

## George Woodcock's *Peoples of the Coast*: A Review Article\*

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The appearance of George Woodcock's *Peoples of the Coast* ought to be a source of dismay for social scientists throughout the Northwest — and beyond the Northwest as well, for the willingness of publishers to print such volumes is certainly not confined to this one region. Woodcock's survey of cultures and culture categories on the Northwest Coast is one of the worst discussions of this subject matter available. Thus the need still exists for a non-technical book on Native peoples of the Coast, written for an intelligent lay audience willing to read with thought and care, synthesizing the knowledge gained by investigators during the past century. Some of the finest fieldworkers in the history of North American ethnology carried out their best research in the Pacific Northwest: Franz Boas, Frederica de Laguna, Viola Garfield, John Swanton and Erna Gunther. Much of this work has been inaccessible to the non-specialist both because of the relative rareness of the original publications outside university libraries and because of the technical difficulty of the publications themselves, whose authors recorded, in minute detail, the tremendous complexity of coastal societies. A summary and integration of the incredible mass of information we currently possess — and the questions which are still open and seriously debated — obviously would be very welcome.

It is impossible for Woodcock's book to fulfill this role. *Peoples of the Coast* is so shot through with basic errors of fact and major misinterpretations that another fair-sized volume would be required to list and discuss them all. Woodcock's control of the ethnographic bibliography is poor; in many cases he reproduces long-discredited arguments and conclusions. He repeats ethnocentric characterizations of Northwest Coast personality types and waits eight chapters to qualify these characterizations. *Peoples of the Coast* is basically a scissors-and-paste effort uninformed by much knowledge of the current state of Northwest Coast studies; this is particu-

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larly evident in Woodcock's attempts to assess the implications of linguistic research. Unfortunately, the book will badly misinform many readers, and for this reason alone a detailed inventory of its mistakes seems worthwhile.

*Peoples of the Coast* contains an unacceptably large number of errors involving factual information. Thus on pages 8 and 9 Woodcock indicates that "the arts acquired since European contact, like argillite carving and silver work, have shown quite a remarkable revival, notably among the Kwakiutl, . . . Haida and especially the Gitksan," but neither the Kwakiutl nor the Gitksan have ever carved argillite. On page 10 Woodcock suggests that Curtis' photographic record of Northwest Coast culture was made "before its decline had reached an advanced stage"; the truth is that Curtis' photographs were mostly staged, because the decline *was* far advanced, and his Kwakiutl film — while accurate in places and interesting — was a scripted performance presenting a mawkish European romanticization of aboriginal life. On page 15 Woodcock, discussing sex-based division of labour, indicates that certain activities were carried out "during the dark rainy months between November and March" which were actually practised all year round. The Tsimshian are said on page 19 to control the eulachon trade because of the runs up the Nass and Skeena, but there was virtually no eulachon fishing on the Skeena; the Tsimshian living on the Skeena were obliged to travel up to the Nass to get eulachon.<sup>1</sup> Moreover, the Tsimshian certainly did not control the Kwakiutlan-speaking people's access to eulachon oil: both Knight's Inlet and Kincome Inlet have huge annual runs, and, as indicated in footnote 1, there seem to have been others as well (at Bella Coola and Kitimat, for example).

Throughout the book one encounters instances in which Woodcock has misunderstood or failed to assimilate the contents of his sources. In rephrasing Drucker's account of Nootkan whaling techniques, Woodcock claims that "the pitch head of the harpoon would explode on impact," which in physical terms is absurd, and indicates Woodcock's failure to grasp details of technology. In fact, the whaling harpoon head was a

<sup>1</sup> People who reside in communities on the Skeena recall the eulachon run there as extremely small and of no economic significance. According to information supplied by the Fisheries Branch of the Canadian Ministry of the Environment, the eulachon run up the Skeena is concentrated in an area in which bottom conditions on the river and other local factors prohibit any exploitation of the run as a source of food (including eulachon oil). Fisheries have also indicated that the people on the Skeena may have in some cases purchased eulachon oil from the Wakashan-speaking Haisla people near Kitimat, who obtained the oil from fish caught in runs up the Kitimat River.

composite device and the pitch simply functioned to affix the replaceable mussel shell blade to the permanently joined valves and lanyard. On page 47 Woodcock asserts that all Athapaskan-speaking peoples are matrilineal, in spite of the fact that the eastern Athapaskans are *not* matrilineal, and on page 171 mentions that northern Coastal peoples — Haida, Tlingit and Tsimshian — used D-adzes, which they never in fact possessed. Errors of this sort — often several at a time — appear on page after page of *Peoples of the Coast*.

In many cases it is difficult to imagine what Woodcock's sources for a statement could possibly have been. Thus on page 28 he indicates that button blankets in some sense took the place of Chilkat blankets, but in terms of manufacture, distribution or use there is no sense in which this claim is true. Elsewhere Woodcock states that "fine poles were still being carved by the isolated Gitksan on the Skeena in the 1920s," which is untrue, and the "Hole-in-the-Sky" pole Woodcock thinks was carved in the 1890s was almost certainly carved thirty years earlier. Finally, Woodcock's remark that currently "no chief's days are dominated by the obsessive preparation for such events," referring to potlatches, only reflects his lack of acquaintance with living Kwakiutls, who spend months, even years, planning contemporary potlatches.

The captions on Woodcock's photographs contain numerous mistakes. On page 81, Woodcock identifies a beaver mask as Haida which is probably Tsimshian; on page 84 a Kwakiutl pole from Alert Bay is presented as Queen Charlotte Island Haida. A rattle on page 86, carved in typical Northern style, is identified as Kwakiutl, while on page 20 a Tlingit frontlet is identified as Tsimshian, and a Tlingit shaman's oyster-catcher rattle is described as a "Tsimshian chief's bird rattle." On page 130 Woodcock claims that Salishan society is unique in having the institution of  $sx^w\text{ə}yx^w\text{i}$  dancers, but this is simply untrue: both the Kwakiutl and the Nootkan peoples had  $sx^w\text{ə}yx^w\text{i}$  dancers, although admittedly the dances ultimately derived from Salishan sources.

Gross factual errors, such as the confusion on page 198 of Haida with Kwakiutl designs, or those cited in the preceding paragraph, force one to conclude that the author simply does not possess the requisite knowledge to make accurate judgments about Northwest Coast art objects. This conclusion is all the more disturbing in light of the key role Woodcock assigns to material culture: "The artifacts of a people have always been the most reliable evidence of their way of life, more reliable than the written or printed records of even a literate civilization." (p. 30.) There is no need to engage in an abstract discussion of this proposition,

which we find extremely debatable; it is enough to present a few typical instances of Woodcock's methodology. On page 63 he refers to a small stone carving found at Yale by the archaeologist C. E. Borden, who identified it as a seal. Woodcock observes that "this identification seems unlikely in a carving found so far from the sea." He continues:

I prefer to be guided by its general resemblance — even though it is done in a less naturalistic manner — to the Paleolithic bear figures of Scandinavia and Siberia and to the small bear images dating back two thousand years, which have been found near Kamloops and which suggest that early Indians in British Columbia were influenced by the circumpolar bear cult that was strong in Siberia and reached its greatest complexity among the Ainu of Japan.

It would be more accurate to say that the Kamloops material allows one to infer only that early Indians in the Kamloops area were interested in bears — quite plausibly, if those talismans were connected with hunting magic as Woodcock supposes in the case of the object Borden found. The main point is that Woodcock's objection to Borden's identification is groundless, for two reasons. In the first place, Yale's distance from the sea is irrelevant, because sea-going peoples regularly visited there. Halkomelem speakers living near the mouth of the Fraser annually travelled upriver to fish in the area around Yale, and these coastal dwellers were fully familiar with seals and ate seal meat when the latter was available. In the second place, seals do follow salmon upriver, occasionally quite far inland, and logically they could ascend the Fraser to Hell's Gate, which is *above* Yale. One wonders how Woodcock would explain the small iron killer whale, with typical killer whale shape and characteristic design motifs, found a few years ago at Telegraph Creek, which is about as far from the sea as Yale is.

On pages 55 and 56 Woodcock develops the very curious argument that although pottery is virtually exclusively associated with agrarian societies, the Native peoples in early British Columbia did use "mineral resources" (i.e., rocks for tools and stones for petroglyph carving) and therefore they *might* have used clay to make pots if only they'd known about it. On the strength of this supposition, and the fact that there was no pottery technology in aboriginal British Columbia, "we can assume that there never was a point at which Coast Indians were in contact with primitive potters . . ." from which, as a corollary, we can infer "that the ancestors of the Coast Indians left Siberia before neolithic techniques of pottery manufacture had reached that area. . . ." Whatever the chronology of movements over the Bering Sea turns out to be, this sort of

attempt to prove a positive assertion on the basis of negative evidence — a common practice in *Peoples of the Coast* — is a poor line of argument at the best of times. There are (or were) many peoples in contact with agrarian societies who never found it desirable to adopt the technologies of such societies, and in the setting of the Northwest pots would have been vastly more impractical than baskets. In this discussion one encounters the usual factual errors, such as the claim that the Coast Indians had “only the rarest metal objects,” in spite of the fact that at the Ozette site, ca. 1670 AD  $\pm$  50 years, there is evidence for at least a dozen metal tools of various kinds, and that observers on Cook’s ship reported large numbers of metal blades in use at Friendly Cove.

As a final example, Woodcock claims that postcontact art forms show greater sophistication and finish than prehistoric artifacts, and in support of this point compares precontact Nootkan art with recent Kwakiutlan objects and the sort of pieces “which form the greater part of modern museum collections.” But it is totally inappropriate to compare precontact Nootkan art with later styles from other areas, because the Nootkan-speaking peoples continued to work within the stylistic tradition known as “Old Wakashan” long after contact, while the more elaborate “Classic” tradition was well established among the northern coast groups by 1800. Since it is these northern groups, along with the Kwakiutl, whose work is the focus of most museum collections, Woodcock’s comparison is meaningless; what one has are two different traditions, one relatively more spectacular from the European point of view, both of which can be traced back into the prehistoric period. As far as Kwakiutlan art is concerned, formal styles began to change around 1880 from Old Wakashan toward the flamboyant style represented in most collections; during this period Nootkan art underwent no comparable transformation, as comparison of contemporary Nootka and Kwakiutl pieces from the end of the century shows. Edenshaw, to whom Woodcock refers in his description of the supposed artistic flowering after contact, actually stands somewhat outside the developing styles of the northern coast; he is a unique figure, hardly typical at all of Native carvers. These sorts of critical blunders are as frequent in *Peoples of the Coast* as factual errors and undocumented, highly questionable claims.

Woodcock’s inability to deal critically with his source material becomes disastrous in his attempts to discuss Native languages. There are the usual out-and-out errors of detail: thus Kwakiutl is said to mean “beach on the other side of the river,” which is a strictly imaginary analysis; the name actually breaks down into k<sup>w</sup>ak<sup>w</sup>-, referring to the association of bands

living at Fort Rupert and appearing also in the word  $k^w ak^w ala$ , the name of the language, and a suffix  $-u\check{t}$  which does not seem to appear anywhere else in the language. Woodcock claims that the difference between Haisla and Kwakiutl (i.e.,  $k^w ak^w ala$ ) is comparable to the difference between English and Dutch. This is a staggering assertion, in view of the fact that almost nothing is known about Haisla. Very little data on the language is currently available and not even a rough comparison between the two languages can be made at present, so that Woodcock's comments here are baffling. Finally, the chapter devoted to the languages themselves (which Woodcock confusingly refers to as "dialects" on occasion)<sup>2</sup> is arguably the very worst in the whole book.

The title of Chapter 4, "Language and Myth: the Verbal Witness," announces the thrust of the chapter: the record of historical movements among the ancestors of current Native populations on the coast which is supposedly discernible in their myths parallels the record suggested by linguistic relations. Unfortunately, the use of linguistic reconstruction as a basis for describing early migrations is full of perils for those who approach the inquiry with inadequate data or insufficient understanding. Consider the case of Haida vis-à-vis Tlingit and Athapaskan. Woodcock writes:

Recently, the languages of both the Haida and the Tlingit have been linked to the Athapaskan or Dene group of tongues, which embraces many Indian peoples in the northern interior of British Columbia and also in the Yukon and Northwest Territories, all of whom, like the Haida and Tlingit, are matrilineal and exogamous in their clan structure. Yet glottochronological evidence apparently indicates that Haida is at least five thousand years away from any primal Dene language out of which it may have originated. (p. 47.)

As noted above, the statement about universal Athapaskan matrilineality in this passage is quite erroneous. Beyond this objection, the "recent"

<sup>2</sup> The term "language" should technically be used to distinguish forms of speech which are not mutually intelligible. Thus Spanish and French are different languages because a speaker of one cannot thereby understand a speaker of the other. "Dialect" refers to speech differences which do not seriously interfere with mutual intelligibility: a Londoner can understand a person speaking an English dialect from the North, even if some of the vocabulary is different. "Dialect" is sometimes used by non-linguists to indicate a non-standard speech form, but this is inappropriate; from the linguistic point of view, both the London and the north of England speech in the previous example are dialects of one language. Woodcock further confuses things by referring to language families as "languages." Thus Salish is said to be a language (page 7). Woodcock seems highly impressed by the fact that different Salishan peoples speak different languages; see his discussion on page 132, in connection with some totally unsupported speculations on genetic changes in the early Salishan population.

hypothesis of a common origin for Athapaskan, Tlingit and Haida was in fact current during the first decade of this century and received its most powerful exposition, by Edward Sapir, in 1915. What *has* occurred in genuinely recent times is that a renewal of interest in and field research on Haida has resulted in findings which reveal no basis whatsoever for grouping Haida with these other languages. Haida grammar turns out to be radically different from the way Swanton perceived it, and historical theories built on his description of Haida turn out to be untenable. As matters now stand there is no evidence to link Haida genetically with any other language in the world; like Basque and Ainu it must be considered an isolate (see Krauss 1969, 1973 and Levine 1979 for discussion and documentation of these points). In the light of our current understanding, therefore, Woodcock's picture of the Haida moving down the Skeena far from their original home in the northern interior and departing, one group at a time, over Hecate Straits for the Charlottes is an unsupported private speculation, irresponsibly presented as the most plausible — or even *a* plausible — reconstruction.

The foregoing has merely been a warm-up, however; Woodcock proceeds to inform the reader of

links that Edward Sapir and other linguists have found between North American language groups, like Athapaskan, and the Sinitic family of languages, which includes of course the Siberian and Mongolian languages as well as those of China itself. (p. 53.)

Sapir's total contribution to the North American/Asian linguistic hypothesis is a brief note which made much of a feature called tone, found in both Sino-Tibetan and Athapaskan languages, but which has been shown to have developed independently, and late, in the history of both language families. There have been a few other attempts to defend the Asian/Athapaskan link which are generally regarded as inadequate, and the firm consensus among linguists at present is that no convincing evidence exists to support the link — which indeed has no new advocacy for more than twenty years. The passage from *Peoples of the Coast* quoted above reveals, once more, how little critical understanding of source material has gone into the book. (For further discussion of Asian/Athapaskan claims, see Krauss 1973.)

When Woodcock writes about historical relations on the southern part of the coast his statements are equally wide of the mark. He comments of Mosan, a hypothetical family embracing Wakashan, Salishan and Chema-kuan (a small family on the Olympic peninsula which Woodcock

ignores): “the evidence for this linguistic relationship is *somewhat less convincing* than that for the original Wakashan grouping [our italics].” Mosan in fact has been abandoned as a hypothesis by Northwestern specialists — including its originator, Morris Swadesh, in the early sixties — for lack of good evidence. See, for example, Swadesh (1962), Voegelin and Voegelin (1967) and Thompson (1973), especially the discussion and citations on page 1000. “Good evidence” to a historical linguist means resemblance between languages too extensive to have come about by coincidence or mutual influence; in these terms, Mosan is now seen as a crashing failure and a textbook case of how not to establish linguistic groupings. Since Wakashan is a thoroughly established, non-controversial grouping and Mosan a grouping with no adequate basis at all, Woodcock’s comment is approximately like saying that the evidence for historical connection between Germanic and Hottentot is somewhat less convincing than that between Germanic and Slavic.

Woodcock refers continually to glottochronology, offering the reader no explanation of what this term means. Glottochronology is a technique for dating the separation between two or more related languages based on assigning a numerical value to the resemblance between parts of the languages’ respective vocabularies. It is now generally regarded with extreme skepticism by linguists because it depends on all languages changing at approximately the same rate over time. There are so many clear exceptions to this notion — the most dramatic being Icelandic, which has replaced only a minute portion of its original vocabulary during the same thousand-year period that Anglo-Saxon developed into modern English — that references to glottochronological calculations have pretty well disappeared from linguistic literature. (A particularly devastating critique of both the theory and methods of glottochronology may be found in Bergsland and Vogt [1962]; note especially the author’s final statements following the discussants’ contributions.) To look for a time depth for the Haida/Na-Dene separation by means of glottochronology, as Woodcock does in the passage quoted from page 47 above, has roughly the same scientific status as a suggestion that use of a dowsing rod would be the best method for discovering the whereabouts of Atlantis.

In short, throughout this chapter Woodcock’s statements indicate almost total ignorance of current Northwestern<sup>3</sup> linguistic studies. Wood-

<sup>3</sup> Ethnographically, the Northwest Coast is a separate culture area from any other. Linguistically this is not the case, since the particular constellation of features which one associates with Coastal languages most distinctively is also found throughout Alaska, in the British Columbia interior and as far east in the United



cock has the Coast Salish moving west to the sea from the interior, when most linguistic evidence points to a Salishan homeland around the mouth of the Fraser, with a relatively late movement through the mountains to the interior. Contrary to Woodcock's scheme, Salishanists generally accept the origin of the Bella Coola as the northernmost part of the original Salishan coastal continuum, separating from the rest of the proto-Salishan population well before Coast and Interior branches of Salishan were formed. As far as Wakashan is concerned, Woodcock has the ancestors of the present Wakashan speakers moving down the Fraser to the coast, up to Alaska to pick up Eskimo-Aleut fishing technology around Yakutat Bay, turning around again and moving back down the coast and then moving lock, stock and barrel over to Vancouver Island. Linguistic evidence overwhelmingly supports Vancouver Island as the site of the Wakashan breakdown into Kwakiutlan and Nootkan branches, but the tortured migratory movements in Woodcock's scenario preceding this breakdown are literally incredible. Given the implausibility of these peregrinations, it seems hardly necessary to ask the final question: since it was, by Woodcock's account, the *common* ancestors of the current Wakashan speaking peoples who encountered the Eskimo-Aleuts, why do the Kwakiutlan groups not also have Eskimo-like gear as the Nootkan peoples do?

In his speculations about Tsimshian, Woodcock finds in the myths of Temlaham sufficient reason to derive the present Tsimshian populations from a downriver movement along the Skeena. The most recent authoritative archaeological work indicates that the Tsimshians are more likely to have arrived on the northern coast via direct routes along the "inside passage" and moved *up* the Skeena; there is also some tentative linguistic support for a greater length of coastal residence than upriver residence for the Tsimshian-speaking peoples. Woodcock refers to myths and traditional genealogies repeatedly and seems unaware that ethnologists frequently get significantly different genealogies from different informants — or even the same informant at different times — regardless of how important exact specification of kinship may be in a society. Genealogies and myths are in many cases charters for the way things are within a particular group; as conditions change the myths may also change, and both myths and kin relations are subject to manipulation by different sectors within a society who have different interests to promote.

There is much more in this chapter which deserves comment, but the

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States as western Montana. Thus the relevant *linguistic* area is referred to as the Northwest.

foregoing gives some idea of Woodcock's reliability in linguistic matters. Those interested in the reconstruction of prehistoric migrations hypothesized by linguists specializing in Northwestern languages should consult the forthcoming article by Laurence C. Thompson and M. Dale Kinkade, "Linguistic Relations and Distributions," in Volume 7 of the Smithsonian Institution's *Handbook of American Indians*.

In several places where Woodcock's comments are based on sound research he appears to have omitted the sources from his bibliography. On page 138 Woodcock refers to a practice of village exogamy hypothesized for the Coast Salish, but does not mention Wayne Suttles' paper first proposing and defending this thesis (Suttles 1963). On page 119 he refers to suggestions that the fat-rich diet universal on the coast compensated for a lack of available starch, a possibility first raised in print by Nancy Turner in Volume 1 of her monograph *Food Plants of British Columbia Indians*, which does not appear in Woodcock's bibliography.

Finally, something needs to be said about the type of ethnic characterizations which one encounters in *Peoples of the Coast*. Given Woodcock's proclaimed sympathy with the cultures of which he writes, we may well be surprised at his repetition of pejorative stereotypes dating from the Boasian era. On page 7 Woodcock refers to "tall and massively carved heraldic poles celebrating the megalomaniac concern with prestige that dominated this richest of primitive cultures"; on page 21 he comments on "this elaborately ceremonious culture, with its curious combinations of aesthetic sensibility and megalomaniac pride. . . ." Eight chapters later Woodcock tells us that Benedict's description of the Kwakiutl as megalomaniac and paranoid is highly questionable, and notes that "the interesting fact is that the memories of those who took part in [potlatches] do not suggest vindictiveness so much as the kind of pleasure in a hard-won victory that we expect in our own culture from a successful sportsman." To give voice to ethnocentric stereotypes when you don't know any better is not commendable; what should be said of someone who does know better and repeats the stereotype anyway? Perhaps Woodcock really does not know better after all; elsewhere in the book he applies the same tired and vacuous old nineteenth-century labels "Dionysian" and "Apollonian" which Benedict used so freely half a century ago and which do so little to illuminate the fundamentally remote societies of the Northwest Coast.

As we have stated above, there is no room in a review article for more than a very partial survey of the errors in this volume; those mentioned in the preceding pages are a small but representative sample. Two questions now arise: how was it possible for such a book to get published

and, even more important, *why* was it published? As far as the first question is concerned, many readers assume that the very fact of publication means some minimum standards have applied; surely the publisher has sent the book out for review to experts who will point out gross inaccuracies. Such is the usual practice, and Hurtig followed it in this case, sending the manuscript to Bill Holm, one of the outstanding authorities on Northwest Coast art, at the Burke Memorial Museum, University of Washington. Holm responded with detailed criticism which Hurtig completely ignored. So much for the publisher's interest in the author's accuracy.<sup>4</sup>

But the second question points to a far more serious problem than the cynicism of publishing houses. What is at issue is not Woodcock's motivation in writing *Peoples of the Coast*, but the fact that the book had a niche available for it — the need for a synthesis to which we alluded at the beginning — that was not filled by any work of genuine expertise. Scholars simply have not written for a popular audience, for the most part; their knowledge and interpretations and especially their methods have remained a private preserve. It is true, of course, that investigators are quite properly reluctant to publish findings with insufficient background and documentation; this in no way excuses Woodcock, who could have contacted people in specialized fields and obtained some idea of what the current state of the art is in those fields in informal conversation or correspondence. But if researchers will not make their findings and even controversies available to the public in relatively non-technical terms at *some* point, they have little right to complain when an uninformed author presents a thoroughly botched version of their field in a readable form and has it enthusiastically taken up by a general audience.

The same sort of non-communication between physical scientists and the public has existed for a long time, and the result has in part been a steady stream of pseudoscience by an assortment of cranks all over the world.<sup>5</sup> But, as far as we are aware, there is nothing within the field of, say, astrophysics which is comparable to *Peoples of the Coast*. A publisher who sends a book on stellar evolution written by a journalist to a top physicist for review will probably pay attention to any citations of major or even minor errors. Non-experts are uncomfortable with the subject

<sup>4</sup> The best one can say on Hurtig's behalf in the present instance is that they are far from the only publishing house which has displayed this sort of gross irresponsibility.

<sup>5</sup> For an amusing — and devastating — review of pseudoscientific writing up to the middle 1950s, see Martin Gardner's *Fads and Fallacies in the Name of Science* (N.Y.: Dover, 1957).

matters of the physical sciences and mathematics, it appears, because these subject matters have nothing familiar about them, and are often defined at physical magnitudes so minute or so vast that they entirely escape visualization. The social sciences are in a totally different position. Everyone belongs to society; virtually everyone has some position in a kinship network; everyone speaks a language. Consider the following description Woodcock provides of his general critical method:

I do not propose to enter the more recondite disputes between anthropologists; in the rare cases where different opinions seem important in affecting the larger pattern of interpretation I shall state the varying points of view and shall argue from the viewpoint which my own observation of the people and their surviving culture — an observation extending over almost thirty years — leads me to regard as the most credible. (p. 22.)

The anthropologist or linguist is well aware that much of what seems to be going on in a society is present only in appearance; that cultures frequently mask their operations behind obscure symbolisms and apparent contradictions; that our own cultural or linguistic preconceptions often blind us to fundamental aspects of social life — our own or others. Much training in these fields is devoted to learning how to ask the right questions, how best to understand frequently bewildering data, and how to keep one's own cultural assumptions at bay, as much as possible. But people outside these fields are rarely aware of any of this, just as they are probably unaware that Woodcock's thirty years of "observation" have not provided him with a basis for choosing sides in technical disputes.

The crux of the matter seems to us to be negligence on the part of experts in the human sciences. But the *reason* for the reluctance of scholars to write for non-scholars is that they have little incentive within their professional worlds to do so; indeed they are often penalized for doing so. Most social scientists are employed by universities, which put a high premium on specialized research and publication and provide no rewards, by and large, for dissemination of information outside the academic world. A linguist who writes a paper about how to prepare teaching materials in Native languages and publishes it in a prestigious journal of psychology or culture will get a certain amount of credit for it from tenure or promotions committees, whereas if she or he actually prepares the material instead of writing about it, it is almost a certainty that there will be no professional recognition of the work within the academic hierarchy; there may even be subtle censure. The knowledge that social scientists gather is potentially a powerful corrective for racist

and cultural supremacist viewpoints which represent a definite threat to democratic society, yet there is absolutely no commitment obvious on the part of most academic institutions to encourage their faculties in getting this knowledge across to the wider community.

Given this situation, it is not difficult to understand the way Woodcock's book has been treated in reviews. Of the dozen or so reviews we have seen, only one was written by an expert in Northwestern studies; this was George F. MacDonald of the National Museum of Canada. As it happens, MacDonald's review is also the only genuinely critical examination of the book we know of. For the various reasons discussed above, there does not seem to be much perception on the part of newspaper and magazine editors that the value of a book such as this one depends absolutely on the accuracy of its contents — not on the author's writing style, not on the layout nor on the attractiveness of the photographs, illustrations, maps or typeface. Given the disgraceful treatment of North American Native peoples by uncomprehending Europeans during the past several hundred years, the last thing we need is yet another distortion of their history.

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