

was first seen by Miss Jordan, and it was viewed for several minutes at very close range by herself and Mrs. Hatch.

Another rare species is the Blue-gray Gnatcatcher, *Polioptila cærulea cærulea*. An individual of this species was here on August 31, 1908, and for some time was watched through powerful binoculars when no farther distant than twenty to fifty feet.

The last case may possibly be considered by some people as a hypothetical one. It was outside this decade and before the days of the binoculars; but the strange, little, gray bird that through long, hot, August days so constantly sang the unfamiliar notes of 'peto, peto' will always be thought by me to have been a Tufted Titmouse that had wandered north of its customary range.

A DIFFERENT ASPECT OF THE CASE OF ROOSEVELT VS. THAYER.

BY THOMAS BARBOUR.¹

MR. Francis H. Allen, in 'The Auk' of last October, has published some comments on the 'case of Roosevelt vs. Thayer, with a few independent suggestions on the concealing coloration question.' Mr. Allen's remarks are very unfair to all those who are unable to agree with Mr. Thayer's conclusions. His independent suggestions are, for the most part, unimportant, and add little to the arguments for either side.

In the beginning of Mr. Allen's recent paper, we find ourselves compelled to take issue with him on the question of what is 'common sense.' He says, "In Columbus's day common sense declared the world was flat." This was a dictate of science, and was as worthy of being believed at that time and in that state of knowledge

¹This would probably have been a paper written jointly with Dr. J. C. Phillips had he not left a short time ago for the Sudan. I assume sole responsibility for it, as it stands. A large part is written from notes which we made together some time ago, and for the permission to make free use of these I thank Dr. Phillips very heartily.

as the fact that the world is round is of being believed now. Again "more recently it [common sense] carefully protected the consumptive from 'night air.'" Here Mr. Allen is unfortunately unable to distinguish between superstition and common sense. Some of us have had great-great-grandmothers who were so unfortunate as to have lived in Salem. There they were hanged as witches, and yet this somewhat common practise can hardly be laid to the door of the 'common sense' of those times, but rather to superstition, which is, as yet, often persistent. We absolutely disagree in believing that common sense is "still an obstacle to the spread of scientific education." We consider it science's most powerful ally as superstition is her worst enemy. We agree heartily with what is said regarding the "arrogant attitude he [Thayer] seems to take in regard to the relative claims of the artist and the biologist to be entitled to form an opinion on the subject of coloration,— even more prejudicial, if less irritating, is the — shall I call it cocksure? — way in which mere conjectures are stated as facts." We also agree with Mr. Allen absolutely that a fair attitude towards Mr. Thayer must begin by admitting that he is an expert colorist, and that his perception of color and the value of light and shadow is probably as far ahead of the average scientific person's perception as night is from day; yet we must remember that Mr. Thayer knows nothing of any other than human color perception, and his haphazard assumptions that mammals, birds, reptiles, and insects see in the same way as human beings do, is just what grates most harshly upon the intelligence of the average scientific person.

We read later "I have detected in Roosevelt's paper and the reply to Thayer's criticism, appended thereto, upwards of fifty instances of misquotations, misrepresentations and perversions of Thayer's statements, and pieces of faulty reasoning in matters of detail." These are serious charges, but we must point out that the offences vary greatly in magnitude. It is a great pity that Mr. Allen did not state how many misquotations and how many pieces of 'faulty reasoning in matters of detail' he found. A misquotation would probably be wilful, while a bit of 'faulty reasoning in a matter of detail' might be an instance of where Mr. Roosevelt's opinion was at least worth as much as that of either Mr. Thayer or Mr. Allen.

Later Mr. Allen says, "Then, on page 162 we are told that the

Scissors-tailed Flycatcher is conspicuous in *shape*, but we are not informed, how a bird can be conspicuous in shape." I can answer this question easily by simply stating that a bird can be conspicuous in shape by being like a Scissors-tailed Flycatcher. I strongly mistrust that Mr. Allen has never seen one of these birds in life; their conspicuous shape and their still more conspicuous method of displaying it in their open Plains habitat would have saved Mr. Allen from making such a naive display of his ignorance, had the opportunity for observation ever been presented to him. Mr. Roosevelt is absolutely correct, when he says that the bird is conspicuous 'in color and in habit, has no concealing coloration, and never conceals itself.' Mr. Roosevelt has obviously seen the bird in life. I also have had the good fortune to observe it. This is not a case where Mr. Roosevelt can be called 'stupid.' In a later paragraph we are given another example of 'Roosevelt's dogmatism.' His statement that the typical red fox and the cross fox are 'equally successful in life' is challenged, and we are asked if equally successful, why is not the cross fox as common as the red fox. We can answer that we have no evidence to show that the cross fox is shorter lived, less vigorous, or less well able to catch food than the red fox, or that it is in greater danger from its enemies. The reason why it is less common is purely and simply determined by laws of heredity, which govern the numerical relationship which a 'sport' bears to the parent stock, when no artificial factor steps in and provides for 'sports' only, mating together. We disagree absolutely with Mr. Allen's absurd quibble that "a very little reflection would have shown . . . that no two species ever live under precisely the same conditions." Why not? We believe that very many birds and, indeed, that many animals of all groups live under conditions so near alike that slight differences could not possibly prevent the same biological forces working equally upon all of them. In the matter of color gradation and counter shading, we admit that Mr. Thayer has made great discoveries in optics. Counter shading is certainly not universally existent. Mrs. Barbour, however, has recently called my attention to its frequency among such garden vegetables as melons, cucumbers, gourds and the like and how ineffectually it conceals them. Its effect is certainly destroyed in many in-

stances by an animal's crouching or lying down, but the most important of all seems to be the fact that it does not seem as effective for an animal seen from end view as it is in one seen from the side, and yet, of course, the animal is in as great danger from enemies which may come head on, or from behind, as from those approaching from the side. Mr. Thayer has perhaps never thought of this: Mr. Roosevelt probably has.

Mr. Allen is evidently blessed with that type of mind which wants to see things definitely settled one way or another once and for all. From his writings we presume that he believes that a definite theory is, by the fact of its being definite, worth more than a vague theory. The truism 'I don't know' certainly does not appeal to Mr. Thayer, and apparently it does not to Mr. Allen. Both want to swallow the theory of natural selection reduced to its lowest terms, hook, bait, and sinker, and bring us to believe that this is an universal law, all powerful in its results or effects. No scientific man, or at any rate very, very few, will follow their ridiculously cocksure attitude in regard to this belief. Mr. Thayer's declaration for 'natural selection, pure, simple, and omnipotent' is a dogmatic statement more jarring to scientists in our present incomplete state of knowledge than Mr. Roosevelt's assertions are irritating to Mr. Thayer. Sexual selection is an entirely different problem. It has been observed in actual operation, and if Mr. Thayer cares to study the habits of many birds and animals, he can see it working for himself,—if he is open minded. We believe that coloration is found to be a negligible factor in the life economy of an immense number of species, of which the crow is an excellent example. Keen wits, in this case, make other protection unnecessary. If we mistake not, Darwin has said that sea birds need no protection, hence their conspicuous coloration; and when we are advised to distribute a number of skins of "forest birds and sea birds impartially in the tree tops in some thick wood and see whether there actually is any difference in their conspicuousness or not," we only say that birds of the field or marsh, if put in the forest in some such way as this, would be equally well protected with the forest birds so far as their coloration goes, and that the conspicuous color of the sea bird is well matched by species of the family Cotingidæ which live in the green woods of South America.

We have been advised by Mr. Allen that ridicule is a powerful weapon and sorely as we are tempted, we are trying to keep away from this sharp-cutting blade. When Mr. Allen says, "the fact that Mr. Thayer may have been mistaken in regard to the habitat of the Peacock does not vitiate all of his experiments," he should have added, truly it does not, yet it certainly does *vitate* the one that had to do with *the Peacock*, and this was *all* that we expected it to do. We must take a crack at the now famous Blue Jay, and his shadow on the snow. The jays are a tropical family, species of jays with blue or green coloration occur wide-spread in both tropical and temperate regions. The Florida Blue Jay is almost exactly similar in plumage to the species hereabouts. It lives where there is no snow, as does our Blue Jay a full half of the year. We are frank to admit that our Blue Jays hereabouts do occasionally match the shadows on the snow if seen in exactly the right position, but 'common sense' tells us that this fact has absolutely no biological significance whatever. In regard to the white rump of the deer, I must add just this suggestion to what may be said regarding deer and their enemies. Deer are hunted by wolves more than by other species of animals. Wolves hunt in packs. The deer's white rump might, under certain rather rare circumstances, fool one wolf out of a pack for a short moment during the pack's pursuit. It might at vastly rarer intervals fool all the individuals of the pack were all their eyes at the right level at exactly the right time, but that it could fool all the members of a keen-nosed pack of hungry wolves long enough to allow of the deer's escape is again a matter where I think 'common sense' must certainly be called in. Personally I have experimented with captive deer under wild conditions; *i. e.* in a large park. I have had excellent opportunity for observing them carefully under many conditions with Mr. Thayer's theories in mind. I have also had color varieties of the European fallow deer, which were both counter shaded and solid colored, some pure white, some deep chocolate brown all over, and some with brown backs shading to light bellies. In every case, the solid colored, chocolate brown individuals were the most difficult to see, especially at dusk, the regular time when the wild deer begin to move about and feed. Mr. Allen backs water very hard when he says, of the possibility

that foxes and dogs may locate their prey by scent, that this may militate seriously against Mr. Thayer's contention that the final spring on all occasions is directed by sight alone. I think the important point here really is that we find no evidence that beasts of prey are unable to maintain themselves perfectly successfully in spite of the operation of all these supposedly adverse conditions. If an animal can get all the food it needs, what more does it want? So much for our remarks on Mr. Allen's paper. They are somewhat disjointed and perhaps prolix. We could pick him up on many other points, but this serves to show that his desire to simply bolster up the arguments of a friend would have been more convincing had they been more impartially conceived.

Some time ago, Dr. Phillips and I reviewed Mr. Thayer's book (Auk, April 1911). We put a number of direct questions to Mr. Thayer at that time which we hoped he would answer, both for his own sake and as an evidence to naturalists in general of his sincere desire to really keep this discussion going, to open up the whole matter of coloration so far as possible, to suggest fields of inquiry and experimentation, and not simply to sit down on the top of a heap of facts, which he claims to have discovered and take the attitude that the whole business is settled. Mr. Thayer claims to be interested only in what he terms facts, whys and wherefores receive practically no attention. Franklin did not discover lightning, but he proved its causation through its connection with electrical phenomena, and for that reason became very great. The least increment to our knowledge of how differences are brought about by evolution, actual endeavours to prove experimentally, if possible, the working of evolution relating to the origin of coloration would be worth more than many pages devoted to proving that an oryx's head may be well concealed in a pine tree. Since Mr. Thayer published his book, he has given us a figure (Pop. Sci. Mon., July 1911, p. 21) showing a lion approaching three antelopes uphill. The 'lion's horizon line' and the level of the plains, 'appearing to meet the level of the lion's eye,' make an angle with each other of about 20 degrees, and under these conditions, according to the 'great optical principle' which 'I have discovered' the antelopes are rendered invisible to the lion through their counter shading. Supposing, however, that the

light was coming from the direction of the antelope towards the lion, or that the ground sloped in the opposite direction; i. e. from the lion towards the antelopes, or supposing that the ground was level or undulating, or supposing, again, that the lion was watching for its prey from some eminence, overlooking the feeding ground of the antelopes, then the protecting value of this coloration would be nil. As a matter of fact, lions kill nightly, or whenever they care to. No traveller has ever found them starving to death or unable to provide as much food for themselves and their young as they needed.

The rabbit's greatest enemy in England is the stoat, in New England, the weasel. These enemies hunt by scent alone. They are the only enemies which the rabbits have that would have a visual horizon line low enough for the rabbits white tail etc. to act in an obliterative manner. Every game keeper in England will tell Mr. Thayer, if he asks, that once a stoat takes up a rabbit's trail, the rabbit is absolutely sure to die. Of course, experiments made with dummies and dead skins do not bring out this fact. Using no living animals Mr. Thayer does not realize that color perception and the range of vision vary widely among different organisms. We call his attention to the enormous mass of past and current literature in animal psychology, having to do with experimental work in just such matters as the color perceptions of animals. Could he not correspond with some of these workers, Prof. R. M. Yerkes of Cambridge, for example, to their advantage and to his.

The question is not always are all organisms protectively colored, but do protective colors protect? This, perhaps, is capable of being tested by carefully controlled experiments conducted with living animals under conditions as nearly as possible natural. We do not wish for interpretations in terms of human vision. We do not care to know what is perceptible to the splendidly trained artist but rather what animals themselves see and how other organisms appear to them. So far, our meagre knowledge permits us to say that we have no direct conclusive proof of the efficacy of special coloration. Davenport, in investigating the number of fowls killed by vermin, i. e., weasels, etc. thought that there was the greatest mortality among the solid colored birds, but Pearl, with

a larger set of figures, found that there was no relative immunity among the 'pencilled birds.' In fact, his figures rather favored the solid colored birds.

He shows (Amer. Naturalist, Feb. 1911, p. 117) that "ever since the first description made by the Nuremburg miniature painter, Rösel, in 1746 of a case of presumably protective coloration, we have been prone to argue that because an organism was colored or formed in such a way as to be inconspicuous, it was therefore necessarily *protected* from attack by its enemies to a greater or less degree. The logic of such reasoning is flawless; it ought to be protected, but a conclusion may be perfectly logical and still not true. In a study of protective coloration, including mimicry, it is essential that a discovery that an organism is to human eyes inconspicuous, or not readily distinguishable from some other organism, shall not be considered the final goal. Let such a discovery be supplemented by an experimental or observational determination of whether this inconspicuousness really helps the organism in actual practise in avoiding elimination by natural enemies." In many cases we have no theories to substitute for those of Thayer, but we do not hesitate, however, to say that the burden of proof rests on him. The evidence is all against him, though it is for the most part of a negative sort. Meagre and negative as it is, however, it is worth a great deal more than pure, unfounded speculation based upon what is seen by a trained man's eye interpreting animal vision. Thayer's color experiments are not really scientific experiments in any biological sense. They are mathematical demonstrations in human optics, pure physics and nothing else. As aesthetic, physical demonstrations, they are of great interest, but as to their interpretation in terms of the organic universe they are of little interest and of no value. Thayer's point of view is summed up in one sentence of his own words (Pop. Sci. Monthly, July 1911, p. 35) "I have been studying for years to find out the exact scene that each costume best represents, and I now beg my readers to come to Monadnock and let me show them the results."

The evidence in Sumner's paper (Jour. Exp. Zool. May 20, 1911) regarding the color response of flat fish, when placed on different background both natural and artificial, is a model which Mr. Thayer might well study. Sumner, though he has seen at first

hand perhaps the most remarkable case of protective coloration on record is careful not to generalize or to force on the reader any such protective value to account for the facts. He concludes his discussion by saying that his few statements illustrate the paucity of our direct evidence on the whole question of protective coloration, and remarks that most of our conclusions are entirely of an inferential nature.

The results of Prof. J. Reighard's studies, at the Tortugas Islands, of the coloration of reef-fishes are very important in this connection and worthy of careful examination. Will Mr. Thayer inform us whether or not he has seen this work?

As to mimetic resemblances our best theories have been entirely inferential in nature. We have jumped at conclusions, obvious enough though they seemed at first sight. In his "Darwinism of Today" Kellog calls attention to a case of overspecialization as an argument against natural selection. He describes the well known Kallima butterfly. After showing how unnecessarily perfect the butterfly's resemblance is, he says "When natural selection has got the Kallima along to that highly desirable stage where it is so like a dead leaf in general seeming that every bird sweeping by sees it only as a brown leaf clinging precariously to a half-stripped branch, it was natural selection's bounden duty in conformation with its obligation to its makers to stop the further modifying of the Kallima, and just to hold it up to its hardly won advantage. But what happens, Kallima continues its way, specifically and absurdly dead leaf-wards, until today it is much too fragile a thing to be otherwise than very gingerly handled by its rather anxious foster parents, the Neo-Darwinian selectionists." My own experience has been that Kallima often, perhaps even generally, rests with wings open or fanning.

It seems a pity to return to the case of the zebra. We draw the following conclusions from the observations of careful naturalists:

I The zebra is one of the most plentiful of all the plains' dwellers.

II That he and the hartebeests form in many regions almost the sole food of the lions.

III The lion kills at will and with little effort. This is shown by numberless actual observations.

IV The zebra shows little concern in the lion's presence. He

feeds down wind to water holes and thick covers, and, in fact, takes not the slightest precaution for his own safety.

These observations seem to be absolutely all that we know regarding the relations which the habits of the lions and the zebras bear to one another. It is hard to fit in any clause relating to protective coloring which would seem to be capable of support by observations, to account for more than the fact that Mr. Thayer has been able to conceal dummy zebras successfully in New Hampshire under various conditions of his own arrangement. We are all mentally prone to inferential methods, this is a common failing of the human mind, and one to which an artist dealing with physical and mechanical phenomena naturally would be very prone. The artist dealing only with the visible and the superficial would naturally turn to the arguments of pure logic rather than to animal experimentation. He lives in an Arcadian land where no conflict of facts or deeply concealed natural laws concern him in the least. The obvious and the all embracing theories are the ones that appeal to him most. We have often pondered on how color patterns may have originated. Mr. Thayer has doubtless done the same thing. His theories demand that we should admit the existence of a constant inter-specific struggle and a selectional value for incomplete color schemes, but we feel grave doubts as to the efficacy of natural selection *alone* in bringing about the species of the present time. Mr. Agassiz often said that natural selection probably explained the survival but not the arrival of species. One cannot account for the arrival of a new organ nor the loss of an old one by Darwinian selection alone. The question of the origin of new characters in general is a problem of the greatest depth and importance, and one that is here out of place, yet how especially difficult is it to imagine with Thayer's reasoning the origin of a new color pattern of doubtful value when complete, and of no selectional importance in its elemental state.

We find birds of such varying types of colorations, living under the same conditions as far as the operation of broad selectional principles are concerned, that it is fair to assume that all cannot be equally protected. There are in the upper leaf zones of the tropical forest, birds of which the following are but a few of the colors displayed in their plumages. One may find white birds and black

birds, pink birds, green and yellow, and black and red, and black and green, and magenta birds, sky blue birds and brown birds of many shades, and many with a bewildering number of conspicuous shapes. We use these words advisedly. Can these birds all be equally protected under the same or almost the same conditions? We ask Mr. Thayer frankly to tell us that if such and such types of coloration are concealing as he says they are, are not perhaps such and such other types of coloration equally conspicuous; and then let us see whether in the environment under discussion, we cannot perhaps find these or similar types of coloration displayed by birds apparently as successful as those supposedly protected by coloration. In other words, we ask Mr. Thayer to answer our questions, to meet our arguments fairly and squarely, and not simply to fall back on dogmatic assertions, based upon his interpretation of the physical laws of human optics. It may seem futile to keep bandying words back and forth. The subject is one, however, which is well worth the opening up it is just beginning to receive. We have been severely criticised by Mr. Thayer for our previous review of his work. We hope now that he will come forward and meet our arguments, not with other examples of his own discoveries, but with definite answers to the questions which we have put to him, now and hitherto. Why should flamingoes be pink, if they lack enemies? Why should sea birds be protected when many of them apparently have no enemies at all? How can black birds, white birds, green birds, and brown birds all be equally protected in the same forest by the same light rays filtering through the same green foliage?



Barbour, Thomas. 1913. "A Different Aspect of the Case of Roosevelt vs. Thayer." *The Auk* 30, 81–91. <https://doi.org/10.2307/4071897>.

View This Item Online: <https://www.biodiversitylibrary.org/item/54992>

DOI: <https://doi.org/10.2307/4071897>

Permalink: <https://www.biodiversitylibrary.org/partpdf/89262>

Holding Institution

Smithsonian Libraries and Archives

Sponsored by

Smithsonian

Copyright & Reuse

Copyright Status: Public domain. The BHL considers that this work is no longer under copyright protection.

This document was created from content at the **Biodiversity Heritage Library**, the world's largest open access digital library for biodiversity literature and archives. Visit BHL at <https://www.biodiversitylibrary.org>.