CHINOOK JARGON AND PIDGIN HISTORIOGRAPHY

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Abstract: The time and manner of the origin of the Chinook Jargon are examined, with an evaluation of a proposal that this was an indigenous, pre-contact event. The contrary is argued, with new evidence; parallels with African colonization and language change are suggested; and the historical-linguistic-antropological method is maintained for the writing of pidgin histories.


Key Words: Chinook Jargon, Ethnohistory, Pidginization, Fur Trade, Northwest Coast.

"No facts speak for themselves to lighten the historian's task... [P]roof is rare and probability comes in many sizes, only to be judged with art and a sense of responsibility" (Shafer 1980:53).

INTRODUCTION
The attempt to establish the existence of aboriginal (that is, pre-contact) pidgins outside the African continent is of rather recent date: Thomason (1980) has argued for Delaware Trader's Jargon; Drehsel (1979, 1981, 1983, 1984) for Mobilian Jargon; and both Thomason (1981, 1983) and Hymes (1980) have argued for Chinook Jargon. Suggestions or assertions are of course different from arguments about CJ's origin. For example, Judge Swan (1857:307) only "think[s]" that the language had been "in use for years" and presumably before its extensive use throughout the fur trading territory of the Hudson's Bay Company. (Ruby and Brown [1976:112] cite but do not quote a letter from Swan dated 28 December 1863 to the effect that CJ was "originally formulated to facilitate white-native trade on Vancouver Island... "). And Stefánsson (1922), himself an explorer, not a linguist, simply asserts the prior existence of the Eskimo jargon on the Mackenzie:

there had been a smaller jargon in use between the Eskimos and the Indians to the south of them even before the whites came, so that the habit of talking to any kind of foreigner was ingrained in all the Mackenzie Eskimos (1920:103).

Written almost 200 years after Russian traders are known to have come into the Alaskan region, this statement must have been made in ignorance of the kinds of facts that account for the origin and spread of a pidgin. Even if these men were diligent observers of Indians in their own day, this quality would not make them perspicacious historians. Historical argumentation requires data of several kinds, the nature of which becomes clear in the following exposition.

In this paper I limit myself to the contradictions of the case of Chinook Jargon, arguing against the view that it existed as a pidgin before the arrival of Europeans on the Northwest Coast of North America. The purpose is not merely to respond to the recent discussion of this language's origin, but also to suggest that since the problem facing scholars is a historical one there must be a multi-disciplinary approach to it. The interest of this problem is also multi-disciplinary. Anthropologists and historians are concerned with understanding the societies that were found in the Pacific Northwest at the arrival of the Whites, and linguistic anthropology looks at communities with a particular point of view. On the other hand, linguists must be informed by data that they are not prepared or inclined to look at, and words like that of Baker and Corne (1982) on Isle de France Creole, which examines both linguistic and demographic data, are not as numerous in pidginist literature as they ought to be. This is why historiography is part of the concern of this paper.

I use the terms jargon and pidgin as they have been commonly understood among linguists when discussing language contact and language change: a jargon is an unsystematic form of speech that characterizes either a given speaker or a group of speakers, or both; a pidgin is a stabilized form of speech, the consequence of pidginization in a language contact situation (for which see Samar 1971). A pidgin can therefore be the object or goal of one's attempt at learning to communicate; a jargon by contrast does not provide such a grammatical model. Derek Bickerton (1984 and elsewhere) has created confusion by using 'pidgin,' in talking about the origin of what he calls creoles, for a kind of chaotic speech, in other words, what has been called something like a jargon. (On
Nothing like the statements by explorers in central Africa (discounted in Samarin 1982a) has yet been found for any of the languages on the coast of northwest North America. Explorers in this part of the western hemisphere do not suggest that they found lingua francas that they could use. (The term is used in the generic sense: any lingual medium of communication between people of different mother tongues, for whom it is a second language). We may very well find that elsewhere on the continent specific kinds of interethnic relations led to the learning of certain languages. This would be a normal development, and knowing what the relations were, we would do well to look for such second-language acquisition. Cree certainly served this function early in the fur-trading period and possibly Ojibwa in the Great Lakes region. Nonetheless, caution must be exercised in dating the existence of these lingua francas, for they can merge very quickly.

This absence of reference to lingua francas is not in itself any argument for the nonexistence of such languages; the well-known historical axiom certainly applies here: silence is not evidence. The possibility of an existing pidginized lingua franca must certainly be considered. And its existence would not be inconsistent with the fact that it took a while before Europeans came to enjoy it. In this case it would appear to be a contradiction of Thomason's (1983:859) statement that the "first reports of a trade jargon in the region come from Europeans' travel journals" of the late eighteenth century. Since no report is actually cited by her, we might assume that what she means is that one could argue on the basis of this literature for a trade jargon different from CJ (possibly a Nootka jargon), whose existence is prepared to accept. In this regard Thomason (1983: 860, fn.46) summarizes the views of W. Sturtevant (1981). But this is not what she meant (personal communication). She means that when people recorded words that were foreign to Chinook or (by implication) to the Chinook area, these words can be taken as evidence for a jargon: for example, Nootka words cited by Lewis and Clark and others cited by Captain Vancouver in 1792 (Grant 1945: 225). In her own words: "Several of the earliest attestations of (genuine) CJ . . . are given as genuine Chinook — but we can tell now that they are actually CJ, for instance from the fact that they contain Nootka words (there weren't any in Chinook itself)" (personal communication). The argument is espoused also by Zenk (1984). Such reasoning is extremely hazardous. My own study of wordlists compiled for Ubangian languages in the 1890s reveals the intrusion of many foreign words (Samarin 1982a). These were recorded in ignorance, since the Europeans were depending almost exclusively on foreign personnel (some of whose vocabulary is included as indigenous) in their contacts with Ubangian peoples (Samarin 1982b, 1984b, 1984a). An analogous situation on the Columbia River would be the recording of a Nootka word by a European who thought he was getting from his informant a local word.

The origin of CJ has therefore been accounted for inductively, drawing on various kinds of data. The generally accepted explanation has been that the Jargon arose at the time of the earliest contacts with Whites. The following (Gibbs 1863:vi) is an example and probably expresses the common view of the time.

The origin of this Jargon, a conventional language similar to the Lingua Franca of the Mediterranean, the Negro-English-Dutch of Surinam, the Pigeon [sic] English of China, and several other mixed tongues, dates back to the fur droggers of the last century. Those mariners whose enterprise in the fifteen years preceding 1800, explored the intricacies of the northwest coast of America, picked up at their general rendezvous, Nootka Sound, various native words useful in barter, and hence transplanted them, with additions from the English, to the shores of Oregon. Even before their day, the coasting trade and warlike expeditions of the northern tribes, themselves a seafaring race, had opened up a partial understanding of each other's speech; for when, in 1792, Vancouver's officers visited Gray's Harbor, they found that the natives, though speaking a different language, understood many words of the Nootka. On arrival of Lewis and Clark at the mouth of the Columbia, in 1806, the new language, from the sentences given them, had evidently attained some form.

Catholic missionaries in the Columbia River area also believed in a post-contact origin of CJ. Take the following from Demers (1871:8), which is discussed below:

With Thomason and Hymes we have the revival of an alternative view, that the Jargon was in existence before the Whites arrived. (The earliest written record of it seems to be that of John K. Gill, compiler of a CJ dictionary, in a letter to James C. Pilling in 1891 (Pilling 1893:35-38). Gill's journalistic attempt at writing language history is evaluated below). Like the preceding, the present argument is also inductive but enriched with more information and different perspectives.

The argument for the pre-contact origin of CJ is examined in three parts: (1) that there are attestations of its early existence (Hymes 1980), (2) that certain linguistic features are best explained by Indian-Indian (and not Indian-White) contact (Thomason 1983), and (3) that indigenous social-cultural structures precipitated the pidginization of Chinook (Hymes and Thomason).

**EARLY ATTESTATIONS**

1. Hymes (1980) claims that Gibbs had noted "the presence of a jargon in the earliest contact near the Columbia." But since the passage referred to is the one quoted above, we see that the assertion may have nothing to do with a lingua franca at all but simply with bilingualism. Hymes, who is obviously relying on Gibbs' reference to "partial understanding" on the part of the Indians "to have been a jargon of mixed parentage," but it is difficult to see why this would be the most reasonable implication. 'Partial understanding' is exactly what one would find with imperfect bilingualism. It is through language that peoples' languages are, to use Gibbs' expression, 'opened up' to each other. On the other hand, if, in
this explicit manner, he meant to identify a contact language, it could just as easily have been one based on Nootka.

2. Hymes claims that Gibbs contradicts his own history of the Jargon in citing the case of Lewis and Clark, on whose arrival in 1806 the language had already "attained some form." For Hymes this would appear to suggest CJ's existence before trading took place, but too much should not be made of this statement. We must acknowledge that this is only Gibbs' interpretation of data not provided. It would appear that Gibbs was writing from memory, because 1805 is the year for the arrival of Lewis and Clark at the mouth of the Columbia River; specifying the day and the month would be somewhat arbitrary. Moreover, it is far from correct to say that these explorers provided examples of sentences from what Gibbs calls "the new language"; only words are cited. Finally, it would be a travesty of textual criticism to identify the "different language" of Vancouver and the "new language" of Gibbs as the same. Whatever the latter was, the former certainly meant nothing more than 'different from what we have encountered,' or something of that kind.

Perhaps Gibbs knew something about the history of White contacts and even residence at the mouth of the Columbia River. Certain it is that "the first sustained white presence near the mouth of the Columbia" was not that of this expedition, as Hymes claims (1980:412), because these explorers themselves report that when they arrived, they found evidence of many trade items in the possession of the Indians and, what is more important, that they were sufficiently versed in English that the Americans could converse with them. We cannot go here into the nature and extent of contact between Europeans and the indigenous peoples of the Northwest Coast. We can merely observe, by way of example, that the ships frequently had to anchor at sea for a month or more before they could cross the bar at the mouth of the Columbia (Blanchet 1956:143). During this time, of course, they would have been in contact with those living on land. Clark's words, in the first draft of his diary, are these: "I saw some Guns, a sword, money Powder flasks, salares Jackets, overalls, hats & shirts, Copper and Brass trinkets with few Beads only" (4 November 1805; Thwaites 1905[3]:195). Clark's record is the following: "I informed those Indians all of which understood some English . . ." (15 November 1805, first draft; Thwaites 1905[3]:224).

Since English-speakers had been in the area since the 1780s, there would have been ample opportunity for some English to have learned.

Whereas it is difficult, and in many cases impossible, to determine who was the very first European to arrive on a site during the period of exploration and discovery, we have at least for Captain Gray the record that his ship 'Columbia' (possibly piloted by John Kendrick, Jr.) entered the mouth of the river by that name in May 1792. Within a year or so, the river became the destination of trading ships in the Northwest Coast waters. For example, the American ship 'Jefferson,' on a voyage during the years 1792-1794, bought dentalium "from the Nootka to trade to the Haida, or to the Chinook from whom she would purchase elk hides for barter with the Haida" (Wike 1951:21). But trading with Chinook speakers probably took place as early as 1788, when Meares came into the Willapa Bay area — and probably even earlier in that decade by others.

My own position is that the patterns of inter-ethnic trade, originally embedded in indigenous political processes (Ray and Freeman 1978:6) were changed and intensified after conquest. Heizer (1942:1), for example, states that "The Columbia River (Plateau) peoples seemed to have visited California intermittently since 1800 (emphasis added) for the purposes of securing horses and for trade." These Sahaptian-speaking groups (Walla Walla, Cayuse, Klikitat, Nez Perce, and Umatilla) had already been touched by trade with the Great Plains tribes since the middle of the seventeenth century (Heizer 1942:4). (I am not suggesting here that horse trading characterized the Lower Columbia area. More is said concerning trade below.) The manner in which this early hunting and trade led to cultural and linguistic diffusion as far as Cook's Inlet in Alaska cannot, unfortunately, be dealt with here.

Hymes, adopting the argument proposed by John K. Gill in 1891 and 1892, cites as "prima facie evidence for the pre-contact existence of the jargon" the case of John Rodgers Jewitt (1783-1821), who was taken captive by the Nootka in 1803. That argument appears to stand only on the claim that Jewitt recalled "about a dozen words," attributed by Hymes to the Jargon. Gill (in Pilling 1893) actually says "about a dozen or less Indian words . . . , at least six of them are congeners of the Chinook." (And one of these six, not identified by Gill, is our own children's mimetic "Pow for the firing of a gun".) In the source cited by Hymes, Pilling casts doubt on what Gill had called "The first attempt at publication of trappers' and traders' Indian Jargon . . .," and had dated 1825. Pilling's criticism indicates the following history of the publication of Jewitt's account of his experiences, with his annotation:

1807 'A journal kept at Nootka Sound,' Boston. (It "contains no linguistic material.")
1815 'A narrative of the adventures and sufferings of John R. Jewitt,' Middletown, Connecticut. ("This work does not contain a Jargon vocabulary at all, but one in the Nootka language . . .")
1822 'The captive in Nootka,' by S.G. Goodrich, Philadelphia. (A few Nootka words and phrases passim.)

In response to this analysis of the ostensible importance of Jewitt's case, Gill actually admits that Jewitt may have written down the words some time after he had heard them and those who edited his work took liberties with his text. Gill may have made an attempt to collect some linguistic material in written form. All we know is that his notes, whatever they contained, were destroyed by his captors in a fit of anger. The published list cited must therefore be considered a sample of the language for literary purposes, a common practice in the period. Nonetheless, I find it unlikely that Jewitt was any better than his contemporaries and even other Europeans as late as the nineteenth century in collecting vocabularies. Documentation can be provided easily for Africa, the region I am most familiar with.

Jewitt certainly had opportunity for becoming familiar with the indigenous language at Nootka Sound, being
captive from 23 March 1803 to 16 or 17 July 1805. This is suggested by McKelvie (1949), who, however, assumes too much, I believe, in saying, "As the young armourer [blacksmith] became acquainted with the Nootkan language he was able to hold many talks with the old chief, supplementing his knowledge of the native tongue with such English as Maquinna [his captor chief] possessed" (1949:18). My own reading of Jewitt's *Journal* does not lead me at all to concur. One ought to consider the possibility that Jewitt depended more on his English than on his knowledge of Nootka, since he had as companion John Thompson, the only other survivor of the ship that had come from Boston, and he kept up his hope of escape or rescue. In any case, it must be considered very unlikely that the members of an ethnolinguistic group, most of them undoubtedly monolingual, would have used a foreign language in talking with men who were the slaves of the chief. Imagining the villagers as eager language learners who wanted to improve their chances in trading with other speakers of English is entirely consistent with the social basis of the fur trade (described in Fisher 1977). This was not in the hands of commoners but in those of the 'upper class' — titleholders in Donald's (1984) analysis.

This account therefore leaves no ground for believing that Jewitt's record provides evidence for the pre-contact existence of CJ. It was Nootka that he learned — possibly a deviant form of Nootka, and one that the natives had allowed themselves by this time to use with Whites. And it may have have had words from other languages. Even if an incipient CJ was already being introduced at Nootka Sound, which by 1803 had lost its former importance as a source of pelts, this hardly demonstrates the pre-existence of CJ.

Whereas a certain linguistic (but not genetic) relationship between Chinook and Nootka can be argued, I have nothing here to say about what we find in Jewitt's journal. There, on 30 September 1803 he recorded that he had learned from visiting Indians that there was a captain with a wooden leg "at the Cheenooks" (Jewitt 1931). From the context it would appear that this was on Vancouver Island and does not apply to the Columbia River area. However, Johnson (1978:30-31) identifies Cheenook with Lower Chehalis at the entrance of the Columbia River.

4. Gill is further cited by Hymes, this time for "direct testimony" from an Indian (one who claimed that he was a "man grown when Lewis and Clarke came to this country") that many tribes talked to each other "in this ancient Volapük" (Pilling 1893:36). Although Hymes credits Gill's report, Horatio Hale (as cited by Pilling) did not, saying that what the Indian must have been talking about was the Chinook (that is, indigenous) language, not the Jargon.

Pilling and Hale were fulfilling the historians' responsibility in subjecting data and testimonies to the test of credibility. In fact, they did not go far enough. Internal evidence in what Gill has written suggests that because he was sympathetic toward the Indians, deploiting the way they had been and were being treated by Whites (who had been expropriating their land since about 1830), he sought greater respect for them. This he did, I suggest, by claiming that they had carried on trade before Whites arrived and that they did this in their own trade language, unlike the one which was an object of mockery among the Whites. Because his goal was laudable and reasonable (and because it parallels so closely recent arguments about American Black English and some pidgins), it does not acquit him of being relatively ignorant of Pacific coast history, of having insufficient linguistic data, and of being naive in his argumentation. We do indeed know more now than was known in Gill's day. Moreover, it is precisely because so much more is known about that period that CJ's post-contact origin seems the more defensible one.

A LINGUISTIC ARGUMENT

Thomason's (1983) discursive argument for the existence of CJ before the Europeans arrived can be summarized as follows. In the late eighteenth century there were two Jargons in the Pacific Northwest, one based on Nootka, the other on Chinook. Whatever the linguistic nature of the former was, the latter was already a "stable pidgin," says she, on and near the Columbia River before the other got there (1983:859). The evidence adduced for the argument is linguistic and social-cultural. Since the latter is similar to Hymes' we shall look at it below, but it is supported, she says, by CJ's phonological and syntactic structure: "The attested structure of CJ is easiest to account for if it [the structure] existed in stable form before the Europeans came; ... it is harder to explain [the phonological and syntactic features] if the pidgin crystallized out of communication between Europeans and Chinook speakers (and perhaps other Indians too)" (1983:859). And of these two parts of the grammar "it is the phonology that critically weakens the hypothesis of European participation" (1983:865). This is so because (1) if the Chinooks and other Indians had borrowed words from other indigenous languages, they would not have altered the phonology, and (2) the Europeans always did.

'Borrow' is my term, based on my understanding of Thomason's text, which she actually rejects: "I don't believe 'borrowing' is the right concept to apply in a situation of pidginization: there's no language, yet, to borrow anything into" (personal communication). Avoiding an involved discussion of the nature of pidginization at this point, I should only record that in the genesis of Pidgin Sango there was indeed 'borrowing' right from the beginning. In fact, two of the earliest borrowed words — for 'spear' and 'paddle' — were not even needed, because indigenous Sango already had words for these concepts! Even more important for the language was the incorporation of the Kikongo copula, introduced by foreigners when they came to the Ubangi basin (Samarin Ms 3). She concludes: "The persistence of such [marked] features is conclusive proof that most Indians learned CJ from other Indians [in pre-contact times], not from whites; they also show, I think, that whites were not involved in the original pidginization process" (1983:865-866).

Thomason's linguistic argument has the merit of being new and of being scientific. Both these features are attractive. Being scientific — in the literal sense — it introduces a quality of rigour to what must otherwise and
inevitably be discursive argumentation, and it introduces theory (in the form of markedness). Nevertheless this attempt is far from satisfying, and there is another way of explaining the existence in CJ of non-simplified, indigenous (i.e., marked) phonetic features. Before Thomason's argument is subjected to critique, two observations can be immediately made. First, apart from whatever merit Thomason's proposal might have for accounting for an Indian phonology in CJ at a given point in time (and her dates are rather late), it does not prove that the arrival of Europeans had nothing to do with the pidginization process - that is, with the very beginning of CJ. Second, whatever cannot be attributed to foreigners (i.e., what is indigenous) can be simply explained by the later development of CJ. In other words, we can assume that there was a very divergent jargon at an early stage that was subsequently 'nativized' as it spread and became more widely used by the indigenous population. This explanation, with evidence from elsewhere, is taken up below.

The phonological structure that is so important to Thomason's argument is weakened when one examines it on textual and sociolinguistic bases.

1. The 'textual-critical' analysis looks at the literary sources which provide Thomason with the data for her phonological analysis. As in the study of ancient manuscripts, the scholar's task is to establish the best text by comparing all versions and by fully understanding their histories. Johnson (1978) has made an important contribution to the philological history of CJ linguistics by demonstrating how various texts are related. It is demonstrated that compilers of CJ 'dictionaries' borrowed from each other. Without having myself examined Thomason's data from this perspective, I note, only by way of example, that her phonemic inventory of CJ is one established by Kaufman. That work is given a date (1968), but it has never been published. Therefore it is impossible to determine whether or not it would pass a 'critical text,' that is, as a kind of textus receptus. Something we can examine is the material prepared by the Catholic missionaries from Quebec (Demers 1971). According to Thomason (personal communication) we find there a 'phonetically sophisticated rendering of CJ words,' because Demers and others "were interested in getting the sounds down correctly." My examination of this material leads me to no such conclusion. Moreover, even if one could argue for some understanding of the nature of complexity of CJ phonology, it would not be possible to attribute it to Demers, the only one of the first group of missionaries who seemed to apply himself to learning something of Indian languages. The work cited by Thomason is something other than what one might deduce from her references. The full title being given below, I only need to point out that although some of it was "composed" in 1838, it was subsequently revised and added to, as Thomason (1983:828) indicates in her text. One must consider the possibility that the revisions were done in the context of changes in the places of missionary activity - new languages (or more speakers of certain languages) were part of the sociolinguistic environment of Catholic missionaries. Without an examination of other texts, especially the original manuscripts of Demers (if they exist), we are obligated to be skeptical of the value of this work in establishing the 'Indianness' of CJ in the 1830s. Others might come to a different conclusion on examining the same texts, but this would only illustrate the fact that textual criticism is only partly philological: it is also an historical science. Hence all argumentation must begin with the credibility of one's texts.

2. The sociolinguistic analysis seeks to determine what can be known about the use and distribution of CJ at the time of her principal literary sources. These, as we have seen, are materials from the Catholic missionaries who came in 1838. It appears that these actually contradict her conclusions:

(a) Although Thomason attributes to CJ a phonology that was 'Indian' in its complexity, the missionaries reported, on the one hand, that there were many ways of pronouncing CJ and, on the other hand, that it was a very simple language. Although it is possible to explain this observation about variant pronunciations as being another instance of the belief in pre-linguistic days of haphazard variation in 'primitive' languages, the context, with the following statement, suggests a different interpretation: "Each Indian tribe gives a special accent to the Chinook . . . " (Demers 1871:12). They reported that the jargon was "distorted in pronunciation" and was very easy to learn (Blanchet 1956:19; cited by Thomason as Notices 1956, but sometimes catalogued as Society for the Propagation of the Faith). The latter statement has to be compared with observations made about other languages of the area: The real Chinook language "is of almost insurmountable difficulty"; all the native languages are "difficult to learn and difficult to pronounce"; the "chief difficulty" in studying the native languages west of the Rocky Mountains was "in the pronunciation" (Blanchet 1956:19, see also 20, 150). On the ease with which CJ could be learned, we note that "in a few weeks [after his arrival] Father Demers had mastered [CJ], and began to preach" (Demers 1871: 18; see also Blanchet 1956:14, 150 for similar observations). An easy CJ would not have had the kinds of sounds that Thomason claims existed at this time.

(b) The kind of 'Indian' phonology that Thomason argues for might imply a generous distribution of speakers among the people of the area, but there are significant limitations on its knowledge and distribution. Between Fort Okanagan and Walla Walla, for example, they could not make themselves understood without an interpreter (Blanchet 1956:18). And in spite of the fact that Blanchet, who was not the 'linguist' of the party, reported that the jargon was "understood and spoken almost everywhere" (1956:169), we learn that the observation was qualified: a "large number" of Cascades natives and a "part of Klickitats" understood the "jargon" (Blanchet 1956: 19, 20); it was downriver from Fort Nez Perce that the jargon was understood.
Thomason, as I understand her.

The social-cultural explanation proposed by Hymes and Thomason for the origin of CJ before the arrival of Whites in the area is based entirely on the fact that the region was linguistically very diverse and that there was a great deal of multilingualism among its inhabitants. What constituted the setting for the emergence of a pidgin was a "vigorous" and "intensive" trade (Thomason 1983:862), exogamy, and slavery. This context, Thomason says, was not only "appropriate for pidgin formation" (1983:866) but actually suggests its "need" (1983:861). In trade, for example, Indians couldn't be fluent in all the languages they had to use (1983:862), presumably either at the mouth of the Columbia or at The Dalles, or at both places, so they pidginized Chinook. The reason that the Chinook language was learned is that the Chinooks were considered superior, and the reason it was learned poorly is that it is a difficult language. So reasons Thomason, as I understand her.

With respect to trade, the argument is not at all convincing. Neither Hymes nor Thomason demonstrate that the structure of trade relations was one that led to the pidginization of Chinook. Thomason only suggests it by implying that the number of Indians actually engaged in trade was high and that they were personally in contact with speakers of a large number of different languages. This is not at all necessarily true. It can be demonstrated from other parts of the world that pre-European trade was sometimes conducted by a small proportion of an ethnic group and that those who engaged in linguistic communication were very few indeed. Moreover, these negotiators had acquired competence in other languages through 'normal' means. (This has been demonstrated for Bobangi trade by Harms (1978, 1981). It was common on the Congo River for one to leave one's slaves or kin up to a year or longer as security against the ultimate completion of a transaction. Since multilingualism and trade have been ubiquitous in the history of human beings, only rarely leading to the pidginization (as far as we know), those arguing that Chinookan trade led to the pidginization of the Chinook language would have to demonstrate that it was unusual indeed.

Boit, who was with Gray on the 'Columbia' in May 1792 when they visited what came to be called Gray's Harbor, said that although the local people understood a little Nootka, when they spoke to each other they used a language which he had never heard (Gunther 1972:84-85). In October of that year Lt. Whidbey stopped at a village in the same harbor where he "thought they spoke Nootkan, but he was sure it was not their own language" (Gunther 1972:85, see also 75). On the other hand, speakers of Salish were found on Vancouver Island by the end of 1790 and could have been there for a long time (Gunther 1972:66-67). (Gunther was presumably working from primary documents, and I have to rely on the accuracy of her understanding of them.) Boit's record would appear to be of capital importance, for it suggests that the personnel of the trading and exploration expeditions (not just the Whites on them) used Nootka (that is, their own form of it: a kind of jargon) in trying to make themselves understood with the local Salish people. If CJ had already existed, they might have noted it. (Boit should lead us to
assume, however, that Whites were always successful in using, say, Captain Cook's vocabulary. And although success or failure, which we will find in the reports by traders in this area, might suggest the distribution of a Nootka jargon, we must also remember that there would have been considerable variation in the competence of Whites in using this vocabulary.)

Although Thomason accepts the fact of extensive multilingualism, she uses linguistic diversity as evidence in favour of her argument for a precontact origin of CJ. Thomason (1983:862) says:

Although many of the Indians encountered by white explorers were sophisticated multilinguals, it is unlikely that the Indians could be fluent in all the languages of the tribes with which they dealt. In this setting of intensive trade among linguistically diverse tribes, in an area with a relatively dense population, this question must arise: how did people communicate with speakers of languages they did NOT know?

Her answer to that question is that a lingua franca was needed. But it is as invalid as are her assumptions. The impression she gives of Columbia River trade is that all (or at least a very high number of) Indians were in contact with all the other languages that figured in indigenous trade. She assumes that any speaker 'A' would have to communicate with speakers 'B', 'C', etc., in their different languages. Such a communication network has not been demonstrated for this area, and in fact is contrary to all that we know about aboriginal economies. Trade items flowed here undoubtedly through the hands of a select portion of the population. As Ray and Freeman (1978:6) point out, we can better understand trade as being embedded in inter-tribal political processes; we can add that trade for the most part was most likely controlled by privileged individuals.

With respect to slavery, Thomason offers another explanation for the origin of CJ without attempting to reconcile it to the one just given. She seems to suggest that the Lower Chinooks themselves pidginized their language to emphasize their superiority both in dealing with other Indians and even with their own slaves (1983:862). (This reading is based on the fact that the relevant paragraph is dealing with pidgin origins, not merely their development and spread. In Hymes' theory of CJ the role of slavery is ambiguous. He says that "a reduced form of Chinook was the medium learned by the slaves [of the Chinook] and used between them and their owners" (1980:417), but he does not make clear whether that is where CJ spread or where it originated.) In fact, she accepts the possibility that "a ban on slaves' speaking good Chinook would have contributed to the establishment of a Chinook-based pidgin" (1983:862). Like the preceding, this hardly constitutes an argument because of both a lack of supporting evidence and also of the difficulty of demonstrating that - in this case - 'superiority' led to language pidginization. In fact, ethnographic data point to the opposite of what Thomason suggests. In this area, possession of certain kinds of knowledge was highly prized. If language was considered a personal or kin-related possession, we can imagine that it would have been preserved 'intact.' Moreover, there is evidence to suggest (Zenk 1984:30-31) that domestic slavery required of masters "proper etiquette."

It was noted, for example, that personal slaves were addressed as 'brother' and 'sister.'

In all the varieties of 'slavery' in the world (and there have been many) pidginization is what one would least expect. (For just one example of the motivation for a slave's learning a language correctly see Lovejoy 1981:228, 237-238. The emergence of a pidginized Arabic in the Chadic sahel (called Chad Arabic) might appear to be just the parallel that would support Thomason's hypothesis, but it is multilingualism (plus other factors) and not Muslim-slave relations that led to its origin. The social-cultural setting is thoroughly described by Cordell 1977.  Determining factors in this institution would be such as these: age of slaves, number of slaves both in a household and in a 'village,' previous experience as slaves, kind and amount of interaction with the natives, function of language in human exchange. No comparative ethnology of slavery has been submitted by Hymes and Thomason, and we are far from understanding Chinookan slavery itself.

The work of Leland Donald (1983, 1984) is the best that we have on slavery in the area under consideration. Although, we learn, slavery was an important factor in the life of the Nootka (who by decision of Tribal Council now want to be known as Nuu-chah-nulth-sht), extensive trade in slaves between the West Coast of Vancouver Island and the Columbia is "unlikely" before European traders arrived. Although he has ascertained that the Chinook had, after contact, slaves of Nuu-chah-nulth, Makah, Klallam, Shasta, Klamath, Kalapuya, Quinault, Umpqua, Lequltok, and Nisqually ethnicity, he insists that as yet we do not know the origins of slaves before contact took place. Moreover, he concludes, "the relatively large numbers of slaves and the long distances involved in the 19th century are probably postcontact phenomena" [although there was some precontact trade in slaves] (Donald 1984:155). (I would take as evidence of the increase in slavery among the post-contact trade-enriched Chinooks the statement that "slavery and the treatment of slaves exist in all their hideousness among the tribes near the sea" (Blanchet 1956:187). In other words, although slavery was a cultural feature of the area, even in the 1830s and 1840s, it was most developed on the coast, that is, where trade with Europeans was first intensified.) Hymes' use of slavery for explaining the origin of CJ, subjected to a critique by Donald (1984:152-153), therefore becomes infrm.

Reconstructing the pre-contact past from post-contact data is fraught with problems. There are of course ways of dealing with some of them, but ignoring the possibility of substantial change in cultural patterns jeopardizes the whole enterprise. It may have been true of the Chinook, for example, as it may have been for the Tlingit (according to Ostenstad 1976), that the number of slaves increased during the fur trade and that before then it had been negligible. Since slavery was part of the trading system, one could do well to consider changes in Indian trade after Whites had introduced powerfully attractive objects, more powerful implements of persuasion (guns), and changes in the balance of power. It was after 1800, we should note, that Columbia River (Plateau) peoples
"seem to have visited California" to get horses and for trade (Heizer 1942:1). And it was in 1795 that coast Chinook went 200-300 miles upriver to intimidate the resident Indians with muskets so as to get goods at their own prices (Wike 1951:43, citing a manuscript dated 7 December 1795). (With information such as this we begin to understand the so-called 'superiority' attributed by Thomason to the Chinooks. Being afraid of guns does not have self-evident linguistic consequences. They were "proud and haughty toward other tribes," reported Frs. Blanchet and Demers (Blanchet 1956: 18), because they were the richest and most numerous. Although Fr. Point, a Jesuit missionary in the territory at the eastern watershed of the Rocky Mountains in 1840-1847, remarked of the Chinooks, "tenderness of heart was wanting in their character" (1967:119), there is nothing in what he writes to suggest that CJ had (yet) reached this area. He is quite explicit in describing the way he communicated with the Indians as a 'pioneer missionary.' Several decades of contact with Europeans would have put them in this advantageous position.) For further discussion about the rapid and extensive changes that were initiated by the fur trade see Ray and Freeman (1978:7).

In arguing that attitudinal factors can be crucial in the origin of a pidgin Thomason cites the case of Tay Boy, the pidgin French of Vietnam, making the categorical assertion (1983:862) that it "developed between speakers of pidgin French of Vietnam, making the categorical asser-

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In arguing that attitudinal factors can be crucial in the origin of a pidgin Thomason cites the case of Tay Boy, the pidgin French of Vietnam, making the categorical assertion (1983:862) that it "developed between speakers of French and Vietnamese under circumstances which did not encourage servants to learn their masters' language," and making reference to Reinecke (1971:47). But Reinecke does not express that opinion at all. Basing his observations on something Hugo Schuchardt had written, he simply said: "The pidgin had gotten its start in the French garrisons in what was then Cochín China in the 1860s; where the garrisons [i.e., soldiers] came from, and whether they had brought with them some sort of military pidgin, as a stimulus or model, is not at present known to me." (One does not easily conceive of African recruits being 'masters' having 'servants.') Reinecke was on the right track, and I am able to contribute to the history of that pidgin. The conquest of the peninsula might have been said to have occurred with the taking of Saigon by a Franco-Spanish expedition on 15/16 February 1859, although the French presence was much earlier (Chassaigneux 1932). From that time until the French abandoned Vietnam there were West Africans in the military forces. A corps of soldiers ('laptots') from Gorée, the island off Dakar, had served the French since 1765. In 1827 at least 200 were in Madagascar. But official history gives 3 December 1853 as the beginning of the Senegalese troops. Later decrees increasingly improved the size and organization of this indigenous force, which had an extremely important role in the colonization of French equatorial Africa and the emergence of the Pidgin Sango. It is known that pidginized French was spoken by these troops. (My principal source for the military history are de Boisboisel 1937 and Balesi 1979, but I have collected my own sample of this kind of French from nineteenth century sources.)

If Thomason needed cases of pidginization arising from contact between only two ethnoloinguistic groups, she might have cited that of Russenorsk, which seems to have arisen out of the trade between Norwegian- and Russian-speaking peoples toward the end of the eighteenth or the beginning of the nineteenth centuries. (The most recent contributions to the study of this pidgin, now extinct, is that of Broch and Jahr 1981.) However, Russenorsk in another way is a counter-example, because the creators of this language did not seem to be an unequal relationship with each other (as seen in Broch and Jahr 1983, also 1984). If this were true, then we have another instance of the absence of the feature 'power' among the causes of pidgins. It is because of cases such as these that I would exclude the dimension of power in a hypothesized program for pidginization. Writers on the histories of pidgins have made the mistake of generalizing for all pidgins what they may have learned for certain ones, in particular those that ostensibly arose in the context of the European slave trade in Africa. Recent ethnohistorical studies demonstrate that indigenous peoples were not necessarily 'dominated' by or feel at the mercy of European travelers, traders, and explorers (see Walker 1982:16 for the Nootka, and Prioul 1981 for Ubangians, where this statement is made explicitly; one can infer the same from a great deal of nineteenth century exploration literature, e.g., that of H.M. Stanley and Savorgnan de Brazza for the Congo River basin). A fuller treatment of the history of CJ would of course examine 'power' from a demographic point of view: what, for example, was the proportion of Whites to Indians at any place in any given time?

An argument based on exogamy, invoked by Thomason, can be dismissed just as easily. Just the case of multilingualism in the Northwest Amazon (Sorenson 1971) would be sufficient to make one incredulous about exogamy as a factor in pidginization. As with the Ubangi River basin, it can, in fact, be a factor that prevented the emergence of a lingua franca.

What is more convincing is an analysis of interethnic 'domestic alliances' (customary marriages and what in recent years has been called common-law marriages) that characterized the Chinook area in the first part of the nineteenth century. Records of the first Catholic missionaries of marriages, baptisms, and burials (Blanchet 1972), give enlightening information about the 'Europeans' working and living in this area (a very large number of whom were mixed-blood Canadians, otherwise known as Metis), and the linguistic diversity that characterized their large families. It is argued elsewhere (Samarin Ms 1) that this particular society in the Columbia River region may have been the place where CJ was stabilized as a pidgin.

Ruby and Brown (1976:112) present another sociocultural factor in writing the earliest history of CJ when they say that the Columbia River peoples had formed a jargon "when allied in what has been termed the 'Multnomah Confederacy,' using their jargon to facilitate measures for mutual protection from the incursions of tribesmen from east of the Rocky Mountains and bordering plateau lands." The source of this view is given as Fee (1941), a criticism of which would us beyond the scope of this paper. Ruby and Brown's brief attempt to account for
CJ, it should be noted, is far from critical and regrettably lacks linguistic sophistication.

CONCLUSION

Since this paper is as much about historiography as it with the origin of CJ, I am happy to be able to agree with Thomason (1982), who says that in the study of contact-induced language change, one must take into account the entire communicative setting. And I fully support her assertion that “Contact-induced language change of all sorts is also social history first and language history second,” a point that is “not fully appreciated by most writers on the subject” (1982:482). However, she would appear to qualify this declaration in the following: “… an inference on socio-historical grounds that a pidgin existed in a former contact situation, about which we have no direct contemporaneous evidence, must be supported – if it is to be convincing – by the linguistic evidence” (1983:863). On the mutually reinforcing nature of linguistic and sociocultural data there can be no argument. Where both are available a language historian must use both, without partiality. Neither one is privileged. For some pidgins we may not have enough of one or the other, but in the case of CJ we do.

I therefore differ with Thomason with respect to some aspects of the study of CJ and, presumably, other pidgins. She says, for example, that other attestations from travel literature of Europeans for a trade jargon are “misleading, so far as the search for the origins of CJ itself are concerned” (1983:859). I hold a different view, believing that that literature is the vast midden out of which the linguist and ethnographer can sometimes reconstitute the ethnolinguistic past. This paper illustrates how this kind of literature can, with discretion and discerning incredulity, be used to advance our understanding of those ages which we cannot study first-hand.

I have been equally concerned in this paper with the way that language contact in general and the origin of pidgins and lingua francas in specific must be studied. I have addressed myself to the kinds of data that are needed, to the use of those sources, and to certain concepts that have facilitated previous discussion. ‘Trade,’ for example, is the proper name for kinds of transactions, but misleading and therefore dangerous when it prevents us from looking deeply and perceiving accurately. The trade that Motu-based jargons made possible (Dutton 1983) is vastly different from what went on at the HBC trading posts. Not denying that trade was indeed a ‘speech domain’ (as Joshua A. Fishman might call it) where CJ may have originated and developed, it helps to also consider ‘work’ as an equally powerful domain. Certainly, in explaining the origin of Pidgin Sango the ‘work’ involved in river transportation on the Ubangi River in the last two decades of the nineteenth century cannot be ignored; in fact, it was more important than ‘trade’ (Samarin 1984b). Equally important was land transportation (portage) — the ‘work’ of getting the material of Europeans from the west coast of Africa to Stanley Pool — in the same period for explaining not only the origin of vehicular Kikongo but possibly even Bangial (now called Lingala) (Samarin 1985). It may be time for us to abandon the term ‘trade languages.’ But if one insists on retaining it, then ‘work languages’ must also be employed.

If I have been critical in this paper of some of the proposals made by Hymes and Thomason, this has only been because the issues had to be made explicit. Every facet of the arguments about CJ’s pre-contact or post-contact origin has to be examined independently. With respect to the thrust of their arguments I am in full accord: the history of CJ is necessarily the history of the people of the Pacific Northwest. Therefore we must draw from every possible source to understand the setting in which CJ arose. That source will be both ethnology and economic history.

Writing the history of CJ has been shown to be a complex undertaking. Those unaccustomed to historical argumentation might be led to express, as one of my readers did, despair at ever knowing the real story of CJ’s origin. Such an attitude automatically and categorically eliminates a host of questions that human beings are entitled — indeed, obliged — to ask. Moreover, whether or not we ever come to ‘know for certain’ if CJ was in existence at the time of the arrival of the first Europeans, or emerged only in Indian contact with them, we learn a tremendous amount about language history in the Pacific Northwest by seeking to understand what happened. Finally, “We must remember,” as Davidson and Lytle (1982:273) wisely observe, “… that when historians confront ‘either/or’ questions, they do not have to answer them in just that way. Their overriding obligation is the construction of an hypothesis or interpretation that give full play to all aspects of the subject being investigated …” This is what this paper has sought to do.

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NOTES

1. In the early 1900s the first Catholic missionaries at Banzyville (on the Ubangi River in northern Zaïre) thought they were learning an ethnic language when they were actually learning Pidgin Sango (PS) (Roeykens Ms). Drechsel (1984) finds evidence for the same misunderstanding with respect to Mobilian Jargon.

2. A critical philological and linguistic-ethnological study of wordlists collected by non-professionals has, as far as I know, yet not been done. As a further contribution to this topic, we can cite the following. Pentland (1982:114) notes a bias in word-collecting in North America, such that English- and French-speaking Europeans frequently excluded words from their own languages when in fact they were being used by the Indians. A serious student of Haitian Creole, Fr. Pelte, reports that in his dictionary of a certain dialect of that
language he intentionally left out words that he knew (Freeman 1984:2).

3. The languages mentioned in the opening lines of this quotation strike a strong parallel with those that fascinated Hugo Schuchardt less than two decades later; at this time he was only 21. If the convergence of interests is just as coincidental, no explanation presents itself at this moment.

4. The pages of the works cited here are so few and of such easy access that it would be tedious to cite every reference.

5. Gibbs (1815-1873) did not arrive in Oregon until 1848, and became collector of the port of Astoria in 1854 (Pilling 1893:33). A serious critique of Gibbs as a source of information about CJ would include a discussion of the changes in the loci of economic activity and the shifting of populations as a result of, for example, disease and epidemics. This undertaking would greatly increase the size of the present paper, so it is left to others.

6. Note, for example, the somewhat boastful claim of Alexander Ross (1788-1856) in a book recounting his experiences during the years 1810-1825: "...no white man had as yet [until his expedition presumably] visited the Columbia [River] to any extent; for if we accept Vancouver's discovery of its entrance, in 1792, and the transitory visit of Lewis and Clark in 1805, the writer [Ross] himself and his associates were the first explorers of that distant quarter" (Ross 1931:88). It should be noted in passing that this report is based on two "Chinook interpreters" (p. 13), with no specifications of the language they spoke.

7. This is possibly the list of 65 entries, including numerals, labelled 'A list of words in the Nootkan language, the most in use," found in Jewitt (1797, edited by Heizer, based on the edition of 1820, Jewitt 1931:88), under "A check list of editions of Jewitt's captivity with bibliographical descriptions of the more important," uses this very heading and also attributes the list to the 1815 edition.

8. McKeachie (1949:94) reports that the Narrative was written with the help of "an experienced writer," and that this work was sold (possibly peddled) by Jewitt in New England until the time of his death. Johnson (1978) actually cites an edition of 1815 (followed by a query) with the words "written by Richard Ashop." In all matters, of course, McKeachie has to be read with reservation. His is a popular and romantic account.

9. Her principal sources, from the 1830s and 1840's (personal communication), are late because there had been around fifty years of contact with Europeans in the fur trade.

10. This seems to contradict Thomason's (1983:836) statement that "When we turn to a comparison of CJ utterances produced by different Indians, we find a striking correspondence from speaker to speaker..." If she means this to be true only of "texts and wordlists elicited directly from Indians" (fn 21), she cannot possibly be referring to the earliest period for which there is no linguistic evidence. If there was uniformity, it could easily have been a late development in the language.

11. In other societies intermarriage would have provided the alliances that facilitated polygamy. Working on this hypothesis to argue against the pre-contact existence of a lingua franca on the Ubangi River, I found, by studying a number of different kinds of sources (personal journals, travel accounts, and maps), that a certain Sango young man from near Banga zipper in 1888 was paying the brideprice for the daughter of a N'zakara chief established within a few days' walk away.

12. The statement illustrates the terminological problems that one encounters in reading about pidgins and creoles, a topic discussed in a volunteered and unpublished presentation at the conference on pidgin and creoles, based on the submitted papers, University of Hawaii, December 1975. In this instance, without precedence I think, a jargon is just a language with a "simplified vocabulary." Hess (1979:16) commits the same error in speculating about the reason for the distribution of the Halkomelem words for 'deer' and 'lake' in other languages. It may have been, he says, that because the Halkomelem were middlemen on the lower Fraser River for trade with both interior and coast peoples their languages came to be "quite widely known — perhaps as an incipient pidgin..." A widely-known language is not necessarily a lingua franca, nor is a lingua franca necessarily pidginized; finally, a pidginized form of speech is not necessarily a pidgin. The linguistic facts must be differentiated, as well as the social phenomena with which they are correlated. For a discussion of lingua francas as a technical term see Samarin Ms 2.

13. Gunther does not tell us where he learned about their understanding Nootkan. Boit's log (Boit 1921:31) records only that "Their language was different from any we have yet heard."

14. The relations are characterized as "Muslim-slave," because the slaves were pagans at the time of their capture, and their captors, though called 'Arabs' by European contemporaries, were not in fact ethnic Arabs at all.

15. This is a large-scale project (undertaken with Donald H. Mitchell) that has gathered material from published and unpublished sources with respect to slavery, warfare, trade, and other aspects of intergroup relations on the aboriginal Northwest Coast (Donald 1984:125).

16. This seems to contradict Thomason's (1983:866) statement that there was "no evidence" of the existence of a pidgin (1983:836) in that he states that he "was justified in citing (1983:866) my research report of 1980 as having tentatively hypothesized that it was the expatriate Blacks in the employ of Whites who had pidginized Sango, but that I have written since then, and more in preparation, will demonstrate that indeed the history of Sango, as well as that of Lingala and Kiruba, could not have been otherwise."

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Ms 3. Congo cohorts and copula sharing in Pidgin Sango.


