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# The PhyloCode: a critical discussion of its theoretical foundation

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**Abstract**

The definition of taxon names as formalized by the PhyloCode is based on Kripke's thesis of "rigid designation" that applies to Millian proper names. Accepting the thesis of "rigid designation" into systematics in turn is based on the thesis that species, and taxa, are individuals. These largely semantic and metaphysical issues are here contrasted with an epistemological approach to taxonomy. It is shown that the thesis of "rigid designation" if deployed in taxonomy introduces a new essentialism into systematics, which is exactly what the PhyloCode was designed to avoid. Rigidly designating names are not supposed to change their meaning, but if the shifting constitution of a clade is thought to cause a shift of meaning of the taxon name, then the taxon name is not a "rigid designator". Phylogenetic nomenclature either fails to preserve the stability of meaning of taxon names that it propagates, or it is rendered inconsistent with its own philosophical background. The alternative explored here is to conceptualize taxa as natural kinds, and to replace the analytic definition of taxon names by their explanatory definition. Such conceptualization of taxa allows taxon names to better track the results of ongoing empirical research. The semantic as well as epistemic gain is that if taxon names are associated with natural kind terms instead of being proper names, the composition of the taxon will naturally determine the meaning of its name.



Authors supporting phylogenetic taxonomy and phylogenetic nomenclature are in broad agreement (e.g., de Queiroz and Gauthier, 1990; Härlin and Sundberg, 1998) that this discussion began with Ghiselin's (1974) and Hull's (1976) proposal to view species as individuals, or composite wholes (de Queiroz and Gauthier, 1992, p. 452), subject to the part-to-whole relation. This contrasts with the more traditional view that species are classes or sets subject to the membership relation (see also Buck and Hull, 1966). The understanding is that classes or sets are tied to an essentialism that conflicts with an evolutionary world-view (e.g., de Queiroz, 1988; de Queiroz and Gauthier, 1990, 1992; see also Sober, 1984; Rosenberg, 1985; Hull, 1999). Hennig (1950) had already treated species and supraspecific taxa as individuals, i.e., as parts of a whole, where the whole is the tree of life (see Rieppel, 2003, for further discussion). The major motivation for the development of the "PhyloCode" (Cantino and de Queiroz, 2003) was to overcome the essentialism believed to be inherent in the Linnean classification, and to develop a systematization of nature (Hennig, 1950, 1966; Griffiths, 1974) that accounted for the individuality of species, or indeed taxa in general (e.g., de Queiroz, 1988, 1992). The debate of these issues has grown increasingly philosophical (e.g., de Queiroz, 1992; Härlin and Sundberg, 1998; Keller et al., 2003); the purpose of this paper is to clarify some of the issues currently debated in the systematics literature as they relate to the definition of taxon names.

## Species as individuals

Whether species, and taxa, are individuals, sets, classes, or natural kinds is a metaphysical (ontological) question, not an epistemological (empirical) one. In presenting his thesis that species are individuals, Hull (1976, 190, n. 9) "presupposed a particular philosophical outlook … which is a lineal descendant of logical empiricism." A full understanding of Hull's (1976)

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position would thus require an analysis of logical empiricism, which is beyond the scope of this paper. A brief sketch will have to do. The "first premise" introduced by Hull (1989, p. 80) in support of his thesis is that "the ontological status of theoretical entities is theory-dependent." This says that the term "species" is a theoretical term (Hull, 1989, p. 79), i.e., a term that refers to unobservable theoretical entities. We do not *see* "the species red cardinal", we only see passing "red cardinal occasions", each of which is a particular event. Events occur in a certain space—time region, and so are taken as particulars, i.e., individuals. The sum of such events constitutes the species, an individual of higher complexity. The ontological status of theoretical entities such as species flows not from biological practice, but from evolutionary theory. The metaphysical thesis that species are individuals (complex wholes) is thus decoupled from epistemic input: species may look like natural kinds "from an epistemological perspective … If so, so much the worse for the epistemological perspective" (Hull, 1989, p. 119). Evolution is a process that occurs in nature, evolutionary theory is an explanatory account of that process, i.e., a linguistic construct. Because, according to Hull (1976), species names function as proper names in this linguistic construct, species must be individuals. Hull therefore deals with semantic and metaphysical issues, not with epistemological (empirical) ones.

What's the alternative? If species were sets, classes or natural kinds, their names would be general names. and these, according to Hull (1976), cannot function in evolutionary theory as names of species or taxa. The reason is that for Hull (1989, 1999), classes, sets, or natural kinds are all abstract concepts with sharp conceptual boundaries, defined by essential properties and numerically infinite in their possible instantiations. Names of classes, sets, or natural kinds cannot therefore pick out evolutionary entities that are localized in time and space, that have un-sharp boundaries, that are devoid of essential properties, and that are numerically finite in their possible instantiations. By virtue of their essential properties, members of classes, sets, or kinds can be governed by universal laws of nature, but there is no universal law of nature known to govern the historically contingent evolutionary process, or, in other words, species names do not and cannot function in the formulation of universal laws of nature (Hull, 1989).

The term "individual" as used by Hull (1989, p. 183) "is a technical term from philosophy. An individual is a particular, a thing, denoted by its name and nothing else." This means that an individual is picked out by its proper name in every possible world in which the individual exists, but it cannot, nor does it need to be described for identification and re-identification across possible worlds. Here, Hull (1976) takes up Kripke's (1972) analysis of the semantics of proper names, and transposes it into a temporal context ("in this paper I am arguing that Kripke's analysis applies to the names of species': Hull, 1976, p. 179, n. 4). According to Hull (1976, 1989), species are subject to continuous change through time while remaining numerically identical (i.e., the same lineage between two speciation events). If a species name is a proper name, then, on Kripke's (1972) analysis, it will pick out the species it designates no matter how much the species changes through time, just as long as it remains numerically identical. But, because the species continuously changes through time, it cannot be described other than perhaps by a potentially infinitely large cluster of disjunct definite descriptions.

Conversely, if one were to describe a species, one would predicate properties of it. In the nominalist tradition adopted by Hull (1989), only spatiotemporally located individuals are real, whereas properties are abstract and universal concepts. Such properties cannot evolve, nor can substances instantiating them. Species therefore cannot have intrinsic properties (in the traditional sense), they cannot therefore be classes, sets, or natural kinds, but they can be evolving entities picked out by a proper name.

Inspired by a Popperian (1971) anti-essentialism (Grene, 1989), Hull (1999) categorizes the world in terms of a sharp contrast: universal classes, sets, or kinds versus spatiotemporally restricted individuals—*tertium non datur*. That to many does not seem right, and it certainly does not reflect biological practice (Grene, 2002; Keller et al., 2003). Whereas philosophical arguments can be construed to the effect that species, or taxa, are individuals, the results are highly counterintuitive. Do lions and antelopes have a "nature" that can be described, analyzed and talked about, as most biologists would argue, or is it the case that a detached and un-descript "lion part" chases a fleeting "antelope occasion" through the savannah (Grene, 2002)? "Surely the most fundamental lesson of evolution is that natures are histories, that they have origins and endings, but not that they do not exist" (Grene, 1990, p. 241). It is true that what Hull (1989, 1999) portrays in his own way as Aristotelian classes or natural kinds cannot evolve, but that does not mean that natural kinds cannot be historically delimited (Keller et al., 2003). In fact, a very good case can be made that both species (Boyd, 1999; Griffiths, 1999; Wilson, 1999) as well as higher taxa (Keller et al., 2003; Rieppel, 2005) can be conceptualized as "homeostatic property cluster natural kinds" (Boyd, 1991). This not only better reflects biological practice, but also shows species, and taxa, not just to be un-descript individuals picked out by rigidly referring proper names conferred upon them in a baptismal ceremony, but rather to be what they indeed are—the result of scientific theory construction (Rieppel, 2005, see below for further discussion).

## Definitions, synonymy and "rigid designation"

According to Hull (1976), species names function as rigidly designating (Kripke, 1972) proper names in evolutionary theory. According to de Queiroz and Gauthier (1992, p. 452), species, or supraspecific taxa, are individuals, i.e., composite wholes, their names therefore are proper names. The phylogenetic definition[1] of such proper names is said to fix their "universal and stable meanings" (de Queiroz and Gauthier, 1994, p. 28). "Universal and stable meanings" invoke rigid designation, but it remains to be clarified how the thesis of "rigid designation" relates to issues of definition, i.e., of synonymy, and how it applies to proper, as opposed to general names.

Definitions are linguistic devices. We cannot define the world, but we can define the meaning of the words we use to talk about the world. In so doing, definitions are typically meant to establish synonymy, i.e., *identity* of meaning, between the words being defined, and the defining words (de Queiroz, 1992, p. 300; Härlin, 1998, discusses various types of definition). In that sense, definitions establish *a priori*, necessary, i.e., analytic truth. Consider "all bachelors are bachelors", i.e., an identity statement of the form "a = a". Such a statement (called a logical sentence, expressing a logical truth) is trivially, *a priori*, and necessarily, true (i.e., the statement is true under every interpretation of the meaning of the term "bachelor"). Now consider "all bachelors are unmarried men". This statement (an identity statement of the form "a = b") is not true under all interpretations; some might say that bachelors are saxophone players rather than unmarried men. However, philosophers who defend the importance of definitions, synonymy and analyticity, argue that the statement becomes *a priori*, and necessarily true if "bachelor" is *defined* as "unmarried man", because such a definition establishes identity of meaning, and hence synonymy, of "bachelor" and "unmarried man." On this account, such a statement becomes true by virtue of the (defined) meaning of its components, not by virtue of what the statement says about the extralinguistic world (since definitions are ultimately conventional, such sentences are also called "stipulative" or "conventional"). This means that *analytical statements* say nothing factual about the extralinguistic world. Statements that do purport to say something about the extralinguistic world are called "synthetic" or "contingent", because their truth (or falsity) depends not only on the (defined) meaning of their components, but also on the way the world is. Most systematists and evolutionary biologists would presumably agree that statements which deploy taxon names, or statements about taxa, should be about the world, i.e., contingent statements, not analytic ones, i.e., statements that are true by definition.

With reference to the definition of taxon names, de Queiroz (1992, p. 296) stated: "I use 'define' in the sense of specifying the meaning of a symbol." The name of a taxon is a symbol, its definition is said to give its meaning. But the concept of "meaning" is used in a rather coarse-grained fashion in this context. Does "meaning" mean just the object that a proper name picks out (refers to), or does it also mean the way in which the name introduces the object it refers to into discourse? Indeed, Härlin and Sundberg (1998, p. 233) pointed out "the difference between meaning and reference is important in philosophy of language, but almost unknown in systematics."

The meaning of a proper name is often claimed (though not by Kripke, 1972) to have two components: reference (the capacity to pick out the individual the name refers to, e.g., the planet Venus), and sense (the way this individual is presented or introduced into discourse, e.g., presenting Venus either as "The Morning Star" or alternatively as "The Evening Star"). Under de Queiroz' (1992, p. 296) analysis, the "meaning" of a taxon name obtains from its being synonymous (de Queiroz, 1992, p. 300) with a description. Consider de Queiroz' (1994, p. 498) advocacy for Popper's distinction of an essentialistic versus a nominalistic interpretation of definitions directed against the use of analytic definitions (Popper, 1996, p. 277, n. 19). In de Queiroz' (1994, p. 498, italics added) rendition, "a nominalist starts with the *description* of a concept or entity that initially must be stated using many words and equates it, by means of a definition, with a single word or short phrase." This type of definition calls upon synonymy of a description (of a concept or an entity) with the term used to *abbreviate* the description (i.e., to name the concept or the entity). This is precisely the relation of synonymy that was famously rejected by Kripke (1972). Indeed, the thesis of rigid designation was borne out of the insight that proper names and definite descriptions are not *necessarily* synonymous. The difference that is of interest to systematists is one of semantics versus epistemology. "Bachelor" is semantically associated with "unmarried man", not empirically; empirically, a bachelor can get married. Kripke's (1972) thesis of rigid designation requires the rigid semantic association of a name with its referent. A proper name is directly and rigidly attached to an individual, like a tag, without any intervening descriptive qualifications (i.e., without any connotation). In contrast, a descriptive component is involved in the naming of a taxon, because such an association of a taxon name and its referent is not just a semantic or stipulative one, but rather an empirical one, i.e., the consequence of scientific theory construction. The definition of a taxon name's meaning by means of the definite description "*the* common

[1]Proper names cannot in fact be defined because they are not concepts. This issue is here set aside for the sake of the argument.

ancestor of *A* and *B*, and all of its descendants'' is stipulative. It establishes a semantic link between a name and a clade, no matter what else besides *A* and *B* is included in that clade. The meaning of the name would thus be exhausted by the clade it picks out: the taxon name would have reference only, but no sense. However, what constitutes a named taxon, i.e., a taxon's *composition*, may change as a consequence of ongoing scientific investigation, and with a changing composition the meaning of the taxon's name changes in two ways, i.e., what exactly the name picks out as part of the taxon, as well as how the name introduces the taxon into discourse (Nixon and Carpenter, 2000).

Proponents of ''rigid designation'' would have to deny the need to know what constitutes a clade at the time when a clade is named, or when its name is used. This is possible only because ''rigid designation'' is a semantic, the composition of a clade an empirical issue. Kripke's (1972) thesis of ''rigid designation'' can be read as an attack on Frege's (Weiner, 1999) distinction of reference and sense for proper names. The reference of a proper name is also called its extension (the way the name reaches out into the world), whereas the definite description under which—according to Frege—the name refers is called its intension (how a speaker intends to use the name), or simply its *information content* (Kirkham, 2001). Kripke (1972) denies the need for descriptive, i.e., identifying information about the referent for the competent use of a proper name.

Definite descriptions associated with proper names are *identifying* descriptions, conveying the information that is required to identify and re-identify the particular picked out by the name. For example, criteria of identification and re-identification are required to recognize that ''The Morning Star'' and ''The Evening Star'' are one and the same planet (Dummett, 1981), and similar criteria would seem to be necessary to establish species/taxon identity or difference. Such descriptive criteria may, indeed, serve to initially *fix the reference* for the name (Härlin and Sundberg, 1998, p. 234; allowed by Kripke, 1972), but, according to Kripke (1972), they ''do not serve the *semantic* role of determining the reference of a name'' (Luntley, 1999, p. 271, emphasis added). Accordingly, and in the context of phylogenetic nomenclature, Härlin and Sundberg (1998, p. 239; see also Härlin, 1998) accepted the idea that ''the definite description is thus not the meaning of the name, or, as Mill (1875) and Kripke (1972) among others have pointed out—names lack intension.'' From a Fregean point of view, Millian (or Kripkean) names are strictly ''senseless'' (in Frege's terminology) in that their meaning is exhausted by their referent. A changing description of the referent accordingly does not change the meaning of a Millian (or Kripkean) name.

John Stuart Mill's famous example (cited in Hanna and Harrison, 2004) for the function of proper names as rigid designators is ''Dartmouth'', the proper name designating ''*The* town situated at the mouth of the Dart.'' That same name would continue to be used for that same town even if an earthquake changed the course of the river Dart away from the settlement. According to Mill, ''proper names are attached to the objects themselves, and are not dependent on the continuance of any attribute of the object'' (cited in Hanna and Harrison, 2004, p. 127). This is precisely why Hull (1976) followed Kripke (1972) in the use of species names as rigidly designating proper names. Given the radical contingency of the evolutionary process, there is no ''continuance of any attribute'' of a species to be expected throughout time and space, but the name still picks out the same species, no matter how much it changes between its beginning and end in time. Proponents of the phylogenetic definition of taxon names similarly claim that once defined, the taxon name will rigidly designate the same clade no matter how much the description of its composition changes over time. That, to some (e.g., Nixon and Carpenter, 2000), does not seem right, and justifiably so for the reasons outlined below.

Hull (1976) considered species names as Millian proper names. This is a semantic, not an empirical issue, a distinction that is important if taxon names are to be accepted as Millian names in phylogenetic nomenclature (as by Härlin, 1998, p. 382; Härlin and Sundberg, 1998, p. 239). The difference can be illustrated by Kripke's (1972) use of Frege's example (Soames, 2003). Let's accept ''Phosphorus'' as the rigidly designating proper name for ''The Morning Star'', ''Hesperus'' as rigid designator for ''The Evening Star''. Let's also accept the principle of self-identity as unproblematic. On that account, ''Hesperus = Hesperus'' is trivially, *a priori*, and necessarily true, whereas ''Hesperus = Phosphorus'' is likewise necessarily true, but only *a posteriori* so i.e., *after* the empirical discovery that ''The Morning Star'' is ''The Evening Star''. Such *a posteriori* necessity is a *semantic* property of the proposition; it has nothing to do with empirical discovery. Millian names have no Fregean sense; their meaning is exhausted by their referents. *If it is the case* that ''Phosphorus'' and ''Hesperus'' refer to the same object, and if this object at the same time exhausts the meaning of these two names, then the meaning of these two names is the same (identical). This means that the two sentences ''Hesperus = Hesperus'' and ''Hesperus = Phosphorus'' express the same proposition (they ''say the same thing''), although many will intuitively find this somehow amiss. The reason for this intuition is the ''*if it is the case*'' clause. It indicates that the meaning of ''Phosphorus'' and ''Hesperus'' is not *merely* semantically determined. Instead, the referent, i.e., the planet Venus, is *empirically* associated with the two names, and that makes a difference. The names ''Phosphorus'' and ''Hesperus'' were not bestowed on Venus through mere ''ostension'' (or baptism), but

came to co-refer to Venus as a consequence of empirical discovery. The same is true for species and taxon names, which, as used by biologists, are not merely semantically (by stipulation), but empirically (through ongoing research) associated with the species, or taxon, they refer to (Rieppel, 2005). What matters, then, is not just whether a (proper) name behaves, semantically, as a rigid designator, but also how that name becomes attached to the object it refers to. Proponents of the phylogenetic definition of a taxon name are right in claiming that a name so defined (by stipulation, i.e., "the common ancestor of *A* and *B*, and all of its descendents") will rigidly refer to a clade that includes *A* and *B*, but their critics (e.g., Nixon and Carpenter, 2000) are equally right in saying that the competent use of a taxon name in scientific discourse (systematics, conservation biology, etc.) requires knowledge of the precise composition of a taxon, which is a matter of scientific discovery, not of stipulation.

## Ostensive definition of taxon names, and the "new essentialism" in systematics

Kripke's (1972) thesis of "rigid designation" is said to have inaugurated a new era of essentialism in analytic philosophy (e.g., Luntley, 1999; Stroll, 2000; Soames, 2003; Hughes, 2004; but see Hull, 1988, p. 496). Indeed, the literature on phylogenetic nomenclature does reflect a new discussion (e.g., Härlin, 1998, 2001) of the so-called "origin-essentialism" (Hanna and Harrison, 2004, p. 280; see also LaPorte, 2004), as it results from the definition of taxon names, or from the putative synonymy of taxon names *qua* proper names with definite descriptions (de Queiroz, 1992, 1995; Ghiselin, 1995, 1997).

Analyticity can be achieved through various kinds of definitions, such as extensional, intensional and ostensive ones. An extensional definition of a set proceeds by an exhaustive enumeration of all its members (for an extensional definition to work, the set must have its members essentially). An intensional definition of a class specifies properties that are singularly necessary and jointly sufficient for membership in the class. It is this latter type of definition that is generally linked to essentialism by taxonomists (e.g., de Queiroz and Gauthier, 1992), who hope to avoid it by the deployment of an ostensive definition of taxon names. Sundberg and Pleijel (1994, p. 20) for example stated that taxa "themselves cannot be defined, but it is possible to ostensively define their names by pointing to a monophyletic group related to the name." But as the discussion of the new "origin essentialism" in systematics will show, such a procedure does not avoid essentialism. Analytic definitions of any kind yield sets or classes marked out by essential properties (this is true for ostensive definitions even if the essential properties of a set or kind so defined remain initially unknown: Kripke, 1972; Putnam, 1996).

The phylogenetic literature is replete with statements that proper names can only be ostensively defined (the possibility to *define* proper names is rightly contested by Härlin, 1998, p. 383; 1999, p. 2202). Commenting on the "ostensive definition" of taxon names, de Queiroz and Gauthier (1990, p. 309f) specified: "an evolutionary ostensive definition … consists of pointing to a clade, that is, to an ancestor and its descendants … This can be accomplished, verbally or on a branching diagram …". However, an ancestor and its descendants cannot be pointed at verbally, because "pointing at" is achieved with the index finger, not with names or words. Furthermore, a "branching diagram" is not an individual, but a pictorial representation of a hierarchy of parts of a whole (Hennig, 1950), which can also be a hierarchy of natural kinds (Rieppel, 2005). To make an ostensive definition work, one must point at particulars that occupy a certain space-time region, i.e., parts of composite wholes or tokens of natural kinds. The confusion here is one between concepts and spatiotemporally located individuals (Mahner and Bunge, 1997). As stated by Nixon and Carpenter (2000, p. 308): "pointing to a hypothesis cannot be taken … as equivalent to pointing at something 'real'." A proper name can be attached to an individual through ostension ("baptism": Ghiselin, 1995): the individual is pointed at with the index finger (hence the term "indication") while a name is bestowed upon it. Similarly, a natural kind term can (according to Kripke, 1972; Putnam, 1996) initially be "calibrated" through ostensive indication of a (presumably) paradigmatic token of the kind. Mere "ostensive indication" is not the same thing as "ostensive definition", however. Baptismal ceremonies are not ostensive definitions. It is therefore important to distinguish ostensive *definition* from ostensive *indication* (Putnam, 1996). If in an act of ostensive *definition* I point at a glass of water and state: "This (and anything similar) is a glass of gin", I will have defined a class of objects, the members of which most other people of my linguistic community refer to as "glass of water." If through ostension I indicate a glass of water and state "this is a glass of gin", I will simply have been wrong (the example is Putnam's (1996)).[2]

[2]Härlin (1998, p. 383) invoked Donellan's (1966) "referential" (as opposed to the "attributive") use of an expression to show that reference can be established even in case of presupposition failure: given the right circumstances, the question "who is the man at the bar drinking a Martini?" singles out a referent according to the speaker's intention even if the glass is filled with water. Kripke (1977) distinguished such "speaker reference" from "semantic reference", where the latter concerns the relation of a sign to a designatum irrespective of the intentions of a speaker on a particular occasion. I take it that for purposes of systematics, Donellan's "speaker reference", i.e., the reference a speaker intends to establish on a particular occasion, is too weak.

As discussed above, analytic definitions are tied to synonymy and *a priori* necessity, and hence to essentialism. The question of whether a merely ostensive (indexical) link between the name and a species, or taxon, also implies logically necessary, i.e., essential, properties, was a matter of dispute between de Queiroz (1992, 1995) and Ghiselin (1995), and a matter of concern to Ereshefsky (2001; see also Härlin, 2001). The phylogenetic definition of taxon names is meant to mark out monophyletic groups, i.e., clades of common evolutionary origin. One version of such a definition reads: The name N "refers to the least inclusive clade comprising A and B" (Schander and Thollesson, 1995, p. 263). The result of such a definition is that a common and unique evolutionary origin becomes the necessary and sufficient property for anything to be a part of a taxon so defined (or also defined as "the clade stemming from the most recent common ancestor of A and B": de Queiroz and Gauthier, 1994, p. 29). Rowe (1987, p. 208) for example stated "common ancestry is one criterion that is both necessary and sufficient for membership in a monophyletic taxon." On this account, the common evolutionary origin becomes an essential (necessary) property of a phylogenetically defined taxon, although the essence is a historical one (Griffiths, 1999; Wilson, 1999; LaPorte, 2004; if the taxon is an individual, its essence is also an "individual essence": Kripke, 1972). This means that the world could not be in a state in which parts of such a taxon had a different evolutionary origin: Martian tigers that are in every respect identical to Earthling tigers except that they share a different evolutionary origin could not be part of the Earthling tiger-taxon (similarly, if the Earthling tiger-taxon went extinct, but re-evolved at a later time, it would not be the same taxon anymore: Sober, 1984; Hull, 1988, p. 501).

This example shows how species, or taxon, names (whether as proper or as general names) function as rigid designators, but how at the same time the concept of rigid designation establishes close ties to essential properties (Soames, 2003). An essential property is one that the object could not lack in any circumstance in which it existed at all, and it is such essential properties that make rigid designation possible in the first place (Hughes, 2004). What, then, is the property that an evolving species/taxon has under all circumstances of its existence through time and space? There would seem to be only one: a common and unique evolutionary origin (Woodger, 1952). Härlin (1998, 1999, p. 2202) rejects historical essences since their introduction does not fully acknowledge the radical contingency of the evolutionary process (see also Härlin, 2003a, p. 146). This seems right, but then it only highlights a problem in the philosophy of the PhyloCode. For de Queiroz and Gauthier (1992, p. 462), a phylogenetically defined taxon name "necessarily", i.e., rigidly, refers to a monophyletic taxon, and it is by virtue of that necessity (rigidity) that a greater stability of the meaning of taxon names is supposed to obtain under the rules of the PhyloCode. But such stability of meaning comes at the cost of "origin essentialism". Suppose, however, that two taxon names are ostensively defined such that they refer to two different clades. These two names would certainly have a different meaning, whether or not such names have a Fregean sense. Suppose that ongoing scientific theory construction renders the two ostensively defined taxon names synonymous. On that account, a change of meaning must have occurred even if taxon names *qua* rigid designators have no Fregean sense, no intension, no connotation, i.e., are not associated with any description.

Let a biologist point to a particular turtle here with one hand, and to a particular bird over there with the other hand, and proclaim: "Let's call these animals and all other animals (and only those) that share the same common evolutionary origin as these two organisms that are here and now pointed at by the name 'Reptilia'!" Now assume another biologist points similarly at a lizard and a bird, proclaiming those as well as all other animals that share the same common evolutionary origin as the lizard and the bird pointed at to be named "Diapsida". We now have an ostensively defined system of parts of a whole (of kinds within kinds), i.e., Diapsida as part of, or nested within, Reptilia. But it is possible that ongoing research shows turtles to share relationships of one kind or another with or within Diapsida (Rieppel and Reisz, 1999; Modesto and Anderson, 2004). Given that commonality of evolutionary origin is a necessary property of a monophyletic clade (by stipulation), the ostensively defined name "Reptilia" still (and rigidly) refers/applies to the clade of all organisms (and to only those) that share the same ancestor as do turtles and birds. But given the new insights into turtle relationships, the *meaning* of Reptilia is no longer the same. Reptilia could exclude organisms previously referred to by that name; Reptilia could now be some subgroup, or part, of Diapsida; or Reptilia could in fact become synonymous with Diapsida (Fig. 1). In practice, the situation is remedied not by the acceptance of a change of meaning of the original name Reptilia, however, but by its re-definition (Modesto and Anderson, 2004). Thus, in attempting to secure a greater stability of nomenclature by an appeal to rigid designation, phylogenetic nomenclature becomes wedded to essentialism (Keller et al., 2003), which is exactly what it was designed to overcome in the first place. For this reason, phylogenetic nomenclature must remain flexible in the definition of taxon names, just as the Linnean system remains flexible in the diagnosis of taxa. Thirdly, the example shows once again that the meaning of a taxon name is not exhausted by its referent, as is the case for Millian proper names, but crucially depends on the potentially changing composition of a taxon (Nixon and Carpenter, 2000).

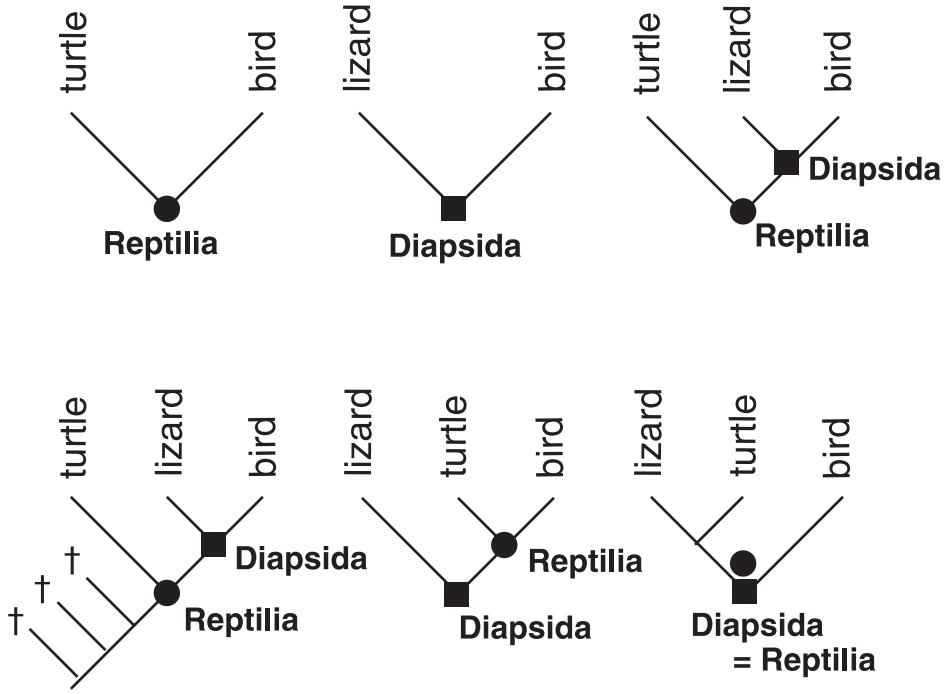


Fig. 1. The ostensive definition of Reptilia (through ostensive indication of a turtle and a bird) and of Diapsida (through ostensive indication of a lizard and a bird) results in a natural system with Diapsida nested within Reptilia. Changing views of turtle relationships result in a change of meaning of Reptilia and Diapsida as a consequence of a change of taxon composition.

**Individuals or natural kinds?**

According to Kripke's (1972) sketch of a theory of direct, or causal (social, historical) reference, proper names are not defined in any way, nor do they refer under a description or explication, but instead they are directly attached to an individual in a baptismal ceremony, from which onwards a causal link-to-link reference preserving chain maintains rigid designation for the name as used in discourse. Kripke (1972) argued that his thesis of rigid designation and his sketch of a causal theory of reference also applied to certain natural kind terms, associated with general names such as "water", "gold" or "tiger." Kripke's (1972) theory has the semantic advantage of "lightening the epistemic burden" (Devitt and Sterelny, 1999). This means that a speaker does not need mental access to a uniquely identifying description in their competent use of a proper name or of certain general names. If Kripke's analysis applies to taxon names, we should be able to use those names without any substantial knowledge about the composition of the taxon after all.

The key insight that underlies Kripke's (1972) thesis of a link-to-link reference preserving chain is the social character of meaning, where the competent use of a name is licensed by deference to other speakers (from whom the name is picked up; see also Putnam's (1996) "division of linguistic labor"), not by substantial factual knowledge about the referent. For example, people can competently use the term "gold" when discussing wedding bands in a reputable jewelry store without knowing the atomic number of the element gold (they also need not to know that the yellow color of some gold is due to copper impurities). Although the reference of a name may pragmatically be fixed descriptively, this description is not necessarily what semantically determines its reference. That is to say, the information content of an identifying description may be lost at some point, or modified, as the name gets passed along the link-to-link reference preserving chain. This, however, is also the major drawback of Kripke's thesis, which Luntley (1999, p. 274) called a "thesis of deferred reference" (the use of the term "gold" by lay-people is licensed by deference to the competent and hopefully honest jewelry store owner), rather than a theory of direct reference. A famous example for the failure of Kripke's (1972) theory of rigid designation, provided by Evans (1973), concerns the name "Madagascar", which originally referred to the African mainland. Through a misunderstanding of Marco Polo, the name came to refer to the island. This shows the incompleteness of Kripke's thesis in the sense that "there is a substantive non-trivial question of what goes into establishing the links in the historical chain" of the use of that name (Soames, 2003, p. 366). For reasons such as these, Evans (1982, p. 78) found it necessary to "trace the history of

the name'' for its competent use, whereas Hanna and Harrison (2004) speak of a socially grounded ''name-tracking-network''. What seems to be missing in Kripke's (1972) account is some explicative component necessary to determine (disambiguate) the reference of a name that is being passed along (e.g., Devitt, 1997). From the perspective of empirical sciences, such an explanatory component cannot merely be a description associated with a name in the speaker's mind, however, but must result from scientific investigation and discovery. Accordingly, ostension, stipulative definition and subsequent propagation of a taxon name through a social network of communication does not sufficiently fix reference for a taxon name. What in addition is required to disambiguate reference of a taxon name is an explicative account of the taxon's composition.

The allowance for reference change of taxonomic names as a function of the taxon's composition lies at the heart of Härlin's (1998, 1999, 2003b) distinction of the ''phylogenetic system of definition (PSD)'' from the ''phylogenetic system of reference (PSR)''. The latter, but not the former system allows taxonomic names to track reference change. However, Hull (1988, p. 497) argued that the ''type specimen method'' would prevent reference change for species names *qua* proper names, and the same is claimed for phylogenetically defined taxon names whose meaning is fixed by the ostension of two or more specifiers (''pseudotypes'': Nixon and Carpenter, 2000, p. 301). Indeed, there is in biological nomenclature a well-codified system in place to track the history of species names, a system that provides a publicly accessible ''name-tracking network'' that secures rigid designation (Hanna and Harrison, 2004, p. 131) for species names. The ''PhyloCode'' likewise proposes a name-tracking network for taxon names that is based on the notions of synonymy and priority (Cantino and de Queiroz, 2003; see also de Queiroz and Gauthier, 1992). The idea underlying the ''type specimen method'' is that a biologist, under the Putnam-style provision ''and the like likewise'', points at a specimen while saying: ''Let this term name the biological species exemplified in this organism'' (LaPorte, 2004, p. 5). But again, securing rigid designation through the ''type specimen method'' is not merely a semantic issue, but an epistemic one as well. The semantic issue is: ''to what does this [token] use of the [species] name refer?''—the epistemic issue is: ''which object's identification as [belonging to the same species as the type specimen] underlies this use' (Evans, 1982, p. 389). The issue, from an epistemological rather than semantic perspective, is to get the parts of the ''species'' right. In the wake of improved methods of molecular systematics, modern conservation biology experiences an inflation of species and species names (Isaac et al., 2004). Hierarchical structure is discovered at ever lesser levels of inclusiveness, such that the partitioning of nature becomes an eminently (species-) concept driven issue (Agapow et al., 2004; see also Martin, 1996). Systematists on a trip to a remote, previously unexplored part of the Amazonian rain forest do not just pick up a token organism, designate it as a type specimen and bestow a name on it in a baptismal ceremony that leaves permanent traces in the public ''name-tracking network''—all of this coupled with the stipulation to call everything that belongs to the ''same'' species (what is it?) as the type specimen by the same name (*pace* LaPorte, 2004, p. 5). Instead, the selection of a type specimen is a matter of scientific theory construction (Mellor, 1996; Rieppel, 2005). The same is true for taxa, where the selection of the specifiers is again a matter of theory construction. Were it not, the possibilities to define taxon names by simple ostension of two or more specifiers with the added stipulation of common ancestry (at an indeterminate level of generality) would become random, and limited only by the number of token organisms recognized as being different from one another to at least some minimal degree.

If reference of species, and taxon names is at least partially concept driven, i.e., based on scientific theory construction, these names should more appropriately be treated as general names (''kind-names'') rather than proper names (Rieppel, 2005). Pleijel and Härlin (2004) noted that Kripke's (1972) analysis also allows the ostensive attachment of taxon names to taxa if these are conceptualized as natural kinds (see also Härlin, 1999, p. 2202). But the way a kind-name acquires its reference does not also determine whether it subsequently functions (semantically) as a rigid designator. Hull (1976, p. 179, n. 4) early on raised the question of whether rigid designation would indeed hold for kind-names, concluding that in their case the ''role of meaning and meaning change seems too important to replace with the process of transmitting rigid designators in a link on link reference preserving chain.'' Indeed, Soames (2002, 2003) has argued that kind-names are not rigid designators (see also Hanna and Harrison, 2004), whereas Devitt and Sterelny (1999; see also Sterelny, 1996; Kitcher, 1995) provided an insightful account of how partial reference, grounding, multiple grounding and re-grounding can support a meaning change of kind-names.

Allowing for a meaning change of kind-names violates the requirement of ''universal and stable meanings of taxon names'' (de Queiroz and Gauthier, 1994, p. 28). The idea here is that ''once a taxon name is phylogenetically defined, it will always refer to the same ancestor/ancestry'', i.e., it will have the ''same meaning under different hypotheses of relationships'' (''as long as the specifiers used in the definition are present in the tree topology at hand'': Härlin, 2003a, p. 144). But as the example of changing turtle relationships discussed above shows, the situation is more complex (see also Nixon

and Carpenter, 2000). An ostensively defined taxon name will, indeed, rigidly refer to the same ancestor (by virtue of its definition), but that does not yet fully fix the meaning of the name. In other words, the meaning of a phylogenetically defined taxon name is not exhausted by the ancestor it picks out by virtue of its definition, but also, and in addition, by the descendants of this ancestor.

Härlin (1998, 1999, 2003b) convincingly argued for the requirement that taxon names must be allowed to change their meaning, but did so while treating taxon names as proper names (Pleijel and Härlin, 2004, p. 587). In another context, emphasizing the need for potential meaning change, Härlin (2003b, p. 140) considered taxon names as "proper names referring to hypotheses of a particular history", or even defended the view that "taxon names can be treated as low-level theories and viewed as paradigms" (Härlin, 2003b, p. 142). Proper names refer to (designate) particulars (individuals), not to hypotheses, nor are proper names themselves hypotheses, let alone Kuhnian paradigms. Härlin's argument in favor of meaning change of taxon names is much better served if taxon names are treated as kind-names (Rieppel, 2005). But what, exactly, is it for a kind-name to change its meaning (reference)?

The prospects of defining count nouns (Kripke's (1972) example is "tigers"), or mass terms (Kripke's (1972) examples are "water" or "gold") seem brighter than for proper names, for general (common, kind-) names are associated with concepts. Let's define "water" as "$H_2O$", rewritten as "water = $H_2O$" to express the synonymy, or identity, relation established by that definition. What this definition means is that the term water refers to any sample of naturally occurring stuff that is made up of $H_2O$. What about "ice = $H_2O$"? If the latter is true, then it must also be true that "water = ice", since identity is transitive. But "water = ice" is not true. Instead, what is true is that both water, and ice, are *composed* of $H_2O$ (Stroll, 2000, p. 232; see also Soames, 2003, p. 441): water *as* liquid, water *as* ice, and water *as* steam, is $H_2O$. This means that even if reference for a kind-name can initially be established ostensively (Pleijel and Härlin, 2004; see also Härlin, 1999, p. 2202; "indexically" in Putnam's (1996) terminology), an explicative component will eventually become relevant in establishing its meaning (in identifying the kind). The same applies to taxon names.

The meaning of a kind-name again is not just a matter of semantics, but also an epistemic issue, i.e., one of scientific discovery. Natural kind terms function in causal theories that explain natural processes; they are "predicates [that] are associated with properties for use in explanations and in inductive generalizations" (Kitcher, 1995, p. 80). Putnam (1996) gave an insightful account of how natural kind terms acquire their meaning. Adopting Kripke's (1972) thesis of direct reference, Putnam (1996) contended that the reference of a kind-name is initially (and rigidly) fixed by ostensive indication of (putatively) paradigmatic tokens of the kind (a "sample" of the kind). But given the theoretical indeterminacy of such an indexical context, some descriptive amplification is required to specify which kind this particular individual is supposed to be a token of (i.e., a specification of what counts as "being of the *same* kind" as the sample pointed at). This specification is provided by a stereotype of the kind, which is based on a descriptive account of its perceptual properties: using Kripke's example, the stereotype for "tiger" would be "a large feline carnivore with a striped color pattern", etc. For it must be minimally assumed that speakers conversing about tigers share some stereotypical knowledge about some or most tigers. But such a stereotype does not provide an analytical definition of "tiger", since three-legged or un-striped tigers cannot be ruled out *a priori* (Hughes, 2004, p. 48). It also does not establish the extension of the natural kind term, it being the set of samples (of "water" or "gold") or the mereological sum of individuals (the species *Panthera tigris*), to which the term truthfully applies. The stereotype of "tiger" would not prevent Martian tigers from being members of the Earthling natural kind "tiger" if they looked sufficiently alike. To establish the extension of a natural kind term (the way the term reaches out into the world) requires scientific research aimed at the discovery of its causally relevant properties (Putnam, 1996, p. 11). Since tokens of natural kinds take part in natural (causal) processes, it is the causally efficacious properties (Sober, 1981) of a kind that determine the extension of the corresponding natural kind term (natural kinds therefore are by no means "conceptual *constructs*" as claimed by Pleijel and Härlin (2004, p. 588; emphasis added); see also Boyd (1999)).

The extension of the natural kind term is the set or mereological sum of things (objects, bodies) to which the term truthfully applies. The meaning of a natural kind term is thus established by its extension. If taxa are natural kinds, if their names (i.e., the name "Reptilia") therefore are associated with natural kind terms (i.e., the predicate "… is a reptile"), then it must be the extension of those terms, i.e., the composition of the taxa, that determines the meaning of the taxon name. If the extension of a natural kind term associated with a taxon name changes as a result of a change in the composition of the taxon following continued empirical investigation (i.e., if the hypothesis of relationship changes), then the meaning of the taxon name changes accordingly (Härlin, 1998, 1999, 2003b). If, in contrast, the meaning of taxon names is to be "universal and stable" (de Queiroz and Gauthier, 1994, p. 28), then new taxon names must be coined to refer to groups of different composition judged to be monophyletic as a result of ongoing research. If such a proliferation of taxon names is to be

avoided, then taxon names must either be continuously re-defined, or allowed to change their meaning as proposed by Härlin (1998, 1999, 2003b). In that sense, Härlin (2003b) is correct when he claims that taxon names refer under a hypothesis only (i.e., that reference of taxon names is concept driven). But that claim holds only if taxon names are general names associated with natural kind terms, rather than being proper names. Millian proper names do not refer under a hypothesis, whereas under a Fregean analysis, proper names refer under a description (stereotype).

## An alternative: the explanatory definition of taxon names

Species and/or taxa can be conceptualized as individuals (Ghiselin, 1974; Hull, 1976, 1988, 1999), or as natural kinds (Dupré, 1993; LaPorte, 2004) as was recognized by Pleijel and Härlin (2004). Systematists have commented on that distinction from an ontological point of view, asserting that individuals and natural kinds "belong to two different ontologies" (Pleijel and Härlin, 2004, p. 588). De Queiroz (1992, 1995) has disclaimed such a sharp distinction, and various philosophical arguments can be put forward both in support of (Sober, 1984; Rosenberg, 1985; Hull, 1999), as well as against (Kitcher, 1984a,b; Dupré, 1993; Boyd, 1999; Griffiths, 1999; Wilson, 1999; LaPorte, 2004), either view. If species and supraspecific taxa are individuals, their names are proper names, and these cannot be defined. If species and supraspecific taxa (Rieppel, 2005) are natural kinds, then their names can be defined, but in the spirit of an empirical science, they should not be defined analytically, but should be defined theoretically (explicatively) instead. Such a theoretical definition is a causal explanation of what is being defined. For example, a taxon *qua* natural kind can be defined theoretically (Boyd, 1999, p. 149) by a description of its causally efficacious properties, i.e., by the relational properties that are homologies (Keller et al., 2003, p. 105). Taxon names theoretically defined track scientific discovery, as taxon names will function in explanatory theories about the evolution of the taxa to which they refer.

The discussion of phylogenetic nomenclature reviewed above reflects the fact that, beginning with Ghiselin (1974) and Hull (1976), systematists have followed the lead of philosophers who abandoned the description theory of reference. Since the original publication of Kripke's *Naming & Necessity* in 1972, more work has been done in the philosophy of language indicating some incompleteness of the picture canvassed by Kripke (1972) of causal reference and rigid designation (e.g., Dummett, 1981; Evans, 1982; Devitt and Sterelny, 1999; Luntley, 1999; Lycan, 2000; Stroll, 2000; Soames, 2002, 2003; Hanna and Harrison, 2004; Hughes, 2004). Individually, these critics of Kripke (1972) pursue widely different philosophical agendas, but what they collectively point out is that in Kripke's theory, a problem of determinacy of reference persists (Devitt, 1997). Kripke's (1972) sketch has been dubbed the "causal-historical" theory of reference, but the causality here invoked is of a purely social nature: it concerns reference-borrowing in a link-to-link reference preserving chain of socially interacting members of a speech community. Devitt (1997; see also Devitt and Sterelny, 1999) asks for more: the anchoring of a term's reference in an object, reference borrowing as the term is passed on in communication, and competence to use the term in discourse that is sustained not only through repeated reference borrowing, but also through repeated grounding and re-grounding of the term's reference in objects and their causally efficacious properties. These are the mechanisms by which whales become recognized as mammals, and birds as reptiles (Rieppel, 2005).

## Summary and conclusions

If it is accepted that the "feathering" of reptiles changed the meaning of the term "Reptilia", it seems more appropriate to treat taxon names as kind-names (names associated with natural kind terms) rather than proper names (Rieppel, 2005). Millian proper names do not allow for meaning change, and the synonymy (i.e., identity of meaning) of a proper name with a definite description is a requirement that suffers from all the shortcomings that Kripke (1972) and his followers have pointed out (e.g., Hull, 1988). However, the concept of a natural kind useful for systematists and evolutionary biologists must also not be a strong, Aristotelian or Millian one, as is the one adopted by Hull (1999), where natural kinds are equated with classes or sets of stuff or things that occur in nature. Rather, for the purposes of systematics, a natural kind should be conceived of in its weaker, empirically more adequate and nonessentialistic conception as a homeostatic property cluster kind (Boyd, 1991, 1999; Griffiths, 1999; Wilson, 1999; Keller et al., 2003; Rieppel, 2005). Such a conception of taxa better reflects biological practice, and allows for continuous epistemic input, as the taxon name is allowed to change its meaning according to the results of ongoing systematic research.

To conceptualize taxa as "homeostatic property cluster natural kinds" allows systematics to break its ties with essentialism. Proponents of phylogenetic nomenclature have chastised the Linnean system for being essentialistic. Completion of the "Darwinian revolution" (de Queiroz, 1988) is said to require the abandonment of the Linnean hierarchy (Härlin, 2001) in favor of a phylogenetic system that is based not on the definition, nor on the diagnosis of taxa, but on the phylogenetic definition of taxon names (de Queiroz and

Gauthier, 1992; de Queiroz, 1992). As a consequence, taxon names are believed to become rigid designators. However, in defining taxon names and treating them as rigid designators, phylogenetic nomenclature got caught in essentialism—which is exactly what it set out to overcome (Keller et al., 2003). True enough, the new essentialism in systematics is not of a Linnean (or better: Aristotelian), but of a new, non-traditional kind (the so-called "origin-essentialism": Hanna and Harrison, 2004; Hughes, 2004; LaPorte, 2004), but it still creates problems for the accommodation of a change of meaning of taxon names that results from ongoing research. If a change of meaning of taxon names is to be possible, taxon names are better treated as general names (such as "tiger") associated with natural kind terms (such as the predicate "… is a tiger"), than as proper names. If so treated, taxon names become associated with a concept, similarly to Linnean taxon names that are associated with a diagnosis. The difference is important, however. The diagnosis of a Linnean taxon name essentially provides nothing but a list of descriptive criteria of identification and re-identification, i.e., a stereotype of a taxon. To recognize a taxon as a natural kind is to recognize its tokens as taking part in shared causal processes (of inheritance, ontogeny, ultimately descent with modification) that point towards a common ancestry.

The issue, then, is not so much a replacement of the Linnean system by the PhyloCode, but the *naturalization* of the Linnean System. The issue, accordingly, is the distinction between nominal and natural kinds. Given that the diagnoses of the Linnean system specify the descriptive properties of a stereotype only, there always remains the possibility that groupings so diagnosed are nominal (artificial) kinds (i.e., paraphyletic groupings) instead of natural kinds (monophyletic groups). The naturalization of the Linnean system is complete when nominal kinds have been replaced by natural kinds, when paraphyletic groups have been replaced by monophyletic groups (Rieppel, 2005).

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