A Response to Tove *et al.* (1998), Marine Birds off the Coast of North Carolina: A Critique

David S. Lee
North Carolina State Museum of Natural Sciences
P.O. Box 29555
Raleigh, NC 27626

In my paper reviewing the marine birds off the North Carolina Coast (Chat 59: 113-171), I summarized what I had learned between 1975 and 1989 concerning the temporal and ecological distributions of local seabirds. The majority of the information presented was from personal research, but I did place this into the context of what had been reported by others, and I provided an overview of the marine avifauna of the state. The manuscript was originally written at the request of the U.S. Fish and Wildlife Service. Its personnel reviewed the entire paper, it was then sent out for peer review, and the USFWS encouraged publication and distribution of the paper. Prior to publication in *The Chat* comments of all reviewers were addressed. In this issue of *The Chat*, Tove *et al.* (1998; 62:49) have critiqued this paper. I appreciate the time and effort they have put into their review. They provide a considerable amount of additional information on the state's seabirds, including important observations made subsequent to my publication. There are, however, a number of points I need to address, and what follows is my response to their critique.

Tove *et al.* (1998) find considerable fault with my 1995 publication, specifically identifying six broad areas of concern. I will first address these broader issues and then review the specific points they present.

1) An inadequate job reviewing the literature. I did an extensive literature review as indicated by 180 references listed in the "Literature Cited" section of my 1995 paper, but I did not intend this to be a complete bibliography of North Carolina seabird literature. The "Literature Cited" section of the Tove *et al.* critique includes four references not available when I wrote the manuscript, and I see only one short note (Ball 1948) pertinent to North Carolina seabirds which I did not include. I am unaware of any "important papers" which were omitted. Several reports (*e.g.*, Am. Birds and 'Briefs' in *The Chat*) that should have been included were inadvertently overlooked. For several of the recent citations that I did include, I unintentionally failed to incorporate all of the information into the text of my accounts. Some of the information presented in the critique has never appeared in print, and I was not aware of it. By my count, from what is addressed in the critique, it appears

that in compiling literally thousands of bird reports, 17 mildly-interesting to important single species/date observations were not included.

2) Disregard of decisions of the Carolina Bird Club's North Carolina Bird Records Committee (NCBRC). There appears to be a misunderstanding about the purpose and scope of my paper. The paper was not written as a submission to the NCBRC as information for the committee to review and accept or reject. The critique says that I entirely disregarded their work. I have simply approached the issues of reports and records from a different perspective. I provided information on the lack of documentation on the species level in three different sections of the text as well as under each species where this is an issue. On page 116 I discuss the seven seabird species that have not been confirmed with specimens or photographs for North Carolina and also included what is known about the occurrence of these species elsewhere in the region. I deliberately did not restate the decisions of the NCBRC, and for all of these species I made it clear that these birds were also not on the N.C. State Museum's state bird list. In nearly all respects my 1995 paper independently supports the findings of the CBC records committee. Collectively these unconfirmed reports differ in only a few minor points from the decisions of the CBC records committee, and mostly these differences are in terminology. Apparently the issue is that the authors of the critique think these birds should not have been mentioned at all. I totally disagree with this philosophy. Rejection by a committee of bird sight records does not automatically indicate the report in question is erroneous. A policy forbidding publication of unaccepted sightings would mean that potentially valuable information would forever be lost (*e.g.*, see my discussion on this under "Bermuda Petrel" in this response). It was entirely outside the scope of my paper to accept, reject or address the decisions of the records committee. Note that I did not refer to other states' records committees decisions for other southeastern states when citing reports from outside North Carolina. 'Official" information on the conservation status which was developed by another North Carolina committee was also not included. As with determinations of the CBC records committee, the conservation information was not included because it too had little to do with the scope of the paper.

The role of the CBC committee is to review reports of bird sightings, not to filter them from publication. In fact, as an aside, I find it intriguing that the CBC records committee can take reports, which by their own standards are judged inadequate for species identification, and then use this same information to identify a bird which they have not personally seen. This is done at least three times in their critique.

In several places throughout Tove *et al.* (1998) the authors criticize that I did not provide detailed identification accounts of the birds in question. No descriptions of any birds were included in this manuscript, and descriptions were never intended as part of the format. In all cases supporting information and the response of the committee has already been published in *The Chat* or elsewhere.

3) Accounts and conclusions inconsistent with theirs. Actually my paper is, in part, an overview of one of only a few North Atlantic sea bird studies where the methodologies can be reproduced by other researchers. Because of the dynamic nature of the region, it is doubtful that the data could be duplicated. The point of my long range and ongoing study is to provide a baseline with methods that can be reproduced. (Also see my comments in this response under "Black-capped Petrel.") I have published reports of individual species or unusual seasonal occurrences of species which have not been substantiated by the field efforts of others in North Carolina as have others. Bird literature, because of the acceptance of reports not supported with specimens, and the nature of birds themselves, does often contain unsubstantiated reports. In this case the authors are referring specifically to two or three species, and I fail to see why this needed to be repeated in the critique since the CBC committee has already detailed their concerns regarding these reports.

4) Standards. My points were misrepresented. I was not "complaining" as they suggest, but stating the fact that without published details it is difficult to know if a particular identification was correct. My specific statements are clearly aimed at reports of tropicbirds, skuas and jaegers prior to the late 1970's. At that time it was not known how many species of these seabirds actually lived in the Western North Atlantic. Long-tailed Jaegers were considered extremely rare, and only one species of tropicbird and and one species of skua were named in earlier editions of Eastern North American field guides. In the cases mentioned, my concerns were in the documentation of early and late dates of occurrence. For jaegers, where identifications remain to some degree problematic, I deliberately chose to be conservative and largely relied on specimen records. For tropicbirds and skuas I used only reports made after bird watchers were aware of more than a single species occurring in our area.

5) Inconsistent use of terms. Contrary to the critique's criticism, as best I can determine my terms of abundance are consistent within orders of magnitude. I had no intention to fine-tune beyond this point. Relative abundance for all species was presented on a month-by-month basis in several

of my previous publications (Lee 1986a, 1991, Lee and Socci 1989; using bar graphs and pie charts). These graphics were updated for this publication but at the last minute were omitted by the editor because of space limitations. Had they been included my terminology would not be a problem. (Actually a fair amount of the text and a number of the figures were omitted, and all figure captions were shortened because of space constraints; all editorial changes were done with my knowledge.) I believe that the variety of terms used allowed for wording that best described the status of a particular species (*i.e.*, my use of "irregular visitor" for Sooty Tern relays more information than the suggested "uncommon to fairly common"). Furthermore, the entire text when taken in context gives a good indication of relative abundance. I find it ironic that the Tove *et al.* critique has used my graphics to determine, or justify, terms of relative abundance believed to be more appropriate. The authors of the critique apparently didn't notice that the scales of these graphics were different from one species to another. The scales alone give a rather good indication of relative abundance. Many of the statements in the critique regarding relative abundance terminology are raw nonsense. Under Greater Shearwater I state that this bird is "common to abundant". The critique's authors suggest that "fairly common, to occasionally common" is more appropriate. At times counts of many hundreds have been recorded from single trips, and there are trips where over 1,000 individuals have been tallied. I consider this to be "abundant" by anyone's standards.

Regarding terminology, I consider a "sighting" to be a report (uncapitalized) and "record" as an identification confirmed with a specimen or photograph (in reviewing my paper I see several instances where I mixed the terms). This use is standard in most ornithological literature and differs from the definitions imposed by the critique. Appendix A of my paper describes the criteria for species inclusion as records and reports, and I fail to see even the perception of a problem here.

6) Misleading statistical methods. There is no statistical analysis in this paper: it is descriptive and contains only bar graphs. These graphs were for species for which there were enough sightings to show patterns from my offshore data and, *when combined with the text*, they illustrate patterns of occurrence and the variation from trip-to-trip, month-to- month, and year-to-year (see Northern Fulmar). The fact that specific portions of a given month are biased by small sample sizes is to be expected and is easily understood. In fact illustrating the potential for fluctuation is one of the strengths of my bar graphs, and collectively they show the dynamic nature of the local marine environment. This is not "stastistical misrepresentation." It is simply how my

data appears. When specific graphs are compared to the information provided in Table 1, it is difficult to imagine that anyone would be unable to interpret them. Unfortunately sample size and other information was cut from the figure captions in order to save space, but the loss of this information does not render the figures useless.

Items not mentioned by Tove *et al.* (1998) are the context of my statements and the timing of my publication. Many of the concerns addressed are taken out of the context of either the paper as a whole or the context of individual species accounts. On pages 117 and 118 I clearly spell out that the manuscript was written prior to mid-1989 and that no reports published after September 1995 are included. I pointed out that in my attempt to bring the text up-to-date unfortunately some relevant information would likely be overlooked. It is true some information was unintentionally omitted. More to the point, several of the same authors of Tove *et al.* (1998) formally reviewed a 1987-8 draft of this publication. If they were concerned about omissions, 1988 would have been the appropriate and constructive time to bring these problems to light.

For the few readers actually concerned with discrepancies, perceived discrepancies, differences of opinion, and my "major" errors, I encourage them to read the above comments followed by a careful in-context reading of the portions of my 1995 paper that are in question. This will clarify most points addressed by Tove *et al.* (1998).

I will now briefly discuss a few issues which are not generally covered in the above statements.

The comments which follow are based on statements from the critique. For the clarifications I make here it may be necessary to first read the corresponding statement in the critique. I have not addressed these statements point-by-point in that most of the issues have been covered above. I have chosen this format and not repeated literature citations to conserve space.

Yellow-nosed Albatross

Actually their statement that no other reports have been received is false. Another report of a Yellow-nosed Albatross is now available for North Carolina. I talked to one of the authors of the Tove critique several months before the critique manuscript was revised and resubmitted to *The Chat* and learned that he had studied this additional report. This is one of a number of instances where the authors have misrepresented facts in an attempt to make their points.

Black-browed Albatross

This report has not been accepted by the museum's state records committee, but it has been accepted by the CBC records committee.

Northern Fulmar

The statement "often common" and the actual mean number of birds per trip is exactly what I intended to show. This is not a discrepancy.

Cape Petrel

The critique's wording concerning my use of literature is deliberately deceptive, and they misrepresent what I wrote. I cite two early Twentieth Century publications that give important additional information on the specimen collected in Maine. I did not ignore the A.O.U.'s Checklist Committee's opinion as the critique says. The A.O.U.'s opinion is clearly stated on line four under "status."

Black-capped Petrel

I did inadvertently overlook three reports for high counts of these birds. The words "documentation," "verification," and similar terms used elsewhere in my 1995 paper refer to a detailed published account, a specimen, or a photograph. Thus, while Black-capped Petrel sightings were not documented with a specimen, or in this case even written up for publication prior to my surveys, they had been reported before 1976. (I was personally involved in several of these pre-1976 sightings.) Identical interpretive problems were presented for the White-faced Storm-petrel. Differences in the total numbers of Black-capped Petrels observed in my studies and from the critique's authors during their bird watching trips are expected. In my surveys I attempted to survey as many offshore zones as possible. I was not just interested in where birds were but also where they were not. On commercial bird watching trips, trip leaders obviously try to give participants the maximum amount of exposure time to rare species, thereby getting higher counts of select species. In order for me to determine relative seasonal densities it was necessary to survey wide cross-sections of various pelagic habitats. Tove *et al.* have failed to understand this. On page 116 I discussed problems with attempts to directly compare different data sets.

Bermuda Petrel

Here Tove *et al.* (1998) refer to information that was available after I wrote the manuscript, and in this case, well after publication of my paper. It is informative to note, however, that the authors of the Wingate *et al.* (1998) paper included my earlier reports in their discussion of the bird's local occurrence. If all reports not acceptable to the CBC records committee were censored from the literature, as they indicate should be the case, then these particular ones should not be available for use. As an aside, the seemingly regular occurrence of the Bermuda Petrel on North Carolina's Outer Continental Shelf, because it is a Federally-listed endangered species, will play a key role in developing conservation priorities as they relate to offshore oil exploration in North Carolina. From a conservation perspective, collectively all these records and reports are important, and it is vital that they were published.

Soft-plumaged Petrel

Contrary to the Tove *et al.* (1998) statement, my reference to this bird as the "*mollis*" complex does not in itself restrict discussion to the southern hemisphere species. For anyone vaguely familiar with problematic taxonomic groups, this notation is straightforward. My "refusal to speculate" on the identification of a cryptic species is not an "error," nor was it "misleading." It was deliberate and conservative, and if one reads my paper the reasons for my approach are clear. For the record, contrary to the critique, Soft-plumaged Petrels have been reported and technically documented for North Carolina (photo and statement in Birding World 5, Haney *et al.* 1993), but these identifications are not necessarily correct.

The critique deliberately overlooks the fact that references I included support my statement that these birds do not lend themselves to at-sea identifications. These references describe birds that were actually in the hands of European experts that were familiar with the species complex, yet they were unable to determine what they were. These references were published in the peer-reviewed academic literature, not in bird-watching magazines. In my opinion the former have higher scientific credibility. In fact, in the original elevation of the North Atlantic populations to full species, Bourne (1983) makes the point that his classification was not based on plumage or appearance.

My statement on collecting (pg. 124-125) will be understood by anyone who reads it. I find the concern of the critique perplexing in that one of the authors of Tove *et al.* wanted to collect specimens of these rare petrels, and his letter-writing to overseas authorities nearly started an international incident (USNM file 1997 letter to U.S. State Department from the American Ambassador in Lisbon concerning the collecting of Fea's Petrel in US waters, and a letter from W.R.P. Bourne in 1993 discussing how inappropriate it would be to collect these birds in US waters.)

Herald Petrel

I was unaware of published multiple reports which became available after I re-edited the manuscript. Perhaps the statement should be corrected to, "Compared to most other tropical species, these birds are usually seen alone." (The way the reports were published in American Birds and *The Chat*'s Briefs for the Files, however, and from what is provided in the critique, it is not possible to tell if sightings are of single individuals seen on the same date or of two or more birds seen together. Reading several of the regional reports listed in the critique sheds no light on this question.) It is my experience that this species does not flock, and there is nothing in the literature to suggest that they do.

Greater Shearwater

My seasonal distribution figures are repeatedly being misrepresented. I am not showing presence or peaks. I am showing averages.

Bulwer's Petrel

My 1979 observation was included for completeness in that it had been published by Haney and Wainright (1985) and Hass (in press). Whether or not this report had been written-up with details or reviewed by a records committee is not relevant. Contrary to Tove *et al.*, it had already been "introduced" by these two papers.

Sooty Shearwaters

Several important reports were unintentionally overlooked.

Little Shearwater

My saying the species has not been confirmed (specimen) and listing sight reports is not contradictory. Furthermore, I am not, as the critique implies, treating individual records as "factual" or "incorrect". As with other species all published reports were simply compiled.

Audubon's Shearwater

Tove *et al.* are correct. I even have specimens from this period, and I am not clear as to how they were missed. The seasonal distributions were correctly stated in the text, but not under spring and winter date records.

Wilson's Storm-petrel

The species is "most common in the 40-800 fathom area" (this is actually a rather narrow zone because of the contours of the Outer Continental Shelf), but readers wanting more specific information on zones of occurrence should refer to Table 2 (pg. 159).

White-tailed Tropicbird

Tove *et al.* are correct. My maximum count was in error as published.

Red-billed Tropicbird

I fail to see how stating that both species are "uncommon visitors" equates to an "equal abundance hypothesis." I did not intend to imply that this tropicbird is as equally common as the White-tailed Tropicbird, and I certainly did not say this. I have published two papers and have several others in manuscript concerning the status of tropicbirds in the Western North Atlantic, all of which say otherwise. I think I clearly made the point that the reports of White-tailed Tropicbirds in question were the ones made prior to the discovery of the Red-billed Tropicbird off eastern North America.

Masked Booby

This species is largely found over Gulf Stream waters (which incidentally in our region flows over areas where the ocean is deep) where it is associated with drifting *Sargassum* mats. This has been confirmed by others as well (*i.e.*, Haney 1986).

Brown Booby

The December report should not have been included.

Red Phalarope

If the scale on the Red Phalarope graph is examined (as well as the scales on many of the other figures criticized for not providing adequate information), the reasons for no phalaropes being shown for October or November can be understood. The graphs do not show "records," as the critique keeps trying to imply. They show *averages*. This is explained in the graphics, and additional information is available in the text.

Pomarine and Parasitic Jaegers

If one has read my previous papers, I have provided documentation for these jaegers. In fact in 90% of the cases the ones I refer to are collected specimens. I did not say I expect all jaeger sightings to be accompanied by supporting descriptions. I did choose, however, not to use any of these reports to define seasonal occurrence because there was no way to retroactively evaluate specific reports.

California Gull

See my account of Black Guillemot.

Thayer's Gull

The papers I cited on page 148 of Lee 1995 address the taxonomy of this gull. The CBC records committee is clearly aware of the taxonomic problems concerning Thayer's Gull. The CBC committee even notes, "The Committee is not addressing the validity of the species" (Chat 54:56).

Ivory Gull

I fail to understand the concern in that it was spelled-out in my paper that the bird in question is unconfirmed. The points of Tove's (1989) paper were not addressed in my paper because as stated above my 1995 paper does not discuss identification issues. The criteria for inclusion of this report in my 1995 paper is no different than that of any other published one referred to in my paper. The information I provided to Tove and Fussell was consistent with what I published. It is interesting to learn 18 years later via this critique that an albino Bonaparte's Gull was seen a few days before. If one reads my original description of the bird in question, however, it is clear from size alone that it was not a Bonaparte's Gull.

Arctic, Bridled Terns and Dovekie

Several key reports were unintentionally omitted from each of these accounts.

Razorbill

The authors of the critique provide a wealth of information that I had missed.

Black Guillemot

The inclusion of California Gull and Black Guillemot would have been useful additions to my 1995 paper, but these reports were never formally published (gull) or they were reported without any accompanying details (guillemot). I chose not to include them because they represent birds new to the state, and as interesting additions to the overall knowledge of the fauna of the region, they deserve attention. It is well-established protocol, however, that persons making the first observations of a species new to a state are the ones expected to present them in publication.

Discussion

Contrary to the statements concerning acknowledgments by Tove *et al.* (1998), my acknowledgments are on page 161. The information on page 155 refers to other seabird studies conducted in other southeastern states. It is not an acknowledgment. In my acknowledgments section (page 161), I simply thank the people who directly helped with this particular manuscript. But, for the record, Paul DuMont and Robert Ake were fully acknowledged in my earlier publications. The doctoral research of Todd Hass (which the museum did help support) was not focused at the time I wrote the manuscript, and I did not include, except where cited, any of his results. Hass's data were not analyzed until long after my 1995 publication appeared, and to date they have not been published. Contrary to the statements of Tove *et al.* (1998), in my section on "Recent Studies and Studies in Progress," I do not cite only my own works, although surely there were a number of other on-going studies I was not aware of.

Inshore waters do overlap with shelf-edge waters as a result of undulations of the bottom contours, storm climate and currents along the eastern limits of North Carolina's Outer Continental Shelf. These are established defined terms that are not unique to my publication. The fact that I did not list any birds between 100 and 300 fathoms is simply an artifact of my having no species that characterize this portion of the ocean which are not regularly shared by those of adjacent areas. This is obvious from reading the text.

I should have said "few" birds inhabit inshore waters, but this is internally clarified in my publication for any readers taking the time to refer to Table 2 (pg. 159), and this information is consistent with what the critique proposes.

Finally, I must comment on the distracting overall message of this critique and its last paragraph. According to Tove *et al.*, authors of manuscripts must have the approval of the people who wrote this critique and the members of the NCBR (there is a considerable overlap of personnel) concerning which birds they write about and which ones they are allowed to leave out. Authors do not have authority to choose methods for presenting their own data, or if we do, this data cannot be at odds with the unpublished, preconceived opinions of the critiques' authors. We are told who to acknowledge. Granting agencies and other contributors do not have influence in the final products they sponsor, and editors and reviewers no longer have the final say on what is worthy for publication in *The Chat.*

Terms such as "seriousness of error," "most glaring," "serious problems," "broad inaccuracies," and "inadequacy of literature review," are all matters of perception, and to me this wording seems extreme. Yes, while it is clear that my paper would have benefitted by my attention to a number of the concerns Tove *et al.* (1998) present, it is not their prerogative to judge this paper by standards I never intended. Contrary to statements in the critique, published reviews of existing information are not precedent-setting in ornithological literature. Nonetheless, the statement that this paper lacks new data is untrue as can be ascertained by even casual reading. The 1986 paper they cite was only four pages in length. The paper they critique was 58 pages.

I found it difficult to respond to this critique in that it is such a odd mixing of both constructive and destructive criticism. I believe much of the text of the critique is inappropriate. Philosophical differences, misrepresentation, distortions, overstatements of minor details, and instances of unnecessarily venomous verbiage combine to distract from their otherwise useful contribution.