in so far as these latter could be classified along with the poliomyelitis viruses as neuronotropic they are not considered as belonging to the group of poliomyelitis viruses. Therefore, it is the opinion of this Committee that there is insufficient justification for the use of the terms "poliomyelitis" or "poliomyelitis-like" in connection with the Columbia SK, MM, EMC, and related viruses.

III. Summary and Recommendations

(1) The term *poliomyelitis virus* should be used to designate strains of the agent originally described as the cause of poliomyelitis in man and only these. It is identified by the characteristic experimental disease in the monkey, by the character and distribution of histological lesions in the spinal cord and brain of infected primates, by its host range, and by its immunological properties.

(2) Strains of poliomyelitis virus have been distinguished by immunological methods. With the exception of the Lansing group, they are as yet poorly defined. Some strains in this group have special properties of infecting cotton rats, mice, and hamsters, as well as primates. Human sera may contain antibodies to these strains. Because they also satisfy all other identifying criteria, their inclusion as examples of true poliomyelitis virus is justified.

(3) Certain encephalomyelitis viruses of mice, such as Theiler's TO, FA, and GD VII strains, have been termed "mouse poliomyelitis" by some. This term should be discontinued and Theiler's original designation of *spontaneous mouse encephalomyelitis* used to describe these viruses.

(4) Other viruses which produce paralysis and neuronal lesions in the anterior horns of the spinal cord in experimental animals, but which do not otherwise satisfy the criteria set down for poliomyelitis virus, should not be called "poliomyelitis virus," "mouse poliomyelitis virus," or "poliomyelitis-like virus."

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Some Considerations of Bird Migration

Continental Drift and Bird Migration

The theory of continental drift postulates an original northern land mass, Laurasia, and a southern one, Gondwana. Each eventually broke into several drifting segments which became the present continents. Laurasia and Gondwana were "separated by a vast sea known as the Tethys." To explain some facts of animal distribution it is assumed that Laurasia and Gondwana occasionally drifted near one another or were at times in contact.

Wolfson (*Science*, July 9, pp. 23–30) has sought to explain the long migrations of some species of birds from hemisphere to hemisphere in terms of this theory. He assumes that at those times when the two hypothetical land masses were adjacent, certain birds happened to acquire a short migration that took them from one to the other. As the land masses drifted apart, he believes that such birds continued to migrate from one to the other until their migration may now extend nearly or quite from the Arctic to the Antarctic.

The theory of continental drift was seized upon by zoogeographers seeking to explain certain supposed anomalies of plant and animal distribution, particularly among fossil forms. Because of the relative scarcity of avian fossils and the unusual powers of dispersal conferred by flight, ornithologists are unable to determine the validity of this theory. Mammals, which are less able to cross water gaps and are represented by numerous fossils, are better material for such investigation.

G. G. Simpson (*Amer. J. Sci.*, 1943, **241**, 1–31), in connection with his extensive studies of fossil and living mammals and their distribution, summarized his conclusions with respect to the theory as follows:

> The fact that almost all paleontologists say that paleontological data oppose the various theories of continental drift should, perhaps, obviate further discussion of this

point and would do so were it not that the adherents of these theories all agree that paleontological data do support them. It must be almost unique in scientific history for a group of students admittedly without special competence in a given field thus to reject the all but unanimous verdict of those who do have such competence.

After reviewing the evidence, Simpson concluded:

> The known past and present distribution of land mammals cannot be explained by the hypothesis of drifting continents. . . . The distribution of mammals definitely supports the hypothesis that continents were essentially stable throughout the whole time involved in mammalian history [*i.e.* since the Triassic].

More recently Simpson (*Bull. geol. Soc. Amer.*, 1947, **58**, 613–688) has shown that North America and Eurasia apparently have occupied their present positions at least throughout the Tertiary, though at times connected by an Alaskan-Siberian land bridge. Even the widely accepted earlier belief in an early Tertiary European-North American land bridge seems now to be contrary to most paleontological evidence. Similarly, the mammalian fauna of Australia is such as to indicate strongly that that continent has been in its present position throughout the era of mammals and not connected with other continents except by an archipelago leading toward Asia, as at present. Even the continental drift proponents admit that the mammals of South America and Africa are so different that these continents must have separated by the beginning of the Tertiary. Wegener himself placed the initiation of separation of Gondwana into continents as pre-Jurassic.

Paleontologists thus think the evidence opposes continental drift, at least since the beginning of the Mesozoic and certainly since the beginning of the Tertiary. In seeking to extend the origin of the migrations of many existing species of birds back to such ancient times, Wolfson writes (p. 26): ". . . by the end of the Cretaceous modern types of birds were well represented. It is conceivable, therefore, that birds were well established and that migratory movements had originated *before* the advent of drift" [italics his].

The known Cretaceous birds, such as *Hesperornis* and *Ichthyornis,* though usually from the upper strata of that period, are plentifully supplied with teeth and other primitive characters. There is no reason to believe that modern types were represented at that time, particularly since even among Eocene birds the majority belongs to subfamilies, if not families and orders, distinct from living birds. This will be evident to anyone examining recent works on avian paleontology—for example, the two papers of Wetmore (*Amer. Mus. Nov.*, 1934, No. 711; *Smithsonian Misc. Coll.*, 1940, **99**, 1–81.)

Among the many birds that have long migrations from one hemisphere to the other are representatives of several families including such advanced types as warblers (Parulidae), the bobolink (Icteridae), swallows (Hirundinidae), as well as water birds, such as sandpipers (Scolopacidae), terns (Laridae), and many others. That the migratory patterns of these existing species were determined by events which occurred, if at all, at least 70,000,000 years or more ago in the Cretaceous period is not, as Wolfson says, conceivable—it is inconceivable.

A further conclusion of Wolfson's theory is that, "Implicit in the present hypothesis is an *origin in the Southern Hemisphere* for those migrants that winter there today" (p. 29, italics his). The suggestion that numerous species of northern nesting sandpipers and other birds originated in the Southern Hemisphere because they happen to migrate there is very unconvincing. Many species like the sanderling (*Ereunetes alba*), which may be found in winter from New Jersey to Patagonia, winter in both hemispheres. Others like the warblers of the genus *Dendroica* contain several obviously related species that nest in the north, some of which migrate to the Southern Hemisphere while the others do not. Are we to assume that the former group evolved in South America, the latter in Canada?

Wolfson stated that birds with long interhemispheric migrations have closely related species in the Southern Hemisphere. The particular species he chose to illustrate this, the antarctic tern (*Sterna vittata*), is unfortunate, since Murphy (*Amer. Mus. Nov.*, 1938, No. 977) has shown that it is allied to the other Southern Hemisphere terns and only superficially resembles the arctic tern (*S. paradisaea*). Some long-distance migrants like the bobolink (*Dolichonyx*) have no close relatives in the Southern Hemisphere.

The major shortcomings of this theory of migration have been mentioned; one or two other weaknesses in the evidence presented to support it may be indicated. The migration of the Greenland wheatear (*Oenanthe oenanthe leucorhoa*) from its nesting grounds in Greenland and Baffinland back through western Europe to Africa is mapped. It is suggested that this route arose as America drifted westward away from Eurasia and the birds continued to cross the ever-widening gap. Overlooking the fact that the African portion of this map of the wheatear's range is greatly in error, as Dr. James P. Chapin informs me, it must be noted that this species nests all across northern Eurasia and well into Alaska. The Alaskan wheatears, like several other Alaskan birds of recent Asiatic derivation, migrate back through Asia. To explain this in terms of direction of continental drift, we should have to believe that the east and west coasts of North America had drifted in toward each other. Most of us will prefer the older theory that a migratory species that has recently extended its range is apt to migrate back along the route of range extension.

Exception must also be taken to the belief that migration is rare in the Southern Hemisphere. If we take into account the much smaller area of land in the southern temperate region as compared with that in the Northern Hemisphere, and the correspondingly smaller number of resident birds, the percentage of migrants in an area such as Tasmania, for example, is much as would be expected in an area of similar climate in the north.

Wolfson makes much of the fact that most bird migration, like most of the assumed continental drift, is northward. Taking it for granted that northern breeding species migrate north, Wolfson then says of the migration of the species that breed in the Southern Hemisphere: "These flights from southern temperate latitudes, it should be noted also, are northward. . . ." Perhaps it is permissible to point out that a similar half truth could be expressed by saying that all migrants go south—in the Northern Hemisphere away from their breeding grounds; in the Southern Hemisphere, toward them.

The possible methods of evolution of long interhemispheric migrations have been considered in numerous publications on bird migration. Wolfson's implication that such migrations are in a class by themselves is definitely erroneous. Every degree of migratory behavior from short or occasional flights to long, regular ones exists, and often in closely related species or even within a single species. Indeed, banding has shown that an individual bird may migrate some years and not in others.

Certain species like the sanderling may winter over a great north-south distance. Doubtless in other species of once similar migratory patterns the individuals wintering at intermediate points were gradually eliminated because of poorer survival. The tremendous but gradual changes in climate and in other factors influencing bird migration associated with the glacial periods or other geological cycles unquestionably played a part in molding migratory routes.

Although, as Wolfson states, some birds seem to migrate further than is necessary to find wintering grounds, this cannot be taken for granted until we know vastly more about their winter requirements and about competition with related species wintering in adjacent or less distant areas. That the arctic tern migrates south chiefly through the eastern North Atlantic, for example, may be an entirely adaptive evolution of the most favorable route, resulting from poorer survival of individuals that once migrated in the western Atlantic. In a thorough study of the migration of this tern, Kullenberg (*Arkiv. Zool.,* 1946, **38A**, 1–80) points out that it is partial to cold, plankton-rich waters at all times. This explains its southward migration through the eastern Atlantic, where the cool waters of the Canary and Benguella Currents leave a vastly narrower expanse of tropical water to be crossed than exists in the western Atlantic. Moreover, the use of Mercator projections in Wolfson's maps of the migration of the arctic tern and of the wheatear gives a highly distorted picture of the distances involved.

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Some More Problems in Bird Migration

The very stimulating paper by Albert Wolfson (*Science,* July 9, pp. 23–30) brings to mind two other interesting problems in the riddle of migration in bird life. During the last few years observers on the Mississippi Flyway have been reporting an increase in the number of migrating blue geese, *Chen caerulescens.* The state of Iowa provides an excellent opportunity to note this change. Iowa is bordered by two main flyways, the Central and the Mississippi. The first list published on the birds of Iowa (Keyes and Williams. *Proc. Davenport Acad. Sci.,* 1903, **5**, 113–161) makes no mention of the appearance of this species of waterfowl in the state at that time. Although Cooke ("Bird Migration in the Mississippi Valley in 1884 and 1885") reports that it migrates through the Mississippi Valley, it must be remembered that only two Iowa ornithologists contributed data to this report, so it cannot be so complete in that respect. Anderson's "Birds of Iowa" (*Proc. Davenport Acad. Sci.,* 1907, **11**, 125–417) listed it as a rare migrant, while DuMont (*Univ. Ia. Stud. nat. Hist.,* 1934, Vol. V) records that it is a recently common migrant along the Missouri River Valley during the spring, though somewhat less numerous during the fall migration. Since 1934 the number of spring migrants has steadily increased to such an abundance that people come from all over the country to view the migration and it has been reported in detail in the press and sporting magazines.

Then the sudden change came. In the spring of 1945 the blue goose used the Mississippi Flyway for the first time. James Harlan (*Ia. Bird Life,* 1945, **15**, 48) recorded the spectacle as follows:

> . . . A flock of approximately 3,000 Blue and Snow Geese are migrating north up the Mississippi River and are now resting in the Green Bay bottoms. Single Blue

Geese have been noted on the Mississippi rarely in the spring in the past, but since historic times the spring flight of this goose has been north up the Missouri River, with some major flocks spilling inland to the northwest Iowa lakes. Ornithologists have been anticipating a change in the spring concentrations because of an irregularity in the fall flight pattern recently. Bird students attribute this flight change to the advent of the mechanical cornpicker, which shatters and leaves lying on the ground thousands of bushels of readily available waste corn.

It would be interesting to know exactly how the flight patterns of birds originated and if they have remained the same during the centuries. For an example, take the case of the common migrant, the white-throated sparrow. A recent study by two bird banders (*Auk,* 1948, **65**, 402–418), in cooperation with other banders who had placed 43,000 tags on this species of finch, proved that some individuals change the flyway that they had been using. A total of 10 individuals were found to have crossed from one flyway to another; for example, one bird starts the migration on the Atlantic Flyway and then crosses over to the Mississippi Flyway. As far as we know, this may be the case with all of our common migrants. The solution will come only from intensive and cooperative banding operations over the entire continent.

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Bird Migration and Magnetic Meridians

In "Bird Migration and the Concept of Continental Drift" (*Science,* July 9, pp. 23–30) Wolfson mentions several times the *eastward* flight of the arctic tern across the North Atlantic and asks: "Is it merely coincidence that the direction of flight is in accord with the pattern of drift? If so, how then, can one explain the east-west flight across the North Atlantic. . . .?"

One possible explanation, which, however, does not conflict with Wolfson's main thesis, is suggested by the fact that if the flight path is referred to magnetic direction, it is seen that the migratory path is approximately magnetic south. To be more precise, if the arctic tern maintained a magnetic heading of south, paralleling the magnetic meridians, on its overwater flight, the resultant of this heading and the prevailing winds of the North Atlantic would be the observed migratory flight path.

Other birds also seem to hold a flight heading parallel to the magnetic meridians in their migratory flights. The overwater flight of the golden plover from Nova Scotia to the mainland of South America also can be regarded as the resultant of a heading of magnetic south and the prevailing winds of the area.

These two cases suggest the possibility that on long, overwater, migratory flights all birds maintain a heading parallel to the magnetic meridians and that their flight path is a resultant of this heading and the wind at the time of flight.

It is suggested that a study of overwater migration as a navigational problem with consideration given to heading, speed of flight, and prevailing winds at the time of migration may clear up seeming anomalies in direction of migration, such as the eastward flight of the arctic tern.

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Bird Migration and Pressure Patterns

In a recent interesting article, Wolfson discusses a new hypothesis which attempts to explain the paths of migration of birds (*Science,* July 9, pp. 23–30). It is not intended to comment on the biological and geological questions raised in this paper. It seems apropos, however, to present a few points that occur to a meteorologist in this connection.

In Wolfson's paper there is no direct reference to the effect of winds on the migration of birds. Birds, after all, have only a limited amount of power available for their movements. Obviously, they will be seriously affected in their migrations by currents in the atmosphere. It is unlikely that they would be in a position to battle head winds for long periods of time.

What strikes the meteorologist in looking at the migration paths shown in Figs. 4, 5, and 6 of Wolfson's paper is the resemblance to certain generalized trajectories of air currents. For example, in the migration of the arctic tern one can observe a close correspondence to the trajectories of the most frequent winds in the late fall and early winter. These winds are northwesterly in the Baffin Bay area, then turn to westerly in the North Atlantic between the North American continent, the southern tip of Greenland, and England. They turn to northerly over western Europe and northeasterly in northwest Africa. The path of migration shows over the North Atlantic a flight pattern around the Middle Atlantic high-pressure cell, commonly known as the Bermuda-Azores High. The recurving of the flight path to eastern South America follows the trajectory of the northeasterly trade winds in the very early parts of the winter. These trade winds reach and occasionally cross the equator in the South American area. Northeast or eastnortheast winds are still prevalent during this season as far south as 35° southern latitude along the east coast of South America. South of that area, especially somewhat off-shore, northerly and northwesterly winds prevail at the west edge of the central high-pressure cell in the South Atlantic. This could easily facilitate the drifting of the birds into the middle of the South Atlantic.

It is similarly interesting that the Greenland wheatear follows a path across the Atlantic and along the African shores which resembles the general trajectory of air in the late fall. In migrating along the African shore, these birds would encounter in the early parts of the winter fairly strong southerly wind components near the equator in the area of the Gulf of Guinea, which might be a contributary cause for stopping them in this general region.

The migration of the Pacific golden plover also shows rather unmistakable signs of a wind-pattern correlation. In the fall, when the Siberian high-pressure cell begins to re-establish itself, northerly components of winds are the rule in the areas of Kamchatka and Japan. These turn to northeasterly components south of Japan and continue in that direction to the equator. Somewhat later in the season, just south of the equator, the trajectories change to northwest and continue with that direction through the island belt of the Dutch East Indies into northern Australia.

It is also noteworthy that Wolfson mentions that the greater shearwater moves back from the middle of the South Atlantic on the western side of the North Atlantic Ocean to high latitudes in spring and summer. This flight path follows a general air trajectory first in the southeast trade and then north of the equator, in air currents on the southern and western side of the Bermuda-Azores high-pressure ridge. In addition, Wolfson states that birds of the Southern Hemisphere do not migrate as far as birds from the Northern Hemisphere. Meteorologically, there is a considerable difference between the general circulation of the atmosphere in the moderate latitudes of the two hemispheres. The general pressure patterns in the Northern Hemisphere change radically with the seasons. In the Southern Hemisphere there is relatively little change in the fundamental pressure patterns, especially over the oceans. The belt of westerly winds in the Southern Hemisphere is also very strong throughout the year, and it would probably be very difficult for a bird to break through the so-called Roaring Forties.

To the meteorologist, it looks as if some of these migratory birds had developed a rather remarkable system of what is called in modern aviation "pressure-pattern flying." This is the system which takes advantage of the maximum possible amount of tail wind in long-distance flights. Since the data collected on these migratory birds are essentially based on records from long years of observations, they should most closely resemble the mean wind conditions as we find them on climatological charts. In any individual year the migration would, of course, be governed by the prevailing conditions at that time, and hence a certain amount of scatter in the recovery of banded birds can be expected.

If powerful modern aircraft, for reasons of economy and safety, adopt the system of pressure-pattern flying, it seems reasonable that birds, which are much more dependent upon assistance offered by these air currents, would follow the path of least resistance. The general circulation patterns in the atmosphere have, of course, changed throughout geological history. If there had once been in existence just one major land mass, the general circulation would have been entirely different. In fact, it would be very difficult to visualize a circulation pattern under those circumstances that would resemble the patterns prevailing in the present geological era. The winds prevailing over a single continental mass, as envisaged by the hypothesis of continental drift, would in its seasonal variations be completely at variance with present conditions. These currents would have forced birds to migrate along entirely different paths than at present. It would seem desirable that any hypothesis of bird migration, such as the one proposed by Wolfson, should include a very careful analysis of present and presumed past patterns of atmospheric currents.

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Bird Migration Over the Mediterranean

I read with interest the article by Griffin and Hock (*Science,* April 2, pp. 347–349) on the homing of gannets in America. This contained mention of a belief that migrating birds flew in straight lines. My "Notes on the Migration of Birds Over the Mediterranean Sea" (*Brit. Birds,* 1919, **13**, 173) tends to show that birds stick to coast lines as much as possible, or to routes where they can see land. They do not cross the Mediterranean at any haphazard place; they stick to one of four routes: (1) the well-substantiated Gibraltar route (not discussed in my article); (2) Cape Bon-Sardinia-Riviera (land bridge in Upper Eocene and Oligocene ages, later broken up into a chain of islands); (3) Malta-Sicily-Italy (land bridge in early Pleistocene); and (4) Egypt-Crete-Greece (never a land bridge, but with land visible all the way to a bird flying high in good weather).

The Cretan route provided me with $4\frac{1}{2}$ times as many observation days of migrating birds and $2\frac{1}{2}$ times as many birds (1.8 as many species) as did the Sardinian route. My observations or discovery of these last three routes was apparently new at the date of publication, and *Ibis* abstracted my article.

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Sensitivity of the Homing Pigeon to the Magnetic Field of the Earth

H. L. Yeagley has proposed the theory that the homing pigeon navigates to its loft because of sensitivity to the magnetic field of the earth and to the Coriolis force (*J. appl. Phys.*, 1947, **18**, 1035). The intersecting magnetic and Coriolis fields give a grid effect such that each point on the surface of the earth is characterized by a unique or almost unique combination of the two forces. The pigeon orients himself to the point on the earth having the "local sign" of his aviary, which is familiar to him. This theory, supported by a series of ingenious experiments, seems to offer a fresh explanation of what has long been considered a most puzzling biological phenomenon.

The theory has been severely criticized for its theoretical assumptions (L. Davis. *J. appl. Phys.*, 1948, **19**, 307; Linnean Society of London. *Nature, Lond.*, 1948, **161**, 996; J. Slepian. *J. appl. Phys.*, 1948, **19**, 306; R. H. Varian. *J. appl. Phys.*, 1948, **19**, 306).

(1) The bird would have to be sensitive to extremely minute gradients of the physical forces involved in order to navigate in the correct direction. Disturbing influences such as static charges in the atmosphere must be disregarded.

(2) The bird would have to gauge its speed of motion over the earth with extraordinary accuracy. The emf supposedly induced in the pigeon by the magnetic field of the earth is related to the speed of the bird in traversing the field. To sense correctly the strength of the magnetic field of the earth, the bird would have to know its own speed.

(3) The theory assumes the sudden emergence in the bird of sensitivities which have never been established for the lower classes of animals. The probability of two sensory capacities spontaneously arising at a given evolutionary level would seem to the biologist to be very remote.

While these theoretical criticisms weigh against the theory, no theoretical argument can cast aside the basic fact of pigeons returning, or failing to return, under experimental circumstances designed to test the effect of the earth's magnetic field, and the Coriolis force. Unless there is some fault in the setup of the experiments (several possibilities have been mentioned by the Linnean Society of London), homing would seem to be due to the sensitivities mentioned or to somehow related sensitivities. Repetition of these observations is definitely indicated.

The author has repeated, with certain modifications, an experiment of Yeagley's which supported the view that sensitivity to the earth's magnetic field plays a part in bird navigation. In the original experiment, 10 birds with magnets and 10 with copper plates of the same approximate mass and size were released by Yeagley at a point 65 miles from the home base. Yeagley believed that the pulsating magnets might induce in the bird's anatomy an oscillating emf which would confuse the perception of the earth's magnetic field. The following figures were given for number of birds returning:

Nov.	Day	Two copper plates	Two magnets	Lost one magnet	Lost two magnets
7	1st	5	–	1	–
8	2nd	3	–	–	–
9	3rd	–	–	–	–
10	4th	–	1	2	2
Total		8			6

These results are suggestive of an interfering effect due to the attachment of the magnets but cannot be regarded as showing such an effect conclusively. The experiment employed only 20 birds; magnets fell from the wings of the birds in the magnet group; the difference in returns was inconclusive (6 birds in the magnet group as against 8 birds in the control group).

The 60 homing pigeons employed in the present experiment were raised at the Pigeon Breeding Center of the U. S. Army, at Fort Monmouth, New Jersey.[1] The birds were between the ages of 3 and 6 months and had been trained on several preliminary flights. Three experimental groups were used: (1) 24 birds which carried magnets of high-flux chrome, size 1″ × .218″ × .025″ on their wings; (2) 24 control birds which carried *unmagnetized* high-flux chrome bars of the same size and weight as the previous group; and (3) 12 birds which carried no weights. The latter group was included to test the effect of attaching weights to the wings of the other birds. The magnets and slugs were glued with Duco cement on the under side of the manus portion of the wings, between the third and fourth metacarpal bones, as described by Yeagley. No magnets or slugs fell off during the runs.

The birds were released from three points—36, 50, and 58 miles distant from the aviaries—selected to allow an analysis of the disturbing effect of the mag-

[1] The pigeons were raised under the direction of Maj. Otto Meyer, chief pigeoneer, Army Signal Corps. The author wishes to acknowledge the valuable help of Mr. Leon Zinner, who assisted in the work.

nets as related to the distance of flight from the aviary. The points of release were in the southwesterly direction, in unfamiliar territory. The direction of flight was such that the birds had to navigate across the gradient of the magnetic field.

The constitution of each of the three groups of pigeons released in this experiment was as follows: 8 with magnets, 8 with slugs, and 4 with no attachments.

Pigeons were released from the three distances simultaneously at 6:30 A.M. on August 27, 1948. They were released singly, with 10 minutes between tosses. No bird was liberated if the last bird was still in sight. The order of release provided for rotation among the magnetic, slug, and control groups.

Results showed that *every bird used in this experiment returned home before nightfall of the day of release.* Time records are incomplete, but are given below for birds clocked on entrance to the aviaries.

	36 miles	50 miles	58 miles
No attachments	Median, 163.5 min (2 birds)	Median, 104 min (4 birds)	Median, 400.5 min (2 birds)
	Mean time, 213.4 min (8 birds)		
Slugs attached	Median, 95 min (6 birds)	Median, 133.5 min (8 birds)	Median, 171.5 min (4 birds)
	Mean time, 162.0 min (18 birds)		
Magnets attached	Median, 106 min (4 birds)	Median, 111 min (5 birds)	Median, 180 min (6 birds)
	Mean time, 160.3 min (15 birds)		

Analysis of these time records shows that the birds with magnets attached returned as quickly as the others. It is not known why birds with no weights attached flew more slowly than the others. Emotional effects may have increased the speed of flying in the magnetic and slug groups.

This experiment fails to show any effect on homing due to the attachment of magnets and the emf supposedly induced in the birds by oscillation of the wings. It is suggested that other experiments of similar nature be carried out in the attempt to see if the unusual results reported by Yeagley can be verified by another observer. If these results can be duplicated, experimentation might well proceed to the isolation of the particular sensitivities involved by such techniques as the reward and punishment methods of comparative psychology.

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On Continental Drift and Bird Migration

With respect to Dr. Wolfson's illuminating and clearly thought-out paper (*Science,* July 9, pp. 23–30), I would like to point out the important distinction between changes in the configuration of continents and ocean basins that may be brought about and have been brought about, as we know through ample evidence, by deformation of the earth's crust, rising continents, subsiding basins, etc.—the distinction, I say, between such deformation, with consequent changes in configuration of land and sea, and *the concept of continental drift as such.*

I suspect Dr. Wolfson's brilliant hypotheses could equally well conform to changes in the face of the earth, to changes in paleogeography, that did not arise through continental drift, but in consequence of other modes of crustal deformation.

That where once there was land, there may now be the waters of the Atlantic and that such a change could have brought about the evolution of migratory birds seems plausible and exciting; that it follows that continental drift was the cause of the changed distribution of land and water does not necessarily follow and *is quite a different problem.*

Dr. Wolfson's thoughtful paper, nevertheless, remains a stimulating contribution, and I agree that "if biologists have an adequate knowledge of the properties, requirements, and behavior of organisms, and have confidence in that knowledge, they can make substantial contribution to our knowledge of the earth's history."

Sidney Paige

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