LETTER TO THE EDITOR

When I received the original version of this letter in August 1973 I refused to publish it because I regarded it as not only abusive but probably libelous by American legal standards. I received this considerably toned down version on 4 November 1974 from President Farner, to whom Prof. Thorpe sent it instead of to me. A holograph postscript to Prof. Thorpe's letter of transmittal to Farner states, "I must make it clear that my letter to the Auk must be published complete and without alteration of any kind." I am running it on President Farner's considered advice, but as our approaching January deadline leaves too little time to have it set in type and the galleys approved by the author, I am breaking Auk style and precedent by printing it by photo-offset, with apologies to all concerned.—O.L.A., JR.

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The Editor,
The Auk,
Museum of Natural History,
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Washington, D.C. 20560.

Sir,

A reviewer is expected to express his own opinions, therefore one does not wish to question them however remarkable they may be. But when these opinions are coupled with alleged "factual statements" based on serious misrepresentation or misunderstanding of data together with a disregard of whole sections of the work, it is important that these be refuted. In order to establish the facts I would like to be allowed to deal with only a few of the many such errors in R.B.Payne's review of my Duetting and Antiphonal Song in Birds (1972), Behaviour Supplement XVIII, pp. 1-197 which appeared in Vol.90 of The Auk; issue for April 1973, pp. 451-3.

Your reviewer criticises us for devoting 22 pages of the monograph to patterns of shrike songs expressed in musical notation, but he ignores the fact that all of these songs are analysed in the preceding 10 pages of tables which give precise details of total durations, overall frequency ranges and much other information. A further 5 pages of tables give the exact frequencies of 406 sound units of Laniarius aethiopicus major. He writes: "The author should have stuck with audiospectrographs for documentary and comparative purposes" and he criticises a sound spectrogram as a result, presumably, of his not having troubled to read the legend. Yet he ignores the remaining 127 sound spectrograms which appear elsewhere in the work! His prejudice against the use of musical notation to illustrate the songs of some Laniarius spp. seems to arise from a complete misunderstanding both of the songs of these species and of the nature and function of musical notation. The general reader will find a discussion of these points in my Duet-Singing Birds, Scientific American: August 1973, pp. 70-79, but some points of special relevance must be dealt with here.

This method was chosen for those species who communicate by means of definitely pitched sound units (tones) and for whom the musical intervals between such units are, to the best of our knowledge, of identificatory significance. The sound spectrograph, even when used with the narrow bandpass filter, is unable to give exact information even to the nearest semitone, and many duets which appear to have identical pitch units on

a sound spectrogram, can be best and most economically distinguished by the use of musical notation. Moreover, by the use of conventional part-writing, the contributions by the different birds are immediately apparent and the sound spectrographic confusion of overlapping parts is avoided.

Payne claims that it is impossible to render the timing of notes in a precise manner with musical notation. This claim is false. It is a simple matter to select a musical symbol to represent a brief period of time, for example: one 1/64 note to equal 10 m/secs., and to relate all other symbols to it; the only requirement is that one should understand the arithmetic of musical notation. Such refinement was not used in the monograph because it was found that the birds themselves are not consistent to this degree; furthermore, the aim was to make the examples comprehensible to anyone who had attended music classes in school - not to bamboozle the reader with unnecessary intricacies. Payne goes on to state: " it is incomprehensible that such notation be expected to show adequately the fine structure of a song ..." (he means, of course, the fine structure of a song-unit). It was not intended to do so; where "fine structure was under discussion sound spectrograms were presented (pp. 75-79), but these rather notably fail to indicate the fine structure, especially after the reduction for publication. Oscillographic and computerised analyses are usually required for the examination of minutae. What your reviewer fails to realise is that there are 'macroscopic' and 'microscopic' aspects of any sounds; there are also sound units and sound forms, and the appropriate representation must be employed as best fits the matter under discussion. When one needs to describe the visual form of a bird - as it was desirable to portray the musical form of the duets - one does not use the language of cytology or molecular biology!

When your reviewer attempts to comment upon the chapter dealing with musical aspects of the vocalizations, he substitutes inaccurate for precise terminology; thus for "concordant notes" and "discordant notes" read "consonant intervals" and "dissonant intervals"; the section under review is concerned with the relationships between tones. The term "minor fifth" is meaningless and does not, of course, appear in the Monograph. The shrikes are not "said to duet usually with concordant notes"; it is demonstrated *a posteriori* that the pitch intervals comprising the duets are predominantly consonant. The authors do not suggest that "discordant notes would interfere and cancel each other"; the suggestion, as properly stated in the Monograph (P. 159), is that a duetting species communicating with tones that frequently coincide or overlap will tend to avoid dissonant intervals because the resultant beating will make recognition of the constituent tones as discrete pitches difficult and prone to error. "This idea ...." is not taken from my book Bird Song (1961) neither does it overlook "possible differences in the auditory perception ....." since it is clearly stated as a hypothetical proposition. The reference to "occidental man" implies that no comparison was made with the music of oriental man; the implication is false - see P. 153 of the Monograph.

Payne states that the biological significance of the variations in duet patterning is unknown (Auk, p. 452, paragraph 4). There are however strong grounds for assuming it to be concerned with individual recognition. We have shown conclusively (monograph pp. 53-75 and 82-83) from a study involving the analysis of 148 examples of duets of *L. aethiopicus major* as well as those of many other subspecies and species of the genus, that (pp. 60-68) the repertoire of duet patterns displayed by a given pair is likely to be consistently different from that displayed by a neighbouring pair, and that in the field it is possible to recognise a pair solely by its repertoire of duet patterns (pp. 66-68). In fact we have never encountered an example, among pairs whose duets we have fully analysed, of two repertoires which were in all respects identical. Payne seems unaware of the great amount of work which has established beyond doubt that the recognition of temporal and tonal individuality of vocal patterns is a basis for individual recognition of both partners and young. This appears to be true of many different types of birds from Auks, Gannets, Gulls and Terns on the one hand to the higher passerines on the other. For

references see appropriate chapters in Hinde, R. A. (Ed.) Non-Verbal Communication; (1972) Cambridge University Press, London and New York. This is not to say that these are the only parameters of the vocalizations used for individual recognition - we actually cite others (pp. 76-79); new parameters may in due course be discovered as in the case of the ability of the Gannet (Sula bassana)to respond to very rapid changes in amplitude, a feature not previously thought to be easily detectable by the avian ear.

As to the six lines of evidence referred to by the reviewer (Auk, p. 452, paragraph 2): (1) A change of pattern is often followed immediately by a change of pattern in the mate as we have shown in L. aethiopicus, L. funebris (pp. 124-129), Cossypha heuglini (pp. 164-168) and Cisticola nigriloris (pp. 177-186). (2) In a repetitious duet the timing is consistent from phrase to phrase as we have shown in several species of Laniarius, in Cossypha and in Cisticola nigriloris, C. hunteri and C. chubbi. (3) is misconceived for, except in so far as a duetting species is likely to have a more precisely developed time sense, there is no reason why the calling rhythm should be more regular in duetting birds than in related birds which do not duet; it is the response time that is of interest. (4) is also misconceived; there is no reason to think that cues other than auditory signals of the partner would not be used for the timing of the duets. A principle of 'Double Assurance' is very widely spread in the animal kingdom and it is probably rare to find that a higher vertebrate animal will rely solely on one parameter of a stimulus when in fact its sense organs are capable of detecting others as well. The reviewer is sceptical about the birds being unable to see each other in dense tropical vegetation and thinks that the vegetation cover should have been precisely measured; yet he ignores the sketch maps (pp. 63, 70, 71, 72 and 110) showing this. Of course there are occasions when one cannot be sure that the birds are invisible to each other but, given the dense vegetation occupied by some of the duetting Laniarius and Cisticola, one can often be quite sure that, when the birds are singing low down in the vegetation, they cannot possibly be in visual contact if they are more than about a meter apart. As to (5) "Playbacks of taped notes of one bird should elicit calling in the partner with the same temporal patterning as in the duetting pair", this is so, see pp. 102-106. Suggestion (6) appears ludicrous! This was a field and aviary study. No neurophysiologist in his senses would embark on a microelectrode study of birds so secretive, so difficult to obtain (and therefore so precious) whose brains are neurologically unmapped and, above all, species not previously successfully maintained in captivity as were the subjects of this section of the study.

It has never been argued that all examples of duetting occur amongst inhabitants of dense vegetation; but it is suggested that it is much more likely to have arisen in such areas. We have to account for the fact that in the nine or so families of birds in which high precision antiphonal song has been evolved, at least seven are primarily tropical and that precise duetting outside the tropics is relatively rare. Of course it was not suggested that the density of the vegetation was the only causal factor; but it does seem probable that the absence of regular seasonal cues for the onset of breeding, a characteristic of the tropics, with its concomitant tendency for birds to remain in or near the breeding territory for the greater part of the life, could well have a part in the story. (See Scientific American, August 1973, p. 73).

Payne complains of the absence of an experimental approach. The whole of one chapter (pp. 84-108) is devoted to the experimental approach as are parts of another chapter (pp. 117-120). He also refers to a paper by himself which, unfortunately, we did not receive in time for discussion in the monograph.

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