

was compiled in 1909, very little detailed observations on the insect remains found in the stomachs of wild birds had been made in Great Britain, and little attention had been given to work done elsewhere.

Judd, who examined many thousands of stomachs, writes, "I do not know of a kind [of bird] that feeds upon butterflies during any month of the year to the extent of one-tenth of one per cent of its food". Beal, who in his lifetime examined 37,825 stomach contents, very rarely refers to butterflies amongst the various items. In his investigation of the food of seven species of American swallows (U.S. Dept. Agric., *Farmers' Bull.*, No. 630, pp. 1-27; 1915) he examined 2030 stomachs and found that 70 per cent of their food consisted of Hymenoptera, Hemiptera, and Diptera. A specimen of *Vanessa atlanta* was found in one stomach. This same investigator in his work on the food of seventeen species of American flycatchers (U.S. Dept. Agric., *Biol. Surv., Bull.* No. 44, pp. 1-67; 1912), of which he examined 3398 stomachs, makes no mention of butterflies or their larvæ. Moths and their larvæ constituted 9.93 per cent of the food of all species.

Cleland (Dept. Agric. N.S.W., *Sci. Bull.*, No. 15, pp. 1-112; 1918) in his work on the food of Australian birds examined 1133 specimens referable to 224 species, and butterfly remains or their larvæ occurred in one case only, namely, a tricoloured chat (*Ephthianura tricolor*) which contained two larvæ. Mason ("The Food of Birds in India", 1912), after examining the stomach contents of 1329 specimens of Indian birds referable to 107 species, states, "Butterflies do not form any appreciable proportion of the food of any one species of bird, though a good many birds take these insects at times".

My own investigations, embracing more than one hundred and fifty species of wild birds and upwards of twelve thousand post-mortems, lend no support to the contention that butterflies are part of the normal food of birds. Interesting as are the observations and experiments of Swynnerton (*Ibis*, 6, pp. 635-640; 1912; *Trans. Second Int. Cong. Entom.*, 1912, pp. 351-354; 1913; *Jour. Linn. Soc. (Zool.)*, 33, pp. 203-385; 1919), they mostly refer to birds in captivity. In the very extensive investigations of McAtee, Fisher, Forbes, Forbush, Barrows, Kalmbach, and others, I find only rare and odd references, such as "and one butterfly".

The work quoted above embraces considerably more than 100,000 post-mortem examinations of the stomach contents of wild birds in the United States, India, Australia, and Great Britain, and the number of butterflies or their remains found was negligible, indeed so infinitesimal as to be of no account economically, whereas other insects occurred in thousands.

From what has been said I think we may conclude that a careful and very extensive series of examinations of the stomach contents of upwards of one hundred thousand birds lends no support to the view that wild birds do prey upon butterflies to an appreciable extent, nor are they "liable to habitual attacks upon the part of birds".

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#### Meteorology in India.

IN NATURE dated May 4, p. 698, there appeared a review notice of the first three numbers of the *Scientific Notes* published by the Indian Meteorological Department. In criticising the first Note by Mr. Mohammad Ishaque on "A Comparison of Upper and Gradient Winds at Agra and Bangalore", the reviewer took exception to Mr. Ishaque's statement that the gradient wind equation does not hold at the equator, and

described the statement as "an unfortunate mistake". Though Mr. Ishaque might with advantage have expressed himself a little more fully, it is profitable to recognise that there may be much truth behind his statement. For, it is not at all certain, as the reviewer would seem to imply, that motion in the free air over the equator under steady conditions is normally along isobars under the sole, effective control of the cyclostrophic component. On this very question Sir Napier Shaw ("Manual of Meteorology", vol. 2, p. 266) has expressed the opinion that "we can no longer assume that the motion is along isobars for the equatorial region; it is controlled by some other consideration, and we do not know how far the new form of control may extend North and South of the equatorial belt".

Mr. Ishaque's work indicated that upper winds at the levels of 0.5 km. or 1.0 km. are less closely calculable from sea-level isobars in India than in England, and the reviewer suggests that the poorer agreement in India is "a measure simply of the point to which accuracy of measurement of barometric pressure has been carried in each" country. I have good grounds for believing that want of accuracy in the actual measurement of pressure at individual stations in India is not the cause of the poorer agreement. Probably, however, the reviewer had in mind the accuracy with which the sea-level pressure field could be estimated from these measurements. Admittedly, the want of a very close network of stations near Agra and Bangalore, as well as the variable degree of validity of the internationally accepted conventions used in reducing pressure-values to sea-level (Mysore stations are more than 2000 ft. above sea), may at times lead to considerable errors in the drawing of sea-level isobars in India. There are also, however, other contributing factors, such as the fact that the conditions are far from steady near the surface owing to the alternation of inversions at night with adiabatic lapse-rates by day, and the disturbing boundary influences exerted at surfaces of inversions, the Himalayas and the Western Ghats.

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I quite agree with Dr. Normand that it is going too far to assert definitely that the possible interpretation of the small correlation between gradient wind and observed wind at 0.5 km. and 1.0 km. height at Agra, quoted above, is the true one, but I think that errors in the determination of the gradient wind are at the present time too large to justify one in accepting the results of comparisons made in different countries as indicative of the influence of latitude, without a critical examination of the effect of such errors.

On reading the chapter of Sir Napier Shaw's "Manual of Meteorology" which contains the quotation cited by Dr. Normand, and in particular the second paragraph on page 249, I concluded that the statement that "we can no longer assume that motion is along isobars for the equatorial region" is meant to apply to *mean* isobars and not to the isobars existing at a particular moment. There seems to be no obvious reason why the virtual disappearance of the term in the gradient wind which depends upon latitude should affect the closeness with which the actual free air wind on a particular occasion approaches the value appropriate to air moving under balanced forces, and a high correlation between the two seems not unlikely over those great stretches of tropical ocean where horizontal gradients of temperature are normally small.

THE WRITER OF THE ARTICLE.