Book Review

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Genes, Categories, and Species: The Evolutionary and Cognitive Causes of the Species Problem.—Jody Hey. 2001. Oxford University Press, Oxford, U.K. 217 pp., 8 figs. ISBN 0-19-514477-5. £ 34.95.

"Jody Hey has something new and illuminating to say," we are told by John Maynard Smith in the foreword to and once more on the back of *Genes, Categories, and Species*. This illuminating news about the nature of species is spread over 14 chapters and organized into three parts. Part I, appropriately named "The Hidden Question," starts with a question that is somewhat unusual for (just another?) book about species: instead of asking "What is a species," Hey asks, "What is it that we require if we are to resolve the species problem?" The answer is given in the second chapter: The species problem is the result of biologists' unwillingness to realize that the underlying difficulties are not ones that, even in principle, could be overcome by gathering better field data or by more debate. Part II develops and explains this answer. To recast these six chapters as a three-step argument, the cause of all the species trouble is that (1) the word *species* is used for both natural entities and for their mental representations; (2) the former can have diffuse and even partly unknowable boundaries, whereas the latter do not; and (3) this is because evolution has designed our cognition in such a way that we both think and speak typologically. If the species problem is unavoidable, however, the practice of biologists, especially in evolutionary biology, phylogenetics, and systematics, is directly affected. This situation is addressed in Part III, where Hey gives some advice on "Living with the Species Problem."

Even though *Genes, Categories, and Species* arrives at some questionable conclusions, it is hard to quarrel about the insights summarized above. Most of the book's theses, far from being especially controversial, are quite agreeable. Therefore, the main question seems to be how new are these insights really, and how illuminating is their presentation?

To start with the latter question, Hey does not succeed in presenting his ideas in an especially comprehensible way. The intention of many passages remained enigmatic to me, even after reading them several times. Generally, and unfortunately, Hey devotes too few pages to some topics and too many pages to others. Some topics are literally mentioned in passing only, sometimes in one (not self-explicatory) paragraph, sometimes merely in half a sentence. This brevity is especially problematic when the topics are both crucial for the argument and far from obvious. Here are six of the most striking examples:

Why should it be that things that gradually become two cannot be counted (p. 81)? Individual organisms become two gradually (and not only ones that use binary fission as means of reproduction), but this has never posed serious problems for their countability. To be sure, becoming two takes more time for species than for organisms, but Hey does not mention why counts such as "*n*, plus a number of *m* which currently are in the process of becoming two" should be entirely unworkable. Instead he holds that this "counting problem" invariably leads to the conclusion that there does not exist a certain number of species (p. 186).

Why should the fact that genomes do not have simple branching histories imply the same for species (pp. 141, 150)? It is perfectly possible that parts have more complicated histories than their dichotomously branching wholes, and Hey does not even attempt to show why this should not be the case for species. This is not to say that reticulation and partial boundaries do not occur, only that those phenomena are far from sufficient for Hey's conclusion. The same category of logical mistake—"what is true for parts must also apply to the whole"—seems to lie at the heart of Hey's statement, "But if a [species] definition is promoted as an unambiguous tool . . . then it cannot be made up of vague words [such as *population*]" (p. 166f). It is perfectly possible to give a precise definition of one concept (e.g., species) using another diffuse concept (e.g., population) as long as the vagueness of the latter concept is restricted to its own level and does not matter at the level of the former concept.

Why (and if so, how?!) should the number of existing species depend on whether they originated sympatrically or allopatrically (p. 155)? Of course, different modes of speciation may affect speciation rate, but how is knowledge of modes of speciation meant to influence the number of species that (now or at any other slice of time) already exist?

Why is it "hard to imagine . . . a self-imposed right-think code of speech and thought about species" (p. 111)? According to Hey (pp. 48ff), statements such as "the baby is a girl," "I am a person," and "he ate a lobster" are quintessential examples of statements of class membership: "individual *x* is (or ate, etc.) a member of category *y*." However, even if we assume that this is the only possible interpretation of those sentences, this fact does not doom language as a whole: if we replace "I am human" with "I am a part of the human species," as far as I can see

the purported typological nature of language has been overcome.

Why are higher taxa not ontological individuals, or particulars, when (according to Hey's definition on p. 74, but contra his own conclusion on pp. 174–176) taxa are *evolutionary groups* and *evolutionary groups* are individuals? Does this mean—to rewrite a species-level rhetorical question of his own (p. 153)—that Hey answers in the affirmative to the question "If there is a monophyletic taxon (e.g., Mammalia) out there, and no systematist is around to diagnose it, will it cease to be a clade?"

Why should geographic isolation be sufficient for species status (pp. 165f, 170), and at which temporal and spatial scale should this understanding of allopatry be made workable? Do I cease to be a part of the human species as soon as I am not engaged in copulation with another part of the species, or when I am alone in my living room, or when I am alone on a mountain top, or only when I am alone on a mountain top and the future will show that I shall not come back home alive?

I am not entirely sure that Hey is really wrong in these examples. However, all these quotes illustrate cases where Hey's conclusions do not follow from the too short accounts he gives of these topics.

However, other topics are covered by quite a large number of pages, given their relevance. For example, it never became clear to me why his discussion of the cognitive and linguistic sciences (especially in chapter 9) had to be so long. Hey probably would not agree with me that this chapter (and allusions to the same subject in other chapters) is unnecessary to make his point. However, if he is correct that "recurrence is the phenomena [sic] that necessarily underlies all natural kinds" (p. 112) or that "recurrence is the cause, and category is the effect" (p. 113), the same also must apply to our perception of individual organisms. What we know about another person is the result of recurrent impressions, yet we would hardly say that the mental representation of that person is a natural kind rather than an individual. Or if Hey or somebody else holds this view, it would still not explain why we have a species problem but we do not have an organism problem. His discovery that "there may be circumstances where our references to natural kinds are defective because they are encumbered by our cognitive apparatus" (p. 59) can be recast, saying exactly the same about ontological individuals. The problem with those findings is not that they are invalid but rather that they are not unique to species, and they therefore do not explain what Hey wants them to explain.

Regarding the ontological status of species, Hey does not seem to be able to decide himself entirely what solution he should opt for. The species-as-individual view holds that species cannot be classes (or categories), whereas some biologists and philosophers argue that species are classes (Ruse) or that they are both individuals and classes (Mahner). Hey's treatment of this question resembles the latter view in some places (e.g., pp. 49, 52, 59f, 105) but resembles the species-as-individuals view in other cases (e.g., pp. 101, 183, 185). The reason for this ambiguity might be that Hey from time to time makes the same mistake that he criticized others for committing and, in summarizing the consequences of his thesis towards the end of the book, urges us not to commit, viz. mixing up species (as entities) and species taxa (as mental representations).

Thus some ideas expressed in the book may well be illuminating, but they are not expressed in an illuminating way. This brings me to the second question: How new are the insights presented? For most of the ideas presented in *Genes, Categories, and Species*, I would recommend other sources that are more clearly written, more focused on relevant aspects, and/or less muddled with inaccuracies. To give a rather subjective selection of titles, I would suggest Ghiselin's *Metaphysics and the Origin of Species* (1997) for ontology of species and his "Categories, Life and Thinking" (1981) concerning our innate tendency to think typologically, Hull's (1997) ideal species concept for the ultimate cause of the species problem, i.e., the incommensurability of the diverse expectations that different biologists have of the noun *species*, Ax's attempts (e.g., 1984) to explicitly distinguish between species entities and species taxa, and Wiley and Mayden's (2000) critique of the phylogenetic species concepts.

This book also contains thoughts that are both new and illuminating, and most of these are presented in the last chapter. Among them is the radicality with which the author argues for keeping separate real species (entities) and species taxa: because species taxa cannot be expected to correspond more than very crudely to real species and because we do not know in which cases the correspondence is good, one should stop pretending the opposite. As one of the consequences, taxonomists or ecologists should not present species counts. Counts of real species are impossible (as per Hey's previous arguments), whereas counts of species taxa are meaningless because they tell more about taxonomists than about nature. No matter whether or not one is prepared to share Hey's pessimism regarding the correspondence (or rather the inescapability of the noncorrespondence) between real and taxonomic species, his thoughts and proposals that follow from this conviction are definitely worthy of notice as both interesting and important.

Hey also makes several unnecessary mistakes or oversimplifications; unnecessary because the mistakes concern subjects peripheral to the main theme of the book. By trying to cover such a diversity of subjects, from ontology and epistemology via cognitive and linguistic sciences to evolutionary biology, systematics, and genetics, Hey obviously moves into some areas he should not have. His treatment of phylogenetic systematics (in his usage, this name refers to phylogenetic or classical cladistics as opposed to pattern or transformed cladistics) reveals that he is not really familiar with the different schools of cladism, especially with the classical one. The statement that "organisms are . . . but the phenotypic manifestation of their DNA genomes" (p. 70) is unnecessarily simplistic given recent theoretical advances in the understanding of developmental systems (Oyama et al., 2001). The contention that it is impossible to count "hidden replication events [i.e., extinct side branches in a phylogeny] by

tree estimation methods" (p. 134) is plainly false (Harvey et al., 1994). Willi Hennig was decidedly not among the adherents of a phylogenetic species concept (p. 151). The erroneous conception that the cladistic (version of the biological) species concept is circular is reiterated (p. 67f) and "documented" by referring to the very passage from Ghiselin (1997:97) that disproves the claim. When he later (p. 166) criticizes the biological species concept on the grounds that "a great many closely related species (by other criteria) are capable or have shown evidence of producing fertile hybrids," one wonders whose arguments are circular, indeed. Printing and grammar mistakes are slightly more common than one should expect.

"Do we really need another book about species?" John Maynard Smith asks in the foreword. His conclusion is, "I think we need this one." I remain unconvinced.

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