

**MISCELLANEOUS NOTES ON
ARCTIC-RELATED TOPICS**

BY

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Editor's notes:

These manuscripts were entirely handwritten on unnumbered sheets of loose-leaf quarto. Page numbers are added for ease of reference. Species names and titles appear in italics, but other underlining is shown as such. Abbreviations are shown in full where the meaning is clear. Editor's notes shown in {}.

Periodic Fluctuations in populations

My first introduction to this subject came about in two ways (1) Julian Huxley gave me to read for a tutorial in early 1922, the remarkable book by C. Gordon Hewitt (1921), on "*The Conservation of the wild life of Canada*", that included curves for a number of the returns of the Hudsons Bay Company. Many showed the extraordinary cycles in numbers, of varying hare ("snowshoe rabbit"), lynx, arctic fox and others. Most of these had an approximately 10-year cycle others (the Arctic fox) about four years, and some a combination of the two. (2) When I was in Tromsø at the close of the Merton College Arctic Expedition (to Spitsbergen), in the summer of 1923, I spent one of my few last pounds on a book by Robert Collett – "*Norges Pattadyr*" (1911). This included the information about lemming-years (of migration, and by implication, high numbers) in Norway.

These data were the essential basis for my conclusions, in a paper (Art. 5) which I now find a bit embarrassing to read, so full is it of uncritical youthful statements and, above all, for my espousing of the hypothesis that the ten-year cycle in Canadian fur-bearers was controlled by a climatic oscillation coinciding with the sunspot cycle. Unfortunately, the latter idea was proved to be incorrect, but had a certain dramatic flavour that attracted good deal of attention. Indeed it got into four text-books before I had been able to reject it for a more limited view. But I have with believed that the regional spread and consistency of these cycles only be explained by the action (in some way) of climate, and consequently of cyclical incidence of climate. We are dealing with an area of millions of square miles. But a long search by me (with the crude methods then available) never yielded this kind of evidence. It remained for a Swiss statistician, Arditi¹, many years later, to obtain just this evidence, with the use of computers, etc.

A young Toronto biologist D.A. MacLulich, obtained a longer series of fur returns from the H. B. Co. and was able to show that they become quite out of phase with the sunspot cycle. He deserves credit for this, though by that time I had myself abandoned the hypothesis. (The subject of instability in populations is discussed at some length in Chapter 9 of *Animal Ecology*).

Turning to the more successful aspect of my paper: there are two important ideas that were new, to at any rate the academic world. It had been vaguely believed that populations of animals remained at a more or less constant level, apart from seasonal cycles of breeding. Indeed, R.A. Fisher, in his 1930 book "*The gene and natural selection*" make this assumption. At the time I began work in this field, there was still a rather surprising mental barrier between the theoretical and practical biologist – meaning workers in forest entomology, farm pests, diseases vectors, and fisheries, – in the early 1900s cod populations of herring, were known to have fluctuations of a natural kind. (Indeed, when I began to edit the new *Journal of Animal Ecology* in 1932, I made one of its aims to try and bridge this curious mental blind gap).

The other part of my paper that still has some theoretical interest, deals with the relation of fluctuating populations to evolution, and the spread of mutants. (See EVOLUTION), especially the possible spread of mutants without the aid of natural or sexual selection.

¹ R. Arditi (1979). *Oecologia*, 41: 219-233.

Plague and fluctuations in numbers.

In *Art. 7* (published 1925) I seem to have been still under the same illusions e.g. about the effects of the sunspot cycle; but the main idea contained in it was that knowledge of “cycles” in numbers might help in the forecasting of outbreaks in rodents and the consequent dangers to man. I don’t think my aim was achieved here. On the other hand, the fact that it was accepted for publication in the *J. Hygiene* suggests that it had uses. These included a survey of the incidences of both bubonic and pneumonic plague in various countries, the rodent carriers, and information more especially about the situations in Eastern countries, the result of very wide reading in many rather out-of-the-way sources. My view held then is expressed thus: “The most important conclusion which can be drawn is that epidemics have a definite periodicity in many cases, i.e. instead of being irregular and unpredictable phenomena, they obey regular laws, and there is therefore the possibility of forecasting their occurrence.

This general subject is briefly discussed in *Art. 20*, with emphasis on climatic controls.

"The Bagley Wood Mice" (Art. 24)

In 1925 I came to a very important decision about studying population cycles. Dealing with records and figures about rodent populations and their enemies was all very well for getting a broad view about cycles in numbers etc. But I now felt that further progress must depend upon the study in the field of actual animal populations. As we have no snowshoe hares or lemmings it was necessary to choose some convenient and common small native mammal that was known to fluctuate in numbers. I decided on the long-tailed field-mouse or wood-mouse (*Apodemus sylvaticus*). This research began in a curiously casual way. I was demonstrating in the Advanced Zoology class one morning in 1925. In a pause I said to John Baker, who was also working there: "Shall we do some work on mice?" He replied: "All right. I'll provide my car." This project needed a whole team of people. Mrs. Helen Goodrich persuaded my friend E.B. (always called "Henry") Ford to undertake a study of parasitic Protozoa (in which she was expert herself); and, to his credit, he also took on a heavy burden of trap-visiting all through the investigation. Dr. A.D. Gardner, who was working at the School of Pathology for the M.C.R., agreed to look for disease organisms etc. It was through his influence that the M.C.R. made me a small grant to pay for a lab. assistant. For the first three months D. Kempson acted for us; afterwards A.D. Middleton, a country lad who was working in a Northamptonshire school, and who remain with us for a long time. Baker was away on an expedition to the New Hebrides for part of the Bagley period. We got a little extra help then.



CSE, with mousetraps in Bagley Wood 1926.

The area we chose to work in was Bagley Wood, about 3 miles from Oxford, the property of St John's College, and the working area over many years of the Department of Forestry. It was a complex of woodland of different types and ages.

I must confess to hear that at that time I believed *Apodemus* to have a regular 3-4 year cycle in numbers, like the lemming. Certainly British populations fluctuated, but I had no real evidence for Bagley. But a study by H.N. Southern in Wytham Woods, using trap-lines 1949-77, has proved that both wood mice and bank voles were fluctuating greatly, though not with great regularity. The latter showed greater changes than the former. (In "*Ecology of small mammals*", edited by D.M. Stoddard, 1979).

In our study, we chanced to start when mice were scarce, after which they increased and eventually had a "crash". Since *Apodemus* is strictly nocturnal, we had, so far as practicable, to visit our trap-lines as soon as possible after dawn. This involved a prolonged effort, and I must note that our team was invariably hard-working and harmonious. The moving of trap-lines, that had 1-200 traps, was an additional task, to avoid trapping-out. The mornings went in analysis of the catches and in January 1928 I got one of the "body-cards" – the 200th – signed by team members, and decorated it myself (see next page). It will be seen that I handle the general data, also the ectoparasites (and helminths, none on this card), Baker the reproduction, Ford the Protozoa, and Middleton the *Leptospiza*. Altogether, we handled over 2000 mice and bank voles (*Evotomys*, now *Clethrionomys*), as well as some shrews. *Apodemus* numbered 1876; and we trap for 595 nights.

Since the details of this research are clearly described in our paper, and summarised in Chapter 8 of *VML*², I shall not repeat them here. What stood out were two things: the great variation in winter breeding from year to year; and a surprising number of parasites.

The parasitic picture was really very astonishing: 41 species on *Apodemus*, not counting micro-organisms. Yet we never saw a sick mouse! But there was a strange disorder at the crash period, as wood-mice died very soon in captivity. The cause was never found.

In this summary, I have not mentioned that our card-index (still, in 1989, kept in what is now "The Elton Library") has abundant data about *Clethrionomys*, still largely unpublished.

This piece of work has largely been ignored by later mammalogists, though it was a great pioneer effort and deserves recognition.

Harvest-mites Art 50 is an analysis, done with the help of Gladys Keay, of the occurrence of this species. *Trombicula autumnalis* on mice and voles. Its main conclusion was that in Bagley the bank vole was the over-wintering carrier, the mites surviving in its ears. Miss Keay later found that the other important host was the rabbit, also in the ears. (*J. Anim. Ecol.* (1937) 6: 23-35).

² {Elton, C.S. (1942) *Voles, Mice and Lemmings: Problems in Population Dynamics*. Clarendon Press, Oxford. 496 pp. Reprinted 1965.}

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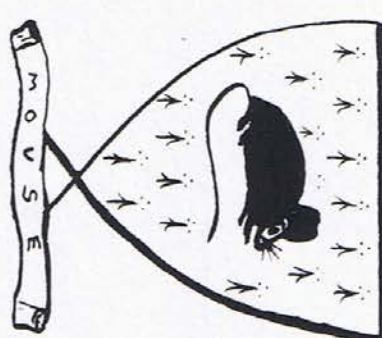
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Sledge-dog and arctic fox epidemics

The following notes relate to Art. 25, published in 1932. The date is important, because at that time the picture of disease had not yet become complicated by the spread of ordinary dog distemper from Southern Canada to the Arctic. Furthermore, my remarks and records have to be read in the context of very early days of virology – I treat the disease as if a single virus was concerned. But we now know that the situation with such diseases is far more complex, owing to the existence or development of mutant forms within one kind of viral disease. Also, as is well known now, the numerous mosquito-borne viruses of Neotropical rain-forest, include a number of different “species” that may produce, at any rate initially, identical forms e.g. of febrile symptoms.

The whole subject is also clearly summarised in my book *V.M.L.*², pages 478-82 (published 1942). I need only mentioned here several main points:

- (1) The epidemics, affecting the nervous system, caused devastating losses to the Indians and Eskimos – my own questionnaire enquiry relating not only to Québec Peninsular, but also to a large number of other fur posts in the North, over many years.
- (2) There was a resemblance to the kind of encephalitis affecting silver fox farms studied by R.G. Green.
- (3) The disease differed in several ways from dog rabies, but most conspicuously in the fact that the dogs or arctic foxes never attacked man; also there were some clear histological differences.
- (4) There is compelling evidence that the arctic fox disease occurred at or after peaks in the four-year cycle, though it does not seem to have distorted the picture shown in fur returns.
- (5) A similar picture of epidemics is found in many other parts of the Arctic i.e. it is a circum-polar phenomenon.
- (6) I was not able to follow the matter later on, because my work turned to other fields.

The barren-ground grizzly bear in Labrador.

While working on the archives of the Hudson's Bay Company and Moravian Missions, I obtained good evidence for the presence in north-west Labrador, at least in the early 19th century of a relict population of a large bear closely resembling *Ursus richardsoni*³ was still well-established in the barrens west of Hudson Bay. This "grizzly" is known from its greater size, grey-tipped hairs (any rate in winter) and habitat. The evidence was from several sources

(1) the fur returns of the company

(2) those of the Moravian Missions

(3) the cogent description by John McLean, an H.B.Co. factor who had also seen grizzlies in Western Canada, was a good naturalist and wrote a book on his experiences.

(4) reports from other naturalists and travellers.

My conclusion had been that there certainly was this population in the early part of the century, but that it is probably extinct now. It must have hung on in the particularly wild and partly inaccessible country of north-west Labrador, where, however, there was plenty of the food it needed. (See *Art.* 84).

This circumstantial evidence received striking confirmation in the discovery of an actual skull of this species during an archaeological excavation at Okak, by a Harvard archaeological man (Steven Cox) in 1975 (see Spiess, A. (1976) *Arctic*, 29:194-200; and *J. Mammal.* (1976). 57: 787-90). Its date is thought to be probably around 1775, but it could be early 19th century. In any case it was before the 20th century expansion westward of the barren-ground bear, and could not be accounted for by a wandering native trading party.

³ {Now regarded as a subspecies *Ursus arctos richardsoni*.}