a valuable addition to the literature on travel and material culture in the North.

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WINTER; AN ECOLOGICAL HANDBOOK. By JAMES C. HALFPENNY and and R.D. OZANNE. Boulder: Johnson Publishing Co., 1989. 273 p. Softbound. Price not indicated.

"Winter" has so many different meanings, depending on latitude and biome: warm and wet, cool and misty, cool and rainy, cold and snowy, cold and dry. This book, with such a general title, is clearly intended as a textbook, so it is disappointing to find it so restricted in geographic and climatic coverage to a part of the southern Rocky Mountains. It is a frustrating book to read. We certainly need more attempts to explain "winter" to the general public, but not like this. There is some good information in the book, but the average reader would be hard pressed to separate it from the numerous errors, misinterpretations and sloppy editing.

A few examples: On p. 20 we learn that "There are three species of weasels in North America: the short-tailed weasel, the long-tailed weasel, and the ermine." (Whatever happened to the least weasel?) A couple of sentences later we read, "A weasel in its white color phase is commonly called *ermine*." (This is not a *colour phase* but a winter *pelage*; besides, we were just told that *ermine* was one of the three species of weasels.) A few sentences more and we read, "Those [weasels] in northern regions expend energy to make two color changes each year. Southern weasels may also perceive winter but the selective pressures are low enough that they do not respond with a color change." This implies that southern weasels do not expend energy in a *moult*, which, of course, they do, just as the northern individuals do.

On p. 39 the table of selected Inuit and Indian names for different types of snow is full of misspellings and has the two languages garbled and partly interchanged. A probable cause is that the present authors cite, as one source, Williams and Major (1984), a publication that was itself riddled with errors and misspellings.

In the discussion of strategies for coping with winter, I looked in vain for any coverage of the exciting studies by C.W. Aitchison on the metabolism and physiology of winter-active subnivean invertebrates.

Throughout the book there is an inordinate attention paid to hibernation as a mammalian adaptation to "winter" conditions. The classic work by Hagmeier and Stultz (1964) clearly showed that mammalian torpor is an adaptation to environmental heat and dryness, not cold and snow. On p. 76 we read that ". . . sheep and white-tailed deer" have "large feet" and this prevents them from breaking through snow crusts. The situation is not so simple. *Ovis dalli* and *Ovis canadensis* as well as *Odocoileus virginianus* have more flotation than does *Cervus canadensis* but much less flotation than *Rangifer tarandus* (Telfer and Kelsall, 1984). In other words, sheep and white-tailed deer are less liable to break through a crust than wapiti, but much more liable to break through than caribou. Telfer and Kelsall (1984) summarized their findings by writing ". . . white-tailed deer have compensated behaviorally for limited morphological adaptation to snow . . . ." On p. 79-80 the discussion of the phenological critical periods of small taiga mammals is mangled. On p. 85, 280 km does not equal 448 miles!

Pruitt (1966a [not 1970, as cited here]), in a study of the mammals of Low Arctic tundra in northwestern Alaska, introduced a "Snow Index" that agreed with species and populations of small mammals found on the same study plots in subsequent summers. Because a tundra snow cover affects small mammals not only by affording some insulation from the supranivean thermal and wind environment but also by giving protection from predation and by governing, to some extent, subnivean photoperiod and quality of light, I concluded that the SI expressed ". . . a rough approximation of the relative ecological values for small mammals of total amount of cover, its thickness and its density." Marchand (1982) misrepresented this SI by implying it was devised only to describe a direct relationship between snow density and insulation value. Marchand then inserted some hypothetical numbers in the SI and showed it did not model the insulative value of a snow cover. SI was not introduced as doing so. He attempted a transfer from an area of windswept arctic tundra to an area of temperate-zone forest in Vermont, U.S.A.!

Marchand's misrepresentation of the purpose of SI is now further compounded by Halfpenny and Ozanne on p. 81. These present authors have changed the name of my "Snow Index" to "Stability Index," say that it does not model subnivean temperature stability, and repeat Marchand's numbers and his erroneous statement of the original description. This entire sequence first by Marchand and now by Halfpenny and Ozanne is a specimen of what Stefansson (1928) called "the standardization of error" and will make a good example for classes in the logic of science.

The discussion of the disappearance of the caribou of East Greenland (not the "western coast") is completely distorted, even though Degerbøl (1957) is correctly cited. The present authors state (p. 67), "In the fall of 1899, large herds of caribou existed along the western coast of Greenland. These caribou had been there for as long as we have records. When the Scandinavian fishermen returned to the coast in the spring of 1900 all the caribou were gone." In actual fact, Degerbøl stated that the caribou had been found in the Scoresby Sound region of *East* Greenland only in 1891-92 by the Danish Ryder Expedition. The Swedish Nathorst Expedition saw only a few herds in 1899, and then in 1900 the Andrup and Kolthoff expeditions did not see any animals. These arctic exploring and mapping expeditions were hardly "Scandinavian fishermen!" And, of course, caribou still exist in "western Greenland," although, as Vibe (1967) clearly demonstrated, there are great fluctuations in numbers in complicated responses to climatic changes. He also elaborated on the extirpation of the caribou of East Greenland (actually Northeast Greenland). A recent detailed history of all the Greenland caribou populations (including local extirpations) is in Meldgaard (1986).

On p. 107 the authors imply that "neatsfoot" oil refers to moose feet, forgetting that "neat" is an old word referring to cattle.

In the discussion of winter reduction of body mass in some large mammals no mention is made of such reduction in small mammals (e.g., shrews), where it is known as Dehnel's Phenomenon, or of Mezhzherin's (1964) explanation of it as an adaptation to winter, or of the extensive studies by Hyvärinen and his students on the physiology and histology of the phenomenon.

On p. 153 we read again the old folktale that caribou use their antlers to scrape away snow from their forage, even though neither Pruitt (1966b) nor Bubenik (1975) could find actual examples. On p.175 we learn that spruce branches ". . . all slope downwards" (they don't) and on p. 180 we learn that ". . . it does not get dark in the polar regions" (it does).

I am particularly concerned about some of the information, or lack of information, regarding human activities in winter. The discussion of the importance of moisture loss through clothing is inadequate. There is no mention of the absolute necessity of moisture-permeable (e.g., felt or duffel) footwear and the types of ski bindings they require. Windchill is discussed in the erroneous terms of "equivalent temperature" instead of the widely used and more accurate statements of heat loss in $W \cdot m^{-2}$. The acronym WET is used for "windchill equivalent temperature." This is a poor pedagogical mnemonic device because it tends to generate confusion between evaporative cooling and true windchill.

Moreover, even if one wanted to calculate "WET," one could not get a correct answer using Figure 96. Falconer's (1968) nomogram showed wind velocity in miles per hour across the top with the equivalent in knots across the bottom. The nomogram, now presented as Figure 96, has wind speed in miles per hour across

the top with erroneous figures for metres per second across the bottom, according to the standard conversion tables in List (1963).

There is a misunderstanding of the basic principles of the quin-zhee. First, the presence of pukak (which they refer to by the "official" but inelegant and actually incorrect name "depth hoar") is necessary because it is almost invariably the warmest layer of the snow cover, and therefore its particles, when distributed throughout the mass of mixed snow, are important bonding sites in the hardening process. Lack of a pukak layer indicates the snow cover is isothermal and therefore, when mixed, will likely not achieve the post-sintering hardness >600 g·cm^{-2} necessary to be self-supporting. Moreover, the colder the weather has been (i.e., the steeper the temperature gradient through the api) the *quicker* sintering will occur. The second principle of the quin-zhee is its use of earth heat to make the enclosed space equithermal with the pukak space. Use of a sleeping platform of packed snow, as this book recommends, frustrates this principle.

Because of these and other deficiencies, I cannot recommend this book.

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