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FABRIC, PATTERN, SHIFT AND DIFFUSION: WHAT CHANGE IN OREGON PENUTIAN LANGUAGES CAN TELL HISTORICAL LINGUISTS.[1]

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1. ABSTRACT. In this paper I examine certain aspects of relationships which have been proposed to exist between those pairs or more numerous groups of so-called Penutian languages, many of which are spoken in what is now the state of Oregon. Much of what has been previously taken as evidence for a genetic relationship between some of these languages turns out to be diffused material which has passed from one potential branch of Penutian to another and which may mask the existence of actual (or at least potential) cognates which exist between these two branches. Other similarities, which are more typological in nature (involving 'pattern' rather than 'fabric'), are the result of intimate language contact and sometimes language shift, factors which have been brought about by the more complex interlockings of patterns of social relationships within and between small and multilingual speech communities, especially in the period before Euroamerican contact. The fact that this material has been diffused, rather than being inherited from a parent-language, has sometimes gone unnoticed, because what is diffused includes certain kinds of items which in other language families, for instance Indo-European, are taken as primary evidence of an ultimate relationship between pairs or groups of languages.

I suggest that the discipline of historical linguistics as it is generally practiced in more firmly-established language families can be enriched by the findings from attempting to relate groups of Penutian languages to one another, and I point out that a lexicostatistical classification of Indo-European (Dyen, Kruskal and Black 1992), which has used methods similar to those which are available to Penutianists, can serve as a (very partial) working model of what sort of issues the attempts at classifying and affiliating languages which are assumed to be related at a time depth of several millennia may have to contend with.

2. THE AMBIGUOUS ROLE OF TYPOLOGY IN 'PROVING' AFFINITY BETWEEN PENUTIAN LANGUAGES. A considerable amount of work on historical linguistics, including some of the work on comparative Penutian, has been concerned with analyzing shared typological properties or combinations of morphemes in the languages (the sort which I will call 'patterns'), rather than busying itself about the actual morphs which go to make up these patterns, which comprise what I will call the 'fabric' of the language.

The typological patterns of a language can only give very limited indications of the likely affinities of a language or language family. There is only a finite number of typological patterns which are available to a language, and this number is often very small. In cases of Agent-Verb-Patient order, for instance, there are only six possible orders of elements, even in those sentences which have full nominal agents and full nominal patients. Furthermore, the work on typological linguistics which has been carried out over the past four decades shows

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that certain groups of typological patterns have a tendency to cluster. If a language is verb-final, for instance, it will almost certainly have postpositions rather than prepositions.

The following paper will concentrate upon discussing certain phenomena found in languages which have been assigned by the slightly more conservative 'lumpers' to the Penutian hypothesis, and which belong to language groups whose members are or were spoken within the bounds of the present state of Oregon. For the record, my list of groups which need to be taken into account in 'Penutian' includes the following: Utian (Miwokan-Costanoan), Yokutsan (which may be specially related with the former grouping as 'Yok-Utian'), Wintuan, Maiduan, Klamath-Modoc, Molala, Cayuse, Sahaptian, Takelma, Kalapuyan, Coosan, Siuslawan, Alsean, Chinookan and Tsimshianic. Most of the languages in most of these families are no longer spoken. This is pretty much the list of branches from which evidence was used by Hymes (1964) when he reconstructed 182 cognate sets beginning in $*c$- and $*s$-, which he claimed might be evidence for the genetic unity of Penutian. (If this is actually the case, then by extrapolation we might suggest that the number of cognate sets reconstructible for Penutian on the testamentary principles used for Proto-Indo-European, may be around a thousand, if Proto-Penutian is valid.) At least for the present I omit from consideration the Mixe-Zoquean languages, which Hymes included (Hymes 1964:223), and Tonkawa, which he did not, and I regard Stanley Newman's championing of the Penutian relationships of Zuni (Newman 1964) as an intellectual scherzo, as I have reason to believe he did too.

The breadth of the list above should not be taken as a claim on my part that Penutian as a non-trivial genetic unity is provable or proven. To a large extent I share the radical dissent about Penutian expressed in Shipley (1980), itself the work of someone who has contributed much to the study of Penutian languages. Indeed, I have purposely used the phrase 'the Penutian hypothesis' above. My current position on the validity of Penutian is that there are several groups of genetically-related languages among those listed above, that these constitute macro-groups, and that two or more of these macro-groups may constitute a valid genetic unit, with a time depth measurable in terms of several millennia. I would further state that because of the great time depth involved, it will probably never be possible to demonstrate the genetic relationship of these languages—if it exists—in such a way that the evidence for it can be distinguished on qualitative and quantitative grounds from the sets of sporadic, superficial and specious resemblances which can be found whenever any two languages are compared.

In regard to another Pacific Rim linguistic hesion, the Oceanic linguist George Grace (Grace 1990:156) once said that if one tried to reconstruct Proto-Austronesian on the evidence obtainable from three languages, choosing for example a Southern New Caledonian language (such as Tinrin/Grand Couli, which Grace does not mention but upon which he has done extensive work), Yapese of Micronesia, and Atayal of Taiwan, and using only these languages, it might be almost impossible to show that these were related to one another at all. And yet the task of showing some kind of genetic relationship between a set of languages is an essential first step towards reconstructing a proto-language.

I feel that this is the position which Penutianists face in trying to link up groups of languages and proving genetic relationships between them. We have to work with what we have, and we cannot be sure that the languages of which records have come down to us are an especially conservative set, in terms of regularity of sound-correspondences in relation to the as yet hypothetical Proto-Penutian. Indeed we may be dealing within Penutian with a large

proportion of what Grace called 'aberrant' languages, languages which do not provide easy, useful or copious evidence about the proto-language.

To put things in Indo-European terms, we might be hoping to find among the Penutian languages for which we have data a number of Sanskrits and Lithuanians, which would make our work easier, but in dealing with Penutian we may have ended up trying to reconstruct the proto-language by using a number of correlates of Albanian, because that is all that have been left to us.

3. TYPOLOGICAL SIMILARITIES BETWEEN COAST OREGON PENUTIAN LANGUAGES. Typological claims about the genetic unity of pairs or larger groups of languages may seem superficially attractive, but they always need to be assessed against other bodies of evidence, rather than being taken for granted. For instance, if two particular languages, which are spoken in areas which are adjacent to one another, both show a highly-marked order as the normal one, for instance Patient-Agent-Verb, this may be an indication that the languages are genetically related.

Or it may not. It may be the case that such a marked order has been brought about in one of these languages through the influence of speakers of the other language, or it may be that the two languages both originally had a different word order (or else that they used two different word orders) and that their systems converged on this third word-order as a result of independently motivated language-internal changes in each of the two languages. The processes which gave rise to these syntactic changes need not have been shared by the two languages and they need not be traced to a common set of rules and motivations.

A common example of this can be taken from two languages of southern Coastal Oregon with materials on which, and with the descendants of whose speakers, I have had the honor of working. Hanis Coos and Lower Umpqua are both recorded in the linguistic literature as having a basic VOS word order, with an alternative OVS word order. This fact has been known to participants in the Americanist field since 1922, with the publication of Frachtenberg's grammars of these languages (Frachtenberg 1922a, 1922b), but it was brought to wider linguistic attention in Greenberg (1963). Both Hanis and Lower Umpqua belong to families, namely the Coosan and Siuslawan families, which are regarded as forming part of 'Coast Oregon Penutian', two-thirds of it, in fact. This is a hesion about which I will have more to say later. (Alsean is the third part of this grouping, and it appears, from the evidence available to me, to have a generalized VSO word order.)

And yet we cannot take this shared word-order as evidence that, even though this typological feature is rare in the world's languages and consequently highly-marked, Siuslawan and Coosan are especially and non-trivially closely related—that is, we cannot be certain that they are each other's closest documented relatives.

There are (at least) two reasons for this. The first is the nature of the evidence itself. Proof of genetic relatability, in this case proof that the Coosan and Siuslawan languages descend from a single language, even if this was spoken several thousand years ago and has left no records (and if it did exist, it hasn't left any), relies upon demonstrating the continuation of transmission of actual elements of linguistic material. We need to see the transmission of free morphemes and the means to bind them together into clauses and sentences. These elements are passed on—often with changes—from one generation of speakers to the next.

What we have here in the case of two languages sharing VOS word order is not genetic material, though. For present purposes it is a typological characteristic, something which

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allows us to bracket Hanis (and the related but rather different Miluk) and Lower Umpqua and the very similar Siuslaw together on 'structural grounds', and to classify them because of these shared structural similarities with some Cariban, Arawakan, Tupian and other, isolated, languages in the Amazon and on the northern reaches of the South American coast, with the long extinct language Hurrian of the ancient Near East, with Ungarinyin and a couple of others in Australia, and with a few more languages in Papua New Guinea, such as Fasu. (A list in Nettle 1999:140 gives the names of most of these languages, including Hanis as the only North American representative of this typological class, and thus omitting Miluk and Siuslaw-Lower Umpqua.) These twenty or so languages are said on typological grounds alone to form a select band of languages, although they are or were spoken halfway round the world. These languages are supposed to be object-initial—a feature which is rare enough in itself—and some of these, such as Hanis, are furthermore supposed to have the verb preceding the subject in main clauses, which is an even rarer typological pattern.

Apart from such purely typological features of major element-order there is nothing else which unites these languages; we have no reason to believe, for example, that they share a common history of development. The feature which brackets these languages together does not include or involve any actual morphemes. It is just an arrangement of constituents, and these constituents differ in actual form, in the morphemes which they contain, from language to language. The same kind of highly-marked constituent order could happen to become supreme in English one day, for all we know.

The second reason why we cannot be sure that Siuslawan and Coosan are specially related is that the capacity to have recourse (as a means of explaining phenomena) to the whole patterning business, which is still so popular with typologists, is built on something of a misunderstanding about what the true linguistic significance of basic word order really is. And certainly it is a misunderstanding in the case of Hanis. In an important cross-linguistic study Marianne Mithun (Mithun 1992) has shown that Hanis Coos does not have exclusively OVS or VOS word order (though Greenberg's claims in Greenberg 1963 about Hanis and Lower Umpqua being postpositional and placing the genitive and the adjective before the head noun are correct enough). True, one can use OVS word order in a Hanis sentence with some assumption that the outcome would have been grammatically acceptable to Jim Buchanan, or Frank Drew, or Lottie Evanoff, or another of the Hanis speaking elders of years gone by whose dictations form the basis of our knowledge of Hanis syntax and pragmatics.

But there are other word orders available. Indeed there are six basic ones, as I mentioned, and Hanis sentences, such as those which have been recorded in our texts from the people mentioned above, can be seen to use any or all of them. None of the six possibilities is ruled out; any or all are possible in a Hanis sentence. The reason for this is that Hanis word order is not syntactically but pragmatically defined, with new information taking priority sentence-initially over old information, and with this pragmatic concern in a Hanis sentence overriding any fundamental syntactically-motivated considerations.

Typological appearances can therefore be deceptive to the genetic linguist. The evidence which is derived from typology can point us in all manner of useful directions in our search for similarities, and the amount of evidence which is provided by typology alone can make it seem as though there is only one valid conclusion to be drawn in a particular situation when we are conducting a historical analysis, but it can never clinch an argument regarding genetic relationship. For example, findings about typological similarities between neighboring languages are invaluable in building up hypotheses about linguistic areas, and in plotting them on maps, but typological change in a language is certainly not unknown, and in any case

areal linguistics is a secondary concern (and the result of secondary developments and convergences) when its findings are placed against those of genetic linguistics. These considerations are not confined to syntax. In Penutian even the 'characteristic form of stem' involving disyllables with identical first and second vowels (Sapir 1921, Tarpent 2000) is only as characteristic as the rules of the historical phonology of the language in question will permit it to be.

Strong arguments about genetic relationships can only ever be made with reference to 'fabric' that is actual morphemes attested in actual languages. Nothing else will suffice; anything else is only conjecture, and any other kind of evidence can be controverted completely if hard morphemic evidence turns up.

4. CHINOOKAN AND ITS MEMBERSHIP IN PENUTIAN: THE TRAP OF TYPOLOGY. If excessive reliance upon the findings of linguistic typology can attempt to compel us to lump things erroneously, it can also cause us to split them erroneously from one another. The problem of the assignment of Chinookan to Penutian is an example of this. The close genetic interaffinities of the Shoalwater and Clatsop varieties of Chinook on the coast, of Kathlamet (and, we assume, the unrecorded Multnomah) further inland, of Clackamas beyond that and of Wasco-Wishram (which is often classified with Clackamas and called Kiksht) are undisputed by all. Chinook, especially the Coastal varieties which were best known to earlier investigators such as Franz Boas, is a head-marking and polysynthetic language; the Penutian languages taken as a potential group tend to be rather aggressively dependent-marking and are somewhat more inclined to subsume fewer morphemes within the space of a word. The Chinookan group therefore stands out anomalously from the rest *prima facie* on typological and patterning grounds rather than on the grounds of not sharing fabric in the form of morphemes which are ultimate cognates with morphemes in other Penutian languages. In fact Edward Sapir and, more recently Dell Hymes, have both made out a strong case for the membership of Chinookan in Penutian and have taken its membership within Penutian as a given fact in their comparative Penutian work (see for instance Sapir 1921, Hymes 1964).

What is more, there is quite a range of typological variety within certain structures which are found in Chinookan, as Michael Silverstein's study of Chinookan tense-aspect systems (Silverstein 1974) shows. In the west, Chinookan TMA systems are structurally simple, with three or so aspects being overtly marked, and they pattern like those of the neighboring Coast Salishan languages. In the east, as one goes further and further downriver, the TMA systems look more like those found in Sahaptian languages in terms of the kinds of distinctions which they make, although not as much in the actual morphemes which they use in order to express these distinctions.

None of this should surprise us if we remember that Chinookan tribes long held sway along the westernmost reaches of the Columbia, and that they enslaved and intermarried with people from a wide variety of tribes (Silverstein 1990); indeed Charles Cultee, Boas' chief consultant (of two surviving speakers then known) for Shoalwater Chinook and the only surviving speaker of Kathlamet Chinook at that time, was one-quarter Kwalhioqua (Washington Athabaskan) by descent (Boas 1901:5). We cannot measure the extent of the linguistic impact of Coast Salish-speaking women or Sahaptin-speaking women upon the Chinookan languages which these transmitted as adult L2 speakers to their children who learned them as their first or coequal language, but we may imagine that it was considerable. The converse, namely the impact of Chinookan upon surrounding languages, is also true,

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although here the evidence points more to diffusion through language contact rather than through processes of language shift.

We also find lexical loans from the various forms of Chinook being borrowed into other neighboring languages, although these have sometimes been taken as evidence for relationship between Chinookan and the borrowing language. For instance most of the lexical links which Frachtenberg (1917) saw as obtaining between Kalapuyan and Chinookan are actually loans from Chinook into Kalapuyan languages, and pretty much all of them happen to occur in Chinook Jargon. Berman (1988:3) has pointed out that Chinook-style gender systems, involving a distinction between masculine and feminine, had begun to be replicated in Tualatin (or Atfalati) Kalapuyan, the Northern Kalapuyan dialect which is geographically closest to Chinookan, using Kalapuyan morphemes to discharge new roles on the basis of models supplied by the neighboring Clackamas, but they are found nowhere else in Kalapuyan. Tualatin has also probably given Chinookan its word for the culturally important tuber 'wapato' (whence it entered English), and Tualatin is the Kalapuyan variety which has donated a couple of words to CJ. Even if we exclude Chinook Jargon from the picture as a potential source of influence—for it had no gender system, being a pidgin, and in any case its spread was largely a nineteenth-century and post-European contact phenomenon—it is clear that Chinookan languages played an important role in influencing other languages throughout much of the lower Columbia area and surrounding lands.

The tide of history has turned, and now few varieties of Chinookan have any speakers whatsoever. Language shift has taken place in the past century or so, from Chinookan languages to Chinook Jargon and then English in the west, and to Sahaptin (with on occasion an ancillary use of Chinook Jargon) and then to English in the east. These shifts of language have not been mutually exclusive. On the Warm Springs Reservation in earlier decades of the twentieth century there were people who knew all four languages: Kiksht, Chinook Jargon, Sahaptin and English. Now only Wasco-Wishram among Chinookan varieties still has any speakers, and even then Sahaptin varieties are more widely spoken at Warm Springs than Kiksht is (though neither Kiksht nor Sahaptin is in a particularly secure linguistic position). In fact Morris Swadesh's consultant for Warm Springs Sahaptin, Eva Winishut (Swadesh 1954:132), was actually a Wasco who was married to a Sahaptin, and who used Sahaptin (and English) as her customary language (personal communication from Kathrine French, 1994).

The fact that Wasco-Wishram borrowed several case-suffixes, meaning 'for', 'at', 'with' and 'by means of', respectively, from Sahaptin (Sapir traced them to the Yakima dialect, but they may have been from Warm Springs Sahaptin), and that it used them productively, as Sapir himself pointed out (Sapir 1911), would have suggested to us, had we not known by other means, that Sahaptin had enjoyed a position of importance at some point, since this kind of borrowing is something which is surprisingly rare in the world's languages. We also have evidence (Rigsby 1965) that Sahaptin was a donor language for a number of loans into Chinook (and often then into Chinook Jargon). Sahaptin and Chinookan even share some free grammatical morphemes, such as *kwáanisim* 'always' (Rigsby 1965), which has also found its way into Chinook Jargon.

What should concern someone who is interested in the historical development of the Penutian languages is not so much that a head-marking language like Wishram has postpositions, but that these postpositions have been taken over, fabric as well as pattern, from another language—and that continued social pressure from this language, namely the Warm Springs variety of Sahaptin, has assisted in the subsequent development of other

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postpositions from purely Chinookan materials, whereas we do not find them in more coastal varieties of Chinookan (Sapir 1911:650-654).

5. FINDING MODELS FOR TYPOLOGY AND DIFFUSION IN DIACHRONIC INVESTIGATIONS. If we want to make the best use of the findings of typological syntax, we need to combine it with findings from other parts of the language, and especially with specific morphemes and specific patterns of morphemes. I have spoken before (in my presentation at the conference held at Davis in 1997, which is incorporated into the present paper) about the problems inherent in trying to 'prove' the validity of Penutian as a genetic unity. Two of the most significant problems which we face are its likely time depth, which is unknown but certainly vast, in terms of at least several millennia, and the fact that we have essentially no records of Penutian languages before the beginning of the nineteenth century.

What we are trying to do is to use a form of the Comparative Method—a method which calls for use of linguistic fabric rather than linguistic patterns—in an attempt to navigate an area of historical time-depth which is certainly some millennia beyond that sphere where the Comparative Method still gives plentiful and reliable results. We are further trying to ensure that as a result of using this method, there will be plentiful enough results for us to be certain that whatever results we have at the end of applying the Comparative Method are not simply the outcome of chance superficial resemblances which could be replicated in any pair or set of languages picked at random.

Penutianists are not unique in being in this quandary. This is the same problem which, for instance, has faced people who are trying to reconstruct Afroasiatic, a language family with a time depth of maybe ten millennia. We have records of two—Semitic and Egyptian— of the six families which are subsumed under Afroasiatic which stretch back for five millennia. This is a distance which is certainly long enough in time to show that even then Egyptian and Semitic were very different, even though they were apparently related, and even though there was an ever increasing degree of mutual contact and influence, mostly upon Egyptian from Semitic. We can't 'prove' the unity of Afroasiatic to everyone's satisfaction according to the dictates of the Comparative Method, although all the fabric-based evidence (much of which is presented in Ehret 1995) suggests that it is a valid macrofamily.

A more familiar language family of slightly smaller time depth shows us what can be satisfactorily reconstructed. We *can* prove with some confidence the existence of Proto-Indo-European, whose ancestor language was spoken maybe 6000 years ago, because we have records of parts of it which stretch back, at the furthest, maybe four millennia (some Hieroglyphic Luwian inscriptions), with plenty of material that is two and a half millennia old—for example material from early Latin, earlier records of Umbrian and South Picene, Attic and Ionic Greek and of course Sanskrit. We can also prove the existence of Proto-Austronesian, which is almost as old as Indo-European, even though we have absolutely no records more than 1600 years old, while most of our relevant data were collected within the last five hundred, or even two hundred, years. And there again most of our data for Penutian languages have been gathered since 1800: the collected pre-nineteenth century Penutian linguistic data would barely fill a school exercise book.

Consequently we welcome any concrete linguistic evidence which will help us to understand or clarify the historical picture of the Penutian languages more easily, even when that evidence points to a period of intensive linguistic contact between two groups, rather than to their non-trivial linguistic interrelationship.

For example, Alsean, Siuslawan and the two Coosan languages, which as previously stated are often classified as Coast Oregon Penutian (henceforth COP), show a large number of similarities, in the shapes of most of their pronominal affixes, in phonological systems (which are largely boiled-down versions of the complex Northwest Coast segmental phonological system), and to some extent in word order (at least we can say that most of them have a predilection for verb-initial sentences). There are some vague similarities of fabric or pattern on occasion between every one of these three families and a non-COP member—for instance some word shapes, though only a few, seem to be shared between Coosan and either Takelma or Kalapuya (though hardly ever both of the latter), while Siuslaw and Molala seem to be more strongly dependent-marking than the other Oregon Penutian languages. Additionally Alsea and Wintu from among the Californian Wintuan languages share a number of similar lexical shapes with similar meanings, most of which are not found elsewhere in COP or among other Wintuan languages (though some of them are: see Golla 1997). For the moment I will rule Wintuan out from the group of Coast Oregon Penutian languages.

But what seems, at least in the eyes of many scholars, to bind these three sets of languages together as belonging to COP is the possession of a large number of words with similar phonological shapes, as was shown in Buckley (1987). These elements of similar appearance are not necessarily cognate sets. Some may be cognates, others may be borrowings from one Coast Oregon Penutian language to another, yet others don't even look very similar (which does not in and of itself mean that they cannot be cognates). Lexical evidence, which is the sort which many subgroupers start with and from which they soon move on with alacrity, is almost the weakest kind of evidence to use when attempting to prove a genetic relationship between languages (though with many languages, as for instance the putatively Penutian Cayuse, we have no other evidence to turn to), but it is still a stronger step towards proof than typological 'evidence' is.

If the Coast Oregon Penutian languages are related, the degree of relationship—and, to take the quickest metric as a preliminary guide, the level of lexical cognacy—is not high. In a classic yet still unpublished paper Kinkade (1978) estimated the number of items shared between Alsea and Hanis Coos on a certain version of the Swadesh 200-word list as numbering 40, and those shared between Siuslaw and Hanis as 32, while some 20 items seemed to be shared between Alsea and Siuslaw. We should remember that the Siuslaw Swadesh list available to us has 41 gaps, and the 200-item one for Hanis has thirty, while a little under 180 Swadesh list forms were available for Alsea, so that these proportions, after scaling down the number of comparanda, are more impressive than they look. 40 resemblances out of a maximum of 172 comparable forms is approximately 24%.

We might wish to compare these figures against those which were provided in a lexicostatistically-based attempt to subclassify Indo-European languages on the basis of reflexes and patterns of cognation in their modern forms. Such an approach has very rarely been employed in the task of classifying Indo-European languages, where material from older language is much more prized. This was conducted 'blind', that is, without reference to earlier stages of Indo-European such as Sanskrit, Latin or Classical Greek, but using modern languages only. The study was carried out by Isidore Dyen, Joseph Kruskal and Paul Black (Dyen, Kruskal and Black 1992). These investigators also used a 200-item Swadesh list, but one which differed from the one used for Penutian languages in about 5% of its entries. Here the percentages for modern Indo-European languages were calculated using linguistic materials which had almost all been collected within the previous century, and many of the

lists used were contemporary ones from native speakers, gathered by linguists who were often themselves native speakers of the languages for which they provided samples.

In short, the general nature of the material which Dyen and his colleagues used was pretty much comparable (at least in its explicit lack of assumptions about possible first orders of subgrouping) with that for the Oregon Penutian languages, although it should be remembered that this kind of examination is limited insofar as it looks at only one kind of linguistic evidence (that is to say, shared lexicon) and excludes shared inflectional morphological features almost completely. But Dyen and his colleagues were working with lexicon in the same way as have other scholars who have commenced preliminary classifications of languages whose structures, though maybe well-attested, were as yet imperfectly explored and understood. In taking this approach Dyen and his colleagues chose to do what Penutianists have to do.

The figure of 24% which appears at first blush to separate Alsea and Hanis is the kind of time-depth of cognation within the confines of the lexicostatistical list which apparently also separates modern Catalan from modern German. (This latter figure was presumably arrived at by the calculators after they had subtracted the few percent of diffusional cumulation which was caused by the diffusion of Germanic loans into Western Romance and of Latin loans into German.) If the Alsea-Siuslaw figures are accurate, they exhibit a time-depth of about 11%, which is parallel to that obtaining between Irish Gaelic and Hindi (Dyen, Kruskal and Black 1992:103).

But a large proportion of the close inspectional similarities among the 240 forms which Buckley documents as occurring between at least two of these three languages look more like loanwords, and what directional evidence there is from the phonological forms of these items suggests that they are loans from several directions. In a few cases they appear to be loans from Siuslawan to Alsea, in quite a few other cases they seem to be loans from Alsea to Hanis and especially from Alsea to Siuslawan, and sometimes they look like loans from Hanis to Lower Umpqua and then to its sister-language Siuslaw. We cannot be sure how long these patterns of contact had been going on, but some of the latter, the Coosan to Siuslawan loans, may postdate European-Native contact (after 1826 in this area) or prolonged Native-European contact (post-1851) in the area. An example is the word for the post-contact food item 'bread', which is *qalax* in Hanis and Miluk Coos and *qalxa* in Lower Umpqua and Siuslaw. This term is unanalyzable in the Siuslawan languages (and in any case the presence of an /l/ in a Lower Umpqua word is a sign of a loan into Lower Umpqua, since /n/ would have been expected), but in Hanis the sense 'bread' is secondary, the primary meaning being 'white clay', which is the impression that the appearance of bread made upon the Native Americans when they first encountered it (Donald Whereat, personal communication).

On the basis of this linguistic evidence, it generally seems as though the Alseas, who were the first of these groups to be contacted by Whites (in 1789) were usually the transmitters of innovations and that the Siuslawan-speaking tribes were especially prone to be the recipients. The Alseas seem to have had a boarding-house reach which permitted them to pass some ideas and innovations onto speakers of Hanis, and maybe also Miluk, without them being recorded for Siuslaw or Lower Umpqua (for example *kwee* as the name of a kind of canoe is shared only between Alsea and Hanis: Buckley 1987:60). But there again the absence of these forms in the Siuslawan material may just reflect gaps in our material for the latter group, since Siuslawan is the most thinly-attested of the three groups.

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On the other hand, some innovations, such as the largely shared counting system, which involves a number of morphemes which are found in Alsea, Siuslawan and Miluk (and to a slight extent also in Kalapuyan), apparently never reached Hanis-speakers, who used a largely unrelated system. (Nevertheless, the trope of using the expression 'one stick' for 'one hundred', which is found also in Hupa in northern California, an Athabaskan language which does not border upon Coast Oregon Penutian territory, is recorded for both Oregon Athabaskan languages such as Tututni and for the potentially Penutian Hanis and Miluk, though Siuslawan and Alsea both use an expression meaning 'ten tens'; Victor Golla, personal communication). Hanis numerals, which are quinary in construction, comprise a number of forms which are not otherwise found in Coast Oregon Penutian languages.

Most of the elements which might cause people to classify the Coast Oregon Penutian languages together as a genetic grouping after a cursory inspection of materials on these languages are unlikely to be the ones which are the best evidence for actually showing these groups of languages as being (probably) interrelated. These 'rogue elements' which attest to diffusion rather than genetic inheritance include some of the lower numerals, the sharing of certain kin terms (which have apparently diffused between Siuslaw and Alsea, being borrowed by the former as evidence by their possession in Siuslawan of an element which is unanalyzable and otherwise unattested in Siuslawan but which is clearly a prefix of some sort in Alsea), the names for a number of cultural items, flora and fauna and so on, and maybe even the presence of that highly-marked VOS word order to which I referred at the beginning of this paper.

Several kin terms are shared by two or more of the COP languages, but especially by Alsea and Lower Umpqua (Frachtenberg 1922b:461-462). In the latter language they appear with what is spelt in the sources as a prefixed *m(i)-*, although Lower Umpqua uses suffixes almost exclusively. This prefix is absent in the Alsea forms of these words, although Alsea, like the Siuslawan languages, uses personal pronominal suffixes rather than prefixes, and a prefix of the shape *m(i)-* would be a verbal prefix marking intransitive and continuative aspect! (See Frachtenberg 1920:286). For example Lower Umpqua has *mîtà* 'father', while Alsea has *ta'a*. This may seem hypocoristic, but the two languages also appear to share forms meaning 'mother', 'elder brother', 'younger brother', 'elder sister', 'younger sister', 'maternal uncle', 'parent-in-law', 'sibling-in-law' and 'son-in-law', while Lower Umpqua and Hanis share similar forms of a term meaning 'two people who are related through someone who is deceased', a commonly-encoded kind of affinal relationship which was accorded a special kin term in other societies in the area (for instance among the Molalas and among the speakers of Grand Ronde Chinook Jargon).

Lower numerals, too, are often shared items in coastal Oregon, as Pierce (1966:385) pointed out in his attempt at ascertaining the nature of the relationship between the Coosan languages, Alsea, Siuslawan and Takelma. These forms look more like loanwords, at least in the case of the forms as they are found in some languages, than a shared set of inherited forms which betoken original genetic unity. To summarize, Hanis, Miluk, Siuslawan, Alsea and Kalapuya (the last is not mentioned by Pierce) and possibly also Takelma share a form with a stem /psin ~šin/ for 'three (the Takelma form is *xíbini*). This is the most widely diffused numeral shape in the area. Siuslawan and Miluk have *xáat'su*, and *at'sú* respectively for 'two', while in Siuslawan 'four' is *xáat'suun*. (Takelma also uses a form of 'two' to express 'four', but in this case the form is simply reduplicated: *gamgám* 'four'). Hanis *kat'ómis* 'five' resembles Siuslawan *qátimx* 'six'. The forms are similar but the differences do not appear to be the result of sound changes, since such sound-changes are not otherwise

paralleled in these languages. The first ten numerals in Hanis and Miluk are, as Pierce shows, almost entirely different in shape and in morphemic structure, even though the genetic relationship of these two languages is beyond doubt.

The results of language contact have spread not only to fabric but also to pattern. The numerals 'seven' and 'eight' involve the forms for 'three' and 'four' plus additional morphemes in Miluk and the forms for 'two' and 'three' plus additional morphemes in Siuslawan and in Alsea (although in Alsea the morphemes used, apart from 'three', are dissimilar to those used in Siuslawan.) However, in Gibbs (c. 1863), a vocabulary of Lower Umpqua, the forms for 'seven' and 'eight' are <shi-na-háu-it> for 'seven' and <kha-art-tso-haút> for 'eight'. The form for 'eight' here is ambiguous, as the first part could refer to 'four' or 'two' (the latter being especially likely in the case of a subtractive count), but the first part of 'seven' is clearly a form of 'three' (Siuslawan *šiinx*), which suggests that Siuslawan originally followed the same pattern for these numerals as Alsea and Miluk did, but that it later changed this, possibly turning an original Alsea-style count into a subtractive count. In the sets of numerals of the first decade in these languages we see ample examples of both diffusion of fabric, and of metatypy of numerative patterns.

Some of the evidence which can best be used to link the Coast Oregon Penutian languages as a genetic grouping is considerably more basic, however, and more compelling. In his discussion of Alsea pronouns Kinkade (1978:8) presents several tables of forms which have similar shapes in Alsea and in neighboring languages, 'Penutian' or 'non-Penutian'. These include the personal affixes, which are used with both nouns and verbs (to indicate possession on the former and to mark agenthood on the latter). They show remarkable similarities across the various COP languages (and, as Kinkade points out, also beyond there, in Salishan). I present his table below:

	ALSEA	SIUSLAWAN	HANIS
1S	*-an, -in*	*-n*	*n-*
2S	*-ax*	*-nx*	*e'-*
1incl. du	*-ast*	*-ns*	*ís-*
1excl. du	*-axan*	*-axun*	*xwín-*
2du	*-apst*	*-c*	*íš-*
3du	*-aux*	*-awx*	*üx-*
1incl.P	*-al*	*-nl*	*lín-*
1excl.P	*-al*	*-nxan*	*lín-*
2P	*-ap*	*- i*	*šin-*
3P	*-alx, -ilx*	*-nx*	*l̃-*

In all three languages the third person singular form is expressed by zero. Note that the Hanis forms are prefixes, while in the other languages they are suffixes. This typological fact in and of itself is no indication that these forms do not show the ultimate genetic relationship of the three groups of languages. These suffixes cannot usually be further decomposed into smaller elements. Any similarities between the sets of suffixes are either the result of massive diffusion of morphemes from one language family to another, or else they reflect the forms of inherited genetic morphemic material.

Another subset of elements of what may be the morpholexical glue which holds COP together, at least as part of a greater group, is much less dramatic in appearance than the batches of lexical items of cultural, ecological or other significance. I refer here to the

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presence of a small number of verbs expressing basic ideas, such as verbs of motion. For example Buckley (1987:61) lists the following:

Alsea *qaa-* 'enter', Lower Umpqua *qaa-* 'enter, be inside'
Alsea, Lower Umpqua *aq-* 'go'
Alsea *ay-* 'to go, leave', Hanis Coos *aya* 'lost, gone'

There are several other common verbs which seem to be shared between two or more languages within Coast Oregon Penutian, for instance:

Alsea *tsinsu-*, Hanis *tsim-(siml)* 'sleep' (the Hanis form is a plural one; Buckley 1987:64).
Alsea *t'ims-* 'agree, close', Lower Umpqua *t'əml-* 'to close' (Buckley 1987:60).

6. THE PENUTIAN HYPOTHESIS AND THE PARALLEL OF INDO-EUROPEAN AS A 'WORKING MODEL' OF DEEPER GENETIC RELATIONSHIPS. We may profitably compare the findings in the section above with the kinds of data which are generally taken as being diagnostic of relationships between Indo-European languages, and which are usually among the first to be used as a means of showing that a given language is Indo-European. The number of actual forms reconstructible for Proto-Indo-European as traditionally understood runs into the low thousands, and they include morphological elements in addition to plenty of lexical items.

Typology can be illuminating but in Indo-European, as in Penutian, it is difficult to establish firm Indo-European types on typological grounds alone, especially if one examines the evidence of modern languages. Hindi has relative clauses preceding their heads, is SOV, uses post-verbal negation and postpositions, all of these being the opposite of what one finds in English. Genitives can precede their possessed noun, as in Hindi, or follow them, as in French; adjectives can precede their nouns as in English or follow them as in Spanish. Articles can precede the noun as in English, follow them as in Swedish, or be absent, as in Russian. Cases can be marked by suffixes, as in Russian, or largely by morphophonemic changes to initial stem-consonants, as in Celtic languages. Most nouns in Scottish Gaelic have the possessive person-marker following them, whereas it would precede them in English, while in Spanish both models of possession-marking are permissible. In short, there are very few typological properties which one cannot find in at least one modern Indo-European language.

And for those in search of a 'quick and dirty' method of linguistic classification, the forms of the first decade of numerals and of the basic consanguineal kin terms (such as *father, mother, brother*) have generally been taken as being diagnostic enough, in and of themselves, to be regarded as a sure sign that a language which possesses most or all of such forms is Indo-European. (However, Edward Sapir is supposed to have once said that he would be able to identify a language as being Indo-European even were he to have before him nothing other than three forms of the verb 'to be').

A demonstration of what the cognate residue between two related but distant languages might amount to after the passage of several millennia is provided by a body of evidence from a source from which many Americanists usually rightly turn with horror or derision, namely Greenberg (1987). As part of his counterblast against the claims of the failsafe efficacy of the Comparative Method, Greenberg (1987:20) lists the bulk of the cognate (rather than borrowed) lexical elements shared between French and English. Indeed, it is hard to think of any which he has omitted, although some of his inclusions, such as the pair

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normally spelt *sœur—sister,* are not true cognates in the fullest sense of the word, since their form in English goes back to Old Norse rather than to Old English and is therefore a loan.

Greenberg provides some 84 sets of 'French—English' cognates, and in doing so he seeks to demonstrate that diachronists cannot rely overmuch upon the evidence of sequences of regular sound correspondences occurring between a pair of languages which do not seem superficially to be closely related, as a means to alerting them to the possible genetic relationship of these languages. In one sense he is making the same claim about the effects of selecting English and French in an attempt to reconstruct a form of Indo-European as George Grace above made about evidence for reconstructing Proto-Austronesian from individual modern Austronesian languages, and he is suggesting what we might infer from it superficially about possible relationships between some languages. (There are rather more than 84 Germanic stems to be found in French as loans, incidentally, while the number of forms taken from French into English runs into the thousands; the corpus of truly cognate material existing between French and English is the smallest of the tranches of morphemic material which they share.)

But there are other ways of viewing Greenberg's little dataset to diachronic advantage. If, despite our knowledge of the sound-changes and other changes which have occurred within Romance and Germanic, and between Romance (or rather, its ancestor Latin) and Germanic as the result of their splitting off from Indo-European several millennia ago, the concrete fabric evidence for the relationship between modern English and modern French comes down to little more than these 84 cognate sets, then perhaps we should not be surprised if we find that the concrete evidence to link any two groups within the Penutian hypothesis is rather sparse. After all, these attempts to link Penutian languages involve languages which are far less deeply documented and far less well-known than French and English, or French and English's respective ancestors, are. Maybe the scanty French-English dataset, in which over half the forms given also appear as cognates on the Swadesh 100- and 200-word lists (amounting to 22%, give or take 1%), is a simulacrum of what the cognate sets of two languages look like after several millennia, and it can be used in that way as an object lesson. Of course, the vast majority of words which because of their similarity would lead the unwary observer to think that French and English are related are actually similar because they are English loans from French which were taken over within the last millennium. Here, as in other cases, the effects of linguistic diffusion bedizen the unsuspecting.

The fact that we have ample ancient attestation of several Indo-European languages is what makes so much of the difference between the task of reconstructing Indo-European and reconstructing (and indeed proving the validity of) Penutian. If we were to assume that Penutian as it is generally conceived of had existed as a genetic unit, and if we further assumed that it split into various branches at about the same time as the separating out of the major groupings of Indo-European (whatever they really were), then, other things being equal, we might imagine that the modern 'Penutian' languages would appear to be as closely or as distantly related to one another from the point of brute inspection as English and French, or Albanian and Welsh.

Within Indo-European we have the kinds of attestations which will allow us to demonstrate deeper and less easily observable genetic relationships, but we don't have this for Penutian. If Penutian is a genetic unit, then the primary branchings which connect the various families and macro-families, and the original Penutian stemma itself, are lost from our sight because of our inability to reconstruct them, whereas in Indo-European we can see these primary branches much more clearly because of the vastly richer and chronologically

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deeper historical record. As Hamp (1979) put it, with Indo-European we are the position to show that Greek *máti* and Albanian *sy*, both meaning 'eye' , are related, whereas with the task of interrelating most American languages we are still at the *pater-fader* stage.

Even though the concentration in the examination both of Penutian above and Indo-European is on findings from lexical data, which can be notoriously liable to change and replacement, we can at least claim that Indo-European as reconstructed, and (thanks to Dyen, Kruskal and Black 1992) as its divergences among the modern languages have been plotted, can be used as a kind of 'working model'. Against this we can optimistically project what degrees of difference the processes of filiation and divergence within Penutian would have resulted in, if Penutian were to be of a comparable time-depth to Indo-European.

One of the major differences between our records of Penutian languages and those of Indo-European languages is that in the case of Penutian, most of the material has been gathered within the past 150-200 years, so that what we have are broadly contemporaneous records of a large number of languages. This is not the case with Indo-European, where almost three and a half millennia separate the first attestations of Anatolian and Albanian, and where even conservative branches such as Baltic and Slavic have been attested only for about 700 years in the first case and less than 1200 years in the second. Over a millennium, and often more, separates the data from Latin and those from Old Church Slavonic which are generally used in Indo-European comparative work. Nobody who is engaged in diachronic linguistics ever seems to compare Old Church Slavonic with, say, Old French, which is its approximate contemporary.

The degree of divergence between two modern Indo-European languages goes as far down as a shared cognation rate of 6.8% between (Iranian) Pashto and Albanian (Dyen, Kruskal and Black 1992:117), and we see on the same page that even between Iranian languages, the well-established branch which seems most deeply internally divergent, the rate of cognation dips to 17% between Pashto and Ossetic. (Compare the 24% which separates Catalan and German, mentioned above.)

Of course, none of this means that we must absolutely not be barking up the wrong family-tree in attempting to solder together the disparate strands of the Penutian hypothesis if there should turn out to be, *sub specie aeternitatis*, no good reason for connecting them genetically.

7. INHERITANCE AND DIFFUSION IN SOME FORMS OF 'PLATEAU PENUTIAN'. Coast Oregon Penutian is not the only Penutian hesion which exhibits features which could be of interest to more general historical linguists. We might also mention some considerations which have to do with the interrelationships of Molala, Cayuse, Klamath and Sahaptian, this last comprising the Sahaptin dialects plus Nez Perce, which, with or without Maiduan in California, have been held to comprise a group called Plateau Penutian. My primary interest here is in the relationship between Molala and Cayuse and with their diffusional connections with other groups, and the question of what among the similarities between Cayuse and Molala is inherited material and what is diffused. This is a topic which has been discussed before, most notably and most copiously in Rigsby (1969).

Berman (1996:16-20) has listed some 25 forms of core vocabulary (including a number of frequently-used verbs) which are shared by Molala and at least one other language in his formulation of Plateau Penutian (a formulation which excludes Maiduan). He has also (Berman 1996:20-22) drawn attention to the presence of a number of lexical items, which he regards as loans into Molala, which occur between Molala (which was spoken in two

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discontinuous bands, Northern and Southern Molala) and other languages, especially Plateau Penutian languages such as Klamath to the south and Sahaptian varieties to the northeast. These number 31 items, some of which may actually be old cognates and a few of which may be examples of onomatopoeia.

Some of the similarities between forms in various potential members of Plateau Penutian, especially forms found in both Molala and other languages, can be accounted for by appeal to linguistic diffusion. Sahaptian-speakers, both Umatilla Sahaptins and speakers of Lower Nez Perce, played quite an important role in the later linguistic history of the Cayuses, but there have been Sahaptian-Molala connections too, especially among people who were classified as Southern Molalas (although our linguistic, and to a slightly lesser extent, our ethnographic information on Molala is derived exclusively from Northern Molalas). We note for instance that the parents of Henry Zenk's Chinook Jargon (CJ) consultant Esther Jones LaBonte were a Sahaptin and CJ-speaking mother and a Molala father, Douglas Jones, who also spoke CJ and Klamath in addition to Molala (Zenk and Rigsby 1998:444). We may assume on geographical grounds that Sahaptian languages mostly influenced Northern Molala and that Klamath mostly influenced Southern Molala, but we have no linguistic data from the Southern Molalas. The last speaker of Molala, Fred Yelkes, died in 1958 (Berman 1996:1).

Diffusion has also played a role in the history of Cayuse, as some of Berman's examples show (Berman 1996:24-26). The original Cayuse language was being replaced by a form of Lower Nez Perce among the Cayuses even in the early 1840s (as Hale 1846:214 pointed out), and the last fluent speakers who were capable of constructing sentences are supposed to have died in the 1930s. We have very little material from them, as these last speakers were unwilling to speak their language to investigators. But what little we do have from Nez Perce and Sahaptin-speakers, often themselves of part-Cayuse extraction, who had heard Cayuse spoken and who remembered a few words and phrases, suggests that its segmental phonology was fairly typical for the Plateau linguistic area, in its possession of velar and uvular fricatives and of glottalization. Both of these are features which would not otherwise have been retrievable from the few available naively-recorded White records of the language. (The material on Cayuse that is in public domain is mostly available in Rigsby 1969.)

As an aside, we should note the fact that, whether it be through borrowing, direct or indirect inheritance, or through a combination of these, Molala and Cayuse share 31 out of the 108 items for which I was able to find glosses for both of them on the Swadesh list. This is more than Alsea and Hanis do, and more than Molala and Klamath appear to do either. Pro-rata that is a little more than the proportion which, according to Dyen, Kruskal and Black (1992), the modern Indic languages Marathi and Romani share, and these latter are two languages which nobody would subgroup separately. But I checked the same Swadesh lists for Molala and Santiam Kalapuya as others had used for Alsea, Hanis and the like, and I was unable to find more than 6-7 percent of even broadly inspectional resemblances on the two lists. The only shared salient feature which seems to mark these two languages, Molala and Santiam (as a representative of Kalapuyan), out together against others in the region is their possession of the sound *f*, which, at least in Kalapuya, is a secondary development from *xw*, and its being shared with Molala looks like the result of areal diffusion—from which direction I do not know, although Cayuse also has *f*, while Cayuse and Molala, alone of languages on the Plateau, also have the velar nasal as a probable phoneme (Kinkade et al. 1998:61).

For their part Molala and Cayuse may not constitute a valid subgrouping but they seem to share more resemblant forms (not necessarily cognates!) with one another than with either

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does with Klamath or with the Sahaptian languages or with anything else. However, when I examined the two pages of the Scots Hudson Bay trader Samuel Black's 20-odd page Walla Walla-Nez Perce-Cayuse vocabulary of 1829, the earliest and fullest record of Cayuse, which have been made available publicly (these pages are presented at Goddard 1996:41, Kinkade, Elmendorf, Aoki and Rigsby 1998:62), I found that although I was able to find equivalents or parallels in my (far from complete) Molala lexical materials for some two-thirds of the Cayuse glosses, only about six of the c. 50 Cayuse forms for which I had Molala equivalents looked even like inspectional resemblances between Molala and Cayuse. None of these were words which had not already been noticed in Rigsby (1966). Regretfully, I was unable to add any new Cayuse-Molala parallels to what Rigsby had found.

The kinds of linguistic resemblances which obtain between Molala and Cayuse are similar in a number of respects to those found between any of the three branches of Coast Oregon Penutian. (They number about sixty, of which maybe 60% are either cognates or extremely deep borrowings, while the rest seem to me to be more readily diffused material which has passed between Molala and Cayuse.) Firstly, they are primarily lexical resemblances. This is an important—and, given our knowledge of Cayuse structure, an unsurprising—consideration. We know little of Cayuse grammar, and what little evidence we have of it we are in no good position to interpret. The temptation to read Molala parallels into the two partial paradigms of Cayuse verbs which H. W. Henshaw collected and which Rigsby (1966:371) discusses, is strong (although the similarities with Molala forms are scanty), but it must be resisted. So must the temptation to try to apply some Indo-European-style 'combinatory method' in an attempt to make sense of the Cayuse verbal system, by appealing to assumed corroborative evidence from assumed Penutian congeners. I for one find the use of the combinatory method far too tentative and the results far too dubious even when it is applied to the task of reading inscriptions in dead Indo-European languages such as Phrygian—let alone if its use were to be transferred into a field such as Penutian.

Furthermore, these similarities of word-shapes are dispersed in an unusual pattern throughout the lexicon. There is a small number of strongly resemblant forms in the basic lexicon, including some verb-stems as well as nouns, but the majority of resemblant forms seem to be found in more peripheral areas of the culturally-shared lexicon. I refer in this case to those parts of the lexicon which relate to everyday phenomena which would have been known to most groups and which are not culturally-diffused traits. Resemblant forms are also to be found in some parts of the culturally-diffused lexicon, such as a form meaning 'trousers' which in Molala is *máyuq* and which Rigsby, citing from Henshaw, gives for Cayuse as *mér-yŭk* (Rigsby 1966:377). There may also be loans into Cayuse from Sahaptian languages (and maybe in the other direction); thus a Cayuse form such as *ti-man-uk-tai-yu* (Berman 1996:23), which Berman connects with a term meaning 'tattoo-marks', resembles Sahaptin *timaš* 'book'.

Additionally, the resemblant forms between Molala and other potentially Plateau Penutian languages, including Cayuse, tend to include a rather large number of ecological terms, especially the names of animals. This is also the case with a number of forms of animal names which look similar in Molala and in Sahaptian languages. Molala is an isolate; Sahaptin as a language cluster shows a fairly small degree of internal divergence, and many of these mysterious Molala animal names are partially analyzable in terms at least of possessing a recognizably Sahaptin affix, while others are not. Among these there may be loan material in both directions, and also words loaned to both languages from a third language, although we cannot be certain. Whistler (1977) has demonstrated the importance

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of presenting a historical analysis of ecological vocabulary in Penutian languages in California because of the light which it can shed on the patterns of historical migrations.

When this principle is applied to the Molala-Sahaptin/Klamath shared tranche of lexicon, we see that of the 31 forms listed, 16 relate to flora and fauna. Klamath ecological loans into Molala include forms with the following meanings: 'brown bear', 'white-tailed deer', 'dog', 'duck','fish-hawk', 'frog' (possibly influenced by onomatopoeia), 'huckleberry', 'mountain lion', 'raccoon', 'robin', 'gray squirrel', 'weasel'. Sahaptian loans into Molala with an ecological meaning are 'cougar', 'ground squirrel' and 'steelhead trout' from Sahaptin, and the possibly onomatopoeic form for 'magpie', taken from Nez Perce. In fact it is possible that the form for 'steelhead trout', Sahaptin šušainš, Molala susa: 'inc, is actually a loan in the other direction, since it involves a formant -inc which forms animal names in Molala (as Berman 1996: 22 points out), and which is also found in other Molala words which do not appear to be loans, such as pskaync 'beard' (cited in Rigsby 1969). But the list above, with its itemization of frequently-occurring species as loanwords, is surprising, especially if one recalls (following Kinkade et al. 1998: 69) that the Native peoples of the Plateau have no recollection of ever having migrated thither from somewhere else—somewhere with a different ecology.

8. WILLAMETTE PENUTIAN AS A 'DEAD STOCK'. Parallels between the nature of the relationship of the three groups within COP and between various members of Plateau Penutian (but especially between Cayuse and Molala) may also be seen in the case of Takelma and Kalapuyan, a grouping which has occasionally been dubbed 'Willamette Penutian'. The most recent discussion (Tarpent and Kendall 1998) suggests that Willamette Penutian is a spent group. Tarpent and Kendall maintain that there is too little linguistic evidence to link Takelma and Kalapuyan together as being more closely related to one another than they are to other Penutian (or any other) languages. That is to say, there is insufficient morphological and lexical evidence to show that they constitute a non-trivial grouping within Penutian. (It should be pointed out, though, that there is little available on Kalapuyan languages which is explicit and directly user-friendly enough about the morphology of these languages to enable most people to make statements about the structure of Kalapuyan.)

This finding runs counter to the observation in Berman (1988: 3-4), which states that there is a special relationship between Takelma and Kalapuyan, that it is genetic, and that it is obscured by the considerable structural and typological differences between the two families, which affect most parts of grammar including both noun and verb morphology. Berman (1988: 4) points out that morphological similarities between the two languages are few, but that there are some significant cognate sets. One involves the suppletive stem -ham, which means 'father' in both Takelma and Kalapuya, and which according to Berman is used only with 1st and 2nd person possessors in both (although it must be said that the Northern Kalapuyan paradigmatic data given in Hale 1846: 565 shows this stem being used with all persons except 1sg). This looks like a well-preserved archaism and one which suggests common origin.

This whole debate, however, may be simply an argument over a matter of degree of depth in the relationship, and a debate over taxonomic terminology. The relationship of the three branches of Kalapuyan to one another is quite obvious, even to a layperson, upon the presentation of sets of comparanda. The relationship of Takelma (which itself shows a small amount of internal dialectal divergence) to this Kalapuyan family is a much more difficult claim to sustain with evidence. What has not been discussed so far—although it may be a

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non-question—is whether Takelma is equidistantly related to all three branches of Kalapuyan, or whether it favors one of them over the other two (or indeed whether one of the Kalapuyan branches is less similar to Takelma than the other two are).

The fact that a special Takelman-Kalapuyan relationship is difficult to prove, however, in and of itself does not invalidate the claim that the two groups of languages are related. And indeed it does not invalidate the claim that Takelma and Kalapuyan enjoy a 'special relationship', namely that they are each other's closest genetic relatives. But it may be the case that although they are each other's closest kin, they are still extremely distantly related, so that the fact of their constituting a single genetic node at some non-trivial point is not very significant. It is also important to remember that it is impossible to prove that any two languages, both of which have been passed on from one generation of speakers to the next via genetic transmission, are not related.

For all that we know, the same may be said of Cayuse and Molala as was said of Takelma and Kalapuyan. They too may once have been the same language, but if so, this period of unity was such a long time ago that next to no evidence of it remains. And in the case of Cayuse and Molala, what evidence does remain may have been obscured by subsequent layers of direct and indirect borrowings from one language to another, from Cayuse to Molala and from Molala to Cayuse, and by borrowings by both languages (singly or jointly) from other sources. Kinkade et al. (1998: 61) cogently suggest that many of the resemblant forms shared between Molala and Cayuse may date from the period when the two languages were spoken contiguously (and, we assume when their territory had not yet been interrupted by the movement of Sahaptian-speakers).

9. A NOTE ON SOCIAL FACTORS IN PRE-INVASION COASTAL OREGON. We have seen that various kinds of linguistic change have taken place in a number of "Penutian' languages in what is now Oregon, and that these changes have often made it difficult for investigators to classify these languages in a more orderly and fine-grained fashion. Comparisons with what has happened in the course of the histories of Indo-European and its daughter languages are illustrative, but they leave a number of questions unresolved, because the picture in Oregon is one of groups of languages between which it is difficult to decide whether similarities are due to a state of initial cognation which has been followed by extensive time periods of separation, or to the borrowing of fabric to an extent which is rarely if ever found in Indo-European language, or to both. All this leads to a situation not unlike that connecting French and English as mentioned above, where the evidence for genetic relationship is present, but has been obscured by extensive waves of linguistic diffusion which have gone in both directions, but where they have moved especially strongly from French to English.

Since language is (among many other things) a form of social behavior, I feel that an attempt towards an appreciation of the situation calls for the application of a Milrovian style of social network modeling of sociolinguistics, of the kind that has been applied to speech communities in Belfast (Milroy 1987), but with some important modifications. The primary modification is the recognition that the clusters of linguistic variables which are of interest in this case, unlike those in Belfast, actually constitute different languages which may not be immediately comprehensible to some people with less strong links to a particular speech community, so that these languages need to be learned somehow. Furthermore, few or none of the languages which were in contact in Oregon at that time would have been widely spoken in any other area; we are dealing in these regions with a mosaic of small speech communities. In Belfast, by contrast, linguistic contact by Milroy's consultants with other

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speakers of what is to members of communities such as Clonard, the Hammer and Ballymacarett clearly the same language as their own (namely 'English') is plentiful in the extreme.

McLendon (1980) has presented a careful sociolinguistically-oriented picture of how a Hokan language, Eastern Pomo of northern California, came to die out, as the multilingual network in which its speakers had participated was weakened more and more by the onrush of Spanish and especially of English. This, and other sociolinguistic models, such as the 'social network' theory proposed by Milroy (1987), and insights from the pioneering work on language shift by Thomason and Kaufman (1988), can be used to shed light on the dynamics which brought about both language shift and language change among speakers of Oregon Penutian languages in the course of their post-contact history.

There are a number of socially-based reasons for the extensive degree of linguistic diffusion among Oregon Penutian languages in the times before extensive White contact, and few of them are exclusive to Oregon. We might suggest at least the following:

a) a tradition of the tacit acceptance of multilingualism as a utilitarian adjunct to everyday living within many communities,

b) the practice of intermarriage at various social levels (between chiefly families, concubinage between chiefs and slaves, and marriages among slaves) between speakers of different languages, with consequent L2 learning on the part of the mobile spouse, which itself was a consequence of

c) the limited choice of acceptable potential spouses within a small community (and within a small speech community),

d) the fact that the subsistence and settlement patterns in this region favored small winter villages in general, rather than the larger towns of the Southeastern US,

e) the presence of alloglot people in many villages (at opposing ends of the social scale, both slaves and shamans were often alloglot immigrants in Oregon tribal villages),

f) the important role of rivers and of villages situated near navigable rivers (including places such as The Dalles) in cross-country traffic, including the maintenance of what were often extensive trade networks for the provision of valued trade goods,

g) the superior technology and greater range of social possibilities of groups with bigger and sturdier canoes, such as the Chinooks.

In addition, the linguistic effects of the reformulation of new multiplex social networks due to intermarriage, and of the custom of addressing individuals by kin terms rather than by their given names, may both explain the wide spread of a number of kin terms with broadly similar shapes and functions across the Coast Oregon Penutian languages.

The population figures provided by Boyd (1990: 136) for Alseans, Siuslawans and Coosans at some unspecified point (but apparently around 1775) are 3060, 2100 and 2250 respectively. These are all a fraction, and a small fraction at that, of the size of populations of speakers of most European languages at that time. It would have been possible for a peasant in, say, Tsarist-dominated parts of Poland, to go throughout life without ever meeting another peasant of their status who spoke a different language, let alone marrying someone whose native language was different from their own. In contrast, and even before the balkanization which led to many speakers of Oregon Penutian languages being exiled to Grand Ronde or Siletz in the 1850s, interaction at many social levels between speakers of different Oregon Penutian languages and indeed different Native languages was a much more everyday occurrence. The very fact that the Cayuses were switching to Nez Perce, as pointed out above, attests to the power of language shift in the era before Native-White contact.

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The sociolinguistic history of Native Oregon has yet to be written. If and when it is written, it will show that multilingualism of various kinds, especially in the early post-contact generations, was widespread, as indeed was a history of shifting from one Native language to another (and in these cases, making a further shift to English; Swadesh worked with the last speakers of several languages during his Penutian Vocabulary Survey). The people who worked with Morris Swadesh on the Penutian Vocabulary Survey of 1953 (which was documented in Swadesh 1954) often had life histories and ancestries which indicated this.

For example, we know that Swadesh's Nez Perce consultant Philip Geyer had had some Cayuse-speaking ancestors, and he remembered a little bit of Cayuse which can be found in Rigsby (1969); in the 1930s Swadesh had tried without success to get Cayuse data of his own, and had collected materials in the variety of Nez Perce which the Cayuses were now using. John B. Hudson, the last person who could speak Santiam Kalapuya, actually tells Swadesh on tape that his mother's Yonkalla Kalapuya speech was unintelligible to his Santiam-speaking father, while during the same investigations Swadesh also secured Wasco-Wishram vocabulary from Jasper Tufti, who was apparently of part-Molala origin. The maternal grandmother of Laura Hodgekiss Metcalf, who worked as Swadesh's Miluk consultant in this exercise, was known by an Upper Coquille Athabaskan name which has been spelt *Ch'ishgeeyu*, and which has the meaning of 'old woman' (it was apparently given because of her precocious interest in the customs of her people). From an earlier period of investigation, Victoria Howard, who provided Melville Jacobs with some Molala data (Berman 1996: 2), knew some Molala, and spoke English, Chinook Jargon and (most famously) Clackamas fluently, and her husband Eustace Howard knew at least English and Santiam Kalapuya. Much of Leo Frachtenberg's work on Lower Umpqua (Frachtenberg 1922b) was carried out with William Smith, an Alsea who had a working knowledge of Lower Umpqua and some English, in addition to fluent Alsea, and who conversed with his Lower Umpqua wife Louisa (another of Frachtenberg's consultants) in Chinook Jargon.

10. CONCLUSION AND A VIEW TOWARDS THE FUTURE. To sum up: all sorts of linguistic evidence are of interest to the Penutianist who is trying to gain more insight into the history of Penutian languages, although these different types of evidence tell the linguist different things. Typological evidence is interesting and a typological dossier for these languages may be worth assembling as a preliminary step, but even the presence of clusters of marked typological features does not of itself allow us to distinguish any Penutian from non-Penutian languages. The corollary is also true: the absence of these features in a particular language does not mean that the language is not Penutian, simply because it does not obey an assumed 'Penutian type', as the history of Chinookan shows. More important than typological 'pattern' evidence, whether syntactic or phonological or whatever, is the 'fabric' evidence which is provided by the presence of various kinds of morphemes.

Some kinds of linguistic fabric evidence point to the likelihood of a history of common descent—the presence in more than one language family of the forms of a number of high-frequency shared verbs, for instance. But much of the evidence points more readily to a history of shared contact, which was at least in part enforced because of the relative smallness of size of most speech communities in Native Oregon. The opportunities for contact via linguistic diffusion, and (when the social occasion was propitious) for multilingualism, second-language learning and use, and a tendency towards shift from one language to another, were plentiful, and both diffusion and shift appear to have happened to a degree far in excess of what we find in Indo-European societies, which in any case constitute much

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larger speech-communities. Consequently, the kinds of material which have been diffused in Oregon Penutian languages are much broader in range and scope than those which are normally found diffused in Indo-European languages, and this naturally has consequences for the diachronic study of such languages. In some cases the linguistic material which has been diffused from various directions in the course of this contact history can swamp by sheer force of quantity that linguistic evidence from the historical period which is actually indicative of shared origins. For all we know this may be the case with the shared material which one finds existing between Molala and Cayuse.

Some of the kinds of 'fabric' which works as being available to the investigator as immediate and instant evidence of genetic relationship in some language families, such as a shared set of basic lower numerals, or kinship terminology, may be effective in Indo-European or in Semitic. However, as markers of genetic affinity they will not work effectively in Oregon Penutian languages, where such concepts are often, or even generally, found to be expressed by the use of locally-diffused material. What works best to provide initial proof of genetic relationships in the Old World need not be assumed always to work as unexceptionably in the New World, as data which have been used to link the Coast Oregon Penutian languages suggest. (However, I would argue that even Indo-European is a language family which still has a number of lessons to teach people who are interested in methods of diachronic investigation, in terms of telling us what kinds of linguistic change are possible.)

The reverse claim may also apply, but historical linguists have been extremely slow to apply those techniques which have been used to prove cognacy among American or Australian or other 'exotic' languages, to the elucidation of first-order relationships among Indo-European or other Continental Eurasian language groups. (Let us remember that there is still not a consensus classification of the branches of Indo-European which most Indo-Europeanists accept.) We can only speculate about what Proto-Indo-European would have looked like if it had been reconstructed using only the methods which have been designed to retrieve earlier forms of American macrofamilies, although the example set by Dyen, Kruskal and Black (1992) demonstrates the kinds of cladistic and other results which one can obtain by comparing one feature of numerous languages, namely Swadesh-list lexicon, and when one is not working to any classificatory preconceptions. If Penutian does exist as the ancestor of its 'classical' components, it may be a few millennia deeper than Indo-European is, but it is useful to see the results of an experiment at cladistic reconstruction which is carried out using equivalents of the kind of data which are all that are available to people working with a hesion such as Penutian, which contains so many single-language branches.

That there is room for fruitful dialogue between Indo-Europeanists and others on this matter is, however, beyond doubt. We need both to recognize the relative inapplicability for some cases of genetic relationship of some of the tools in the European diachronic toolkit (although most or all should be tried out first), and by contrast, to extrapolate from what we have found effective in 'exotic' cases, in order to increase the number and range of instruments which we may use.

Nevertheless the presence of such diffused material in a language is significant in another way. It does tell us a lot more about the range and depth of contact between groups speaking different languages, and about the possibility of language shift (often from one Penutian language to another, and often as a result of exogamy) at some point in history. The most reliable elements of fabric which will point to clear linguistic affinity in at least Oregon Penutian languages (but they may not be universal cross-linguistically in their diagnostic reliability) are personal affixes, whether they be prefixed or suffixed to the stem, as is

suggested by material in Coast Oregon Penutian languages, and clusters of morphological irregularity or cases of suppletion, as are found shared in Kalapuyan and Takelma. These kinds of evidence generally work very effectively for Indo-European languages as well.

And in any case, it is true that the role of diffusion as a factor which contributes towards the momentum of linguistic change has been seriously underplayed in the historical linguistics of many major families, as have the kinds of items which can be and which are diffused. Simply because a certain kind of item, say a bound inflectional morpheme, is never borrowed among the members of one linguistic family does not mean that borrowing this item from one language into another is a practice which is embargoed globally.

Indeed, if as much attention had been paid to the manifestations and consequences of diffusion, and of uniquely shared innovations, among Penutian languages, as has been given to more purely typologically-based speculations about their interrelationships, we might be rather more advanced in our attempts to reconstruct what Proto-Penutian (had it existed) might have looked like.

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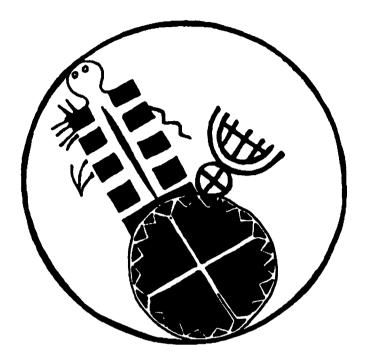
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PREFACE

The year 2000 was the 30th anniversary of the first Hokan languages conference. That first conference was imagined, planned and run by Prof. Margaret Langdon at the University of California at Berkeley, with the assistance of Prof. Shirley Silver of California State University at Sonoma. Almost every year since then, Hokan workshops and then Hokan-Penutian workshop in the previous few years had been either very small or even cancelled due to the lack of a sufficient number of people submitting paper titles. There was some thought of abandoning the Hokan-Penutian workshops altogether. Margaret felt that it would be a shame for this long tradition to end without a last hurrah, and so I offered to hold a Hokan-Penutian Workshop at Berkeley in conjunction with the "Breath of Life" Language Workshop for California Indians. The Breath of Life Workshop is a biennial gathering of California Indians here at Berkeley, and is designed primarily for people whose languages have no speakers left. We give them tours of the campus archives and show them how to use publications, fieldnotes and recordings of their languages for their own purposes – primarily language learning and teaching. I felt it would be a good thing to show the linguists who spent their careers working on these endangered languages to see the use their work is being put to by the descendents of the very people they worked with years ago. Therefore, the first session of the Hokan-Penutian Workshop consisted of presentations by the participants in the Breath of Life Workshop. The anticipation of this treat may have played a role in bringing a relatively large crowd here in 2000, perhaps along with billing the workshop as "The (Last?) Hokan-Penutian Workshop." Sixteen papers (not counting the Breath of Life presentations) were given at the workshop, eight of which are published in this volume.

With both the Hokan and Penutian hypotheses in doubt, there is always a question as to which languages should be included at the workshop. Although my sympathies are with the "splitter" camp in linguistics, I'm definitely a social lumper. Therefore, for purposes of the workshop I chose to define "Hokan" and "Penutian" as rubrics rather than language stocks, and advertised the workshop as being "for any language that has ever been hypothesized to be Hokan or Penutian." We thus have papers ranging from Tsimshianic to Zuni, and—oh, well – we even accepted Juliette Blevin's excellent paper on Yurok, an Algic language, which has never been hypothesized as either Hokan or Penutian.

At the business meeting held at the end of the Hokan-Penutian workshop, no-one wanted to say that this was the last one. Instead, we voted to continue with the workshops on a biennial basis, to be held here at Berkeley from now on, overlapping with the Breath of Life Workshop as it did in 2000. As I write this preface, the two years have already passed, and we are preparing for the 2002 Breath of Life Workshop, which this year will overlap with – not the Hokan-Penutian Workshop – but the 50th Anniversary Celebration of the Survey of California and Other Indian Languages. The upcoming conference for the Celebration subsumes participants in Hokan-Penutian Workshops. I imagine that our biennial gathering will continue on: whether it will be a Hokan-Penutian workshop in 2004 or something broader than that remains to be seen.

Leanne Hinton
Director of the Survey of California and Other Indian Languages

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