ple are perfectly aware of the fact that animals procreate much as we do, and that they often live in family groups having some analogy with our own.

The hypothesis that we have an innate disposition to classify according to the sort of hierarchy that Atran describes ought to be taken very seriously indeed. Our ancestral environments were certainly such that they provided strong selection pressures in favor of being able to classify animals and plants. But how much detail has been bred into us, in other words, how much the oxymoronic notion of a "synthetic a priori" is to be taken seriously, is open to serious doubt. As a fall-back position we can suggest that the system in question is the product of capacities and dispositions that are widely used and that folk taxonomies are the product of something that is more of a "general purpose" adaptation. How about folk anatomies and folk sociologies?

Another point that bothers me is Atran's emphasis on induction as the basis of classification. Naive inductionism as a basis for explaining the goals and methods of scientific classification has long been discredited, but it seems to be a part of the folk epistemology that naive scientists are inclined to apply. Scientists identify the underlying causes of the order that they make out of nature, and restructure their classifications accordingly. The reasons for preferring a classification that is etiological, rather than phenomenal or symptomatic, should be obvious to every scientist. In folk, and pre-Darwinian taxonomy the underlying causes are perhaps assumed to exist, but the groups are treated for the most part as if they were natural kinds. Nonetheless, it would seem that however much folk taxonomies distinguish between sows and boars, they put them together with piglets, much as they associate men, women, and children. One might wish to take a harder look at folk taxonomy, and how it interacts with scientific, with the hypothesis in mind that we have some sort of "module" for causality.

Folk biology and external definitions

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Abstract: Atran's thesis has strong implications for the doctrine of externalism in concepts (Fodor 1994). Beliefs about biological kinds may involve a degree of deference to scientific categories, but these categories are not truly scientific. They involve instead a folk view of science itself.

The way people understand and categorize the natural kinds of the biological world has been the subject of much heated debate in psychology. There have been those such as Keil (1989), and Atran himself, who have argued for a specialized conceptual module designed specifically to attend to theoretically relevant information in the stimulus input to identify categories of living things. Hence the evidence for very early differentiation of living and nonliving kinds by infants, and of the later use of deep causal structure rather than surface similarity in making inductive generalizations.

Equally influential has been the notion of psychological essentialism introduced by Medin and Ortony (1989), which is the hypothesis that people believe folk-biological kinds to have essences that have a causal role in producing the similar physical phenotypic appearance and structure of the organisms of a particular type. Belief in such essences appears to be much stronger for biological kinds and inorganic substances than for other natural language terms such as artifacts. (The evidence for how tightly people may hold such beliefs has recently been challenged, see Hampton 1995; 1998.)

Both of these psychological approaches lend themselves to an alliance with the philosophical doctrine of *external definitions,* expressed in the recent *BBS* target article by Millikan [A Common Structure for Concepts of Individuals, Stuffs, and Real Kinds 21(1) 1998]. The human mind/brain is assumed to have evolved to be able to "track" externally defined types. Put simply, our cognitive representation of a class has to be specified independently of what it is that the representation represents. Cognitive science must first determine the real nature of kinds in the world and then describe the ways in which people represent those kinds psychologically and how they come to acquire such representations. Defining the kinds purely in terms of internal conceptual representations can lead to a multitude of problems (Fodor 1994).

Atran makes the excellent point that neither of these theories of concepts may take for granted that there is a real world of scientifically respectable categories such as *species* that can be used by the theorist to ground our conceptual categories of biological kinds. First, to provide an account of evolution of different forms from a common ancestor, it is clearly not possible for a biological kind to have a fixed essence. Second, folk-biological terms such as *tree, sparrow,* or *fish* often do not correspond to monovalent terms in any scientific account. Third, different scientific purposes may require different taxonomic structures. Whatever it is that external definitions are intended to offer to a theory of concepts, it is clear from Atran's arguments that it is *not* a grounding in the real nature of the world – at least not as it is understood by current science.

Atran's comparison of Mayan and American folk biology is interesting, but ultimately frustratingly difficult to interpret. The research described here (sect. 1.2.2.1) is perhaps illustrative of the difficulties of cross-cultural research in general. To make the inductive task meaningful for the two cultures, it was apparently necessary to ask the Itzaj about susceptibility to diseases of three essential parts of animals or plants, whereas the Americans were asked about the likelihood of a type having a particular disease, protein, or enzyme labelled by a letter. Materials clearly have to be made comparably meaningful for the two groups, but there is a danger of circularity here, if by rendering the materials equally meaningful one also renders their inductive power the same. In this research, there were notable differences in responding between the groups, which Atran tends to ignore in favor of outlining the similarities.

In Western culture, it appears that there is an important influence of what we might call "folk science" on the use of folk-biological terminology. One owes some deference to what one may believe (or may have been taught) science has to say about living kinds. Children are taught in school that whales are not fish (although they are not taught that there is no such category as fish). At the same time, individuals must defer to the naming practices of their linguistic community, which will themselves frequently vary from context to context.

It is interesting that, with the ready availability of wild life films on television, it is no longer necessary for people to accept many of these categorizations on trust. To see whales caring for their calves, surfacing for air, and singing to each other is to understand in a direct way about their important difference from other creatures of the deep. We no longer need to defer to science; we can base our knowledge on evidence that we ourselves can also understand. Kalish (1995) asked people to say whether a dispute about the class (e.g., "elephant") to which some animal belonged was one that could always be settled as a matter of fact, or whether it was a matter of a difference of opinion that could not be resolved. He found that more than 25% of the judgments about animals were considered unresolvable by fact. Language use therefore has an uneasy relation with science. We may be prepared to alter our categories when science directs us to knowledge that is incompatible with our existing usage, but only in cases where that knowledge is seen as relevant. By and large, language users retain the right to use terms in ways that are subject to social negotiation within a much broader society, in which the scientific community is but one small voice.