

SCIENTIFIC OPINION

LEARNING TO USE *OCHLEROTATUS* IS JUST THE BEGINNING

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In my 8 years as the systematics/taxonomy editor for the *Journal of the American Mosquito Control Association (AMCA)*, no single manuscript has caused more rancor in the taxonomic community than that of Savage and Strickman (2004). The manuscript initially was sent to 7 of the leading taxonomists in Culicidae. Eventually 9 reviews were returned; the original 7 and 2 unsolicited reviews. I received numerous e-mails and phone calls (some international) over the pros and cons of accepting the paper by Savage and Strickman (2004). Many experienced taxonomists strongly felt that in accepting this paper, the *Journal of the AMCA* was losing sight of science and the publication of papers based on the results of original research. One reviewer claimed that the issue has gone beyond the question of *Aedes* versus *Ochlerotatus* and entered the realm of "taxonomic anarchy." They pointed out that the Principle of First Reviser should only be invoked when "the precedence between two names or nomenclatural acts cannot be objectively determined.

Reinert (2000) objectively determined that *Ochlerotatus* should have separate generic status, whereas Savage and Strickman (2004) have taken an entirely subjective approach to refute Reinert's action in the absence of hypothesis-driven investigation. I don't agree with Savage and Strickman (2004) that *Ochlerotatus* should be replaced as a subgenus of *Aedes*. I outline below my reasons and hope that these reflect the objections of the many individuals who spent hours reviewing the manuscript. In the end, the paper by Savage and Strickman (2004) was accepted to promote interest and the opportunity for discussion of impending issues in the mosquito community at large.

First of all, the *Journal of the AMCA* readership should realize that in terms of principles the source of the acrimony elicited by Savage and Strickman (2004) is very old. Although there are many definitions of taxonomy and systematics, I teach my students that taxonomy is the science of species classification and that systematics is the science of estimating evolutionary relationships among species. If I'm lucky, this usually evokes the questions: So, what's the difference? Why do we need 2 definitions (and will they both be on the exam)? The answer allows me to talk about the criteria by

which species are classified into higher taxonomic grouping (e.g., subgenera, genera, tribes, and so on). I emphasize that these criteria often involve easily identified morphological characters that the taxonomist(s) hope will allow accurate and rapid species identification in a dichotomous key. Based upon the similarity of these characters to those in already-classified species, the new species is assigned to an existing subgenus or genus and automatically falls into place with regards to the higher taxonomic groupings. The critical division between taxonomy and systematics rests in the fact that the characters used to place species into higher taxonomic categories are chosen for convenience during identification, and usually do *not* accurately reflect evolutionary relationships.

Ultimately, if you are a pragmatist this all boils down to the following question: Who cares what generic name is associated with a species? As long as a specimen is correctly identified to species, is it important to which genus the species is assigned? After all, species are the units of concern for public health workers, not genera. In fact, keys often are developed that include only species. It was the dream of Willi Hennig, the father of modern cladistics and systematics, that one day higher taxonomic groupings would accurately follow evolutionary relationships (Hennig 1966). Specifically he hoped for a "natural" classification system in which genera contained species that had all evolved from a single ancestor (were monophyletic). In other words, Hennig hoped that in the future there would be no difference between taxonomy and systematics. These issues are not merely academic; there are important practical advantages to a natural classification system. On a global basis, there are frequently widely varying opinions among taxonomists about the placement of a species into different subgenera or genera. A natural classification system would eliminate this ambiguity among taxonomists; genera would have to be monophyletic. In addition, traditional taxonomy based on the similarity of easily identified characters frequently hides or confuses important information. Biogeography, morphology, vector competence for pathogens, life history characters, larval ecology, physiology, and other factors might be more apparent and make more sense if understood in an evolu-

tionary framework. A natural classification scheme might indicate how many times a particular morphological or physiological character arose over the course of evolution.

Up until the last 15 years, there was a very practical problem with the construction of natural classification systems. Systematists had no objective means to assess which morphological characters accurately reflected evolutionary relationships and which characters were misleading (i.e., were similar among unrelated groups of species [polyphyletic]). Yet these were the only characters that were available and numerous enough to use in phylogenetic reconstruction (in building evolutionary trees). It wasn't until the advent of the polymerase chain reaction and DNA sequencing that insect systematists 1st had access to a large number of objective characters (nucleotides) with which to estimate ancestral relationships.

What bothers me about the paper by Savage and Strickman (2004) and their desire to return *Ochlerotatus* as a subgenus of *Aedes* is that such a decision postpones the inevitable. Regardless of the inconvenience associated with changing generic names and reconstructing and overturning higher taxonomic categories, natural classification will eventually prevail. It is a system that unites taxonomy and systematics, facilitates an evolutionary understanding of speciation and adaptation, and, perhaps most importantly, provides an objective system for classification. The use of molecular systematics to estimate evolutionary relationships among Anophelinae is already well underway (Krzywinski and Besansky 2003) and I am aware of at least 2 groups pursuing this goal among Culicinae. It seems to me that the goal of having a natural classification system for mosquitoes (and other arthropod vectors) will be attained within the next 5–10 years. My colleagues and I have been personally involved in this effort for ticks (Klompen et al. 2000). I predict that eventually *Ochlerotatus* and many other genera will arise in place of *Aedes*. Many species may have to be moved into different extant genera, some existing genera will have to be eliminated, and new, monophyletic genera will be erected. In the end, I predict that *Aedes* will include too many monophyletic groups to be of any descriptive value except as a much higher taxonomic category.

Finally, I would like to respond to the many, often angry, phone calls and e-mails that I have received during fall 2003. What has come to trouble me the most about this *Aedes* vs. *Ochlerotatus* issue

has been the disregard for scientific methods of investigation, the whimsical preference for names based on customary usage, and the touting of government agencies to enforce taxonomic usage as though it was a political issue outside the purview of science. It is my opinion and the opinion of several of the reviewers that in the long run it won't matter what the United States Department of Agriculture, Centers for Disease Control, and Department of Defense advocate and put into their databases. The international scientific community united in the common goal of developing a natural system of classification for mosquitoes will eventually come up with the right answers, and the governmental agencies will have to follow suit.

Savage and Strickman (2004) make it acceptable to continue to use the term *Aedes*. However, I will continue to use and teach *Ochlerotatus* because I believe in natural systems of classification. Recently, I had to revise one of my manuscripts on the genetics of sex determination in *Ochlerotatus triseriatus* (Graham et al. 2004) because neither the editor nor one of the reviewers realized I was working on a mosquito and the other reviewer thought that I was working on some obscure mosquito taxon. In the short term, *Ochlerotatus* is inconvenient, but in the long term *Ochlerotatus* is inevitable. I personally look forward to this continued debate and I hope that the community of mosquito biologists will consider the approaching taxonomic storms with an open mind. Learning to use *Ochlerotatus* is just the beginning.

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