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Rangifer and human interests

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Abstract: This article reviews biological and anthropological literature on wild and tame *Rangifer* to demonstrate the powerful effect that this species has had on the imaginations of biologists, social scientists and local hunters. Through identifying a general 'human interest' in *Rangifer,* the author argues that there is great potential for these three communities to work together. To demonstrate this idea, the paper reviews several examples of successful and unsuccessful 'alliances' between local peoples and both natural and social scientists which have had a fundamental impact upon the history of these sciences. The paper examines recent theorerical models which suggest that human action is a major factor in the behaviour and ecology of the animals. The paper also analyses the ideas of many indigenous people for whom there is no categorical difference between semi-domesticated, semi-sedentary and migratory *Rangifer* through comparison with many 'anomalous' texts in English and Russian language wildlife biology. By reviewing the history of scholarly interest in *Rangifer,* the author argues that contemporary models of *Rangifer* behaviour and identity could be 'revitalised' and 'recalibrated' rhrough the establishment of that dialogue between scientists and local peoples which so characterised the 19th century. Such a dialogue, it is argued, would help mediate many of the political conflicts now appearing in those districts where *Rangifer* migrate.

Key words: Arctic, caribou, indigenous peoples, migration, nomenclature, reindeer, systematics, traditional ecological knowledge.

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Introduction

Reindeer or caribou occupy a special place in the minds of scholars and hunters alike. There may be disagreement as to the degree to which *Rangifer* shape northern landscapes but it is impossible to imagine a boreal or tundra ecoscape not punctuated by this species. Migrating *Rangifer* leave a trail deeper than a simple trophic equation. They represent a powerful image which engages many communities in conversation. When rural peoples from all over the circumpolar North encounter one another, photographs of *Rangifer* provide an immediate focus for discussion. Images of *Rangifer* also link far more distant human communities. They often fan the imaginations of urban-based ecological activists intent on 'saving the caribou' and, of course, frame the dreams of small children in urban centres at Christmas time. One should expand this list to include, of course, the distant community of university scholars. In this paper I would like to reflect on the way that *Rangifer* represents a common human interest. By this I mean to show that anthropologists, biologists, local hunters and ecological activists alike to a great degree build their own nations, disciplines and identities through thinking about *Rangifer*. Moreover, I suggest that all this talk and action surrounding *Rangifer* by different human communities is a significant part of *Rangifer* ecology.

The content and style of this paper, is something of an experiment. It has been drafted by a 'social' scientist especially for the delegates of 10th Arctic Ungulate Conference, the vast majority of whom are trained as 'natural' scientists. In addition, it has been drafted by a peculiar anthropologist who not only believes that knowledge systems cannot be easily divided into 'natural' and 'social' types but who also believes that there exist forms of useful or important knowledge which cannot be tested or replicated empirically. These two beliefs have led me to adopt a point of view which may seem unexpected to many readers. First, I take it as self-evident that scholarly communities, like any other human community, have theit own histories, ide-

ologies and biases and therefore are open to anthropological and historical analysis. The first two sections of the paper, therefore, consist of a broad review of the scholarly literature generated on *Rangifer* in both biology and anthropology from the middle of the 19th century to the end of the last century. The review acknowledges the insights given to both biologists and anthropologists by aboriginal peoples but also indicates that four concepts in each discipline ('gradual social evolution', 'specific and sub-specific identity', 'caribou management' and 'calving-ground fidelity') represent contingent docttines that have had positive and negative implications for the communities involved with *Rangifer* populations. The conclusion that I draw from this analysis is a happy one: biologists, anthropologists and local 'aboriginal' hunters have historically demonstrated the ability to identify common interests in the study of *Rangifer*. At certain times, however, this alliance of interest has been silenced or even worked against the interests of science and of local peoples. Second, I present the idea that people play a great role in shaping landscapes and thus indirectly effect the size and nature of *Rangifer* populations. This idea is developed in the last section and in the conclusions of the paper. In these sections, as throughout the paper, I take as accepted many ideas taken from discussions with *Rangifer* hunters and herders throughout the circumpolar north. These ideas include the notion that there is no clear difference between semi-domesticated, semi-sedentaty and migratory *Rangifer* and that the behaviout of scholars, wildlife managers and local peoples has a strong influence on the migratory behaviour and the population structure of *Rangifer*, and that *Rangifer* movements and behaviour are best described with adjectives that ascribe intentionality and a certain subjectivity rather those which imply that they are solely physical entities. I take these ideas as starting points for a discussion; I do not baldly assert them. In the last two sections I make references to controversies and anomalies in the biological literature to indicate that the analysis of existing data is not so tidy and there is room for discussion of fundamental concepts. Finally, this paper takes a truly circumpolar perspective. A major part of my argument defending behavioural and various models of understanding *Rangifer* ecology rests upon the content of little known texts on *Rangifer* ethology and management published in the former Soviet Union between 1930 and 1970. This literature can be best described as 'anthropo-biological' since it is pitched midway between the 'natural' and 'social' sciences as understood in Europe and North America and also, interestingly enough, also midway between 'local knowledge' and 'professional scholarship'.

While composing this article it occurred to me that many of the arguments would strike the reader as 'anecdotal' (which is not necessarily bad, according to Blehr, 1997). This suspicion has come from some unsuccessful attempts at defending these ideas in the past and also from the fact that time did not permit me to elaborate ideas with graphs, charts, and long textual examples. However, unethical experiments upon two anonymous referees of an earlier version of this text established that many of the arguments came across not only as anecdotal but as unsubstantiated opinions which in one case were felt to be attacks on wildlife biology itself. This is not my intention and indeed is the opposite of my intention. The main argument of the paper is that it is possible to identify a general human interest in *Rangifer* which is scientifically rigorous, empirically based and ethically sound. I argue this through comparing the ideas on *Rangifer* of many human communities. I do not privilege either western biology or anthropology; indeed I argue that in different periods both have been culpable of gross exaggerations as well as capable of making fine and refined distinctions. Although I use several indigenous idioms I do not argue that 'local knowledge' is mysteriously more truthful than 'scientific knowledge' but instead suggest that the two are always in a partnership whether they like it or not. However, I will concede that if one strongly believes that a statistically based analysis of certain attributes of *Rangifer* is the only reliable way of knowing *Rangifer*, then this article might seem more controversial than most. Although I suspect that it is beyond my power to offer sufficient data to engage a strict statistical empiricist, I would offer only one observation: thete are many important issues in the relationship between *Rangifer* upon which decisions must be taken without empirical data. These range from setting a proper course of action before an irrupting caribou population to trying to guess the impact of various human activities upon *Rangifer* behaviour. Empirically, the only way objectively to understand the implications of one course of action or another is take a sample of a dozen populations and to deliberately let the populations crash, be disrupted, or to grow wild for want of human interaction. The results of these experiments would take

decades if not centuries to analyse and would, in the meantime, generate not only considerable hardship on local communities but would make most northern landscapes unrecognisable. It is difficult to prove if the error margins of the insights of an experienced caribou hunter are valid nine times out of ten. The purpose of this papet is to establish that these insights are valid at the very least as hypotheses and at the most as examples of successful strategies of action which, to a great degree, are responsible for cultivating the populations of *Rangifer* which we are all analysing.

Although the paper may already seem impossibly broad, I have concentrated on the litetature describing three specific contexts: the situations of the Taimyr and the northern Yukon (Porcupine River) populations of wild reindeer/caribou - of which I have direct experience - and the northern Québec/ Labrador population, of which I have learned exclusively through library research. For my purposes these three examples are interesting for they are all 'classic' cases of the theoretical and practical problems created for people by large populations of migratory *Rangifer*. All thtee cases are complex. In two of the three, semi-domesticatedated *Rangifer* form part of the story of these populations. Furthermore, in each setting there is an understudied interaction with sedentary 'woodland' caribou which always fit rather uncomfortably in models of migration and herd identity. Finally, in all three settings these populations have been directed, managed and even harnessed by a multitude of human hosts. All three populations travel through multiple political jurisdictions - in one case over an international border. In each case more than one indigenous population takes responsibility for setting ethical relarionships to the animals. Furthermore, each of the three situations have become laboratories for the efficiency of three different state management regimes: rational nature-use in Taimyr, co-management in the northern Yukon and classical wildlife management in northern Québec/Labrador. However this choice is somewhat arbitrary and I have been forced to leave out interesting literature on Scandinavian and Kola peninsula populations and the large literature on other Alaskan and Canadian barren-ground populations.

A short history of *Rangifer* and circumpolar interests

To paraphrase the title of a classic paper on caribou management, *Rangifer* are universally a 'vital tesource' (Allison, 1978). Whether one is drinking tea in a reindeer-skin Evenki tepee (*d'iu*) in the heart of the Putoran plateau, or watching the movements of groups of bull caribou while sitting in a half-ton truck on the Dempstet highway, people all across the Arctic rim are vitally concerned with observing, consuming and knowing this living resource. Caribou provide an important element of the diet of aboriginal families world-wide. In Siberia, Alaska, and Scandinavia, semi-domesticatedated *Rangifer* provide an important source of income and pride for local families. *Rangifer* world-wide help people in arguments about their identity. With the rise in power of circumpolar state administrations after the Second World War, and more recently with the rise of First Nation, Siberian, and Sami nationalism, control over the range of *Rangifer* has become important in discussions about managing landscapes, people and the future of nations. At the end of this century a common emblem of authority for administrators, northern politicians, or entite First Nations themselves is the silhouette of a (male) reindeer or caribou on the cover of a report, the corner of letterhead, or in the centre of a flag or a shirt.

Since the turn of the century there have been thtee peaks of interest among Eurasian, North American and European students of *Rangifer*. Each peak generated a great deal of literature and in some cases has created entirely new ecological relationships. Ironically, the very earliest circumpolar discussions were the most comptehensive in terms of the exchange of ideas between local hunters, biologists and anthropologists. In the post-War period, alrhough there has perhaps been an increase in citations concerning *Rangifer*, thete has been a considerable reticence by scholars and local peoples to engage with each other's terminology. With the relaxing of Cold War tensions and the rise of co-management regimes throughout the North, the present millennium now promises a return to an intensive exchange of ideas on the significance of *Rangifer* between all circumpolar communities.

The last century began with extreme curiosity about the concrete cultutal, biological, and zoological links which united the continents. This period is perhaps one of the better examples of a dialogue between local observers and scholars, as represented in the ethnographic works of the Committee on the Notthwest Tribes of Canada (Tylor, 1884) or the Jesup North Pacific expedition (Bogoras, 1902; 1929; Krupnik, 1996). Both of these collaborative endeavours strove to understand the social evolu-

tionary status of peoples living along both sides the Pacific Rim. Within wildlife biology, circumpolar interest was represented in the ambitious cultural experiment of translocating of semi-domesticated *Rangifer* from Siberia to Alaska and Canada at the behest of northern administrations with the help of Sami herdsmen (Olson, 1969). The early circumpolar interest can best be categorised as an interest in cataloguing and cultivating northern landscapes. Both the ethnological and biological literature stress the need to identify the languages and habits of local peoples precisely as well as the need to focus upon suitable species for 'taming the frontier'. Specific policy actions connected to these scholarly studies had far reaching ecological effects. In the case of ethnology, early investigation of the language and identity of aboriginal peoples would eventually alter the human geography of whole nations as hunters were settled into villages or special enclaves which were not always designed to respect the specific relationships that those communities maintained with the lands and animals around them. In the case of biology, the penetration of Sami and Siberian knowledge of semi-domesticated *Rangifer* into a setting dominated by migratory *Rangifer*, introduced a different type of ecological concern over the carrying capacity of the land and the significance of migrations and also created a new setting in which to discuss *Rangifer*. As will be argued below, the influential ideas of 'calving ground fidelity' can be traced to this particular penetration of ideas on how to manage migratory and semi-domesticated *Rangifer*. Interestingly, it is also in this period of closest collaboration between scholars and indigenous people that there is the most experimentation with trying to typify and distinguish different sub-species of *Rangifer*. Dialogues with local communities considerably enrich the vocabulary of scholars with respect to sub-species and ecotype.

Directly following the Russian Revolution and Civil War, there was an intense, competitive interest in various ways of harnessing wild and tame reindeer for social development. This literature is perhaps best illustrated in the pages of the journal *Sovetskoe olenevodstvo* [the Soviet Reindeer Industry] ot in the typescript volumes of the Canadian Royal Commission to Investigate the Possibilities of the Reindeer and Musk-Ox Industries (Canada, 1922). This literature, while continuing certain economic interests from the turn of the century, treats indigenous ideas concerning *Rangifer* and muskoxen as obstacles to the intensification of northern production. Indigenous people figure as rather passive recipients of knowledge and aid rather than active partners in the 'taming of the frontier'. Within this period the works of scholars from both sides of the growing circumpolar political divide are cited to establish legitimate parameters of study and state action. It is to this period that one can identify the origins of state wildlife management in both Russia and North America.

One can identify the most attentive reading of information on communities vitally linked to *Rangifer* at the height of the Cold War. In the 1960s, North American biologists such as Pruitt (1960) or Lent (1966) integrated into their own work Formozov's (1946) research on the effects of snow-cover on migrations or Michurin's (1963; 1965) work on systematics. Although ethnographers in the same period rarely wrote comparative works, there was an implicit applied interest in identifying advantageous development policies for local communities. In Russia, state policy was held to be a positive example of how technology and science could integrate *Rangifer* hunting and herding into a national economy (Gurvich, 1961; 1977). In Canada and Alaska, research was conducted into how best to separate *Rangifer* hunting (and to a lesser degree, herding) into 'subsistence' sectors which operated independently from general market principles (Baliksi, 1963; Nelson, 1973; Caulfield, 1983). In terms of action, in the late 1960s and 1970s the circumpolar states invested in several experiments in the translocation of muskoxen ostensibly to make the high Arctic landscape more 'economically useful' (Yakushkin, 1978; Klein, 1988). It was also in this period a classic text of Syroechekovskii (1975; 1984) concerning the 'rational use' of wild *Rangifer* was made available in English translation. This period, corresponding to the apex of the period of state wildlife management, is peculiar for its aggressive neglecting of local traditions. While the indigenous hunter was seen to have a romantic aura at the beginning of the century, or a simple nature before the Second World War, in the 1960s and 1970s aboriginal peoples were held to be culpable for the potential or past extinction of circumpolar species such as the bison and the musk-ox (Hone, 1934; Owen-Smith, 1987; Morgan, 1997; Isenberg, 1997; Lent, 1998). Thus this period is characterised not so much as by an economic interest in *Rangifer* but by a highly centralised and protective interest in the animal aligned to the imperatives of state

building. Interestingly, in terms of a social history of the study of systematics, this period of centralisation coincides with consensus on there being a single generic and specific type of *Rangifer* which could be measuted and undetstood without much attention to local categories and observations. This conviction, characteristic of Notth American wildlife biology, has caused some debate concerning the proper way to divide up populations.

Although these 'peaks' of intetest have generated significant work and discussion, the reticence with which scholars based in the circumpolar region have used this material in model building is remarkable. These diplomatic (or perhaps 'cold') rules of engagement between circumpolar sites are most obvious in works published in Russian or English, where authors from both intellectual traditions conservatively acknowledge the presence or absence of parallel research initiatives but rarely experiment with each other's concepts or terms. This lack of engagement is most noticeable, sadly, in my own field where the number of fundamental comparative works in circumpolar ethnography since the Jesup expedition can be counted on two hands (Schweitzer, 1993; 2000). Recent North American concepts of 'herd' as defined by a 'calving ground' are not compared with the behaviourally based Russian aggregate of *stado.* Fundamental articles on *Rangifer* are more concerned with raw population statistics (Klein & Kuzyakin, 1982, Williams & Heard, 1986) or 'impacts' and 'management' (Klein, 1991; 1996) rather than the subtler issues of migratory behaviour or collaborative arrangements with indigenous communities. What is also noticeable in this cold 'lack of engagement' between circumpolar scholars is the marked contrast with the passion local peoples display fot details of the lives of their animal neighbours. Thanks to the Inuit Circumpolar Conference, the International Working Group on Indigenous Affairs and the growing intetest of many local aboriginal land-claim administrations, aboriginal 'wildlife users' are becoming more aware of common problems with heavy metal contaminants, the dislocating effects of resettlement policies and the bitter-sweet experiments with wildlife management and the construction of parks and wildlife reserves. I expect that it is this locally generated agenda of a circumpolar intetest which will generate renewed interest in the skills of anthropologists and biologists alike.

To summatise, in studying the history of scholarly interests in *Rangifer* it is possible to identify three tendencies. First, though *Rangifer* are a universal interest for all circumpolar communities, there has been a growing reticence to acknowledge and incorporate the models of indigenous thinkers in scholarly discussions on *Rangifer.* As will be argued below, even recent attempts to recognise 'traditional ecological knowledge' (TEK) or 'local knowledge' pale before the quality of collaboration that one can identify at the start of the last century. Second, the set of published analyses among scholars tends to reflect the interests of state administrations in a region that has increasingly become an arena of political tension rather than the interests of local populations. Finally, the future of scholarly interests in this region and this species will depend upon 'revitalising' academic models such that they reflect the lived interest in *Rangifer* of local communities. I have not identified these three tendencies in order to criticise the validity of current models or to predict the course of future developments. However this short overview suggests that scientific interests can be calibrated to reflect general circumpolar interests be they those of nation-states, local communities, or various definitions of economic development. This raises the question of what is the best calibration?

Calibrating scientific interest in *Rangifer*

There has been much debate on how scientific interests in *Rangifer* differ epistemologically from the 'knowledge' of local rural hunters (Freeman & Carbyn, 1988; Osherenko, 1988; Thomas & Schaefer, 1991; Stevenson, 1996, Johnson, 1992). Typically, scientifically valid statements about the world are felt to be empirical and quantitative. They ate said to be honed to provide predictive models which can be proven or disproved and, in the field of wildlife biology at least, they focus upon the parameters of the population in question in terms of discreteness, size and recruitment. By contrast, models which ate extrapolated from 'tradition' or 'local knowledge' are primarily qualitative. They are oriented towards providing a guide for ethical action and, with respect to *Rangifer*, tend to be more interested in describing the behaviour of groups or bands without much reference to the macropopulation. While in many specific cases it is clear that there is a difference in approach, there is growing literature which disputes the extent to which differing methodologies amount to a different way of knowing (Agrawal, 1995; Cruikshank, 1998;

Dorais *et al.,* 1998). Although I agree with the general lines of this critique, it is not my intention to weigh the various philosophical and political arguments within this literatute. Instead I wish to take a shortcut to two simple points. First, some of the most 'scientific' concepts have their roots in the most 'traditional' settings. I suggest that groups of people who today distinguish themselves as *Rangifer* 'scientists' and 'users' have been knowingly or unknowingly engaged in a sort of partnetship of inquiry for at least a century. Second, in sketching out this history I point out that the scientific interest can be calibrated or focused in multiple ways which may either emphasise or ignore the pragmatic interests of local groups. It is possible that both 'users' and 'scientists' may recently have developed radically different paradigms of perception and action.

This analysis in the history of *Rangifer* scholarship focuses on four examples, two of which have had positive implications for this alliance and two of which have had negative implications. In tetms of a positive alliance I will cite the examples of the earliest work which went into distinguishing *Rangifer* genus itself, and the most recent investigation into the relationship of *Rangifer* to the so-called 'calving grounds'. In both instances I argue that the mingling of 'local knowledge' and 'science' is so complete that the two merge into one. In tetms of a negative alliance I will cite the powerful idea of 'gradualist social evolution' and the no less influential idea that wildlife can and should be managed by state administrations. In both cases I make the argument that although indigenous perspectives may seem to be silenced or even rejected by these models, images of indigenous hunters nevertheless play a crucial role in each theory. In short, Europe could never crown the scale of social evolution without first becoming obsessed with the idea that eatly man chased a *migratory* animal. Neither could state managers establish their authority without first creating the image of the careless or childish caribou hunter. Thus in all four examples, indigenous images are harnessed at all levels in the creation of *Rangifer* science in what ate described as principled and unprincipled ways.

Rangifer *and social evolution*

Rangifer tarandus is a classic 'species' in both the history of zoology as in the history of anthropology. One finds thoughts on the significance of reindeer and caribou structuting the very foundations of each profession. In the first and successive editions of his *Systema Naturae* (1758-1767), Linnaeus identified a circumpolar type (*Cervus tarandus*) on the basis of his own observations of Scandinavian wild and domesticated reindeer and on written reports of barren-ground caribou (Banfield, 1961: 48, 53). The origins of social anthropology can be traced to same period. The awareness of the mutability of human society and the human form itself owes its origin to 18th and 19th century meditations on the significance of *Rangifer* remains found in early archaeological sites. Early theorists wove broad theories linking the material remains left by early *Rangifer* hunters within caves in France and explorers' accounts of actual *Rangifer* hunters in the expanding European colonies (Trigger, 1989). This alliance of interest is often acknowledged in the foreword of major works on wildlife biology. The enigma of cave drawings, or the account of a caribou corral, are often used to draw readers into an analysis of the systematics ot population dynamics of a species representing a common and ancient human interest (Murie, 1935; Banfield, 1961). The early appearance of *Rangifer* in the scientific imagination is more than an historical curiosity; it has influenced many concepts which still shape scientific action.

The most impottant but capricious 'alliance' between anthropologists, taxonomists, and indigenous peoples revolves around one of the roots of positivistic science today - evolution. The fitst collections of chipped stones and split legbones found in France were equally useful as proof of a Biblical flood as they were of the presence of an ancient culture of reindeer hunters. It required a considerable marshalling of geological, palaeontological and archaeological evidence to establish that the landscape had as deep a history in terms of time as it had extended engagement with the human form (Eiseley, 1961; Stocking, 1987). To pull examples from only one prominent populariser of gradualist evolution, Sir John Lubbock (1890: 300-304; 1978: 26), lithographic reproductions of cave drawings of reindeer hunts were superimposed upon descriptions of primitive abattoirs in an effort to prod the Victorian public to accept the idea that human activity is not only recognisable in subterranean digs but also the simpler point that human activity had progressed. This was on the whole successful. As is well known in overviews of the history of 'civilisation', the movement from hunting *Rangifer* to herding them captures the dawn of the first organised productive activity (Childe, 1951: Ch.6).

Rangifer hunter/herders are said to straddle humanity's first adventute into rational productive activity (Ingold, 1980: Ch.2). There were two inescapable conclusions to be drawn from the successfully negotiated image of 'man' as the Palaeolithic hunter: first, that time brought with it progressive change and second, that people maintaining a relationship with wild *Rangifer* were already living in the past.

There are numerous printed examples of how eatly travellers, ethnologists, biologists and publishers used descriptions of *Rangifer* and their human hosts to contradictoty ends. On the one hand, those people who hunted of wild migratoty *Rangifer* provided prosaic material illustrating the independence and hardiness of the human spirit and yet were almost invariably classified as the most 'savage' and 'primitive' in the newly discovered spectrum of human forms. On the other, those who herded semi-domesticated *Rangifer* were respected as founders of humankind's productive tradition. Of the nations consideted in this papet, Gwich'ins (*Dene dinje, Loucheux*), despite their refined and respectful way of attending to migratory *Rangifer,* play the thankless role in early ethnography of representing a society which contrasts so much with civilisation as to define it. Two early anthropological theorists neatly capture the consensus by citing Gwich'ins as 'the most northern, the most bastardised but also the most ptimitive of Indians' (Durkheim & Mauss, 1975: 63). In another widely read account of diffusionist evolution, these 'undoubtedly really primitive' people serve as bookends to the epic stoty of mankind, appearing only twice; at the beginning and the end (Perry, 1923: 5, 469). For Frank Speck (1935: 3), who wrote in other respects a very sensitive ethnography of Naskapi religion and ritual, the 'savage hunters of the Labrador peninsula' represented 'the borderline of the life of the past merging with that of the present'. Reindeer herders, although often employing similar concepts and rituals to Gwich'in and Naskapi hunters (and often hunting similar populations of caribou) were described with much mote sympathy. Although the teindeer herders of Taimyr were described in Middendorf's (1859: 489-90) volume on fauna, they were praised for the 'economic' use of their deer right until 'the last drop of blood'. Moreover, their talents of domestication were placed at a higher 'level of development' than the 'primitive' people of Notth America. Kastten (1860: 343) went as far to identify Yenisei Evenkis as 'aristocrats of the tundra'.

The adjectives used by these turn-of-the century authors seem so naive and exaggerated that they are now politely ignored when reading these fundamental works. However, it is important to recognise that these examples were not chosen randomly or in error by these theorists. Our scholarly forerunners in geology, biology and ethnology were under great pressure to exaggerate the mutability of human relationships to drive home the point that evolution, both biological and social, existed. Furthet, these examples were not simple cases of overzealousness. There was an important subtlety in this struggle. Any admission by an author of too much kinship between utban Europeans and the savage hunters ran the risk of discrediting the arguments of liberal evolutionists (Stocking, 1971). Strategically, the hunt of the wild caribou needed to be represented as a 'survival' of past forms of adaptation linked to the 'impulsive' and 'childlike' behaviour of savages in order to underscore the rational natute of agricultural or industrial society (see Lubbock, 1978 [1870]). Thus, from an early date, scientific workers entered into a marriage of convenience with *Rangifer* huntets. Hunters and their prey might be studied with intetest and with sympathy but the authority of scientific classification was vested in the itrationality of the modern savage. Although the relationship between proponents of TEK and those of applied zoology might be a warmer one today, it is not new.

The genus Rangifer

The sttategic exaggerations in early evolutionist thought belies the implicit evidence of strong collaboration between travellers field explorers and the so-called savages. As Banfield (1961) illustrates throughout his 'tevision' of the genus, the earliest systematics of *Rangifer* owe much to the insight of local rural huntets in various encounters with explorers going back to 1487. The first sample of *R. t. pearyi* made its way to a museum as a gift from Samuel Hearne's Chipewyan guide (Glover, 1960). The earliest prototype of Gmelin's *R. t. caribou* was chased down by a group of Métis huntets on the St. Lawrence river (Banfield, 1961: 78). Although Linnaeus reputedly made his own observations of Sami reindeer, one wonders how his attention might have been directed if there were not Sami present. At a more conceptual level, thoughts on the identity and discreteness of the species have been guided by debates with aboriginal informants. The concept of the 'woodland caribou' comes from a Micmac

behavioural category of 'the shoveller' (Wright, 1929). According to Banfield (1961: 3), the generic name for the species, *Rangifer*, is derived from a Sami tetm for a young reindeer. A quick glance at history of the regional distribution of specific and sub-specific categorisation of the genus shows that the first classic encounters between the biologist and *Rangifer* occurred at the main cross-roads of Western and Eastern trade colonialism: the St. Lawrence river valley, the Mackenzie Delta, the Ob' river system, the Seward peninsula. The idea of *Rangifer* itself, in contrast to the idea of social evolution, is a product of a successful calibration of scientific with local intetests.

'The Buffalo of the North'

There is a remarkable reconfiguration of scientific interests in wildlife following the world wars. Paralleling the growth of new, nationalistic political orders there also appears a new idea that states must play a major role in the regulation of relationships between people, animals and national territories. Anthropology itself was created as a profession during this period through the successful argument that professional ethnological observation was useful to governments interested in regulating cultutally diverse rural populations (Stocking, 1971; Kuklick, 1991). Although biology and zoology have rather older professional genealogies, the idea that a government should support professional and applied wildlife biology dates from this post-colonial period (Feit, 1998). The interests of scholars in making their research areas into professions would lead to pethaps the sttictest and least principled alliance with local populations.

The paternal issue at the heatt of this new intellectual development was the responsibility of states in the protection of rural peoples and rural species. The seminal idea of this movement, like with the campaign to propagate an idea of social evolution, was the need to wrest control of environments from local residents and to vest it in the hands of properly trained professionals. The management of *Rangifer* became the test case for demonstrating the viability of this idea.

In order to justify this idea, thete was a need to develop a negative example. The sad fate of another migratory species was useful in this exercise: the near extinction of the North American bison. Through a revival of earlier 18th century stereotypes of savage life, it became a common (but untested) assumption that with the provision of new technologies, such as rifles, the undisciplined nature of rural peoples would lead to the extinction of another rather romantically portrayed migratory animal (Trudel, 1985; Kelsall, 1968). Without dwelling on the history of this issue, it is a remarkable exercise to trace the genealogy of this example in the very first works of wildlife biology on two continents. In both Canada and Russia, *Rangifer* came to be thought of as 'the buffalo of the North' (e.g. Bergerud *et al.*, 1984).

The foundation of *Rangifer* management in Canada came from a series of widely read works in Canada by A.W.F Banfield (1954; 1956) which made the strong argument that there was a 'caribou crisis' stemming from the overkill of 'barren ground' caribou by native hunters in Labrador and the Northwest Territories. It was surmised that without immediate state action, the nomadic caribou would soon disappear as had the North American bison. The actions of the growing Canadian state apparatus were swift. In the late 1950s the Canadian Technical Committee for the Preservation of the Caribou was formed to discuss various courses of action ranging from the organisation of the first tagging of migrating caribou (Thomas, 1969; Parker, 1972; Kofinas, 1998: 87-88) to proposed restrictions on the sale and export of caribou skin clothing (Banfield, 1950). As with an earlier epoch, the telation between people and *Rangifer* and the perceived lack of restraint and foresight on the part of a population which came to be called 'users' would only bolster the authority and power of a closed group of managers. The strategy of identifying caribou crises has framed the success of wildlife biology from the original 1954 barren ground crisis (Banfield, 1954), the 1983 Kaminuriak and Beverly caribou crisis (Miller, 1983) through to the most recent 1993 Porcupine caribou crisis (Kofinas, 1998).

In the former Soviet Union, the distinct management school proposing the 'rational use of natural resources' (Kriuchkov, 1973; Syroechekovskii, 1984; 1990) was justified by similar anxieties of mass extinctions at the hands of improvident citizens. In one of the founding works of this school, Kriuchkov (1973: 40) prominently cites the example of the extermination of the North Amettican bison to support his vision of the planned use of the landscape (for an even more exaggerated post-Soviet example see Zimov & Chuprynin, 1991: 94). As is well known, the proposals and the actions of managers in the fotmer Soviet Union differed in em-

phasis but not in theme. Instead of protecting populations of migratory *Rangifer*, the solution most often proposed was the zoning of human use such that significant portions of the landscape are reserved for the exclusive use of semi-domesticated *Rangifer* while other portions become the settings for massive industrial hunts of the migtatory population (Sdobnikov, 1958; Andteev, 1968, Syroechkovskii, 1986; Pavlov, 1983; Klein, 1980).

The rhetoric of the rational use of nature, as practised in Taimyr, is different from the rhetoric of 'caribou preservation'. Indeed, it is an interesting example for my purposes that the recommended methods of slaughter at river crossings are in fact industrialised versions of traditional Dolgan and Nganasan techniques of harvesting migrating *Rangifer* (Gracheva & Kholbystin, 1984). Despite a somewhat different engagement with *Rangifer*, and with local hunters, the political structure of this school of management is nonetheless quite similar to that of their North American colleagues. Decisions as to the timing and scale of slaughters and the location of zones reserved for semi-domesticated and wild *Rangifer*, are made in centralised locations by professional rural economists and not in local communities. The justification for rigid control over land use came from the example of chaotic use illustrated by the myth of the bison.

While the notion of a crisis can be lauded for directing state resources to the study of *Rangifer*, it should be treated carefully as a scientific strategy. I characterise it as an unprincipled alliance of scholarly and local intetests for four reasons. First, although the notion of a crisis speaks to in imputed 'global' interest in the preservation of a species it can only do so through the allied but and silent presence of local 'users'. As Feit (1998) identifies clearly, wildlife managers pitch their arguments on behalf of the wildlife or the 'ecosystem' (and not the people) but can only justify practical action in the name of future generations of people (and not in the name of the landscape). As much as local interests are ignored, or in fact seen as contradictory to the preservation of the species, they are necessary to make an effective atgument for management. This is a rhetorical strategy which contrasts remarkably with botany and medicine, both of which also tequire the participation of local populations and both of which have managed to negotiate a more equal alliance (Feit, 1998). Second, as has been often noted in histories of past crises, the predictions of the imminent collapse of a population is often followed by unexpected jumps in the population - often to quite unsustainable levels. This criticism points out not deception but a lack of humility in developing and applying technical tools in complex environments. It would seem that in each setting whete a crisis has been declared there has been a corresponding jump in population size several years later as a tesult eithet of curtailed hunting or better measurement techniques. This was the case in Québec/Labrador (compare Banfield, 1954, Juniper, 1975, and Courtier *et al.,* 1990), the barren grounds (compare Banfield, 1956, Gates, 1985 and Osherenko, 1988) and the Taimyr peninsula (compare Syroechekovskii, 1966; Andreev, 1983; and Pavlov, 1996). Thirdly, the idiom of a crisis, as efficient as it is in attracting the attention of distant administrators, weakens the basis for future collaboration. The time frame of a crisis implies quick, coarse and estimated action but not discussion and consensus between actors. Finally, restrictions on hunting which may come out of an imputed crisis in fact change the ecology of *Rangifer* itself. In addition to restricted predation, restrictions on hunting through licenses, committees and outright bans change the relationship between people and deer. In most cases, sttict regulations interfere with the ttansfer of skills from elders to youth. Encounters with *Rangifer* might be re-centred in new localities, or become 'grey' activities. Although it is difficult to identify a ditect correlation, it is interesting to note that as local communities become increasingly regulated by wildlife regimes, whose goal is to rationalise the hunting of *Rangifer*, the population structure and migratory behaviour of *Rangifer* becomes more chaotic, unpredictable and unmanageable.

Rangifer *'herds'*

Although from the very first identification of the species in the 18th century it has been clear to both local huntets and European ttavellers that *Rangifer* often move in large and impressive aggregations, the nature of those aggregations has been a source of debate. In North America it has become an unquestioned doctrine that 'sub-populations' of migratory *Rangifer* can be identified on the basis of their 'fidelity' to a 'traditional calving ground' (Skoog, 1968; Davis *et al.,* 1986). This idea, which seems to have appeared in print in the mid-1950s with reference to the border region between Alaska and the Yukon Territory, is a second positive example of a

successful collaboration between applied scholars and local hunters.

It has proven difficult to research the genesis of the widely-accepted idea of 'calving ground fidelity'. The classic citations for the idea of distinguishing populations by the repeated or regular use of specific calving grounds are Skoog (1968: 213-14) and Thomas, 1969: 7). It is significant that although each of these authors name populations by the name of a prominent lake or river located at the calving 'area' each of them keep the idea of a subpopulation separate from the idea of a herd. LeResche (1975: 127), Lent (1966: 484), and Kofinas (1998: 170-171) indicate that this idea was general knowledge among Alaskans throughout the 1950s. Earlier glimpses of the idea are said to be found in the works of Munro (1953), Skoog (1957; 1962) and (Buckley, 1957) but I have not been able to verify these citations.

Local informants have played an active role in identifying exactly which deer are 'different'. The clearest cases of aboriginal involvement in the doctrine of herd identity is in the case of the 'George River', 'Leaf River' and 'Torngat Mountain' populations of *R. t. caribou.* In northern Québec/Labrador Low (1897) and Elton (1942: 363) first popularised the idea that there are three 'herds' which overwinter in discrete ranges in Northern Québec. Both were drawn to these observations by a nameless Naskapi informant (Low 1897: 319). Although local peoples were most likely aware of regions where caribou preferred to drop calves, the search for 'the calving grounds' was only undertaken in 1973 (Goudreault, 1985: 246). The idea of Québec/Labrador caribou herds as a distinct units identified with a discrete space was published for the first time at the end of the 1970s (Juniper, 1979; Le Henaff, 1980). As late as 1988 'incidental reports from residents of the Ungava' led to the formal identification of another population, the Torngat Mountain herd (Schaefer & Luttich, 1998: 486).

The penettation of local knowledge and the process of identifying calving grounds seems tighter in the northern Yukon, although the process is poorly described. Murie (1935: 68-9) in his discussion of the 'migratory habit' of 'northern herds' quotes an 'Indian at Old Crow Village' who identified that the caribou of the Northetn Yukon effected North-South migrations to the coast where they 'mingled' with other groups. The recent work of Kofinas (1998: 171) implies Gwich'in hunters spoke consistently of a preference of *R. t. granti* for certain calving 'localities' as early as the 1940s but that this idea was acted upon by biologists only at the end of the 1950s. His interpretation comes from a rather nicely phtased manuscript observation by the local hunter Knut Lang (1952):

I believe, as many old natives do too, that the cows prefer to return to localities where they raised their fawns the previous year, and the young animals like to return to the parts in which they were born, if no serious hazards prevent it.

This statement refers to discussions with Inuvialiut and Gwich'in people from 1922-52. Although this observation is quoted in at least in one significant Canadian government report (Kevan, 1970), thete is no surviving evidence that it or the three decades of previous personal communications had any direct effect on biological field work or concept formation. The only hint of past debates is the rather modest and defensive way that the old man insists on this idea, as if he already realised that he was contradicting closely held ideas. The final signpost to the possible existence of a dialogue with local hunters as to the migratory behaviour of populations comes from the classic wotk of Ronald Skoog (1968) himself. In the two places in his epic dissertation on Alaskan caribou where he discusses the propensity of caribou to teturn to regular calving grounds (pp. 103, 213-14) he exclusively cites literature referring to the calving habits of semi-domesticated reindeer either in Alaska or in Scandinavia. His key authority is Palmer (1926). This general model of comparing the behaviour of semi-domesticated reindeer with migratory caribou is hallmark of the thought of aboriginal reindeer herders.

What is remarkable about this example of the blending of local and scholarly thought is not the seamless and anonymous manner in which it occurred but the speed with which it took root. From an epicentre in Alaska and the northern Yukon, the idea diffused quickly across North America such that by the mid 1970's the entire map of North America had been divided up into a series of bounded and generally mutually exclusive territories representing the 'range' of a particular 'herds' (see, for example, Hemming, 1971; Williams & Heard, 1986; Crête *et al.*, 1990). This development, again to the eyes of an outsider, tepresents a remarkably quick adoption of a common paradigm and a corresponding rapid amnesia concerning older models of caribou and reindeer dispersion. For example,

pre-1960 works tend to characterise caribou movements as 'nomadic' and somewhat unpredictable. The images range from subtle descriptions of relative density (see the illustration in Murie, 1935: 51) to nuclear-age comparisons with effect of magnetic fields upon metal filings (Banfield, 1954: 17). There seems to have been much ambivalence concerning the sudden appearance of the hypothesis of calving ground fidelity as a description of caribou social sttucture. The fine ethological work by Pruitt (I960) uses the words 'herd' and 'calving ground' in quotations. Banfield's (1954: 23) flatly rejects the idea that there can be a concrete 'calving ground' and instead describes populations of cows dropping calves in a wide arc along a general trajectory. Skoog (1968: 213) identifies herds as sub-populations displaying 'an attachment of sorts, for certain portions of their range (especially the calving grounds)' [parenthesis in the original] and in his srrongest formulation uses an optical metaphor to categorise them as merely 'a focal point for population dispersion' (1968: 202) and 'which encompasses the best habitat' (1968: 356). Skoog's seminal idea of a 'focal' point is perhaps the most honest rendition of a ptocess whereby a complex set of ideas was very quickly 'refocused' in order to emphasise management priorities. In the next section, I question whether the notion of there being calving 'grounds' which attract 'discrete and fidel herds' is a useful concept from the point of view of local interests.

I do not think that in identifying fout discrete instances of overt or implicit conversations between scholars and local peoples that I am establishing anything new. In this boreal community of scholars and locals, as in any other community, it is only common sense that these interested groups should be talking, comparing models, and learning from each other. However in discussions about the comparative virtues of 'science' and 'TEK' it seems that this element of common sense sometimes gets overlooked. Rather than inventing a story of a recent *détente* between these groups, which are now often colleagues on co-management boards, it is useful to reflect on the fact that there has been conversation for at least a century.

Scientific interest is not a stable configuration but one that can be calibrated, focused or refocused. It is, therefore, possible to identify a set of unprincipled calibrations orchestrated to tatify certain 'globalising' conceptions - such as gradualist evolution or the idea of a 'caribou [population] crisis'. Ingold (1993) argues that concepts which seek to attain such an overarching perspective risk negating the meaning and indeed the purpose of scholarly inquiry. At the same time, it is possible to identify cases where local and academic voices merge and become indistinguishable as in the two examples of the identification of discrete forms and identity structure of *Rangifer* populations. By establishing that scientific interests have a rather wide 'focal length' for calibration, it becomes possible to argue for more negotiation on what exactly are the common human intetests in deer, rather than assuming that 'TEK' and 'science' are mutually exclusive bodies of thought. Like wild and semi-domesticated *Rangifer*, 'TEK' and 'science' ate in fact part of the same species.

Finally, this history of collaboration between scholars and local peoples can be used to illustrate one important point in the controversy between 'scientists' and proponents of 'TEK'. If there is any widely recognised role for 'TEK' in the study of *Rangifer* it is merely as one technique of many which is useful for gathering data on the nature of reindeer and caribou. In contemporary studies of co-managed populations, the words of knowledgeable elders occupy equal space but do not carry equal weight among graphic representations of tecent aerial surveys or satellite tracked deer (Kofinas, 1998; GRRB, 1997; Legat *et al.,* 1997). If there is any single lesson to be learned from studying the history of collaboration between scholars and local people it is that the insights generated from these conversations are far more profound that just a set of statements which can later be proved or disproved by experimental techniques. Instead, these conversations tend to lead to the heart of the matter through the formation of several fundamental categories and by direct attention of field workers to significant observations. Thus if 'TEK' is to be merely one of many techniques in the tepertoire of scientific study, this will represent a considerable demotion in the significance of this dialogue. Instead of condensing 'ttaditional ecological knowledge' into a neat acronym which merely supports more articulate expositions of population dynamics or systematics, it would seem mote appropriate that this alliance of interest be spelled out in detail.

Human interests in the cultivation of *Rangifer*

Up until this point I have considered the history of common interest between anthropologists, biolo-

gists and local huntets and herders which has manifested itself in historically stable categories of 'herd fidelity', systematics and somewhat less fortunate ideas of management and social evolution. I have also suggested that the history of this common interest can be categorised as a series of principled and unprincipled alliances. In considering the shape of possible future alliances I will instead concenttate upon recent instances where scientific interests are becoming calibrated with those of local communities. I have deliberately chosen the word 'cultivation' for this process both to underscore the idea that hunting, too, is a productive activity but also the idea that 'savage' local thoughts are also 'cultivated'. Many examples of this cultivation have been exercised by local populations for centuries and have now been undertaken anew by regional and First Nations administrations engaged in co-management of local populations of *Rangifer*.

Cultivating difference in Rangifet

As has been implicit from the start, I have not made a strict division between migratory, semi-sedentaty, and semi-domesticated *Rangifer* but have followed Evenki and Dolgan consultants by considering them all a single species with differing behavioural and ecological qualities. The lumping of vatious sub-types of *Rangifer* as members of a single taxonomic species has been accepted since the works of Flerov (1922), Sokolov (1937) and Banfield (1961). Moreover, as every reindeer herder knows and fears, there is very little practical difference between the physiology of so-called wild and semi-domesticated populations such that the unexpected arrival of migratory *Rangifer* in the Spring or during the tut can easily break the domestic hold that people hold over 'tame' *Rangifer*.

There are of course some important morphological differences between semi-domesticated reindeer and migratory caribou which may stem from the domestication project itself. World-wide, biologists and herders distinguish the reproductive cycles of the two sub-species such that the rut and calving period is separated by several weeks. Without this crucial difference it might prove impossible to hold semi-domesticated *Rangifer* at all. As Banfield (1961) notes, in Siberia, pelage is an important second qualifier. Finally, as Druri (1949: 60-61) notes, the differing travels of the two types imply that migratory *Rangifer* are a better fed and better culled which holds implications for their behaviour and health.

Despite the current unanimity on the circumpolar specificity of *Rangifer*, there has been a remarkable elaboration of the debate as to which groups nevertheless represent different discrete sub-species. Again, this debate is not surprising from either a scholatly or practical viewpoint. To argue that all *Rangifer* are similar is not to argue that they are the same. However what is surprising is the fact that authoritatively ratified differences at this level tend to hinge upon identifying elusive morphological qualities, which are closed to modification, tathet than considering behavioural qualification, which are open to cultivation. It is this aspect which turns out to be most at odds with both local assumptions but local interests. This sub-section will argue that a range of historically stable behavioutal traits in *Rangifer* constitute a distinction which can be useful in organising scholarly activity. It will do so through a set of questions on motphology and with examples of successful biological studies of behaviour.

The manner in which various taxonomic clines are segregated, or gradually blend, is a mystifying area for an anthropologist reading the zoological literature. In tracing the history of these ideas it would seem that the search for objective typological considetations are not entirely unrelated to the search for manageable sub-populations of *Rangifer*. The best example of this overlap is in the work of Banfield (1954) who often intertwined the concept of 'race', 'herd' and 'population'. Howevet, what makes this search seem forced is a large set of anomalies in the circumpolar literature on morphological classification. Working from the ideas of the Number One Reindeer Brigade in Taimyr and some recent conversations with Gwich'in hunters, it would seem that a number of these typological paradoxes could be better resolved with reference to behavioural parameters.

One of the most anomalous populations in the literature seems to be that of the northern Yukon ('Porcupine') population of migratory caribou. Although from a relatively early date several wintering grounds and somewhat capricious migration routes have been documented, there has constantly been trouble in identifying the boundaries of the population. Banfield (1961: 58-59) commented that the morphological identification of Alaska-Yukon caribou was the 'most difficult problem' in his revision of the genus. The difficulty for Banfield and other scholars came from the exchange between semi-domesticated reindeer of the Mackenzie Delta

(*R. t. sibiricus*), barren-ground caribou (*R. t. groenlandicus*) of the 'Bluenose' population, semi-sedentary woodland caribou (*R. t. caribou*) of the upper Peel river (Farnell & Russell, 1984; Watson *et al.*, 1973) and Alaskan 'herds' of *R. t. granti* (LeResche, 1975; Skoog, 1968: 259). This was challenging for those, such as Banfield (1961), who tried to identify 'pure races' and for various generations of managers who have tried to estimate population size. It also poses a particular challenge for the North American orthodoxy of the 'herd' with calving ground fidelity because a large proportion of the *Rangifer* there do not have a distinct calving atea. The 'traditional calving ground' may be a successful idea in so far that it identifies a rough region where aerial surveys can be conducted but it does not capture all the complexity of the relationship between *Rangifer* and the landscape.

The paradoxes of the 'Porcupine' population have led some scholars to suggest a revisiting of Banfield's (1954:23) idea that *Rangifer* develop a trajectory upon which caribou drop calves - but not a 'calving ground' (Surrendi & Debock, 1976). Farnell & Russell (1984) suggest the necessity of experimenting with ideas of distinct or 'traditional' rutting areas in this region (Farnell & Russell, 1984). Although the idea of a traditional calving ground is also a behavioural distinction, it is one that somewhat mechanically depends upon a concept of instinct or a propensity to 'home in' to a particular spot in the landscape. The ideas of a calving trajectory, or a rutting area, represent a different set of behavioural attributes better suited to ecological relationships. At the very least, these ideas are echoed well in local ideas about these populations. For local hunters, the disappointing tendencies for *Rangifer* to be 'unfaithful', 'indiscreet', or 'unpredictable' only adds to the beauty of a landscape which can be counted upon to offer wholesome meat in any season at a number of different sites. The task in this instance becomes understanding what factors led various forms of *Rangifer* to choose differing routes or trails, rather than assuming that *Rangifer* is bound to a single route. It is this interest which dominates conversations in the Gwich'in community of Fort McPherson. Many factors influence the complex migratoty pattern of *Rangifer* according to Gwich'in hunters. They may refer to the quality of snow cover or the effect of fires. Or, they may refer to the impact of the scent of recently translocated musk-oxen or the garbage littering the side of the highway. While a focus upon different migtatory propensities (rather than specific calving sites) may not ditectly imply that people have a cultivating influence like the way that a herder 'breaks' a young bull reindeer, it does open the possibility that the landscape can be made attractive to *Rangifer*.

The ecological and behavioural moments distinguishing various grades of *Rangifer* has been echoed in recent biological research. Bergerud (1996) makes a strong argument that the zoological distinctions between barren-ground and woodland caribou be abandoned in favour of an *eco-typical* distinction between *Rangifer* which choose a semi-sedentary strategy and those which choose a migratory strategy to avoid predation. This corresponds to the reports of other researchers who have identified a difference in type, for instance, between Taimyr or Québec/Labrador caribou on the basis of their behavioural propensities (Michurin & Murenko, 1966; Harrington, 1991).

To an anthropological eye, it is sttiking how much the discussion of 'race' in *Rangifer* studies overlaps with politics, just as politics distorts the discussion of race in studies of *Homo sapiens.* One difficulty with identifying the typological discreteness of 'Porcupine' caribou is that they are not the property of a single nation-state. This is not the case with Québec and Taimyr caribou. In both cases, intensive study has led local zoologists to suggest unique names for these populations. Thus there was a very energetic but evidently unsuccessful movement to have the populations of *R. t. tarandus* in Taimyr renamed as *R. t. taimyrensis* (Michurin, 1965; Pavlov *et al*, 1989). Recently a morphological study of the 'George River' herd in northern Québec suggests that they have physical qualities distinct from other populations (Couturier *et al.*, 1989). I do not want to suggest that these physical differences are 'imagined' in either population but as with the discussion of human race it seems mote likely that these populations are large enough to be in turn distinguished into even more discrete categories, as local hunters in Taimyr do, on the basis of behaviour.

This logic of being attentive to the strategic and behavioural qualities of *Rangifer* is the subject of my own research into the local understandings of 'wild' (*baiur*) and 'tame' (*oror*) reindeer amongst Evenki hunters and herders in Siberia (Anderson, 1998; 2000). Although Evenki 'tundtamen' make a linguistic difference between migtatoty and sedentary *Rangifer* they see the actions of people as one signifi-

cant factor which determines the predictability of the movements of the deer. Through careless actions, a semi-domesticated 'herd' can 'go wild'. Indeed their major critique of state management policies since the 1960s is that their Evenki reindeer have changed in behaviour to such a degree that they are becoming similar in type to 'wild' migratory reindeer. This process usually begins because of political pressure to increase herd size, which in turn leads to reindeer losing their gregarious consciousness of 'herdness'. These bureaucratically managed 'wild' herds may bolt back to older abandoned pastures or to be swept away by an irrupting wild population (Syroechkovskii, 1984; Klokov, 1997). Instead of using a different set of concepts ot terms to study semi-domesticated, semi-sedentary, or migratory *Rangifer*, local herdsman use the same set of conceptual tools. This includes a seamless identification of forage and pastures to identify good spots to cultivate wild and tame deer, a knowledge of places which are preferred migration routes and ritualised notions of respect which are important for attracting or holding both types of *Rangifer*. Many of these ideas of the unity of wild and tame populations have been built into classic Russian works on wildlife biology (Naumov, 1933; Slobodnikov, 1935). Although the local ecology of Taimyr has been severely disrupted both by industry and by the 'rational' hunts of meat-hungry state planners, older herders still speak fondly of 'their' own reindeer which they may have raised by hand as well as 'their' own wild reindeer which they greeted at special places in the mountains twice a year as they effected their regular and predictable migrations.

An emphasis upon behavioural gradations or contingencies in *Rangifer* may not be a classic zoological strategy but it is an interesting area which serves to underscore human interests in *Rangifer*. One clear example of how ecological and behavioural research can supplement taxnomic categorisation is in Daniel Clément's work (1995: 427-8) on Montaignais ethnozoology (translated from French):

In describing phentotypes, the habitat of an animal is perceived by the Montaignais as one of the criteria for describing phenotypes and taxa. ... Therefore, for the Montaignais, or at least for those who participated in our survey, there exists at least three terms for distinguishing caribou as a function of their habitat: minâshkuâu-atîku *'the caribou of the forest',* mûshuâu-atîku *'the caribou from the place where there are no trees' and* mînipeku-atîku *'the caribou from the ocean shore'. ... A mammologist would recognise only one sub-species for the caribou present in the territory {traditionally} occupied by the Montagnais:* Rangifer tarandus caribou.

The measurement of morphological differences may seem objective and replicable (and may indeed be so) but it is a scientific endeavour closed to considerations of the effect of people on landscapes. However a discussion of behavioural distinctness opens the consideration of anthropogenetic influences on *Rangifer* populations. The radical thought of knowledgeable Gwich'ins, Evenkis and othet circumpolar peoples is that their own actions towards *Rangifer* and towards the landscape in which *Rangifer* travel directly effect the behaviour and, thus, the discreteness of those populations. These actions may tange from strictures on personal appearance and demeanour, such as the way that one speaks about caribou or keeps one clothing clean during a hunt (GRRB, 1997: 25, 37), and ranges to way that one should keep the landscape clean of distracting litter (Zoe *et al.*, 1997) to altering the landscape with corrals or fences (GRRB, 1997). All of these are examples of ways that migtatory *Rangifer* can be cultivated.

I do not want to argue that personal models of behaviour should replace the study of morphology, or the study of *Rangifer* aggregations which facilitate aerial surveying. However, I do want to suggest that attention to contingent behavioural factors is not only an interesting topic but a topic which is allied closely to local concerns. Moreover, it opens the discussion of techniques of landscape management which can be used directly or indirectly to attract *Rangifer* populations.

Managing movements

The link of human action to a study of the discreteness *Rangifer* populations opens up an area of 'management tools' which may not be as disruptive to local communities as ate the tools used to measure and control population dynamics. The well known ethological biases of local, circumpolar peoples in understanding *Rangifer* have different management implications. Almost all circumpolar communities that I have encountered, or have read about, identify their local populations of *Rangifer* with the places where those deer can regularly and predictably be encountered. I have yet to hear of or encounter a local community which has developed a passion for surveying, tagging, and enumerating its local population of deer. Rather than assuming that this lack of interest in concrete numbers represents a lack of

discipline, as suggested by 'savage survival' models of the past, it might be constructive to focus on that area of human action in these local communities which is strictly regulated in day-to-day life. I suggest that the more principled management interest in the circumpolar north is not the use of aerial surveys or satellite tagging but the strict and subtle management of the movements of people and deer. This can be illustrated with reference to two literatutes - the older 'pre-calving ground' North American literature and also the Soviet literature on *Rangifer* behaviour.

In the older scholarly literature on *Rangifer* thete is much to suggest a strong alliance of interest in *Rangifer* habitual movements. Older models of *Rangifer* aggregations show not so much an interest in identifying those places where *Rangifer* gather so that they can be easily counted from an aeroplane but instead where *Rangifer* are practically encountered by both biologists and hunters. Murie (1935), Banfield (1965), Bergerud (1973) all place the emphasis in their models of *Rangifer* discreteness not upon whete deer drop their calves but upon whete herds *over-winter*. Although the wintering grounds may not be distinct not tepeated they are significant if one wishes to know *Rangifer* in order to interact with them.

The subtle study of *Rangifer* movements is a hallmark of Soviet ethology science. The fundamental work of I.V. Druri (1949: 39-43), for example, uses a relational and behaviour definition of 'aggregation' (*stado*) which changes by season and activity. Botrowing language from local herders, winter activities of deer are described as somewhat unsttuctured and chaotic. Spring and summer actions, on the other hand, are described using the language of reindeer husbandry (*pastitsia, vstrechat'sia, vypas*). Empirically, the 'hetdness' of *Rangifer* is shown to vary during the microecological conditions of each season. An emphasis upon the behaviour foundations of migrations and social structure also characterises the work of Baskin (1969; 1970). Syroechekovskii (1990; 1995: 47) is well known for his colourful metaphors of kaleidoscopic and pendulous movements of *Rangifer*. While these descriptors again make *Rangifer* seem like rather logical mechanisms, they do suggest a rather more complex set of relations between populations and landscapes than a fixed or centred 'calving ground'.

The relational charactet of Russian work on populations is also evident in the broad way that populations are characterised on a regional basis. The Taimyr population of caribou is usually described on the basis of its migratory behaviour and in direct contrast to the North American school, not by its calving ateas. Typically, the population is distinguished by Western and Eastern 'flows' (*potoka*) (Geller & Vostryakov, 1984; Kolpashchikov *et al.*, 1989) and not by the fact that it has three discrete calving regions (see Pavlov, 1996). It is ironic to imagine that if the pioneering work of Skoog (1968) and Bergerud (1973) had been conducted on Taimyr we would be today writing of the Vorontsovo and Agapa 'herds' rather than a single Taimyr 'population'.

The interesting aspect of the story of the study of population behaviour in Russia is that the *lack* of a model of calving ground fidelity does not harm the management interest. The Russian interest in discrete migratory routes, rather than in discrete calving grounds, is rooted in the state's industrial interest in knowing where to establish their butchering points along the Piasina river. Although the scale of this hunt is rather shocking when compared to the much more respectful way that local hunting is effected, this interest in migratory routes can clearly be seen to stem from careful consultation with local observers and a clearly formulated alliance of scholarship with respect to harvesting.

These telational models of migrations are supported by a rather less well known side of Soviet biology and ethology which stresses the voluntaristic and direct effects of people on natural phenomena. This is a rather controversial issue because of the scientific legacy (or heresy) of Trofim Lysenko, who championed a very old Lamarkian argument about the capacity of an organism to control its own adaptation to its own surroundings (which he unfortunstely connected to genetics). However, Russian biology has built upon multiple variants of this argument, just as the work of Charles Darwin also relied upon this old idea (Eiseley, 1964). For example, an entite school of more conservative thinkers have been investigating the zoogenetic effects of large herbivores on the construction of entire climates (Tiskhov, 1985; Zimov *et al.*, 1995). I.V. Druri (1949: 54) in his classic work on wild reindeer directly speculates that the efforts of people might be able to direct the course of migration of wild reindeer. S.M. Druri (1956) reported experiments on the ttanslocation of forest domesticated reindeer into tundra zones of the Kola peninsula. Experiments like this with semi-domesticated teindeer became a hallmark of Soviet reindeer ranching

in the 1960s (Anderson, 2000) and reached a fever's pitch with various experiments with the translocation of muskoxen and a recent proposal for the translocation of Canadian wood bison into high Arctic settings (Stone, 1998).

While these portrayals of *Rangifer* as a rather plastic and responsive species might seem unorthodox in contrast to models of movement which stress the importance of forage, 'instinct' and hunger, they do bear a provocative overlap with indigenous conceptions. Evenki and Dolgan pastoralists in Taimyr insist that it is the actions of people which attract migratory *Rangifer* and not the vagaries of climate. Ethnographic accounts from across the circumpolar north stress a similar philosophy. Innu elders in Labrador suggest that the relocations of people from certain regions have dramatically impacted the migtatory behaviour of caribou (LIA, 1977: 112, 165; Elton, 1941: 370). While models of forage-controlled migrations are becoming increasingly popular among Gwich'in managers, statements of older hunters also stress the importance of 'luck', proper use of meat and proper care of the land when considering the migration of 'Porcupine' caribou (GRRB, 1997). The hypotheses of local hunters based on their very intimate and petsonal relationships with *Rangifer* have not been given as much direct suppott in the North American biological literature as in the Soviet literature. However there are models which are compatible. Instead of using models which focus upon the behaviour of *Rangifer* as appropriate to effecting population counts, a signpost to a more subtle alliance with local interests might be an interest in other types of regular behaviour such as repeated migtatory water crossing points, habitual wintering grounds, regular migratory 'corridors', and other management tools which lend themselves to *Rangifer* cultivation (Surrendi, 1997; Banfield, 1954; Crête *et al.*, 1991).

To summarise, having established that scientific interests can be calibrated in multiple ways to reflect local interests, this section argued that the scientific study of how to make landscapes attractive for *Rangifer*, and the scientific study of migratoty behaviour, represents a possible avenue for collaborative research. I did not argue that studies of morphological discreteness ot 'calving-ground' fidelity need to be abandoned. But I did argue that attention towards how landscape ecology affects herd discreteness, and how human action affects migratory behaviour might be an equally fruitful area of research. In a positive manner, it was shown how Soviet models of *Rangifer* discreteness have successfully incorporated behavioutal and ecological information. It was also suggested that a concern for understanding how *Rangifer* choose strategies of aggregation, as well as strategies of migtation, reflects not only the views of circumpolar indigenous communities but is also echoed in certain works of North American wildlife biology.

Conclusion: towards an environmental history of *Rangifer*

The intention of this article has been to survey a wide litetature on *Rangifer* with the aim of demonstrating an alliance of interests between all circumpolar communities, local and scholarly. This alliance can be demonstrated historically by studying the history of academic disciplines. It can also be demonstrated practically by demonstrating the compatibility of insights on the variability of *Rangifer* or *Rangifer* movement. Moreover, I have tried to establish the need to be conscious of the human interests to which scholarly activity is calibrated. Scholarly concerns which are allied to the nationalist project of states perform a useful role in gathering fundamental data on the size of populations and have developed, in alliance with local people, useful notions of herd identity and fidelity. However these same interests could be better applied to developing an equally rich store of concepts which describe *Rangifer* motions and establish the foundation for a discussion of what kinds of landscapes *Rangifer* finds attractive.

One way to recalibrate biological interests might be to link the history of scholarly categories to the history of communities in the circumpolar North. In his prominent critique of population studies, Bergerud (1996) goes some distance towards constructing an environmental history of *Rangifer*. He calls for the development of complex models of predation, and predation avoidance, which take into account the effect of anthropogenetic effects scaled broadly. For example, he argues the significance of calculating into population models the effects of centralised wolf control programmes, the effect of snowmobiles on wolf harvests and the changing fortunes of fur markets. I would suggest that his graph (1996: 104) might be made even more responsive to human interests by adding in the local use of snowmobiles fot hunting and herding. For example, the classic study of reindeer herding in Scandinavia by Pelto (1973) demonstrates that the introduction of

snowmobiles into reindeer herding alters the behaviour and attitude of teindeer. Similarly, the shift to snowmobiles by local hunters in the Canadian Arctic simultaneously teduced the need to feed dogs caribou meat in the barren-lands but also changed the pattern and quality of *Rangifer* movements around northern communities. Other human factors might be added such as predation on moose populations (which also will in turn affect wolf populations), or examples of direct local control over wolf populations. An ambitious modelling system might also enter into the equation the effects on *Rangifer* ecology of the massive resettling of experienced caribou hunters throughout the North in the 1950s and 1960s.

The behavioural and ecological hypothesis that I am defending here for the understanding of the nature of *Rangifer* also has fat-reaching implications fot studying the history of *Rangifer* population cycles. Present accounts of population dynamics rather assume that *Rangifer* macropopulations are discrete entities which have not only been stable since the beginning of aerial surveys in the 1950s but that also reach far back in time. One tadical extension of this thought extends the Beverly and Kaminuriak caribou 'herds' back into prehistotic times as geographic markers by which to interpret archaeological remains (Gordon, 1996). A serious consideration of the anthropogenetic capacity to cultivate herds suggests that aggregations are relational entities which respond to current actions. Thus, while it is unquestionable that *Rangifer* have existed for as long as we find remains of human culture, when we know that the history of human communities has changed is it not reasonable to assume that the identity and quality of *Rangifer* populations has also changed? A careful study of environmental history would be necessary to answer this question.

This survey of the literature on the human interest in *Rangifer* suggests that *Rangifer* is a prominent circumpolar species with a special alliance to boreal populations. Both indigenous consultants and biological researchers argue that *Rangifer* do not merely supply food to eat and food for thought but that they also respond to the quality of the ecological setting. While it might be difficult for applied zoology to accept that *Rangifer* are best regulated through keeping one's clothing clean and being attentive to one's dreams, this survey of the literature in the northern Yukon, Taimyr, and Québec/Labrador suggests that the cultivation of boreal landscapes has an important impact on the kind of deer that one would expect to find. This leads to an interesting thought with which to close this review. If *Rangifer* behaviour and population discreteness is a responsive trait, as indigenous hunters assert, then it is entirely possible that the last few decades of state controlled predation in the northern Yukon and northern Québec, as well as 'rational use' in Taimyr, have cultivated a historically unique ecotype of *Rangifer*. This is a hypothesis raised recently by many indigenous observers throughout the circumpolar North. In Taimyr and the northern Yukon, elderly hunters report that contemporary migratory *Rangifer* no longer behave predictably, have become too many, ot have 'gone wild'. To extend the ecotypes of Bergerud (1996) one step farther, it might be possible at the beginning of this century to note the existence of two *anthropogenetic* ecotypes of *Rangifer* in the circumpolar North based on a reading of environmental history: *Rangifer tarandus habitus* characterised by a close and intimate interrelationship with local communities who depend on them and *Rangifer tarandus étatocraticus*, whose identity, behaviour and discreteness has been shaped by the strict zoning and licensing regulations of state-sponsored management organisations.

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Region of Canada's Northwest Territories. Here I am indebted to the advice and wisdom Peter & Ben Vittrekwa, Mr. Neil Collins, and Charlie Snowshoe. I also am grateful for the recent insights of Rob Wishart. Finally, I have combined these preliminary hypotheses with library and archival research and some interviews in English, French, and Russian. Much of the historic and bibliographical material in this papet has been identified by Craig Campbell and Dr. Franca Boag to whom I owe a great debt due to the large number of locally circulated or 'grey' manuscripts in this field.

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