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NORTHEASTERN BIRD-BANDING ASSOCIATION

THE SCIENTIFIC ASPECTS OF BIRD-BANDING

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In accepting the Editor's invitation to contribute an article to the columns of the *Bulletin*, I feel that I am taking a grave risk in my choice of subject matter, which necessitates the adoption of a critical attitude. But when one considers that thousands of acres of paper and uncountable gallons of good printer's ink are wasted annually, in these days of endless publication, without seeming to accomplish anything in particular beyond, perhaps, getting authors' names into print, one is induced to attempt to put something that will at least stimulate thought into an article written by request. Since my interest in banding is mainly biological, it occurred to me that a certain number of banders might be interested to know what biologists—the people who are reputed to spend their time in laboratories with one eye down a microscope and the other on a musty old tome in technical "jargon"—think of their activities. I readily concede that I am not qualified to speak for biologists in general and that I am, rather, voicing a personal opinion, but it is nevertheless an opinion tarred with the brush of the microscope and the musty old tome. Still, neither my fellow biologists—nor an innocent editor—are to be blamed for my sentiments.

The link that brings banders and laboratory men into touch with each other is the ever fascinating problem of bird migration. It has intrigued the professional and the lay mind alike for many centuries. Its literature is full, not only of the names of innumerable amateurs who can lay no claim to scientific training but also of names famous throughout the world in the fields of philosophy and pure science. Neither camp has successfully solved the real riddles of migration. By this I mean that we have no idea yet either *why* birds migrate nor *how* they do so, and these are the crucial points about migration. We only know that they *do* migrate, that their migrations are regular, that they appear to bear constant relationship to the seasons and the poles, and that certain birds seem to take certain routes.

There must be an explanation for this state of affairs.

Largely, I fancy, it lies in the fact that the migration of birds is primarily a biological problem rather than an ornithological one. Not only is it an example of animal behavior but one of extreme complexity, and as such it can never be solved by the layman, no matter how good his intentions or how conversant he may be with the field facts. As long as he lacks the fundamental knowledge of the mechanism of living organisms—anatomy, physiology, genetics, etc.—he cannot even begin to speculate intelligently on the theoretical aspects of migration. But this argument, of course, can be applied in two directions, and it is just as true to say that no matter how able or well-informed in biological theory a scientist may be, unless he is familiar with the field aspects of migration the subject is to him a closed book also. To my mind it is this unfortunate separation of the two vital interests—a separation that is almost universal—that accounts for the migration of birds remaining the mystery that it does in spite of the attempts of hundreds of good brains to solve it. In this expression of opinion, at all events, I am not alone. In that excellent volume, "Problems of Bird-Migration," by Dr. A. Landsborough Thomson, (incidentally, let me recommend every serious bander to read it) the author suggests again and again that he is addressing two audiences, the layman and the scientist. For the former he constantly points out the biological implications that underlie the facts of migration. For the latter he spreads out a panorama of established fact. The author is not only an able scientist but also a well-informed ornithologist and an active bander.

It may sound ungracious of the biologist to 'refer' to the average ornithologist as an amateur and a non-scientist but as a generalization it appears to me to be justified. The vast majority of ornithologists take up their study as a hobby. It may be confined to collecting eggs, to photographing, to banding, to watching, or even to "hair-splitting" in the museum. It may embrace several or all of these. None of them necessarily implies science or even the scientific outlook. Even the last, which is widely considered to be the pinnacle of "scientific ornithology," may be no more than the rankest of amateur effort. It requires no scientific knowledge whatever to ascertain that one skin differs from another in minute measurement or in detail of color, nor does it further science in any respect to celebrate the discovery by affixing a new name thereto.

Now the determination and recognition of geographical races is a matter of considerable importance to ornithology,

but such recognition should express definite relationships. To do so it should be based on recognized principles. If carried out with these at the back of it and with method instilled into it, it not only achieves something useful for ornithology but contributes to the far wider fields of biology which seek to determine the ultimate relationships of all living organisms. In such case the recognition and naming of differences becomes both an intelligent and a profitable procedure. But, as already stated, there is nothing inherently scientific in naming differences in skins. It depends on how the thing is done.

Again, although they themselves will rarely admit it, yet it is a fact well known to their friends, that a large percentage of bird-photographers who call themselves ornithologists can lay no honest claim to such title, for they are really photographers getting pictures of a particular kind, to wit, of birds. Their knowledge of ornithology may be nil: their knowledge of photography possibly profound. Yet they persist in styling themselves ornithologists and considering themselves scientists on the strength of it. Yet again, we read appeals here and there from banding "leaders" for more banders and more banders, anybody who is willing to band. No stipulation is made that they should first be ornithologists and secondly banders. As the invitation stands it simply means that the criticism of a well-known bander when he deplores the existence of "picnic banding" is only too true and very apt. Picnic banding will advance neither science nor ornithology. It merely vitiates the returns of bird-banding generally and casts a slur on the conscientious and valuable labors of innumerable qualified workers. Still again, the amassing of eggs and arranging them nicely and neatly in cabinets serves no better purpose than does the collecting of stamps or book-plates or china saucers, if that is all it amounts to, and that is, in fact, all it does amount to in a very large number of cases. On the other hand, such collecting, if done in another spirit and on a broader basis, may contribute richly to ornithology and ultimately to scientific ornithology. Apart from information to be derived from the specimens themselves, the egg-collector has unrivalled opportunities of studying the nesting-behavior of birds.

A chain is no stronger than its weakest link, and the first thought of a trained mechanic in need of a chain would be to look for such a link. Scientific argument, if it is to be worth anything at all, must be based on unassailable facts. Darwin's theory of natural selection, in spite of the critical probing

and analysing of three subsequent generations of scientists still remains the cornerstone of biological thought simply because its author assembled, for the exercise of his analytical genius, a chain of facts that was both comprehensive and fundamentally sound. The scientist to-day who sees migration in its true perspective and recognizes therein a series of wonderful adaptations and one of the most remarkable examples of animal behavior known among vertebrates sees also therein the possibility of unearthing hidden biological principles that may prove crucial and perhaps reach far beyond the immediate field of bird-migration. The unknown has ever intrigued the mind of man and particularly that of the scientist. Here is a problem that in its complexity and vastness must prove a very particular draw, and yet the scientist in general refrains from approaching it. Why? It seems to me that the answer lies in the fact that he is quite unable, from an enormous mass of ornithological literature, to sort out the chaff from the wheat and to put together that chain of facts so essential as a starting-point, that shall be without any weak links. He finds a wealth of good material drowned in a sea of wasted printer's ink.

I trust I am not misunderstood in the above remarks. I am not for a moment suggesting that the amateur is an impediment to scientific progress. The reverse is actually the case. Even though he be entirely innocent of the most elementary schooling in science he may yet be a highly important contributor to it. I can think of no simpler example than the man living in some isolated region who undertakes to keep meteorological records. He may be completely ignorant of weather phenomena or the significance of the very records that he is keeping, yet they may prove to be of vital importance to the deductions of some central bureau. But whether those records are going to be of value or not depends on his methods. If he only gets out of bed to read his gauges when he feels like it and at other times leaves them alone, his efforts will prove entirely worthless. If, on the other hand, he attends to his instruments in the precise and conscientious manner required of science, he is contributing to science even though he be anything in the world but a scientist. Perhaps that is the most serious weakness of the average amateur, that he does not grasp the fundamental importance of precision and accuracy. And lack of it may convert something that might be perfectly good into something that is absolutely useless. Moreover the present age, with its numberless journals and unlimited facilities for publication, seems to

have proved a veritable death-trap, to the scientist as well as the amateur. It is fashionable at the moment to publish. People speak with pride, not of the work they have accomplished,—all too often it is casual and shoddy and not worth talking about,—but of the mere fact that they have got it, and incidentally their names, into print. As though that were an accomplishment of note when dozens of editors are hunting for "copy". Ornithological journals suffer as do all others.

The lack of precision that characterizes so much amateur work is reflected here and there in much of the banding literature. One frequently sees banding, for instance, referred to as an experimental method. Yet this it is not. The placing of a band merely marks a bird. It interferes in no way with its business of life. A return is entirely dependent on chance. Five minutes after a band has been placed on the leg of a sparrow the carrier may be wiped out of existence by a hawk. Or five years later it may walk into the very trap that made possible the placing of the band in the first instance. Experiments, on the other hand, are always carried out under control. That is one of the essential merits of the experimental method.

Lists designed to assist the innocent bander in his work and to suggest profitable lines of "research" appear quite frequently. And yet, when one examines them critically, one finds that a good many of them are largely based on the assumption that all banders are trained biologists and that by the mere ringing of birds they can solve problems that Darwin himself, were he alive today, could not solve in the present state of biological knowledge. Banding can do much for ornithology but it cannot work miracles.

I cite these two items because they are probably familiar to everybody and they serve excellently as examples of inaccuracy of *thought*, just as pernicious as inaccuracy of record or statement.

The possibilities of banding as an adjunct to scientific ornithology strike me as being almost unlimited. At all events I do not believe that its limits have as yet been demonstrated. And yet it really does nothing more than associate some particular bird with two or more precisely known spots on the map in conjunction with specific dates. This is not in itself an overwhelming amount of information. But its definite and precise character gives it immediate scientific value. If any factors are introduced that may contaminate that accuracy, the system loses all its merits. Hence this "come-on-and-band" spirit addressed to the universe at large

cannot be too severely deprecated. "Picnic banding" is not only useless in itself, but it casts doubts on sound banding. A "picnic bander" may, for instance, trap a Lapland Longspur, look through some of the illustrated books to salve his conscience, put a band on it, liberate it, and send in a return for a Harris's Sparrow. Even one such error may have far-reaching effects. In the event of the band being returned sooner or later it may, since the ranges of these two species overlap at certain seasons, fail to arouse suspicion at the time. But it puts a wrong record on the map. As Harris's Sparrow returns get more abundant, some critical mind will sooner or later spot a "fishy" record. What is the effect? The possibility of many similar mistakes having been made will immediately suggest itself, and all Harris's Sparrow records may have to be scrapped or will at least fall into disrepute. But worse still, there will be no tangible evidence to incriminate the offender. All people who have banded Harris's Sparrows will innocently become suspects. Their other records will be looked at askance. Is the picnic game worth the candle? Most emphatically, *no*. The vital point in banding is its accuracy. It should be held sacred by hook or by crook. This continent, in its universal tongue, in its enormous size, and in the fact that many extensive migrants never leave it, enjoys so many advantages (over the European countries, for example) and has such assurance of generous returns, that there is not the least need, and *in any case no excuse whatever*, for seeking to inflate returns by inviting the coöperation of the casual performer who wishes only to amuse himself.

Accuracy in identification may, I think, be placed as the primary requisite in all banding activities. And precision of record would appear to come next. By this I do not mean solely that there should be no error in the date and locality. That much goes without saying. It is often possible, as numerous banders have shown, to add to such minimal data further information, equally precise. Sex, state of plumage, and age, where these can be definitely determined, are obvious examples. Such information may quite unexpectedly bring out a fact previously unsuspected. To illustrate what I mean, I might cite the following case. In my recent attempts to apply the experimental method to the solution of certain phases of migration, I have been using chiefly juncos. To save time—a commodity of which I am always short—I trap my birds only during the height of the passage through central Alberta. I can usually secure all I want in ten days. As far as my work was concerned, the exact day of capture

was apparently of no significance. It seemed sufficient to know that the birds had been taken in active southward migration at the regular season. But as a matter of principle the *exact* date of each capture was recorded nevertheless. Most of my birds are eventually sexed in the laboratory, where portions of their "insides" are preserved and fixed in various ways for microscopical examination. The sexing of the majority is therefore accurate. My liberated birds have, of course, not been so sexed, and since juvenile males cannot always be told from females, the number of females shown may be slightly inflated. But the error is not large enough to effect the general fact brought out by tabulating the sexes separately according to dates as has been done below.

JUNCO TRAPPING RECORD

1926							1927					
Female			*Male*				*Female*			*Male*		
X			X			...September 21st.....	X	X	X	X	X	X
							X	X	X			
							X	X				
X	X	X	X	X	X	...September 22nd....	X	X		X	X	X
					X					X	X	X
X	X	X				...September 23rd....	X					X
X	X											
X				X	X	...September 24th....	X					X
						...September 25th....	X	X	X	X	X	X
							X	X	X	X	X	X
X	X	X		X	X	...September 26th....	X	X	X	X	X	X
X							X	X	X			X
							X					
X	X	X	X	X	X	...September 27th....	X	X	X	X	X	X
X	X		X	X	X					X	X	X
			X	X	X						X	X
X	X	X	X	X	X	...September 28th....	X	X			X	X
X	X		X	X	X							
X	X	X		X	X	...September 29th....						
X												
X	X		X	X	X	...September 30th....	X			X	X	X
			X	X	X							X
			X	X	X							
					X							
X	X	X	X	X	X	...October 1st.....	X			X	X	X
X	X	X	X	X	X							X
X	X	X	X	X	X							
			X	X	X							
				X	X							
X	X	X	X	X	X	...October 2nd.....						
X	X	X	X	X	X							
X	X		X	X	X							
				X	X							

X	X	X		X	X	X	...October 3rd.....			X	X			X	X	X	
				X	X	X											
				X	X	X											
				X	X	X											
				X	X	X											
				X	X	X											
X				X	X	X	...October 4th.....			X				X	X	X	
				X	X	X										X	
					X	X											
X				X	X	X	...October 5th.....			X				X	X	X	
				X	X	X								X	X	X	
				X	X	X									X	X	
				X	X	X											
					X	X											
				X	X	X	...October 6th.....			X				X	X	X	
				X	X	X	...October 7th.....										
				X	X	X											

From this table, taken in conjunction with the fact that the passage continues till the beginning of November, it is evident that the sexes travel separately in the fall, the females preceding the males, exactly the reverse to what obtains in the spring. This in not merely an interesting feature but, from my point of view, one that has unexpectedly elucidated a snag in my experimental results.

Banding is essentially a method of collecting facts. And these vary greatly in value. The fact that a bird ringed as a juvenile is taken six hundred miles away two *weeks* later is of greater significance than that such a bird is taken six hundred miles from the point of banding two *years* later. Moreover, the correct interpretation of an isolated banding fact must almost always be a matter of time. If a breeding duck has been banded in northern Alberta and is recovered during the winter in California, that fact does not attain its full significance until more records like it have shown that it was not accidental. Moreover, not until many intermediate records have been procured and the route of the species has been indicated, probably a matter of many years, has the original record reached finality. It is easy to speculate and guess how the bird came to be in California, but it is one of the particular assets of banding that it will eventually make guessing unnecessary. It establishes facts.

But facts can be more speedily procured if an organized attempt is made to get them. And the extent of success further depends largely on coöperation. The bander who puts out the occasional trap or makes the odd raid on a colony of nestlings is, of course, contributing to returns. But, owing to the fact pointed out above, it may be years before

such returns assume definite significance. If, on the other hand, a bander with limited time and opportunity makes up his mind to band, let us say, on a selected area for the same two weeks for three years in succession, his results, for several reasons, have more importance than if he spent the same amount of time (or even more) in different places at different times of the year. Not only has he three sets of facts that are strictly comparable, but mere repetition has the value of corroboration. And, as many workers have now demonstrated, the chances of repeats, at least in certain species, are very good under such conditions. For reasons such as these methodical banding is greatly superior to casual banding, and wherever possible an attempt should be made to introduce system. One merely has to point to the results obtained at such an organized station as Mr. Prentiss Baldwin's to clinch the argument.

Properly organized stations that are able to carry on continuous trapping really have certain fields peculiarly their own. Such observations as those on the progress of moult and color-changes of soft parts in the individual that keeps on repeating; the exactitude with which certain species follow an identical route year after year; the duration of the sedentary period of both winter and summer; the precise return of some birds to the same breeding or the same wintering grounds; such questions as these all come particularly into the province of the single station, and they include some of the most interesting of the many desirable facts that can be obtained by banding.

For work of wider import coöperation is almost essential. If the work of European stations is compared with activities on this continent it will be noted that much more complete information on the movements of certain species is available on the other side. A hunt for the reason—apart from the time factor which is in favor of the older European stations—leads one to notice that there has been coöperation between widely separated stations and that they have frequently worked together in an onslaught on selected species. Such coöperation is particularly commendable because a complete understanding of the movements of two or three species is of greater theoretical value than a slight knowledge of the movements of scores.

There is a type of coöperation that has, as far as I know, never been tried but that should eventually be possible, particularly on this continent and that should prove exceptionally remunerative. There are many stations situated on breeding

grounds and many on wintering-grounds of migratory species. Probably already there are cases of birds banded on a northern station during the breeding-season that have been taken on their wintering-grounds by some southern station. If such a link exists it could be exploited with the most extraordinary advantage, for it would connect both ends of a complete migration-range. The possibilities of organized coöperation in such a case would be beyond compute. There are countless much wanted facts to the procuring of which such a form of attack would particularly lend itself.

If I seem unduly to stress the collecting of facts it is merely because this is the essence of banding and is the bander's primary objective. In the majority of cases it is his only one. Only a few are qualified to put an interpretation on the facts. And this is not in any sense a slur on banders. A reason has already been offered. Without a sort of bird's-eye conception of the possible underlying biological principles a profitable conjecture on the theoretical value of the facts is the most unlikely thing in the world. Let me offer a simple illustration of what I mean. Included in modern ornithological literature is a vast collection of articles and pamphlets to some of which very famous ornithological names are appended. Any layman reading these would be permanently convinced that agriculture is wholly dependent on the activities of birds for its very existence. Such exceptional countries as Italy, where birds are scarce enough to throw serious doubt on the local application of this popular belief, are discreetly ignored. And yet it can be logically and far more convincingly shown that if birds were completely absent from an agricultural country the insect parasites of insects, through themselves remaining unmolested by birds, would keep harmful insects under better control than they are kept today by the parasites enjoying the assistance of vast hosts of "beneficial" birds. For an excellent presentation of this interesting case let me commend to my readers the article "Can Birds Hold Injurious Insects in Check?" by Professor E. H. Strickland (*Scientific Monthly*, Jan., 1928, pp. 48-56). There are no doubt hundreds of ornithologists to whom this viewpoint, undoubtedly the correct one, has never occurred. Yet in biological literature, written by people who are generally assumed to know nothing of the field aspects of entomology or ornithology but nevertheless possess that biological knowledge that gives them the advantage of the "bird's-eye" view, indispensable to a correct perspective of the problems of life and living organisms, one occasionally sees such remarks as this: "But

probably far more caterpillars are eaten from inside by parasitic insects than from outside by birds" (Haldane, *Possible Worlds*). Here is the correct interpretation of the facts in a nutshell; a comment *en passant*, in the writings of a *biochemist*.

What is perhaps the most nearly sound theory extant on the mechanism of annual migration was propounded some twenty years ago by a world-famous physiologist, Sir E. Sharpey-Schaefer (*Nature*, Vol. 77, pp. 159-163). Had he been an ornithologist in his leisure time and been more conversant with the field facts, he might well have solved the problem completely.

So far I have purposely persisted in speaking of the amateur who is without biological knowledge, for he undoubtedly represents the vast majority in the ranks of popular ornithology and bird-banding. But there is the other who has also had no professional training but who has educated himself by wide and judicious reading. Amateurs of this class have made momentous contributions to science. It is only necessary to mention such names as the priest Mendel's, whose experiments in heredity are still the basis of an enormous output of professional effort, or W. F. Denning's, probably the world's greatest authority on meteors, or Faraday's, a bookbinder by profession, to point the argument. There are, in fact, no limits to the attainments of a talented amateur of this class. It is, perhaps, such as these in the ranks of ornithology who will ultimately show us the full biological import of the migration of birds.

If this article has the appearance of being combative, it has been written in no such spirit. Perhaps my arguments are actually full of flaws. As I suggested at the outset, even biologists can let their eyes wander from the microscope and become temporarily human and it is human to err. But as some one who is attempting to apply the methods of experimental zoölogy to the very complex problem of bird-migration and who may thereby claim a more than casual interest in the fruits of banding, I must of necessity hold concrete views on the possibilities and limitations of the system. These views, if they are of any value at all, are surely the more so by virtue of being critical.

They can be summed up as follows: Banding is in essence a method of collecting facts. In itself it goes no further. Since the facts obtained pertain to the biology of birds, their interpretation must rest in the hands of the student of biology, amateur or professional, inside or outside of the ranks of banding, it matters not. The quality and quantity of produc-

tion—both are desirable—depend on the efforts of the active workers, the majority of whom are undoubtedly not scientists. Let them remember that quantity without quality is wasted effort; that the same ingenuity and perseverance exhibited by them in the designing and perfecting of their traps can be extended, and should be, to the cultivation of precision, method and cooperation, upon which the ultimate success of banding largely depends; that it is no more creditable to be a biologist than to be anything else; that if they happen to be something else and yet have scientific aspirations, there is this consolation: The conscientious acquiring of facts constitutes not only as vital a contribution to the progress of science as the formulation of theories, but a more stable one.

In case I have sounded discouraging, may I add another word. Provided their interest is serious, there cannot be too many banders in the field or too many stations. The more there are, the greater will be the returns and the sooner will banding bring those results that it alone can bring, essential to a full understanding of migrations of birds. Even the bander with a limited knowledge of birds can work in such a manner as to safeguard himself from all criticism. He can, for instance, devote his time to the study of certain species only and let all other birds depart without band or record. That would constitute scientific precision, would increase the output of *reliable* data, would bring neither his own nor other people's work into disrepute and would greatly assist the progress of scientific banding. Perhaps I should also add that I am well aware that there are hundreds of banders who have adopted the principles that I have tried to outline above. My remarks are addressed to the other hundreds who have perhaps never even thought of them.

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