REMARKS ON THE CASE OF ROOSEVELT *VS.* THAYER, WITH A FEW INDEPENDENT SUGGESTIONS ON THE CONCEALING COLORATION QUESTION.

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COLONEL ROOSEVELT in his recent paper on 'Revealing and Concealing Coloration in Birds and Mammals' [1] makes an attack on the work of Messrs. Abbott H. and Gerald H. Thayer and sounds the slogan of 'common sense' as against the Messrs. Thayer's 'wild absurdities,' as he is pleased to term some of the views set forth in their book. Other persons have spoken approvingly of the sound 'common sense' of Roosevelt's paper. Now common sense is an excellent thing; I might go farther and call it indispensable; and yet, with the greatest respect for it, we must admit that it has its limitations. In Columbus's day common sense declared that the world was flat. More recently it carefully protected the consumptive from 'night air.' And, if I mistake not, it is still an obstacle to the spread of scientific education. It is hardly safe, I think, to trust to common sense alone to settle the question of concealing coloration or any other scientific question. It is science that must settle it, though she must call on both common sense and imagination to help her,— imagination as well as common sense, for one without the other would be only a hindrance. Any science that goes deeper or soars higher than the mere accumulation of facts must make use of the imagination.[2] This is a truism, of course, but it seems necessary to insist upon it a little under the circumstances.

And we must also face the fact that it is not always the best, *i. e.* the most accurate and diligent, observer that makes discover-

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[1] Bulletin of the American Museum of Natural History, Vol. XXX, Art. VIII, pp. 119–231, New York, August 23, 1911.

[2] "My success as a man of science, whatever this may have amounted to, has been determined as far as I can judge, by complex and diversified mental qualities and conditions. Of these, the most important have been — the love of science — unbounded patience in long reflecting over any subject — industry in observing and collecting facts — and *a fair share of invention as well as of common sense.*" — Charles Darwin. (The italics are the quoter's.)

ies. It is the thinker rather, never the *mere* observer. It must be admitted, I think, that light on scientific problems sometimes comes from the outside, that is from outside the group of workers who fancy themselves the only ones who know anything about their specialty. Of course we welcome the light no matter what its source, even though it may come from one who has given most of his life to art rather than to science, even though that artist may have adopted a far from deferential tone towards the naturalists whom he is trying to convince.

I cannot help thinking that Mr. Thayer (to save trouble I shall speak of him in the singular number) has prejudiced his case among ornithologists not a little by the manner in which he has presented it. Even more prejudicial than the rather arrogant attitude he 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. His book would have gained much in weight, I think, if a proper distinction had been made between those propositions which were in some sort susceptible of proof and those that should have been put forth only as suggestions. Nevertheless, though the book is far from being a safe guide for the uninstructed, it ought to be possible for scientific men to read it in an unprejudiced spirit, making all proper allowances for the 'artistic temperament' that shaped its form. It is a regrettable fact, however, that some reviewers have *seemed* to be more intent upon bringing Mr. Thayer into ridicule than on arriving at the real facts in the case of concealing coloration. 'Seemed' I say, for, though perhaps I do them an injustice, that is the impression a reader is bound to carry away with him. Ridicule is a powerful weapon and the temptation to use it unsparingly is a strong one. But I want to ask fair treatment for Mr. Thayer. Even if we don't agree with him, it is not necessary either to cut him into little pieces or to break every bone in his body with the 'big stick.'

If we adopt a fair attitude towards Mr. Thayer and his book, we must begin by admitting that by virtue of his profession he *is* an expert in all matters pertaining to color. A scientific man may know all the artist knows about the laws of light and color,

but no man of science who is not also a painter has the habit of mind that keeps him constantly on the watch for effects of color, pattern, and light and shade. How many ornithologists are in the same class with Mr. Thayer? Mr. Fuertes, perhaps, who agrees with Thayer in the main, but certainly not Dr. Barbour, nor Dr. Phillips, nor Colonel Roosevelt. Mr. Thayer of course cannot as an artist claim exclusive right to weigh the facts and render judgment. That is the office of the scientific men. But he has a right to testify to the facts and to be accorded a respectful hearing. Scientific men will not reject the artist's testimony because he may have the 'artistic temperament,' though it is proper to take that into consideration in determining the admissibility of the evidence. I venture to express the opinion that Mr. Thayer knows more about the coloration of animals in its relation to the concealment of those animals than any other man in this country. Perhaps he knows a few things that 'ain't so,'— personally I think that some of his knowledge does belong to that category,— but it will not do to deny him the credit of a really vast knowledge of this subject. His opinions, therefore, are entitled to respect, much more respect than they have received at the hands of some of his critics.

Mr. Thayer was the first to call attention to the function of counter-shading in the concealment of animals. I think most naturalists admit its importance. We owe Mr. Thayer a debt of gratitude for pointing out this interesting fact. It was because he was an artist that he discovered it,— because he had formed the habit of seeing things as they *looked* rather than as he knew or suspected them to be. Scientific men had been at work on the problems of coloration for many years without discovering this thing that now seems so obvious. We see it now, and we admit it. Perhaps some others of Thayer's discoveries — to use his own word — are less obvious, but that does not make them necessarily any the less truly discoveries. We ought to hesitate to reject them without considering very carefully whether in these other cases the expert in colors and appearances may not be in the right. And again I respectfully suggest that common sense cannot settle the question. Common sense has made up its mind. Open-minded science must settle scientific problems.

Having tried to show why Mr. Thayer should be accorded a considerate hearing, I will now attempt to show why Colonel Roosevelt, with all his wide field experience, is not a safe guide to follow implicitly. I adopt this method of approach because I fear that Roosevelt's paper, following that of Barbour and Phillips, has had a reactionary influence out of proportion to its importance, and I am led to believe that it has not been examined very critically.

I have detected in Roosevelt's paper and the reply to Thayer's criticisms appended thereto upwards of fifty instances of misquotations, misrepresentations, and perversions of Thayer's statements and pieces of faulty reasoning in matters of detail, while the paper is full of dogmatic utterances which must be just as offensive to fair-minded readers as any of Thayer's unguarded overstatements — and more so. A few specimens will be sufficient, I think, to show Roosevelt's inaccurate habit of mind and slapdash style of thinking. In two places (in the footnote on page 156 and on page 220) he instances the photographs of certain birds taken by Messrs. Job, Finley, and Chapman as showing the conspicuousness of those species in a state of nature, quite overlooking the obvious facts that the photographers naturally chose the conspicuous subjects, avoiding those that were at all obscured and getting their cameras into positions where the birds would come out most clearly, and thus made the birds as conspicuous as they possibly could, which was the end and aim of their work. I take it that the birds in most photographs do not appear at all as they would under average conditions in their natural surroundings. Then, on page 162 we are told that the Scissor-tailed Flycatcher is conspicuous in *shape*, but we are not informed how a bird can be conspicuous in shape. I suppose if a row of Kingbirds were pinned against a white screen and a Scissor-tailed Flycatcher were placed in the middle of the row, the latter bird would be made conspicuous by its shape, but how could it be so in its natural surroundings? It is evident that by 'conspicuous' Roosevelt here means unusual, remarkable, but the words are by no means synonymous. I shall have something to say later on this confusion of ideas that tends to call an unusual or brightly colored object conspicuous. As to the Scissor-tail, Roosevelt goes on to say that it is conspicuous

"in color and in habits, has no concealing coloration, and never conceals itself," and that "its long tail merely adds to its already great conspicuousness." Though this is stated as a fact, no evidence is given in support, and we must regard the statement merely as the expression of an opinion,— an opinion which, in view of its author's confusion of mind as to what constitutes conspicuousness, may be taken for what it is worth. One of the most astonishing of Roosevelt's blunders is his failure to see that white cannot possibly show light against a clear moonless night sky. On page 176 he says that "even against the sky line" the white rump of the prongbuck is "always advertising at night"; and on page 179 he says that "at night white is not normally a sky color,.... so that these white stern marks are *not* 'sky pattern marks' at the very time when, according to his [Thayer's] theory, they serve as such." Now the night sky, if not what we should call white in the daylight, is yet the lightest thing to be seen on a moonless night, and the deer's tail or the antelope's rump, not being luminous in itself, cannot possibly be any lighter than the sky which is the source of light.[1] White is white, of course, only by virtue of its reflecting all the light that strikes it. It needs only a little knowledge of the significance of colors and a little 'common sense' to see that, but it is easy enough to prove it by experiment too. And Roosevelt's failure to appreciate it is the more astonishing when we are informed that he has experimented with a white towel, only to find that his own views are completely vindicated! In this one point, it seems to me, Roosevelt has shown so complete an ignorance of the most elementary laws of color as to go far towards unfitting him for any discussion of the subject of animal coloration, while showing besides an inability to profit from observation which must vitiate to a great extent the value of the observations which his wide field experience has enabled him to make.

I have before spoken of Roosevelt's dogmatism. Akin to that is his propensity for loose statements such as that on page 184, where he says that the two forms of the red fox, the typical red and the cross fox, are "equally successful in life." If equally successful, why is not the cross fox as common as the red? Since

[1] A little observation will convince most persons that the light on such a night comes from the sky as a whole rather than from the individual stars and planets.

it is not so common, how can he prove it is as successful? He speaks so frequently of one animal being as successful in life as another that one comes to believe that he thinks all animals are equally successful in life!

On page 202, Roosevelt says that Thayer "states that a crouching hare is 'boldly conspicuous' when seen from the position of any 'quadruped pursuer' that would have to look upwards at the hare's tail," and then he goes on to ridicule Thayer and talk about 'preposterous theories' and 'wild absurdities' and use other language which might be appropriate (though impolite) *if Thayer had said anything of the kind.* In reality, however, Roosevelt has made a flagrant misquotation. What Thayer actually said was that the crouching hare was "boldly conspicuous when seen from the position of a mouse or cricket." [1] Now I do not suppose that Roosevelt would really call a mouse, or even a cricket, a 'quadruped pursuer' of the hare! It is obvious that his eagerness to punch Mr. Thayer has led him into a grossly careless misreading of him.

Another ill-considered statement occurs on page 218, where he says, "Birds and mammals living under precisely the same conditions have totally different types of coloration, and display totally different traits and habits when seeking to escape from enemies or to capture prey." Of course, a very little reflection would have shown him that no two species ever live under precisely the same conditions. The very fact of their having different habits in seeking to escape enemies or to capture prey constitutes a difference in the conditions of their lives.

But the most serious of all the misreadings of Thayer that we find in Roosevelt's paper has to do with counter-shading. He entirely overlooks the fact that Thayer's claims for the efficacy of counter-shading concern only those natural backgrounds which the animal resembles in *color*, or, to quote from page 15 of Thayer's book, it is "when seen against a background of color and pattern like its own" that the counter-shaded animal "will be essentially indistinguishable at a short distance." Overlooking this, Roosevelt says, on page 136, "Mr. Thayer insists that the animal escapes observation, not because its colors match its surroundings, or

[1] *Concealing-Coloration in the Animal Kingdom*, motto to Fig. 103, opp. p. 150.

because it sits motionless like a stump, or clod, or some such inanimate thing, but purely because of its shading, which he says is rendered obliterative by the counter-gradation of shades." Then, after adducing considerable evidence as to brown rabbits on green lawns, etc., he says (of woodchucks and pikas) that he never found any difficulty in seeing either when he "could get it on an entirely smooth surface of rock or ground, unless the color of the surface happened to agree absolutely with the color of the coat." Of course not; no one could be expected to, and Mr. Thayer would be the last man to suggest it. Again, on page 189, he tilts at the same windmill. In fact, much of his elaborate argument against counter-shading falls to the ground when we see that it is founded on a misconception.

This matter of color-gradation Roosevelt completely fails to grasp, and his apparent stupidity about it is really amazing. On page 137 he considers it a point against the efficacy of counter-shading that it does not show in a rabbit sitting stern on and that nevertheless the rabbit is no easier to make out in that position than if sideways to the observer. Now of course counter-shading is of avail only where a shadow is cast and in all other positions it is not needed. He falls into the same blunder on page 158 where he says that the female Bob-white on her nest is concealingly colored in spite of not being counter-shaded in that position. Similarly in another place he adduces the absence of counter-shading on the body of a swimming duck; and in his Appendix on protective coloration in *African Game Trails* he remarks on the difficulty that some animal or animals had in making him out as he stood in the forest, even though he was not counter-shaded; though it is hard to see how a man standing upright, or any other upright object with practically no under side except what rested actually on the ground, could possibly be counter-shaded. Does he imagine he might have been still more invisible if he had worn white trousers?

But I think I have said enough to show that Colonel Roosevelt's methods of thought are such that we cannot place implicit confidence in the accuracy of his observation or the soundness of his judgments. If Thayer has been carried away by his enthusiasm, Roosevelt has been carried just as far in the opposite direction

by his prejudices. It is evident, I think, that we cannot accept Roosevelt's conclusions without careful examination, and equally evident that Thayer's testimony, expert though it is, must be weighed in the judicial balance before we can know how much it is worth. And it is for scientific men to make the examination and do the weighing, not for artists nor for hunters. I do not, of course, propose to attempt to settle the question myself. I am simply going to try to draw the lines a little more clearly,— to whistle up a breeze that shall blow away some of the dust and smoke that have settled down over the field, so that the issue may be seen more distinctly.

In the first place, then, what is Mr. Thayer's main contention? It is stated in the Introduction to his book as follows: "The colors, patterns, and appendages of animals are the most perfect imaginable effacers under the very circumstances wherein such effacement would most serve the wearer. For any animal to be seen looking conspicuous means no more than that he is not at those moments looked at under the circumstances for which his concealing-colors are effective."[1] Obviously it is impossible to prove this general statement in particular. No one knows enough of the conditions of the lives of all animals to do so. I think Mr. Thayer is justified, however, in his contention that *if* his conclusions are found to be reasonable in an overwhelming number of cases, it is only fair to give him the benefit of the doubt in those remaining cases where proof is more difficult or even impossible with our present knowledge. Of course, if it can be proved that these hypothetical exceptional cases are *not* cases of concealing coloration, that will settle the question so far as the existence of a universal law is concerned; but it is notoriously hard to prove a negative. I mean by this that Thayer is not called upon to explain the coloration of every single species of bird or mammal. *If* we find his theory true in the main, we can accept the doubtful cases on faith. That is what we have done with the evolutionary theory. It is not necessary to account for the development of every species by natural selection or any other means. We believe in the theory in spite of the inexplicable cases. It must be admitted, however,

[1] *Op. cit.*, p. 9.

that the theory of evolution stands on a somewhat different footing from that of concealing coloration, in that it is in its very nature a universal one, while to account for coloration there may be several theories which are not mutually exclusive. In fact, there seems to be no good *a-priori* reason for *seeking* a universal law for coloration. The old theories, of protective coloration, sexual selection, mimicry, warning colors, directive markings, etc., have long been considered satisfactory enough. If, however, there actually is such a universal law as Thayer asserts, we must come to accept it in the end. It remains to learn the facts.

I need not go into the various methods by which, according to Mr. Thayer, concealment is brought about,— counter-shading, background-picturing, ruptive and secant patterns, masking of eyes, bills and feet, iridescence, appendages, etc. It may be taken for granted that my readers are familiar with the main principles as enunciated by him. Let us turn at once to Colonel Roosevelt's conclusions and see just wherein they differ from Thayer's. In the first place, then, he refuses to accept the theory of natural selection as accounting for such concealing coloration as he admits to exist. He says it is *possible* that the tendency towards concealing coloration is the result of natural selection, but to his mind much more probable that the major part of the tendency is due to the effect of physical surroundings upon all the individuals of a species.[1] This theory has, of course, been held by many, but though moisture and dryness, heat and cold, and diet, all doubtless do have an effect in certain cases, as Mr. Beebe has proved, for instance, in his experiments with birds in captivity, it is in the main a vague and unsatisfactory theory, since it cannot show the method by which such changes are brought about. To most of us, I think, the theory of natural selection seems the most reasonable explanation of most of the facts of evolution. Whether or not Roosevelt's doubts about it are due to a difficulty in reconciling his ideas of advertising coloration with it, he does not state, and we can only conjecture.

Here, then, is one distinct issue,— a belief in natural selection, which, of course, Thayer believes to be universal and Roosevelt

[1] *Revealing and Concealing Coloration in Birds and Mammals*, p. 212 (1).

refuses to accept. Another, but less important, issue is the cognate one of sexual selection, which Roosevelt accepts, though guardedly, but Thayer rejects (implicitly if not explicitly) on the ground that it implies conspicuous coloration, whereas he holds that truly conspicuous coloration does not exist. We will revert to this subject later on.

The next important conclusion of Colonel Roosevelt is that "as regards the majority of birds and mammals the prime factors in securing their safety, are habit (including bodily capacity) if they do not trust to concealment, and habit and cover if they do trust to concealment."[1] "Among these birds and mammals," he says, "the coloration is always a minor, and often a negligible, factor." Now no one doubts the importance of habit, bodily capacity, and cover in protecting animals, but to me it seems an impossibility to settle just what factor is most important. As a matter of fact they are all interwoven, habit depending on coloration, capacity, and cover, coloration depending on cover and habit, etc., etc., so that it seems futile to think of one without the others. To discuss their relative importance would remind one of the discussions in the old-time debating societies, and one might as well argue the question whether the blood or the brain was the more necessary to the life and welfare of man. Of course, when it comes to stating, as Roosevelt does here, that coloration is often a *negligible* factor, that is properly a matter for observation and argument, and that is one of the points upon which Roosevelt's observations and arguments must be weighed.

Roosevelt's next important conclusion — and this, in fact, is the sum and substance of his whole paper so far as it concerns our birds — is that "a large majority, probably at least three fourths or over, of the birds of temperate North America, have coloration patterns which, either in whole or in part, either all the time in both sexes, or all the time in one sex, or some of the time in one sex, are advertising."[1] In support of this conclusion he brings an argument which may prove to be a strong one and well founded. At least it has a certain plausibility and will bear investigation. This is the suggestion that the miscellaneous character

[1] *Op. cit.*, p. 214 (5).

of forest cover is such as to make the detection of an animal of *any* color difficult so long as it remains motionless. (It will be observed that Roosevelt in this gives up the claim that animals under such conditions are actually conspicuous. He admits that they are not, but holds that their coloration has no significance under such circumstances and that therefore it could not have been brought about through natural selection.) One obvious answer is a statement of the well-known fact that the birds of the treetops run quite largely to the brilliant colors, while sea-birds, for instance, show an entirely different style of coloration, largely white and gray and black. As before stated, it is difficult to account satisfactorily for such differences without involving the theory of natural selection. Certainly sexual selection will not explain them, and without one or the other of these methods of selection we should have to fall back upon the vague and unsatisfactory theories of 'harmony in nature' which, if they have any definiteness at all, are really more metaphysical than scientific. But wholly aside from probabilities and theory, what are the facts in the case? Is it true that a bird of any color would be inconspicuous in the forest and that no coloration scheme can make any difference in its conspicuousness? It seems to me that the obvious way to settle this question is by experiment; observation under purely natural conditions being unavailable in this case. Distribute a number of bird-skins, forest birds and sea-birds, impartially in the treetops in some thick wood and see whether there actually is any difference in their conspicuousness or not. It ought not to be a difficult experiment. I am not aware that Mr. Thayer has ever tried it in any of his demonstrations. I hope he will, and I hope that others will.

This suggestion of Roosevelt's,— or rather this pronouncement, for of course he does not offer it as a mere suggestion, though we can accept it as such,— this suggestion that all colors, bright or dull, may be inconspicuous in a forest landscape is to my mind the most important point he makes. Except for this one point, indeed, it seems to me that the value of his paper depends almost entirely on whether we can accept his interpretation of his own

[1] *Op. cit.*, p. 214 (6).

observations. He states, without qualification, that such and such an animal is advertisingly colored. Well, perhaps it is, but the mere statement does not prove it. I have tried to show in an earlier part of this paper why we should be slow to accept all his statements without question, while giving him credit for wide experience and honesty of intention. Let us consider his methods of study as compared with Thayer's, and determine, if we can, which are the more trustworthy. Roosevelt's methods are those of pure observation in the field, with the animals under natural conditions: Thayer uses experiment in addition to observation. Now I should be the last man to depreciate observation. It is the particular form of scientific work that most appeals to my personal tastes. Laboratory methods in the study of living animals have their uses, however, and are more and more being used. By these methods only can we control conditions, so as to isolate the particular class of facts that we are investigating. Rightly safeguarded, this mode of research is invaluable. And it seems to me that in investigating concealing coloration we cannot get along without it, for the simple reason that it is impossible to *observe* anything that is concealed from the eye. When an animal is showing to the best advantage the concealing power of its coloration, that is the very time when we do not see it. I suppose, therefore, that the times when the observer can see this principle in operation in the field are so infrequent, comparatively speaking, that one may get a wholly wrong impression as to the relative conspicuousness of an animal from mere observation and memory. The number of times when a bird, for instance, just fails to elude us ought to be multiplied by a substantial figure in order to include all those individuals which actually do elude us. Of course, no bird of any color can blend into its background all the time. All birds of potentially concealing coloration must sometimes, often in fact, be seen against backgrounds that reveal them. And it is on just these occasions that the observer is most likely to see them. Moreover, the birds that *are* seen against a background that they match, detected by following their flight perhaps, hold the eye as long as the observer watches them and so tend to be regarded as conspicuous. In other words, as Thayer has pointed out, it is the animals that are seen that make their

impression upon us, not the perhaps larger number of individuals of the same species that come within sight of us when we are in their haunts but of which we have no knowledge. Experiments like Thayer's, conducted privately or in conjunction with other persons, are, it seems to me, the most profitable mode of study of the concealing power of coloration, for only by some such means can these inherent difficulties be avoided.

I am aware that there is some distrust of Mr. Thayer's methods of experiment and demonstration, on the ground that in some cases he has not reproduced accurately the natural surroundings of the animals experimented with. It seems to me that such failure to duplicate natural conditions need not be counted against the method itself. Of course, experiments are of little value if we do not know just what the environment of an animal is, but if we do know it and can reproduce it approximately in our home landscape, the method is a perfectly legitimate one. The fact that Mr. Thayer may have been mistaken in regard to the habitat of the Peacock does not vitiate all his experiments, but he and any one else who conducts experiments along this line must, of course, take pains to copy natural conditions faithfully. And we must not leave all the experimenting to Mr. Thayer.

There are other tendencies of the human mind that must be guarded against in prosecuting our studies. One of these I suspect ornithologists are especially subject to. That is the tendency to see what we *know* is there rather than what actually appears to the eye. We are probably more subject to it than most persons because we deal so constantly with what I may call *absolute* color, — color that is such by virtue of pigmentation and structure, not color as seen out of doors in varying lights and subject to the influence of countless neighboring colors. We see a bird's under parts as white, sometimes doubtless because we *know* they are white, and sometimes by the eye's unconsciously making allowance for the effect of shade. In the case of an unrecognized bird observed in the field, we take the greatest pains to get it in the best possible light, and we are constantly translating its apparent colors into terms of the absolute colors that we know or suspect them to represent. That is the only way that we can identify an unfamiliar bird,— without having it in the hand, where no

such translation is necessary. But that is not the way to pursue studies in concealing coloration. We must cultivate the artist's power of seeing only what appears on the surface, if we would see things as the wild creatures see them.

Another tendency that must be guarded against is that of confusing brightness of color with conspicuousness. It is natural, of course, to suppose that a brightly colored bird would be conspicuous, but brightness is by no means synonymous with conspicuousness. There is danger of regarding a bird as conspicuous simply because its colors are unusual and pleasing to the human eye. The real question is whether the bird itself is particularly easily seen in the landscape, and recognized as a bird, by a creature which is merely looking for *any* bird to eat and is not concerned either to wonder at and admire its beauty or to identify it as belonging to a particular species. Of this confusion between unusualness and conspicuousness I have already noted a flagrant case in connection with the Scissor-tailed Flycatcher, whose tail Roosevelt called conspicuous *in shape.* Another very common instance is that of the Blue Jay in a snowy landscape. This bird never looks more beautiful, I think, than against a background of snow, which sets off his blue plumage to perfection. Deceived by his beauty, we are prone to call him a very conspicuous bird, but careful observation will convince any one that he is really not a bit more conspicuous on an even expanse of unshaded or wholly shaded snow than any other dark-colored bird of about the same size, a Robin for instance, while, seen among tree-shadows on the snow, he is actually inconspicuous, as I have noticed on several occasions. The blue of his plumage when lighted by the sun matches the blue of the shadows almost exactly. This is one of the points upon which Mr. Thayer has been most sharply attacked, but any one can prove to himself the relative inconspicuousness of the Blue Jay against snow in the woods if he will try a few experiments and do a little real observation. Take notice, however, that I am merely stating a fact, not drawing any inferences. Whether the fact that the Blue Jay is concealingly colored for winter in a wooded region where snow abounds has any particular significance — whether the coloring has any real protective function other than a purely incidental one — is another matter entirely, and one which I shall not go into at present.

Another element of confusion comes in when we consider recognition-marks or identification-marks. Mr. John T. Nichols has not avoided this confusion of ideas in his discussion of certain recognition-marks in the January 'Auk'. The white stripe down the wing of the Spotted Sandpiper in flight is an excellent field-mark for identification purposes, but I very much doubt if the bird is rendered one whit more conspicuous by it, as a mere bird. Identification-marks have no necessary connection with conspicuousness. In fact, any distinctive marking on a bird may serve as an identification-mark to the ornithologist, and it doubtless may also serve as a recognition-mark for other individuals of the species. The particular class of markings which have come to be called recognition-marks are those which, in the case of birds, are shown prominently in flight, generally on the wings, tail, or rump. They doubtless serve as a means of identifying birds to others of the same species when seen at some distance, the more striking markings operating at the greater distances. But this, though Thayer himself does not appear to have perceived it, does not militate in the slightest against his idea that white markings on wings, rumps, and tails are really concealing rather than revealing in their effect. When I advanced this opinion in conversation with a good friend of mine, he accused me of holding that a bird could be at the same time revealingly colored to its friends and concealingly colored to its enemies. This is not at all what I am saying, however. My point is that these markings are not revealing to either friend or enemy; that is, they do not reveal the bird, they simply identify the species. This, it will be seen, is of little or no importance to the enemy, which is not concerned to know whether the prey it is pursuing belongs to this or that species, but it is of very vital importance to the species that individuals should easily find one another and keep together.

Here, then, are four special tendencies to error to be guarded against in the study of concealing coloration: the tendency to regard any animal actually seen as conspicuous and to take no account of those individuals which escape observation, the tendency to see things as we know or suspect them to be rather than as they really look to us, the tendency to confuse brightness of color with conspicuousness, and the tendency to regard recognition-

marks as of necessity advertising in function. Mr. Thayer, himself, as I have pointed out, in rejecting the theory of recognition-marks fails to see that it is by no means incompatible with his own ideas. There are other cases where it seems to me that his views and those of his opponents are not irreconcilable. One of these is that of a particular class of recognition-marks which under certain conditions are probably advertising and which, indeed, seem to owe their usefulness as recognition-marks to their revealing power. These are the so-called banner-marks of deer, antelopes, rabbits, etc.

Thayer's treatment of these white stern-patches I have thought to be the weakest link in his chain of evidence and argument. His claim that the deer's white flag, for instance, is actually a concealing mark is one of the hardest for us to admit of all the claims he has made. The banner-mark theory, for one thing, was such a neat and satisfactory one and seemed so thoroughly to 'fill the bill' that we dislike to give it up. Any fair-minded and unprejudiced person, however, who will take the trouble to try experiments, or who can see the logic of the situation, must admit that, seen against the sky, in the long run the white must prove to be concealing rather than revealing. The difficulty lies in convincing ourselves that the flag would actually be seen against the sky most of the time rather than against foliage or a hillside. I must confess that I have not yet tried enough experiments to assure myself on this point. What little I have done goes to indicate that Thayer may be right. My idea would be to take from time to time in the woods the point of view of panther or wolf and see whether the interstices in the foliage at the height of a deer's tail at varying distances were numerous enough to spot the landscape pretty thoroughly with glimpses of the sky. It must be borne in mind that it is not necessary to concealment that the white flag should actually relieve against the sky; if it appears among a number of scattered sky spots so that it does not attract attention to itself to the exclusion of other things in the neighborhood, that is quite sufficient. I recommend that this experiment be tried before we indulge in any more ridicule of Mr. Thayer's lack of 'common sense' in this matter.

But if our experiments prove that Thayer is right as to the con-

cealing properties of the deer's white flag, it may still be that this concealment is only incidental, for I suspect that in the deer's case, as in the case of the Spotted Sandpipers before referred to, the white markings may still serve the purpose of directing the deer's companions. The deer's head is carried above the level of his tail or perhaps on a level with the uplifted tail, so that normally the flag would appear to another deer against a very different background from that which the panther or wolf would see it against; that is, of course, when the deer in front is not bounding high in the air, but the flag is thrown up first while the deer is still on the ground and may even be carried that way at a slow canter, and then, too, the following deer is also bounding and so could often catch the white gleam from the deer in front as its tail relieved against the foliage or the ground. I merely throw this out as a suggestion of a possible reconciliation of the theory of directive markings with that of the inherent concealing power of all markings. There is much to be learned about these things, and common sense *plus* experiment and thought will be a safer tutor than unaided common sense. I will add that the last time I saw the deer throw up their white flags, the white, being seen against an evenly clouded sky, was inconspicuous. Of course, it was visible, because this was in the daylight and the deer were in plain sight, but it detracted from, rather than added to, the conspicuousness of the deer.

Sexual selection is another theory that seems to me not at all incompatible with Thayer's main contention. Bright colors, as he shows, or endeavors to show, are not necessarily revealing, but that is not saying that they may not be attractive to the opposite sex. It seems to me that many of the bright and beautiful colors and markings in the plumage of birds may be produced by sexual selection but afterwards acted upon by natural selection. Sexual selection, that is, may supply a short cut to the production of concealing colors when they happen to be bright ones; or, to put it in another way, natural selection may set bounds (I borrow the expression from Roosevelt) to the colorations produced by sexual selection. It seems to me that Mr. Thayer is unnecessarily shy of sexual selection. I cannot see that it tells against his theories at all, and it is a reasonable explanation of the primary, or perhaps I should say the secondary, cause of many forms of coloration.

In the case of those species which make evident displays of brightly colored parts, it is particularly indicated. But colors and markings that are so made use of in the close quarters of courtship are not necessarily conspicuous to enemies or prey at a distance or which if in close proximity are not already aware of the presence of their wearers.

But I shall go no farther at present into this subject of concealing coloration. This paper is not intended as a complete review of Mr. Thayer's book nor even of Colonel Roosevelt's paper. I simply have not been contented to let things stand as Barbour, Phillips, and Roosevelt have left them, because I believe that, in their anxiety lest Thayer's 'heresies' should be too widely accepted, they have failed to do justice to his work. I think it must be admitted, even by those of us who are most appreciative of Mr. Thayer's work, that he has not yet *proved* his main contention. That is, he has not convinced us beyond a reasonable doubt that all coloration of animals has a concealing function and owes its existence to natural selection. I will not say that this is insusceptible of proof, but other ways of accounting for certain colorations seem fairly satisfactory as yet. Nevertheless, Mr. Thayer has shown us several things that we had not seen before, most important of which, doubtless, is the use of counter-shading, though hardly less so is the possibility of bright colors as well as neutral tints being actually concealing in effect. I think that no one can read his book carefully and study his pictures or witness his experiments and demonstrations, no one can experiment for himself out of doors in a leafy and sunlit landscape, without becoming convinced that nature is full of brilliant colors that can be matched only by correspondingly bright hues in the birds. Mr. Thayer's book comes very near being a work of genius, and I submit that scientific men can ill afford to treat it lightly.

When the foregoing paper was read (in a somewhat different form) at a meeting of the Nuttall Ornithological Club, March 18, 1912, one of the members, a leading ornithologist and a Fellow of the A. O. U., stated that he doubted the necessity of protective coloration; that he considered the wariness and intelligence of

animals the only necessary factor in their preservation. My answer to that was that it seemed to me there must be some significance in the fact that there existed in so many undisputed cases so evident a concealing power in coloration. Would this concealment exist if it conferred no benefit upon its possessors? To me it seems only reasonable to assume that concealing coloration has a real reason for being. This is not saying, of course, that all animals are protectively colored. It may be, as is generally believed, that many animals do not need such protection, but the more one studies Mr. Thayer's discoveries in this field of concealing coloration, the more one comes to suspect its approximate universality, and the readier one is to believe that the concealment thus brought about has a protective value that is of benefit to the wearer. As to the relative value of coloration on the one hand and wariness and intelligence on the other, it seems to me, as I have already stated, that the various factors are so interdependent that it is impossible to say which is of the most use.

Another interesting objection to one of Mr. Thayer's theories was brought to light by the testimony of two members as to the unerring aim with which foxes and dogs sprang upon prey that they had located only by scent. This may militate seriously against Mr. Thayer's contention that the final spring, even in the case of animals that habitually hunt by scent, is directed by sight alone. More observation is necessary to settle this interesting point. A failure to establish Mr. Thayer's claim here would go far to weaken his position on the banner-mark question.