Red Wolf Specimen Identity

I read "Molecular Genetics of Pre-1940 Red Wolves," by M. S. Roy, E. Geffen, D. Smith, and R. K. Wayne (*Conservation Biology* **10:**1413–1424) and am pleased that *Conservation Biology's* editorial policy approved of listing the specimens analyzed in the study by their museum catalog number. Many editors believe such information is superfluous. I would not be able to clarify identities if the specimen numbers had been omitted from the report.

Robert Wayne sampled specimens of canids in our National Museum of Natural History Mammal Collections in 1990, and a collaborator sampled additional specimens in 1991. Robert Fisher wrote to Wayne in 1991 to point out that two of the specimens that had been assumed to be red wolves were coyotes. Nevertheless, one of these (USNM 265986) appears as one of several "red wolves" from Oklahoma (p. 1415). Further inquiry not only confirms that specimen 265986 is a coyote but that it came from California, not from Oklahoma.

As part of the authors' premises leading to the hypotheses tested in their research, they make the statement that ". . . the red wolf is a native species of the American South, originating in the Early Pleistocene, and is ancestral to modern coyotes (*C. latrans*) and gray wolves (*C. lupus*)." They attribute this premise to Nowak (R. M. Nowak. 1979. North American Quaternary *Canis*. Monograph no. 6. Museum of Natural History, University of Kansas, Lawrence, and R. M. Nowak. 1992. The red wolf is not a hybrid. Conservation Biology **6:**593–595). Nowak (1979), however, states that "The most reasonable exlanation is that *C. rufus* represents a primitive line of wolves that has undergone less change than *C. lupus*, and has thus retained more characters found in the ancestral stock from which both wolves and coyotes arose." Nowak does not say that red wolves were ancestral to coyotes and gray wolves.

Although the conclusions reached by Roy et al. may stand the test of additional research, it appears that the authors have been less than rigorous in interpreting the literature and confirming the identity of their material. Wayne has known since 1991 that there was a problem with the identities of two of the samples he analyzed. Our collection of mammals approaches 600,000; as should be assumed when working in any collection, expecially one of this size, errors exist in the identification and provenance of specimens and with other data associated with the specimens. Finding and correcting these errors is an ongoing process for the collections management staff. We provide information to the users of the collection pointing out that errors do exist and that investigators have the responsibility to confirm the identity of the material they are studying.

Alfred L. Gardner

Biological Survey Program, USGS Patuxent Wildlife Research Center, National Museum of Natural History, Washington, D.C. 20560, U.S.A.

Conservation Biology and Agroecology in Europe

Ecologists working in the Americas are justified in their concern for the troubling division between conservation biology and agroecology (Vandermeer & Perfecto 1997; Jules & Dietsch 1997). By contrast, the dichotomy between conservation biology and agroecology in much of Europe has to some extent lessened. This is due not so much to a concerted effort to integrate the two disciplines, but rather to the essential recognition of historical facts, namely, that nearly every square kilometer of the continent, with the possible local exceptions of steep gorges, cliffs, and the highest mountain tops, is culturally modified. Over the last 10,000 years, and even longer in the south, Europe has been grazed, burned, deforested, reforested, plowed, and sown. The idea of protecting the biodiversity of a "pristine" system surrounded by a managed landscape is invalid in a European context. Instead, conservation biologists must often determine the form and extent of human activity with which biodiversity can best be enhanced, often within national parks or other protected areas. In doing so, emphasis is placed on understanding not only the workings of the "natural" system, but also the effects of various stocking rates of domestic livestock or changing cultivation practices.

The marriage in Europe between biological conservation and agroecology can lead to management prescriptions that in the Americas may seem paradoxical, such as the reintroduction and encouragement of traditional agricultural practices in areas designated as national parks. Attempts to establish parks and protected areas that exclude traditional forms of land use and husbandry are often politically unrealistic on lands that have been in private or common ownership for centuries. The history of human use and modification is in many cases the very precursor to the attributes now worth conserving, and many well-meaning efforts to exclude or severely curb land use have actually led to a decline in biodiversity. In Europe, as in the Americas, modern, intensive agricultural practices are a threat to biodiversity and environmental integrity. In Europe, however, the exclusion of every form of agricultural land use, given the continent's his-

tory, is not always a sound conservation measure.

Although free from the pre-European-settlement reference point often used in the Western Hemisphere as a baseline for judging "naturalness," Europe is not without its cases of conflicting conservation interests. Small-scale livestock operations, for example, based on extensive use of open forested and alpine ranges, are promoted in many countries. Although considered environmentally and socially sound alternatives to large-scale industrialized meat production, small, free-ranging flocks are increasingly susceptible to predation by recovering populations of bears, wolves, and lynx (Breitenmoser 1998). In the ensuing conflict, interests on both sides of the predator-livestock debate can argue in the name of biodiversity. Discussions of "who came first" and of what represents a natural ecosystem become moot in the Old-World context.

Efforts are now under way to promote forms of agriculture and land management that enhance biodiversity, primarily by attempting to channel subsidies into more environmentally benign, often traditional, production systems. The design of agricultural production systems is usually considered the domain of the agronomist, or more recently of the agroecologist. Defining goals for biodiversity enhancement will, however, require the conservation biologist's lens worn by scientists willing to work in both "pristine" and "tainted" ecosystems.

Jerry T. Warren

High Mountain Ecological Research Station-Finse, c/o Biological Institute, Box 1050, University of Oslo, 0316 Oslo, Norway, email jerryt@bio.uio.no

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Putting Genes Back into Their Lab Bottles: Much Ado about Nothing?

Clinchy and Krebs's (1997) claim of the "domination of conservation biology by lab scientists" is unfounded. If lab-based or "small population-paradigm" researchers are dominant, it is not in terms of their publication productivity. In a comprehensive review of biodiversity literature (over 2500 surveyed papers) published from 1987 to 1995 in five major international journals (*Conservation Biology*, *Biological Conservation*, *Biodiversity and Conservation*, *Ecological Applications*, *Journal of Applied Ecology*), neither of these two related disciplines comes anywhere close to "dominating." In terms of study methodology, lab experimental papers represented only 3–7% of the total published effort, compared to levels of 5–33% for field experimental studies, and 23–53% for field observational studies (France & Rigg 1998). In terms of study system, only 2–8% of all papers were lab- or modeling-based, whereas 20–39% were forest-based and 10–43% were grassland- or agricultural-based. Finally, in terms of agent stress, lab- or field-based studies assessing population viability (i.e., the so-called "small population paradigm") accounted for only 1–25% of all studies, in contrast to field studies pertaining to habitat loss (i.e., the "declining population paradigm") which represented 19–43% of the total research effort. Caughley (1994) paints a picture of a biparadigmal basis for conservation biology based on study of either declining populations or of small populations, and Clinchy and Krebs state that "the basic distinction in conservation biology is between field biologists and lab scientists." There is, however, no equality of research productivity between the two groups. Despite Clinchy and Krebs's belief in the "greater publication rate among lab scientists," lab studies of population viability account for only a small proportion of the measured research productivity of all conservation biologists. One must seriously question, therefore, what all the fuss is about in the first place (e.g., Hedrick et al.'s [1996] contention that "hostile political forces are attempting to discredit many conservation efforts" or Clinchy and Krebs's referral to being "brought before the Inquisition on charges of heresy").

Our recent findings (France & Rigg 1998), however, point to several interesting conclusions about the performance merits of these two groups of researchers. The observation that the efforts of the small-population paradigmists or lab scientists are perceived as being so threatening to others, despite the fact that they really produce only a small fraction of the total research effort in conservation biology, suggests that their few papers may carry a far greater weight than the many produced by field scientists or declining-population paradigmists (to the credit of lab-based conservation biologists). And, the observation that declining population paradigmists or field scientists are so productive, despite the fact that "lab techniques produce publishable results in a matter of weeks, whereas data in field biology usually take years to accumulate" (Clinchy & Krebs 1997), suggests that field research is translated into final products more efficiently than that of lab scientists or small-population paradigmists (to the credit of field-based conservation biologists).

Clinchy and Krebs make the further point that "lab scientists have almost all the money because advanced capitalist economies are driven by the production and consumption of high-tech gadgetry." Our additional findings (France et al. 1998) demonstrate a strong correlation between national economies and publication productivity of conservation biologists and suggest that the efforts of field scientists

are commendable. If Clinchy and Krebs are correct in their assertion of an economically impoverished environment for field research, then those individuals practicing such should definitely receive a pat on the back because they are producing publishable science much more cost-effectively than are their lab colleagues.

In the end, all this amounts to a celebration of a job well done by field conservation biologists rather than a reason to develop some sort of defensive inferiority complex fueled by impressions of greener grass on the other (in)side of the laboratory window. In short, relax (emotionally) and keep up the good productive work rather than engaging in jealous, potentially acrimonious, knicker-knotting frets.

Robert France

Harvard University, Graduate School of Design, 48 Quincy St., Cambridge, MA 02138, U.S.A., email rfrance@gsd.harvard.edu

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