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Changes in Haida Argillite Carving, 1820 to 1910
UCLA Ph.D. Anthropology

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KAUFMANN MANUSCRIPT: GENERAL COMMENTS

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Trick

This is the most revealing and important study yet made of Haida argillite carving, and especially of its earliest phases which are the most interesting. Its strength lies in the scholarly way in which a large number of examples have been found, dated, and subjected to a revealing kind of analysis. Even for readers who have reservations about the kind of analysis used, the study will be accepted for its impressive descriptive and historical contributions. My main single reaction is a very positive one, because the study does add a great deal to our understanding of this art form.

It is also a contribution to theory, and I am not sure that I am as competent to judge that aspect. The theoretical approach, componential analysis, is one that is now in vogue, and this study is one of about half a dozen known to me that attempt to apply this new approach to the study of material culture. I believe that advocates of the approach would consider the study at least a qualified success. For the general reader, the theory will get in the way and make the book harder to read. Some may be baffled or annoyed by it. My own reaction is that I am not completely sold on all of the machinations involved and I am intrigued but not entirely convinced by some of the deeper findings, but the payoff in historical results is so impressive that I wouldn't challenge the approach.

I give below a bushel or two of negative reactions on small and middle sized details. I guess that is what a reader is supposed to do. Please keep them in perspective, because I do not mean them to add up to any sort of rejection of the study.

I find the writing in places unclear, overly casual, or a bit "flip". Careful editing down to the sentence level may be required.

It takes quite a long time to get to the really interesting parts of the manuscript. Chapters I and II have the appearance of dutiful attempts to provide a brief context, but they suffer from the usual dilemma of such an attempt: they have sections too brief to be meaningful (there are some things that just can't be reduced to a paragraph), and they are riddled with small errors. The early parts of Chapter III are hard going, but the payoff begins to come in the later parts. For me, and for most readers, Chapter IV is the most interesting by far. I am not sure what my recommendation would be on this point; maybe I could just put the question: Is the format of a doctoral dissertation the best format for a book?

Another comment that is really a question is this: To what degree would you present this as an "art" book? The study is not concerned with art in the most usual sense, which involves aesthetic value. It is a study of formal and iconographic attributes without reference to their aesthetic effects, and could be just as well applied to painted pots or imbricated basket (well, almost, I think you get my point). It just happens that a lot of the argillite pieces are fine and intriguing works of art. If you do a great job on the illustrations you will give the reader that unmentioned bonus.

Kaufmann thesis: Comments on Details

Introduction

- p. 1. Only two studies of any value? I can think of more: Swanton, 1905, Ch. 10; Emmons' study of Chilkat blankets; some mention here should be made of Barbeau's several books on argillite.
- p. 2. I disagree that her cut-off point of 1910 has the meanings she claims for it. The "end of Haida aboriginality" and the time when the Haida came to be under "direct Canadian domination" would be better set at 1890. In fact she gives a lot of evidence later (p. 32) for 1890 as the turning point of Haida culture.

Having arbitrarily chosen the date, she uses it later to mark the end of the tradition of argillite carving (p. 55), "the last years of the tradition" (p. 49). Her wording on page 64 is closer to the truth: "by 1910, the entire tradition wanes". I agree that there was a waning about that time, but the tradition continued (as any reader of Barbeau's "Haida Carvers" knows) and is still alive and changing today.

I don't object to her using 1910 as a cut-off date for her study, as long as she doesn't load that date with other meanings. While on this topic, though, I have to add that I am not fully convinced that she obtained a fully representative sample for that decade (but one can't tell from the information she gives).

- p. 2. On the population figures, 7000 to 588: she should make it clear that she is referring only to the Canadian Haida (between 1/4 and 1/3 of all Haida lived in Alaska). The figure of 588 is for 1915 (see also page 34 where she uses the same figure for 1905).
- p. 5. Who's Berger? She doesn't appear in the Bibliography.

Ch. 1. Haida History

- p. 10. Haida also live in Alaska.
- p. 10. The Queen Charlottes lie between 51-55 and 52-15 N. Lat. and about 130-00 and 133-10 W. Long.
- p. 10. The east shore of Graham Is. is not "dissected" (?) with inlets and rocky slopes.
- p. 10. What are "tentative origins"?
- p. 11. The summary of family histories is too brief to have any meaning, and contains errors: the Sand Town People and Middle Town People didn't "merge"; they didn't migrate to Alaska from Masset, but Langara Is.; and they didn't go to Kasaan, but more likely to Kaigani.

- p. 12. In her description of social organization she doesn't distinguish clearly enough between the household group (consisting of ego and the others she lists), the "lineage segment" (consisting of probably several related households), and the village (consisting usually of more than one lineage segment, despite Swanton's general comment).
- p. 12. Ou Haade means Gao Haida "Inlet People", the general name of the people of the northern Charlottes, not all Haida.
- p. 12. For "Northwest Coast creation myth" read "Haida creation myth", as Ne-kil-stlas is the Haida name and other tribes had different names for this being.
- p. 13. I am not sure whose lists of crests she is quoting, but I don't think it is complete. Also, I think "rainbow" and "clouds" are two separate crests.
- p. 15. She doesn't give her source for saying that "The existence of the Queen Charlotte Islands was noted by DeFoyte and others in the 1640's". What does she mean? What others? I don't think historians would accept this comment.
- p. 16. It was at the north end of Graham Island that Douglas met Connehaw.
- p. 17. Suggest deletion of "on the Northwest Coast there" (Cook had seen houseposts at Nootka earlier).
- p. 17. Douglas planted a few beans, did not introduce potato cultivation, and Suttles does not say so. The Haida already cultivated chewing tobacco.
- p. 17. Haida women spent the night aboard, but there is no evidence that Captain Douglas (ahem) "spent the night" with one of them.
- p. 17. If "quelched" wasn't a word before, it is now, and a dandy.
- p. 19. 80 feet is really too big for a dugout. The largest measured ones were about 65 feet, and traditionally the largest state canoes were "12 fathoms" long.
- p. 22. It was "the North West Company", and it merged with the HBC in 1821.
- p. 23. (bottom) In 1849, the British Government, through a Crown Grant, awarded the Hudson's Bay Company the lands of Vancouver Island, on the condition that they open them for colonization.
- p. 24. There was no white settlement on the Queen Charlottes until the 1880's. No treaties were ever made with the Haidas, and no "land allotments" until 1882.
- p. 25. The "Una" incident was only one of a series. Quite a lot of mineral investigation proceeded. That is why Captain Torrens was "nearly massacred" (?) in 1859.

Essexton?

- p. 27. In what sense was it "final" change?
- p. 28. The blanket had replaced the sea otter cloak many decades before 1870.
- p. 28. There was no such thing as the "United Church of England". Collison belonged to the Church of England or Anglican Church.
- p. 29. Slavery wasn't abolished on this part of the Northwest Coast in 1855. Collins, whom she quotes as source, was writing about Indians of Washington State. The Haida kept slaves until they were Christianised about 1880.
- p. 30. For Stirling and Sith read Sterling and Smith.
- p. 30. The paragraph on Indian administration is misleading here. Haida reserves were not laid out until 1882; they were not meant for "agricultural pursuits"; and no "family allotments" were laid out.
- p. 32. What the Haida feared was that their acceptance of reserves would prejudice their case that the aboriginal title to all of the land had not been extinguished.
- p. 33. That is, the Canadian Haida. The Alaskan Haidas had villages of their own at the time.
- p. 34. For 1905 read 1915.

Ch. II / The Argillite Activity

- p. 35. There seems to be the implication that the potlatch was itself a "religious" event. This was not the case, according to the way in which we use the concepts "religious" and "secular" on the NW Coast. It might however accompany a religious event. See later discussion about the term "sacred".
- p. 36. Holm has illustrated the bowl collected by Dixon, and made the point made here. Perhaps Holm's reference should be acknowledged. Inverarity also illustrates the bowl.
- p. 36-7: There is no need here to mention ground slate; it needlessly brings in a matter about which there is some debate. The "slate", needless to say, was not Haida argillite.
- p. 37. The stone sculpture complex discussed by Duff was far to the south, in the Gulf of Georgia and Lower Fraser. Perhaps that should be made clear.
- p. 37. This idea that animals and mythical creatures were treated essentially as human figures, with added identifying features, is often claimed, but only true in a small number of cases. Most animals had animal forms (see what she says about the 1820 pipes on pages 123-4).

- p. 38. Please: the Royal Canadian Mounted Police. Also Hobson was not the first white man to visit the quarry; many have been there; eg., Duff photographed it for the Provincial Museum in 1954.
- p. 39. Why would the amount of argillite packed out by earlier carvers depend on their canoes? The limiting factor is the back muscles.
- p. 39. I don't believe that the earlier carvers used fish glue to prevent the argillite from cracking. I have heard that they buried it in wet earth or wet sacks before carving. This was supposed to prevent it from hardening, and it is true that its surface hardens appreciably after long exposure to the air.
- p. 41. The ancient labrets found in the Fraser River region were not made of "argillite" in the present sense. The term is a geological catch-all for rocks of a certain character; nobody would suggest that these labrets were of Queen Charlotte argillite.
- p. 41. The Masset labret reported by MacKenzie was said to be of "slate", and I think in this case he meant real slate, not Haida argillite. The Haida had never heard of labrets of any kind of stone.
- p. 41. While on this point, the "charm" Figure 4-3 was collected about 1890. Stylistically, it looks older than that, but Miss Kaufmann has little basis for assigning it to the 1820's rather than say the 1830's. Is this a case of bending the evidence a little bit to produce an aboriginal argillite charm?
- p. 41. In the 1890's and later, a lot of wooden model totem poles were also made, many of them commissioned by Deans, Swanton and others.
- p. 45. Sea otter were not "unavailable" by the 1820's. but they were getting scarce.
- p. 49. "The last years of the tradition" ??? The two pieces then mentioned were bought in 1896 and 1901. A good 12" pole by a present day Haida carver goes for over \$500. The last years are yet to come.

Ch III - Componential Analysis of Argillite

- p. 52. explains the purpose of Appendix I. This may be a good place to make a couple of comments about Appendix I. (pp. 170-173). It lists the key objects which were used to create the chronology.
- a. The list of objects is of little use to the reader because it gives no information on what the objects are (functional prototype, iconography, etc.). It is just a list of numbers. A few, but not many of them can be found in Appendix II. I would like more information about these crucial objects - in fact I would like a list of all items included in her study.

b. A study of the list raises some questions about her decade designations, questions which cannot be answered with the data she provides. Why, for example, does she designate 5 of the Wilkes pieces to the 1830's and 4 to the 1840's? As far as I have been able to find out - and Miss Kaufmann gives no information to the contrary - all the Wilkes pieces were collected at one time, 1841, at the mouth of the Columbia River, a gift from a Hudson's Bay ship captain who had just arrived from the northern coast. Taking all the Wilkes pieces listed in App. I and II, Miss Kaufmann allocates 1 to the 1820 decade, 7 to 1830, and 4 to 1840. Why?

Consider also the 3 Catlin pipes, all obtained by Catlin from Clark in 1832. Miss K. assigns 1 to 1820 and the other 2 to 1830. Why?

She seems to show a tendency to assign the pipes with Haida iconography to the earlier decades and those with Western iconography to the later ones, thus strengthening her conclusion that the earliest pipes were exclusively Haida in iconography. I tested this suspicion with the pipes in Appendix II which have definitely firm dates. Of the 4 in the 1820 decade, 2 have Western elements of iconography; of the 7 in the 1830's, half are Western. On this evidence, Western iconography is contemporaneous with Haida in these two decades (negating one of her main conclusions), although Western predominates later. Now I am very interested in this point, and with the data she provides that is the result I get. Quite likely her conclusion is correct; my point here is that she hasn't provided me with full enough data to test her conclusions.

p. 54. On first reading I found the "preview conclusion" impossible to understand, because she doesn't give quite enough information on what she means by HH, HHW, etc., and one has to read on to find out, then come back and attack it again. I am still not quite sure what I am being told by "An HH and a VH may indicate a fourth series from 1880 to 1910".

pp. 54-55. As I at first was, I think other readers will be taken aback by the statements: "Primarily Haida sources are used in the first decade" and "1820 artifacts are characteristically Haida in both form and content". Here's why:

a. Content. See comments above, I still have the impression that Haida and Western iconography are contemporaneous in the earliest argillite.

b. Form. These items are mostly pipes, and the pipe is not aboriginally Haida but was introduced by the white men. The Haida grew a form of tobacco, until the 1870's, which they chewed. I do not know when they adopted smoking pipes, but I would like to see some pre-1820 oval wooden pipes from the Haida before I accept the oval pipe as a "Haida" form.

The "panel" form has no apparent Haida precursor (although some of the iconographic content on the panels may be derived from rattles). When the content is Western, the panels seem to take their form from a ship's hull in many cases. I could argue that the "form" is therefore Western, not "characteristically Haida".

I think she is overstating the point that many or most of the earliest argillite pieces exhibit a typically Haida iconography.

dispute p 115

p. 55. I wonder whether she is over-reacting against the theory of the scrimshaw origin of the argillite activity? Everyone will grant her point that there already existed a well established Haida iconography. But something stimulated them to use the new medium, argillite, and to use it for new forms (non-functional pipes and pipe panels). She admits elsewhere that scrimshaw "influenced" those argillite items bearing Western iconographical content. I don't see that there is anything incompatible between a pre-existing Haida art and an influence derived from seeing scrimshaw to initiate these new forms.

In other words, I don't think her logic disproves the theory.

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Reading through all of the above, I realize that I might unwittingly be shooting a good manuscript down in flames by emphasizing minor points with which I disagree. That is not my intention.

Just a couple more points:

Chapter IV, pp. 140, 147. She says that subjects depicted in the later Haida II phase were often "sacred". I do not agree with that interpretation.

On page 116 she says that totem poles are "relatively sacred" objects. Actually they were simply displays of family crests, demonstrations of secular social prestige. There was no concept that they held any supernatural power; they were in no sense idols to be worshipped; by the usual distinction between secular and sacred they were not sacred.

If totem pole crest displays are taken as sacred, then the panel pipes of the Haida I period, which show crest animals, must also be considered sacred, and that would spoil her argument.

On page 140 and 147 she refers to "ceremonial boxes with sacred designs". On page 140 she says that such boxes "were kept from profane eyes", and cites Keithahn, 1963:94 as authority. Keithahn's comment is in the caption of an illustration showing a different type of box (which had no lid but was covered with a woven envelope of cedar bark). It is a dubious source, because: a) he shows a different kind of box, b) the box he shows was not the sort that had the cedar bark cover anyway, and c) nobody else has ever suggested that such designs were sacred, and I am certain that the decorated wooden chests which seem to be the prototype for the argillite chests were not "kept from profane eyes" or regarded as sacred.

The "sacred" idea could be better made for the model shaman figures (but where does that leave the argument if, as she says, the prototype for the early panel pipes was the sacred shaman's rattle?).

My own view would be that the best word for the late Haida forms would be "ethnographic", and that they were created in response to the new demands of the market - museum collectors rather than curio collectors.

p. 157. She claims here and in the abstract that her analysis of the anthropomorphic eye motif can be used as a dating marker for Haida art in other media. It is an intriguing thought, if true, but the couple of examples she gives unfortunately are not nearly enough to demonstrate its validity. If she is going to put it forward as anything more than a suggestion, she should give more examples to support it.

She goes further and says that the eye motif can designate a chronology for other tribal styles, such as Tsimshian and Tlingit. Again, I'd like to be shown.

Appendix II, p. 178, Figure 4-4 (for example): In the documentation on some of the Wilkes pieces in Appendix II is the comment "Fort George, Milbank Sound". I don't understand what that means. Fort George was the newer name for Fort Astoria at the mouth of the Columbia River. Milbank Sound is up the B.C. coast in Northern Kwakiutl territory where Fort McLoughlin was located. Can she make this clearer?