CORRESPONDENCE

Subspecific Values

Editor of 'The Auk':—

By general concept, the subspecies is a minor genetic group within the species. Theoretically, within a highly variable species, their number may be infinite, but practically the number to be recognized within a species is limited by the technique and ability of observation. Most of the broader and more easily recognized variants of North American birds have been described and named, and present systematists within the field have been reduced to concentration upon finer and finer distinctions and the exercise of nicer and sharper discriminations. The more any one studies a subject the more he can see in it. An ample series of material and long, intensive examination of it are almost certain to reveal refinements inappreciable to a cursory survey or to those who lack the opportunity or equipment for equal concentration upon the particular subject. To study all the species that may be concerned thus intensively is beyond the limits of most working life-times. It follows that the ordinary, or even the unusual, ornithologist, unable personally to test all dicta that may be advanced, must accept some of them with slight or no verification, placing strong reliance upon the known personal equation of the proposer and his habits of thought and work. It is not that these distinctions do not exist, but that some of them are too fine for ordinary resources, and may, in fact, in extreme cases, be recognizable only to highly gifted, specially developed senses, or in long series of pertinent material. The danger of thus having to accept authority without verification is obvious, and raises the question as to how finely it is *expedient* formally to split and to demand universal recognition therefor.

Every possible variant, no matter how slight, is legitimate field for investigation. In fact, slight superficial differences may be accompaniments of those of more fundamental but less obvious importance. Unfortunately we are quite unaware of the essence of specificity. The morphological guides upon which we base our diagnoses are largely adventitious accompaniments of specificity, not its basis. A Song Sparrow is a Song Sparrow, not because of certain spots, stripes, colors, size and proportions, but it bears these significant insignia because it is a Song Sparrow. It is quite possible for two birds to be so much alike in their gross morphology as to be inseparable to our senses, yet fundamentally be genetically, even specifically, distinct. Some of the *Empidonax* flycatchers approximate this condition and others, unknown to us, may carry it still farther. Two grains of wheat may be indistinguishable even to the expert, yet vary widely in their ripening period and resistance to drought, disease or insect attack. One strain of mosquito may be a dangerous vector of disease, and others indistinguishable from it may be harmless. Something similar may be true in some of our minor bird strains. That they are exclusively limited in their ecological, climatic and geographical reactions indicates that they contain certain inherent constitutional factors that render intolerable to one what may be tolerable or even necessary to the other.

All this may be philosophical justification for the finest splitting possible, but it does complicate our taxonomic system to a degree that greater features may be hidden amidst the lesser ones. One may not be able to see the forest for the trees. The trouble is that all such recognizable variants, the large and the small, the material and the immaterial, are represented in the system as of equal weight,—they are all subspecies without qualifying distinction. An example may be the

Fox Sparrow, *Passerella iliaca*. There are three well defined and easily recognized races or groups of races within the species, represented by reddish *iliaca*, slaty *schistacea* and chocolate *unalaschensis*. To date *iliaca* has not been successfully split into subvarieties, but *schistacea* has been more or less divided, and *unalaschensis* has been split into many races upon finer and finer characters of varying stability. We do not here question the validity of any of these lesser forms but submit that the above *iliaca, schistacea* and *unalaschensis* groups are of higher taxonomic order, yet in our formal system all are represented as of equal weight and importance, nor is there in general literature or usage, any word of warning that all subspecies are not of equal value or desirability of recognition.

Describers are often to blame for some of this. Perhaps enthusiasm carries them away, or concentration upon minutiae warps their sense of proportion. Terms like "decidedly larger," "much darker" and the like are freely sprinkled through their diagnoses when in fact weaker adjectives are called for. And we are more and more differentiating averages. We are not here referring to intergrades of the nature of hybrids that naturally occur near the common boundaries of closely allied races, but to individual variations within the centers of distribution of the specialized forms where a comparatively pure line of descent should occur. A group may average larger or smaller, lighter or darker, yet no single individual may be certainly determinable of itself without reference to its geographical origin. There may be theoretical or even practical justification for doing this, but the process has already brought some discredit upon descriptive zoology, and is it *expedient* to lumber up our general system with indeterminables? That these minor divisions should be studied and delimited genetically and geographically is evident, but do they all require particularizing 'name-handles,' and if so, should such names be presented as of equal value and weight with other entities of more direct and evident importance?

One method of making distinction between the lesser and the greater would be to revive the old term 'variety' (var.) or something similar, as 'strain' which is well understood, expresses the relation, and has not had previous use, in a new sense of sub-subspecies. This, unless used only upon particular occasions as is done with the generally disregarded subgenus, would in effect be to introduce quadrinomials into an already extended trinomial system and in practice it might produce differences of opinion that might complicate almost as much as it would simplify. In any case it would be strongly resisted and is, perhaps, impractical however theoretically desirable.

What can be done, however, within the framework of the present system, is to demand more restraint in formally naming the finer results of investigations and to bear in mind and to stress that all subspecies, even those that find full-fledged support in authoritative check-lists and legitimate use in specialized research, are not of equal value, and that the necessity or advisability of individual recognition of them varies with the case and the circumstance of their use and application.—

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'BIRDS AND WIND'

EDITOR OF 'THE AUK':—

In the October, 1941, issue of 'The Auk,' Mr. W. L. McAtee claimed that in 'Birds and the Wind,' (Bird-Lore, Nov.–Dec., 1938) I over-simplified matters. In

the July, 1942, issue of 'The Auk,' Mr. Francis H. Allen, writing in my defense, believes that Mr. McAtee is probably right. I agree also. I, no doubt, did oversimplify the problem.

The primary principle that the net movement of a bird, as we watch it from the ground, must always be the result of its own movement through the air and the movement of the air in relation to the ground is simple enough. The problem, itself, however, is complex. No two species of birds fly exactly alike; different birds have different wing-loading, lift and speed and the wind as we know it is never the result of but one motion but is usually the result of several motions occurring simultaneously.

Mr. McAtee has kindly pointed out faults in writing, which Mr. Allen felt it necessary to defend by saying that Mr. McAtee took me too literally. Aside from these faults, it seems to me that the real issue is in different conceptions of the wind. Personally, I believe that when we study a flying bird we should take the bird's viewpoint of the wind and not the traditional ground view.

As we stand on the earth in a wind, it seems obvious to us that the air is moving and that it is striking us with a force that varies as the square of the speed. In fact, we define wind as "air in motion" and in writing 'Birds and the Wind' I oversimplified matters by holding to that definition. That definition, however, begs the assumption that the earth is stationary. We need only watch the sun to know that the earth also moves. Both the air and the earth move and what we know as wind can only be relative motion, or the difference between two motions, and not a true motion. That relative motion can have no force except at the plane of collision between the two moving elements. A simple rule would be that no object can feel the force of the wind unless it is both on the moving earth and in the moving air at the same time.

To illustrate, let us assume that we have two parallel railroad tracks with a train on one moving at 30 miles per hour and a train on the other at 40 miles per hour. The relative motion between the two—or what we might call a "railroad train wind"—is 10 miles per hour. Unless he puts out his hand and touches the other train, that 10 miles per hour has no meaning to a passenger in either train. He can sit, or stand, or walk, or climb up and down and not be affected in the least by the fact that the two trains are moving in relation to each other. If we call one train "air," then a bird can move at will in it and not be affected by the fact that the earth is moving under him. He will, of course, go where the air goes, just as a passenger must go where the train goes. If the air goes up, the bird will go up, just as if the tracks spread and the two trains moved away from each other. If the tracks approach each other, the trains will move closer together. If air and earth approach each other, the bird will come down.

If a passenger on one train looked out at the other and had no third object, or background, for comparison it would be impossible for him to tell whether it was his train, the other train, or both that moved. From far back in the mists of time we have always assumed that our train was stationary and that it was the other train—the air—that moved. A passenger in the other train could make the same mistake and assume that the entire relative movement was in the earth and that the air was stationary. That is the viewpoint of the bird.

If we take the bird's viewpoint that the air is stationary, it is easier to understand why there is no force that can strike, hinder, maim or kill a bird. The air does move, of course, at a terrific rate if we measure its movement with reference to

the sun. But the bird is no more conscious of that movement than we on earth are conscious of the movement of the earth and for the same reason. The bird is a part of the movement of the air just as we are a part of the movement of the earth. We find it no more difficult to walk west against the rotation of the earth than east and the bird is just as free to move in the air.

If we accept the bird's viewpoint, a tail wind can be defined as that condition where the point on the earth that the bird would like to reach is coming toward him. A head wind would be the condition in which a bird would literally have to chase the earth. That would probably necessitate an increase in the speed of the bird. An airplane can double its speed only at the expense of cubing the power, which comes about in this way. With no resistance to overcome, doubling the power would double the speed. But air has substance and doubling the speed doubles the force with which the airplane strikes the air, and the power must be doubled to overcome it. Doubling the speed also doubles the amount of air struck in any interval of time and the power must again be doubled. Even if I grant that many birds can change the angle of incidence and wing area and thus reduce resistance, the energy necessary to get anywhere in a fast head wind must soon exhaust the bird.

To make a vivid example let us suppose a bird has enough energy to cruise at 20 miles per hour for eight hours. If he rides a tail wind of 20 miles per hour (when the earth is coming toward him at that speed), he will in eight hours arrive at a point 320 miles from his starting point. If, however, he flies in a head wind of 20 miles per hour, (when he must fly after the earth) he must increase his speed or not move at all in relation to the earth. If he doubles his speed, he will, in one hour burn up all of his eight hours of cruising energy and be but 20 miles from where he started. In my opinion, only humans are in such a hurry that they will deliberately fly under those conditions.

In the article 'Birds and the Wind' I made the statement that "a flying bird, which is essentially a part of the wind, cannot be struck by it any more than a man can be struck by the automobile in which he is riding." Mr. Allen writing in my defense says that "a man *can* be struck by the automobile he is riding in if the brakes are put on too suddenly." I would like to ask if it is not true that it is the man who strikes the automobile rather than the auto which strikes the man? When the machine stops, the man continues to move and will strike the windshield with a force that varies as the square of the speed. Which brings me to turbulence which Mr. McAtee confused with wind.

The highroad of the air is not always smooth any more than a highway on earth. Pilots do avoid thunderstorms as Mr. McAtee claims and for the same reason that he would avoid an extremely rough road if he could not decrease the speed of his automobile. There are waves and eddies and "bumps" in the air but the bumps do not hit the bird and airplane so much as they hit the bumps. If we had a bump on a highway on earth we could park an automobile on it and nothing would happen to it. At a speed of ten miles per hour we could drive over the obstruction with no ill effect. But if we hit the bump at eighty miles per hour we would strike with sixty-four times the violence as at ten—the ration of ten squared to eighty squared—and we might wreck both the automobile and ourselves.

If we can accept the average cruising speed of a bird as 20 miles per hour and an airliner as 160 miles per hour, then the airliner is going to strike turbulence with sixty-four times the violence of the birds. Inexperienced pilots have wrecked airplanes in thunderstorms by building up their air speed instead of decreasing

it to a safe minimum. Can any bird fly fast enough to kill itself in turbulence? Personally, I doubt it. I find it hard to believe that birds could commit mass suicide in that way and I find it impossible to believe that wind could cause a massacre. As Mr. McAtee says, there are records of dead birds falling but I would hazard the guess that sometime before they had been carried so high that they starved for oxygen. Or, perhaps, lightning was the cause. We who fly in modern airplanes are virtually inside Faraday cages and are protected, but are birds? In attempting to find an answer to one airplane crash, authorities guessed that concussion from a near bolt of lightning stunned the pilots. That might be the reason for the bird mortality. These are only guesses but I believe they are better guesses than that wind killed the birds. From the bird's viewpoint that is impossible.

Wind can be the "great and good friend" of the birds if they choose to make it so. No doubt, they do occasionally make mistakes and get on the wrong train to be carried off course. Personally, I can do little less than marvel at how often I have found them riding the right train even above the clouds out of sight of land. How they know is, quite frankly, a mystery to me. I am beginning to suspect that sound has something to do with it. We who fly in noisy silence are apt to overlook the fact that birds must be able to hear not only sounds originating on the ground but their own reflected cries. The sonic altimeter for aviators is still in the experimental stage. The birds may have been using it for centuries. At least they should be able to tell from their own echoes whether they are over land or water.

In conclusion, it seems to me that the real issue between Mr. McAtee and myself is not whether I made mistakes in writing but whether it is correct to use our view of the wind or the bird's view of the wind and whether my conception of what the bird's view must be is correct.

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'STARLINGS AND WOODPECKERS'

EDITOR OF 'THE AUK':—

In 'The Auk' for January, 1943, p. 91, Mr. A. B. Howell describes the killing of young Downy Woodpeckers by Starlings: "A Starling with something in its beak approached the hole and appeared to dangle it temptingly at the entrance for a moment, before giving a mighty jab. This was repeated several times. Evidently it was trying to entice, with bait, a young downy within reach of a crippling blow by its beak."

Instead of crediting *Sturnus vulgaris* with deliberately planning such a 'diabolical' act, a simpler explanation of the observed behavior may be offered. Adult birds of various species have been reported carrying food to young of other species. The Starling, attracted by the food calls of the young Downies, takes food to the hole; the unfamiliar appearance of the nestling inhibits the feeding impulse and invokes a hostile reaction. One is reminded of Lorenz's example of the Muscovy Duck that responds to the distress cry of a Mallard duckling by defending it, but responds to its pattern of down by killing it as a nest enemy.

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DR. STRONG'S 'BIBLIOGRAPHY OF BIRDS'

EDITOR OF 'THE AUK':—

Though the author-catalog of the "Bibliography of Birds" was published over three years ago (Field Mus. Nat. Hist., Zool. Ser., 25, parts 1 and 2, 1939), there has been unavoidable delay in the publication of the third and much larger volume, which consists mainly of subject- and finding-indices. Galley proof for subjects as far as "Locomotion" in the subject index was received in the summer of 1941, also 110 pages of page proof, which includes subjects as far as "connective tissue." Occasional requests have been received for information contained in these indices, and I am sympathetic with these efforts.

The finding-index cannot be finally arranged until all of the page proof has been produced. However, all of the items have been written, and they have been sorted for those subjects covered by the page proof received. As a topic may occur in various parts of the subject-index, the finding-index is essential. It is not feasible to attempt to locate at this time all of the references dealing with a topic. However, many can be located, especially those grouped in the subject-index.

I cannot take the time necessary to search through the manuscript of the subject-index or through the many thousands of slips which bear the finding-index items. However, I am willing to employ and direct a student to do what is feasible in collecting references to topics which may interest correspondents. Such service can probably be obtained for 50 cents an hour, and it should ordinarily not take more than a few hours to do what is feasible on any single topic. This expense would be charged to the correspondent. Care would be taken to avoid impracticable searching. My memory of the location of items would of course help.

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NOTES AND NEWS

As this number goes to press, word has been received of the death of Rear Admiral Hubert Lynes, R.N., Honorary Fellow of the American Ornithologists' Union, on November 10, 1942, and of Dr. Leonhard Stejneger, Fellow Emeritus of the Union, at Washington, D. C., on February 28, 1943.