

Understanding Animal Classification

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I. Binomial Nomenclature.

The classification of animals began in a rather haphazard manner. In the early days (prior to Linnaeus), most creatures were given a descriptive name. Unfortunately, such names varied according to the language used and even by the circumstances that prevailed when the organism was “discovered” — often independently by several different people. Consequently, the names that applied to the same type of animal might equal the number of persons who had encountered it. Some feel for the confusion that prevailed is evident when people from different parts of the world try to communicate while relying exclusively on common names. For example, Europeans speak of an “elk,” which North Americans call a “moose.” The animal called an “elk” in America is referred to as a “wapiti” by Europeans — and the “Irish elk” is a different beast altogether. All “robins” are birds, but English “robins” are quite different than those found in the Western Hemisphere (which are called “wandering thrushes” in Europe). A German herpetologist might speak of a “Wüstenleguan,” to the consternation of an English speaker, who would refer to the same animal as a “desert iguana” (*Dipsosaurus dorsalis*).

Such an unwieldy system soon became overburdened when Europeans, during the “Age of Discovery,” began to carry back from their voyages samples of wildlife from all over the world. A remarkably simple and flexible alternative was developed and popularized by Carl Linné, a Swedish botanist (Linnaeus was the Latinized version of his name under which he published his many volumes on the plants and animals of the world). In his system, organisms are ordered in a hierarchical manner, based largely on the number of common traits possessed by various forms. Thus, animals sharing but a few fundamental similarities were placed in the same higher category but were segregated at the next lower rank. Only those individuals that were essentially alike shared

the full gamut of categories and were placed in the same “species.” Linnaeus’ system was easily adapted to the vast quantities of new knowledge merely by adding additional categorical ranks.

Fundamental to the Linnaean system is the “binomen” (= two names), which is used to designate any species (this is the reason we refer to a “binomial system of nomenclature”). The first part of the name is that of the genus to which the animal belongs. Genera (the plural form), according to Linnaeus, were groups of species which shared more characteristics with each other than with species placed in any other genus. The generic name is a proper noun and should always be capitalized (as should all names of higher categories). As nouns, generic names have gender, which accounts for many of the different endings (e.g., many “feminine” nouns end in *-a*). The second half of the name is the trivial or specific name. Used to distinguish species within a genus, these



Subadult *Iguana delicatissima* from Anguilla. The critically endangered Anguillian population represents the northernmost extent of the species’ range. The meaning of this species’ trivial name is self-evident. Interestingly, although considered a delicacy throughout most of its range, Anguillian animals have not been exploited for food by local inhabitants. The precarious state of the population is due almost entirely to habitat alteration and competition with feral livestock. *Photograph: Stesha Pasachink*

names are adjectives or possessives that modify the generic name. Trivial names must agree in gender with the generic name and should never be capitalized. The entire binomen is always set off in some distinct way, usually italicized (in print) or underlined.

An additional feature of the Linnaean system is that all names are Latin (or Latinized; many actually are derived from the ancient Greek or even from various modern languages). Because Latin is no longer the vernacular of any culture, it is an extremely stable language and the grammatical rules are fixed. Consequently, a Pole or an Argentinean could name a species, and the other would immediately recognize it as a scientific name and maybe even understand its meaning — even though they might otherwise be totally incapable of communicating.

Applications of this system give us names like *Dipsosaurus dorsalis* (the “desert iguana”). “*Sauros*,” the root of the generic name means “lizard” in Greek. “*Dipsos*,” also from the Greek, means “thirsty,” an obvious reference to the desert habitats in which these lizards are found. The trivial name “*dorsalis*” is an adjectival form of “*dorsum*,” a Latin word that means “back,” an allusion to the crest of enlarged scales that runs down the middle of the back in these animals. “*Iguana*” presumably was modified from an Amerindian word meaning a kind of large lizard; “*delicatissima*” is from the Latin, meaning “very delicious.” Descriptions of new species should include an etymology, which provides the origin and meaning of the new name. Unfortunately, at the time when many common species were first described, the ‘rules’ were not always observed; as a result, we’re often forced to guess the meaning of a name if its derivation is not obvious.

Although daunting when first encountered, mainly because the Latin (or Latinized) names appear to defy pronunciation, the system makes a lot of sense. Compare the binomial to a person’s name. The surname (at least in English-speaking cultures) is equivalent to the generic name (the family to which a person belongs is analogous to the genus to which a species is assigned), and a person’s first name equates to the specific name (identifying that individual much like the trivial name identifies a unique species).

II. Rules of Priority.

In Part I, I mentioned the confusion that could arise when different people at different times gave different names to the same animal. Unfortunately, this problem is not unique to common names. On more than one occasion, several properly constructed scientific names have been given to the same species. This may have occurred because someone was unaware of an earlier description, possibly published in an obscure journal, maybe even in another language. Other reasons have included descriptions as different species of males and females of the same sexually dimorphic form, or descriptions as different species of different stages in a life cycle (tadpole versus metamorphosed amphibian or a juvenile with a distinctly different appearance than adults of either sex), or descriptions as different species of individuals from different regions or merely individuals demonstrating the considerable variation known to occur in some animals. Regardless of the reason, the problem of multiple names led to the necessity for synonymies, lists of all of the names that have been applied to a particular taxon. Usually, the first name is given precedence over all subsequent names, but exceptions sometimes occur. The “rules” that govern animal classification are enforced by an International Commission on Zoological Nomenclature, and the Commission must approve any exceptions. Better than a prolonged explanation is a relevant example taken from a recent issue of the *Bulletin of Zoological Nomenclature* (2001. 58:37–40), the formal publication of the Commission (reproduced below in its entirety, except for the list of references and the addresses of the authors):

Case 3143

Euphryne obesus Baird, 1858 (Reptilia, Squamata): proposed precedence of the specific name over that of *Sauromalus ater* Duméril, 1856

Richard R. Montanucci, Hobart M. Smith, Kraig Adler, David L. Auth, Ralph W. Axtell, Ted J. Case, David Chiszar, Joseph T. Collins, Roger Conant, Robert Murphy, Kenneth Petren, Robert C. Stebbins

Abstract. The purpose of this application is to conserve the long used and well known specific name of *Sauromalus obesus* (Baird, 1858) for the chuckwalla (family IGUANIDAE) from the southwest of North America by giving it precedence over the little used name *S. ater* Duméril, 1856.

Keywords. Nomenclature; taxonomy; Reptilia; Squamata; IGUANIDAE; *Sauromalus ater*; *Sauromalus obesus*; chuckwallas; southwestern North America.

1. In 1856 Duméril (p. 536, pl. 23, figs. 3 and 3a) described a new genus and single new species of iguanid lizard as *Sauromalus ater* on the basis of a single specimen presented by Lieutenant M. Jaurès to the Muséum National d'Histoire Naturelle, Paris. The holotype (MHNP 813), which lacks locality data, was collected somewhere in western Mexico during a world circumnavigating voyage of the French frigate *La Danaïde*.
2. The absence of a type locality for *Sauromalus ater* has remained an acknowledged problem for systematists working with *Sauromalus* (see Schmidt, 1922; Shaw, 1945; Hollingsworth, 1998). Shaw (1945, p. 273), unable to study the holotype due to political conditions in Europe, drew upon descriptive information in Duméril & Bocourt (1870) and Mocquard (1899), and concluded that the holotype must

have originated from one of the islands off the southern coast of the Baja California peninsula. Hence, in referring to the type locality, Shaw (1945, p. 284) stated: 'Not definitely known but undoubtedly one of the several islands in the southern part of the Gulf of California where this species is known to occur', Subsequently and without justification, Smith & Taylor (1950) further restricted the type locality to Isla Espiritu Santo.

3. Two years after Duméril, Baird (1858, p. 253) described the new genus and single new species *Euphryne obesus* and noted that it was 'abundant in the canons of the Colorado, of California, collected by Maj. Thomas, Mex[ico] Boundary Survey, and Lt. Ives' Expedition'. The type specimen was given as USNM 4172 in the U.S. National Museum, Washington. Subsequently, Baird (1859, p. 6, pl. 27) indicated the locality of USNM 4172 as 'Fort Yuma'. Van Denburgh (1922) and Shaw (1945) correctly noted the location of Fort Yuma in California. Montanucci (2001) discussed the confusion caused by Baird's piecemeal publication of data and clarified the particulars relating to the collector and type locality. Cope (1864) commented that the name *Euphryne* Baird, 1858 was a synonym of *Sauromalus* Duméril, 1856, but both generic names continued to be used in the literature until Cope (1875) and Coues (1875) placed



Subadult *Cyclura nubila* from the U.S. Naval Base at Guantanamo Bay, Cuba. For many years, this species was thought to contain three subspecies; however, new evidence indicates that the Grand Cayman population is distinct at the species level.
 Photograph: Robert Powell

Euphryne as a synonym of *Sauromalus* (see Hollingsworth, 1998, p. 40). *Sauromalus* has been used since that time.

4. Prior to 1922, the name *Sauromalus ater*, and not *S. obesus*, was used in most papers, including checklists and distributional accounts. Most notable among these publications are Cope (1875, 1900), Stejneger's (1891) description of a new species of *Sauromalus*, the checklists of Yarrow (1882) and Stejneger & Barbour (1917), and Van Denburgh's (1922) *The Reptiles of Western North America*. The recognition of *S. ater* and *S. obesus* as separate species came with publication of Schmidt's (1922, pp. 640–641) study of the amphibians and reptiles of lower California, and was followed by the later checklists of Stejneger & Barbour (1923, 1933, 1939, 1943). The taxonomic treatment of the genus *Sauromalus* by Shaw (1945) reinforced the concept that *S. ater* and *S. obesus* are separate species, a view held by virtually all subsequent workers except Hollingsworth (1998).
5. In his recent monographic revision of *Sauromalus*, Hollingsworth (1998) placed *Sauromalus obesus* in the synonymy of *S. ater*, and restricted the type locality of *S. ater* to southern Sonora. However, Montanucci (2000) argued that Hollingsworth's analysis to determine the provenance of the type specimen was unconvincing due to limitations in his statistical data, leading to ambiguous results and an unsubstantiated conclusion. Accordingly, Montanucci (2000) concluded that, in the absence of any new, compelling information, the type locality of *S. ater* remained open to speculation and conjecture.
6. The literature using the name *Sauromalus obesus* is substantially more abundant and significant than that using the name *S. ater*. Beaman, Hollingsworth, Lawler & Lowe (1997) listed 626 titles of technical and popular articles pertaining to the genus *Sauromalus*. Out of this total, the name *S. ater* is used in about 46 papers; most of these (34) were published before 1950, and nearly all pertain to taxonomy and/or distribution. The literature for *S. obesus* is profoundly more extensive by comparison, being conservatively estimated to be about 90% of the total literature for the genus as a whole, or some 550 papers. The name *S. obesus* is used, almost to the exclusion of *S. ater*, in the literature dealing with physiological ecology and thermoregulation of chuckwallas (about 133 papers), most of the basic ecological works (about 71 papers), as well as morphological studies (about 92 articles). Over 100 papers dealing with distribution use the name *S. obesus*. While the name *S. ater* has been little used and is essentially restricted to publications in technical journals, the name *S. obesus* appears in numerous papers, magazines and books, ranging from technical to popular. Clearly, the name *S. obesus* has had a long history of usage to the present, and is deeply entrenched in both the scientific and popular literature. Hence, any proposed change of this long-recognized name would certainly create extensive confusion and instability.
7. We propose that, if the names *Sauromalus ater* Duméril, 1856 and *S. obesus* (Baird, 1858) are considered to be synonyms, *obesus* should be conserved for the combined taxon by giving it precedence over *ater*. If the two names are considered to refer to different taxa (species or subspecies), then both names are available for use. If the application is approved by the Commission both names will be placed on the Official List. As mentioned in paras. 1 and 3 above, the holotypes of both nominal taxa are in existence.
8. The International Commission on Zoological Nomenclature is accordingly asked:
 - (1) to use its plenary power to give the name *obesus* Baird, 1858, as published in the binomen *Euphryne obesus*, precedence over the name *ater* Duméril, 1856, as published in the binomen *Sauromalus ater*, whenever the two are considered to be synonyms;
 - (2) to place on the Official List of Generic Names in Zoology the name *Sauromalus* Duméril, 1856 (gender: masculine), type species by monotypy *Sauromalus ater* Duméril, 1856;
 - (3) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *obesus* Baird, 1858, as published in the binomen *Euphryne obesus*, with the

endorsement that it is to be given precedence over the name *ater* Duméril, 1856, as published in the binomen *Sauromalus ater*, whenever the two are considered to be synonyms;

- (b) *ater* Duméril, 1856, as published in the binomen *Sauromalus ater*, with the endorsement that it is not to be given priority over *obesus* Baird, 1858, as published in the binomen *Euphryne obesus*, whenever the two are considered to be synonyms.

Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o The Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

III. Changing Paradigms.

Throughout most of history, animals were grouped into categories using as criteria readily observable morphological similarities. Nearly all early naturalists, who were educated in systems based on literal interpretations of the Bible, viewed classification as a means to better understanding God's plan. The underlying basis for the resulting groups, however, has changed. In the latter half of the 19th century, almost entirely in response to Charles Darwin's work, the interpretation of classification by most scientists changed dramatically from one based on similarity and a revelation of God's plan to one based on "descent with modification." That one could switch from interpreting the existing Linnaean system of classification as a scheme of similarity to one based on an evolutionary history is testament to the system's innate flexibility.

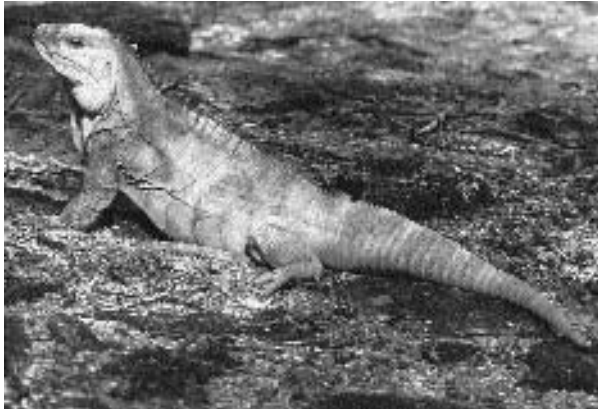
Yet, even today, the transition from one school of thought to the other is incomplete. This is predicated by the reality that animal classification contains two distinct and sometimes mutually exclusive entities: (1) a purely technical aspect concerned with establishing a set of rules that define how classifications will be implemented (and which is unconcerned with the philosophy of the person describing a new taxon), and (2) a conceptual aspect that seeks, above all, to determine just what a classification means (i.e., identifying and understanding the underlying factors responsible for the relationships that a classification



Subadult *Iguana iguana* from southern Veracruz, México. Traditionally considered a single, widely distributed species, new evidence suggests that *I. iguana* actually constitutes a species complex. As details are resolved, these new data will eventually be reflected by taxonomic changes that more closely approximate the reality in nature. Photograph: Robert Powell

implies). Some insight into the first aspect was provided by the petition to the International Commission on Zoological Nomenclature in Part II of this series. The second aspect is more difficult, in practice if not in principle.

Willi Hennig, a German systematist, proposed in 1950 a major reform of existing animal classification schemes, a new approach that has become known as "phylogenetic systematics." Until its implementation and widespread (though not universal) acceptance by practicing taxonomists, the recognition that our understanding of animal genealogies is woefully incomplete resulted in a general willingness to use criteria that rely heavily (or solely) on similarities to define categories. In other words, "relationships" reflected in taxonomy might be relationships of similarity or relationships of genealogical descent or even some mixture of the two. Hennig suggested that "relationships," in a rigorous evolutionary sense, must include only considerations of descent, defined as the relationship between an ancestral species and its descendants. No longer could the placement of a taxon into a particular category be justified by arguments based on similarities. Thus, the association of crocodilians with lizards, because they are superficially



Adult male *Cyclura ricordii* from Parque Nacional Isla Cabritos, Dominican Republic. The generic name ("ring-tail") is based on the whorls around the tail, which are very evident in this specimen. The trivial name is a possessive patronym (indicated by the *-ii* ending), which honors Alexandre Ricord, who collected the type specimen. Photograph: Robert Powell

similar, was no longer acceptable (genealogically, crocodilians share a much closer common ancestry with birds and dinosaurs than with lizards and other squamates). Although sound in principle, the new paradigm was difficult to implement in many instances — mainly because we still lack the evidence necessary to understand many animal genealogies and, to a lesser degree, because we often are unwilling to reject familiar categories based on similarities or flawed interpretations of evolutionary relationships.

Another refinement of classification emerged from Hennig's work. Systematists previously had recognized two kinds of groups. "Monophyletic" groups were composed of the descendants of a single common ancestor and "polyphyletic" groups contained organisms that did not share a common ancestor. For example, Mammalia is monophyletic, but Homeothermia (a group composed of mammals and birds based on their ability to physiologically maintain elevated body temperatures) is polyphyletic because the two constituent groups have different "reptilian" ancestors. Hennig noted that many recognized "monophyletic" groups were not really monophyletic at all, and recognized a third type of group. Because these assemblages did not contain all of the descendants of a common ancestor, he called them "paraphyletic." Phylogenetic systematists asserted that paraphyletic groups were as unnatural as polyphyletic groups.

The most easily understood example is the Class Reptilia, as traditionally defined. Reptilia constitutes a paraphyletic group because some descendants (mammals and birds) are left out. Another example is the Family Pongidae, which consists of the great apes but excludes humans (who are placed in their own family, Hominidae). Because paraphyletic groups are logically inconsistent with the phylogenies that classification should reflect, the idea that we should abandon traditional schemes that include paraphyletic groups is growing in acceptance (although almost every textbook in print presents classifications that include paraphyletic groups).

How would a revised, purely phylogenetic classification of an iguana look? Actually, the answer is less than obvious, because the criteria that define the higher categories in particular are often vague and frequently subject to revision. However, one scheme (modified from a phylogeny presented in Pough et al. 1998. *Herpetology*. Prentice-Hall, Inc., Upper Saddle River, New Jersey) might look a lot like this:

- Chordata (animals with notocords, pharyngeal gill slits, and dorsal, hollow nerve cords)
- ..Vertebrata (chordates with at least rudimentary braincases and vertebral columns)
- ...Sarcopterygii (lobe-finned fishes and their descendants)
-Tetrapoda (amphibians and their descendants)
-Amniota (those tetrapods that reproduce via cleidoic eggs; excludes amphibians)
-Reptilia (excludes the common ancestor of mammals and all of its descendants)
-Diapsida (excludes "reptiles" with anapsid, synapsid, and euryapsid skull structures)
-Lepidosauria (the common ancestor of squamates and rhynchocephalians and all of its descendants)
-Squamata (lizards and snakes)
-Iguania (non-scleroglossine lizards; includes acrodonts as well as iguanids in the broad sense)
-Iguanidae (the oldest common ancestor of all species in the genera *Amblyrhynchus*, *Brachylophus*, *Conolophus*, *Ctenosaura*, *Cyclura*, *Dipsosaurus*, *Iguana*, and *Sauromalus* and all of its descendants)
-*Iguana* (the oldest common ancestor of *I. iguana* and *I. delicatissima* as currently defined and all of its descendants)
-*Iguana delicatissima* (the binomen, indicating a group consisting of the oldest common ancestor of all populations now assigned to *I. delicatissima* and all of its descendants)

An examination of this example quickly points to a problem inherent in a phylogenetic classification, namely that the number of groups can easily exceed the number of Linnaean categories. Some systematists have proposed alternatives that include using numerical prefixes instead of categorical names or even a “rank-less” system in which the hierarchy is indicated merely by indenting (as above). Unfortunately, these alternatives are not without problems of their own. Numerical systems can be applied only to one phylogeny without becoming confusing (does a particular number have the same meaning in each context?), and rank-less systems quickly become so cumbersome that determining who is related to whom is impossible (note that the example includes only one alternative at each level; if all chordate groups were included, the classification easily would fill many large volumes).

IV. What Classification Can — and Cannot — Do

Modern classification should demonstrate the relationship between phylogeny and taxonomy by identifying and naming hierarchical groups that define genealogies. That recognition acknowledges the underlying hypotheses that a classification seeks to examine, namely that groups reflect genealogical relationships. If a classification compares favorably with a phylogenetic tree, the hypothesis is supported. Unfortunately, the lack of available evidence for relationships of many groups often results in classifications based on similarities or even the intuition of the systematist. These often are merely efforts to organize diversity until a group can be adequately studied. Obviously, newly acquired evidence renders many of these tentative schemes unacceptable, and this is at the very root of many taxonomic changes. The distinction between classifications based on great quantities of pertinent evidence and those that reflect mere “guesses” is impossible — unless the reader is very familiar with the group(s) being classified. However, in both instances, a reader should remember that any classification is based only on hypotheses addressing the group relationships that exist between the organisms being classified.

The rank of a group does not necessarily reflect the distinctiveness of that taxon. A system-

atist may raise the rank of a group to reflect her views of its distinctive nature, but rank in and of itself does not provide that information. For example, when Frost and Etheridge (1989. A phylogenetic analysis and taxonomy of iguanian lizards (Reptilia: Squamata). *Univ. Kansas Mus. Nat. Hist. Misc. Publ.* (81):iv + 65 pp.) elevated what had been considered “iguanines” or “Iguaninae” to full familial rank (“Iguanidae”), the intent was to better reflect historical events, not to suggest that the group was any more distinctive than had been previously recognized.

Emphasizing the fact that rank does not denote degrees of distinction is the reality that ranks cannot be and are not applied equitably to different groups. In other words, the ranks in any given classification represent genealogical relationships by subordinating descendent groups to those representing older (ancestral) taxa; thus, the number of ranks merely reflects the relationships in a particular group rather than being definitive arbitrators of a certain level of distinction. In other words, a family of insects is not comparable to a family of squamates (as a matter of fact, if equivalency were the goal, an insect family may equate with an order or even a class of vertebrates). Rather than being a defect of modern classification schemes, these inequalities testify to the system’s flexibility. The idea that unrelated (actually distantly related) groups of comparable rank are biologically equivalent dates to the antiquated perception that all types of organisms represented rungs on a “ladder of life” that represented a scale culminating in “perfection” (in these ancient schemes, humans invariably resided on the top rung; what a humble species we are...).

Finally, classification cannot remain stable and unchanged. All taxonomic decisions are, in fact, hypotheses subject to additional testing and potential rejection when new contradictory data are uncovered. Consequently, like science in general, classification must be dynamic — and this is a strength rather than a flaw of the system. If classification were stagnant, it could not be scientific. Although we all bemoan the need to constantly relearn new classifications, we should instead be toasting the changes that reflect new knowledge, scientific progress, and are probably closer to the truth represented in nature.

V. Do 'Species' Exist In Nature?

When I was an undergraduate, I was taught that the species was the only “real” taxonomic rank, based on the assumption that it could be subjected to testing by applying criteria applicable in nature. Of course, the concept of species to which reference was being made was the “biological species,” defined as a group of similar, reproductively interacting (or potentially interacting) organisms reproductively isolated from all other such groups. The emphasis on reproduction certainly was appropriate. I often tell my students that the primary role of individual organisms in nature is to reproduce, and that all other functions are secondary. Also, the contention that reproductive isolation was testable appeared reasonable — but was it? I began to have doubts when discussions veered to the many exceptions. Obviously, the principal criterion was not applicable to asexually reproducing organisms or to parthenogenetic organisms (those all-female species in which offspring are produced by development of an unfertilized ovum), but could it even be applied adequately to sexually reproducing populations with non-overlapping distributions? If organisms are geographically isolated (allopatric), is any test of reproductive isolation appropriate? When I began to examine examples with which I was familiar (allopatric “subspecies” of North American amphibians and reptiles or insular populations of West Indian forms), I found that assumptions based on degrees of morphological similarity or simple guesswork invariably were substituted for empirical testing. This conclusion then led me into a review of the voluminous literature pertaining to species concepts.

The best summary of my search for answers came when I ran across several papers which emphasized that speciation was a process, and that “species,” variously defined, could exist at any stage in that progression. The biological species concept applied accurately only to groups of sexually reproducing organisms that had essentially completed the process. At the other extreme were populations of common ancestry that had only recently become isolated (geographically or ecologically) and had barely begun to differentiate as a consequence of differing selective pressures, mutations, or random changes in genetic compo-

sition often associated with small population sizes. Some of these isolates will never diverge to an extent sufficient for recognition as a distinct species, others may do so eventually — even to the extent of becoming reproductively isolated. Many of the competing species concepts actually address such “works in progress.” For example, the “evolutionary species concept” recognizes populations as species that have diverged enough to be diagnosable and which, due to geographic (or ecological) isolation, have unique evolutionary trajectories. Against the argument that such diverging populations would readily interbreed if they should come back into contact with one another, proponents of the “evolutionary species” rightly note that science cannot deal with events that have yet to occur.

So, where does that leave the biologist who is trying to assess the biodiversity of a region (typically expressed as the number of species present), establish a conservation plan for a particular taxon (generally, species-level taxa are provided greater consideration than subspecies or isolated populations when management plans are developed), develop a captive-breeding program with the potential for the eventual release of progeny into the wild (releasing hybrids into naturally occurring populations will merely dilute or pollute the existing gene pool), or even engage in research using as a model a particular species, the identity of which is crucial (if only to provide an accurate label for the model)? Actually, the answer is not that difficult once agreement can be reached on a common definition of “species.” The best I have found is that a species is a natural entity that derives its existence from historical evolutionary (ancestral) relationships (a phylogenetic emphasis), interbreeding (the traditional biological species concept), or some combination of both. If we can accept that definition (recognizing as we do that speciation is a dynamic process), then the “general lineage concept” should be acceptable to all. This concept recognizes species as segments of population-level lineages — and all contemporary species definitions, in one way or another, consider species to be segments of population lineages. Consequently, all species concepts are components or variations of this main underlying concept, although they may variously emphasize different diagnostic criteria.

What does this mean in the “real world?” It means that species definitions will vary to some degree depending on the criteria applied by the researcher and her agenda (if interested in conservation, she might be more inclined to recognize a population as a full species than, for example, another researcher who merely wants a label for his model, and would prefer that which is most familiar to his colleagues). To avoid such varying standards, some systematists have suggested substituting the term “operational taxonomic unit” for species. This apparently unconventional approach might seem strange at first, but has the very distinct advantage of setting aside disagreements over which species-defining criteria to emphasize by focusing on the actual entity — the population(s) that exist in nature.

How does this apply to iguanas? Catherine Malone (2000. *Mol. Phylogen. Evol.* 17:269–279) presented data suggesting that green iguanas from different regions in the Western Hemisphere are genetically distinct. These distinctions may or may not lead to interpretations suggesting that *Iguana iguana*, as currently defined, is actually a complex of separate species. However, the conservationist and the breeder can no longer, in good conscience, treat all green iguanas as if they were interchangeable. Conservation of unique insular populations, regardless of taxonomic rank, should be a high priority when management plans are developed and implemented. Allowing animals from different areas to breed could result (and has undoubtedly in many instances resulted) in hybrids that are no more “natural” than the “designer snakes” bred by some hobbyists.



Adult *Cyclura stejnegeri* from Isla Mona. This population was originally described as a species distinct from populations of *C. cornuta* on Hispaniola. Recent evidence indicates, however, that Mona Island animals are not genetically distinct, suggesting a very recent (possibly even human-mediated?) colonization of the island by Hispaniolan stock. Photograph: Robert Powell

In that same paper, Malone demonstrated a very close relationship between populations of *Cyclura* on Hispaniola (*C. cornuta*) and Isla Mona (*C. stejnegeri*). I have recently advocated the species-level recognition of the Mona Island population based primarily on allopatry and differences in morphological and biological (reproductive) traits. If additional data indicate that these populations do not differ sufficiently to warrant full-species status (suggesting that the differences merely reflect local conditions and that the two entities have only recently become separated), the general lineage concept would still apply. Any conservation plans that emphasize the preservation of both sets of populations would not be any less valid, nor would efforts to separate any captive populations by their origin be any less important.

Do ‘species’ exist in nature? Sure, but their identity may or may not correspond to our efforts at defining them. Plus, the animals don’t read our textbooks and they certainly don’t care what we call them.

