierarchy the taxa, which are mutually exclusive, exhaustively partition the locally perceived flora and fauna. Lay taxonomy, it appears, is universally and primarily composed of three absolutely distinct hierarchical levels, or ranks: the levels of *unique beginner*, *basic taxa* and *life-forms* (cf. Berlin *et al* 1973).

The unique beginner refers to the ontological category of plants or animals (cf. Keil 1979). Some cultures use a special marker for the unique beginner, like the numerical classifier *tehk* for plants, as with the Tzeltal Maya (Berlin *et al* 1974). Others use a descriptive phrase, such as 'the hairs of the earth' (*muk gobul nor*) for the Bunaq of Timor (Friedberg 1982). Yet

others have no word or ready-made phrase for PLANT or ANIMAL, although from an early age all humans seem to distinguish these categories conceptually, as indicated by studies of young Mayan (Stross 1973) and American children (Dougherty 1979; Macnamara 1982), New Guinea highlanders (Hays 1983), Indonesian natives (Taylor 1984), etc.

The basic level is logically subordinate, but psychologically prior, to the life-form level. Ideally it is constituted as a *fundamentum relationis*, that is, an exhaustive and mutually exclusive partitioning of the local flora and fauna into well-bounded morpho-behavioral gestalts. For the most part, taxa at this level correspond, within predictable limits, to those species of the field biologist that are spatially sympatric (i.e. coexisting in the same locality) and temporally nondimensional (i.e. perceived over at most a few generations), at least for those organisms that are readily apparent, including most vertebrates and flowering plants (Mayr 1969, p. 37). Since the frontiers of a cultural group do not always correspond to the boundaries of a set of sympatric species, partitioning can fall short of the ideal: e.g. migrating birds may be only intermittently or vaguely represented.

But this basic folk kind also generally conforms to the modern conception of the genus, being immediately recognizable both ecologically and morphologically. In fact, the scientific distinction between genus and species is largely irrelevant in any local area since most local genera are represented by a single species (Atran 1987b). This state of affairs underlies much of the confused and seemingly inexhaustible controversy over whether the genus (cf. Bartlett 1940) or the species (cf. Diamond 1966) constitutes the psychologically and historically primitive grouping. In this regard, some ethnobiologists refer to basic taxa as 'generics' (Berlin 1972) while others term them 'specimens' (Bulmer & Tyler 1968, p. 350). In reality, the species-genus distinction makes little sense to the common-sense vision of the world; for, in that world the species and genus are habitually coextensive. That is why I have designated the basic folkbiological kind 'general-specime'<sup>8</sup>.

The life-form level further assembles generic-specimens into larger exclusive groups (tree, grass, moss, quadruped, bird, fish, insect, etc.). Life-forms appear to partition plants and animals into a contrastive lexical field. This comprises a pre-theoretical *fundamentum divisionis* into positive

<sup>8</sup> Species are more often than not isolated from their congeners. So in a given locale, species and genus are usually perceptually equivalent. Species having congeners within the range of a single language community are apt to have distinct names if they manifest distinct morphologies and ecological strategies. Generally, congeneric species are given separate mononomial labels if the local biological family containing them is monogeneric or minimally polytypic. Historically, a taxonomic notion of species as a perpetually self-reproducing unity from a common seed was introduced by Cespalino's in the first treatise on systematic botany (1583, pp. 1, 28). A conception of the genus as a perceptually and mnemonically privileged rank immediately superordinate to the species was originally codified by Tournefort (1694, pp. 13–14). But these historical occurrences pertain to the necessity of constructing as world-wide classificatory system, and are not motivated by local concerns (Atran forthcoming).



features that are opposed along one or more perceptible dimensions (size, stem habit, mode of locomotion, skin covering, etc.) (cf. Brown 1977, 1979). By and large plant life-forms do not correspond to scientific taxa, while animal life-forms approximate modern classes, save the phenomenally 'residual' invertebrate groups ('bugs', 'worms', etc.).

One consequence of division and ranking at the life-form level is allowance for phenomenally and ecologically marginal groups to assume the status of monogeneric life-forms. The phenomenally peculiar (though taxonomically regular) characteristic of monogeneric life-forms is that they appear to have intuited aspects of both generic-specimens and life-forms. As generic-specimens their facies are readily perceptible at a glance. As life-forms they occupy a distinctive role in the economy of nature. Because they are so distinctive, they may be easily marked by diagnostic characters chosen from dimensions spanning other life-forms: e.g. 'cactus' for many American and French folk, as well as for the Aguaruna of Peru (*ikamas*), can be segregated from 'tree', 'grass' and the like by a rather simple set of diagnostic oppositions (cf. Atran 1985a).

These principles of folktaxonomy also account for the apparently 'unaffiliated' status of such generic-specimens as bats and whales in many cultures. Consider the now classic case of 'Why is the Cassowary not a Bird?' (Bulmer 1967) for the Karam of New Guinea. Most anthropologists, psychologists, philosophers and historians of biology who have commented on the matter accept Bulmer's claim that questions of 'cultural cosmology' must be considered in regard to the special status of 'isolated' generic-specimens like the Karam *kobit* (cassowary), that is, basic taxa not included under any of the more extensive higher-order taxa. The upshot of his argument for this paper is to undermine the claim that the epistemology of folkbiological kinds, as opposed to that of artifacts, neatly avoids involvement in human function.<sup>9</sup>

Admittedly *kobit's* referents are aberrant creatures by perceptible-ecological standards: it is an exclusively terrestrial bird that has no wings and doesn't fly; it has heavy, strong and very human-like leg bones, and a large bony casque on the top of its skull; and its behavior is correspondingly odd. Still, such factors allegedly do not suffice to explain its distinct status in Karam taxonomy. Why not? The reasons given are two: the cassowary is clearly a generic-specimen, yet it is isolated linguistically from other basic taxa and contrasted with the larger (life-form) groupings

<sup>9</sup> An extreme extrapolation from Bulmer's study is Wilson's (1982, p. 552) argument: 'It is an anthropological fact that labeling an ostrich a "bird" will strike the natives as more a distortion of meaning than any bomber dubbing'. Under certain circumstances an airplane may presumably be labeled a 'bird' (or the native equivalent) *even after it has been discovered to be an artifact*. But even if Bulmer had demonstrated the importance of social function in folkbiological classification (and he hasn't) nothing indicates the Karam would ever think of cassowaries as anything but *animals* - never as artifacts. It seems that humans, at any age, never take one and the same thing to be, e.g., a hawk and a hand saw (cf. Keil in press).

such as the flying vertebrates (bats and birds); and it has a privileged place in Karam ritual and mythology.

Now, should the Karam be presented an emu or ostrich they would, I imagine, be as likely to group them with the cassowary as would Australian aborigines (who value the emu) and African tribesmen (who put a symbolic premium on the ostrich). In this vein, Sperber (1975, p. 15) notes that although the Dorze of Ethiopia accord a singular status to their one snake taxon, *shosh*, when a Dorze travels to the nearby Rift Valley where many other species of the serpent are found he invariably applies *shosh* to them as well. Logically, then, there is no anomaly. The cassowary is simply a monogeneric life-form with but one known representative specimen, much as the aardvark is the only known species of the monospecific scientific order Tubulidentata. Monogeneric life-forms are exceptions that prove the rule. Nor must the undeniable socio-symbolic import of a taxon be considered in ascertaining a taxonomy's logical or substantive nature. Perceptual aberrance often serves to focus symbolic evocation, although it also frequently happens that perceptually aberrant monogeneric life-forms have no special symbolic status. Thus, Hunn (1975, p. 310) cites the Tzeltal folk taxon *majil ti bal* (armadillo) as an 'unaffiliated generic', but he emphasizes that 'no extraordinary ritual significance is attributed to these 'anomalous' animals'.

The claim for universal principles of folkbiological taxonomy is not for the universal status of particular *taxa*, only for taxonomic *categories*. Taxa are particular groups of organisms (e.g. dogs, trees). Categories are ranked classes of taxa (i.e. generic-specimen, life-form). Taxa and categories thus comprise different logical types. The categories of generic-specimen and life-form are universal. The delimitation and placement of particular taxa is not. Applied to a local biota, universal taxonomic principles (including presumptions of underlying natures) tend to yield basic-level groupings that correspond to biological species, at least for the phenomenally salient vertebrates and flowering plants.

Formal taxonomic constraints are deductive and inductive. The deductive constraint requires transitive inference as to group adherence: so, if one discovers a new kind of oak, then one knows it to be a tree. The inductive constraint allows for inferences as to the general distribution of taxonomic (and ancillary morpho-ecological) features throughout the local flora and fauna: so, if one discovers two organisms to possess a feature, then one may infer that the feature belongs to all organisms in the lowest ranked taxon containing the two. The inductive character of life-forms pertains primarily to the ecological and morphological relationships between species. Some cultures classify bats with birds, others place bats with quadrupeds, still others accord bats their own (monogeneric) life-form status, depending upon the bat's perceived relationships with the totality of the local fauna (and flora). Since the distribution of ecological boundaries and morphological characters varies from one locale to another, so may life-form boundaries. All the same, universal taxonomic principles operate whatever the case.

Taxa below the generic-specieme level, that is, at the level of what ethnobiologists call the folk-specific and folk-varietal, are culturally idiosyncratic in a way that generic-specieme and life-forms are not. In fact, it was this realization that led to the discovery of universal folk-taxonomic principles. In the early work on folk classification, the notions of 'lexical contrast' and 'levels of inclusion', rather than rank, served the anthropologist to assemble and distinguish folk taxa. For example, the level of terminal contrast, that is, the level at which there is no further lexical categorization, was thought to indicate the bottom, or basic, level of folk thinking about the world. But this purely ethnolinguistic artifice indiscriminately mixed monogeneric life-forms (cactus), monospecific genres (redwood), monovarietal specifics (red maple) and folk-varietals (spotted white oak). Using such data, anthropologists, biologists, linguists and psychologists erroneously concluded that there was no systematic correspondence between the extensions of biological and folkbiological taxa (Rosch 1975, p. 198; cf. Conklin 1962; Levi-Strauss 1962; Berlin *et al* 1966).<sup>10</sup> But although their extensions are frequently in approximate accord, their respective intensions are not. For scientific taxonomies – at least modern evolutionary ones – do not assume the necessity of fixed sets of manifest attributes nor do they presume the existence of fixed essences or natures.

#### IV *Nature and Necessity*

Folk-taxonomic structure for living kinds may be characterized thus:

1. Every natural object is either a living kind or not.
2. Every living kind is either a plant or an animal.
3. Each plant or animal belongs to one and only one basic taxon, *GS*.
4. No two basic taxa share all of their characteristic phenomenal properties; that is, for all *GS<sub>i</sub>*, and for any *GS<sub>j</sub>*, *GS<sub>i</sub>* lacks at least one readily perceptible feature characteristic of *GS<sub>j</sub>*.
5. Every basic taxon, *GS*, belongs to only one life-form taxon, *LF*.
6. For every *LF<sub>i</sub>*, there is at least one phenomenal property, *D<sub>i</sub>*, which

<sup>10</sup> 'Taxonomy' is often defined in the cognitive literature as: 'a system by which categories are related by class inclusion' (Rosch 1978, p. 30; Frake 1961; Kay 1971). But such notions of 'category' and 'taxonomy' fit neither folkbiology nor the Linnaean system. In biological classification, 'category' denotes a hierarchical *rank* and not the taxonomic grouping, or taxon, itself. Taxa are the elements of categories. Organisms are the elements of taxa. Species, genus, family and class are Linnaean categories. Generic-specieme and life-form are folkbiological categories. Disjoint taxa are termed higher and lower with respect to one another not because they stand in any inclusion relations, but because they are members of different categories: 'robin', 'pike' and 'gnat' are taxonomically related to 'cat' by reason of common generic-specieme rank (the same class of classes), and not by reason of inclusion of reference within some shared superordinate taxon (class). Analyzing the Linnaean system in terms of levels of abstraction and terminal contrast would lead to an equally odd collection of taxa (e.g. with monospecific families and orders placed at the same level of inclusion as species and monospecific genera).

is characteristic and diagnostic in the sense that for all *LF<sub>i</sub>*, *D<sub>i</sub>* is not characteristic.

Hierarchical ranking of living kinds is apparently unique to that domain. The field structure for artifacts, while often confounded with that of living kinds, is in fact quite different. For one thing, that taxa of the same category are disjoint precludes artifact groupings entering into ranked taxonomies. Not only can artifactual items belong to more than one 'taxon' within an inclusion series (a wheel-chair as both 'furniture' and 'vehicle', a piano as both 'furniture' and 'instrument'), but a given item may belong to different series (the same item as a crate used for packing furniture or as a table used as a piece of furniture). Of course, a community might also possess highly inclusive groupings of living kinds that overlap (e.g. the horse as both 'domestic animal' and 'herbivore' as opposed to 'wild animal' or 'carnivore'). But these (often pragmatic) socially parochial groupings are not to be confused with culturally ubiquitous life-forms (Atran 1987).

Also, artifacts fail to meet the deductive and inductive requirements of ranked taxonomies. As Hampton (1982) has shown, artifactual judgment may be non-transitive (e.g. 'car-seats' may be judged varieties of 'chair', but not of 'furniture, even though 'chair' is normally thought of as a type of 'furniture').<sup>11</sup> And it is hardly plausible that we *induce*, say, that tables and chairs are naturally four-legged from the fact that they are normally observed to have four legs.

Talk of artifact 'natures' is idle as well. For example, one and the same item can literally *be* an instance of 'waste paper basket' in one context and 'taboret' in another if oriented differently. It is the fact that artifacts are defined by the functions they serve, rather than by any inherent perceptual properties, that allows a given time to belong to different categories of artifacts in different circumstances. Thus, the itemized extensions of artifact categories may be indeterminate, but this in no way implies that the conceptual distinctions between artifacts are themselves fuzzy. Still less does it imply that 'labeled concepts do not provide a privileged, acontextual background system that is differentially tapped' by items actually called into use (Dougherty & Keller 1985, p. 170). Unlike living kind categories, labeled artifact concepts are not meant to classify particular items. Instead,

<sup>11</sup> Unfortunately, Hampton attempts to generalize these findings from artifacts to living kinds. To this end, he cites Randall's (1976) study of non-transitivity in folkbotany; however, many of Randall's examples evince the same confusion as semantic memory studies which confound, e.g., fruits with living kinds *per se*, and those of Randall's findings that do pertain to living kinds seem to confuse classificatory judgments with identification strategies (see Atran 1985a, 1987).

It is also worth noting that ranked hierarchies can be imposed artificially and Procrustean-style on nonliving kind domains. Biological taxonomy historically served as a model for the classification of chemical substances, and social – even artifactual – types are often rigidly ranked in various cultures. Humans, it seems, are apt to use natural means of ordering as expedients even where such means do not spontaneously apply. But such contrived uses are generally intermittent or incomplete, except in specially delimited ritual circumstances. To claim that social or artifactual hierarchies constitute the original models for biological taxonomy is really to put the cart before the horse.

they discriminate mental plans for serving functions – plans that are ‘differentially tapped’ when the same or different items are actually put into service. Since the defining characters of living kinds presumably ‘come from nature’ and the organisms themselves, living kind taxa are both well-bounded and well-defined.

Still, according to Rosch (1973, p. 111), just as: ‘some colours to which English speakers apply the word “red” are “redder” than others[.] some breeds of “dog” (such as the retriever) are more representative of the “meaning” of dog [than a Pekinese].’ But the analogy with colour is untenable. If a Pekinese is not properly, or only peripherally, a dog, what other kind could it be confused with? It may be difficult to decide where ‘red’ ends and ‘orange’ starts, or where ‘cup’ leaves off and ‘bowl’ begins (cf. Labov 1973, Kempton 1978); however, this is certainly not so for ‘dog’, ‘oak’ or any other such living kind. Perhaps there is a lesser degree of confidence in the judgment (especially the child’s judgment) that a Pekinese or Boston terrier is a dog and not another basic kind such as a cat, than in the judgment that a retriever or German shepherd is a dog rather than a cat. But Pekinese and Boston terriers cannot be anything but dogs.<sup>12</sup> For members of a living kind, but not an artifactual kind, are presumed to have essential underlying natures that operate in a causally identical manner regardless of the extent to which those members actually differ in physical appearance. It is this that underpins the taxonomic stability of ordinary living kind terms.

Yet, there seems to be an obvious objection to positing *logically necessary* features that uniquely characterize a living kind, since a given exemplar may always lack one or more of these typical features. In line with this objection, Ziff (1960, p. 184) claims that any attempt to define, say, ‘tiger’ in terms of such characteristic features as ‘being striped’ or ‘quadrupedal’ would lead to the absurd conclusion that a three-legged tiger which had lost its stripes is a *contradictio in adjecto*. As Fodor (1977, p. 148) remarks, cows differ from other animals in:

familiar ways, but it is no part of the MEANING of *cow* that cows say ‘moo’, and give milk, and look thus-and-so. These are not NECESSARY truths ... a cow that did not say ‘moo’ would still be a cow, and so would one that did not give milk or was purple.

If these objections hold, it would appear that only a ‘family resemblance’ of perceptible features can cover the full extension of many living kind

<sup>12</sup> This does not preclude prototypicality judgments being crucial to mnemonic processing strategies and perceptual verification procedures. It does disallow that they always necessarily pertain to the ‘meaning’ of terms. For example, although the numbers one, two and three are undoubtedly prototypical prime numbers they are no more nor less perfectly ‘prime’ than any other unfactorable numbers (cf. Arnsprung et al. 1983). Similarly, Rosch’s prototype indicator, ‘perches on trees’, no more defines ‘bird’, than, say, ‘wears a white smock’ indicates the meaning of ‘nurse’.

terms, and that for such terms all perceptible features are merely contingent (Mervis & Rosch 1981). The ‘ness’ (or linguistic equivalent in other languages) that seems to attach to ordinary living kinds would then appear to denote only the prototypical ‘configuration by which members of the category ... are recognized’ (Hunn 1976, p. 514). Instead of definitions there could only be ‘default values’ (Miller & Johnson-Laird 1976). But consider this anthropological observation as to why, e.g. a bean bag chair hasn’t ‘its’ legs though a legless tiger has ‘its’:

If an animal does not actually possess a feature ascribed to it by its definition, then it possesses it virtually: not in its appearance but in its nature. In such conditions it would be hard for empirical evidence to contradict the definitions of folk taxonomies (Sperber 1975, p. 22).

This is more than just a grammatical point: we can say of a tiger born without legs that it didn’t ever get *its* legs, but not of a bean bag chair that it didn’t ever get *its*. Sperber further implies that, say, a plucked bird is still thought to have *its* feathers ‘virtually’ just as a coneless pine ‘virtually’ has *its* cones.

This is why it is appropriate to say, e.g. that tigers are always ‘quadrupedal by nature’ while tables aren’t.<sup>13</sup> Thus, although for American folk being taller than a person and having a perennial stem seems to be criterial for being a tree (since non-woody palms and banana plants are classed as trees), insofar as bonsais are trees they are still ‘by nature larger than a person’. If predicting entailments in the lexical field of living kinds requires the abstract property ‘by nature larger than a person’ for marking a ‘tree’, then if a given referential token (exemplar) of the semantic type (the concept TREE) lacks the property’s perceptual correlate it is a contingent fact of the world and extrinsic to the semantic logic of living kinds.

Yet, how is it there is no anomaly? How can ‘quadrupedal’ ever be truthfully predicated of tigers that never have four legs? Knowing the meaning of a living kind terms must at least put us on the right track in search of an answer. For, if not, then what in the world can meaning conceivably be about? Since an object may fail to manifest all aspects of the meaning of the term under which it falls, to consider such aspects as conditions that must hold for the object to be seems counter-intuitive. The

<sup>13</sup> True, one might say that, e.g. a car lost ‘its’ windshield wipers or never had ‘its’ wipers installed. But wipers do not form part of the *definition* of car. Rather, most makes of automobile have wipers by design and not as part of their nature. It is not *necessary* that the plan for making automobiles include windshield wipers or that the design for tables include four legs. Designs and plans are intended to make proper use *possible* – to make it likely that, as a matter of fact, an artifact can serve its prescribed function (cf. Miller 1978). Automobiles with blow-dry windshields and legless tables that hang from the ceiling may be perfectly respectable cars and tables for which it would not make much sense to say that they virtually have ‘their’ wipers or legs or possess them by nature.

problem is not simply one of a three-legged tiger losing or growing back a defining property, but of perhaps never having it in fact. The knowledge problem for ordinary living kind terms thus appears to require at least this for its resolution: grasping the meaning of the term involves knowing that, in principle (if not how, in fact), instances which fall within the true extension of the term necessarily *would* manifest the attributes. This requires explication.

In line with Aristotle let us first make the following rough distinctions.

Of the whole set of attributes of a given living kind, there is a subset intrinsic to that kind and a subset incidental to it. For example, if it were true that tigers are large, striped felines seen only on Tuesdays, then being a feline and being large and striped would be among the intrinsic attributes of tigers and being seen on Tuesdays would be incidental to tigers. Thus, the counterfactual (7) is true:

7. It would not be a tiger, unless it were feline.

And (8) is false:

8. \*It would not be a tiger, unless it were seen on Tuesdays.

But what of (9)?

9. (?)It would not be a tiger, unless it were striped.

To answer, we are first obliged to note that the intrinsic attributes of a living kind appear to come in two grades, one more essential, but less well known, than the other. The essential trait, or nature, (e.g. the peculiar felinity of tigers) 'underlies' the better known perceptible features (e.g. being large and striped); that is, the perceptible features of a kind are *presumed* to be natural consequences of, or to be *naturally caused* by, the essential nature of that kind, even if the essential nature is largely unknown and perhaps effectively unknowable.

Now, being a natural consequence is dependent on a 'normality' clause. When an exemplar possesses an essential underlying trait, e.g. when Tio the tiger has its peculiar felinity essentially, then *whenever normal conditions obtain* it will necessarily manifest the perceptible features of its kind. So, we believe (10):

10. It is *natural* for tigers to be four-legged, that is what we expect to happen unless something physically *hinders* their normal maturation (cf. Aristotle *Physica* 199b15).

In other words, it is presumed that every individual of a given living kind has each an inner causal nature that normally leads the individual to mature in accordance with the morphological type of its kind. But it is possible to envisage situations where normal conditions do not obtain, where some external or internal event has broken the putative natural chain which physically links Tio's having a particular essential nature and Tio's being large and having stripes. Unusual environmental perturbation or natural malfunction, such as shaving Tio or Tio being a naturally deformed dwarf would be examples of such interference.

We ordinarily cope with such situations by distinguishing semantic properties of the kind from the naturally manifest perceptible features of particulars. We say of the shaven Tio that it has no stripes, but that it is

'striped by nature'. Hence, (9) now suitably modified as (11) by an implicit normality clause is true:

11. It would not be a tiger, unless it were striped by nature.

In other words, in virtue of its essential nature, the tiger is endowed with the *propensity to manifest* four legs, and will so manifest them in fact whenever normal conditions obtain.<sup>14</sup> In effect, it is to propensities (e.g. inherently quadruped) that semantic properties advert and not to manifest features *per se* (e.g. actually four-legged).

When fully analyzed, the common sense locution 'by nature' yields the ontological relation *because of* in the following sense: let *p* be a naturally manifest feature, and *P* the corresponding propensity; then:

12. Nec ( $P(x)$  & normal conditions obtain  $\rightarrow p(x)$ ).

A manifest feature *p*, as such, is not essential to members of a kind; rather if and when it does occur, then we say it 'necessarily occurs by nature' because of *P* (and of the underlying nature of which *P* is an integral part).<sup>15</sup> This commonsense notion of natural necessity is a conditional necessity: it will be necessary that if *p* is manifested, then if the organism has the propensity *P* to manifest *p* as a constituent of its essential nature, and normal conditions obtain, then indeed *p* is manifested. To discover the physical nature of individuals of a kind is thus to find out *what it is* for them to have the necessary features they would have were normal conditions to obtain. Because two generic-specimens cannot share all their natural features, they cannot have (or be discovered to have) natures of a kind.

But what happens when appearances do not cooperate with our typical (meaning-induced) expectations? How do we know to classify 'deviant' cases with 'normal' ones? The strategies for coping with these situations are basically two, one pertaining to environmental circumstance, the other to origin. Since local generic-specimens are in the main well-bounded geographically and ecologically, organisms that occupy the same niche and interact with members of one kind, and are clearly not identified with any other kind, are presumed to be of a kind with the organisms with which

<sup>14</sup> Propensities seem to come in two varieties. There are those that pertain to behavioral dispositions (e.g. for a dog to bark), and those which pertain to developmental capacities (e.g. for a lion to be large). The former are related to Lockean dispositions of an entity to display a multiplicity of distinct (though interrelated) modes of comportment at the same or different times without, however, requiring dispositions to have particular molecular and geometrical loci. The latter are closely akin to Aristotelian potentials without, however, implying privation (i.e. that the developed being is ontologically distinct from the undeveloped being). Realization of a developmental capacity for, e.g., mature tigers to be large causally requires that, e.g., tiger cubs are small. So, the smallness of a tiger cub is just as conditionally necessary to being a tiger (to tiger-ness) as the largeness of mature tigers, and both derive their necessity from the nature and propensities of a tiger that cause it to develop in the ways it should.

<sup>15</sup> Speaking of the nature of a kind is ambiguous. Whether people consider the kind itself, together with its nature, as a distinct being rather than (as with Aristotle) simply the nature of the kind of (lawful tendency in the) organism that organism remains moot. Both alternatives are compatible with daily experience at the commonsense macroscopic level.

they interact. If genealogical linkage is also observed, all the better. But for folk in pre-scientific societies, an awareness of genealogical linkage may not suffice in itself, especially if distinct morphological varieties (or even males versus females, juveniles versus adults) of the same species occupy separate niches. For the sophisticated layperson who is influenced by the scientific assumption of historical continuity between organisms of a kind, a plausible genealogical linkage appears sufficient to establish a transtemporal identity in kind between morphologically dissimilar individuals. But this genealogical linkage need not be exclusively phylogenetic as science would have it. For example, hawks and sparrows are American folk kinds, but they are morpho-ecologic kinds that are not phylogenically unitary; nonetheless, it suffices to know that some bird is the progeny or progenitor of a hawk or sparrow in order to be able to class that bird as a hawk or sparrow.<sup>16</sup>

What of deviance in respect of life-forms? One must realize that to class an organism under a life-form is not simply to presume it has the nature of that life-form; rather, it is to predicate of the organism membership of one or the other of the generic-speciemens that has as part of its nature the nature of its life-form. So, as Theophrastus (1916, p. 25) stresses, when mallow, which is normally not like a tree, grows tall like a tree it departs from its 'essential nature' (*physis*). In this case mallow is said to be merely 'tree-like' (*apodendromenti*), and not a tree 'by nature' (*phusei*). Similarly for American folk, although pussy willows may not always look like trees (especially in parts of the western United States), the fact that they are

<sup>16</sup> For certain artifacts (Ford cars versus Chryslers, Ete versus Parish prints) *origin* is salient and one might be tempted to argue that these artifacts have all or most of the properties held to be unique to living kinds. But this isn't so. Insofar as origin figures into notions of underlying nature, it implies transtemporal continuity of (some) constitutive matter, but not continuity in structure. Although the progeny materially 'derives' its structure from its progenitors, the identity of progenitor and progeny are distinct. Persistence of one does not entail persistence of the other. As for Fords made at Ford plants, or groceries bought at grocery stores, necessity of origin concerns only place of origin and existence of a causal line from that place to the present whereabouts of the artifact, with no necessary transference of matter along the line. True, for ironwork wrought from iron there is a notion of 'being-made-out-of' requiring material transference. But unlike the case for living kinds, it also requires persistence of the thing (iron) that does the becoming.

Also, for ordinary living kinds (and not bacteria identifiable only with respect to their growth cultures, or the painted roses of Wonderland) willful intrusion of circumstance cannot affect a nature in isolation. But mere reappraisal of circumstance may well alter an artifact's functional 'essence', even to the extent that well-intentioned, skilled craftsmen out to create one thing actually produce another. For instance, if a broken table is too flimsy to use, but folk in the current situation are in dire need of firewood or a bedboard and thus unlikely to seek repair, there may be a tendency to deny the object in question is still literally a 'table' and to assert it is something else. Given the role of circumstance, then, artifactual terms, unlike living kind terms, cannot stand for physical sorts. They cannot have physical natures, because the same material item may, in one circumstance, be one kind of artifact and, in another (possibly concurrent) circumstance, be a different kind.

considered willows, and that willows are considered trees, implies that pussy willows are nonetheless trees.

The nature of the evidence for a cross-cultural presumption of essential natures that underlie the stability of folkbiological taxonomy is threefold. First, such a presumption saves the facts, namely, the rigidly ranked structure of folkbotanical and folkzoological taxonomies observed the world over. Despite obvious variation among exemplars of a given folk kind, there is little doubt that perceptually prototypical and atypical individuals veridically instantiate the kind. To presume an underlying nature makes sense of this.

Second, in addition to being observationally adequate, a presumption of essence is a descriptively adequate rendition of native intuitions. For it renders such ethnographic observations as these immediately comprehensible: among the Tobelo of Indonesia (Taylor 1978/79, p. 224):

one often hears of a particular small sapling ... 'this weed (*o ruruibu*) is a tree (*o gota*)' (non-contrastive sense of *o ruruibu*); or of the same sapling ... 'this is not a (member of the) herbaceous weed class, it is a tree' (*o ruruibu* here contrasts with *o gota*).

And it does so by appealing directly to informants' views about the organization of the biological world: 'Thus, for Rofaifo [of New Guinea] species share an essence which ... immediately renders the idea, species, intelligible in a natural (biological) sense' (Dwyer 1976a, p. 433).

Finally, more than a glimmer of explanatory adequacy appears with the postulation of a such a presumption of physical essence. A universal belief in underlying nature constrains the character of semantic and conceptual knowledge for living kinds generally. It does so in a way that helps to explain the relatively uniform acquisition of such knowledge across cultures and the relative ease it is acquired by the children of any given culture (cf. Stross 1973; Macnamara 1982). At a limit, we need only once point to an animal (even in a zoo or book) to trigger the mapping of abstract and prior taxonomic schema onto the world. A presumption of essence plausibly enables the young child to rapidly fix a morphotype in mind despite very limited perceptual encounter with exemplars. This, in turn, allows the child to immediately classify and relationally segregate an example from instances of all other taxa.

Clearly beliefs about essences cannot be *post hoc*. They must constitute conceptually a priori impositions on the taxonomic ordering of perceptual stimuli into morphotypes. Otherwise, how would it be logically possible for the child to take an instance of experience and 'instantaneously' predict its extension to an indefinitely large set of complexly related instances? The child, it seems, just perceptually 'fills in' abstract taxonomic schema that are naturally at the mind's disposal.

Presumption of underlying nature is also a necessary condition for any appreciation of the temporal development (maturation) or spatial distribution (ecological productivity) of individuals of a species. It is thus

prior to any knowledge of the cultural utilizations of a species *qua* species. For example, the Inuit of East Hudson Bay have a dozen or so terms for various developmental stages of the reindeer linked to socially important uses (Roué 1986). But only the presumption that reindeers all have the natures of their kind allows this.

The cross-cultural disposition (and plausibly innate predisposition) to think this way about the organic world is perhaps partly accounted for in evolutionary terms by the empirical adequacy that presumptions of essence afford to human beings in dealing with a local biota. Knowledge of species *qua* biological species, and knowledge that organic individuals naturally fall into groups within groups, is a knowledge humankind shares, whether bushman, layman or scientist. Such knowledge determines the way we see the world and regulates our inductions about what we do not see.

In brief, living kind terms are conceived as 'phenomenal kinds' whose intrinsic nature, or (to use Locke's notion) 'real essence', is presumed, even if unknown. The essential role of possibly unknown underlying structure is to permit variation, and even change, in reference without a change in the corresponding phenomenal type classed in the dictionary. By incorporating auxiliary empirical knowledge (e.g. on metamorphosis, courtship behaviour, genetic structure, niche sharing, etc.) into encyclopedic theories of underlying traits, one may thus come to include, e.g. the caterpillar under the concept of BUTTERFLY and the tadpole under that of FROG, despite the fact that caterpillars and tadpoles share few, if any, perceptible features with normal frogs and butterflies. Theories of underlying traits may also facilitate acceptance of mutants, ecological variants and so on. As a result, we are able to accommodate unusual and novel aspects of the physical world to our conceptual system without compromising our basic stock of ordinary knowledge about everyday matters.

Usual variation requires little more than mere presumption of an underlying nature and knowledge of local ecology. But when exotic species are reported or actually introduced into the local scene on a large scale (as among sections of post-Renaissance Western society) a more elaborate notion of underlying natures and their origins is required to accommodate the new to the old without destroying the local taxonomic scheme. This elaboration may be partially deferred to science, but not wholly inasmuch as science often rides rough-shod over the local order. This overriding concern with maintaining the integrity of our ordinary knowledge about the living world distinguishes terms used by both sophisticated and unformed folk from those employed in fields of scientific expertise.

## V *Science and Common Sense*

Now, the causal theory denies that a principled distinction exists between ordinary and scientific ascriptions of meaning to natural substance terms. According to Kripke's (1972) account, a causal relationship is established

via an initial baptism ceremony between a natural substance term, a (first, typical or otherwise privileged) sampling of objects and an underlying physical trait. This link is preserved by a descendent linguistic community committed to the term's 'rigid designation' – whatever the experts may determine *that* to be. The extension of the term simply becomes the set of objects having that physical trait which is nominally responsible for the existence of those objects (with all their dispositional features). That trait however, is not determined by features of any concept that language users may attach to the term, no matter how appropriate those features might at first seem.

A problem with this scenario is that it fails to account for actual historical changes in reference. Thus, English colonists first used the term 'indian corn', that is, maize (indigenous to America), to denote a variety of 'corn', that is, wheat; just as *kaslan isim* (the Mayan term for 'Castilian maize'), that is, wheat, was used by Indians to signify a foreign variety of *isim*, that is, maize (Berlin 1972).<sup>17</sup> Over time, the unfamiliar kind proved to be of such significance in the local ecology that the generic-specieme term, 'corn', was eventually applied to maize only. In a similar development, the Mayan term for deer (*cih*) was ultimately transferred to sheep introduced by the Spaniards (with deer now referred to as a variety of 'forest sheep', or *te hiki' cin*). Such transference of reference is obviously not a mere conventional twist, but turns upon a complex appreciation of the local relations between species which folk taxonomy expresses. This precludes there being some originally privileged or otherwise isolated sample that historically somehow (and Kripke never does tell us just how) fixes reference.

It would, however, be consistent with rigid designation to parry the objection by arguing that, in calling maize 'corn', the colonists performed a new baptism which simply ousted the old. How and why this new baptism came about, although psychologically and anthropologically important issues, would not be philosophically pertinent. It suffices to note that terms designate rigidly only after baptism occurs. As in the case of proper names, it may not be essential that Kripke be called 'Kripke', but once he is called that the name picks him out rigidly.

All this is logically possible; however, it ignores history and hardly accounts for matters of psychological or anthropological fact, matters that concern the ways human beings actually think and behave. Historically,

<sup>17</sup> This seems to be a common folk procedure for labeling unfamiliar plants and animals (cf. Wilkowsky & Brown 1983). Usually, they are labeled with binomials of the sort 'foreign x', where x is the base name of the indigenous generic-specieme that the foreign species most closely resembles. Once an unfamiliar species is labeled and identified by perceptual analogy with the facies of the closest generic-specieme, however, it may itself assume the status of a generic-specieme over time. That is, increased familiarity with the once unfamiliar plant or animal may eventually lead to the structuring of a distinct facies which clearly places it on a par with other generic-speciemens. When this happens, the 'new' generic-specieme acquires a distinct unimodal label and drops the base name of the generic-specieme to which it was originally attached.



the rigid designation story gives not the slightest clue as to the reason for a term's shift in reference because it fails to acknowledge that there must be continuity in the reference of folk-biological kinds with respect to the whole local biota and the taxonomic structure which describes it. Psychologically, 'baptism' is just too impoverished a notion to imply much at all about mental processing.

A psychologically plausible correlate of baptism for ordinary kind terms might be deemed *expected ostension*, that is, the act of making apparent (one's intention to make apparent) what every human being would naturally expect to be manifest to anybody. Suppose humans are endowed with highly articulated cognitive faculties for 'fast-mapping' the world they evolved in, and for which their minds were selected. The 'automatic' taxonomic ordering of phenomenal species, like the spontaneous relational ordering of colours (Kay & McDaniell 1978), would then be a likely product of one such faculty.<sup>18</sup> Intuitively, ostension of living kinds works ever so well for humans in any culture, and at any age, because it calls our attention to what it is about the (biological) world that comes most naturally to everyone's mind, that is, (all and only) readily perceptible species. For living things, then, expected ostension invokes a semantic primitive, namely, 'species', in the sense of a readily perceptible kind of plant or animal that necessarily differs typically, and by nature, from all other species.

In Putnam's (1975, pp. 141–2) version of the causal theory, 'natural kind' terms,<sup>19</sup> such as 'tiger', can be given by an 'ostensive definition' with the following empirical presupposition: that the creature pointed to bears a certain sameness relation (say, *x is the same creature as y*) to most of the things which speakers in the linguistic community have on other occasions labeled 'tiger'. Leaving aside the historical fiction of baptism, the interesting claim is that this nomological relation of sameness may be 'operationally' determined by a conceptual stereotype.

<sup>18</sup> To put the matter in somewhat modified Fodorian terms: assuming a number of highly specialized sensory input systems (perceptual 'modules') and assuming further a number of basic, domain-specific concept-forming faculties, we might conjecture that basic faculties have a privileged access to the mental representations that input systems compute (Fodor 1983).

<sup>19</sup> It is not altogether clear what Putnam (or Kripke) include under 'natural kind'. Its domain seems to go beyond living kinds and chemical substances, to dispositions (fragile, malleable), diseases and even colours. But other causalists are more discretionary; Goossen (1977), for example, excludes dispositions and colours. Yet even granting these exceptions, it is debatable whether there is an ordinary language class of 'natural kind terms' encompassing living kinds and chemical substances. Kell (1979: 160) thinks not. He argues that an unlabeled node in our ordinary language tree of ontological categories includes artifacts and living kinds, but not chemical substances. He calls this class of objects 'substantial': meaning that each of their parts is not just a smaller instance of the whole. So, cutting a tiger or airplane in half doesn't get you two of a kind; but cutting a rock in two does. Kell is surely wrong: cloth, paper and a host of other artifacts of the sort are just as 'nonsubstantial' as rocks; and so are living kinds like thyme and coral. Still, there is no solid *ordinary language* evidence for a semantics restricted to living

Although Putnam offers no explicit account of how stereotypes actually operate, one which does ample justice to his proposal is as follows: first, select a sampling of exemplars as distinct from one another as is compatible with the taxon's stereotype. For maximum generality, foils chosen from other taxa could serve to demarcate limits of compatibility. One would then seek the most specific nomic relation that holds between every pair of exemplars and which cannot be extended to pairs containing a foil. Failing that, one would look for the nomic relation covering the widest variety of pairs in the sampling. Thus, assuming that whales were at one time ordinarily included under 'fish' and bats under 'bird', no nomic relation would have been available for all and only those pairs of exemplars falling under 'fish' or 'bird'. For example, if one of the exemplars of a fish-pair were a whale then the most specific nomic relation applicable to all fish-pairs would extend to the mammals. So, rather than obliterate the distinction in 'meaning' between 'mammal' and 'fish', here the preferred strategy would be to accept the nomic relation with the greatest partial scope, viz., that which characterizes fish exclusive of whales.

There are problems, however. Consider 'sparrow'. If scientific taxonomy is indicative of nomic relationship, then 'sparrow' as commonly perceived does not have a nomic extension: it is ordinarily taken to denote only species of plain-coloured birds in the finch family and birds of the genus *Passer* in the weaver family. To accord with science, ordinary users of 'sparrow' would probably not restrict the term to plain-coloured finches, since the most specific sameness relation applicable to plain-coloured finches also applies to goldfinches and canaries (which are normally perceived as foils to 'sparrow'). Moreover, the restriction excludes birds usually accepted as sparrows by Americans (e.g. the house sparrow), and typically viewed as such by the English. The alternative is to limit the term to the widest (i.e. highest ranked) grouping wholly included within the common extension of 'sparrow', such as the genus of weavers (as opposed to any one of the heterogeneous collection of plain-coloured finch species). But this would mean that our most typically American sparrows (e.g. the chirping sparrow) are not really sparrows at all, and that is plainly counter-intuitive.

Kripke argues that such considerations 'may make some people think right away that there are really two concepts ... operating here, a phenomenological one and a scientific one which then replaces it. This I reject' (1972, p. 315). His point is that commonsense terms must ultimately either prove co-extensive with some nomic kinds, or simply cease to be natural kind terms. Kripke fails to appreciate that the 'phenomenological concept'

kinds and chemical substances. It is even unclear whether 'inanimate non-man-made substances' compose a single conceptual domain. Do we presume 'water', 'air', 'earth', 'fire' and 'brimstone' have necessary semantic properties, or underlying natures, or both? There has been little serious study of the matter. But there is at least some experimental and cross-cultural work on artifacts and living kinds, if only enough to bring out the confusion that abounds.

may persist as an underlying trait term regardless of science's opinion on the matter. 'Hawk' and 'sparrow' persist as underlying trait terms because their usual denotations are readily perceived to be components of local nature; the Tzeltal Maya, for instance, recognize similar taxa (Hunn 1977, pp. 143, 190). The traits underlying common sense kinds need not be (or even include) nomic traits, though they may.

With pre-scientific folk there is usually, and with sophisticated laypeople seemingly always, a presumption of historical continuity between commonsense traits, or natures, to the effect that 'like begets like'. But this presumption of historical continuity does not, as with science, necessarily imply descent from a common ancestor: sparrows do beget sparrows but not in a phyletic line. Also, with sophisticated laypeople there is usually, and with pre-scientific folk seemingly always, a presumption that it is in the nature of a kind to bind its members together into an interactive ecological community (cf. Bulmer 1974, p. 12). So, genealogical and ecological criteria largely figure into a determination of the nature of a common sense phenomenal kind. Socially functional criteria, though, do not (Atran 1985a). Indeed, if ordinary living kinds were to depend upon social functions, Kripke could justify his claim that they then cease to be natural kind terms that denote underlying natures; but nothing of the sort happens.

'Tree' and 'grass' are cases in point. Once perfectly respectable taxonomic terms, they have now disappeared from systematics; however, unlike countless ill-fated terms for microscopic and extinct organisms which have since gone the way of phlogiston and the ether, they have not also vanished from common parlance. This is because trees and grasses are phenomenally, though not nominally, natural kinds. In other words, they plainly look as if they must be natural kinds, even though *scientifically* they are not. Supposing evolutionary taxonomy the best available representation of the true structural history of plants and animals, then trees and grasses are not central historical subjects. This is not to deny the ecologist's legitimate interest in the 'objective' correlates of their phenomenal properties, any more than it would be to deny the physicist's circumstantial concern with colour phenomena. But to extrapolate from Quine (1969, p. 127), cosmologically, trees and grasses would no more qualify as kinds than would colours.

Why, then, do we continue to adhere to these folkbotanical life-forms? A partial answer turns on the observation that such life-form divisions are apparently made on the basis of those habits of life that determine the place of each being in the local ecology which pertains to man's everyday life. Subjective appreciation of what plants are *relative to us*, however, will bear little relation to an objective appreciation of what plants are *relative to one another* once the bounds of the local environment have been transcended: trees are bigger than people, and grasses smaller; trees are where birds most often perch and grasses are where most mammals forage; trees determine exposure to sun and moisture – hence the density of other vegetation – and the possible habitats of many of the animals familiar to local folk. Yet such holistic ecological frameworks are scarcely of value in

organizing knowledge of the world outside the local economy of nature. It is for this reason that they were ultimately banned from taxonomy; for once devoid of local context they become 'scabrous' and 'lubricious' when used as frameworks for organizing flora world-wide (Linnaeus 1751 sec. 209).<sup>20</sup> That there is a definite anthropocentric bias in these pre-theoretical divisions cannot be gainsayed. But such (possibly even innate) bias can in no way be construed as a variant of some utilitarian or cultural viewpoint; 'tree' is no more derived from a functional preoccupation with wood than 'green' is derived from a cultural preoccupation with plants in general, and it is plain that children do not call a tree a 'tree' because it fills some culturally parochial function such as wood-use (Stross 1973; Dougherty 1979).

The layperson's stubborn adherence to the (phenomenal) validity of everyday kind terms thus markedly contrasts lack of prior commitment to the (non-phenomenal) terms of science.<sup>21</sup> Take 'animalcule', which first arose in the scientific and popular literature of the late seventeenth century as a natural kind term for all microscopic organisms (including spermatozoa). By the end of the following century this term had ceased to denote a natural kind. Although today the term barely lingers as a qualifier to a heterogeneous collection of micro-organisms (barrel animalcule, wheel animalcule, etc.), 'animalcule' is no more considered a natural kind term by scientist or layperson than is 'caloric'.

It is not that scientific developments cannot affect common sense appreciation of the perceptible world. Only, the theory of ordinary meaning is not directly related to scientific reference in a way that levels the distinction between the terms of common sense and science. Science may help convince folk in the eastern United States that the locust tree is not an acacia, but few folk would accept it as one of a kind with such other leguminous plants as vine-growing peas and beans, herbaceous clover or shrubby broom because it is not morphologically and ecologically of a sort with them. Actually, many people regard broom as a kind of bramble, while (other) bramble (raspberry, dewberry, blackberry) is usually associated with the rose family. In short, scientific compatibility is neither a necessary nor sufficient for determining whether one sort can be linked to another via an underlying trait.

Moreover, evidence of scientific incompatibility between sortals is neither necessary nor sufficient to indicate lack of an underlying trait compatible with commonsense realism. Under what circumstances does one decide when two sortal terms are connected to the same natural kind term in virtue of some common underlying trait (e.g. 'maple' as applied

<sup>20</sup> But compare sec. 153 on apprehension of life-forms as the product of our 'natural instinct'.

<sup>21</sup> Of course if there is no prior phenomenal concern to stand in the way, the layman may extend his ontological commitment by proxy to non-phenomenal terms ('cancer virus', 'electricity', etc.), and be willing to defer to any scientifically motivated decisions about the meaning (or meaninglessness) and reference (or null extension) of such terms.



to the red maple and the sugar maple) and when not ('wolf' applied to placental mammals of the dog family versus the marsupial wolf, *Thylacine*)? One such condition of 'compatible commonsense realism' may pertain to the fact that the largest grouping with an immediately recognizable morphological configuration, or facies, is usually restricted to the biological family or order. Thus, many a heterogeneous collection of species falling within a given family or order, if previously considered a phenomenal kind, is likely to remain so whatever the state of scientific knowledge. Beyond the level of family or order, the aspectual tie between members of a previous kind is generally too vague to preserve the kind intact without the aid of other apparent factors such as a shared role in the local ecology: thus, allowing dissociation of whales from fish, salamanders from lizards, marsupial from placental mice, lampreys from eels, hedgehogs from porcupines, etc.

Dissociations of this kind usually happen in either of two ways: at the life-form level, or at a level intermediate between generic-specieme and life-form. Such intermediate groupings are recognized by folk whether or not (in fact, mostly not) explicitly named. Mostly they correspond to groupings of a scientific family or order. But they do not constitute an exhaustive partition of the local flora or fauna. This is not surprising given the fact that associations of family and order are pocketed with gaps in any locality, unlike associations of species (generic-speciemes) and life-forms which form a morpho-ecologic quilt across a local area (Atran 1983). In contrast to taxonomically arrayed generic-speciemes and life-forms, the boundaries of these usually covert groupings are fuzzy, while the core is generally the recognizable facies of a modern family or order.<sup>22</sup> Accordingly, generic-speciemes only peripherally associated with such fragments are susceptible to dissociation. Thus, early English settlers in Australia probably combined an implicit grouping of marsupial and placental mice by weak association of morphological aspect. But today few Australians would think marsupial and placental mice are of a kind.<sup>23</sup>

<sup>22</sup> In fact, it was the attempt to form a determinate series of local fragments that inaugurated the 'natural method' in botany and zoology that lasted from Linnaeus to Lamarck. By looking to other environments for similar as well as different fragments, and by using such partial series drawn from many different environments, naturalists sought to fill in the gaps in each and every environment. But the benefits of this approach went far beyond local understanding, since completion of a local series implied completion of all other partial series. As a result, and for the first time in history, a universal (exhaustive and world-wide) morphological series could be envisaged (Atran 1983).

<sup>23</sup> New Guinea Highlanders today readily distinguish the family-level grouping of marsupial mice from the family-level grouping of placental mice, but they also hold to the belief that the placental dog is of a kind with the indigenous marsupial *Satanellus* (cf. Dwyer 1976a). Should other species of the placental dog family *Canidae* be introduced into the local area, however, it is likely that these New Guinea folk would themselves effect a dissociation as they did with mice. In a related sense, although Americans tend to assimilate the Old World hedgehog (an insectivore) with the New World porcupine (a rodent), science would probably be able to convince most folk that these animals of

Dissociation at the life-form level occurs most readily for generic-speciemes whose phenomenal affiliations with their respective life-forms are only marginal (although from a strictly logical standpoint there is no *taxonomic* anomaly). For the most part, vertebrate life-forms correspond to modern classes: mammals, birds, fish, etc. (Atran 1986). Perhaps since we ourselves are vertebrates, subjective (life-form) apprehension of vertebrates *as they appear to us* turns out not very far removed from a morphologically objective appreciation of vertebrates. But folk naturalists and scientists interpret these groups differently. For folk, such groupings represent locally distinct modes of life, ecological roles and morphological frames (Brown 1979). Most often, folk views on the extension of these life-forms differ from scientifically construed extensions of the corresponding classes in regard to what, from the folk viewpoint, are rather marginal cases (and for natural history traditionally the most problematic): bat, ostrich, whale, etc. Indeed, as often as not these marginal groupings assume a separate life-form status of their own, hence they are treated as monogenic life-forms. As such, they differ from the other life-forms in being minimally polytypic and in having a role which is restricted, rather than wide-ranging, in the overall local economy of nature. Given the tenuous phenomenal associations such groups thus bear to other groups in respect of life-form, modern folk are amenable to a shift in their life-form status which would be in conformity with scientific opinion.

These (tentative) conditions on 'compatible commonsense realism' argue against any straightforward reduction of commonsense terms: phenomenal conditions appreciably constrain the intricate relationship between common sense and science. In this respect Dupré (1981, p. 69) aptly stresses that: 'the general picture is of science as a largely autonomous activity, in spite of subtle and pervasive interactions with the main body of language.' But when assessing the relationship between the classifications of organisms in ordinary language (OLC) and in scientific taxonomy, Dupré errs in imposing a 'functionalist viewpoint' on OLC:

A group of organisms may be distinguished in ordinary language for any number of reasons: because it is economically or sociologically important (Colorado beetles, silkworms or Tsetse flies); because its members are intellectually intriguing (trap-door spiders or porpoises); furry and empathetic (hamsters and koala bears); or just very noticeable (tigers and giant redwoods).

<sup>24</sup> Different mammalian orders are no more of a kind than are any other pair of basically distinct mammals. In the case of the European versus American robin, though, the outcome is not so predictable. These two species of birds are both thrushes, but biologically no more of a kind than are the species of hawk or sparrow. Yet, unlike the European sparrow, the European robin is by and large absent from America. This absence might more readily dispose American folk to the scientific opinion that 'robin' ambiguously denotes a disjoint extension rather than (as in the case of 'sparrow') a single extension of biologically disparate sorts.

Porpoises do intrigue folk. They are evocative because, along with whales, that are phenomenally aberrant at the life-form level, however, their evocative or symbolic power is not the *source* of their taxonomic status. Hamsters may be empathetic, but their taxonomic status is no different from that of the rat, raccoon or house cat – or tiger. And when, as with silkworms, there is a finer appreciation than is usually the case with invertebrates, it is not because of their functional importance as such. Rather, it is because constant and direct intervention by man actually creates an isolated role for them in the local environment, which makes them phenomenally salient. The anthropocentric bias, it seems, is fundamentally cognitive and phenomenal, not functional in a socially parochial and pragmatic sense.<sup>24</sup>

At the generic-specieme level of classification (to which Dupré's examples above belong) ethnobiologists largely agree that considerations other than morpho-ecologic affinity are rather peripheral. Apart from invertebrates and cryptogams, when disagreement between science and commonsense occurs at the generic-specieme level it is usually because the reproductive criteria of the scientist conflict with the morpho-ecological criteria of the layperson. Most often these two sets of criteria overlap at the generic-specieme level, but when they do not, ordinary folk generally refrain from acceding to scientific opinion. In regard to most bugs and non-flowering plants, as these are not salient species-wise, they are often lumped into residual taxa. But the residual character is phenomenally compelling enough to resist scientific pressure to restrict, say, 'moss' to the bryophytes or 'worm' to the annelids.

In sum, the epistemological aim of lay taxonomy differs from that of scientific taxonomy. Both provide a classification that is a key to underlying nature, but they have different presumptions about what that nature is. So far as I can see, there is no scientific advance which would *necessarily* lead to a restructuring of lay taxonomy. Moreover, we need not, and

<sup>24</sup> This is not to deny that culturally parochial considerations may affect the interactions between science and common sense. It is only to deny that an underlying nature would primarily be sought for just those functional properties assigned the members of a kind. An underlying nature can be held physically responsible for whatever ancillary functional features a kind may have; however, it cannot be presumed to underlie all and only such features as long as the kind is considered a natural kind (rather than an artifact-related kind of biological origin like game animal, berry, flower, etc.). Thus, the alliaceous plants (onions, scallions, garlic, chives) are ordinarily thought to form an (intermediate) kind not because of some functional nature, such as 'being edible' or 'being spicy', but because of their readily perceptible properties (including characteristic odor and taste, but also bulbous form, distinctive sheathing, basal leaves, umbellate flowers, etc.). So, e.g., if a poisonous form of onion were found it would be no less literally an 'onion' than a poisonous mushroom is a 'mushroom'; although cloves may be functionally of a kind with garlic, phenomenally they are not, etc. Nevertheless, while function is not, in the first instance, responsible for people grouping the onion with its morphological allies, functional considerations may influence the layman's refusal to consider alliaceous plants of a kind with lilies, although they belong to the same botanical family. But surely one reason why folk would more readily admit an association of, say, tulips to lilies than onions is because tulips just *are* morphologically closer to lilies than onions.

normally do not, seek to reconcile the fact that, e.g. 'tree', 'bug', 'thistle', 'butterfly', 'hawk', etc. have no biologically valid extensions. When it does happen that some commonsense taxon falls within the extension of a scientific taxon, then folk *may* come to accept a modification of the commonsense taxon so that it corresponds more closely to the scientific taxon (e.g. including whales with the mammals and excluding bats from the birds). But this is only possible if the scientific notion can be given a phenomenal expression, and if expert opinion is not incompatible with everyday common sense realism.

Yet, even if such an accord proves feasible, the lay concept still diverges from the scientist's, that is, the folk taxon differs in structure from the evolutionary taxon. For example, the layperson may regard 'mammal' as an 'air-breathing, warm-blooded, milk-giving' creature. The evolutionary taxonomist, however, may well view 'Mammalia' as a portion of the genealogical nexus of evolution. As such, it would be a term for a logical *individual* localized in space and time and not, as with the layman, a term for an eternal class (Ghiselin 1981). Axiomatizations of evolutionary theory (or at least the formalized subset known as 'selection theory') that are compatible with Mayr's (1982, p. 273) widely accepted definition of the species as a 'reproductive community of populations ... that occupies a specific niche in nature', use species terms as primitives for denoting populations conceived as individualized 'Darwinian subclans' (Williams 1985). In any event, things that evolve cannot have essential natures (cf. Hull 1978). Ordinary folk, though, tend to view living kindhood essentially and *sub specie aeternitatis*.

It is debatable whether rival scientific theories are logically comparable (Popper 1963) or incommensurable (Kuhn 1962). But whatever scientific epistemology is adopted there is a methodological presupposition to the effect that science seeks to decompose and explain the known in terms of the unknown. Commonsense distinguishes itself from science by aiming principally to maintain the familiar composition of the world and, if necessary, to assimilate the unknown to the known.

Science and common sense thus do not presuppose the same ontologies,<sup>25</sup> and their respective semantic frameworks deal, as it were, with

<sup>25</sup> There is one way science could assign ontological status to phenomenal kinds, namely, as kinds restricted to 'human ecology'. These are kinds whose lawfulness consists in the relation of the human kind to 'its' physical surroundings; that is, to those aspects of the environment that naturally manifest themselves to us. Most philosophers of science, though, would be unwilling to admit kinds of such a limited scope as genuine ontological posits. From an omniscient standpoint, as in much philosophy of science, phenomenal kinds are at best faint echoes of reality – starting points on a road that must either end in nomological truth or a false lead. Those cognitive psychologists who accept the causal theory's claim that folk tacitly steer towards omniscience might do well to reflect on the cognitive payoff. What possible conceptual advantage would assimilation of everyday understanding to scientific knowledge actually afford our species? Indeed, to presume humans aspire to omniscience (if not to all particulars, at least to nomic structures) *even when* day to day life most requires being down to earth seems a most extravagant faith.

'different' worlds.<sup>26</sup> In the study of living things, science requires the postulation of the existence of sets of entities to which we have no direct access in sense experience, and that cannot be reduced to those to which we do have access. These sets of entities include, *inter alia*, the various genetic components of evolutionary processes that are theorized to underlie regularities in the organic world. But, e.g., sparrows or trees can be of a kind in lay thought even though science does not recognize them as 'natural' (phyletic) kinds. That science does not recognize sparrows or trees as genetically lawful kinds does not eliminate them from the ontology of everyday life, which layperson and scientist alike ordinarily adhere to. In this respect, science's failure to displace or influence folkbiological taxonomies does not result from the layperson's conceptual ignorance, obstinacy, lethargy or naivety. It owes rather to the fact that folk 'automatically' come to systematic grips with the everyday world in ways that are substantially different from the scientist's relentless endeavour to understand the cosmos.

## VI Conclusion

This essay in cognitive anthropology has focused on two basic issues. The first is: whether there are domain-specific cognitive universals that account for the peculiar kinds of regularities apparent in folk systems of knowledge and belief world-over; or whether those regularities are the product of general processing mechanisms that cross such domains as living kinds and artifacts. To claim, as I have, that living kinds are everywhere ranked into transitively structured taxonomies, with no other natural-object domain so structured, favours the former alternative. The latter is implied in the opposing contention that categorization of all natural objects centres upon prototypes, and that the perceptual nature of prototypes as well as the somewhat indefinite extension of their range is partly determined by functional considerations of use and context.

The second issue concerns the following: whether, in ordinary use of substance terms, humans naturally aspire or cede to indications of (scientific) omniscience regarding the universe at large, including its astronom-

<sup>26</sup> In this paper I have specified only the structure of folk taxonomy. Elsewhere, I have analyzed in detail the considerable historical changes in the developing structure of scientific taxonomy and in the ever-pervasive areas of overlap between folk and scientific classifications (Atran 1985b; 1986a). But since Aristotle first conceived a means to unify into one Nature the diverse natures of folk biological kinds, the referents of the terms used to describe the lay world and the scientific universe have been functions of diverging conceptual frameworks.

ical, microscopic and evolutionary dimensions; or whether, under normal constraints of everyday life, humans principally seek to better know their phenomenal world by rightly denoting just its natural kinds, that is, those kinds – including living kinds – we most readily apprehend owing to the natural selection of our cognitive make-up. The 'causal theory' of 'historico-scientific determinism' implies that all human knowledge-seeking aims at omniscience; for when interpreted as a theory of cognition it makes commonsense meaning necessarily dependent on scientific reference. By contrast, here it was argued that no such necessary connection exists in day to day life.

CNRS UA 882

Laboratoire d'Ethnobotanique et d'Ethnozoologie  
Muséum d'Histoire Naturelle de Paris

Ms. accepted November 1986

## References

- Anglin, J. 1977: *Word, object, and conceptual development*. Norton.
- Aristotle 1980: *The physics* [Physical] (2 vols). P. Wicksteed & F. Cornford (trans.). London: Heinemann.
- Armstrong, S.; Gleitman, L.; and Gleitman, H. 1983: What some concepts might not be. *Cognition*, 13, 263–308.
- Atran, S. 1983: Covert fragments and the origins of the botanical family. *Man*, 18, 51–71.
- Atran, S. 1985a: The nature of folkbotanical life-forms. *American Anthropologist*, 87, 298–315.
- Atran, S. 1985b: Pre-theoretical aspects of Aristotelian definition and classification of animals. *Studies in History and Philosophy of Science*, 16, 113–63.
- Atran, S. 1986: *Fondements de l'histoire naturelle*. Brussels: Complexe.
- Atran, S. 1987: The essence of folkbiology: a response to Randall and Humm. *American Anthropologist*, 88.
- Atran, S. forthcoming: Origins of the species and genus concepts. *Journal of the History of Biology*, 20.
- Bartlett, H. 1940: History of the generic concept in botany. *Bulletin of the Torrey Botanical Club*, 47, 319–62.
- Berlin, B. 1972: *Speculations on the growth of ethnobotanical nomenclature*. *Language and Society*, 1, 63–98.
- Berlin, B. 1978: Ethnobiological classification. In *Cognition and categorization*, E. Rosch & B. Lloyd (eds.), London: Erlbaum.
- Berlin, B.; Breedlove, D.; & Raven, P. 1966: Folk taxonomies and biological classification. *Science*, 154, 273–75.

- Berlin, B.; Breedlove, D.; & Raven, P. 1973: General principles of classification and nomenclature in folk biology. *American Anthropologist*, 75, 214-42.
- Berlin, B., Breedlove, D.; & Raven, P. 1974: *Principles of Tzeltal plant classification*. New York: Academic Press.
- Bright, J. & Bright, W. 1965: Semantic structure in Northwestern California and the Sapir-Whorf hypothesis. In *Formal semantic analysis*, E. Hammett (ed.), American Anthropologist Special Publications: vol. 67.
- Brown, C. 1977: Folk botanical life-forms: Their universality and growth. *American Anthropologist*, 79, 317-42.
- Brown, C. 1979: Folk zoological life-forms: Their universality and growth. *American Anthropologist*, 81, 791-817.
- Brown, C. & Chase, P. 1981: Animal classification in Juchitan Zapotec. *The Journal of Anthropology*, 1, 61-70.
- Brown, C., Kolar, J., Torrey, B., Truong-Quang, T.; and Volkman, P. 1976: Some general principles of biological and non-biological classification. *American Ethnologist*, 3, 73-85.
- Bruner, J.; Oliver, R.; & Greenfield, P. 1966: *Studies in cognitive growth*. New York: Wiley.
- Bulmer, R. 1967: Why is the cassowary not a bird? *Man*, 2, 5-25.
- Bulmer, R. 1974: Folk biology in the New Guinea Highlands. *Social Science Information*, 13, 19-28.
- Bulmer, R. & Tyler, M. 1968: Karam classification of frogs. *Journal of the Polynesian Society*, 77, 333-85.
- Carey, S. 1985: *Conceptual change in childhood*. Cambridge, Mass.: MIT.
- Cesalpio, A. 1583: *De plantis libri XVI*. Florence: Marescot.
- Conklin, H. 1962: Lexicographical treatment of folk taxonomies. In *Problems in Lexicography*, F. Householder & S. Saporta (eds), Report of the Conference on Lexicography (11-12 November 1960), Indiana University.
- Diamond, J. 1966: Zoological classification system of a primitive people. *Science*, 15, 1102-04.
- Dougherty, J. 1979: Learning names for plants and plants for names. *Anthropological Linguistics*, 21, 296-315.
- Dougherty, J. & Keller, C. 1985: Taskonomy: A practical approach to knowledge structures. In *Directions in cognitive anthropology*, J. Dougherty (ed.), Urbana: University of Illinois.
- Dupré, J. 1981: Natural kinds and biological taxa. *The Philosophical Review*, 90, 66-90.
- Dwyer, P. 1976a: An analysis of Rofaifo mammal taxonomy. *American Ethnologist*, 3, 425-45.
- Dwyer, P. 1976b: Beetles, Butterflies and bats: species transformation in New Guinea folk classification. *Oecania*, 14, 188-205.
- Fodor, J. A. 1983: *Modularity of mind*. Cambridge, Mass.: MIT.
- Fodor, J. D. 1977: *Semantics: Theories of meaning in generative grammar*. New York: Crowell.
- Frake, C. 1961: The diagnosis of disease among the Subanon of Mindanao. *American Anthropologist*, 63, 113-32.
- Friedberg, C. 1982: *Les Bunag de Timor et les plantes*, tome 4. Thèse de Doctorat d'État, Université de Paris V.
- Gelman, R.; Spelke, E.; & Meck, E. 1983: What preschoolers know about

- animate and inanimate objects. In *The acquisition of symbolic skills*, D. Rogers & J. Sloboda (eds). New York: Plenum.
- Ghiselin, M. 1981: Categories, life, and thinking. *The Behavioral and Brain Sciences*, 4, 269-313.
- Gilmour, J. & Walters, S. 1964: Philosophy and classification. In *Visitas in botany* (vol. 4), *Recent researches in plant taxonomy*, W. Turrill (ed.), Oxford: Pergamon.
- Goossens, W. 1977: Underlying trait terms. In *Naming, necessity, and natural kinds*, S. Schwartz (ed.). Ithaca: Cornell.
- Hampton, J. 1982: A demonstration of intransitivity in natural categories. *Cognition*, 12, 151-64.
- Hays, T. 1983: Ndumba folk biology. *American Anthropologist*, 85, 592-611.
- Hull, D. 1978: A matter of individuality. *Philosophy of Science*, 45, 335-60.
- Hunn, E. 1975: A measure of the degree of correspondence of folk to scientific biological classification. *American Ethnologist*, 2, 309-27.
- Hunn, E. 1976: Toward a perceptual model of folk biological classification. *American Ethnologist*, 3, 508-24.
- Hunn, E. 1977: *Tzeltal folk zoology: The classification of discontinuities in nature*. New York: Academic Press.
- Inhelder, B. & Piaget, J. 1964: *The early growth of logic in the child*. London: Routledge & Kegan Paul.
- Kay, P. 1971: On taxonomy and semantic contrast. *Language*, 47, 866-87.
- Kay, P. & McDaniell, C. 1978: The linguistic significance of the meaning of basic colour terms. *Language*, 54, 610-46.
- Keil, F. 1979: *Semantic and conceptual development*. Cambridge, Mass.: Harvard University.
- Keil, F. 1983: On the emergence of semantic and conceptual distinctions. *Journal of Experimental Psychology*, 112, 357-85.
- Keil, F. in press: The acquisition of natural kind and artifact terms. In *Conceptual change*, A. Marrat & W. Demopoulos (eds). Ablex.
- Kempton, W. 1978: Category grading and taxonomic relations: A mug is a sort of cup. *American Ethnologist*, 5, 44-65.
- Kripke, S. 1972: Naming and necessity. In *Semantics of natural language*, D. Davidson & G. Harman (eds). Dordrecht: Reidel.
- Labov, W. 1973: The boundaries of words and their meanings. In *New ways of analyzing variations in English*, C. Bailey and R. Shuy (eds). Washington: Georgetown University.
- Linnaeus, C. 1751: *Philosophia botanica*. Stockholm: G. Kiseewetter.
- Loftus, E. 1977: How to catch a zebra in semantic memory. In *Perceiving, acting and knowing*, R. Shaw & J. Bransford (eds). London: Erlbaum.
- Macnamara, J. 1982: *Names for things: A study of human learning*. Cambridge, Mass.: MIT.
- Markman, E. & Hutchinson, J. 1984: Children's sensitivity to constraints on word meaning: taxonomic versus thematic relations. *Cognitive Psychology*, 16, 1-27.
- Mayr, E. 1969: *Principles of systematic zoology*. New York: McGraw-Hill.
- Mayr, E. 1982: *The growth of biological thought*. Cambridge, Mass.: Harvard University.
- McCloskey, M. & Glucksberg, S. 1978: Natural categories: Welldefined or fuzzy

- set? *Memory and Cognition*, 6, 462-72.
- Mervis, C. & Rosch, E. 1981: Categorization of natural objects. *Annual Review of Psychology*, 32, 89-115.
- Miller, G. 1978: Practical and lexical knowledge. In *Cognition and categorization*. E. Rosch & B. Lloyd (eds). London: Erlbaum.
- Miller, G. & Johnson-Laird, P. 1976: *Language and perception*. Cambridge, Mass.: Harvard University.
- Putnam, H. 1975: The meaning of 'meaning'. In: *Language, mind and knowledge*, K. Gunderson (ed.). University of Minnesota.
- Quine, W. 1969: Natural kinds. In *Ontological relativity and other essays*. New York: Columbia University.
- Randall, R. 1976: How tall is a taxonomic tree? Some evidence for dwarfism. *American Ethnologist*, 3, 541-57.
- Rey, G. 1983: Concepts and stereotypes. *Cognition*, 15, 237-62.
- Rosch, E. 1973: On the internal structure of perceptual and semantic categories. In *Cognitive development and the acquisition of language*, T. Moore (ed). New York: Academic Press.
- Rosch, E. 1975: Universals and cultural specifics in categorization. In *Cross-cultural perspectives on learning*, R. Britslin, S. Bochner & W. Lonner (eds). Hastead.
- Rosch, E. 1978: Principles of categorization. In *Cognition and categorization*, E. Rosch & B. Lloyd (eds). London: Erlbaum.
- Rosch, E. & Mervis, C. 1975: Family resemblances: Studies in the internal structure of natural categories. *Cognitive Psychology*, 8, 382-439.
- Rosch, E., Mervis, C., Gray, W., Johnson, D. & Boyes-Braem, P. 1976: Basic objects in natural categories. *Cognitive Psychology*, 8, 382-439.
- Roué, M. 1986: Taxonomie et savoir INUIT concernant le caribou. Paper presented at the Colloquium 'Les Inuit du Nouveau-Québec: Appropriation du milieu naturel et savoirs autochtones'. Centre National de la Recherche Scientifique, Paris, May 28-30, 1986.
- Schwartz, S. 1978: Putnam on artifacts. *Philosophical Review*, 87, 566-74.
- Schwartz, S. 1979: Natural kind terms. *Cognition*, 7, 301-15.
- Smith, E. & Medin, D. 1981: *Categories and concepts*. Cambridge, Mass.: Harvard University.
- Smith, E., Shoben, E. & Rips, L. 1974: Structure and process in semantic memory. *Psychological Review*, 81, 214-241.
- Sneath, P. & Sokal, R. 1973: *Numerical taxonomy*, New York: Freeman.
- Sperber, D. 1975: Pourquoi les animaux parfaits, les hybrides et les monstres sont-ils bons à penser symboliquement? *L'Homme*, 15, 3-34.
- Stross, B. 1973: Acquisition of botanical terminology by Tzeltal children. In: *Meaning in Mayan languages*, M. Edmonson (ed.). The Hague: Mouton.
- Taylor, P. 1978-1979: Preliminary report on the ethnobiology of the Tobelorese of Hamahera, North Moluccas. *Majalah Ilmu-ilmu Sastra Indonesia*, 8, 215-29.
- Taylor, P. 1984: 'Covert categories' reconsidered: Identifying unlabeled classes in Tobelo folk biological classification. *Journal of Ethnobiology*, 4, 105-22.
- Theophrastus 1916: *Enquiry into plants*, A. Hort (trans.). London: Heinemann.
- Tournefort, J.-P. 1694: *Éléments de botanique*. Paris: Imprimerie Royale.
- Vygotsky, L. 1965: *Thought and language*. Cambridge, Mass.: MIT.
- Wierzbicka, A. 1980: Defining art and artifacts. *Philosophical studies*, 38, 385-89.

- Wierzbicka, A. 1984: Apples are not a 'kind of fruit': the semantics of human categorization. *American Ethnologist*, 11, 313-28.
- Williams, M. 1985: Species are individuals: Theoretical foundations for the claim. *Philosophy of Science*, 52, 578-90.
- Wilson, M. 1982: Predicate meets property. *Philosophical Review*, 91, 549-89.
- Witkowski, S. & Brown, C. 1983: Marking-reversals and cultural importance. *Language*, 59, 569-82.
- Ziff, P. 1960: *Semantic analysis*. Ithaca: Cornell University.