

In defence of "anecdotal data". A case study from a caribou area in West Greenland

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Abstract: The author pleads for a modification of ethological science that allows for the presentation of even tentative hypotheses, based on what is at present disparagingly referred to as "anecdotal data". It is argued that such data are crucial for the neglected study of the habituation of free-ranging large mammals. In such studies of learning, relevant behavioural observations lie outside the ethologist's control, and can only be replicated by further chance encounters. Observations in their anecdotal form should therefore be made available to other ethologists despite their lack of quantifiable data. This would allow for the creation of a pool of more or less unique observations helping to better understand behaviour.

Key words: *Rangifer*, muskox, ethology, habituation, methodology.

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Throughout June of the five consecutive years from 1985 to 1989 I carried out ethnoarchaeological surveys in the inland area north and northeast of Kangerlussuaq airport, Greenland. This is an important caribou (*Rangifer tarandus groenlandicus*) high density area.

On my return in May/June 1991 I found the situation changed. Descendants of the muskox (*Ovibos moschatus*) which had been introduced in the neighbouring area to the south in the 1960's, was fast approaching the carrying capacity of the area (Riis Olesen, 1993). This had led to some animals spilling over into caribou range at Kangerlussuaq.

My first observation of muskox-caribou interaction was from a mountain ridge facing the small hill where I had my base camp. From a down-wind position two female caribou approached a brink below

which a muskox bull was grazing. Making visual contact with the muskox below, the caribou stopped and after some hesitant steps apparently panicked and dashed off, only stopping once to look back. During the following week I had the opportunity on two occasions to confirm similar flight behaviour by other caribou in their encounters with the muskoxen. During the same period I also regularly observed bands of caribou that were fleeing apparently for no obvious reason. However, on the occasions when I backtracked along their path, I inevitably met with the area new ungulate. In other words, caribou were observed to behave in precisely the same energy-expending manner towards muskox as they do in their confrontations with wolves (Crisler, 1956) and humans (Blehr, 1997). This should come as no surprise. After Lent (1966) there

seems to be no indication that the escape reactions of caribou differ according to the species causing the alarm (cf. Kelsall, 1957).

The initial contact between caribou and muskox thus seem to have disruptive consequences for the well-being of the former. The duration of this period before the whole population of caribou will be habituated to the presence of muskoxen, and stop wasting energy by fleeing when confronted with the new ungulate is therefore clearly important when evaluating the desirability of introducing muskoxen into an area where caribou are indigenous.

Three weeks of fieldwork in 1991 did not yield any observations of interaction between the two species which could sustain any hypothesis as to the initial phase of the caribou's habituation to muskox. However, I consider an observation in June 1986 relevant to a tentative hypothesis. Then I surprised two male yearlings up-wind of me. Upon discovering my presence they ran off in different directions. I stopped dead as soon as I saw them, and after a short run one of them halted and came back toward me, only to be scared away by the noise of my camera shutter. It returned almost at once, only to be scared away again by the same sound. The fact that yearlings approach in this way is not unusual at this time of the year, recently left by their mothers, they will often approach as long as one does not move and they do not get one's scent.

Unusual about this occasion was that the yearling started to graze about 15-20 meters away from me. It grazed back and forth, maintaining approximately this distance all the time. When after about 40 motionless minutes I again started to walk, it ran away and disappeared over a ridge ahead of me. When I reached the ridge, I observed the same animal grazing upwind of me at a distance of 20-25 meters. Without showing any signs of alarm it continued to graze as I walked by, occasionally looking up at me until I disappeared over the next ridge. The yearling had evidently developed what I consider to be an extraordinary, though undoubtedly temporary, tolerance towards me.

How can this observation, from a period before the muskox had started their expansion into the caribou area, be relevant for an understanding of the caribou's eventual habituation to muskoxen? Assuming that the escape reactions of caribou do not differ depending on the species causing the alarm (Lent, 1966), I suggest that eventually yearling caribou might start to behave towards the presence of muskoxen in the same manner in which

they behaved toward me. In this manner, it is possible that yearling caribou act as "brokers" during the initial stages of contact with another species, and that their tolerance will eventually spread throughout the entire population of caribou. But obviously, many more observations of the interactions between these two ungulates will be necessary before this tentative hypothesis can be strengthened or refuted. This does not, however, diminish the value of initial anecdotal observations. On the contrary, such data can, as illustrated in the above case, serve as a source of inspiration in generating hypotheses about habituation.

My main reason for presenting this example is merely to illustrate the use of so called "anecdotal data". The narrative above is necessarily anecdotal as it recounts a particular observation. Due to its anecdotal character, it is considered to be without scientific value by some biologists. Undoubtedly, rejection of data labelled as anecdotal is a reflection of methods used by biologists within experimental research. There data is not data until it has been presented in tabular form, and provides the basis for statistical analysis of a given hypothesis. I do not question the fruitfulness of this type of scientific approach in experimental research as such. However, as I argue below, I do feel that this fetishism of tabulation has consequences for ethological research.

Ethologists who have this attitude to data will when studying free-ranging animal populations have to confine themselves to counting anything which can be counted, however trivial, simply because such observations can be enumerated and put into tabular form. The number of observations considered necessary for hypothesis testing in these studies are by definition fairly easily obtainable. In other studies however, especially when, as in the above case, it is a matter of studying habituation in free-ranging large mammals, the situation is quite different. Here only persistence and good fortune will enable the observer to obtain the relevant data. This is simply because the observations required remain outside of our control, and therefore can be replicated only by further chance encounters. There is therefore a low probability that sufficient data for hypothesis-testing will ever be obtained, even after years of fieldwork. This might explain why ethologists avoid such studies.

As I pointed out above, such an "either/or" outlook, uncritically adopted from experimental research, naturally constrains the nature of biological

questions asked. What I therefore plead for is a less formalistic and rigid approach. I want to see an ethological science that allows for the presentation of tentative hypotheses on habituation, derived from the "anecdotal data" that is unacceptable at present. Hypotheses that next could serve as an inspiration to the international community of ethologists. And just as important, I would like to see the pool of more or less unique anecdotal observations these tentative hypotheses rest on become, so to speak, common property. In this manner, one might hope that sufficient data for hypothesis-testing within the ethological subfield of habituation could be acquired over time. Or at the very least, that a greater understanding of the processes whereby the behavioural forms under scrutiny are generated would be achieved.

The paramount importance of such a return to "descriptive ethology" can hardly be overestimated. Imagine, for instance, what would happen if the often-cited work by Crisler (1956) was submitted to a zoological journal today. It would be labelled anecdotal by the referees, and subsequently be rejected. Her unique observations on the interaction between wolves (*Canis lupus*) and caribou in the Brooks Range, Alaska, would have been lost to scientists. Another example might be Bubenik's (1975) pioneering study of the significance of antlers in the social life of barren ground caribou. But would any zoological journal today have accepted the article, rich as it is in "anecdotal data" and tentative hypotheses? Would it not also have been labelled anecdotal by the referees, with all the consequences which follow from such damning judgement?

I find this attitude to observations questionable. It appears as if dismissal by biologists of works as anecdotal on the grounds that they are based on non-tabulated data is simply an attempt to avoid scientific debate, rather than a serious criticism of methodology. As an anthropologist, I am baffled by this contempt for observations (cf. Tinbergen, 1963) that cannot be presented neatly in a tabular form which supposedly lends itself to statistical analysis of a hypothesis. In my experience, working with aspects of former lifeways in the Arctic, data has more often than not been of a fragmentary character, and I have therefore to be grateful when I am able to present even meagre evidence to sustain the validity of a hypothesis.

I do not believe the outlook is as bleak for ethologists as it is for me. Nevertheless, they should accept that science provides no guarantees that a

particular use of method will ensure final tests of truth about anything. So, instead of hankering for the respectability of what is seen as "hard" or experimental science, I would like to see students of animal behaviour free themselves from what I have labelled the "fetishism of tabulation", and instead to stand up to the challenge presented by the use of the so-called "anecdotal data". Not the least since data of this kind, as noted above, will be of paramount importance in the neglected study of habituation of free-ranging large mammals.

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