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Bert Murray, 1933–2010
(Photograph taken in Kenya by Patti Murray, date unknown.)

Bert Murray (né Kusanobu), Professor of Ecology and Evolutionary Biology at Rutgers University and a Fellow of the AOU (1977), was widely recognized for his original approaches to biology and persistent challenges to conventional ideas in demography and population biology. For few others was Cervantes' assertion that "Facts are the enemy of truth" so fitting a maxim.

Short and dark-haired, reflecting his partial Japanese ancestry (wartime hysteria prompted a surname change in 1942), Murray was born in Elizabeth, New Jersey, the eldest of three sons of an academically uninterested family. He had no special interest in birds until pursuit of a Boy Scout merit badge led him to the Urner Ornithological Club, a demanding training ground that from the late 1940s to 1970s spawned three Presidents and perhaps a dozen Fellows of the AOU. A star birder by his early teens, he was envied for a keen ear and an uncanny ability to find spring-migrant *Oporornis* warblers in the now obliterated Bound Brook swamp. He had no thought of higher education. He was interested in adventure and travel, and after high school served briefly in the U.S.

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Navy. Birds attracted him to Cornell University but, when course requirements precluded him from immediately taking the classes that interested him most, he left after three semesters. The highlight of that period, and the "best birding moment" of his life, was cutting classes and hitchhiking to South Carolina to spot what may have been the penultimate Bachman's Warbler.

He re-enlisted in the military, this time the Air Force, where he served four years stationed in England and became expert in interpreting aerial photographs and stereoscopic images. In 1958 he returned to New Jersey and enrolled at Rutgers University, where ornithologist–ecologist James Baird inspired him to stay in school and consider advanced training. This led to a Ph.D. under H. B. Tordoff at the University of Michigan in 1967. After a post-doc at Cornell, he taught briefly at Michigan State, but left when the department chair insisted that his own text be used in Murray's course. He joined the Rutgers faculty in 1971, retiring as professor emeritus.

In 1958, Murray began serious ornithological studies by banding fall migrants at Island Beach, New Jersey. His findings immediately resulted in several papers on the biology and behavior of migrants in coastal situations. The Blackpoll Warbler became a special interest, because he documented extensive coastal movement and could find no evidence in New Jersey or elsewhere for the notion that most blackpolls in autumn migrate across the ocean from New England to northern South America (e.g., Auk 106:8–17, 1989). Although his position was savagely attacked (Journal of Field Ornithology 66:612–622, 1995), his data and arguments persuaded the AOU Check-List Committee (1998) to reconsider its earlier (1983) acceptance of the transoceanic route and judge the question unresolved.

Murray was not a disputatious person, and he did not set out to challenge authority. However, he had little faith in pronouncements from experts, because reasoning told him that much "accepted" knowledge was invalid. He also realized that every set of data could have more than one interpretation and felt personally challenged to find and examine alternatives. An early example stemmed from his thesis (1967) on the ecology of Sharp-tailed and LeConte's sparrows, in which he began to consider the evolution and consequences of interspecific territoriality. He later developed that subject in a series of papers in which he argued that interspecific territoriality, rather than being an adaptive trait, stemmed from misdirected intraspecific territoriality. His doubts about the objectivity of editors and reviewers were fertilized in this period when his article "A critique of interspecific territoriality and character convergence" was found acceptable by *Evolution*—provided that he revise it to coincide with the view of the prominent ecologist he was criticizing! It was later published, as submitted, in *The Condor* (78:518–525, 1976).

Although he had great respect for his colleagues' ability to do descriptive work, by the 1970s he became convinced that progress in biology was stalled by studies that resulted in ad hoc explanations for particular data sets. A good example was the plethora of ideas advanced in that period for the evolution of reversed sexual size dimorphism. With philosopher J. Brownowski, Murray believed that the ultimate goal of science was "to discover unity in the variety of nature." Accordingly, he began to approach problems theoretically, by employing the fewest assumptions necessary to deduce general principles (or laws) from which concrete predictions could be made. In championing this method he followed physicists like Einstein, who "despaired from determining principles from facts." This resulted in his further estrangement from the biological establishment, which had universally concluded that laws were acceptable in physics but that biology was too complex for laws. He directly countered that view in several papers that developed his "Three Laws of Evolution" (e.g., Proceedings of the 22nd International Ornithological Congress, 624–637, 1999; Biologial Reviews 76:255–289, 2001).

Murray's main interests converged on problems in demography and population biology, even though he had little mathematical training beyond high school. In the days before computers he used pencil and paper to simulate theoretical populations consisting of so many males and so many females, with the females producing so many eggs or young. From this he was able to derive for himself the Lotka equations in their discrete form and understand what the symbols and numbers meant. This allowed him to think theoretically and begin constructing "thousands" of new and more informative kinds of life tables, which in turn led to equations linking demography, population dynamics, and life-history traits. His resulting book, *Population Dynamics: Alternative Models* (1979), discussed new models of population dynamics, predation, competition, and the evolution of clutch size. He also continued to develop demographic ideas stemming from the interpretations of life tables ("you can't do population biology without them"), as well as dealing with ecological questions including the calculation of "little *r*," the meaning and measurement of fitness, and "the myth of density-dependent regulation" (e.g., Oecologia 53:370–373, 1982; Oikos 69:520–523, 1994).

Murray collaborated with several colleagues who had long-term data sets on various species. His first genuinely new theory concerned demography and the evolution of clutch size, which allows investigators to predict clutch sizes from other demographic parameters. The Murray-Nolan equation (Evolution 43:1699–1705, 1989, with Val Nolan, Jr.) led to publications on annual and lifetime reproductive success (Auk 108: 942–952, 1991); a new explanation for considering asynchronous hatching as a means of maximizing reproductive success (Auk 111:806–813, 1994; 123:798–721, 2006); the evolution of monogamy (Ornithological Monographs 37:100–107, 1985) and mating systems in birds (Evolutionary Biology 18:71–140, 1984); and the evolution of sexual size dimorphism (Current Ornithology 3:1–86, 1986, with J. R. Jehl, Jr.). The value of his theoretical approach to mating systems was quickly validated by his prediction that males should outnumber females in the polyandrous Spotted Sandpiper, when experts held that the sex ratio was 1:1. Working with life tables also allowed him to devise an equation that related a population's mean size over time as a function of life expectancy at birth (Annales Zoologici Fennici 40:465–472).

Despite his accomplishments, he was often disappointed in not finding an audience. His publications were deliberately ignored, without being refuted, and many manuscripts were repeatedly rejected. The tyranny of editors who blindly accepted reviewers' opinions and gave the author no chance to respond was a sore point. He responded anyway. In some cases rejections stemmed from the reviewers' unfamiliarity with hypothetical-deductive methods or, worse, their lack of grounding in the philosophy of science; in other cases he ascribed it to their innumeracy. Calculating breeding success, for example, is an essential component in population studies. In 1961 Harold Mayfield had recognized that the customary method of dividing successful nests (or eggs) from total nests was imprecise because it did not allow for nests found after incubation had begun. Mayfield's solution was immediately adopted by authors of more than 1,300 papers, who grasped the tail of the elephant and followed along. But did anyone test it? Murray simulated a population for which entire nest histories were "known," did the math, and got different results. The Mayfield method was flawed. One might have thought that his demonstration and explanation would be of great interest. Yet a half dozen journals turned it down, illustrating yet again that "it is difficult for a reviewer to change his opinion after he has published it in a refereed journal."

Another typically "Murrayan" manuscript was stimulated by the report of an Ivory-billed Woodpecker in 2005. He had accepted the sighting and immediately wondered what his chances of seeing one might be. Few would dare to approach this question scientifically, because so little is known about the biology of the ivorybill. By extrapolating natural-history information from other large woodpeckers and applying his unique knowledge of the dynamics of small populations, he was able to calculate how large the population in 1950 (last verified sighting) would have to have been to produce a survivor in 2005. It turned out that the odds of anyone adding the ivorybill to his life list were not good. One might have thought that this would be of great interest to conservationists—or those planning an expensive recovery plan. Not so. Reviewers termed extrapolation irrelevant, and "Demography and population dynamics of the Ivory-billed Woodpecker" was rejected by several journals.

His passion for travel never abated. With his wife, Patti, a professional wildlife photographer, Murray visited all the continents, returning repeatedly to Africa. Travel ended in the mid-2000s, when a persistent "pneumonia" was belatedly diagnosed as nonsmoker's lung cancer. The delay was consequential and surgery proved ineffective. Ever the scientist, he volunteered to commute from New Jersey to Washington, D.C., for a series of painful experimental treatments at the National Cancer Institutes at NIH. He died on 3 August 2010. In his last months he continued birding when able, and worked on completing his final book.

Murray's ideas and accomplishments remain to be fully appreciated, in part because he was writing in a language and using models that reviewers failed to understand. His papers "Are ecological and evolutionary theories scientific?" (which he considered his most important; Biological Reviews 72:255–289, 2001)" and "Is theoretical ecology a science?" (Oikos 87:594–600,1999) tossed down the gauntlet and revealed his frustration with colleagues reluctant to approach science in predictive and theoretical ways. Nevertheless, he continually sought them out, asking them to share or collect data in ways he could use to test new ideas. Despite his disappointments, he remained open-minded, asking only that challenges to his ideas be aired publicly or, at least, that editors, reviewers, and colleagues respond to his rebuttals and tell him why he was wrong.

Over his career, Murray published 100 papers, 80% of them single-authored. He considered that his most important work involved theory and the proposal of laws in biology. However, his prescient essay criticizing the fable of continuous economic growth ("Continuous growth or no growth? What ecologists can teach the economists,"*New York Times Magazine*, 10 December 1972; reprinted in Dialogue 6:43–50, 1973) should not be overlooked. It can rightly take a place alongside Hardin's "The tragedy of the Commons" as required reading for budding ecologists, economists, and, especially, legislators.

At his last scientific meeting (2010), Murray rose to comment on a paper involving breeding success, stated that the Mayfield method was in error, and invited discussion. He was no longer concerned by the lack of response from his contemporaries, but he was severely disappointed that younger colleagues were unwilling to challenge dogma. His philosophy, procedures, and way of thinking, and some unpublished papers (including those on the Mayfield method and the ivorybill) can be found in *What Were They Thinking? Is Population Biology a Science?: Papers, Critiques, Rebuttals, and Philosophy* (Infinity Publishing, West Conshohocken, Pennsylvania, 2011). It also includes dissections of two of the most sacred cows in the ecological herd—logistic population growth and density-dependent population regulation—not to mention world-class responses to editors and reviewers. He would appreciate an audience and rebuttals.

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