FACTS, INFERENCES, AND SHAMELESS SPECULATIONS

Dowitcher DNA

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*Short-billed Dowitcher (*Limnodromus griseus*). Photograph/Frank Schleicher/VIREO (s19/17/025).*

ROLLO BECK (SEE *AMERICAN BIRDS* 40:385–387) emerged from the fog, once again. It was Beck, after all, who in May 1946 strode into the University of California Berkeley's Museum of Vertebrate Zoology with a gunnysack of Central Valley-wintering dowitchers, emptied it onto Frank Pitelka's desk, and challenged him to figure out what was going on with dowitcher taxonomy.

Were I suddenly buried in dowitcher bodies, I most likely would have grabbed from the pile and flailed back. Pitelka instead wrote a monograph. He established that the dowitchers, long and short-billed, were different. His work helped resolve one of the more vexing species-pair problems of the day and has since become a classic in avian systematics (Univ. Calif. Publ. Zool. 50:1–108).

Almost three decades later Claudia Wilds pointed out how to distinguish them reliably in the field, at least some of the time (Wilds & Newlon; Birding 15:151–166). Now John Avise and Bob Zink have risen to Beck's challenge to tell us just *how* different these two forms actually are (Auk 105:516–528).

Avise and Zink ground up dowitcher mitochondria and poured them into test-tubes. From the mitochondria they extracted DNA and then carried out a series of chemical procedures that revealed an extraordinary finding. Long-billed and Short-billed Dowitchers are not merely distinct forms of a basically similar beast. These species are *so* distinct that they differ by more than most congeners. Zink and Avise estimates that Long-billed Dowitchers and Short-billed Dowitchers have been moving along their own distinct evolutionary pathways for 4 million years, not too different from human separation from the other Great Apes. Better birding through chemistry, to say the least!

Why does this matter, other than to that small group of Beckians who track every example of his resurrection?

First, bear in mind that there are some other examples emerging from the mitochondrial DNA literature suggesting large genetic divergences among forms that look like basically the same bird. Black-capped and Carolina chickadees have been separate for 3–4 million years (Mack, Gill, Colburn, Spolsky; Auk 103:676-681). Avise recently found unexpected genetic patterns in Seaside Sparrows, which, among other things, reveal that some better choices could have been made in the Dusky Seaside Sparrow recovery plan (1988 A.O.U. Annual Meeting). And looking beyond birds, the pattern is even clearer. Morphological similarity can mask immense genetic differences. Salamanders provide stark evidence on this point. Appearance and genetics in salamanders has been decoupled: similar forms may be very different genetically; apparently different forms may be quite similar once one looks closely at their genes.

Fine. This matters to some small ca-

*Long-billed Dowitcher (*Limnodromus scolopaceus*). Photograph/P.G. Connors/VIREO (c05/1/037).*

bal of clannish classifiers who worry about mystical decouplings between morphology and genetics. In all likelihood if they didn't squint with such relentless fervor at their electrophoretic gels, searching for truth in fast and slow electromorphs, they would probably be able to tell the species apart in the field. Actually, don't believe it. Some of those gel jocks rank among the very best of the field ornithologists. And even more, their work truly is revealing unperceived evolutionary heterogeneity in North American birds (and South American, and African, . . .).

In truth, it matters far beyond the fields of agar in which these scientists play, and even beyond the confines of avian systematics. I will argue here that there are two major and very different implications of this work. But before I get to that I should mention a bombastic debate that is running through ornithology these days. The issue is what makes a species.

On the one hand, you will find advocates for the traditional "biological species" concept. They contend that species are things that are reproductively isolated from one another. This view has dominated ornithological thinking since Ernst Mayr and others first proposed it four decades ago. (*Systematics and the origin of species,* Columbia University Press, NY). On the other hand, you will encounter a new body of theory swirling around the "phylogenetic species" concept. By this view, the limits of a species are those individuals that share a common evolutionary history and whose gene pool is independent of other sets of individuals (Cracraft, *Current Ornithology* Vol. 1:159–187).

Laying aside the rhetoric, there is much in common between these two schools of thought, but there are also substantial philosophical differences. One central point is that phylogenetic species concept does not confuse the pattern (different species) with the process (how those species are achieved). The biological species concept, according to phylogenetic species concept advocates, leaves room for no means to produce species other than reproductive isolation. Phylogenetic species concept advocates maintain this is a poor way to run a science because the answer (reproductive isolation) is built into your question (how do species differentiate).

I think there is some merit to that reasoning, and while some would decry the presumptuousness of a challenge to so revered a concept as the biological species concept, the phylogenetic species concept emerges as less presumptuous as a scientific theory. It does not presume the mechanism nor does it depend upon the hypothetical, future success of the inability of individuals from one population to mate with those from another.

At the risk of oversimplification, the best way to contrast these theories is by a hypothetical example: Imagine two "populations" of a flycatcher which, because of geographic separation, do not interbreed. It matters not how they look: they could be indistinguishable or strikingly different. Let's assume the latter. By the biological species concept, if there is evidence that they could interbreed if the geographic barriers disappear, then they would be the same species. By the phylogenetic species concept, if there is genetic (or other) evidence that the two populations are evolutionarily independent, then they are separate species. What might that evidence be? One example: mitochondrial DNA work showing they have not exchanged DNA sequences for several million years. For a case example see Bob Zink's analysis of towhees (*Condor* 90:72–82).

Both species concepts have their problems, particularly in translating the basic theory to decisions about splitting and lumping. With the biological species concept prevailing over the last three decades we have witnessed waves of lumping. If it prevails, the phylogenetic species concept may reverse this trend.

So back to the original question. What are the implications of the mitochondrial DNA work beyond the labs of biochemical systemacists? I see two of import:

The first lies in conservation Our legal framework for protecting species rests principally in the Endangered Species Act. Many factors enter into determining what makes a species endangered and hence eligible for statutory protection. Somewhere along the line someone asks how many individuals there are in the population. Here the difference between biological species concept and phylogenetic species concept could become vitally important, especially if we have the genetic tools—mitochondrial DNA—to reveal pockets of independent evolutionary units.

Let me give you an example. Along with several of its northwest chapters, the National Audubon Society recently petitioned the U.S. Fish & Wildlife Service to list the Marbled Murrelet as threatened or endangered. In the Pacific Northwest, all available evidence indicates that this species depends critically upon old-growth forest for nesting, a habitat that is disappearing at catastrophic rates. Population numbers in this region are small. Yet one can look north to the Gulf of Alaska and find 50,000+ pairs breeding in rocky talus slopes. Few if any morphometric differences exist between birds of these areas, despite the dramatic differences in habitat choice. Are they evolutionarily independent? The traditional tools of ornithology have little to reveal. Yet mitochondrial analyses might, if performed, demonstrate marked separation. This finding would demand immediate protection for the Northwest form as threatened or endangered.

I am indulging in blatant speculation. But as the case histories of genetic segregation of similar forms mount, possibilities like this become ever more plausible. Our landscape may be littered with far more species than anyone would have dared contemplate. Instead of a broadly continuous pattern of geographic variation, mitochondrial DNA analyses may reveal mosaics of small, genetically independent populations which by the phylogenetic species concept should be considered separate species. The tools (and the challenge!) this would provide for the Endangered Species Act (the Endangered Gene Pool Act?) would be extraordinary.

The other impact of mitochondrial DNA and the phylogenetic species concept is more fundamental to ornithology and it may be destructive: those unique bonds that link amateur and professional ornithologists will be stretched thin, perhaps irretrievably so Ornithology, more than any other science, has thrived because of the continued contribution of nonprofessionals to the research process. You contributors to and readers of *American Birds* are witness to that with every page that you turn.

Chemistry lost its amateurs as the issues at stake left the realm of the visible, as they began to hinge on matters utterly out of the reach of those bereft of equipment and without very specialized training. The same happened to physics, where only in select areas of astronomy do amateurs sustain an important role—straining at the edge of visual telescopes to detect new comets and other bodies careening through the heavens.

With mitochondrial DNA and the phylogenetic species concept, we may encounter an ornithology whose basic unit, the species, will become as inaccessible to the nonprofessional as would the quark have been to Newton. Surely this is hyperbole, but not outlandishly so. The arguments in systematic ornithology will hinge ever-more frequently on facts and measurements that require laboratories and ultracentrifuges, not binoculars and calipers. Jargon will escalate. Access to the literature will become ever more specialized. Ultimately the species map followed by professionals will be quite different than that of the birding community. And their paths will intersect far less frequently.

And what would Rollo Beck have to say about this? I suspect he would have donned his hipboots and melted off into the Los Banos marsh after a few more dowitchers.

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