

## SUMMARIZING REMARKS, PART I

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Let me start by thanking Frank Pitelka for his efforts in putting this symposium together. I have enjoyed each of his papers, many of his monologues, and his several summaries.

I would also like to compliment the participants for offering such a stimulating group of contributions. Rather than rehash the papers, I want to discuss some of the major points that were raised, as well as some ideas about possible future research.

Obviously, we have come a long way recently in our studies of shorebird distribution and ecology. One very impressive point was that we are starting to get some solid data on how shorebirds utilize certain areas through an entire season. Also, up to now, most of us have looked very narrowly at habitat requirements, and we have been made aware of the importance of alternate feeding or roosting sites that may be used when the prime habitat is disturbed, flooded by fresh water, etc.

We have also learned that some species are highly philopatric and may return to the same pond or stretch of beach each winter. This, of course, has important management overtones.

We are also beginning to get a better handle on the distribution and importance of staging areas, such as Bolinas Lagoon or the northern High Plains. And is there anything to compare with the Copper River Delta and its breathtaking hordes of migrants? We are sobered to realize that there are no other areas to take its place if it is disturbed.

We do not yet have sufficient data on several topics. For example, we know very little about geographic patterns in the distribution of age and sex classes in most species, although for a few it is clear that adults and juveniles may occupy different areas in winter or may utilize different migration routes. Such patterns are so widespread in migratory passerine birds that it is surprising that they have been overlooked or unstudied in shorebirds.

We also need more thinking about the role of tradition in the establishment and maintenance of migration routes and wintering grounds. Much important work on this subject was done by Al Hochbaum years ago, and it is a subject requiring further study.

In reviewing the presentations on ecology, I think we must all be impressed with problems faced by our migratory shorebirds. Consider a bird programmed by 10,000 years of postglacial evolution to hit a specific staging area after a flight of hundreds of miles. It arrives exhausted, fat reserves nearly gone, only to find that what was a slough a few months ago is now a parking lot. And no alternate sloughs are available. I think that the data we have heard on philopatry, migratory routes, and tradition all tie into a nice package that we can use to document the need for wetlands preservation. With these data we are in an excellent position to suggest more appropriate responses to the environmental actions that confront us.

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Although our data on shorebird distribution are relatively good, they are unsophisticated. Most of the literature contributed by the amateur community has been concerned with range extensions or rarities. While such data are important in determining trends or shifts in populations, for present needs they are largely irrelevant. What we don't yet know, for example, is what percentage of the Sanderling population on the west coast of the Americas winters, say, between 40-50°N, nor do we know how wintering populations of this (or any) species are segregated by age or sex classes.

Some of these data can be derived from Christmas Counts, and the lagoon surveys such as have been conducted in California, and which should be expanded to other areas. Such data are important, because in planning for the future we might want to trade off "juvenile habitat" for that frequented by adults.

The possibility of different wintering areas of age and sex classes raises some interesting ecological questions. As you recall, Dr. Pitelka long ago advanced the view that the early departure of adult sandpipers from arctic breeding areas could be a mechanism for insuring a more abundant food supply for the chicks and remaining adults. Similarly it has also been suggested that it would be ecologically advantageous for a species to split wintering areas, with males in one area and females in another. That idea, however, is fallacious, because in species in which there is pronounced sexual size dimorphism (as in most shorebirds), the sexual segregation would only increase the frequency of similar morphs in one area, and the expected result would be to *increase* intraspecific competition. It is fun to speculate on ecological matters, but we might be better off to gather some solid distributional data first. Sometimes the world is not quite as we would like to design it.

I have been impressed by new techniques discussed today. For example, the ability to analyze castings of oystercatchers and other species means that in some cases we do not have to collect specimens to study feeding habits. And furthermore, we may now be able to study the daily or seasonal changes in foraging patterns of individuals. This technique requires a lot of hard work, and it should be encouraged.

In order to have a better understanding of shorebird movements, we need more extensive banding studies. As an example, studies in Europe have indicated that the eastern population of Curlew Sandpipers migrates to northern Africa, and molts there before continuing to wintering grounds in south Africa, whereas the Asian population completes its migration to Australia before molting. Without information on populational differences in behavior, we might arrive at erroneous conclusions about the importance of staging areas, feeding grounds, or molting localities that would defeat the purpose of any management/conservation plans.

But in some cases, as Semipalmated Sandpipers, we find there is sufficient morphological variation to distinguish local populations. Using mathematical techniques developed over the past decade, it would be possible to refine our knowledge of geographic variation in many species, and thereby study migration patterns faster, easier, and cheaper than can be done by setting up nets, ringing birds, and hoping that some will be recovered somewhere—eventually. I think such morphological studies are of great potential importance, but they are unlikely to be funded at present. Besides, they are mostly unfeasible, because they rely on series of specimens of breeding birds from several areas within a species'

range. In most cases, such series are unavailable for given species even for one area, even for Barrow, where taxonomically minded California ornithologists have been studying for many years. Current collections, amassed over many years, often with no special purpose in mind, and often by persons of varying competence, are simply not adequate for these kinds of modern biological studies. Collecting the needed material would have no effect on populations, and as biologists, we should encourage (or at least not impede) such programs.

The other problem with using museum collections is that it is no easier to pull together information from collections today than it was in 1910, when Wells W. Cooke wrote his treatise on shorebird migration. We need a national retrieval system for collections so that we can take inventory of our needs and move ahead.

Finally, the most obvious overtone that has permeated the meeting so far is that everything we do will have to be evaluated in terms of "management." We are continually asked to plan on a sustained yield basis, which we seem to accept when it comes to cattle or tuna, but not to birds or marine mammals. And we are increasingly being asked to compromise—to evaluate the effects of an action and to be prepared for trade-offs.

So perhaps our greatest immediate need is not more data but what Daniel Kozlovsky has called "an evolutionary and ecological ethic"—a philosophy that gives us some guidelines in our relationship to the environment, and one that may help us live with the hard decisions that will have to be made in the near future.