

Steward's materialist arguments are often remembered or imagined as the introduction of Marxist analyses to anthropology. This effect, as Kerns' recognizes, places Steward in a key role in the history of American anthropology. She describes his approach as having "... a propensity for the concrete" and that "[h]e used an impressive array of ethnographic and archaeological evidence to support a range of creative, generalizing conclusions about how, in his own words, 'similar subsistence activities had produced similar social structures'" (p. 3). This approach led him to develop the sub-field of cultural ecology, and to train several key materialist anthropologists in the process. For these reasons, it is fascinating to understand the details of Steward's life, and to come to a fuller appreciation of the lives that Steward was influenced by and in turn influenced.

Beginning with Steward's formative years, Kerns argues that he was heavily influenced by problems inherent in the daily life of arid environments and the labour that it takes to organize irrigation work to solve them. Following this focus was Steward's initial academic and then professional material on the American Great Basin. Kerns demonstrates that the focus on the organization of subsistence labour and its relationship to the physical environment remained a central component in Steward's *oeuvre* on development and change. Detailing fully Steward's early ethnographic and archaeological work and demonstrating the full mix of personal responsibilities and professional desires for the ambitious young scholar, Kerns relates the stories of his development from both his first and second wives. These perspectives from both women well compliment the public and professional history of Steward's academic career. A particular example of this dynamic was his move from the University of Utah. This was due to the break-up of his first marriage and overlapped with his second marriage. Leaving the security of his first marriage and his position at the University of Utah forced Steward to look for work during the difficult economic times of the Great Depression. His responsibilities as the head of a household to provide influenced his theorization of the formation of social relationships and societies. Insights from his first and second wives and his searching correspondence with Alfred Kroeber permit Kerns to triangulate various factors, thoughts, and demands that affected Steward at that time in his career. This serves to present Steward, who often is not a sympathetic character, in the most human of terms.

Steward's unwavering focus, and his work with many of the GI's returning to university after the war and the burgeoning field of academic anthropology, spawned numerous influential studies about the nature of Indigenous societies, and materialist analyses about the development and change these societies experienced. His cross-cultural analysis, generating a nomothetic explanation of cultural development, differentiated his approach so thoroughly from the dominant Boasian tradition as to develop an entire new area of study. As a method of analysis of multilineal evolution, cultural ecology helped re-codify evolutionary theory within a scientific rhet-

oric. Detailing Steward's professional development and personal relationships from Berkeley to Washington, D.C. and continuing the root metaphor of the patrilineal band offers a fascinating insight into the motivations and concerns that anthropologists were facing during the Great Depression and after the Second World War. This is the context that must begin to be appreciated if we are to understand the generation of foundational pieces in anthropological theory, particularly those who adhere to a "scientific" position within the field. Kern's text provides this context; and it systematically undermines Steward's claim to an objective, scientific method for his conceptual basis for the root of society. The patrilineal band was merely a reflection of his own social habits projected into his theoretical paradigm. The model ensconced a male-centred approach to anthropological method and was replicated and promoted in his theory and practice.

Although the text addresses Steward's failure, like many in his time, to appreciate the role of women in his analyses, and the contribution of the women surrounding him, it does not soundly question the basic assumptions that he makes about Aboriginal societies as a whole. To this point, a further discussion of Steward's role in the Indian Claims Commission proceedings and the relationship of his theory to colonial legal ideology would be fruitful. Exposing the gender bias is but one crucially important component of Steward's approach. Of equal significance is the oppositional relationship between Steward's position and that of John Collier, the Superintendent of Indian Affairs who led the Indian Reorganization Act (1934), and how Steward's approach helped to facilitate the U.S. government's stance in claims cases against the Indians. This is a deeper context within which to understand the relationship of the patrilineal band and anthropological theory to a greater number of real social relationships.

Notwithstanding my claim about the omission of the Indian Claims Commission period in Steward's life, this text is a tremendous addition to several of the recent biographical and historical works in American anthropology. Kerns' contribution well documents Steward's academic history, and augments this chronicle with the personal insights of those close to him. This is a useful and interesting book for the history of American anthropology, the theorization of hunter-gatherer societies, and gender studies within the academy.

Blair A. Rudes and David J. Costa (eds.), *Essays in Algonquian, Catawban, and Siouan Linguistics in Memory of Frank T. Siebert, Jr.*, Algonquian and Iroquoian Linguistics Memoir 16, Winnipeg: Algonquian and Iroquoian Linguistics, 2003, ix + 296 pages.

Reviewer: *Paul Proulx*

As a specialist in Algonquian, I will limit my comments to the Algonquian papers. Three are written by scholars generally

found lunching together at Algonquian conferences: Ives Goddard, the group's leader; Richard Rhodes, his disciple; David Costa, a student of Rhodes and the Algonquianist co-editor of the volume. The signs of intellectual inbreeding are everywhere, and Goddard clearly suffered from too gentle an editorial hand.

Group isolation and status within it can be estimated from the volume bibliography, which indexes frequency of citation. I count 31 separate Goddard publications cited, versus only seven for Bloomfield. Rhodes has 10 and Costa six, while Peter Denny, a psychologist (these days converted into an amateur archaeologist) who hangs out with them, and the remaining linguist, Philip LeSourd, are tied at four.

Goddard's contribution is of monograph length (65 pages). Its merit lies in assembling a huge amount of data on Algonquian demonstratives. However, rather than comparing all the Algonquian languages, as the comparative method calls for, Goddard bases his reconstruction of the Proto Algonquian (PA hereafter) demonstratives only on his two fieldwork languages (and some neighbouring ones he regards as closely related to them).

These he reconstructs internally, producing something like a 1960s underlying structure. This may resemble an earlier stage of a language, or may not. In any event, it is a synchronic representation, that hypothesizes in simplified and abstract terms about the daughter language studied, and how to get from there to its more complex concrete (surface) structure.

Goddard then reconstructs PA comparing the two underlying systems, and argues away all the rest of the data in the second half of the paper. Once this choice of language sample and method are made, nothing like a genuine PA reconstruction is possible. He has discarded far too much information.

For example, Goddard claims that demonstratives "may undergo phonetic reduction beyond what can be accounted for by the general sound laws of a language" (p. 80). He proposes the ad hoc deletion of whole demonstrative roots (VC-) in some languages. In Ojibwa, "PA *4eyo(:)- (set A) was reduced to CO *4o-, and PA *4en- (set B) was lost completely" (p. 63); in Cree-Montagnais, "word-initially PA *4ey- was lost" (p. 69); in Menominee, "the initial sequences PA *4eyo- and PA *4eni- were reduced to M *4a-" (p. 76); and in Miami, "initial I 4iy- is lost in set A, and initial I 4n- is lost in set B" (p. 78).

I submit that this is not just wrong, it's utter nonsense. Even if one believes in ad hoc sound change, wherever Goddard claims that reduction has totally deleted a root, he has no evidence that the root was ever there in the first place. Arguably, it is simply a convenient fiction, to avoid recognizing that the following element is itself a root, and thus the existence of additional stems. (For those who believe in the regularity of sound change, where his ad hoc reduction has totally deleted a root, although regular sound change would not have done so, he has *proven* that it was never there.)

Many of his other claims also go against linguistic intuition and common sense. For example, he says that Fox 4i:niya "that

(animate, inaccessible)" and Eastern Cree 4(a)niya: "that (animate, inaccessible)" do not attest a stem PA *4eniy- as suggested by Pentland, but rather are separate parallel compoundings of his roots *4en- and *4ey- (p. 38, 71, 91).

However, notice that the attested stems are not Fox *4i:n i:ya nor Eastern Cree *4(a)n-aya-, as a word-initial position for the second syllable would have produced by his own rules. The compounding, if such it was, clearly came early, and one wonders by what criteria he refuses to reconstruct it for his "Western dialect" of PA (in which initial *4e > *4i). His hypothetical compounding can be seen as another convenient fiction to artificially reduce the number of PA stems.

Nor do I believe that Blackfoot 4anno- "this," Mahican 4no: "this (inanimate)", and Menominee 4enoh, 4enom "that (animate)" are chance similarities (p. 89). This merely gets rid of a PA stem *4eno-.

These are not incidental details. They show Goddard's PA reconstruction to be invalid in its basic methodology, producing multiple omissions. Goddard may feel that the quest for simplicity justifies extreme methods, but he should remember the words of Albert Einstein: "Everything should be made as simple as possible, but not simpler."

Charles Hockett also had a dictum: "who explains everything, explains nothing." He illustrated this along the following lines. If someone uses "God wills it" as an explanation, we learn nothing we did not already know. It is too powerful an explanation and so, in the scientific sense, it explains nothing. Goddard's methods are too powerful as well. He is not discovering his conclusions, he is creating them.

Goddard's paper is somewhat tedious and dry reading in the multiplicity of its argumentative details, until near the end (p. 86-93), when he begins an attack on Pentland (1979, 1991, 2001a, 2001b), and Proulx (1988). He makes no reference to Proulx (2001), which addresses many of his objections.

Rhodes and Costa's paper (p. 181-216) is well organized, easy to read or consult. It reconstructs the PA numerals from one to ten, clearly presenting the full data. The reconstructions are not new, but a full presentation of the data is most welcome. I wish they had organized the Eastern data into the four genetic units it is composed of, but that's a detail.

Besides the simple numerals, the authors present a number of compound ones (p. 193-196), and describe what seems to have happened to them in the daughter languages. They are unable to explain these developments, and sometimes speak of "the phonological reduction characteristic of numbers" which "cannot be explained as a normal sound change." This is the Goddardian idea that laws of sound change do not hold, except when convenient. It leaves a number of things described but unexplained.

Nevertheless, they have made an important discovery about these compounds, that they postdate PA and are found in all the non-Eastern languages. They argue further that "the construction is recent" (p. 194). This implies that until recently the non-Eastern languages were able to innovate jointly (albeit clearly across dialectal boundaries), which strengthens my

view that these languages descend from a Proto Central Algonquian (PCA) much more recent than PA. They also provide a plausible if somewhat speculative account of the influence on Cree vs. Ojibwa relations of the rise and fall of Missippian societies just to the south of them. However, some other ethnographic reconstructions are too specific for the very small amount of data considered, and do not convince.

The paper by Denny (p. 15-36) is evidently an overview of the account he has developed in recent years, on the archaeology of times and places he suspects of having harboured Algonquians. The framework for this is more art than science, and it almost never convinces me. The main problem is that the linguistic evidence he is working with is much too tenuous, and will not really sustain a detailed account of any kind. Costa's paper (p. 1-14) consists of notes on Shawnee.

The paper by Philip LeSourd (p. 141-164) is easily readable, a fine introduction to Malicite-Passamaquoddy for the Algonquianist unfamiliar with it. After introductory remarks on the phonology and grammar of this language, the topic is the noun substitute, best known throughout Algonquian for its use when one has forgotten a word ("whatyoumaycallit"). However, LeSourd discovers unsuspected uses for it: to announce a clarification, including a switch in gender of a referent, and as a generalizing modifier meaning "of some kind." He explores its historical phonology, inflection, and syntax. Rich with examples taken from texts, it inspires confidence. He also discusses its relation to the suffix *-4èy* "pertaining to, consisting of," also found in Micmac, that forms modifiers (adjectives) indifferent to gender. These are unique within Algonquian, as far as I know, and thus of special interest.

References

- Pentland, David H.
 1979 *Algonquian Historical Phonology*, Doctoral Dissertation, Toronto, ON: University of Toronto.
- Pentland, David H.
 1991 Mahican Historical Phonology, *Schmick's Mahican Dictionary*, Carl Masthay (ed.), Memoirs of the American Philosophical Society, 197, Philadelphia: American Philosophical Society: 15-27.
- 2000a Inaccessible and Absentative Inflections in Algonquian, *Algonquian and Iroquoian Linguistics*, 25(3): 25-27.
- 2000b The Origins of the Cree Demonstrative Pronouns, *Algonquian and Iroquoian Linguistics*, 25(4): 35-38.
- Proulx, Paul
 1988 The Demonstrative Pronouns of Proto-Algonquian, International, *Journal of American Linguistics*, 54: 309-330.
- 2001 Proto-Algonquian Demonstratives and the Size of PA Socio-Political Units, *Algonquian and Iroquoian Linguistics*, 26(2): 15-19.

Barker, John and Douglas Cole (eds.), *At Home with the Bella Coola Indians: T.F. McIlwraith's Field Letters, 1922-4.* Vancouver: UBC Press, 2003, 224 pages.

Reviewer: *Brian Thom*
McGill University

This publication of T.F. McIlwraith's field letters chronicles an important point in the life of an historically significant figure in Canadian anthropology, and provides an important window into the personal and professional relationships that inform the writing of ethnography.

Before McIlwraith became one of the first Canadian anthropologists to be established in a Canadian university (University of Toronto), he was employed by Edward Sapir to engage in fieldwork in the Nuxalk (Bella Coola) community of coastal British Columbia. The enthusiastic, 23-year old Cambridge graduate who had studied under A.C. Haddon and W.H.R. Rivers, set out to comprehensively document Nuxalk traditional culture from kinship systems to the potlatch to shamanism and religion. Out of his two field seasons of work in 1922-24, McIlwraith produced a seminal 2-volume ethnography (*The Bella Coola Indians*, Toronto: University of Toronto Press, 1948) that is a distinctive Northwest Coast ethnography from this time in his departure from the general Boasian research agenda of focussing on native texts. The current volume completes the publication of his writings on Nuxalk culture, providing his home-bound family and professional letters from his time in the field, along with the three other short pieces which were obscure or previously unpublished.

The preface by Barker and Cole describing the impetus for the work is followed by an introduction that supplies background information on McIlwraith, the Nuxalk and the texts that follow. The letters read as freshly and lively as if they were written last winter. The letters and papers are richly annotated with endnotes and bibliographic citations that give enough detail to prevent the reader getting lost in the particularities of language, personal relationships or historical events that were current in the 1920s. The book is significant for the new contributions to early 20th Nuxalk ethnography, and perhaps more widely significant for the insight and point of self-reflection it provides for the practice of ethnographic fieldwork.

As Barker and Cole assert in the introduction, the book has been highly interesting to contemporary Nuxalk people. Through these writings, they gain a rare glimpse into the lives of their ancestors and some close descriptions of aspects of their culture as it was practised during the years that McIlwraith was in the community that were not included in his larger study of "traditional culture." Indeed, the letters provide an interesting contrast with *The Bella Coola Indians*. They are concerned with the immediacy and presentness of events and relationships in the community, whereas the ethnography is primarily concerned with the memory culture of pre-contact issues.