capacity" are alike of an order difficult for man to appreciate. But so far as known the difference is one rather of degree than of kind. Applied to the case of the wasp, *Pelecinus*, is not the established olfactory sense sufficient to explain how the elusive males can find the females, thus being drawn forth from their retirement, probably from no such great distances as we may sometimes be led to imagine, and revealing themselves to the comparatively dull visual faculties of the naturalist? Every hunter has experienced a parallel case, hardly less striking, in the magical appearance of swarms of blowflies which arrive to "inspect" his game almost as soon as it is dead. It cannot be seriously questioned, I believe, that the highly refined olfactory sense is adequate to account for all this, and that it is the same in kind as that which brings the bear to the bait from afar and enables the dog to trail his master through the crowded street.

It is not the intention to deny the possible existence in animal life of other senses than the orthodox five that come within the pale of human experience; far from it. That the "homing sense" is a sixth one may well be true. When we shall have learned more about the functions of all parts of the internal ear and shall have added something more definite to our knowledge of what has been called "muscle sense," then this question may possibly be answered with a degree of assurance. While freely admitting the attractiveness and stimulating effect of formulating working hypotheses and theories, the point I wish to emphasize is simply that we should first of all exhaust the explanatory possibilities of the scientifically proven sense functions, in the analysis of observed phenomena where matters of this nature are involved, before proceeding to draw from the realm of the unknown. On the evidence adduced I feel that this procedure has not been followed in the case of the two vultures, and that the assumption of the existence of an "active sense which may be called 'occult'" even "simply because it is hidden from the experience and understanding of man," is not justified.

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The Search for Food by Birds.

Editor of 'The Auk':

The following remarks suggest interpretations that may be placed upon observations, different from those associated with them by Messrs. Beck and Grinnell in 'The Auk' for January, 1920 (pp. 55–59 and pp. 84–88). In the former article, an occult sense is invoked to account for Turkey Vultures finding the carcass of a mad dog thrown out of sight in a sinkhole by fox hunters. From evidence given in the article, there can be no certainty that the entire performance of killing the dog and throwing it in the hole was not watched by buzzards. Had some of the

birds been sailing overhead at a considerable height (a common habit), probably they would not have been seen by the hunters, yet every move of the latter might have been observed by the birds; the presence and actions of the pack of hounds would almost certainly have attracted the attention of any birds on the wing, even had they just left the supposed nearest roost, eight miles away. Furthermore the observations as reported do not exclude the possibility that the vultures were already in the hole where the carcass was thrown. Either of these suggestions seems easier to entertain than that the buzzards were guided to the carcass by a means outside of human experience.

Certainly in the classic experiments of John Bachman as reported by Audubon (Orn. Biogr., Vol. 2, 1835, pp. 44–49), both Turkey and Black Vultures, showed their absolute dependence for food-finding upon the sense of sight, and ignored food they would have found immediately had they been able to smell, much less had they been possessors of an "occult" food-finding faculty. Consider the following extract, "The most offensive portions of the offal were now placed on the earth; these were covered over by a thin canvass cloth; on this were strewed several pieces of fresh beef. The Vultures came, ate the flesh that was in sight, and although they were standing on a quantity beneath them, and although their bills were frequently within the eighth of an inch of this putrid matter, they did not discover it. We made a small rent in the canvass, and they at once discovered the flesh, and began to devour it. We drove them away, replaced the canvass with a piece that was entire; again they commenced eating the fresh pieces exhibited to their view, without discovering the hidden food they were trampling upon."

Dr. Grinnell's thesis is that certain call-notes may have been fixed by selection on account of their utility in preventing individual birds from seeking food in areas recently searched by another bird. His examples are the Ruby-crowned Kinglet and Audubon's Warbler. The eastern representative of the latter bird, the Myrtle Warbler, is similar in habits and has a very similar call-note. This is uttered frequently whether by the few warblers or perhaps single wintering bird in a given locality, or by the individuals of a perfect swarm of the warblers such as winter in coastwise parts of the Carolinas. In the former case risk of searching the same area twice practically does not exist; in the latter that the same area will be gone over more than once daily is inevitable. In either event the call-note cannot have the significance hypothecated by Dr. Grinnell.

In fact birds do habitually go over the same places. A tree infested by bark-beetles is not freed of its pests by continuous work on the part of woodpeckers; on the contrary they return to it again and again. Our feeding-stations with practically inexhaustible supplies are periodically visited, and tempting as they are, usually do not localize the birds. These have other business elsewhere, but they return. Many observations by the writer, confirmed by comparing notes with others, indicate that vari-

ous birds have more or less regular beats which they cover approximately on schedule. This means they do repeatedly go over the same trees; but in their territory they undoubtedly make excursions, for when we test them by exposing food supplies they quickly find them. Their system of food-finding, like that of various other animals (as ants and mice), is, I am convinced, to look everywhere in their domain. They have all their time for the work, and searching all day every day, in the comparatively restricted area, to which most birds at any given time, appear to confine themselves, it is inevitable that the same spots will be inspected again and again.

The appeal to theory when observed facts really have nothing particularly mysterious about them, seems to be due to taking too seriously the so-called "struggle for existence." Except at the breeding season, an individual bird has practically nothing to do but to search for food. Under anything like normal conditions there must be no great difficulty in securing the required amount. In fact in the case of Audubon's Warbler and numerous other birds of mixed feeding habits there is always available a reserve food supply, in the form of overwintering fruits, upon which the birds can draw at will. Such birds, therefore, distinctly are not under constant pressure of necessity of food-finding. They at least have leisure, though their actions may belie it. In the writer's opinion, all birds, normally, are not in dire straits for food. Of the smaller species, at least, I would say, they make countless unnecessary excursions, they peck a hundred times for each morsel of food secured, they are, they must, they will be busy. This ceaseless unproductive activity in itself is sufficient evidence that the struggle for existence is not the gripping, controlling thing some would believe.

In conclusion I would mention briefly certain other points in the two papers reviewed that seem rather too highly tinged by theory. The sense of direction, admittedly marvellously developed in certain birds, is not entirely occult to man. Australian natives and other savages have been recorded as having it in marked degree and civilized man certainly does not entirely lack it. The wonderful cases of male insects finding females immediately after issuance from their pupal cases certainly are more satisfactorily explicable on the basis of a finely developed tropic sensitivity than on an occult mate-finding sense. Results of experiments certainly support this view, since female moths emerging in indoor cages, as in greenhouses, have attracted numerous males, though the circumstances could not agree in the province of any mate-finding sense that would have developed under natural conditions. In other words, since greenhouses have never been part of the normal environment, an "occult" mate-finding sense developed by natural selection would not take male moths into such a structure. However, a very sensitive tropic reaction would take them there or to any other accessible place where the excitatory object, the female, happened to be.

With respect to Dr. Grinnell's note, it should be pointed out that in winter when the observations were made, insect life, for the most part, does not "move about again." Hibernating insects are relatively stationary and a considerable part of the insect food available to small birds at this season consists of the eggs and chrysalides of numerous insects, and adult scale insects, which do not change location at all. Furthermore, since there is no hard and fast line between non-flocking and flocking birds, any sequestration theory is bound to run counter, to the recognition-mark and related theories. Indeed, does it not appear that theories are best avoided? When facts accumulate sufficiently, their average tendencies, which we are in the habit of calling "natural laws," are apparent of themselves.

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[In connection with the oft-quoted Bachman-Audubon experiment which Mr. McAtee once more falls back upon, why cannot some of our ornithologists in the Southern States, where Vultures abound, try this experiment over again? We are not usually willing to accept a statement of this sort without corroboration and why should we not have more light upon this matter?—Ed.]

Ridgway's Birds of North and Middle America, Vol. VIII.

Editor of 'The Auk':

In a monumental work such as Ridgway's 'Birds of North and Middle America,' errors are certain, however careful and competent the worker may be. Part VIII of that work has just been received and I hasten to indicate rather an unfortunate mistake so that correction may be at once undertaken.

On p. 608 appears "*Larus affinis* Reinhardt, Siberian Gull," and its only claim to inclusion in the work appears to be the record of the type described from "Nenortalik, Julianehaab, S. Greenland." I have shown that the type was not referable to the Siberian Gull so-called, but was a specimen of the form of *Larus fuscus* Linné which Lowe had separated under the name *L. f. britannicus*. This has been accepted by all British ornithologists and the entry in Ridgway's synonymy, p. 609, "*Larus fuscus affinis* Kennedy, Ibis, Jan. 1917, 31" refers to this fact and not to the "Siberian Gull." Consequently all the matter under the heading "*Larus affinis*" on pp. 608–609, save that dealing with Reinhardt's specimen and the one above quoted, must be eliminated as not pertinent to the American fauna. The essential references in confirmation read:

Lowe, British Birds (Witherby), Vol. VI, No. I, p. 2. June 1, 1912.

Lowe, Bull. Brit. Orn. Club, Vol. XXIX, p. 119. July 17, 1912.