The Sixth Edition of the "Check-list of North American birds," like the Fifth Edition before it, represents a massive amount of work and will probably become a standard authority and reference work. In addition to the area covered by earlier check-lists, this edition contains information on ranges, taxonomic levels, and relationships of Central American birds, including those of the Bahamas, the Greater, and most of the Lesser Antilles. The inclusion of these areas represents a major addition for which the Check-list Committee deserves commendation.

Because the "Check-list" is the work of a committee, it tends to be somewhat conservative. Not only are published decisions based on a majority vote of Committee members whose taxonomic philosophies vary greatly, but also the results of new research require some time and discussion before they become acceptable to the Committee as a whole. Consequently, we might expect a volume produced under these conditions to be solid and devoid of idiosyncrasies, but also not sparking with innovation. The "Check-list" generally lives up to these expectations. Some aspects of the work seem definitive, but in other cases the character of the "Check-list" has become stagnant. In the following discussion, we will focus our comments on the underlying logic, that is, on the "deep structure" of the "Check-list." This approach is adopted because we believe the nature of its assumptions and reasoning will be the ultimate determinant of the volume's stature as a scientific work and that, consequently, this aspect deserves as much, if not more, attention than do the details of the organization and species accounts.

Subspecies.—Subspecies are not listed in this edition. The Committee notes in the Preface that this is a matter of necessity, not choice—adequate time was not available to analyze Central American forms in terms of trinomials. This decision marks a significant departure from the format of past check-lists, but one not without salutary effects. For example, many of the previously listed subspecific taxa were arbitrary units, at best statistically distinguishable only when using large samples from breeding populations; others were arbitrary chunks of geographically continuously varying phenotypes. Thus, trinomial designations were biologically misleading in many cases. Nevertheless, because of lumpings at the species level associated with the interpretation of avian taxa in terms of the biological species concept, some taxa with unique evolutionary histories and allopatric ranges have now disappeared as discrete entities from the "Check-list." Thus, although some well-marked "subspecies" of such birds as the Dark-eyed Junco and Seaside Sparrow are mentioned in the species accounts, many others, such as those of the Horned Lark and Fox and Sharp-tailed sparrows, are not.

Species.—At the species level, the "Check-list" consists of a sequence of species accounts that include
information on the original description, a brief description of habitat, a fairly detailed statement of range, including breeding, wintering, migration, and accidental occurrences, and some notes on taxonomy, including vernacular names, synonyms, and superspecies status.

As is well known by now, numerous taxa of North American birds, listed as species in the 1957 (fifth) edition, are not given specific status in this volume. These changes were first published as supplements to the 1973 and 1982 volumes of The Auk. Unfortunately, the Committee did not publish its reasoning in making the more recent of these decisions for the individual cases. (In the 1973 and 1976 supplements, some citations were given.) In the Preface, however, they do indicate their belief that species are real, fundamental biological entities, and that they follow the biological species concept of Mayr in making decisions about species status. They point out (correctly, we believe) that the major interpretational problems with these decisions concern situations in which there are: (1) limited hybridization of formerly allopatric populations now in contact, or (2) completely isolated allopatric populations that are weakly differentiated. These are problems, because, in cases of hybridization, ornithologists (e.g. Short 1969) distinguish between specific and subspecific status on a qualitative basis using such indicators as extent of hybridization, width of hybrid zones, extent of assortative mating, existence of presumed isolating mechanisms, and presumed magnitude of gene flow. The little quantitative modeling that has been done on these phenomena makes us less than sanguine about the efficacy of such judgments; almost any detectable amount of gene flow will eventually result in complete mixing of gene pools in the absence of selection and environmental change, but even large amounts of interchange may take thousands of years to produce a noticeable effect of introgression over a substantial fraction of a "species" range. Hence, interpretations of species status based on qualitative estimates of gene flow at least require documentation of assumptions and logic.

Under the species concept used here, specific status of isolates is conferred depending upon the extent of phenotypic, vocal, and behavioral differences between the allopatric populations, relative to the extent of differences generally found among "good species" in the same genus and family. This is species status by innuendo. The Check-list Committee notes that there was internal disagreement in some of the cases, and alternative views of species status are briefly mentioned for especially difficult situations. The reasoning underlying even these contested cases is not described, however, and references are generally not provided. This is unfortunate, because species definitions are entering a new period of intense exploration. In particular, the biological species concept has recently been criticized from phylogenetic, population genetical, and ontological points of view. These problems, briefly summarized, concern the fact that, for the very situations that do involve interpretational difficulties associated with hybridization and differentiated isolates, the biological species concept, as it is usually applied, results in species taxa that lack status as evolutionary units, potentially comprise assemblages of non-monophyletic taxa, are hundreds of thousands of years removed from future equilibrium of gene pools (if environments do not change in the meantime), and are classes rather than individuals (in the philosophical sense) and hence cannot be the proper units of evolutionary theories or historical explanations. Many of the cases treated in the "Check-list" will figure prominently in future discussions of these issues. Furthermore, with the omission of subspecies, as described above, some of the actual evolutionary units of North American birds are now missing from the "Check-list," reducing its value to systematists and others interested in comparative evolutionary studies.

Superspecies.—The "superspecies" concept has been widely accepted within systematic ornithology, and the tradition is maintained in the Sixth Edition of the "Check-list." Undoubtedly, this acceptance has occurred because the ostensible purpose of superspecies—to facilitate biogeographic and evolutionary analysis—is so eminently desirable to all systematists. Yet, as generally understood and applied, the concept has several serious difficulties, and it is surprising that these have not been discussed within the ornithological literature. Because those problems have important consequences for interpreting the scientific content of the "Check-list," we wish to note some of them here.

The difficulties of the superspecies concept are direct manifestations of the definitions typically used by systematists; within ornithology, two are generally used:

1. A superspecies is "a monophyletic group of entirely or essentially allopatric species that are too distinct to be included in a single species" (Mayr 1963).
2. A superspecies is "a group of entirely or essentially allopatric taxa that were once races of a single species but which now have achieved specific status" (Amadon 1966).

Three elements are included in these definitions: a statement about relationships, one about distribution patterns, and another about the degree of phenotypic differentiation. These elements are not necessarily concordant, however, and this ensures that the superspecies concept can be applied only arbitrarily. Some potential problems for works such as the "Check-list" include:

1. If a differentiated taxon is broadly sympatric with one or more of its close relatives, that form might be excluded from a superspecies created for those taxa. In a check-list, this would disguise information about phylogenetic and biogeographic pattern.
2. If differentiated, allopatric taxa are not judged to be sufficiently distinct from one another, they might be united as subspecies in one species rather than being maintained as a species of a superspecies. Here the species and superspecies concepts suppress the fact that differentiation has occurred.

3. If a form exhibits a marked degree of differentiation relative to a group of its close relatives, the former taxon will likely be excluded from the superspecies erected for those forms that more closely resemble each other. Again, phylogenetic and biogeographic patterns are being obscured.

We agree with the Committee that it is useful to recognize subsets of closely related taxa within a genus, and indeed we would advocate additional hierarchical levels to accomplish this. But the superspecies concept adopted by the Committee, and ornithology in general, can easily obstruct biogeographic and evolutionary analysis rather than help it. An example is the one chosen by the Committee for discussion, the genus Sphyrapicus (pp. xiv-xv). In the “Check-list” proper, three species—varius, ruber, and thyroideus—are recognized. The Committee considers varius and ruber to constitute a superspecies. An important point, however, is that without the discussion in the Preface, the reader could not tell from the classification itself (pp. 387-389) that there are actually at least six differentiated taxa in this genus, two included in thyroideus and four (maybe five) different lineages within the varius-ruber complex (Johnson and Zink 1983). In this case, the species and superspecies concepts of the Committee yield a classification obscuring part of the actual complex biogeographic and evolutionary pattern (i.e. there are at least four evolutionary units that must be accounted for in any biogeographical or speculation analysis of the varius-ruber complex, not two; Johnson and Zink did realize this in their analyses).

Generic concepts and limits.—The Committee extended its application of the principles of evolutionary systematics to decisions at the generic level. A modification of Mayr’s (1969) definition was adopted: “a group of species of common phylogenetic origin that are more closely related to one another than to any others and that differ from others by a decided gap” (pp. xv). But the Committee went further and proposed a criterion of its own (p. xv):

“We have sought particularly to recognize as genera those species or groups of species that have reached different adaptive plateaus with the potential for further diversification in other evolutionary directions. We have adopted a middle course, avoiding recognition of monotypic genera that do not appear to meet this criterion but also avoiding submergence of adaptively distinct forms into large genera, thus obscuring their distinctiveness.”

Given this philosophy, it is not surprising that the Committee often found the application of this generic concept to be “arbitrary,” “subjective,” and “inherently difficult,” thus forcing them to exercise “practical judgment.” Indeed, their definition unites two criteria that, within evolutionary systematics at least, are frequently antithetical to one another. On the one hand, the definition invokes a phylogenetic criterion, but on the other espouses a criterion based on perceived degrees of difference. For a taxonomist with a phylogenetic perspective (i.e. one who maintains strict monophyly), the two criteria can be applied in a way that minimizes subjectively defined genera. But, from an evolutionary systematic perspective, subjectivity is increased because the two criteria are not always used as compliments but sometimes as antagonists when the degree of difference criterion supersedes that of phylogenetic relationships (this point has been discussed repeatedly in the technical literature). That this is what the Committee had in mind is demonstrated by their decision to delimit genera on the basis of whether they had attained an “adaptive plateau [not defined] with the potential for further diversification in other evolutionary directions.” Although we have not had firsthand experience with the prescient qualities of the members of the Committee (and both of us count them all as our good friends), they should have foreseen a logical difficulty with this criterion: because all species must be placed in a genus, and because these genera are said to be delimited on their future adaptive potential, the implication is that all the genera recognized by the Committee have the potential for future diversification, including presumably Cochlearius, Gymnogyps, Pandion, Eurypygus, and Steatornis, among many others. But what does this all mean, and what is its scientific justification? What is the scientific rationale for basing a classification on what might happen (as the Committee also did in making decisions at the species level; see above) rather than on best hypotheses about what has happened? In fact, of course, the Committee did not sit around a table prognosticating on the future potential of avian genera. Instead, they generally maintained the status quo or lumped (examples abound: Phalaropus, Calidris, Sterna, Columbina, Athene, Melanerpes, Picoides, and so on).

In the above examples, as elsewhere, justification or documentation often is not given for taxonomic decisions, and instead the reader is referred to “some authors,” “recent studies,” or similar statements. We recognize the enormity of the taxonomic issues confronted by the Committee, and we sympathize with the problems and practical decisions they repeatedly faced, yet in a scientific treatise that is supposed to be the standard reference volume for years to come, we deplore the absence of documentation that peppers this volume.
Higher taxa.—The Sixth "Check-list" generally follows the Fifth Edition with respect to the recognition of higher taxa (families, orders) and their sequence. The sole newly created order is the Phoenicopeteriformes (flamingos), and two new additions are the Tinamiformes and Sphenisciformes, both the result of an expanded geographic coverage. A total of 23 orders (not 18 as misprinted in the Contents) and 93 families are recognized in the Sixth Edition, compared to the Fifth Edition figures of 20 and 85, respectively. This comparison is misleading, however, for although the Sixth Edition now includes many more families because of the addition of Middle American taxa, many families of the Fifth Edition have been reduced to subfamilies in the present volume. Inasmuch as this has increased the hierarchical structure, and thus the information content of the classification, we see this change as an advance. Again, however, the same persistent problem plagues these alterations: virtually no documentation is provided, and inconsistencies can be found. We note here a couple of examples.

In the Preface (pp. xviii-xix), the Committee lists 10 major changes, including the reallocation of genera from one higher taxon to another and the lowering of family-rank taxa to subfamilies of greatly expanded families. For example, genera such as Aitila, Rhytipterna, and Tityra, among others, are removed from the Cotingidae and placed in the Tyrannidae, and in another instance, Donacobius is no longer referred to the Mimidae but to the Troglydytidae. Previously recognized families such as the Parulidae, Thraupidae, and Icteridae are now some of the subfamilies included in the Emberizidae. Sylviidae, muscicapids, turdids, and timaliids are reclassified as subfamilies of the large family Muscicapidae.

Naturally, we accept rearrangements in principle, because such changes should represent more precise statements about phylogenetic relationships. In the case of the "Check-list," however, documentation for such changes is usually lacking. For example, we are unaware of any published evidence supporting the monophyly of the Emberizidae as accepted in the "Check-list." The only presently available phylogenetic analysis of the nine-primaried oscines (Raikow 1978) postulates a very different set of relationships than found in the "Check-list" (e.g. icterines, emberizines, and cardinalines are more closely related to fringillids than to thraupines or parulines). Perhaps the Committee followed the unpublished results of DNA hybridization analysis, which appear to support more closely the monophyly of the Emberizidae sensu the Committee (Sibley and Ahlquist in press): one cannot know: the reasoning is not documented.

Sequencing.—We noted above the conflict between using phylogenetic relationships and degree of difference when establishing generic limits. The Committee compounded this confusion when they applied those same criteria to determining sequences. Although they state that the "Check-list" is supposed to represent our best estimate of phylogenetic relationships (e.g. p. xix), this can hardly be so because the Committee not only rejects that goal in principle but also in actual practice. Thus, they state (p. xvi) that "In the course of avian evolution there have been numerous and repeated branchings; even if these were all perfectly known, they could not be clearly represented by a linear sequence of names." This conclusion is, however, a myth: classificatory procedures designed to reflect a branching sequence precisely have been discussed repeatedly in the systematic literature, and, given that the Committee desires their classification to be as phylogenetic as possible, it is disappointing that this literature was not considered seriously. In fact, in their example of Sphyrapicus (p. xvi), they ignore the general procedures of phylogenetic classification by placing a more distantly related taxon after the closest sister-species, thus precisely reversing the sequence-subordination scheme devised to deal with this very problem.

This same general problem reoccurs at all levels of the "Check-list." It is not possible to infer the within-taxon phylogenetic relationships of subordinate taxa from the listed sequence. In any given case, the sequence may reflect phenetic similarity, phylogenetic relationship, or inferred phenotype of an unknown common ancestor (e.g. the case of Sphyrapicus, p. xvi).

It may seem churlish to dwell on a few perceived shortcomings given the obvious exemplary scholarship of much of the "Check-list." As we mentioned above, the inclusion of Central America and Caribbean forms represents a major advance, and the species accounts, including the habitat and range statements, will be an important source document for scientists, government officials, and birdwatchers. However, as a scientific document, the "Check-list" suffers from two major deficiencies. First, the sources, logic, and reasoning behind decisions are not documented—this is true of species status, of the level and membership of higher taxa, and of sequencing within taxa. This makes at least some of the results nonreproducible. Second, the concepts comprising the epistemological basis for the taxonomic levels (especially species) and their sequence result in a set of "species" and a sequence of taxa that obscures evolutionary units and phylogenetic relationships. Consequently, at the present time there is no summary document to which a researcher can turn in seeking a list of the units of North American birds with discrete evolutionary histories. Nor will the proposed Check-list Committee's volume on geographical variation (p. xiii) meet the need if it follows the subspecies philosophy of the fifth "Check-list." Taxa with their own unique evolutionary histories ("evolutionary species") will have to be carefully distinguished from the arbitrary populations of continuously vary-
The Sixth Edition of the American Ornithologists' Union's (1983) "Check-list of North American birds" features numerous changes from previous editions in this series, most notably in taxonomy. One aspect of the taxonomic approach has changed very little, however, having remained much the same since the publication of the First Edition of the check-list in 1886. As is traditional, the A.O.U. has published its views on taxonomy essentially as faits accomplis, with little or no effort being made to provide the rationale for them. This was a dubious practice even a century ago, and it certainly is one that needs to be corrected as we near the twenty-first century. The provision of such rationale is essential for the proper evaluation of A.O.U. taxonomy, and without it such views lack the credibility that they deserve.

I realize that there are arguments against the publication of the rationale for A.O.U. Check-list taxonomy. For example, the provision of such could require a significant amount of additional space, which would be an added expense. This argument seems especially telling for those cases in which the views are based on unpublished evidence, the presentation of which would indeed require more space. Another argument is that the rationale for these views is available from the Check-list Committee (Committee on Classification and Nomenclature), and it can be had on proper inquiry. One might also argue that the rationale is obvious enough to anyone who is properly versed in avian taxonomy, thereby making its provision unnecessary. Perhaps there is even some feeling that, given that the Check-list Committee is made up of some of our most respected taxonomists, their involvement alone is sufficient rationale for the views.

In many cases, the provision of the rationale for A.O.U. views on taxonomy should involve little more than the citation of relevant publications. As an example, the Sixth Edition of the check-list contains the statement (p. 149) that the sequence and placement of genera in the Rallidae largely follow Olson (1973). This approach could have been used throughout that check-list, but it was not. Instead, where any statement at all is made on taxonomic treatment, it is likely to be some nonexplanation (e.g. "considered a separate species by some authors" or "formerly placed in the genus . . . ."). Even where more definitive statements are made, there is often no specific citation of the source of that evidence—as in the reference to "recent studies of vocalizations" as a basis for splitting the Brown Creeper (Certhia americana) from its Old World relatives (p. 520). In some cases, of course, the provision of the necessary rationale would require extended discussion (as to sort out conflicting points of view or to review unpublished evidence); this would indeed require more space. In spite of such considerations, however, the reader's "need to know" should be reason enough for the A.O.U. to provide the rationale for its views. I would go so far as to suggest that, if means cannot be made available to publish the necessary rationale—either in the check-list or other appropriate outlets—then no attendant changes in taxonomy are warranted. In essence, if the evidence does not merit enough consideration to be published, then it does not warrant use in generating taxonomic views.

I believe that the A.O.U.'s provision of the rationale for its taxonomic views would have substantial benefits. First, it would place firmly on record the evidence that forms the bases for these views. Secondly, it would help elucidate the contexts in which the Check-list Committee generates these views. Thirdly, by providing such information, the A.O.U. would enhance the credibility of these views—which now may easily be mistaken for edict rather than the product of objective deliberations. As a side benefit,