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Received: 2 February 1997 Reviewed: 24 August 1997 Revised and Accepted: 18 September 1997

> Chelonian Conservation and Biology, 1997, 2(4):619–621 © 1997 by Chelonian Research Foundation

# Galápagos Tortoise Nomenclature: A Reply

#### PETER C.H. PRITCHARD<sup>1</sup>

<sup>1</sup>Conservation International, 401 South Central Avenue, Oviedo, Florida 32765 USA

In my Galápagos tortoise book (Pritchard, 1996), I undertook to examine the names that have been proposed for Galápagos tortoises, to determine their legitimacy according to the Rules of Nomenclature and the known, probable, possible, or unknown provenance of the type specimens, as well as to identify those populations that did not have scientific names but that might ultimately be found worthy of them. In that I did not erect a single new name, I reject the criticism by Zug (1997) that I have potentially "enlarged the current nomenclatural morass associated with these tortoises." I did identify several decisions that will require petition to the ICZN for resolution, but those will be the Commission's decisions, not mine.

I considered myself to be "first reviser" of the early proposal (by Van Denburgh, Rothschild, etc.) that all of the Albemarle tortoise populations (except for the Volcan Wolf form) were taxonomically distinct. This self-designation was offered simply to resolve the dilemma of

which name to use when these various forms were synonymized, in that vicina and microphyes were proposed simultaneously, and elephantopus was unidentifiable, at least at the subspecific level. In this context, the authors cited by Zug (1997) would not qualify because they were actually proponents of the "full species" nomenclature for the Galápagos tortoise populations rather than revisers of that concept, and they did not elect to synonymize these forms. The dilemma that I sought to resolve (use of microphyes vs. vicina) did not exist at the time when the forms were both considered valid and distinct. Nevertheless, I concede that Mertens and Wermuth (1961) could be considered to be the first proposers of the synonymization of the Albemarle forms, although they presented no detailed justification and also included the distinctive form becki within their single named Albemarle population.

Zug (1997) criticized me for my lack of subspecies/ species diagnoses, and suggested that this omission emphasized that my book was merely an historical review of Galápagos tortoise nomenclature rather than a systematic review of populations. I do not deny the charge (indeed, see the title of my book!), and my failure to provide the diagnoses reflected not only that other authors, from Van Denburgh (1914) to Ernst and Barbour (1989), have already offered such diagnoses, but also reflected that my own extensive field experience with all surviving populations has led me to the conclusion that these diagnoses and keys rarely lead to an accurate identification. I intend no disparagement of Van Denburgh's excellent work, but he himself did not see the tortoises in the wild, whereas I have had the advantage of access to large series of live wild specimens, including good to excellent series of some populations (e.g., Alcedo, Chatham, Hood) for which Van Denburgh had extremely small series. I specifically examined contemporary specimens (live and recently dead) for such alleged key characters as the form of the eighth marginal and the contact between the pectoral scutes, and found them to be too variable to be appropriate for use in keys.

While one might be able to develop a key to identify large adult males of the more divergent populations, my emphasis was the opposite of this: that one cannot identify juveniles, most subadults, and many adults without knowing where they came from, and this is a strong argument that speciation, while conceivably in process, is not complete. Thus, it is clear to anyone who has spent time with them that the Galápagos tortoises do not fit cleanly into any particular systematic arrangement, and that there is a considerable element of subjectivity in the question of whether any given island population should be considered a species, a subspecies, or just a taxonomically unrecognizable morphotype or an isolated but essentially undifferentiated population. The forthcoming genetic analysis of the various populations by Ed Louis (pers. comm.) will be immensely useful, but the relation-

ship between that which is genetically definable and that which is worthy of nomenclatural recognition is still less than clear. The isolated populations have diverged morphologically to varying degrees, in part through genetic change (selection, founder-effect, etc.), in part through response of individuals to very different environmental conditions (especially temperature, food availability, and humidity on different islands). In that I had read the relevant literature, seen the key museum specimens, and had familiarity in the field with all of the populations (an opportunity that few taxonomists have enjoyed), I felt qualified to undertake this task, but with full recognition that any given classification of a complex, archipelagic species or species complex will never satisfy everybody. Actually, for those who argue vigorously for the "full species" concept, it is quite easy to set up a "show and tell" quiz to see if they can identify that which they claim to recognize. With the exception of the most extreme saddleback forms, they never can, even though the experienced, uninstrumented human eye remains, in my opinion, the best device for identification of Galápagos tortoises to the extent that this is possible at all. In view of the wide allometric and individual variation in most characters, if a character or parameter has to be measured for it to be evident, it is probably not useful for identification purposes, and the time has passed when whole new sets of shell ratios will offer worthwhile new insight into how to classify and name Galápagos tortoises. Conservatively, I have taken the "middle road," retaining subspecific status for the truly isolated populations, rather than either full species for each or a single binomial for all Galápagos tortoises.

As regards the Albemarle forms, it was once argued that mutually isolated tortoise populations existed along the chain of volcanoes that make up this large island (Rothschild, 1901). But it is now known that tortoises exist or existed in all of the intermontane lowland areas with the exception of that between Volcan Wolf and Volcan Darwin. Furthermore, the juveniles are uniform and similar in shape in all these populations, while the adults show such variation even within a population that an individual in hand cannot be assigned to a given population on morphological grounds. Individual exceptions may exist (e.g., some large male Sierra Negra and Cerro Azul tortoises sometimes have markedly flattened carapaces, and have been given the name guntheri), but even in this case, it has been shown by Ed Louis and Linda Cayot (pers. comm.) that this is more of a morphotype than a subspecies, in that it occurs in various parts of both mountains and is not associated with a specific geographic area. Furthermore, the generally smaller adult size of tortoises in some particularly arid areas (e.g., Cabo Rosa or Tagus Cove) is almost certainly a manifestation of environmental rather than genetic factors.

In 1984 I was entitled, under the Rules of Nomenclature then prevailing, to identify *Testudo californiana* as a *nomen oblitum*. It qualified for this designation, and there has been no objection raised in the subsequent 13 years.

Nevertheless, in 1997, Testudo nigra cannot be so dismissed. When I "resurrected" this name in 1984, the Rules allowed me to do so, and indeed would allow me to do so today. It would be the action of suppressing this name, not of advancing it, that, since 1985, would require petition to the ICZN. Moreover, nigra does not qualify for designation as a nomen oblitum. It was never a "forgotten" name, having been used extensively both in the last century and this, and the general mandate to "preserve nomenclatural stability" (which, taken literally, would forever ban any nomenclatural changes, however justified) has been qualified to recommend that special consideration should be given to situations in which nomenclatural stasis has existed for 50+ years. Even then, this is not a mandate for preserving manifestly incorrect names based upon type material that is of a different species from that with which the general public associates the name (see Pritchard, 1986, for another example in the general field of large or giant tortoises). Rather, it seeks to protect otherwise perfectly satisfactory and legal names from displacement following the discovery of obscure senior synonyms, of which I would consider the Aldabran elephantina vs. dussumieri question (Bour, 1984, and ongoing unpublished discussions) to be a good current example. Had Zug (1997) quoted the earlier part of Article 23b of the Code, he would have noted that the cautionary words refer to the reinstatement of an unused name, not just an older one, of senior status. I have argued abundantly in my book that nigra (used by Darwin himself!) could never qualify as an unused name.

To quote Zug (1997) again: "[Pritchard] argued that it [californiana] is a nomen oblitum, which it is not or at least it is no more of a forgotten name than nigra was when he resurrected it in 1984." It is true that the name californiana appeared subsequently in some of the more thorough synonymies for one or other of the Galápagos tortoises (e.g., Boulenger, 1889; Mertens and Wermuth, 1961), and thus was not literally a forgotten name, but it did not appear in the literature for 65 years following its first proposal despite an ongoing body of publications on the species as a whole, and it was never considered a valid name, in sharp contract to nigra which achieved extensive use as a preferred name for about a century. Moreover, the fact that I declared californiana a nomen oblitum in 1984, without challenge, while reaffirming nigra as the valid name is crucial. These were both legal steps.

Zug's (1997) suggestion that the popular, or nonspecialist, literature, should be the final arbiter in nomenclatural matters (what may be called the "democratic" approach, or "vox populi, vox dei") is so radical

that a detailed counterargument is unnecessary. I shall merely observe that it is a recipe for anarchy, would reduce systematics to unweighted vote-counting, and would logically result in the dissolution of both the Rules of Nomenclature and of the ICZN itself. In any case, Zug erred in stating that his preferred name, elephantopus, for the Galápagos tortoises, has the status of a nomen veneratum with 50+ years of unchallenged status. It was first proposed for the Galápagos tortoises as a whole by Mertens and Wermuth (1955) — i.e., less than 50 years ago, and only 29 years before I made the case for nigra — a decision that has, in general, been followed by subsequent serious systematists and reviewers, even if not by all non-systematists or amateur cheloniophiles. It is also a supporting (although not a crucial) argument that, in the case of a decision of this kind, it is very desirable to utilize a name for which the type specimen still exists — and that conceivably, using modern or even future technology, might even be identified some day!

What *is* a data-based analytical perspective, if not that which I have wrought?

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Received: 4 June 1997 Reviewed: 24 August 1997 Revised and Accepted: 9 September 1997

## IN MEMORIAM

Chelonian Conservation and Biology, 1997, 2(4):621–622 © 1997 by Chelonian Research Foundation

## A Man to Envy: James J. Parsons, 1915–1997

### **DAVID R. STODDART**<sup>1</sup>

#### <sup>1</sup>Department of Geography, University of California, Berkeley, California 94720 USA

James J. Parsons, whose monographic review of the green turtle, *Chelonia mydas*, published 35 years ago, helped define the course of modern studies of that marine reptile, died aged 81 on 19 February 1997. A student of the cultural geographer Carl Sauer, he succeeded that formidable figure as doyen of Berkeley geographers when Sauer died in 1975. His doctoral work on colonization in western Colombia, completed in 1949, introduced him to Latin America.

Because of this Colombian connection, and having met people from the Colombian Caribbean islands in Cartagena, he then turned to the high reef-encircled western Caribbean islands of San Andrés and Providencia, where he worked with the support of the Office of Naval Research during the spring and summer of 1953. In this study he ranged widely through the English-speaking settlements of the western Caribbean, including Belize, the Bay Islands, the Corn Islands, and Bluefields, as well as working on archives in Kingston and Washington. The results of this study in historical and cultural geography were published in 1956 under the title San Andrés and Providencia: English-Speaking Islands in the Western Caribbean. Chapter 6 dealt with the development of the sea turtle trade between Grand Cayman and the Nicaraguan Banks, as well as with the West Indian monk seal, Monachus tropicalis, by then already probably extinct. Parsons did not work on Grand Cayman itself, however; another of Sauer's students, Edwin Doran, completed his doctoral work there in the year that Parsons went to the western Caribbean.

Archie Carr had already begun his long campaign for the study and conservation of turtles with his *Handbook* of *Turtles* in 1952. His long-continued field work in Florida and Costa Rica was well under way by 1955, and *The Windward Road* appeared in 1956. Parsons, his curiosity thoroughly aroused by the San Andrés and Providencia study, was encouraged by Carr and began work on his wide-ranging monograph *The Green Turtle* and Man, which was published in 1962. I was soon to begin my own academic career and was able to send Parsons some information I had found in the Belize archives on early turtle conservation in Bermuda which was based on the western Caribbean experience. Thus