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Studies in Linguistic Paleontology is a venue for works dealing with the reconstruction of prehistoric cultures from the evidence of linguistic prehistory, and with the method and theory involved. While the series is intended primarily to present the editor's own scholarship, Studies in Linguistic Paleontology welcomes suitable work by others, of monograph length or shorter.

Series Editor

Paul Proulx

P.O. Box 111

Heatherton, Nova Scotia

Canada B0H 1R0

Email: paul\_proulx@yahoo.com

Editor's web site:

<http://www.geocities.com/CollegePark/Bookstore/4583>

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# Women in Proto Indo European Society

Paul Proulx

Heatherton, Nova Scotia

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# ABSTRACT

The Proto Indo Europeans had recently shifted from a forager base supplemented by such domestic cattle as they could overwinter, to a fully agricultural economy. The shift probably responded to urgent security needs favoring population concentration. It was possible because women were able to increase production of hitherto marginal grains. This in itself made women enormously important.

However, the same intensive warfare that favored population concentration also decimated the warrior population. As a result, it became difficult to base social organization on male kinship, and women became the crucial live social links among the surviving male population.

The result was that men tried hard to exert influence over women, as the key figures holding the society together, and to organize themselves through systematic matrilinear cross cousin marriages. At the same time they acknowledged female superiority, as women were key contributors to the survival of this very stressed society. The needs of women with respect to child raising and farm work produced wife centered residence, which in turn favored endogamy, and served as the basis for matriliney.

Only long after PIE times, and after migrations of conquest, did the sexual balance of power begin to shift toward patriarchy in those areas whose soils permitted plow agriculture. This was based on male possession of agricultural land by right of conquest, and

marriage alliances with foreign people (to legitimize their possession of the land). Native IE women lost most of their importance in these new societies (India, Greece, Rome), and they were gradually marginalized from both economic and political affairs.

(Key words: women, prehistoric societies, lexical reconstruction, intensive warfare, Crow-Omaha organization, female status, Indo European society, Indo European kinship, Proto Indo European)

# Preface

The first of the lexical reconstruction projects I undertook, about 15 years ago, was the reconstruction of Early Lenapean. It turned out that before it could be completed, some other studies had to be undertaken, and many misconceptions corrected.<sup>1</sup>

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<sup>1</sup> At the time I was handicapped by the received idea that Proto Central Algonquian as reconstructed by Bloomfield was really Proto Algonquian. This meant that I falsely projected back to Proto Algonquian times what I knew (or thought I knew) about Proto Central Algonquian society.

Since there was a shift in residence from wife centered to husband centered between Proto Algonquian and Proto Central Algonquian times, in my account of Early Lenapean society I was working with an incorrect starting point.

This left me to explain a supposed shift from Proto Algonquian husband centered residence to Early Lenapean wife centered. Murdock had claimed in 1949 that a direct shift of this sort was impossible, and this view was widely accepted. Divale had later suggested that shifts in residence from husband centered to wife centered did occur when people migrated into territories already fully occupied (for the current level of technological exploitation), but he did not claim that the shifts were direct. That is, his account could easily accommodate a transitional stage of mixed residential patterns (multilocality).

The problem was, in all known Algonquian cases, shifts from

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husband centered to wife centered residence do involve transitional stages of multilocality, and multilocality destroys the terminological evidence of residence type. However, this evidence was inherited intact from Proto Algonquian to Early Lenapean (Unami Delaware). Therefore, a multilocal transition was most unlikely in the Lenapean case.

Uneasy due to this paradox, I began the investigation of Proto Algonquian (and Proto Central Algonquian) society which led to the first monograph in this series (Proulx 2004a), as well as the recognition of a Central Algonquian branch of the Algonquian family.

Another prerequisite for the present monograph was a good understanding of the role of women in prehistory. Neglected if not actively denied for the better part of a century, the achievements of women play central roles in any account of kin based societies experiencing intensive warfare.

In contrast with the activities of men, that had been described in great detail and often, I found that to learn much about women I had to really search the literature diligently and then put the pieces together myself. In the end, I discovered enough about the evolution of the status of women to produce a separate monograph, also included in the present series (Proulx 2004b).

I also knew that Quechua society was vastly different from the societies scholars usually have in mind when they form hypotheses based upon perceived universals, and I set out to see how widely accepted social theories would



Perhaps the greatest delaying factor in the reconstruction of Early Lenapean societies was that my evidence led me to reconstruct rather exotic societies for them. It was so unexpected, that I tried every other hypothesis before coming to it. And even then, I had trouble believing what the evidence was telling me.

Then one day, it occurred to me that the complex but still kin based societies I was reconstructing for the Early Lenapeans might have an analog with the Proto Indo Europeans. I had never been at all impressed with the flimsy "evidence" given for the received reconstruction of PIE society as patrilineal patrilocal.

Moreover, the IE expansion across Europe and southwest Asia looked a great deal like the Lenapean expansion up and down the East coast of North America. PIE kinship terminology, when I examined it, in fact did suggest striking structural parallels with Early and Middle Lenapean societies.

And so, for a long time I worked on PIE and Early and Middle Lenapean in tandem, trying to distinguish what was in fact similar from what was different. In the end, it appears that the two cases are substantially similar, though there are surely important differences as well.

The Lenepean and IE reconstructions confirm one another. It

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account for the Quechuas. It turned out that these theories mostly had to be modified a great deal to do so. So again, the present monograph should be read in the context of what Quechua society has to tell us (Proulx 2005a-b).

would be unlikely that by chance alone the evidence in the one or the other case should point to an exotic structure and seem to exclude all others. However, for chance alone to explain evidence of largely similar exotic structure in two societies entirely separate in time and space, that would be far more unlikely yet.

For a variety of reasons, I can be more confident of my Lenapean reconstructions than of the one I will here propose for PIE. First, I am a lifelong specialist in Algonquian linguistics, but have limited knowledge of Indo European. Second, Early Lenapean is a relatively recent society (about 1000 years before present), while PIE is much more ancient (about 6000 years before present). PIE is what a Micmac friend called "a cold trail."

Third, there were 3 allied Lenapean societies, and only one IE one. For all these reasons, I was able to find much more evidence in the Lenapean cases (see Proulx 2005c).

The very best way to read the present monograph is to begin by reading my reconstruction of Proto Algonquian society, and then to study my Lenapean monograph. However, it can also be read independently, simply as a critique of received ideas on PIE society, together with an alternative hypothesis (though my hypothesis may appear somewhat wild without the background of the monographs discussed above).



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"The comparative method in linguistics as a tool for reconstructing prehistoric linguistic data, developed chiefly ... by an investigation of the so-called Indo-European languages ... The reconstruction of prehistoric linguistic data can be of the greatest help to prehistoric ethnological reconstruction."

"... success depends on ... imagination, which is not a dreamer's or a poet's fantasy, but a capacity, trained by experience, to visualize a possible situation."

"All prehistoric reconstruction is of course purely hypothetical ... based on conjectural assumptions ... a guess. This can, however, not be inverted: it is not true that every guess is a scientific hypothesis. ... A characteristic feature of a scientific hypothesis lies therein, that it makes possible predictions and hence can be tested by experiments".

"If the final proof for the correctness of case-by-case reconstruction lies herein, that they close together into consistent and compact systems, the very fact that we do get such coherent units is a confirmation of the soundness of our procedure as a whole .... Our method of operation can best be likened to the restoration of a mosaic ..."

--- Paul Thieme (1964:585, 594) in *The Comparative Method for Reconstruction in Linguistics*.



# Chapter 1: Introduction

## 1.1. The Indo Europeans in history.

The Indo European peoples have played a large role in the history of the modern world, and were important in ancient times as well. Nearly all European languages (except Basque, Finnish, and Hungarian) are descendants of Proto Indo European, as are Iranian and the languages of northern India.

The homeland of the Proto Indo European people is generally thought to have been somewhere in the southwestern part of the old Soviet Union. Many other theories have been proposed, some rather far-fetched.

"This quest for the origins of the Indo-Europeans has all the fascination of an electric light in the open air on a summer night; it tends to attract every species of scholar or would-be savant who can take pen to hand. It also shows a remarkable ability to mesmerize even scholars of outstanding ability to wander far beyond the realm of reasonable speculation to provide yet another example of academic lunacy" (McKenny 2000).

I will not speculate on the subject. However, where ever they may have lived originally, at around 4000 B.C. the Indo Europeans began to occupy more and more lands. To the northwest, they expanded into the thinly populated territories of forager peoples.

These foragers could not have resisted the invasion very much. They probably retreated further north, and may have traded products of the hunt for agricultural ones produced by Indo Europeans. Some may have married Indo Europeans and been assimilated. In any case, Indo European society was not much changed by the encounter.

The Indo European expansion to the south and southeast was of a very different nature. Here they found populous agricultural peoples, who resisted invasion. They fought them, defeated them, and became their overlords.

Since these southerners produced complex literate societies quite early, they have been much studied. As a result, Proto Indo European society has sometimes been modeled upon them, or at least thought to resemble them to a large extent. However, they are the products of conquest and plow agriculture, radically transformed from what PIE society was.

## 1.2. PIE as a Proto Language and Proto Society.

It seems almost too obvious to need saying, but PIE was a proto language, and its speakers formed a proto society. Proto languages are the parent languages of language families, the last unified form of a language as its speakers were splitting up, before their languages evolved their separate ways.

It has often been said that proto languages are ordinary languages, and should look like them. This is true in some ways.

However, they were spoken by peoples whose societies were on the point of fragmenting, and this is not an ordinary social situation.

There are of course many reasons why a society might subdivide, and each has different implications. However, the reason of immediate interest to us is intensive warfare.

Intensive warfare kills many men, and leads to a very high fertility rate among women, as they try hard to produce more sons than they fear are likely to die in battle. A by-product of this effort is many daughters, who rarely die in battle and like their mothers are highly fertile.

Counter-intuitively, therefore, intensive warfare produces a population boom, and the need for living room -- including hunting grounds -- soon leads to out-migrations into adjacent or more distant territories. This out-migration fragments the proto society and the language it speaks into several divisions. This produces a proto language.

Ordinary warfare is common enough in this world, but intensive warfare is rare. It probably most often results from societies fighting desperately for scarce resources, essential either to the society as a whole or to men (the warriors) in particular. Whatever the reasons, it produces great social disruption, and thus a society notably different from the great majority.

Few if any of us alive today has seen a small kin based society

undergoing intensive warfare, and we have no detailed historical record of any. Therefore, we should not be surprised if some proto societies have features that seem unusual to us, that are not exactly duplicated in any society we are familiar with.<sup>2</sup>

In a small scale kin based society, where every able bodied man is a warrior engaged in the intensive warfare, an extraordinary social situation of this sort is likely to leave marks on the lexicon, notably on the kinship terminology. These changes to the kinship terminology are what I mainly use in my reconstructions.

Proto Indo European society and three eastern Algonquian proto societies (Proto Medial, Proto Coastal, and Proto Subboreal) were small kin based societies that I hypothesize were undergoing intensive warfare.

These prehistoric proto societies survived thanks to their women, who by a high birth rate more than made up for those who fell in battle, who substituted for men in political roles, and who became the main providers of their families through farming.

This farming not only provided food for a larger population, it allowed people to concentrate in defensible palisaded villages, rather than living in small vulnerable forager camps. For these achievements, women were justly honored and admired.

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<sup>2</sup> The building blocks out of which such societies are constructed, although rare, ARE almost all attested in a few contemporary and historically attested societies (perhaps as survivals of the recent prehistoric past). However exotic, we can be sure they are mainly possible features of societies.

The techniques for reconstructing a proto language from the clues in its scattered daughter languages are collectively called the Comparative Method. When we reconstruct a proto language by the Comparative Method, often (but not always) we are reconstructing the language of a people who had just undergone such an intense struggle to survive intensive warfare, and had more than succeeded.

Having had a population boom, they occupied new territories near and far, in each of which their language pursued its separate evolution. That is what made their language of prior to the migrations a proto language.

If, thanks to having reconstructed their ancestor proto-language (like Proto Algonquian) or thanks to internal reconstruction, we know what kind of societies they had before the intensive warfare began, we are in a position to reconstruct their adaptations to the crisis step by step.

In the case of PIE, internal reconstruction allows us to glimpse some aspects of their society a short time before PIE times themselves (at least a few decades, more likely a century or two). This earlier society is called pre-PIE. However, it was already agricultural and involved in intensive warfare. We know little or nothing of Indo European society before that time.

### 1.3. The Structure of This Book.

There are three main parts to this book. First, I argue that the

widely accepted evidence of PIE patriliney, patrilocality, and exogamy are very flimsy indeed. Nowhere else in linguistics is such doubtful and even improbable reasoning given credence. It should not be taken seriously here either, despite the wide consensus it commands.

Second, I argue on the evidence of lexically reconstructed PIE kinship terms that Proto Indo European society may have been somewhat similar to prehistoric Lenapean (Algonquian) societies (with wife centered residence, endogamy, and perhaps matriliney, see Proulx 2005b). Third, I discuss the implications for various alternate theories about Proto Indo European society.

In addition to these three main parts, I end the book by discussing the history of reconstructions of Proto Indo European society, in the context of anthropological debates of the times. I also look at anthropological method and theory themselves, and suggest how they could benefit from the application of anthropological theory to the reconstructions of proto societies.



## Chapter 2: The Bosrup Hypothesis and PIE Society

Particularly useful in understanding PIE society are Ester Bosrup's criteria for classifying agricultural economies, as elaborated by Jack Goody.

### 2.1. Did PIE Society Have Plow Agriculture?

Undoubtedly one of the most important sets of insights anyone ever had into the nature of agricultural societies we owe to Ester Bosrup's book *The Role of Women in Economic Development*, and to Jack Goody's elaboration and confirmation of her hypothesis (Goody 1976).

What Bosrup discovered is that agricultural communities fall into two broad types: "the first type is found in regions where shifting cultivation predominates and the major part of agricultural work is done by women. In such communities, we can expect to find a high incidence of polygamy (polygyny), and bride wealth being paid by the future husband or his family. The women are hard-working and have only a limited right of support from their husbands, but they often enjoy considerable freedom..."

"The second group is found where plough cultivation predominates and where women do less agricultural work than men. In such communities, we may expect to find that only a tiny minority of marriages, if any, are polygamous; that a dowry is usually paid by the girl's family; that a wife is entirely dependent upon her husband for economic support; and that the husband has an obligation to support his wife and children..." (Bosrup, cited in Goody 1976:32).

How the second configuration developed out of the first is a matter of some importance. The farming of the first configuration provides adequately for one's family, with little or no surplus. As long as this is the only agriculture known, men have no interest in getting involved in it (beyond helping their wives with the heavier work). They regard it as an unprofitable and tedious task, which they happily leave to women.

But once the plow is available, as Goody points out, it permits a man to cultivate about 16 times more land than he could without such a plow, and to become wealthy by accumulating land and surplus grain grown on it. This motivates men to acquire individual ownership of land and hoard it, making it scarce. But how would an individual man acquire communally owed land controlled by women?

The usual way was probably that a man agreed to serve as a soldier in some army of conquest, and take a piece of land as payment for his services. He then legitimated his claim to the land by

marrying a woman of the family whose land was confiscated. His working the land himself with a plow further legitimated his claim to it.<sup>3</sup>

Once the plow came into use in large areas around the Mediterranean Sea, where the light soil could be worked with the primitive plow, most land soon passed from female to male hands. In time, this placed women in the position of dependents, and produced the first social classes: landed versus landless men.

Women, who no longer owned the land, were progressively left to process the grains of the landowners (such as their husbands), much as women in hunting societies are left to process the meat obtained by hunters. Regardless of just why and how this came about, what is certain is that it did in many Old World societies.

Processing activities are often time consuming drudgery, especially where most Old World grains are concerned (notably wheat). In addition to other factors, this occupied women's time and restricted their participation in the wider (non-domestic) activities of a society.

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<sup>3</sup> It has been suggested that cultivating a field with a primitive plow drawn by semi-domesticated cattle is heavy work, suited only to strong men. Since the land belongs to those who work it, men acquire possession of the land and its crops when plow agriculture is introduced. However plausible though this account may be, there is little empirical evidence in favor of it. On the contrary, when Quechua men use animal drawn plows on the family fields, as they often do, ownership of the crops remain firmly in the hands of their wives.

Basing himself on Hole and Flannery, Goody (1976:24) estimates that in the Middle East "the role of intensive agriculture, by plough or by irrigation, is seen as critical in the developments that occurred after 2,000 B.C."<sup>4</sup> In adjacent areas, it came somewhat later.

However, it's clear from the fact that the earliest attestations of Indo-European languages already show marked differentiation, that they had been diverging for many centuries prior to the earliest Indo-European texts (a little after 2,000 B.C.).

Indeed, both the magnitude of the differences among the main Indo-European languages when they were first written down, and the degree to which they were internally subdivided into dialects, are somewhat comparable to what one finds among the Romance languages, and we know from the written records of Latin that the Romance languages have been diverging for roughly 2,000 years.

Hence, Lehmann's (1995:266) estimate of 4,000 B.C. as the latest possible date for the breakup of PIE linguistic unity is very plausible. That's about two thousand years before the transformation of Indo-European society produced by plow

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<sup>4</sup> An early date for a transition to patriarchy may show up independently in the ideological realm. "Somewhere around 2000 BCE, the remnants of the prehistoric matristic cultures begin to be eliminated in new religions, new cosmologies, new ritualistic works of literature" (Thompson 1996:194).

However, in most IE cases patriarchy may have come later. For example, Bachofen interprets the *Oresteia* of Aeschylus as the dramatic representation of the conflict between declining mother-right and the new father-right that arose and triumphed in the heroic age" (Engels 1884, Preface to the Fourth Edition, 1891).

cultivation.

## 2.2. PIE Agriculture.

### 2.2.1. Plow Agriculture?

Cattle drawn plows cannot be reconstructed for PIE times, as the European and Indo-Iranian terms do not agree.<sup>5</sup> Most of the European languages have a verb 'to plow' cognate to Latin ar-, while Wojtilla (1986:30) traces Sanskrit karsati 'to plow' (and Avestan yao- karš- 'to cultivate barley') to PIE \*kwels- 'to scratch, draw a line'.<sup>6</sup> The simplest interpretation of the data is that the term for 'plow' developed separately in the two areas, sometime after the long distance IE migrations.

However, even if a verb meaning 'to plow' in the daughter languages were reconstructible for PIE, it still would not prove that the PIE people knew of the cattle drawn plow in particular, as a comparison with the Quechua (Inca) languages makes clear.

Ayacucho Quechua taklla- 'to plow' has cognates in all or nearly all the Quechua languages and dialects, suggesting that it dates back

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<sup>5</sup> Several other terms related to agriculture show the same split between European languages and Indo-Iranian ones (Lehmann 1995:280, Meillet 1964:397-98).

<sup>6</sup> In fact, to someone used to working the soil with a digging stick, one of the salient features of animal-drawn plows is how they necessarily till the soil in a line (rather, say, than making "hills" between stumps as is common in shifting agriculture).

to Proto-Quechua, hundreds of years before Columbus. And the verb itself surely does. Only, before the Spaniards introduced the animal-drawn plow, it referred to working the soil with the digging stick (now known as the chaki-taklla 'foot-plow'). A word can change its meaning in a parallel way across several dialects and closely related languages. As technology changes, a shift from 'working the soil with a digging stick' to 'plowing' is a particularly likely one.

Neither is there evidence of PIE plowed fields. In a detailed analysis of PIE \*agrós, which contains the root we find in agriculture, Thieme (1964:591) showed how 'field, acre' is a secondary meaning of this word in Gothic, Latin, and Greek. Even in these three languages, early citations of the word or its derivatives sometimes refer to 'pasture' (the only meaning of the term in Sanskrit). In addition, PIE \*agró- can be etymologically analyzed as \*ag- 'lead, drive' (typically used of the action of driving animals to pasture) plus suffix \*-rō.<sup>7</sup>

Evidently, the PIE people had pastures, to which they drove their cattle. When they had finished pasturing sheep and pigs on a plot - and these had manured the soil and cleared it of weeds and roots - the women probably worked it with digging sticks much as did the pre-Colombian Incas. It is evidently only much later, after the departure of the Indo-Iranians from the IE homeland on migratory conquests, that the IE people began to convert pastures

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<sup>7</sup> PIE \*agró- may be a late thematic stem, built on earlier athematic \*ag- plus nominalizing \*-r, as attested in Latin ager.

into agricultural fields cultivated using animal drawn plows.

### 2.2.2. Shifting Agriculture?

The best evidence of PIE gardening is indirect.<sup>8</sup> Early European agriculture was established by about 4500 B.C., and from the first the archaeological record features domestic cattle prominently (Smith 1995:102). Moreover, PIE had a term *\*peku* 'movable wealth, sheep' (Benveniste 1970:318), and some specific terms for farm animals, such as *\*óvis* 'sheep' and *\*guōus* 'cattle', and these are deeply embedded in the PIE language and culture (see Thieme 1964:589-593).

However, cows could not be kept over the winter in temperate Europe unless adequate amounts of vegetable foods had been set aside to keep them till spring. It is possible that the Indo Europeans of 4500 B.C. used cultivated grains mainly as winter fodder for their cattle.<sup>9</sup> In such a case, they may have referred to them mainly as

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<sup>8</sup> Direct evidence of PIE gardening is not available, but early horticulture is very hard to distinguish from gathering, if one has only reconstructed vocabulary to go by. Absence of evidence isn't evidence of absence.

Unless one can reconstruct a verb specifically meaning 'gather seeds for planting' or 'plant seeds', there will be no way to tell from the lexicon whether the seeds gathered are wild or domesticated varieties. In the IE case, no verb meaning 'gather seeds for planting' ever developed, and one meaning 'plant seeds' is limited to Europe. However, it is perfectly plausible that PIE agriculturalists simply used the equivalents of the descriptive English phrases, rather than monolexemic verbs.

<sup>9</sup> "Even when they were forced further to the north and west, the Semites and Aryans could not move into the forest regions of western Asia and of Europe until by cultivation of grain they had made it possible to pasture and especially to winter their herds on this less favorable land. It is more than

"fodder," and the names of the particular varieties may have had little importance. Nevertheless, the production of fodder (hay, straw, and grain) was surely a horticultural activity, crucial to subsistence.

Because meat tends to be lean toward spring, an adequate diet requires some carbohydrates to go with meat at that time of year. Hence, almost certainly some stored grain was eaten by the PIE people, if only in late winter and early spring. Human consumption of cereals had the potential for expansion, should the need arise.

### 2.3. Goody's Diagnostic Tests for Shifting Versus Intensive Agriculture.

Expanding upon and testing Bosrup's thesis using data from the Ethnographic Atlas, Goody (1976:28-29) concluded that intensive (plow) cultivation correlates with complex social organization ('large states') and social stratification, particularizing rather than classificatory kinship terminology ('sibling kin terms'), lineal endogamy (familial in-marriage, especially 'father's brother's daughter marriage'), and, to a lesser degree, with monogamy, cross sex inheritance ('diverging devolution', especially in the absence of a same sex heir), in-marriage, and prohibited premarital sex. None of these is reconstructible for PIE society.

O'Brien (1980:130) reconstructed only four terms for PIE social groupings, which I would gloss as follows: \*domos 'household',

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probable that among these tribes the cultivation of grain originated from the need for cattle fodder and only later became important as a human food supply" (Engels 1884).



\*weikos 'group of households', whose original meaning could simply have been 'camp' or 'clan camp' though later it came to mean 'village, hamlet, neighborhood', \*ġenHos 'kin group', and \*teutā 'people, population'.<sup>10</sup> These terms in no way suggest a complex or stratified society.

Moreover, Lehmann (1995:254, 278-279) reconstructs few numbers: "we accept the set to 5, and at a later period to 10, as Proto-Indo-European". Again, this provides no evidence of a complex society.

There's a PIE verb 'to sew', but no simple (monolexemic) noun 'tailor'; 'to weave' but not 'weaver'; 'to form the earth' but not 'potter'; 'to row' but not 'sailor'; 'to grind' but not 'miller'; 'to forge' but not 'smith' (Meillet 1964:384-386). Any two or three of these pairs could result from the chances of preservation, but not very likely the whole set. These terms show no sign of PIE craft specialization.

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<sup>10</sup> There are derivatives in \*-no 'head of' and compounds in \*potis 'lord of' to designate the leaders of these units in some of the daughter languages. However, these don't all agree, and those that do could be parallel innovations or loans among the early dialects.

The only monolexemic term for a leader was \*reg-, and it has secure cognates only in Latin and Old Irish (the alleged Sanskrit cognate evidently being spurious, see Lehmann 1995:68). This limits it to Europe, suggesting possible post-PIE origins.

However, even if it should date back to PIE, this would leave open the question of what kind of leader. Even the Crees have a term for a leader, okima:w 'chief', but all it refers to is a man whose opinion is highly valued (or, failing that, the oldest hunter in the camp). It's not even limited to political leadership: a man skilled in building canoes, whose opinions others seek when they need to build one, is also a 'chief' in that context.

Only Descriptive kin terms, clearly separating lineal kin types from collaterals, suggests close attention to matters of inheritance, notably for purposes of inheriting scarce land. In the European context, this would imply plow agriculture.

PIE had three classificatory kin terms (\*awos 'grandfather; mother's brother' and its reciprocal \*nepotis 'sister's son, grandchild', and \*genHr 'daughter's husband, sister's husband').<sup>11</sup> Classificatory kin terms suggest a lack of attention to matters of inheritance, notably due to the abundance of land associated with horticulture or shifting cultivation.

Two institutions usually limited to monogamous societies are concubinage and prostitution. It might be supposed that 'concubine' is reconstructible for PIE (from Greek parakoitos and its cognates, see Friedrich 1966:20), implying monogamy and thus plow cultivation.

However, the Homeric Greek word is (par)ákoitis, and has a masculine gender counterpart (par)ákoitēs. Gates (1971:19) explains them respectively as "the personal relationship between two people who happen to be man and wife" and as "an emotionally charged word for husband."

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<sup>11</sup> These are least well preserved in Greek and Indo-Iranian (O'Brien 1980:134, Benveniste 1969:267-275), suggesting that their speakers were among those who in the relevant respects departed earliest or most radically from the PIE subsistence economy. However, classificatory terms were eventually lost from all or nearly all branches of the family, presumably with the spread of plow agriculture.

Also, this cognate set probably reflects PIE \*perə-kói-to-s, consisting of \*perə- 'before, ahead of' (Meillet 1964:350), \*koi- 'lie, bed, beloved, dear' (Watkins 1985:27-28), and the passive nominalizer \*-to (Meillet 1964:269). The etymological meaning may have been something like 'premarital lover' or 'preferred sweetheart', and may have specified a class of preferred potential spouses (like the Siriono yande class, and similar ones in many kin based societies).

Typically, simple unstratified societies are very free about sexual activity between members of 'sweetheart' classes, even in those cases where the parent's permission is required for marriage. However, those with plow cultivation demand virginity (due to the importance of controlling inheritance by arranging marriages).

Hence, a shift from 'sweetheart' to 'concubine' or even 'prostitute' is only to be expected when free love is severely repressed with the advent of plow cultivation, and is a plausible parallel development in the societies for which it's attested. However, the meanings in Homeric Greek retain more of the original connotations.

In conclusion, by Goody's tests lexically reconstructed PIE society did not have any of the characteristics of a society with plow agriculture.<sup>12</sup> One additional test for social stratification is whether a

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<sup>12</sup> Early textual evidence cited by Friedrich (1966:20) suggests bride-wealth and polygyny in some early Indo-European societies. If these survived into early Classical times, they must have been present earlier. Certainly they

people has the concept of 'punishment', implying that one person has power over another and can compel obedience.

In simpler egalitarian societies, the wronged may seek revenge, but neither they nor anyone else has the power to 'punish'. In fact, there IS a widespread IE word for 'punishment' in most of Europe, but it turns out to be a patent loan from Doric Greek via Latin (Meillet 1964:378).

Real cognates of the Greek word do exist, permitting the reconstruction of PIE \*kwoinǵ, but the meaning of this word was 'compensation for damage'. Evidently if one harmed someone in PIE times, it was prudent to compensate the victim's family and so avoid revenge. However, the fact that the IE word for 'punishment' was borrowed from the Romans shows that concept of 'punishment' came many centuries later (from a stratified society with plow agriculture).

## 2.4. PIE Widows.

Recognition of the PIE social type leads one to discard etymologies such as PIE \*wydh(e)waH- 'widow', supposedly from \*wydh- 'to be empty, inadequate', which Friedrich (1966:9) describes as "widely accepted and inherently reasonable". One can also discount the idea that, because the term for a widow survives better than the one for a widower, "widows may have been marked for some

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would be most unlikely to have originated in the plow cultivation societies in which they're attested.

mortuary ritual; sacrifice and interment with the husband".

The difference in the survival rates of these terms could be a matter of chance, or a sign that widowhood was more important or long-lasting in the case of a woman than in that of a man. However, the differential survival takes place in post-PIE times, and so survival rates tell us about conditions in post-PIE societies (which developed plow cultivation), not the conditions in the PIE protosociety itself.

An etymology for 'widow' suggested by Watkins (1985:74), 'woman separated from her husband' (\*weidh- 'divide, separate'), is much more plausible. It's also interesting in that it doesn't distinguish between a woman separated from her husband by death versus by any other cause. The later restriction of the term to one whose marriage ended by death presumably reflected the shift to plow cultivation, where dowries made divorce nearly impossible.

# Chapter 3: A New Reconstruction of PIE Society

Chronological relationships among early IE institutions and events are important, and in order to keep the temporal relations as clear as possible, the main Early IE institutions are here presented, for the most part, in the likely chronological sequence of their origins.

## 3.1. MCCM.

Two lines of reasoning lead to the reconstruction of MCCM.<sup>13</sup>

First, there's a term for a class of wife givers to one's family characteristic of that type of marriage: PIE \*awos 'grandfather; mother's brother'.<sup>14</sup> Second, PIE \*swek'rwHs 'woman's mother-in-law' begins in reflexive \*sew-/\*swe- 'own' (Meillet 1964:337), which in PIE always refers to a woman's own self or to close consanguineal kin. Cross culturally, a father's sister is the ONLY close consanguine ('own' kin) who's ever prescribed as a mother-in-law. Since with MCCM a woman marries her father's sister's son, the equation of a

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<sup>13</sup> Another relic of PIE consanguineal marriage may be Old Norwegian mágr 'male affine' beside cognate Old German mâc 'blood relative' (Bjerke 1969:48, 59), suggesting an erstwhile equation of affinal and blood relatives.

<sup>14</sup> Benveniste (1969:226) argues that Latins considered the main referent of auus to be the father's father, and attached no importance at all to a mother's father. Hence, he suggests that auunculus 'mother's brother' can't be extended from 'mother's father'. However, the extension took place in PIE society, thousands of years earlier and in a vastly different type of society. The opinions of the Latins only reflect their own times.

father's sister with a woman's mother-in-law always implies MCCM.

Apparent counterexamples, the use of *\*sew-*/*\*swe-* for a woman's father-in-law in PIE, and for a variety of affines in some of the daughter languages, attest secondary developments. Benveniste (1969:249-251) points out that *\*swek'wros* 'woman's father-in-law' was derivationally secondary from *\*swek'rwHs* 'woman's mother-in-law' - and indeed that it was renewed from it in some of the daughter languages, showing that it sometimes remained secondary. He suggests that this is because her mother-in-law would have been more immediately important to her than her father-in-law.

This is quite plausible, as there is reserve and even avoidance between a daughter-in-law and her father-in-law in many societies. MCCM provides an additional reason for the lesser importance of a woman's father-in-law, at least in PIE society: he was a non-consanguine related to her mainly through his wife, who was her father's sister.

When post-PIE people began to marry unrelated persons, in some of the IE languages both parent-in-law terms continued to be used despite beginning in *\*swe-*. In that context, *\*swe-* could be reinterpreted as simply meaning 'in-law', and be extended to other affinal terms (see Benveniste 1969:330).

### 3.2. Superior PIE Wife Givers and pre-PIE Husband Givers.

There are three clues that wife givers were superior to wife takers in PIE society. Two of them also show that this reversed an earlier superiority of husband givers in late pre-PIE society.

First, Lehmann (1995:250, citing Walde and Pokorny) suggests that PIE \*awos 'grandfather; mother's brother' may be from demonstrative \*awo- 'that one'.<sup>15</sup> The use of a demonstrative pronoun for close kin is commonly found in societies where 'respect' relations produce a reluctance to use the referent's name or even call him by a kin term. That is, it's a sign of avoidance of the sort one might expect if the kin types in question were senior members of a class of PIE wife givers, and that wife givers were superior.

Second, one of the words used for a dominant husband in some late IE societies was \*potis 'lord, master, powerful one' (Friedrich 1966:19). PIE \*nepotis 'sister's son, grandchild' is sometimes analyzed as containing the prefix \*ne- 'negative' plus \*potis, and etymologized as 'non-powerful' (Friedrich 1966:24 and 26, quoting Trubachev).<sup>16</sup>

In a society with MCCM, a man's sister's son is his potential

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<sup>15</sup> Generally, it is rare for classificatory kin terms to be used of coresidents, who are generally intimate kin whose types are specified individually. However, in the case of one based on a demonstrative pronoun and signaling avoidance, intimacy is unlikely. Such persons likely were avoided precisely because they were in the immediate environment, and indeed could be pointed to (hence the demonstrative pronoun).

<sup>16</sup> The ending of \*nepotis has generally lost or reshaped but was probably \*-is, as suggested by Lithuanian nepotis.



son-in-law. The 'non-powerful' condition of a potential son-in-law suggests the superiority of PIE wife givers (particularly senior ones).

The choice of the term is still somewhat unexpected, however, unless a man's sister's son had previously been called \*potis 'lord, master, powerful one' not only by his bride but by her whole family, including her father. If so, however, pre-PIE \*potis 'lord, master, powerful one' would have been the general term for a 'husband given' to the man's family, not just the wife's term for her own husband. It would have implied the superiority of pre-PIE husband givers, even junior ones like the husband himself.

Third, etymologically PIE \*swek'rwHs 'woman's mother-in-law' meant 'own mistress' (Friedrich 1966:20). It begins in reflexive \*sew-/\*swe- 'own' (Meillet 1964:337) followed by \*k'wrH- 'master, mistress, powerful one', which undergoes taboo deformation by metathesis of \*wr in this word (Benveniste 1969:249-250, cf. Friedrich 1966:11). Taboo deformation negates the literal meaning of the word, in the present case the powerfulness of the woman's mother-in-law, much as the prefix \*ne- 'negative' negates the power of the \*nepotis 'sister's son, grandchild'. Thus, husband givers were no longer powerful in PIE times, presumably because power had shifted to wife givers.

However, before the taboo deformation, back in pre-PIE times when a 'husband given' was a 'lord,' his mother would indeed have been powerful. This too implies the superiority of pre-PIE husband

givers.<sup>17</sup>

The taboo deformation which applied to \*swek'rwHs 'woman's mother-in-law' did not apply to \*swek'wros 'woman's father-in-law' - implying that he was still a powerful relative in PIE times though his wife had ceased to be - probably because of his role in PIE affinal alliances (see below).

In conclusion, wife givers (e.g., a man's father-in-law) and the wife herself were evidently superior to wife takers in PIE society, but this reversed a pre-PIE state of affairs where husband givers (e.g., a woman's mother-in-law), and the husband himself (a man's son-in-law) had been superior.

### 3.2.1. Connubium.

When two lineages or clans join in a marriage alliance, one is sometimes perceived as benefiting more than the other, and in return, is expected to acknowledge the superiority of the other. Usually, the superior group is the one providing the more valuable spouse, especially if that spouse relocates.

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<sup>17</sup> PIE society didn't limit \*genHr to a son-in-law. It applied it at least to a sister's husband, producing the well-known equation: daughter's husband = sister's husband. Both these men were 'husbands given'. If the early Indo-Europeans were as diplomatic as are most other societies, this equation probably originated in pre-PIE times, when husband givers and the husband himself were superior. Generally, people are more likely to acknowledge classes of superior people, than to draw attention to membership in inferior ones.

Where an unequal alliance is continued through several generations, the inequality tends to be intensified. Three kinds of inequality are commonly found:

(1) In many societies with husband centered residence, in which women are mainly responsible for horticultural production, the husband and his group are considered beneficiaries who should defer to their benefactors. Here, wife givers are said to be superior to wife takers.

(2) In a society with wife centered residence, which is waging intensive warfare, and where high male mortality makes men scarce, it is instead husband givers who are superior to husband takers, as long as men have a variety of good marriage choices.

(3) However, in a society of type 2, parallel brothers usually find it a great advantage to marry into the same families (sibling set marriage), and thus to stay together after marriage and fight side by side. Usually, they also pick closely allied men (mother's brothers) as fathers-in-law. When this effectively limits men's marital choices to one lineage, this is known as connubium (co-marriage).

Connubium puts wife givers in the superior bargaining position, since the petitioner for a wife has no realistic alternatives. He must be grateful for his wife, and acknowledge the superiority of her group. In addition, his dominant father-in-law is also his most senior local consanguine, and the head of his household or kin cluster.

In a society like pre-PIE, connubium would likely provoke a shift from the superiority of husband givers to the superiority of wife givers, and this is what I reconstruct as having taken place shortly before PIE times.

### 3.3. The High Status of Women.

PIE \*sweso:r 'sister' was a high prestige term meaning 'own adult woman', consisting of a prefix \*swe- 'own' plus \*so:r 'woman'. Cross culturally, such terms are applied by men to their sisters in societies where (1) hunting is reduced to below 35% of the subsistence economy, (2) internal warfare is absent, and (3) the female contribution to the subsistence economy has reached about 30-45%, always or almost always due to horticulture or shifting agriculture (Proulx 2005b:section 11.3).

It is true that Bantu evidence suggests (4) a negative correlation between high status cross sex sibling terms and an economy where herding milk cows is the main pillar of the subsistence economy.

In particular, among the Bantu horticulturalists who developed high status terms for a man's sister, animal husbandry existed but was generally limited to a few goats and perhaps a few sheep. In sharp contrast, the largely patrilineal areas of southern and eastern Africa, where the economy was based upon herds of dairy cows, produced not a single example of such a term.

Had herding had the place in the lives of the pre-PIE peoples

that it did for the southern and eastern Bantus, it doesn't seem likely that they would have innovated the prestigious 'man's sister' term they did.

However, in temperate Europe, cows could not be kept over the winter unless adequate amounts of vegetable foods had been set aside to keep them till spring. Since the production of this hay, straw, and grain was a horticultural activity, herding milk cows could never independently be the main pillar of the subsistence economy. It remained dependent upon horticulture, and the women who provided the fodder shared the credit for the herds.

Presumably, the pre-PIE peoples who innovated \*sweso:r 'man's sister' were farmers who kept some cows and small animals, but whose dairy herds never overshadowed horticulture in importance, as was the case for the eastern and southern Bantus.

This term wasn't old enough in PIE times to be opaque, nor very recent either, since \*so:r 'woman' was evidently archaic in this and a few other terms, notably pre-Latin \*wkso:r 'wife' (Friedrich 1966:20).<sup>18</sup> Therefore PIE \*sweso:r 'man's sister' suggests a sudden promotion of women to very high status, shortly before PIE times.

If PIE \*sweso:r was originally a man's term, one may ask what a woman called her sister. Every kin term ending in \*-Hte:r but one was used for a member of the nuclear family (\*maHte:r 'mother',

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<sup>18</sup> The best-attested PIE word for 'woman' is \*gwena: (Friedrich 1966:18), from which we get English queen. It too was evidently a high prestige term.

\*pHte:r 'father', \*bhraHte:r 'brother', \*dhwgHte:r 'daughter'), and every member of the nuclear family but two was called by a kin term ending in \*-Hte:r.

The exceptional kin term ending in \*-Hte:r is \*ynHte:r 'husband's brother's wife'. However, the reconstruction of MCCM with connubium for PIE society means that the adult sister of a married woman was expected to be her husband's brother's wife. That is, a PIE woman normally called her adult sister \*ynHte:r 'woman's sister = husband's brother's wife'.

Presumably, the equation 'woman's sister = husband's brother's wife' continued as long as this type of marriage remained the rule. Only after PIE times, with the replacement of MCCM by general marriage, were two separate terms required. At this time, women evidently adopted the male term \*sweso:r for their sisters, leaving only the meaning 'husband's brother's wife' for \*ynHte:r.<sup>19</sup>

Finally, MCCM explains why there is no separate term for a wife's sister's husband (pace Friedrich 1966:10): it's presumably simply because he was called \*bhraHte:r 'brother'. \*bhraHte:r never means 'wife's sister's husband' in any daughter language because (1) no Indo-European society coined a high status term for a woman's brother to compete with it as a 'brother' term, and (2) none

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<sup>19</sup> The reason we traditionally assign the meaning 'husband's brother's wife' to \*ynHte:r is presumably because that is its meaning in the daughter languages where it survives. However, this meaning accurately reflects only post-PIE times.

preserved connubial MCCM.

Moreover, most descendant societies had husband centered residence, so a wife's sister's husband would typically be a non-coresident stranger of no immediate interest.

### 3.4. The Composition of Households.

#### 3.4.1. Wife Centered Residence.

The PIE kin terms lacking clear etymologies generally fall into two divisions: (1) thematic stems ending in productive *\*-s*, versus (2) non-thematic ones ending in *\*-r*, including the subset ending in *\*-Hte:r*. Since the most frequently used kin terms are more likely to have retained older morphology, and infrequently used ones to have adopted productive morphology, one should expect terms for the closest kin types to be non-thematic, and those for more distant kin to be thematic.

In fact, in all cases but four, non-thematic stems are used for kin types who ideally might have formed a person's household in a society with wife centered residence and MCCM. Thematic stems are used for those kin prototypically living outside it (see table 3.1). Since this hypothesis accounts for 16 out of 20 PIE terms, it's strongly supported ( $p = 0.0115$ , Fisher's Exact Test). In addition, it proves easy to account for the remaining four terms.

	Non-Thematic Stems in *-r̥ (10)	Thematic Stems in *-s̥ (10)
Household Residents	<p>*<u>maHte:r</u> 'mother'</p> <p>*<u>pHte:r</u> 'father'</p> <p>*<u>ynHte:r</u> 'woman's sister; husband's brother's wife'</p> <p>*<u>bhraHte:r</u> 'brother'</p> <p>*<u>dhwgHte:r</u> 'daughter'</p> <p>*<u>ġenHr</u> 'daughter's husband; sister's husband'</p> <p>*<u>daHywe:r</u> 'husband's brother'</p> <p>*<u>wkso:r</u> 'man's wife'</p>	<p>*<u>newystha:</u> 'brother or son's wife' (?)</p> <p>*<u>potis</u> 'husband'</p>
Outside Household	<p>*<u>sweso:r</u> 'man's sister'</p> <p>*<u>syVHr</u> 'wife's brother, his family'</p>	<p>*<u>snwsós</u> 'son's wife'</p> <p>*<u>swHnws</u> 'son'</p> <p>*<u>swek'wrHs</u> 'husband's mother'</p> <p>*<u>swek'wrs</u> 'husband's or spouse's father'</p> <p>*<u>ġHlows</u> 'husband's sister'</p> <p>*<u>nepotis</u> 'man's sister's son'</p> <p>*<u>hanəs</u> 'grandmother'</p> <p>*<u>awos</u> 'grandfather, mother's brother'</p>

Table 3.1. PIE Residence in Small Wife Centered Households (P = 0.0115, Fisher's Exact Test).

Three of the four exceptional kin terms are possessed forms of general nouns, that have evidently retained the morphological characteristics of their sources. PIE \*sweso:r 'man's sister' is derived from \*so:r 'woman'. The basic meaning of PIE \*potis was 'lord,



master', and only secondarily 'husband given to one's family'.<sup>20</sup>

Finally, Proto-Slavic \*nevěstka 'brother or son's wife' is derived from \*nevěsta 'bride or virgin' - which implies that the same is true of its antecedent, PIE \*newystha: 'brother or son's wife', if this is indeed a genuine PIE reconstruction (see Friedrich 1966:13 for discussion).

The remaining exception is \*syVHr, usually glossed 'wife's brother', which Friedrich (1966:17) describes as "the single affinal term that implies a linking wife rather than a linking husband." It is attested only in Slavic and Indic, where it also denotes a variety of cogenerational and junior kin types (female and male, consanguines and affines) related to a man through his wife.

If residence was wife centered and words ending in \*-r referred to one's household coresidents, as I suppose, \*syVHr could have been a generic term used by a man for his wife's kin who were irregular residents in her household.

I suppose that the person most likely to reside irregularly in this way would be a brother of the set of sisters who formed a household's residential core. Under a variety of circumstances, a brother might sometimes end up irregularly remaining in or returning to his natal home, persuading his wife to join him there, and having children there. All of these irregular residents may have

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<sup>20</sup> If \*men- too was a PIE term for a husband, its ending may also have been \*-r (compare Old Norse mannr 'man', Watkins 1985:39).

been called \*syVHr.

Residence by her brother in a PIE woman's household may have been fairly common, as it was his natal home. However, once husband centered residence became the norm in late post-PIE times, it would presumably have been unusual for a man to live in his sister's husband's household, which would explain the loss of \*syVHr from most branches of IE before it could be recorded.<sup>21</sup>

I conclude that, aside from the 3 terms which were possessed forms of general nouns, the division of PIE kin terms into two sets reflected ideal coresidence (non-thematic) versus non-coresidence (thematic) in a household with wife centered residence, in a society with MCCM. However, it's possible to be more precise.

In a household with wife centered residence, a daughter and her parents are coresident before and after marriage, and so are a set of sisters; in a society with MCCM and connubium, so ideally are a set of brothers. As a result, for a person of at least one sex, \*-Hte:r meant 'normally coresident with me before and after marriage' and referred to consanguines.

A daughter's husband, a sister's husband, a husband's brother, and a wife and her irregularly coresident brother, were all coresident affines after the referent's marriage, or one's own. Consequently, in

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<sup>21</sup> There is about 1500 years between the onset of plow agriculture, which Goody (1976:24) puts at around 2000 B.C., and the dates attributed to early Latin and classical Sanskrit (500 B.C.). In 1500 years, lexical attrition can remove many infrequent words from a vocabulary.

all kin terms \*-r implied coresidence in the same household after marriage. The term for an irregularly coresident brother presumably didn't end in \*-Hte:r because the postmarital coresidence was not a prescribed one, and perhaps regarded as temporary. The terms ending in \*-s all referred to persons normally living outside the possessor's household after marriage.

### 3.4.2. Household Membership and Size.

A \*pHte:rwos 'father's brother', like all members of ascending generations other than parents, evidently resided outside one's household since he was called by a term ending in productive \*-s. However, one's own \*bhraHte:r 'brother' remained a coresident with a man after marriage. Both things are only be likely to be true if a couple at first remained in the wife's parents' household for a few years after marriage, but then typically moved out and built their own dwelling.

The likeliest time for a family to build its own dwelling would be either when their oldest child was mature enough to help care for junior siblings (giving them more independence), or when their eldest daughter married, and the family acquired a son-in-law. That would usually be about 12-20 years after a couple married, that is, when they were perhaps 32-40 years old.

The former is perhaps the better guess: linguistic usage, including that of kin terms, tends to become fixed at about age 11,

before which words are easily learned but easily forgotten. Hence, \*pHte:rwos 'father's brother' in \*-s suggests that by age 11 most children were no longer coresident in the same household with their father's brother, nor with their grandparents. Also, people probably LIKE the independence of their own household, and set it up as soon as they are able.

Either way, just before its division, the pre-PIE household would have typically consisted of a woman and her husband, their daughters and sons-in-law below middle age, their young grandchildren by coresident daughters, and sometimes an irregularly coresident brother and his conjugal family.

Household division would have cut a PIE lifetime into three significant periods: childhood (in a grandparent's household), young adulthood (in a parent's household), and maturity (as head of one's own household).

### 3.4.3. Community In-Marriage.

Grandparents, though living outside the household, were evidently normally members of a young adult's local community. \*hanəs 'grandmother' means 'old woman',<sup>22</sup> and unmodified general

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<sup>22</sup>Friedrich (1966:5-6) reconstructs \*han- for the cognate set: Hittite hannaš 'grandmother', the reciprocal diminutive Common Slavic vUno kU 'grandchild' and Old High German eninchili 'little grandson', and Lithuanian anytas 'husband's mother'. To these we can add the reciprocal diminutive Lithuanian anukas 'grandson', and Greek annís, Armenian han, and Old High

nouns are only likely to become genuine kin terms when used of persons residing in close propinquity, whom that propinquity helps identify.

Also, Lehmann (1995:250, citing Walde and Pokorny) suggests that \*awos 'grandfather, mother's brother' may have been derived from \*awo- 'that one'. The use of a demonstrative pronoun implies that its referent or his home typically must have been within line of sight, i.e., in the same \*weikos 'village, hamlet, or neighborhood'. In a society with MCCM, the propinquity of a mother's brother is consistent with local in-marriage (local endogamy).

Smith (1995:103) illustrates a cluster of archaeologically reconstructed early European hamlets of the right time period to have resembled pre-PIE ones. They vary in composition from two to several dozen longhouses, and are internally subdivided into neighborhood house clusters. Close kin (a \*pHte:rwos 'father's brother', \*hanəs 'grandmother, mother's mother', and \*awos 'grandfather, mother's father') probably lived in the same house clusters. Affinal kin (a mother's brother, husband's sister, etc.) may have lived in another house cluster in the same community, or possibly in another community.

#### 3.4.4. Post-PIE Premarital Avunculocality.

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German ana 'grandmother' (Benveniste 1969:224, 234). Etymologically, the underlying term means 'old woman' (cf. Latin ānūs 'old woman'), suggesting respect and seniority.

PIE mother's brothers and sister's sons called one another by thematic kin terms (\*awos 'grandfather; mother's brother', \*nepotis 'sister's son, grandchild'), suggesting that they didn't usually live in the same households. Only after the marriage of a PIE sister's son to his mother's brother's daughter did reference to him become non-thematic, suggesting household coresidence (\*genHr 'daughter's husband, sister's husband').

However, the main innovated Indo-Iranian term for a son-in-law (Sanskrit jāmātar-, Avestan zāmātar-) ends in \*-Hte:r (Benveniste 1969:256), added to the root \*gem- 'bind'. This suggests that in these societies, a man and his father-in-law were coresident before and after marriage.

Also, Indo-Iranian substituted \*nepter for \*nepotis 'sister's son, grandson', showing a post-PIE substitution of \*-Vs by \*-r 'coresident in the same household after marriage' (or, with haplology, \*-Hte:r 'coresident before and after marriage'). This suggests that in these societies, a man and his mother's brother were coresident after marriage, and possibly before.

This implies avunculocality and, since a man also resided with his father-in-law after marriage, it implies continued MCCM. In fact, cross cousin marriage of some sort is attested ethnographically for ancient India (Gates 1971:43). Finally, since a man was probably also coresident with his father-in-law before marriage, the avunculocality was likely of the premarital variety.

Later, Indo-Iranian \*nepter was also extended to a brother's son (see Benveniste 1969:265), classifying him as a coresident after marriage. This implies that a set of brothers now remained coresident even after their own sons began to marry, that is, through all three significant life periods. This reflects husband centered residence, and larger coresidential units than when its reciprocal, PIE \*pHte:rwos 'father's brother', classified its referent as a non-coresident.

At the western edge of the expanded IE territory, partly similar developments are found in some Celtic languages. Gallic ewythr and Breton eontr 'mother's brother' show a similar post-PIE substitution of \*-Hte:r 'coresident before and after marriage' for \*-Vs in \*awos 'mother's brother, grandfather' (Benveniste 1969:256). This too signals premarital avunculocality. However, here there is no evidence that MCCM continued.

Evidently, premarital avunculocality developed at the eastern and western margins of the main expanded PIE domains, among peoples who intruded into fully occupied territories at the end of long distance migrations. Presumably, it was this vanguard of the IE expansion that met the greatest resistance, and had the highest male mortality. Evidently, these conditions elicited premarital avunculocal residence, which would have made the mother's brother even more powerful than in PIE times.

In contrast with the edges of IE territory, the homeland area

presumably became relatively secure as the expansion proceeded, and late developments associated with high male mortality never took place there. Consequently, there was no addition of *\*-Hte:r* 'coresident before and after marriage' to terms for fathers- and sons-in-law there, as one can see from thematic Homeric Greek pentherós 'wife's father, sister's husband, daughter's husband' (from *\*bhendherós* 'associate, ally'), and Greek gambrós 'sister's or daughter's husband'.

### 3.4.5. Old Norwegian Nuclear Families.

PIE *\*swHnws* 'son' was reshaped to Old Norwegian sonr (Bjerke 1969:55).<sup>23</sup> This could reflect one of two things. First, it could reflect husband centered residence. Second, over the millennia, blood relationship came to overshadow residence in determining significant IE social groups, giving rise to the folk saying "blood is thicker than water." Possibly, the *\*-r* which referred to coresidents in PIE times came to signal membership in one's nuclear family in Old Norwegian.

This development shows clearly that athematic stems are not all older than thematic ones, though most surely are. Rather, in kinship terms at least, the contrast between thematic and athematic stems is often used to signal membership in kin classes.

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<sup>23</sup> Watkins (1985) also cites Old Norse mannr 'man' (IE terms for 'man' are commonly used for 'husband').



### 3.5. Indo-European Alliance and PIE Wife Centered Residence.

IE languages formed kin terms of alliance from a variety of verb stems meaning 'bind' and 'link'. In a given language, when a binder was of one sex, a link was always of the other. In the cases where postmarital residence is known, a binder evidently was expected to stay put, while a link normally relocated.

In attested Indo-European languages, spoken in societies with husband centered residence, typically the binders were male in-laws (especially a wife's father and a daughter's husband). Meillet (1964:391) and Friedrich (1966:11-13) give several examples: Homeric Greek pentherós 'wife's father, sister's husband, daughter's husband' (from \*bhendherós 'associate, ally': Lithuanian benâdras 'associate'), Vedic Sanskrit sam/bándhin 'wife's father' (root \*bhendh- 'bind, attach'), and Greek gambrós 'sister's or daughter's husband', Sanskrit jā mātār 'son-in-law', Avestan zā mātār 'son-in-law' and za:maoya 'daughter's husband's brother' (all from \*ǵem- 'bind'). The nominalizer in the Greek words is the same ending found in \*swek'wros, namely agentive \*-(e)ró (see Meillet 1964:267). A daughter (presumably relocating as a bride) was sometimes said to link the two families, e.g., Welsh geneth 'daughter' (\*ǵenH- 'link').

In contrast, the junior binder of a PIE alliance was clearly a daughter-in-law, since PIE \*snwsós 'daughter-in-law' is derived from \*snew- 'tie, bind' plus agentive \*-esó (see Meillet 1964:260). The

link was a son-in-law since PIE \*ǵenHr 'son-in-law' is derived from \*ǵenH- 'link'.<sup>24</sup>

Clearly, in productive formations the sexual alignment of junior binders and links reversed between PIE times and when the IE languages were recorded.<sup>25</sup> This presumably reflects the change from wife centered to husband centered residence which had occurred.<sup>26</sup>

### 3.6. Matrilineal Descent.

We have seen that herding was present well before PIE times, implying an important shifting agriculture base, so PIE population densities were likely high enough to support unilineal organization. Also, MCCM is suggestive of unilineages of some sort, since the

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<sup>24</sup> Besides 'link', \*ǵenH- also means 'to give birth, beget; with derivatives referring to aspects and results of procreation and to familial and tribal groups' (Watkins 1985:19). For example, there is Latin gens 'a gens, a people, a nation; offspring'. In PIE, the two meanings of \*ǵenH-, 'link' and 'bring forth', are presumably related in the sense that people are linked by their offspring.

<sup>25</sup> The reversal in the sex of the junior binder and link may possibly have an echo in the sexual associations of the celestial luminaries. Evidently the moon (the lesser luminary) was seen as male by the Proto Indo Europeans, but female by the patrilineal patrilocal Greeks and Latins. See the article "Proto-Indo-European Sun Maidens and Gods of the Moon" (Robbins Dexter 1984: 137-144).

<sup>26</sup> There is one more line of reasoning that suggests wife centered residence for PIE society. One alternate PIE kin term for a son, if the cognate set be valid, transparently means 'lad, boy' (Friedrich 1966:7). Cross culturally, when a kinsman is known by a term appropriate for a child, it's generally because that kinsman isn't normally coresident with the referent as an adult (but was as a child).

World Ethnographic Sample (Murdock 1966:table 3) shows only 3 out of 49 societies with MCCM (6%) to be bilateral.

Friedrich (1966:21) compares Sanskrit janas, Greek genos, Latin gens, genus, Gothic kuni and reconstructs PIE \*ǵenHos 'patrilineal kinship group'. The gloss fits the descendant languages, but they were all spoken in patrilineal societies three or four thousand years after PIE times.

Etymologically, PIE \*ǵenHos is built from a verb root \*ǵenH- 'to give birth, beget; with derivatives referring to aspects and results of procreation and to familial and tribal groups' (Watkins 1985:19), for example, Latin gens 'a gens, a people, a nation; offspring'. These are obviously meanings in themselves compatible with any form of descent.

Nevertheless, it's still possible to infer the PIE descent type from the root of this term. First, in clear cases, \*ǵenHos refers to one's own descent group; never does it refer to an affinal kinship group. Second, Indo Europeans sometimes refer both to a class of relatives and to its most important or prototypical kin type using the same term, or a by-form of the same term.

For example, Old German had geswisterîden 'a set of siblings' from swester 'sister', and similarly Old Norwegian had systkin 'a set of siblings' from systir 'sister' (Bjerke 1969:45, 55).

Old German documents also show class meanings for several

terms, beside more traditional specific meanings inherited from PIE times. For example, neve 'sister's son' was extended to any 'contemporary or younger male relative', while yeter 'father's brother' was extended to any 'contemporary or older male relative in the male line' (Bjerke 1969:51).

This suggests that, since both terms share the root \*genH-, a PIE \*genHr 'son-in-law' was the focal member of one's \*genHos. A PIE son-in-law was a sister's son due to MCCM. In a matrilineal society, a man's sister's son is his direct matrilineal descendant, and thus a prototypical member of his descent group. In a matrilineal society with MCCM, PIE \*genHr 'son-in-law' and PIE \*genHos 'matrilineal kinship group' form a natural pair.<sup>27</sup>

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<sup>27</sup> There is no way that a sister's son could be the prototypical member of one's descent group in a patrilineal society with MCCM. He would inherit his patrilineal affiliation from his father, who would necessarily belong to a lineage distinct from that of his mother's brother. Somewhat similarly, in a bilateral society, cross cousin marriage divides families into non-overlapping categories (proper subsets) of close consanguines versus potential affines. Only the consanguines are important for calculating shared descent, not the whole kindred.

For example, when Dunning (1959:72-73) asked his bilateral, cross cousin marrying Northern Ojibwa informants who they were related to, they listed only members of their exogamous groups (parallel kindreds). "Non-kinsmen are all others in the society, including other cognates and affines, and are designated kawin-odinawaymase, literally, not related". Hence, although, in a bilateral society with MCCM, the son-in-law is a member of one's kindred, he isn't at all likely to be a prototypical member of one's focal descent group.



## Chapter 4: Some Familiar Hypotheses About PIE Society

It is nearly two centuries since the earliest attempts at reconstructing PIE society, and there have been many attempts at interpretation of the data. Several of these have long been dominant in the field, and indeed are still widely accepted as established truths, as a quote from a recent respected textbook makes clear:

"Proto-Indo-European society was patriarchal, patrilineal (descent through males) and patrilocal (bride went to live with husband's family). It was stratified: three levels of social stratification have been identified, namely a tribal king, nobles or warriors, and peasants and farmers" (Campbell 1999:341).

### 4.1. Three PIE social classes.

Three classes or castes are often identified for PIE society, said to have resulted from military conquest: a priestly class headed by a divine king (conquerors), a warrior class (conquerors), and peasants (conquered people). This is done mainly on the basis that most Indo European societies later had such classes or castes. However, these later societies in no way resembled the PIE one.

It is also argued that modern Indo Europeans tend to divide their governments three ways, such as into legislative, executive, and judicial branches, and that tripartite organization is a kind of

inherited IE ideology. If there is any truth in this, it need not have its origin in classes or castes.

PIE society had MCCM. A society with MCCM must have at least three distinct intermarrying groups: one's own, that of one's spouse givers, and that of one's spouse takers - all close enough at hand to be reliable allies. For alliance to have any value, joint action must be possible, and requires leadership. However, since the three groups are equal, each should have the leadership of something significant.

Speculatively, if each of the three allied PIE groups traditionally provided leadership in different domains (say, ritual, political, and military), this custom could have developed into the tripartite organization manifested by most of the later IE societies (e.g., see Benveniste 1969:258-259, 310).<sup>28</sup> No more elaborate explanation, such as castes resulting from conquests, is required.<sup>29</sup>

## 4.2. Traditional Arguments for PIE Patriarchy.

### 4.2.1. Husband Centered Residence.

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<sup>28</sup> Tripartite leadership, with or without MCCM, isn't unique to Indo-Europeans. For example, the Lenapeans and Lake Algonquians both had shamans, peace chiefs, and war chiefs.

<sup>29</sup> Judging by the tripartite division of the Dorian Greeks into wik- 'tribes', in PIE times each of the intermarrying groups of a \*teutā- 'people, population' may have been a \*weikos 'neighborhood, hamlet', occupied by a localized \*genHos 'matrilineal kin group', and composed of several \*domos 'households'.

FACT: There are dozens of ethnographic cases of patrilocality, and in every one of them a man has terms for his in-laws.

TRADITIONAL REASONING: PIE society did NOT have terms for a man's in-laws, so it must have been patrilocal.

The traditional reasoning predates most ethnography, to be sure. It has attained the status of a received idea, passed along for generations, untouched by critical thinking. Still, it is an idea accepted by many otherwise fine scholars for over a century. Just to be sure we are right, let us examine the evidence in some detail.

If there's one thing in the realm of PIE linguistic paleontology that nearly everyone has been agreed upon, it's that PIE society had husband centered residence. Moreover, all sources cite the same piece of evidence in support of it: a set of five reconstructed in-law terms glossed as relatives of a woman's husband, and a lack of corresponding terms for relatives of a man's wife (other than \*syVHr).

This is a textbook example of taking the absence of evidence for evidence of absence ("there are no reconstructible terms for a man's in-laws, therefore a man didn't have terms for his in-laws"). Admittedly 5 terms for a woman's in-laws and none for a man's isn't the product of random lexical replacement: it does show a pattern. However, there are alternative explanations much better than the one proposed.



(1) In many cases it's easier to reconstruct kin terms used by female speakers, compared to those used by male ones, for a simple reason. In the great majority of kin-based societies, especially those which have male-biased social organization like the late IE ones, most political matters are primarily male concerns. Since all politics are kinship politics, kinship terms serve men as do titles to political offices in state societies.

As political organization changes, the kin terms used between the men who have changing political ties may change too, and IT'S MEN WHO INNOVATE THE REPLACEMENT TERM. Women, who in such a society are less involved in the politics, may adopt the new term used by the men. However alternatively, they may for a time simply retain the old one.

In particular, since the main means by which men manipulate alliances in kin based societies is by marriage, in-law terms are those most likely to change when alliances change. Consequently, if one looks at the terms for in-laws in a language family whose speakers have experienced a lot of kin-based political change, male usage may show widespread innovations which postdate genetic unity. Predominantly female usage, in contrast, may be more conservative and allow the reconstruction of proto-terms.

We have seen that there was a great deal of organizational change between PIE times and the recording of IE kin terms in the daughter languages. There is thus nothing surprising about the

replacement of most of the in-law terms used by men. If this lack of reconstructible PIE terms for a wife's kin is meaningful at all, it most likely reflects widespread changes in kin-based politics in the post-PIE period.

(2) Most of the terms which replace the PIE ones for a man's male in-laws are terms describing them as 'allies'. Since there is a strong cross cultural tendency in kin based societies for men to call one another by the term which expresses the closest tie between them, ignoring more distant ones, this implies that in the societies that produced the terminological replacement (late post-PIE societies with plow agriculture), a man's male in-laws were neither his own blood kin nor his coresidents.

Had they been so, terms highlighting these closer relationships would generally have been used. So the etymologies of the new terms tell us that the emerging societies were out-marrying. Moreover, 'ally' is an inherently reciprocal term, implying an approximate equality between the in-laws despite generational differences. These were presumably negotiated alliances, where the benefits to the two parties were more or less in balance.

All this is in marked contrast with reconstructed PIE society, where wife centered residence put a man in his father-in-law's household, MCCM made his father-in-law his mother's brother, and the superiority of wife givers made the relationship very unequal. With change of this magnitude in the affinal relationships among

men, it is in no way surprising that male in-law terms changed in language after language, nor even that the change was largely of the same kind.

In reality, PIE society surely did have terms for a wife's relatives, since all attested societies do. They are not reconstructible because they were widely replaced in post-PIE times, leaving too few cognates for reconstruction.<sup>30</sup>

(3) The classical argument is that a man living in husband centered residence rarely saw or talked about his distant in-laws, while his wife was in daily contact with hers. A woman, so the claim goes, had terms for her in-laws because she lived with them; a man didn't have terms for most of his because he lived with his own blood relatives. This is considered self-evident by everyone but Saussure (1984:479).

But consider the implications. Do children in modern societies, living in communities at some distance from their 'grandmother' or 'aunt', forget the kin terms for them? If memory is not that poor even in literate state societies, how could it have been so in an oral kin based one? However, since the classical argument has been so widely believed, let's look at it more closely. What exactly would it mean to not have terms for a man's in-laws?

If what is meant is only that a man's in-laws were known by

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<sup>30</sup> Exceptionally, Gates (1971:42-43) recognized that PIE did have such terms, and reconstructs them as extensions of others.

extensions of terms for closer kin, i.e., had no special kin terms of their own, that should not prevent reconstruction. Some classificatory kin terms are in fact easily reconstructible for PIE, with some languages preserving the term for one kin type and some for another. In principle, in-law terms would still be reconstructible if they had been classificatory ones.

If alternatively one insists on a genuine absence of kin terms for a wife's relatives in PIE, and this is taken to reflect the "slenderness of the marriage tie and the lack of obligations to these kin", such a state of affairs is unknown synchronically. Indeed, only traditional Nayar kinship terminology provides a reasonably good analog of it, or more precisely of its mirror image (Gough 1962:382).

The traditional Nayars were a matrilineal people of southern India, who had consanguineal family units consisting of siblings and their uterine children. Women had a ritual husband who played little or no role in their lives, and had children by a visiting or live-in barta:vu. There were no kin terms for the relatives of a barta:vu.

The usual English gloss of barta:vu, 'husband', probably tells us more about ethnographic culture bias than it does about the Nayars. If one makes the assumption that a woman can't live without a husband, and one then finds a society where women don't have one (other than a ritual husband), one tends to try to find some man to fit into the slot.

However, it seems to me that the prior ritual marriage, and the

absence of kin terms for the barta:vu's relatives, signal quite clearly that he WASN'T a husband. At most the barta:vu was a concubine; often, just a long term visiting lover. Moreover, in modern times Nayar men "have assumed rights in and obligations to their children" (Gough 1962:383), and "terms formerly restricted to matrilineal kin are today sometimes used to address corresponding patrilineal kin." As the barta:vu takes on the roles of a husband, kin terms for his relatives appear.

Is it possible that PIE men didn't have genuine wives, only concubines or lovers? Not likely concubines: concubines are found in societies like the Nayar one with plow cultivation, but not in those which like PIE society had shifting cultivation. In shifting cultivation societies, secondary marriages are always recognized as marriages (see Goody 1976:17, 52), and affinal terms are used even for the relatives of secondary wives.

If PIE men had had only lovers, they would have had no claim on the children they sired. Their descendants would have to have been their sisters' children, just as in the Nayar case. However, the Nayar pattern of in-law kin terms is the opposite of the one alleged for PIE: the Nayars lack terms for a MALE lover's kin, not for a FEMALE lover's kin.

(4) I know of no society in the world in which there are no terms for, say, a married man's father-in-law, despite the high frequency of husband centered residence worldwide. In suggesting

analogies with attested terminologies, Friedrich (1966:23, 5) mentions "Classical Mongol, and a few other Central Asiatic Omaha societies", and "the Kazak and other semi-pastoral groups in Southwest Asia".

However, Dole (1965:51) suggests that Descriptive terminologies (Sudanese, or, to use her term, Lineage) often evolve out of Omaha ones, and specifically says that "the Kazak, Masai, and several Mongol groups" which she has classified as Descriptive "clearly have vestiges of Omaha terminology". In other words, if she's right, Friedrich's examples are societies in transition from Omaha to Descriptive systems. Descriptive terms typically are used in societies where, due to the importance of inheritance or of factionalism, some finer distinctions of kin type need to be recognized.

The first kin types in these Omaha systems which switch to Descriptive terminology may in some cases be those for more distant or noncoresident kin, the higher frequency terms being the last to change. Misunderstanding may result when an ethnographer finds Descriptive terms for less salient relatives used alongside Omaha terms for more salient ones. This misunderstanding is increased if the natives state that the less salient ones aren't relatives at all (as several do, see Dole 1965:44).

However, an example from Northern Ojibwa may help make clear why they do this. When Dunning (1959:72-73) asked his

informants who they were related to, they listed only members of their exogamous groups (parallel kindreds). "Non-kinsmen are all others in the society, including other cognates and affines, and are designated kawin-odinawaymase, literally, not related". Yet, the Northern Ojibwas do have kin terms for all these "non-kinsmen", some of which Dunning cites.

Mongols and Kazaks may describe relatives outside their patrilineages as "non-kinsmen" and call many or all of them by Descriptive terms. However, they assuredly do have terms for them, and we can be quite sure that PIE men had terms for their in-laws too.

If alternatively what is meant when it is said that PIE men had no terms for in-laws, is only that the in-laws of men were of low salience (unimportant), and thus that the terms for them were apt to undergo more frequent lexical replacement, notice that any change which occurred in or prior to PIE times would have been shared, and thus undetectable in any reconstruction of PIE.

It's in the descendant societies, not PIE society itself, that a random lexical replacement preventing reconstruction would have to have taken place. Hence, it's in these daughter societies that the terms would have to have been unimportant, if that were the reason why we couldn't reconstruct them. This would tell us nothing about PIE society.

Even for those post-PIE societies where the random lexical

replacement would have to have taken place, moreover, low salience of a wife's kin (relative to a husband's) need not reflect a pattern of husband centered residence. Alternatively, it could reflect polygyny, where a man has several sets of wives' relatives, no one of which is of particular importance to kin politics. "For with plural marriage, alliance ... is likely to be more hesitant, more fragile, more temporary: a man is unlikely to get much help from a wife's brother unless the union with the sister has a relatively high probability of persisting, and unless it is also an individual one" (Goody 1976:62).

In contrast, a woman in a polygynous society has only one husband at a time, making his kin somewhat more important. Polygyny is a reasonable expectation in a society with shifting cultivation, and even 1,000 years is quite sufficient a time for random lexical replacement to make low salience terms unreconstructible.

Moreover, polygyny would explain better than residence why we can reconstruct \*awos 'mother's father, mother's brother' and its reciprocal, \*nepotis 'grandson, sister's son' (Friedrich 1966:24). In a society with husband centered residence, these affines would be every bit as non-coresident with a man as his wife's relatives. However, assuming polygyny, the relationships with his mother's and sister's relatives would be more stable than those with in-laws. A man's mother remains his mother, and his sister his sister, regardless of what marriages break up.

In conclusion, not only do we have a much better explanation for



the lack of reconstructed PIE kin terms for a man's in-laws, but the one usually given has multiple flaws. That it has been accepted so readily with so little scrutiny by so many for so long tells us nothing about PIE society, but it tells us a great deal about the values and beliefs of the societies that produced the scholarship.

Utilizing different data, widespread Indo-European idioms such as "leading a bride into marriage" have sometimes been seen as evidence of husband centered residence. Five branches of Indo-European specifically use *\*wedh-* 'lead' in this way. However, several languages use alternative verbs of the same meaning, e.g., Latin *ducere*. Evidently, the idiom arose in the context of marriage rituals suggestive of this meaning, which may well have spread over a large cultural area after the IE languages had diverged.

This is much like the widespread Algonquian idiom which associates European-type marriage with "standing together" - often using cognate verbs - though of course there's no question of a Proto-Algonquian date for the European-type marriage ceremonies being described. The Indo-European "lead = marry" idiom does perhaps reflect husband centered residence - and possibly male dominance - but there's absolutely no reason to believe that it predates plow agriculture.

Finally, a completely alternative approach to PIE residence is using unilineal descent as an index of it (see Ember and Ember and Pasternak 1974:70-71).

#### 4.2.2. Patriliney.

PIE society is usually said to have been patrilineal on the basis of the Omaha-like skewing found in its terminology. However, as pointed out by Szemerényi (1977, cited by Lehmann 1995:250), Friedrich's statistical arguments to the effect that Omaha skewing is a very good indicator of patrilineal descent are based on data from Murdock's "World Ethnographic Sample" (Friedrich 1966:27), and this uses cousin terms as its criterion for Omaha status. Omaha cousin terms aren't reconstructible for PIE, so the argument is invalid.

It might be argued that Omaha-like skewing of any sort is enough to signal patrilineages. However, this isn't the case, for several bilateral societies (like the Sirionos, Kayapos, and Pomos) have some instances of Omaha-like skewing.

It might be suggested that Omaha-like skewing is nevertheless much more frequently found in patrilineal societies than in bilateral ones. However, such statistical data reflects the situation over the last few centuries only, a period during which patrilineal organization has been very common.

We know that the population density of many parts of the world has greatly increased since 4,000 B.C., and Ember and Ember and Pasternak (1974:82) have shown that unilineal organization is rarely found when population density is lower than 5 persons/square mile.

Few if any forager societies have that high a population density, so few if any pre-agricultural societies had lineal organization. Hence, it's very unlikely that societies with patrilineages were nearly as common in PIE times as they later became. In principle, we can't even be sure any existed.

Friedrich says that "ancient Greece and India yield the best evidence on patriliney." However, these societies are among those which adopted plow cultivation the earliest, and are thus most likely to have diverged from the PIE prototype in the direction of emphasizing male succession, notably for the inheritance of arable land.

"For a man with a hammer, everything looks like a nail". There's usually little difficulty finding what appears to be evidence of whatever one believes or wants to believe. Subjected to careful scrutiny, however, such arguments fall apart. Although they may tell us a great deal about the society which produced and believed in such poor excuses for scholarship, they tell us nothing whatsoever about the prehistoric society they purport to describe.

In the present case, the traditional claims of PIE patrilocality and patriliney rest on nothing in the least plausible. They represent received ideas, originating at a time when scientific standards were not quite what they later became. Even at that, it is doubtful if they could ever have been accepted had they not fulfilled the emotional needs of some male scholars.

### 4.2.3. Patriarchy.

There is a broad consensus that PIE society was patriarchic. However, the only evidence ever mentioned is the patriarchy of the Classical civilizations, that came some three to four thousand years later.

In fact, if PIE society was matrilineal and had MCCM as I reconstruct it, most male power within the extended family would have been held by a man's mother's brother. Also, as a member of a wife giving family, and as its alliance binder, his daughter may have shared or inherited aspects of his status and power with respect to her husband's family (including the husband).

Beyond this, it is very difficult to identify any power in PIE society. It is uncertain that terms like "village head" date back to PIE times, and if they do, it is uncertain how the position was filled. A reasonable guess would be by heads of matrilineages. Certainly there is no reason to suspect the father of having had any great power.

### 4.3. The Kurgan Hypothesis.

One widely accepted hypothesis on IE origins<sup>31</sup> is that the IE people were the Kurgans, a "nomadic pastoralist people from the

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<sup>31</sup>As Lehmann (1995:270) notes it's "now widely identified with Gimbutas, but held by many earlier scholars".

southern Russian steppe" who invaded "Old Europe" in the period between 4500 and 2500 B.C. using light, spoked chariots drawn by horses, and "transforming social structure and religion from matrilinear to patrilinear and from matrifocal to patrifocal" (Gimbutas 1991:89-91).

Prior to these invasions, the "Old Europeans" would have been peaceful agriculturalists (presumably practicing shifting agriculture or horticulture, since plow agriculture wouldn't prevail for many centuries).

This theory is very popular. As Alinei (1998) puts it, "surprisingly, although the archaeological research of the last few decennia has provided more and more evidence that no large-scale invasion took place in Europe in the Calcholitic, Indoeuropean linguistics has stubbornly held to its strong invasionist assumption, and has continued to produce more and more variations on the old theme. Mallory, himself a supporter of Gimbutas' theory of the 'kurganisation' of Europe in Calcholitic (e.g. Gimbutas 1970), has reckoned that in the last hundred years no less than seventy theories of IE origins (and thus of invasion) have been published (Mallory 1989), of which Gimbutas' is the most popular among linguists, and Gamkrelidze & Ivanov's (1995 = 1984) the last of the series, suggesting an invasion from Armenia."

Without taking a position on these hypotheses, one can assess the implications of the linguistic evidence for them. The linguistic

evidence makes it clear that, of the two peoples sketched above, it must have been the "Old Europeans" who spoke the PIE language, not the Kurgans (unless they were another branch of the Indo-European family).<sup>32</sup>

PIE vocabulary contains within itself evidence of matrilocality, matriliney, and a high status for women.<sup>33</sup> This implies that the warring parties that devastated Europe either were all Old European people, or that the PIE people assimilated their invading conquerors.

The traditional idea that conquerors impose their language on subject peoples probably is based on the colonial model. Whether administered by modern European states or by Romans, colonies ruled from abroad are indeed likely to adopt the language of the "mother country" or Empire.

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<sup>32</sup> That is, the theory of linguistic continuity between Old Europe and IE is essentially right. This does not require that the linguistic differentiation of IE have begun in the Paleolithic as Alinei (1996, 1998) proposes (nor do I suppose it did). This is a separate question, whose answer in part may involve matters of PIE dialectology, in part questions of the rate and nature of lexical replacement.

Alinei recognizes the separateness of the two questions: "even assuming persistence of a basic IE unity throughout Neolithic (an untenable thesis, as I have shown in my book), there would be no need for a mass invasion of totally intrusive people to justify language differentiation. Aggressive expeditions of local elite groups, with subsequent territorial expansion and domination upon autochthonous societies, which became increasingly frequent in Chalcolithic and Bronze age, could have unleashed a series of waves of differentiations, without changing the basic ethnolinguistic picture of Europe" (Alinei 1998).

<sup>33</sup> Ironically, archaeological evidence is emerging that women may have had high status and served as warriors in some Kurgan societies (Osborne 2005). So reality is probably much more complex than is often assumed.

However, where the conquerors are relatively independent groups of mobile adventurers who maintain at most loose ties with the society from which they come, in at most a few generations they generally assimilate to the culture and language of the people they rule over. Examples are the Norman conquest of England, and the Manchu conquest of China.

In the present case, Anthony (1990:908) points out that in the area in eastern Hungary where the Yamna (Kurgan) migrators first overwhelmed the local culture, "the Yamna females display gracile Mediterranean morphological features that are commonly found among the earlier native population (males and females) and are rare among Yamna males, perhaps suggesting that male Yamna migrants took native wives." This would go a long ways towards explaining assimilation.

In any case, the linguistic assimilation of nomadic warriors by the more populous agriculturalists they conquer is only to be expected, and if Kurgans conquered Old Europe, they likely adopted not only its language but much of its culture, including its social organization.

Their main cultural contributions may have been military tactics and organization. However, the word for the horse likely spread through PIE dialects after they had begun to diverge (Lehmann 1995:271). This suggests only post-PIE acquaintance with the animal, and that the Kurgan's invasion postdates PIE times.

In contrast to the invasion hypothesis in all its variants is the notion that the warfare of the PIE period is mainly indigenous. Lehmann (1995:285) quotes Häusler sympathetically when he denies the invasion hypothesis, stating that "his portrayal of continuity in culture for Europe is also highly credible."

If Häusler is correct, it seems not unlikely that competition for scarce agricultural land or products developed during the cold period around 4000 B.C. (see Adams and Otte 1999). Evidently this led to external and perhaps internal warfare, which over time could have left the many ruined temples and the like which Gimbutas takes as evidence of a Kurgan conquest.

If a Kurgan conquest did occur, it's unlikely that the IE people were defeated slowly, and experienced depopulation and multilocality. Any long period of multilocality would presumably have destroyed the distinction between residential insiders and outsiders. This in turn would presumably have ended the distinction between IE "binders" (localized) and "links" (relocating), something which didn't happen.

Also, in a kin based society like PIE with a high status term for a man's sister, the onset of intensive external warfare which threatens noncombatants tends to produce a high status term for a woman's brother as well, if initially the men cope successfully with the crisis.

However, there is no such term in any IE language. Evidently, IE men never achieved this ascribed status of heroes, at least not



while their societies remained kin based.

Therefore, unless it came very late (when the society was no longer kin based), a Kurgan conquest must have been sudden, with not even an initial period of successful resistance. The conquest cannot have continued from 4500 to 2500 B.C. There may have been war during those years, but not sustained intensive external warfare, at least not till the social organization had developed a territorial base, as may have happened toward the end of the period.

In summary, PIE is the language of Old Europe. If a Kurgan conquest did occur, it was quick, and the Kurgans were assimilated by the Old Europeans, leaving no known linguistic traces.

## Chapter 5: Some Other Hypotheses About PIE Society

Besides the familiar and widely accepted proposals of features for PIE, there many more that have never won wide acceptance. Most are methodologically flawed in some way, but others appear to have been discarded merely for not fitting expectations. In this chapter I discuss a very few, begining with one that I think has much merit.

### 5.1. Survivals of PIE Type Social Structure in Homeric Texts?

Gates (1971:61-63) calls attention to two studies with which he disagrees, by Molly Broadbent (1968) and M. Miller (1963), whom he takes to be the same person. She studied the genealogies in Homer's poems and concluded that they reflected matrilineal descent and three asymmetrically intermarrying groups (which implies MCCM).

Gates found the idea "startling," no doubt because he firmly believed PIE society to be patrilineal and to lack cousin marriage. However, we have seen that PIE society had wife centered residence, matriliney, and MCCM, which implies at least three intermarrying groups.

Gates makes a variety of criticisms of the Broadbent/ Miller hypothesis, repeatedly pointing out that the genealogies in Homeric texts are contradictory between variants, and even self-contradictory. I do not doubt the correctness of his observations, as Homer's poems are a collection of aural traditions handed down from an earlier age. Contradictions in the details are only to be expected.

However, some language data may cast light upon the matter. Homeric Greek mētrōs 'mother's brother' was innovated on the model of pátrōs 'father's brother', so evidently in Homeric Greek the terms for a father's relatives tended to be a model for those for a mothers, rather than the other way around.

This suggests patrilineal organization, but also that Homeric Greek tēthís 'aunt' (Gates 1971:20-22) probably involves an

extension from 'father's sister' to 'mother's sister'. If so, we get the Homeric Greek equation tēthē 'grandmother (father's mother)' = tēthís 'aunt (father's sister)', in origin highlighting a class of husband givers in a society with MCCM.<sup>34</sup>

This raises an intriguing question. Do the genealogies in Homer reflect, in an imperfect and garbled way, a much earlier time when the pre-Greeks still had MCCM, a class of husband givers, and matrilineal organization? Such organization would have been meaningless to the patrilineal Homeric Greeks, and it would not be surprising if they sometimes confused genealogies which had only been meaningful in terms of it.

It is perhaps impossible to answer this question with confidence, but one observation by Gates (1971:6-7) about the Homeric texts is suggestive: "the father-son relationship was looked at chiefly in terms of the affection and concern of the father for his son, rather than the son's respect for his father." This is classically the situation in matrilineal societies, not patrilineal ones. It looks like a survival.

## 5.2. Women Warriors?

Sanday (1973:1683) proposed an interesting theoretical

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<sup>34</sup> As one alternative interpretation, Lithuanian anytas 'husband's mother' from PIE \*han- 'grandmother' may be taken to suggest the equation PIE \*hanəs 'grandmother (father's mother) = father's sister', since in PIE society a husband's mother would typically be a woman's father's sister due to MCCM. These women would have formed a class of senior husband givers, the mirror image of the class of wife givers.

framework for the emergence of female prestige, based on Denham's view that survival requires attention to three main tasks: reproduction, defense, and subsistence. Since women typically put many hours into child care, men do more of the other two and gain prestige from it.

However, when men are unable to do this, and women take over some tasks traditionally done by them, it may provide women a power base and female prestige may suddenly rise. "Hence, female status is hypothesized to be a function of significant participation in subsistence or warfare activities. Due to the demands of child rearing it is more likely that, when conditions encourage it, females will engage in subsistence activities".

Nevertheless, Sanday's theoretical framework suggests that there could be instances of significant female participation in defense activities, and that this would also tend to raise female status relative to male.

IE mythology is full of warrior goddesses. In particular, Gimbutas (1991:118-121) describes the conversion of Old European bird goddesses into vultures or birds of prey carrying swords (or warrior women wearing bird headdresses) in southern Europe at around the time of the military activities which she takes to be Kurgan invasions.

In Aristophanes' comedy *The Birds*, women wearing bird masks demand that men give up plans for a war. They "occupy" the space

between earth and sky (i.e., between men and gods) and interrupt communications between the two planes - clearly a military tactic. In northern Europe, real life women warriors were encountered in Britain by the invading Roman legions many centuries later.

A useful source of analogy for these scraps of information may come from Nigeria, where Sanday (1981:86-89, 140) described societies in which women had social institutions parallel to men's. The Dahomeans, for example, had a dual sex army - 5,000 of which were women serving under female officers. These included many elite units, including the king's household troops.

Igbo women, who rioted against the colonial regime, called themselves "vultures" and, as Sanday points out, among the neighboring Ibibio vultures are "messengers of God."

Judging by analogy with this largely patrilineal area of Nigeria (coded "Af" in the Ethnographic Atlas), it may be worth investigating a possible association of such forms as PIE \*magh- 'fight' as the possible root of \*maghu- 'young woman' (see Wolfe 1980). Female soldiers of the Dahomean army were required to remain celibate, and serve till middle age, i.e., while they were young.

If the PIE military recruited some unmarried women, an ideal place for them would be as archers on chariots (where they could avoid the disadvantages of hand to hand combat with larger men). This could explain a possible association of PIE \*maryannu 'elite

charioteer officers' with \*mari- 'young women' (see Stone 1976:64-65).

PIE \*maryannu may be composed of \*mari- plus \*-ya: 'agent noun' and later 'feminine gender' (Meillet 1964:282) plus \*-no 'head, leader of' (Saussure 1959:226-227), for an etymology of "leader of young women".

As further evidence of important female participation in IE warfare, one might point to terms in some of the daughter languages like Latin vīrāgo 'a man-like woman, female warrior, heroine', and the feminine gender morphology (\*-a:) of some archaic-looking terms for military roles, like Sanskrit rathí:h 'chariot driver' and Old Slavic (voje)-vod-a 'army guide' (see Meillet 1964:282).<sup>35</sup>

However, whatever the merits of these particular speculations, important female participation in the military activities of PIE times isn't unlikely.<sup>36</sup>

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<sup>35</sup> There's circular reasoning in Meillet's argument that nouns in \*-a: and \*-ya: don't necessarily have feminine gender in origin because such nouns sometimes "designate male beings", as in the case of Latin agricol-a 'farmer' and Old Slavic bal-ījo 'doctor'. Why suppose that the early post-PIE farmer or shaman was necessarily a "male being"?

<sup>36</sup> While some PIE women were likely warriors, they were probably never the majority. PIE was a militarily stressed society, or had been recently, where male mortality was quite high. The ability of women to bind alliances (in place of dead men), to produce a local supply of food and thus permit population concentration, and to raise enough children to make up for military losses, these were their truly vital functions.

### 5.3. Was the PIE Homeland the Jordan Valley?

Adams and Otte (1999) discuss the possibility that PIE could have been spoken in the Jordan Valley, and that PIE society could have sent waves of migrants into eastern Europe following one or more climactic improvements, when Europe would have had a very thin population relative to the Middle East. The last two of these climactic improvements came 12,800 and 8,200 years ago.

The earlier one, the Younger Dryas period, comes before the agricultural revolution, though it may have motivated the first experiments in managing wild cereal crops, and perhaps even in planting them (Smith 1995:79). A migration at that time would imply that PIE was spoken by gatherer-hunters, who at most were experimenting with planting seeds. Moreover, animal domestication comes another thousand years later (Smith 1995:83).

Yet, PIE speakers were herders and, judging by *\*sweso:r* 'man's sister', matridominant agriculture was already producing about 30-45% of the subsistence diet in pre-PIE times. Therefore, this earlier date must be abandoned.

None of this precludes the later date. But it would leave unexplained why the Early Indo-Europeans didn't employ tokens, which had been in use in the Near East since about 10,000 years ago, as long as these societies had been agricultural (Lehmann 1995:272-273, Smith 1995:81).

In the light of these problems, if the Indo-Europeans originated in the Jordan Valley, a more plausible idea is an early pre-PIE migration. If so, however, it would be millennia before, in a separate set of migrations, PIE spread over a large area by conquering or culturally swamping its neighbors, probably as a result of a population explosion, due to intensive warfare and high male mortality. Only after this would its daughter languages emerge.

If I were to speculate on possible climactic causes for the division of IE society, I'd rather guess that the cold period the authors give as beginning 5,900 years ago (c. 4000 B.C.) might have stressed already agricultural populations living in territories already at their carrying capacities, and led to competition for resources, raiding, population concentration, increased agricultural activities, and the rest.

#### 5.4. The Paleolithic Continuity Theory.

Alinei (1998) suggests that PIE society is to be located in the Paleolithic, which would make it a simple foraging society. This requires the assumption that all the Neolithic vocabulary regularly reconstructed for PIE have diffused along with the things they refer to, at a time when the PIE had long broken up into its constituent languages.

I find it hard to believe this could have escaped the detection of specialists in Indo European linguistics, who have specialized techniques for recognizing loans (Mallory 1996:112-113).



In addition, vocabulary relating to domestic cattle is deeply embedded in the language, and very unlikely to consist of late diffused loans. Alinei is probably right about cultural and linguistic continuity between Old Europe and PIE, but PIE itself was surely a Neolithic society.

Alinei (1998) suggests that "by far the largest part of the Neolithic vocabulary is differentiated in all or most IE languages." However, this means little, as most vocabulary in general is non-cognate among the IE languages, reflecting the time depth of PIE.

Alexei Panshin also explores the idea of a very early origin for the Indo Europeans (or pre-Indo Europeans). "The second major event in the Indo-European story -- its breakup into a cluster of daughter languages -- would have also left obvious signs in the archaeological and genetic records. And as it happens, we do have evidence of one and only one such radical fragmentation. It took place at the time of the Last Glacial Maximum, some 20,000 years ago" (Panshin 2005). However, the time scale here is in major conflict with the linguistic evidence as presently understood.

Panshin guesses that they had patrilocal bands. "The patrilocal band normally appears where a culture is primarily dependent on male hunting for food and other resources and women's plant-gathering makes a far more limited contribution. Such cultures tend to be patriarchal (with men wielding most of the power), patrilocal (with men remaining in their father's hunting territory after they

grow up and women leaving their own families to join those of their husbands), and patrilineal (with descent reckoned solely from father to son.)

"This patrilocal structure is an exact match for the model of society deduced from the most ancient Indo-European kinship terms. For example, there were specialized words meaning 'son's wife', 'husband's father', 'husband's mother', 'husband's brother', 'husband's sister', and even 'husband's brother's wife'. But there were no equivalent terms for 'daughter's husband' or 'wife's father' or the rest. They were simply not needed" (Panshin 2005).

In fact, patrilineal organization would be most unusual in a society of hunters, because of the low population densities.<sup>37</sup> The evidence from the kin terms is simply the traditional argument, which as we have seen makes no sense.

## 5.5. Technological Determinism.

A perennial question is "if women were once free, where did patriarchy come from?" One answer explains the development of patriarchy by technological change, and suggests that further technological change may abolish it.

"If revolutions in technology once made dominance by men--and

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<sup>37</sup> In addition, patrilocal band theory thrived in the Algonquian context in the early twentieth century, but more careful ethnographic research (notably Dunning 1959) caused it to be abandoned for Algonquian. It is very unlikely to have any merit if applied to the (pre-)Proto Indo Europeans.

thus patriarchy--inevitable, it follows that when machines replace bodies altogether, as they have arguably begun to do today, patriarchy may well disappear" (Osborne 2005).

There is a grain of truth here. Technological change is in fact often a prerequisite for the decisions that produce social change. But these decisions may be delayed for centuries, like the decision to grow a vegetable once it is domesticated, and nothing ensures that it will be made at all.

For example, the Quechua evidence shows that equality can be quite robust in the face of just the kinds of technological change that are often claimed to produce patriarchy. Centuries ago Quechua men built, and have since maintained, agricultural terraces on the sides of mountains, and huge systems of aqueducts to irrigate them. Yet, the seed planted in those terraces is still chosen by women, and women still own the crops produced.

This suggests that patriarchy too may be able to survive technological change. It certainly would not be wise to sit on one's hands and await technological liberation by machines. At most, technological change may produce an opportunity to effect social change. The opportunity must be seized, or little is likely to happen.

# Chapter 6: How to Reconstruct a Proto Society

## 6.1. Lexical Reconstruction.

The broad outlines of prehistory are known, or can be inferred, from archaeology, history, and ethnography. For the prehistoric period, they mainly have to do with technology and material culture. In addition, archaeology can sometimes tell us if a people had wife centered or husband centered residence, and it usually can spot social stratification (a class society). But most of the details of prehistoric social structure remains unknowable archaeologically.

The potential of lexical reconstruction to fill out the details of prehistoric social relations and their evolution has barely begun to be exploited. It is true that in the nineteenth and early twentieth centuries attempts were made to reconstruct the social organization of the Proto Indo Europeans.

These took place in the context of two main schools of myth-like explanations of the origins for social institutions, especially for those which govern the relations between the sexes. The first claimed that early human society had woman centered social organization, to which men were somewhat marginal, and where women held all authority. The second claimed the opposite: that men had always been central, and had absolute authority, in all times

and places.

The advocates of patriarchy were the majority. And despite minor revisions, their conclusions remain at the heart of the received ideas on the subject to this day. Although the scholars in question may have been sincerely looking for the scientific truth, with hindsight one can see they were motivated largely by their sexual politics.

The main lesson these reconstructions teach us is that if a researcher starts out with some firm theoretical opinion, he will be alert for any evidence that he thinks supports his theory, and mostly ignore the rest. Neither the matriarchy nor patriarchy theories of the Proto Indo European social organization are actually supported by the objective evidence. If one is to do real science, one must not "want" any particular conclusion. One must want only to know the truth. One must be prepared to discard several preliminary hypotheses before finding the best one.

In the more cautious late twentieth century, explanations for social institutions tended to account for a single institution out of context, using one-factor cross-cultural theories only tested statistically. Significant statistics lends the illusion that one has explained something, when in fact one has only shown that something or other is common (and not explained why).

Statistical reasoning also leads to the common practice of limiting one's attention to social types relatively frequent in the

ethnographic record. This means one may be blind to crucial aspects of social history, which either have become less frequent in recent times, or never were frequent.

A proto society is an unusual one in that it is at the point of breaking up into two or more independent entities, and its social organization too may be rather unusual. Proto languages may thus reveal unusual societies, too rare to lend themselves to statistical analysis.

This being the case, one surely doesn't want to ignore the institutions of the less frequent types of societies in seeking to understand the proto society one is reconstructing.

Neither approach, political nor statistical, will ever produce a credible account of social prehistory. Any credible account must be data driven. It must develop out of the attempt to explain the data set for some particular proto language.

### 6.1.1. The Transition in Social Structures from Forager to Agricultural.

Often, a proto society was a newly agricultural one, that became involved in intensive external warfare due to territorial conflicts. This might be over garden plots, or because denser population depressed levels of game animals, and male quarrels over hunting territories spiralled out of control. When the birth rate rose to

compensate for high male mortality, local overpopulation forced migrations in search of living room.

When foragers first adopted agriculture, they as yet lacked the social institutions appropriate to their new lifestyle (territorial organization, chiefs, stratification, and the rest). Acquiring these took time.

In the beginning, and during a fairly long period of transition, I contend that foragers had to adapt their kin based gatherer - hunter social organization to new circumstances, for which they were not ideally suited. For example, since larger and larger social units were needed for defense, these larger units were put together by systematic arranged marriages between members of much smaller units, linked in alliance chains.

These transitional forms of social organization had not existed in forager societies, and in most cases they would soon be replaced by territorial organization. But they are a kind one may find in reconstructing proto societies. This is one more reason we should not expect a proto society to be of a sort commonly attested in the ethnographic record.

## 6.2. Reality constraints on lexical reconstruction.

(This section is repeated here from an earlier monograph, for the convenience of those readers who have not read it. Others should feel free to skip it.)

Many books have been written on the methodology of science, and this is not the place for that. Here, I will only discuss a few points that are often not understood, but which are crucial to reconstructing prehistory.

### 6.2.1. Recently conditioned social features versus survivals.

Variants of social organization, behavior, or technology which are motivated by current or recent conditions may themselves be recent. Those which are not obviously so motivated are more likely to be inherited from a time when conditions were different. Let's look at a hypothetical example.

Suppose alien scientists landed somewhere in Europe or North America, in a conservative neck of the woods where men wore pants and shirts, while most women wore dresses but a few wore pants. They might hypothesize that everyone once wore pants but that women were beginning to wear dresses, or that everyone once wore dresses but now only some traditional women did, or that both types of clothes had long been used (say, by people wishing to make statements about their sexual identity). The third hypothesis would explain the persistence of the difference, but not its origins or particular form.

If they learned the English language, they would soon discover that while women are said to "dress up" when they put on their clothes, so are men (they don't "pant up"). They might then



tentatively conclude that once upon a time men wore dresses, and literally dressed up in the morning. Later, when clothes changed, the old expression might have survived. If the aliens then studied Spanish, they'd discover something similar: the root vesti- of the verb vestirse 'get dressed' is also found in vestido 'dress' but not in pantalones 'pants'. All this wouldn't tell them why men changed their "dress code." But it would tell them that it was the men who innovated, and the women who mainly retained the old type of clothing (though of course changing their details). They might then look at changes in male activities for the cause. If they studied history, they'd learn that in the nineteenth century men frequently rode horses, and most women almost never did. They might also discover that in ancient times, in cultures ancestral to those of Europe and North America, men rode in chariots and not on horses. In those days men wore a dress-like garment called a "toga." As a hypothesis, they might propose that togas evolved into dresses, and that men shifted from dresses to pants when they began riding horses. The wearing of pants can be explained by horse riding, while there is no obvious explanation for why anyone would need to wear a dress, other than to follow tradition. Therefore, they'd tentatively conclude that dresses are the earlier type of clothes. (This may oversimplify things, but it illustrates a point.)

Now, to complicate our example a bit, suppose the aliens instead were to come in the twenty-second century, and do a survey which showed that women wore dresses in only about one tenth of the communities. Suppose further that they failed to notice the clues

hidden in Indo-European languages, and instead reasoned as follows: "most of these people wear only pants, so this was probably true of their ancestors. It's more likely that only a few groups changed their mode of dress, rather than many." Of course, they'd be wrong, but pseudo-statistical reasoning of this sort is very common among scholars who miss the clues found in language. It assumes that the changes involved are random events, hence equally probable, which isn't often true. Most changes are motivated, at least important ones. One must find their motivation, not just count their frequency.

Awareness of the motivations for cultural features allows a prehistorian to perform something like the differential diagnosis of the medical practitioner: eliminate all the possibilities but one as being otherwise-motivated, and tentatively retain the remaining possibility as being inherited. For this reason, I will sometimes refer to the cultural and linguistic developments used in this way as Differential Developments.<sup>38</sup> You may recognize eliminating all but

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<sup>38</sup> The method of Differential Development is a particular instance of the comparative method widely used in the social sciences. It resembles the comparative method used by the Neogrammarians in reconstructing proto-languages, in that it aims at reconstructing an earlier state of affairs which is unknown. These Neogrammarian techniques are very complex and many books have been written about them. However, for our present purposes it suffices to point out that in the realm of phonology, they have been very well understood for nearly a century and present no major problems.

Differential Development also resembles the variant of the comparative method used for explaining the causes of social change (see Little 1991:31-38), in that it depends on identifying the causal factors of developments found in one case but not in another. This in itself is simple enough. However, because in Differential Development there are at least two unknowns involved (the initial state and the factor or factors causing change), one must

one hypothesis as a favorite method of Sherlock Holmes, one no doubt learned from Dr. Watson.

The opposite of a cultural feature motivated by recent conditions is one whose motivation is unknown. Poorly motivated cultural elements generally are left-overs from older traditions, reflecting conditions which no longer obtain, and are called survivals. Of course, survivals are often difficult to date and don't in themselves permit reconstruction of social institutions, but those are other problems.

### 6.2.2. The principle of continuous transmission.

As we've seen, when a people divides into two or more groups that live separately, the gradual change in their language that normally comes with time takes place a bit differently in each group, producing families of related languages. By the use of complex but well established techniques which we can take for granted here, a comparison between the languages within a family permits historical linguists to establish the form of the language prior to dispersal, known as a proto-language. In the simplest cases, the words in a proto-language tell us directly about what its speakers talked about. For example, the names for particular trees and animals tell us much about where they lived (somewhere where those trees and animals

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consider as many alternate hypotheses about causal factors as there are plausible reconstructions. It might seem that this would tend to leave us with several possible alternative solutions. However, I have never encountered a case where more than one of these alternate solutions made sense in historical and cultural context, once all factors were taken into consideration.

are found). So far, this is fairly simple.

However, some words bear two kinds of meaning. A word always identifies some entity or action. This is called its denotation. But some words suggest some further meanings, or connotations, and their original connotations are often revealed through their etymologies. For example, the denotation of Proto Algonquian \*a:kama:tkwa is 'white ash tree', but its etymology is 'snowshoe wood' (\*a:kama 'snowshoe', \*-a:tkw 'wood').

Clearly, when this word originated, the innovators were in the habit of using white ash wood for making snowshoes. But did the Proto Algonquian people themselves coin this word, just before splitting up? Or had their ancestors invented it centuries earlier? Is it possible that the Proto Algonquian people themselves had moved and made snowshoes of some other wood or no longer made snowshoes at all? In such a case, they might still have had the word for them, but their etymology would no longer have been relevant. There's a serious danger here, of reconstructing for a proto-society a feature which had existed earlier but no longer did. Even one such mistake can be very misleading.

It turns out that the Micmacs (and some other ethnographic Algonquian peoples) traditionally use white ash wood to make snowshoes. Moreover, they still call the snowshoe and white ash tree by their inherited names, although pronunciation has changed (respectively to axam and axamox in the Micmac case). That is,

both the technology and the words associated with it are found together not only sometime in or prior to Proto Algonquian times, but long after Proto Algonquian times as well. Since technologies of this sort tend to be stable (between the time of their origin and that of their replacement by better ones), the implication is clear: there was a continuous cultural transmission of snowshoe technology down through Proto Algonquian times. Any other explanation is far fetched.

Moreover, some Algonquians, such as the Innu of eastern Quebec and Labrador, migrated out of the range of the white ash tree. Among the Innu, where the wood preferred for snowshoe making is the gray birch, the Proto Algonquian terms are replaced by asham 'snowshoe' and ashamashku 'gray birch, snowshoe frame'. So we see that where the technology changed, the terms were likely to change as well.

The Principle of Continuous Transmission must be kept in mind wherever etymology is at issue. However, in reconstructing social institutions and rules which tend to fluctuate among a very few alternatives with changing conditions, such as the rule of postmarital residence (which determines with whom newlyweds will live), the principle of continuous transmission alone can't guarantee that there wasn't some fluctuation in the rule between the point of origin of a term and the ethnographic end point. One must find some further evidence that fluctuation didn't take place, or the inference of continuity is a very weak one. Examples of this are complex, and are

best considered in context as they occur.

The evidence of continuous transmission is enormously important in determining just what kinship terms connoted in Proto Algonquian times, and thus what kind of social organization the Proto Algonquian people had. One must always keep this in mind. Without this precaution, one could construct a Proto Algonquian society using a collection of etymologies, some of which might in principle have been diagnostic centuries before Proto Algonquian times, but no longer diagnostic in Proto Algonquian society itself.

Social organization is a synchronic system, each feature being meaningful only as part of that system. In reconstructing Proto Algonquian society, we must be careful to use only features present in Proto Algonquian times themselves.

Unfortunately, due to the great time depth and to the radical change in societal type that came with the introduction of the cattle drawn plow, the chances are slim that any functioning PIE institution survived to be recorded. As a result, the features we reconstruct for PIE may have in fact been present in PIE, or in principle may have been present in pre-PIE (and no longer have been in PIE times).

It is usually possible to distinguish the two situations by examining the internal logic of the evolution of kinship institutions, as known from cross cultural sources. However, the possibility of ambiguity should inspire a salutary caution.

### 6.2.3. Received ideas, intellectual fads.

Even when differential developments and the principle of continuous transmission are taken into account, another major source of potential error remains: the opinions of trusted fellow scholars. As young children with little knowledge of the world, we all had to learn to accept what our parents told us, whether we fully understood it or not, or we would have suffered severe consequences. When we went to school, we transferred this attitude to our teachers and it served us well: the "right" answer on a test was the one which matched the teacher's opinion. Once we graduated, many of us began trying to evaluate things rationally, basing our conclusions on nothing but the evidence. However, it was a new habit, not easily acquired. Moreover, we simply don't have the time or energy to go back and reconsider every conclusion we once came to. This baggage of secondhand conclusions acquired in childhood and in school is known as "Received Ideas," and it contributes a little sand to nearly everyone's foundations.

A whole generation of scholars often shares the same received ideas on some subject, having gotten them from the same source. Perhaps some excellent scholar tentatively suggested some hypothesis or other, in the absence of very much evidence. Others thought it sounded reasonable and, as the years passed, began to believe that someone would surely have disproved it if it were not correct. They passed it on to their students, who took it as infallible truth.

In other cases, a doctrine was simply produced by the swinging of the pendulum, as everyone rushed to get away from an alternative hypothesis, suddenly cast into doubt by some new discovery. Scholars hate to be associated with an idea regarded as disproved or out of date, even if it only needs a few minor adjustments to be more reasonable than any of its competitors. This produces intellectual fads, which become received ideas once passed on to the next generation.

The reader - particularly the young reader - needs to remember that scholars are human, and thus easy prey to received ideas, to intellectual fads, and sometimes to illusions of infallibility and to stubborn pride. Testing each idea scientifically is impossible. No one lives that long. Only a few key ideas are ever evaluated rationally, at least with any great care, by an individual scholar. It's an uncomfortable admission, but even after all the best efforts, these sources of error can never be entirely eliminated. Moreover, there's another source of error, a scholar's own inevitable mistakes.

#### 6.2.4. Scholarly infallibility.

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"That's a long winded way of saying 'I don't know'." Lucy Calder grinned. "Or, to translate from O-1, 'the absence of knowledge is retained in my mind'" (Allen 1986:72).

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One of the best scholars I ever met was Charles Hockett, one of my teachers during graduate school at Cornell in the 1960's. He was widely recognized as a great scholar, and we went to his classes with a sense of awe. One of his favorite methods was to get us working on a problem he had already worked out, and to coach and encourage us, correcting our errors and gently nudging us toward a solution.

One day, he started us on a particularly complex morphophonemic analysis which turned out to be too tough for the beginning students we were. Again and again, he would have to give us bits of the solution. Finally, no one dared venture an opinion, until my friend Cornelius and I decided that two heads were better than one and began working on it together (we told Hockett we were doing this, lest we be accused of "cheating"). We took turns proposing sets of rules, some of which Hockett accepted, some of which he showed us the flaw in. Near the very end, I proposed a set of final rules that would complete the analysis and generate all the correct forms. Hockett looked at my rules with interest, looked at his watch, and said that no, it was very good but not quite right, and since class was nearly over he'd give us the correct solution. His solution of course worked, but it took an extra rule to do it.

For no very good reason, I cut the next class and was relaxing in my room when Cornelius barged in demanding to know where I'd been. "Hockett was looking for you," he said. "He wanted you to explain your rules to the class again. It seems he was up all last night working on the problem, and decided your solution works, and is

simpler than his own."

Although I thought I'd been quite clever at the time, my many subsequent mistakes have taught me that the episode provides a rather different lesson. Even the best scholars make mistakes or propose imperfect solutions, especially in dealing with complex questions, and even a first year student can sometimes catch one. The real mark of quality in a scholar is the ability to quickly recognize one's error, give credit where it's due, and adopt the correct solution. The reader who finds a flaw somewhere in the present complex work is invited to write me about it, so it can be corrected next time I return to the subject.

Someone may object that correcting one's mistakes impairs the image of the scientist as pundit, as omniscient and infallible. But that, I think, is a healthy thing. Few things are less conducive to scientific progress than the defensive attitude of a scientist who has collected a lifetime of uncorrected errors. Understanding the limits to one's knowledge is just as important as acquiring the knowledge itself.

#### 6.2.5. Academic politics.

Academic politics is the art of getting ahead in an academic bureaucracy, and in some ways very similar to the politics found in any other bureaucracy. The main point is never to give your competitors an opportunity to say that you made a mistake,

something best accomplished by always agreeing with the majority. That majority may really be totally wrong: that doesn't count. In bureaucratic gamesmanship, the majority is always right.

The main difficulty in this tactic is that majority opinion may change if new ideas triumph over old. When they do, yesterday's "correct" ideas, widely held in the belief that they afford protection from criticism, suddenly become errors. It isn't so bad if the new idea came from a recognized genius, if all your competitors are caught in the same error, and if you are among the first to switch. It's terrible if the new idea came from an average scholar, or if you are more associated with the old "wrong" idea than most.

A very common response is to try to prevent new ideas from being published, and to ridicule them when they are. When this fails, a campaign is sometimes launched to discredit their authors. Since many (or at least several) scholars usually have a vested interest in protecting an old idea, and they tend to lunch together at learned conferences, the similarity in the attack may make it appear to be the product of a deliberate conspiracy. Others may then join in, seeking safety in membership in the aggressor group.

In some cases, it is not old wrong ideas that scholars defend, but those favored by some powerful man, be they old or new. Years after the end of the USSR, and many more years after his death, there are still scholars stubbornly defending ideas favored by Joe Stalin as if their lives depended upon it.

### 6.2.6. Anthropological reconstruction as story.

One might think that science would consist of gathering evidence and reasoning logically, and so proving something or other. This does happen, particularly in laboratory situations, and perhaps wherever evidence is unlimited. However, in studying the past, evidence is always limited. There are always a lot of gaps in the record. Conclusions are not self evident.

One response to this is to say the past cannot be known, and that to try to study it is unscientific. That was Radcliffe-Brown's response. Another is to examine only those rare situations when the evidence on some particular point is especially strong, and to reconstruct only some isolated fragment of prehistory. However, this isn't much better. A few disconnected fragments of prehistory tell us very little, especially since they are easily misinterpreted without their context.

The only way there is any hope of understanding prehistory is by using all the evidence available, weak as well as strong, and boldly linking it all together into a plausible story. Every part of such an account must be compatible not only with theory, but with every other part of the account, as well as all of the relevant data. This introduces additional constraints on the interpretation of an individual datum, and thus strengthens evidence which otherwise might be too weak to be useful.

Despite the most careful analysis, some points in such a story are likely to be wrong. This is inevitable, since there always remains a good deal we do not know. This is bad news indeed to those playing academic politics. But it does not deter those who are willing to adjust to growing knowledge, correcting each error as it is discovered, and learning from it.

### 6.3. Interpreting lexical reconstructions.

At the 98th annual meeting of the American Anthropological Association, Jane Hill delivered a paper entitled "Why do Native American Place Names Look so New"? In it she suggested that the Old and New Worlds have contrasting "ideologies of naming." In the Old World, place names are usually just names, and although they were presumably meaningful in origin, rarely has that meaning remained transparent. In contrast, however, she observed that Native American place names retain "descriptive semantic transparency even in cases where we are fairly sure that speakers of the language have lived in the named area for two thousand years or more" (Hill 1999:39).

My research has convinced me that on the whole, for Algonquian languages Hill's insight is valid and can be extended to semantic domains other than place names, with extremely important consequences. In particular, I'm convinced that just as New World peoples often use place names as maps, and the names of trees and plants as guides to their principle uses, the system of kinship

terminology of an oral kin-based New World society is often its political constitution.<sup>39</sup>

Every time a kin term is used in such a society, the speaker is taking a political position as to proper social organization, and any deliberate social reorganization is accompanied by a change in kinship terms or in their referents. Only when a social institution loses its importance slowly and imperceptibly is a term reflecting it likely to gradually grow semantically opaque.

It is unlikely that geography accounts for the difference in the two naming patterns. More likely, most Old World societies have long been fairly large scale ones, and territorially based. People speaking different languages need to recognize the same place names, and changing a name to make it more clearly descriptive in one language would create a problem for others. So names are largely traditional and meaningless.

In the New World, societies are mainly smaller scale ones (or recently were), where everyone speaks the same language and changing a name to make it more clearly descriptive does not pose a problem. Or so I suppose. Whatever the reason, PIE (a small scale Old World society) does have several kin terms that appear to be descriptive.

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<sup>39</sup> Societies like the modern industrial ones in which kinship terminology remains unchanged during social reorganizations are territory-based, not kin-based.

The varied kinship terminologies in a set of genetically related kin-based societies sometimes collectively contain in themselves a picture of the proto kinship relations these societies originally shared, plus the record of the successive changes which postdate their divergence. Like tree-rings, they provide a long-term record of growth.

#### 6.4. Faulty Methods Used in Earlier Reconstructions of PIE Society.

The reader may be wondering how a non-Indo-Europeanist like myself has the gall to propose a reconstruction of PIE society, especially one so radically different from the standard account accepted, with minor variations, by so many Indo-European experts. Let me explain.

##### 6.4.1. The History of PIE Reconstructions.

Linguistic paleontology was born in the early nineteenth century as an offshoot of comparative philology, which in turn was developed for the study of Indo-European language and culture. This was the focus around which the comparative method of linguistic reconstruction, and modern linguistics as a whole, gradually emerged.

It consisted of a great deal of trial and error, which took place in the context of anthropological debates of the day which now seem

quaint. In the absence of any systematic cross-cultural information on possible types of social organization, the emerging science of linguistic paleontology relied heavily on what was known about classical Indo-European societies and what creative anthropological speculation could conjure.

By the time Ferdinand de Saussure brought synchronic linguistics into existence in his *Course in General Linguistics* (1906-1911), the excesses of linguistic paleontology had led him to the conclusion that its claims were almost entirely spurious.

He told his students: "we cannot expect language to furnish such information for the following reasons: First is the uncertainty of etymology ... scholars were also wrong in assuming that the absence of a word proves that the primitive society knew nothing of the thing that the word means ... the possibility of loan words is a third cause ..." (Saussure 1959:225). He made some partial exceptions - notably for "common terms indicating kinship" - from which he nevertheless only concluded that "among Indo-Europeans the family was a complex and stable institution, for their language could express subtleties that ours cannot".

Besides a keen intelligence, what perhaps allowed Saussure to spot the fallacies in the work of his predecessors was his clear separation of diachronic (historical) linguistics from synchronic (descriptive). Up to that time, most if not all scholars tended to see a reconstructed proto-language as an idealized form abstracted



synchronously from several imperfect models (its daughter languages).

Saussure's breakthrough was the realization that while all languages are abstractions in some ways, protolanguages aren't different from the others in that respect. For example, take the word 'tree'. Concretely, we have maples, birches, spruces, and so forth, big ones and small ones. 'Tree' is an abstraction, which covers all the concrete examples by ignoring their differences.

Many early scholars evidently thought that this kind of abstraction also applied to the notion of a language as a collection of dialects. They reasoned that what people actually spoke was a variety of regional dialects, and that to call them collectively a language was just an abstraction of the same sort, achieved by ignoring their differences. This wasn't entirely wrong, for in practical terms we do ignore regional differences each time we talk to someone who speaks a dialect other than our own. That's a valid synchronic view of dialects.

However, there's also a diachronic view. If a language is spoken by few enough people, all living in one place, it will consist of a single geographical dialect because people unconsciously copy one another's speech - especially the speech of those they admire - and so eventually level out any differences that arise.

However, if some of the speakers of this language migrate far enough away so as to rarely speak with those left behind, changes to

the way of speaking in one area will rarely spread to the other. In time, the language will split into regional dialects and, after many hundreds of years, the accumulation of differences will be so great that their speakers will no longer understand one another. At this point, we say there are separate languages, which belong to the same family.

Many early IE scholars failed to fully grasp that the historical view is primary, and that the synchronic view of a language or language family as a abstraction is only possible once diachronic change has brought about differences. Some even believed that the differences among the daughter languages were due MAINLY to geography, adaptations to different climates and living conditions. (Such adaptations do lead to changes in the names for some things, of course, but usually little more.)

All would probably have admitted that the drift away from the original form of IE took time, but evidently most saw the time elapsed as incidental and fairly short. Consequently, they expected PIE society to be an archaic version of the societies of Classical Antiquity, not something radically different.

Even Benveniste (1969:316-317), who at other times was evidently well aware that the Classical societies had evolved away from PIE society, suggested that in depicting the Heroic Age, in some respects Homer provides "a picture of what PIE society must have been ... The way the family and clan assemble, the way their

leaders speak and act, must reflect exactly enough the behavior of the warrior class in the Indo-European world".

He quotes Agamemnon: "Arrange the men by tribe and phraternity, so that phraternity can give aid to phraternity, and tribe to tribe". He seems totally unaware that in the approximately 2000 years from PIE times to those described by Homer, social organization can have changed; nor that, even if some basic organizational principle were to have continued, the social scale could be vastly different.

For example, the etymology of Greek phrē:trē 'phraternity' (from PIE \*bhraHte:r 'brother') suggests that it likely once simply referred to a set of brothers. The etymology of Greek phûla 'tribe' is unknown, but a dialectal variant, Dorian wik- 'tribe', reflects PIE \*weik-, whose original meaning could simply have been 'camp' or 'clan camp' though later it came to mean 'neighborhood, hamlet, village' (see Benveniste 1969:310).

If Agamemnon was reciting ancient military prescriptions for combat, the one cited could originally have meant "arrange the men, with brothers and camp-mates together, so that brother can help brother, and camp-mate can help camp-mate".

Two scholars of the time were partial exceptions to the prevailing views on the possibilities of linguistic paleontology, Antoine Meillet and Leonard Bloomfield, both students of Saussure. Meillet warned his readers not to forget that "between the Indo-

European period and that of the oldest texts in each language, hundreds of years have gone by" and that the "vocabulary which reflected the concepts of the half-civilized Indo-European world" is hard to find in those of "literary languages which portray advanced civilizations" (Meillet 1964:382-383). That is, he was aware that PIE society and the classical texts referred to cultures distant in time and in type.

Yet, even he very commonly cited a cognate set and failed to reconstruct the PIE form it attested, as if the reconstructed form was just an abstraction, a shorthand, for the cognate set, rather than a part of a concrete language spoken thousands of years earlier in a society very different from that of Classical Antiquity. Again and again, his descriptions focus the reader's attention on the facts of the daughter languages and hence daughter societies, with only occasional brief references to their parent language. Consulting his work often feels like trying to read a palimpsest.

#### 6.4.2. Linguistic and Archaeological Reconstructions.

Perhaps because he began anew, with an Amerindian language family, Bloomfield did better. Only rarely did he pad his descriptions of Proto-Algonquian with material from one or two of the daughter languages, only, I assume, when he couldn't decide what to reconstruct.

However, he did reconstruct Proto Algonquian words for

'whiskey' and 'gun', knowing full well these concepts were unknown to the Proto-Algonquians. His comment was that while the concepts were new, the habits of word formation were old. That may be a noteworthy point, justifying a comparison of the forms in question, but it doesn't justify reconstruction, unless one thinks a reconstruction to be a synchronic abstraction. Old ideas die hard.

I emphasize these points because they suggest that not even the best linguists have always kept present in mind the true significance of linguistic reconstruction. If a linguistic reconstruction is to be useful in understanding prehistory, it must be a true and faithful representation of a real language, spoken by a real people with a real culture and social organization, at some specific point in the (usually distant) past. A full diachronic account of it would consist of a series of successive synchronic accounts - each stage separate from each of the others. Each stage would have to be historically valid.

Those who don't use the proper diachronic techniques when comparing languages are left with a list of similarities among them. These have been variously called inter-language or, in the Indo-European case, Standard Average European, and have provided the basis for Esperanto. However, these inter-languages differ from PIE by all, or nearly all, of the widespread changes which have come to Indo-European language and culture over the last 6,000 years.

For example, if a word apparently cognate with telephone is found in enough Indo-European languages, and means the same

thing in all of them, then we have to admit that it's Standard Average European. Of course, no one actually proposes to reconstruct such terms for PIE. The danger is only that one may slip into similar errors in cases where the absurdity is less obvious.

Archaeologists are among those most likely to be interested in linguistic paleontology, for as Dean Snow (1980:17) put it, "archaeology allows the direct recovery of only a small portion of the material culture of an extinct community and none of its nonmaterial culture". Despite some valiant attempts to glean more from the archaeological record in recent years, linguistic paleontology is still the only way of filling in most of the missing information. However, archaeologists are perhaps even more likely than linguists to misunderstand the nature of linguistic reconstruction.

Snow (1980:20) defined an archaeological phase as "a set of components that appear to be closely related in time and space on the basis of dating, proximity, and similarities in their assemblages". I presume that an archaeologist might tend to see a language as an analog of a phase.

If I understand archaeology correctly, a phase is an abstraction created by comparing, for example, several similar digs in a particular river valley, and ignoring the differences among them. That is, it's a synchronic abstraction, but not necessarily one that comes about because a once-uniform culture has diverged in the various localities over time. Just as likely, a variety of local cultures have become more

similar to one another as a result of several technologies, styles, or beliefs diffusing among them.

A good example of this is the wholesale adoption of Iroquoian technologies and social traits by the Delawares. In this case at least, any reconstruction of a phase covering both the Iroquois-speaking and Delaware-speaking members of the Iroquois Confederacy would be a synchronic abstraction, and with no deeper diachronic validity. Deep historical reality is better reflected by language. Delaware is a typical Algonquian language descended from Proto-Algonquian, and bears no resemblance whatever to any Iroquoian language.

In contrast to Snow's widely-diffused early views (later modified in Snow 1995:70, correcting Snow 1980:27-28), protolanguages are real languages rather than normative abstractions. In origin they're spoken by relatively small, homogeneous speech communities at a particular point in time. If these speech communities expand over large areas, dialect differences do develop and may last for hundreds of years - as a first step toward a final split into distinct languages. Features of these dialects can often be reconstructed - but in principle the theoretical status of a dialectal form is very different from one found in the pre-dialectal protolanguage.

The main reason the evidence about migration and the like which can be gained from language typically doesn't fit archaeological sequences is that, through no fault of their own, linguists are rarely able to estimate time depths with enough precision to be useful, and,

despite careful attention to the matter, archaeologists still can rarely distinguish those discontinuities in the archaeological record which result from population shifts from those due to purely cultural changes (including diffusion).

The exceptions are few, and involve really shallow time depths. Moreover, even at those shallow time depths, it's very easy indeed to misinterpret the archaeological record as to its implications. It's only when sophisticated linguistic and archaeological reconstructions are made separately, and then compared, that in a few favorable cases one may recognize how they reflect the same events.

Otherwise, the attempts to link specific archaeological complexes to linguistic reconstructions remains speculative. What is more useful is matching reconstructed lexical items with broad cultural changes that are clear in the archaeological record. This can help put dates on linguistic information.

#### 6.4.3. Recent Work on PIE.

Although Indo-Europeanists have gradually accumulated new and more persuasive insights into PIE society in the decades since Meillet's magnum opus of 1937 (Meillet 1964), most have nevertheless gone on repeating some of the methodological errors of their predecessors.

In 1980, in his paper "Social Organization in Western Indo-



European", Steven O'Brien still needed to warn that "it can be said that there exists a number of inadequacies with the methods employed in the study of Indo-European social organization. Researchers have relied too heavily on historical data from a single, or at best very few Indo-European groups. Ethnographic theory and data have been largely ignored. Diachronic aspects and economic determinants have received little or no attention. Written records have been employed without sufficient concern for their validity or applicability" (O'Brien 1980:129).

As to the possible conclusions to be arrived at from the data, O'Brien (1980:126) points out that "little thought has been given to the possibility of a type or types of social organization whose structural framework was different from any of the existing examples (in the daughter societies) but whose development potential would be compatible with all."

Perhaps the most original of O'Brien's insights regarding Indo-European data is that "among Indo-European groups, there is a tendency for those which appear earliest in the historical record to have experienced the greatest exposure to more complex social systems" and that "we can expect some changes in the social organization directly attributable to the same agent which was responsible (directly or indirectly) for the documentation" (O'Brien 1980:128).

In other words, the very fact that we have texts in some language

suggests that its speakers may have undergone massive social change since PIE times. If so, contrary to the common claim that such-and-such is likely to be of PIE origin because it's attested in the early texts, that attestation may in fact be an indication that the cultural artifact in question is likely the product of a more complex literate society.

Finally, on a purely linguistic note, Indo-Europeanists often discard all cognate sets not attested in more than two languages. While this may be justifiable conservatism when limited to establishing the phonological laws of PIE, it can't be justified when one is doing lexical reconstruction of PIE society.

There in principle it tends to introduce a systematic bias, selecting against the evidence of the social features most likely to have undergone change, and which are therefore likely to be the least well preserved. As a non-Indo-Europeanist, I don't know how much data (if any) has been discarded in this way. I do know that any such discards are likely to be very important.

The proper approach to poorly attested words is to subject them to exactly the same kinds of tests as better attested ones. Do they have regular correspondences? Are they found in geographically separate languages? If so, they should not be discarded.

A different criterion, requiring that each cognate set be attested in both Europe and Asia, has more merit. It is possible that there was some joint innovation reflecting new cultural items in just

Europe, and just Asia, during or shortly after migrations into these areas, while the migrators were still in touch with one another.

However, this situation is hard to distinguish from cases where an item was present in PIE, but dropped in all the European or all the Asiatic languages because it was not present in the newly colonized area. An element of doubt will always attach to such items.

The proper approach to the interpretation of PIE linguistic data is to examine PIE reconstructions in light of what the social sciences tell us about human beings and societies in general, without any preconception as to what kind of a society we may be dealing with.

This may be difficult for someone who has spent a lifetime becoming overly-familiar with the cultures and texts of Classical Antiquity, and with the earlier hypotheses about Indo-European society itself. Consequently, familiarity with technologically simpler societies, such as those of many of the Algonquians, is an excellent way to restore perspective.

#### 6.4.4. Eastern European Work on PIE.

For much of the twentieth century, the Soviet Union (and to a lesser extent other parts of Eastern Europe) evolved intellectually in relative isolation from the West. Today, as contact is renewed, we find we have two somewhat incompatible cultures which have grown

independently from shared nineteenth century roots.

As a result, when I read Eastern writings involving anthropology, I often find myself thinking "this sounds like it was written in the nineteenth century." Moreover, because there was a major break with nineteenth century anthropology in the West, the impression is "this is hopelessly out of date."

In some respects, this may be justified. Surely Western anthropology has made a great deal of progress in the last few decades, and probably some of these insights have not found their way into Eastern anthropology as fully as they deserve.

Yet, I am uncomfortable being too smug about this. Could not my Eastern colleagues sometimes have had valuable insights, which were never recognized as fully as they deserved in the West? Could not my Eastern colleagues sometimes be thinking that some Western ideas remind them of nineteenth century writings? And if so, would they necessarily be wrong? Surely anthropologists of all people need to be sensitive to the dangers of ethnocentric chauvinism.

In other cases, the two traditions simply have fallen into different mental habits, which may be equally unhelpful in dealing with particular questions. For example, I find that often my Eastern colleagues are rather too quick to use exotic ethnology as a model for prehistory, and to conclude without sufficient evidence that our ancestors of a few centuries ago were vastly different from ourselves.

However, I also find that commonly my Western colleagues seem trapped in the here and now ("presentism"), and seemingly unable to conceive of the past as potentially different from what is more familiar. Some even elevate this lack of imagination into a doctrine, which they call Uniformitarianism (borrowing the term from biology, as if culture were as slow to change as nature is to evolve).

Recently Sergey Kullanda (2002) wrote a paper claiming that "patriarchal clan society was not the earliest form of Indo European social organization as has been believed and that the latter seems to have been based on age-sex stratification." He argued further that "the early stages of social evolution simply did not have kinship in the strict modern sense of the word. Individual genealogical kinship did not play a significant role in social patterns... (Kullanda 2002:93).

It is all too easy to see in this a throwback to nineteenth century anthropology, when it was common to speculate that some of our very recent ancestors had been essentially ape-like (or chimp-like) in their behaviors. It is all too easy to see in it Joe Stalin's ideology, enforced upon Soviet scholars by the threat of Siberia, that the family can easily "wither away" because it's a recent invention. But is this really all there is to Kullanda's paper?

My own research, which grew out of the Western tradition, led me to the conclusion that kinship in a kin based society - one

lacking significant territorial organization - was vastly different from kinship in a state society. In a state society, politics broadly conceived is largely the domain of territorial organizations of various kinds. Kinship is largely reduced to the nuclear families one belongs to at different points, and within these only domestic matters are usually treated.

However, before territorial organization took over political matters, politics were a part of kinship. They still are, in simple kin based societies, except when they deal with outsiders. I find the century old debate over whether biological families or larger political units are "primary" in such societies to be sterile and unhelpful. I see them as largely advocacy for particular ideological views of how modern society should be organized. I think such advocacy should be kept separate from our attempts to understand the past (and contemporary kin based societies).

In the pre-Neolithic societies I know best, Proto Algonquian and Proto Central Algonquian, the significant local groups (regional groups, villages) were ideally large in-marrying kindreds. Ideally, coresidential politics coincided with kinship relations. One was not primary and the other secondary: they were the same thing.

In practice, of course, not everyone in these societies was able to find an ideal mate. A significant percentage married non-relatives. There are terms for such acquired relatives (true affines, in the modern sense). For example, a Proto Algonquian woman

distinguished her husband as either an insider to her kindred, or an outsider. Egos of both sexes distinguished between a child-in-law's parents who were kin, and those who were merely coresidents.

After a generation, however, newcomers were considered assimilated. No one ever distinguished parents, aunts, or uncles by their origin within the kindred or from outside it. Kindred membership was based upon long coresidence, not traced genealogical descent.

From the point of view of a Proto Algonquian, all long time coresidents were relatives of one sort or another, provided they married a group member. Coresidential politics were extremely important, but there was rarely if ever coresidence which did not turn into kinship.

It is less certain how Proto Algonquians named a family member who married out of the village (kindred). By Proto Central Algonquian times, however, such men were usually called by a distinct term, sometimes one otherwise used of an outsider who married in. That is, he was evidently thought of as an 'outsider relative', an irregular resident in the process of changing his group membership. A lack of coresidence was slowly turning into non-kinship.

There are no reconstructible Proto Algonquian or Proto Central Algonquian terms for outsider women. Perhaps women seldom changed group, or perhaps when they did they were immediately

fully accepted as kin.

Among the Neolithic Proto Indo Europeans, coresidence was again important. The most salient group was probably the extended household, whose residents were generally distinguished as to membership by birth vs membership by marriage or other close relationship to a female member (such as in the case of a man living in his sister's household). Again, assimilation appears to have taken one generation: parents are all classified as residents by birth, regardless of their origins.

When a large household broke up, as it evidently tended to do sometime before the founders' grandchildren grew up, those outside a young person's new household were soon reclassified as outsider relatives (aunts, uncles, grandparents).

The Proto Indo Europeans recognized matrilineal descent, but it added little to the kinship conferred by residence (which was wife centered). Kinship was essentially based on enduring coresidence, and coresidence formed the only significant political groups other than those of outsider allies.

Age and generation were the only basis of ascribed social status in Proto Algonquian society. Proto Indo Europeans also recognized the superiority of wife givers. It appears that sex per se conferred only slight prestige (to warriors or hunters) in Proto Algonquian society. Probably sisters outranked their brothers in pre-Proto Indo European times, but the superiority of wife givers would have



counterbalanced this once it became established (due to MCCM).

This explains why Kullanda (2002:89) finds that "Indo-European words commonly treated as kinship terms" are used "mostly in the plural as markers of belonging to certain social groups...". Kinship terms did assign people to coresidential groups and inform how long they had been there, as well as distinguishing their generation, affinal vs consanguineal status, and often sex.

In other words, kinship terms were political. They were used to fit people into the residence based social organization where they belonged by virtue of their relationship to a woman (a mother, by birth; a wife, by marriage; or a sister, presumably by invitation). They did not trace genealogy per se, as kinship terms in societies long lineal do, or those where factions are very important. They often did not assign specific individual places in a kinship network, as would be necessary in a society with complex rules of inheritance. Nevertheless, since affinity vs consanguinity are always clearly distinguished within the coresidential group, the proper label for these words in my opinion is still kinship terms.

## Chapter 7: Summary and Interpretation

Strictly speaking, nothing about the speakers of early pre-PIE is reconstructible. As guesses, they were probably foragers, who lived

in nuclear or minimally extended families, scattered about for better foraging (see table 7.1), and recognized bilateral descent.

The speakers of late pre-PIE evidently had a mixed economy, combining gathering and hunting with some simple horticulture or shifting agriculture, and the keeping of a few domesticated cattle.

Early Pre-PIE	Late Pre-PIE (c. 4500 BC)	PIE (c. 4000 BC)	
Economy	foraging	shifting agriculture, cow herding, foraging	----->
Warfare type	small scale external	intensive external and internal	----->
Marriage type	general	MCCM	MCCM with connubium
Superior	nobody	husband givers	wife givers
Marriage mode	agreement	----->	bride price
Man's sister	<u>*ynHte:r</u>	<u>*sweso:r</u>	----->
Woman's sister	<u>*ynHte:r</u>	----->	----->
"Binders"	???	daughters-in-law	----->
"Links"	???	sons-in-law	----->
Residence	???	wife centered	----->
Family type	minimal extended	----->	----->
Population	scattered families	concentrated hamlets	----->
Descent	bilateral	transitional	matrilineal
Organization	kin based	----->	----->

Table 7.1. The Early Periods of Indo-European Prehistory.

Evidently, climactic or other stress factors had brought about competition for scarce resources (and intensive warfare), in response to which they developed MCCM and wife centered (avunculocal) residence. The relocating groom 'linked' his family to his wife's, while the localized bride was their 'alliance binder.'

Because of his value as a warrior, the gift of a bridegroom made husband givers superior to husband takers. Because she was a husband giver, the bridegroom's mother had high prestige relative to her brother (the bride's father), and was also powerful relative to the bride. The bridegroom himself was 'lord' over his wife and her family.

Eventually, population was concentrated defensively in villages, and dependence on agriculture greatly increased, giving female farmers high prestige. Differential status by sex spurred the development of PIE sex gender (mainly feminine gender). This organizational revolution took place fairly late in pre-PIE times. It may coincide approximately with the establishment of archaeologically visible European agriculture, about 4500 BC (Smith 1995:102).

By PIE times matrilineal descent and connubium had developed, and power had shifted from husband givers to husband takers (wives and wife givers like mother's brothers). Husbands and nephews were no longer 'lords', and their mothers were no longer 'powerful'.

In all probability, an increased human fertility produced by the

high male mortality beginning in late pre-PIE times led to the breakup of PIE linguistic unity as people moved further and further in search of new lands to occupy. At the outer fringes of this expansion, there are signs of premarital avunculocality.

It would be another 2500-3500 years before plow agriculture was to bring about the patriarchal configuration ancestral to Classical Antiquity. In areas where the early plow could not be used, matrilineal organization may have survived until proto historical times.

PIE (c. 4000 BC)	Early Post-PIE	Late Post-PIE (c. 1000 BC)	
Economy	shifting agriculture, cow herding, foraging	----->	plow agriculture
Warfare type	intensive external, internal	----->	large scale external, internal
Marriage type	MCCM with connubium	----->	negociated alliance
Superior	wife givers	----->	about equal
Marriage mode	bride price	bride price	dowry
Man's sister	* <u>sweso:r</u>	----->	----->
Woman's sister	* <u>ynHte:r</u>	----->	* <u>sweso:r</u>
"Binders"	daughters-in-law	???	fathers-in-law
"Links"	sons-in-law	???	daughters
Residence:	wife centered	+premarital avunculocal	ambilocal but mainly husband centered
Family type:	minimal extended	large extended	----->
Population:	concentrated hamlets	villages	cities
Descent:	matrilineal	matrilineal	mainly patrilineal
Organization:	kin based	chieftaincies	states

Table 7.2. The Late Periods of Indo-European Prehistory.

# Appendix A: Anthropological Theory

Old anthropological theory is interesting in that it shows clearly the major interaction between it and political ideology. As we get closer to our own time, our own biases may blind us to these influences. Yet, we need to understand them, if we are to study reality, and not simply project our own unconscious fears or desires upon the past.

More recent theory is fragmentary and synchronic, but once the fragments are assembled and ordered temporally, it helps one understand exotic societies like the Proto Indo European one, that otherwise might seem quite baffling.

## A.1. Early Accounts of Unilineal Descent and of Domestic Authority

Anthropology was born in the latter part of the nineteenth century, a time when the traditional roles of women and men in society were being fiercely debated. In these early years, the most influential anthropological ideas were those derived from the brilliant speculative hypotheses of Henry Louis Morgan.

When Morgan wrote *Ancient Society* in 1878, it took its place alongside *The Origin of Species* in proposing revolutionary ideas which tended to undermine the intellectual underpinnings of the social order of his time, and became a source of inspiration for Marxists (Engels 1884).

What made Morgan's theory so revolutionary was his idea that the family had not always taken the forms then familiar to his audience (patriarchal and monogamous). Rather, he claimed, it had gone through three earlier stages of great duration, during which women had sexual and personal freedom.

Moreover, patriarchy and monogamy were not seen as the final outcomes of this evolution. Instead, he suggested at the conclusion of his discussion of what he called "the monogamian family" that "it is at least supposable that it is capable of still farther improvement until the equality of the sexes is attained. Should the monogamian family in the distant future fail to answer the requirements of society ... it is impossible to predict the nature of its successor."

Morgan lived in upper New York state, and he had noticed that the kinship terminology of his Seneca neighbors (and of the other Iroquois peoples) differed from those more familiar to him in a very interesting way. Among these peoples, a MOTHER'S SISTER was called "mother", a FATHER'S BROTHER "father". Consistent with this, their children were classed not as cousins, but as siblings (sisters and brothers).

In contrast, a MOTHER'S BROTHER'S children and a FATHER'S SISTER'S children were classed as cousins, and their parents as aunts and uncles.

A mother's sister's children are said to be parallel relatives, because mother and her sister are of the same (parallel) sex; a

mother's brother's children are said to be CROSS relatives, because mother and her brother are of different (cross) sexes. The difference between parallel and cross relatives, especially cousins, turns out to be of fundamental importance in many societies, as we shall see.

Returning to Morgan's theories, he noticed that, for example, the Senecas have sibling terms and cross cousin terms, but no terms for parallel cousins, because among the Senecas parallel cousins are called by sibling terms. Morgan interpreted such assignment of the same label to two or more kin types as evidence that in origin people had not been able to distinguish the kin types so labeled, and his speculations about prehistoric societies focused on such lacks of distinctions, and their ultimate socio-historical causes.

Morgan reasoned that a terminology such as that of the Iroquois would only arise in a society where families consisted of either a group of sisters collectively married to unrelated men, or a group of brothers collectively married to unrelated women (Tooker 1985:ii). Because in such families they wouldn't know exactly who had begotten them, the children would call all the adults in the family "mother" and "father", and other kin terms would conform with this pattern.

For example, a man's brother's children would be equated with his own because he and his brothers might share the same wives; and a woman's sister's children would be equated with her own because she and her sisters might share the same husbands. But children of



cross sex siblings (a woman's brother and a man's sister) would NOT be equated with one's own because siblings would not marry each other. Morgan called this family type the Punaluan.

Morgan then went on to examine other kinds of kinship terminologies, and to explain their origins in differing types of hypothetical prehistoric families. He proposed a Consanguine Family, where the regular form of marriage would have been between sets of sisters and their own collective sets of brothers, as the oldest type.

The sets involved in these group marriages could be quite large, as in some societies cousins are all classified as siblings (sisters and brothers) - and in some sisterhood and brotherhood are extended to the whole clan. This would mean an individual woman might simultaneously have dozens or even hundreds of "husbands" (and men "wives").

Morgan's evidence for this "consanguine family" consisted of kinship terminologies of the type used by the Hawaiians. In this type of system, all relatives of one's own generation are "siblings", all those of the first ascending generation are "parents", and all those of the first descending generation are "children". For this reason, the terminology is sometimes called Generational.

Morgan (1985:410) reasoned as follows: "Speaking as a Hawaiian, all the wives of my several brothers are my wives as well as theirs. As it would be impossible for me to distinguish my own

children from those of my brothers, if I call any one my child, I must call them all my children. One is as likely to be mine as another." And so forth.

Another consequence of the theory of unknown (or at least imprecisely known) paternity was that people would not be able to trace their descendants or ancestors through men. Any system of descent must then necessarily be matrilineal, that is, descent would have to be traced through mother-child links only.

From this, Morgan concluded that matriliney was the normal state of affairs in early prehistoric times, and patriliney a relatively recent innovation. Moreover, since in matrilineal societies a mother presumably knew who her children were, and a father did not, any parental authority would naturally be exercised by a mother.

Also, since in simple societies political authority was known to be based on kinship, it too would be expected to be in the hands of women. This he no doubt felt was confirmed by the great authority of the Seneca "clan mother". Thus, matrilineal descent was taken to be an index of erstwhile matriarchy.

The latter half of the nineteenth century saw the first wave of feminism, and Morgan's hypotheses were bad news indeed to defenders of patriarchy. Up to that time, they had been able to claim that the patriarchal order had existed from the beginning of time, and was natural and immutable.

Fortunately for them, however, Morgan was not alone in proposing exotic origins for the human family. The then recent discovery that humans were primates, and that primates had a variety of family forms, no doubt stimulated the scientific imagination. Darwin proposed that in the earliest form of the human family "each male would firmly keep his women to himself, in the manner attributed to the gorilla" (Lang 1908:139).

One of the best known proposals of the Darwinian type was the one put forth in *Totem and Taboo* by Sigmund Freud, who postulated an original family consisting of one dominant male plus his wives and descendants. This patriarch would have demanded celibacy of all the males of the family other than himself, and would have reserved for himself the work of impregnating all the women of the group (wives, daughters, granddaughters).

The second form of the family, according to Freud, would have come into being when the old man's sons killed him and ate his body. Feeling guilt about this, they then voluntarily renounced sex with their mothers and sisters, creating the first incest taboo.

Of course, these myths by themselves were initially a poor match for Morgan's, which at least was supported by large amounts of anthropological and linguistic data, however poorly understood. But schools of anthropology soon developed dedicated to refuting Morgan, and they dominated anthropology into the late twentieth century.

Even in Morgan's day, the ethnographic evidence was clearly against group marriage: the Iroquois didn't have it, nor did any of the other numerous societies with Iroquois (Punaluan) terminology. So Morgan had to propose that kinship terminologies are very stable, and that the peoples with this type of terminology had simply preserved them as archaic survivals from a very remote epoch when this type of marriage was the rule for much of the human race.

However, it was soon observed that kinship terminologies changed rapidly in response to changes in social organization, and hence that survivals of the kind Morgan proposed were unlikely for more than a few years. Moreover, the direction of change was not always that which he had predicted.

To choose one example among many, the Delaware Indians are descendants of the Proto-Algonquians. They have Hawaiian cousin terminology (supposedly reflecting the most primitive stage of kinship organization), but their Proto-Algonquian ancestors had Iroquois cousin terminology (a supposedly later stage).

It's a curious feature of the human mind that a proposition, once some key aspects of it have been refuted, is often taken to imply its opposite. For example, in the movie *Witness for the Prosecution*, a wife gets her husband acquitted on murder charges by testifying against him, and doing it so unconvincingly that she discredits her own testimony.

The jury reasons: "she says he's guilty, she's lying, so he must be

innocent." Scholars too regularly "throw out the baby with the bath water". When some aspect of a hypothesis is refuted, even if only a minimal part of it, the exact opposite of the whole hypothesis often becomes established doctrine, without any evidence that the new hypothesis is really true (or even very plausible).

In the present case, abandonment of the notion that survivals from prehistoric times explained systems of kinship terms led to the abandonment of all ideas about social evolution, except some lingering interest in the fluctuations among the types of social organization attested historically.

Instead, there emerged the opposite notion, codified in a doctrine of Uniformitarianism borrowed from evolutionary biology. However, biologists had only claimed that the physical processes and contexts that governed evolution had remained relatively stable over the course of the evolution they described (thus excluding Noah's flood as an explanation).

The anthropological version was much more radical. By their doctrine, one is required to assume that on average the social world never changes significantly. Supposedly, only the present social variation has ever existed, and no other ever will. Supposedly, the present is to be explained exclusively in terms of itself, without any reference to the past. The practical effect was to define the past as lying outside the proper domain of science.

One of the most influential men of this period was Radcliffe-

Brown, and in his famous Introduction to African Systems of Kinship and Marriage he repeatedly warns against "pseudo-historical" theories and "false ideas", "invented by imagining". He goes on to complain that "this legacy of erroneous ideas is only gradually being got rid of by field studies aiming at the analysis of social systems as they are without reference to their origin ..." (Radcliffe-Brown 1964:1, originally published in 1950).

He argues that due to a lack of written historical records "we cannot have a history of African institutions" (nor, presumably, Australian, North or South American, etc.). Then, with both diachronic eyes firmly shut, he goes on to assert that "while there is a very wide range of variation in their superficial features there can be discovered a certain small number of general structural principles" governing societies (Radcliffe-Brown 1964:2).

If one accepts this, the possibility that these "general structural principles" might be recent developments cannot be investigated. The only change allowed for is of the sort he himself calls superficial.

Radcliffe-Brown doesn't mention names at this point, and one might think he had Freud's theory in mind as much as Morgan's. But later, speaking of group marriage, he specifically says "this fantastic example of pseudo-history was put forward by Lewis Morgan in his *Ancient Society* (1885)" (Radcliffe-Brown 1964:23).

Also, it is surely Morgan's school of ideas he is opposing, rather

than Freud's, when he speaks of possessive rights as rights in rem, under which a man's "child, apprentice, and wife are in fact held to be things", and insists that "in the formation of systems of kinship and marriage these possessive rights over persons are of great importance" (Radcliffe-Brown 1964:12).

The adherents of social Uniformitarianism sometimes propose it as only a methodological assumption, to be sure, but this doesn't justify it. Making a methodological assumption that leads to a particular conclusion in such cases, and then treating that conclusion as established by the methodology, is simply circular reasoning.

One need not be a scholar to realize that the world has changed a great deal in the last 6000 years, or to predict that it's likely to change a great deal in the future. Moreover, much of this change is presumably more or less unidirectional, and thus evolutionary. It's not just a matter of superficial fluctuations.

Besides the flaws found in Morgan's hypothesis, subtler factors help explain why the views propounded by his critics convinced so many for so long. One factor may be the effects described by Evelyn Fox Keller in *Reflections on Gender and Science* (1985:10): "scientists, as human actors, find some pictures or theories more persuasive and even more self-evident than others in part because of the conformation of those pictures or theories to their prior emotional commitments, expectations, and desires."

Another reason for the persistence of the Freudian and

Darwinian speculations is that, never having been buttressed by evidence of any sort (except the analogy with the gorilla), they could not be refuted. In contrast, Morgan's explanation was supported by mountains of linguistic data, which could be subject to alternative explanations, and eventually were. Hence, critical attention was focused mainly on Morgan, and diverted from the alternatives to his views.

Whatever the reasons for the passion and extraordinary persistence which went into attacking Morgan's ideas for half a century or more, the result has been an intricate web of anti-Morgan doctrines, assumptions, and unconscious biases which were imposed upon generations of anthropology students.

Careers were built around the acceptance and promotion of these doctrines, never around questioning them. The few stubborn doubters were easy to dismiss and ignore. They could be identified as Marxist ideologues (which in fairness some may have been), rather than "objective" scholars.

I well remember after 30 years the sharp tone substituted for rational explanation, when my classmates asked the wrong questions in graduate school. We didn't ask a second time. To quote Keller (1985:11) again: "predilections based on emotional (as well as social and political) commitments express themselves precisely in the domain of those social and linguistic practices that help determine, within the scientific community, the priority of interests and the



criteria of success." We wanted good grades, and quickly learned not to challenge irrationally held doctrines. "Political Correctness" isn't new, just the current ideas about what is "Correct."

From a political perspective, the most obvious and important dogma which grew up in response to Morgan is that men dominate women in all societies, and always have. Coupled with social Uniformitarianism, it implied that they always will.

Indeed, most of the other dogmas seem designed to buttress this view, directly or indirectly. Presumably, this was meant to discourage and demoralize those who might oppose patriarchy. But when one looks at the matter closely, what it really suggested was that the proponents of eternal patriarchy greatly feared change. They would not have feared it if they didn't believe at heart that it was possible. Why else would they have devoted so much energy to making sure it didn't happen?

Of course, even if the oppression of women had been pervasive in the past, the future would still be another matter. Social Uniformitarianism is a preposterous proposition. New things do happen all the time, and there is no known limit on this. The main reason we study the past is not to see what the future will be like, but to learn how not to repeat mistakes.

Before leaving the topic of the primitive human family and its sexual mores, one may wish to take a quick look at an example of the reality which the matriarchical and patriarchal myths of the

nineteenth century were supposed to explain. This is a vast topic due to societal variability, but some sense of it can be gained from even a brief anecdotal account.

Despite his politically progressive views, Morgan was still a Victorian man. He could imagine biological paternity being unknown, but never simply unimportant.

Yet, it's quite clear from the many collections of Native American texts that the important men in a boy's life were the one who hunted for him, the one who gave him a name, the one who taught him the skills of manhood, the one who guided him in his Vision Quest. Rarely if ever was there any interest in knowing who made love to one's mother nine months before one was born.

In accord with this, biological paternity was unimportant, which gave a woman greater flexibility in her personal choices. Male jealousy does occur in Native American societies, but it's generally viewed as simply a comical or tragic character flaw, a sign of personal weakness, not an essential pillar of the social order.

For example, the patrilineal Prairie Potawatomis tell the story of a teenage boy who is sent by his married older brother to get him some tobacco from his wigwam, and who is there seduced by the brother's wife. After a while, seeing that his younger brother isn't coming, he goes to get the tobacco himself and finds the two making love.

Being jealous, he says "Very well. You want my wife, have her! As for me, I'm going hunting." He then persuades the whole camp to go buffalo hunting with him, leaving the lovers behind to starve, since these are hunting people, and his younger brother is not yet a hunter. The old people try to talk him out of it, but despite this and his younger brother's entreaties, he insists on his vengeance.

Seeing that they're abandoned, the young man falls weeping at the feet of his lover. "Alas, sister-in-law", he says, "I have brought us to ruin! Can you ever forgive me?" "Take heart," she answers. "Perhaps a Higher Power will assist us. As for me, I'm more than pleased with my new husband."

She makes him a bow and arrow, and blackens his face and sends him on his Vision Quest. He finds a Spirit Protector, and quickly becomes a skilled hunter. When some passing band of hunters stops to visit, he gambles with them and wins a gun and some horses. He kills many buffaloes, and his wife preserves the meat and hides.

After a year, a bedraggled band of starving Indians limps into his camp. As his wife feeds them, he recognizes them as the people who abandoned them earlier. He heaps presents on them, and tells them how glad he is to see them again. The next day, he leads them on a successful buffalo hunt, after which they beg him to be their chief. He then insists that his older brother move into his lodge. "And do not by any means hesitate to show affection for my wife," he tells him. "After all, you are my own brother."

As this and other stories show, the ideal in many hunting and gathering societies is for brothers to be very close (and sisters as well), particularly when they live in the same camp. In most simple hunting societies men like to stay in a territory they know well, hunting cooperatively with other men they know well. Hence, after marriage they tend to stay in the community where they were born, something called patrilocality if they have patrilineages, and virilocality otherwise ( viri- from Latin vir 'man'). Both are forms of husband centered residence.

In some agricultural societies where women work the soil, their need to know the microclimate of their gardens and work cooperatively with women they know is more important. Moreover, women benefit from mutual assistance in child raising, particularly if they have many children.

In such cases men sometimes move to join their wives after marriage, and we say that residence is wife centered: matrilocal if they have matrilineages, and uxorilocal otherwise ( uxori- from Latin uxor 'wife'). In any case, either sets of brothers or sets of sisters generally try to live together. Moreover, if a set of sisters marries a set of brothers (something called Sibling Set Marriage), both sets of siblings live together.

Of course, sibling set marriage is hard to achieve for individual families. The law of averages produces about the same number of females and males in a large group, but this tendency diminishes as

the group gets smaller. Consequently, there's a strong tendency for sibling sets to be enlarged by including parallel cousins when a society values sibling set marriage. Better yet is to work with lineage or clan "siblings" (clan sisters and clan brothers), as is often done rather systematically in more populous societies, creating lineage alliances.

Most commonly, in order to maintain these alliances across generations, sets of lineage brothers marry sets of their mother's brother's daughters, something called Matrilateral Cross Cousin Marriage or just MCCM. It encourages a woman to try to marry one of her father's sisters' sons (or any of their clan brothers, whom she would call "male cross cousins related through my father").

Her clan sisters all try to do the same, and so mainly end up married to men of the same clan. Of course, this links the two clans in a strong kinship alliance.

More rarely, in matrilineal societies, clan or lineage brothers marry cross cousins outside their matrilineage, that is, father's sister's daughters, something called Patrilateral Cross Cousin Marriage or just PCCM. Both types of Unilateral Cross Cousin Marriage (UCCM) tend to keep same sex siblings and lineage siblings together, and to form strong kinship alliances.

When same sex siblings live in the same camp, they often are substitutes for one another, notably caring for each other's orphaned children and widowed spouses. Hence, the ideal attitudes required

of a man's wife and of his brother, and of a woman's husband and of her sister (particularly one living in the same camp) are those that make secondary marriages between them easy and natural.

They are expected to be in some sense "sweethearts", and indeed the traditional Algonquian term for a sweetheart is "little cross sex sibling-in-law". Sensible people didn't get too upset if they sometimes got a bit carried away with their demonstrations of affection.

There was no group marriage, but the units that Morgan identified as engaging in it were often in fact groups of "sweethearts" and, in circumstances of high mortality, serial spouses. In some societies with high male mortality, like the Cheyennes, "sweethearts" were simply called "husband" and "wife" or, as in the Blackfoot case, "distant husband" and "distant wife".

Although the children of such groups commonly called each other "sister" and "brother" without distinction, they knew who their individual mothers were (the woman who cared for them), and assumed that their mother's husbands at the time of their birth were their fathers. They didn't worry about the possibility of error, because it didn't much matter to them. And none of this prevented them from calculating descent in the male line if they so chose, as the Potawatomis in fact did.

Judging by this, Morgan's account, however flawed historically, is at least fairly true to the values and psychology of the exotic people

he knew best: the North American Indians of the Northeast. Darwin's and Freud's myths, in contrast, tell us much about the dark side of Victorian male psychology, but nothing at all about American Indians.

Morgan's and Freud's mythical accounts provide the intellectual context within which, throughout much of the twentieth century, anthropological questions have often been formulated. One example of this is particularly interesting in the present context. As late as 1963, it was still argued in a journal of no less prestige than the *American Anthropologist*, that UCCM originated in "oblique marriage" between egos and their aunts and uncles (Moore 1963).

The claim was that, for example, in a patrilineal society a husband might acquire the right to take his wife's brother's daughter or his sister's daughter as a secondary wife. However, if his own son couldn't find a wife, he might alternatively relinquish this right to his son which, if it became regular practice, would tend to produce MCCM since men would be marrying their mother's brother's daughters. Where oblique marriage is supposed to have come from is not explained.

Not only is the logic here of the sort that led Morgan into error, it's combined with Radcliffe-Brown's elaboration of Freud's myth, notably including the idea that women are property to be given away on whim (possibly true in some societies, but surely not all). The worst of both mythical traditions had come together.

Fortunately, these academic traditions were far from the concerns of Irawati Karve when she set out as a young public health nurse with the task of immunizing children in the hundreds of Indian villages she was to visit in the course of a long career. Initially distrusted by peasant women as an outsider, she set about winning their confidence by talking about their children, and then about other family members.

Soon she was keeping notes of her conversations, and trying to understand the complex and variable kinship terminologies of each region and language group in India. Guided only by such scientific method as she had learned in nursing school, by her own good sense and intelligence, and by the help she eventually sought from matchmakers and other village people with an interest in the matter, she came to understand the social organization of all the peoples of India.

By the time a chance academic acquaintance persuaded her to publish her notes (Kinship Organization in India), she had genuine answers to most if not all the questions which had provoked a century of bizarre academic speculation in the West.

In particular, she had a simple explanation as to why some Dravidian peoples favor marriage between a woman and either her mother's brother or his son: after collecting genealogies, she saw that among Dravidians "the kin in the immediate family is arranged not according to generations but according to age categories" (Karve



1965:250).

When a woman married her mother's brother, it was her mother's MUCH YOUNGER brother, someone within the usual age range of a cross cousin. Thus paradoxically, myth-bound India had produced a simple scientific explanation for observational data which the scientific West could only explain mythically.

As to why some societies have strict generational endogamy (that is, require marriage within one's own generation) while others do not, Elizabeth Dole (1991:386) points out that, in the Amazonian region, lack of generational endogamy "appears to be an adaptation here to a scarcity of eligible mates in one's own generation".

Returning to speculations on the consequences of oblique marriage, one of these has been thought to be the development of Crow and Omaha kinship terminology. A Crow terminology is one in which the term for an older woman is extended downward to other women of father's matriline, e.g., a father's sister's daughter becomes a terminological "father's sister" like her mother, and other terms are adjusted reciprocally.

Usually Crow societies are matrilineal, and an Omaha terminology is roughly speaking the patrilineal mirror image of a Crow one. The reasoning deriving Crow and Omaha kinship terminology from oblique marriage is that, for example, if a man can marry women of two kin types, he may come to call them by the same kin term.

Or, more or less equivalently, that if a mother and daughter are called by the same term, it may reflect the fact that the daughter is the mother's heir of some important property or status, such as being someone's potential wife (see Moore 1963:307-308). Once generational skewing of some kin terms is produced in this way, it can be extended analogically to produce full Crow-Omaha systems.

Because oblique marriages really do exist in several societies, which usually have UCCM and often Crow or Omaha terminology, these speculations have survived much better than Morgan's. But, are they and others like them really correct?

If they are not, then some of what little we think we know about social prehistory is illusion. Traditional speculations, even once abandoned, have continued to influence the INTERPRETATION of otherwise perfectly valid cross cultural research results. To this extent, our present theories about social evolution are still shaped by nineteenth century myths.

Alternatively, other scholars have acknowledged that in the current state of affairs they were unable to provide a diachronic account of their data. For example, in the conclusions to her landmark study of domestic relations in matrilineal societies, Alice Schlegel (1972:136) speaks frankly about the limitations of her study as follows:

"where it has been least successful, in my opinion, is in indicating the socio-cultural factors leading to the three domestic

authority patterns, or the inputs. This is particularly true for differentiating between those inputs leading to Husband Dominance on the one hand and those leading to Brother Dominance on the other. ... This question will require further research."

In conclusion, while many interesting questions have been raised about prehistoric social systems, we really don't know much about them (other than what we can infer from synchronic sources). Hence, any investigation of them must begin with essentially a blank slate. Yet, social anthropology has made a great deal of progress in synchronic analysis over the last century, providing insights without which I could never have understood my Algonquian and Indo European data. These insights and their limitations are the subject of section 2.

## A.2. Insights From Synchronic Studies

This section summarizes much of the best twentieth century anthropology of exotic social organization. It is rather complex.

Schlegel's work on domestic relations continues a long twentieth century tradition of anthropological studies dealing with the disadvantaged position of women relative to men in most human societies. Although the vagueness and generality of the earlier of these works leave the modern scholar unsatisfied, especially as to proposed causes, it is perhaps worth mentioning a number of insights from these and other sources which have been useful in understanding the Algonquian and Indo European case histories.

From its beginnings in the nineteenth century, the distinction between matrilineal and patrilineal descent has been associated in anthropological theory with the question of the cross cultural status of women, and it still is (see Sanday 1981:177-178).

Early understanding of the matter was necessarily very crude. Patrilineal societies such as those of the early Hebrews and Romans were known to be patriarchal, and so matrilineal ones were sometimes assumed to be (or to recently have been) matriarchal. And while field work never produced any evidence of this, it did show that in at least some matrilineal societies women were held in high esteem and treated much as equals with men. It also confirmed that the degradation of women was very widespread in the more complex patrilineal societies.

In 1937, Simmons (1967:241-243) did a statistical analysis of a world-wide sample of 71 societies, testing a large number of the hypotheses current in his day. In particular, he was able to summarize his findings on "the skein of cultural forces influencing the status of woman" as follows: "suffice it to say, that the status of woman appears decidedly influenced by maintenance mores (subsistence economy) and the family organization."

Building on this and the work of several other scholars, Murdock (1949:184-225) concluded that economics governed social status, which was the main influence on residence norms, which in turn governed the type of unilineal descent and kinship terminology

which might then emerge. These insights remained important in the work of many later theorists (e.g., Janet Saltzman Chafetz 1984:113).

Nevertheless, in addition to the central importance of agriculture, Murdock (1949:205) mentioned several minor factors as capable of tipping the balance in favor of a high (that is, equal) status for women or against it, noting in particular that high female status requires "relative peacefulness ... for war enhances the importance of men".

In 1971, Ember and Ember demonstrated the importance of internal warfare (feuding, civil war) as a factor determining the type of unilocal residence. Divale (1984) then proposed an explanation of the origins of matriliney based on episodes of intensive external warfare<sup>40</sup> (that is, genocidal war with foreign enemies). This proposal suggests contra Murdock that war may sometimes indirectly enhance the importance of women.

Both views show up (in different contexts) in Chafetz (1984:86-89 vs 72-73), who sees chronic warfare as enhancing the status of men, but long male absences and high male mortality during intensive external warfare as opening up opportunities in "previously male-monopolized activities" for women.

Another body of twentieth century research established a strong

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<sup>40</sup> Correctly, I think, except in viewing all cases of avunculocality as part of a transition from matrilocality to patrilocality.

statistical correlation between matriliney or uxoriolocality and communal polities, versus patriliney or virilocality and factional or mixed (factional/communal) polities (Swanson 1969, 1974, Paige 1974).<sup>41</sup>

Kin-based factions form around Fraternal Interest Groups (FIGs), consisting of co-resident brothers and usually clan brothers, who prioritize their special interests above the welfare of the whole society (e.g., see Swanson 1969, Paige 1974, Ember and Ember 1983:chapter 9). Factional polities legitimize the pursuit of special interests, and often tolerate feuding and other forms of internal warfare.

In contrast, communal polities generally scatter brothers (typically, by matrilocal residence), emphasize multiple cross-cutting loyalties, and tend to produce social peace, consensus, and common objectives.

Factions can be seen as an instance of what sociologists call "strong ties" (between a few close friends or relatives) in contrast with "weak" ones (with acquaintances, fellow tribesmen, and the like).

Quoting Pool, Granovetter (1983:210-211) says "the utility of weak links is a function of the security of the individual ... A highly insecure individual ... is under strong pressure to become dependent

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<sup>41</sup> Since descent is a very good index of residence, the more significant correlation here may be between polity and residence.

upon one or a few strongly protective individuals. A person with resources on which he can fall back can resist becoming dependent on any given other individual and can explore more freely alternative options".

McKinley (1971a, 1971b) reviews the extensive and diverse literature on the genesis of Crow-Omaha societies (those with skewed cousin terms) and goes on to propose a better account. By such skewing, one terminologically transfers more distant consanguines such as cross cousins into classes of closer ones, an alternative to forging closer links through new marriages.

According to McKinley, the extension of terms for close affines of the first ascending generation to their descendants is a way of retaining close affinal alliances without the necessity of renewing them through new marriages (for at least a few generations). This should be useful, he surmises, for people who feel the need to form new affinal alliances while retaining the old, that is, it permits a system of dispersed alliances as opposed to concentrated ones.

Consistent with Granovetter's insights, McKinley suggests that dispersed alliances should tend to develop mainly in societies with good internal security (a lack of internal warfare). Hence, he predicts that Crow-Omaha cousin terms will be found mainly in tribal societies, that is, in societies with territorial organization capable of discouraging internal warfare.

Throughout the two papers, McKinley emphasizes his view that

kinship terminologies do not merely reflect social realities, they are designed to help create them. In his words, "no element of social reality (e.g., kinship nomenclature) is strictly passive in its relation to the rest of social life".

Rather, "the formulation of a terminology now becomes an act of construction, a part of man's 'world building activities'". And, "as such it becomes immediately involved in the construction and maintenance of kin relationships themselves" (McKinley 1971b:410-411). To the extent that he is right, this means new terms are most likely to originate at about the same time as the new social arrangements they describe, and not later.

Although in simple kin-based societies there is seldom a clear dividing line between public and private domains (see Mascia-Lees and Black 2000:58), the studies which treat them separately can be taken as exploring the opposite ends of the continuum of activities which correspond to these two domains in more complex societies. Moreover, in my Algonquian case histories the two ends of this continuum do behave somewhat differently. Hence, it's fortunate that we do have the intellectual tools to distinguish them.

As noted above, in 1972 Alice Schlegel classified 66 matrilineal societies into 3 types solely by their DOMESTIC relations: Neither Dominant (where women are relatively autonomous), and those where women are dominated by their husbands (Husband Dominant), or their brothers (Brother Dominant).



Of these three types, only Neither Dominant ones closely fit the stereotype of matrilineal societies as granting equal or nearly equal status to women. Neither Dominant societies are nearly always matrilocal, and indeed matrilocality is the only factor Whyte (1978a) found to have significant value for predicting a high (that is, equal) status for women.

In a somewhat complementary study, Sanday (1981: appendices E-F) developed criteria for classifying societies into 3 types by the status of women in the PUBLIC domain: Equal, Unequal, and those with "Mythical" Male Dominance.

Another body of literature focuses upon the distinction in stateless societies between factional and communal polities (Paige 1974, Swanson 1974). On the whole, patrilineal societies tend to be factional, and matrilineal ones communal. That being the case, patriliney can be taken as an index of factionalism, and matriliney an index of communalism.

As a result, the vast literature on unilineal societies helps complete the picture of the factional and communal polities of which they are indices. For example, patrilineal and virilocal societies tend to participate in what Divale and Harris (1976) call "the Male Supremacist Complex", characterized by militarism, the oppression of women, and sometimes female infanticide.

In contrast, "inter-group alliance is generally strong and group-exclusiveness weak in matrilineal systems", "matriliney in itself does

not provide a strong authority structure" - leaving room for individual initiative, and matrilineal societies provide for "open recruitment of talent and manpower, strong inter-group alliance, (and) scope for achievement" (Douglas 1971:128-129). This suggests that, overall, matriliney - and thus communal polity - is not only associated with equal status for women, but a higher quality of social life for all members of society.

Now we come to a difficult but important point. Often the Fraternal Interest Group is seen as a main cause of sexual inequality, and egalitarian societies are explained by identifying them with matrilocality and pointing out that this type of residence disperses blood-related men "and thereby discourages them from consolidating their political power" (Nielsen 1990, summarizing Martin and Voorhies).

However, since Schlegel has shown that there are two types of domestic Male Dominance, a single causative factor such as the Fraternal Interest Group is inadequate to explain them. There is an obvious ethnographic association of Brother Dominance with matriliney, but Husband Dominance is also fairly common in matrilineal societies.

However, for matrilineal societies with UCCM, Schlegel (1972:68-69) provides a valuable clue as to the origins of dominance types by showing a significant statistical association between Brother Dominance and PCCM, and Husband Dominance and MCCM.

Although this statistical correlation is hard to interpret out of historical context, Schlegel's explanation for it in matrilineal societies is insightful in that it recognizes the role of UCCM in the consolidation of power by older men over younger. An apparently alternative explanation proposed by Eyde and Postal (1961) also helps explain the Algonquian and IE case histories in that it associates MCCM with avunculocality, and suggests that in time MCCM tends to be replaced by PCCM in Crow societies.

Showing how these factors may interact in historical context to produce Schlegel's three types of domestic relations is one of the major achievements of my second Algonquian study (Proulx 2005c, too complex to even summarize here). It also illustrates well the problems with limiting oneself to synchronic data as proposed by the classical social Uniformitarians.

Despite the great quantity of high-level scholarship devoted to the question over the years, none of the several competing out-of-context theories on the origins of UCCM has been convincing enough to produce a consensus. The main problem, as I see it, is that without any notion of temporal sequence, they're unable to distinguish an original cause from its primary or secondary effects.

Hence, there are few constraints on the hypotheses proposed, and they proliferate inconclusively. I submit that any credible theory about the origins of either UCCM or of Male Dominance (or of the relative lack of it), must be able to explain why the two are

statistically associated, and it must be subjected to diachronic constraints so that cause can be distinguished from effect.

Another great weakness of synchronically-based theories of social institutions - one that I've never seen acknowledged - is that, being statistical-correlational in nature, they tend to mainly deal with relatively frequent configurations. Because only stable steady-state societies endure long enough to become cross culturally frequent (see Hawkins 1990:101), such studies tell us little or nothing about the unstable transitional configurations which best reveal the origins of the stable ones.

What increases the difficulty is that an organizational solution to a social problem, to the extent that it's successful, does away with the problem which gave rise to it (that is, an effect often systematically obscures its cause). Hence where a purely synchronic study concludes that "X is present if and only if Y is absent", leaving the impression that Y prevents X, the truth may instead be that "X is a successful solution for Y" (or even "Y causes X"). Without diachronic information, there's no way to tell.

The study of a long and complex case history, one with regional variants partly parallel yet differing in significant aspects, provides a wonderful constraint on synchronically-based theories of social institutions. Only those which can plausibly explain the reconstructed events (or be modified to do so) need be retained, and all those retained must fit together in an integral whole rather than

be seen as unrelated fragments. Such is the effort undertaken in my second Algonquian study (Lenapean).

This is important, for although the simpler forms of social organization there examined have little future on this planet (barring a collapse of civilization), the principles of social psychology they reveal as governing the historical evolution of societies are presumably still operative.

My Lenapean study involves the complex account of a cluster of Algonquian societies located along the Atlantic Seaboard, over several turbulent centuries, adapting variously to partly similar and partly different circumstances.

The conditions are favorable for reconstruction: the ancestral proto-society (Proto Algonquian) has been reconstructed in enough detail to provide the necessary background (with a few details added), and many of the subsequent kin term innovations have identifiable etymologies, suggestive of systematic organizational changes.<sup>42</sup>

From time immemorial, the Algonquian peoples have tended to

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<sup>42</sup> Serious work on the reconstruction of Proto Algonquian society began in 1964, with Hockett's reconstruction of much of the Proto Algonquian kinship terminology, on the basis of which normative cross cousin marriage could readily be inferred (Hockett 1964). Subsequently, a consensus emerged that this cross cousin marriage was bilateral (Hickerson 1967, Callender 1978a, Proulx 1993). Next, Siebert (1967) reconstructed terms for trees and animals, indicating that the Proto-Algonquian people lived in southeastern Ontario (by his estimate, about 3000 years ago). Pentland (1979) reconstructed some technological terminology suggesting that they were a hunter-gatherer-fisher society.

treat their kinship terminologies like a constitution, to be amended with each socio-political change of consequence. Moreover, early social subdivisions and some later outmigrations from the core innovating area means that later innovations in some of the Lenapean societies usually didn't entirely wipe out the evidence of earlier ones, and hence that some relative dating is possible.<sup>43</sup>

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<sup>43</sup> Descriptions of Lenapean institutions (like that of Proto Algonquian society) all depend on lexical reconstruction. Other methods have been attempted, but I've argued elsewhere (Proulx 1993) that these are totally unreliable.

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## Glossary

accessible = entities in their normal state (alive, awake, present, functional).

affines = relatives by marriage.

agnatic succession = inheritance in the male line.

Algonquian = a family of languages spoken aboriginally in much of northeastern North America. The best known languages of the family are Cree, Ojibwa, Menominee, and Fox.

alternation = one thing in one situation, another in another; a set of alternatives.

animate gender = a partly arbitrary classification of nouns, implying that the entities they name can move about of their own volition, whether in the real world or in Myth. Many objects which don't appear to ever move are included.

avunculocality = residence with the husband's mother's brother.

big game = animals that move over large areas, requiring the hunter to pursue them long distances.

bride service = a year or two spent with his parents-in-law by a newly married young man, proving he is capable as a hunter.

bridging argument = something (B) that would explain how things could go from A to C.

carbohydrates = foods the body burns for energy, when available. When they are not available, the body burns protein for energy. Carbohydrates, when available, are thus protein-sparing.

carrying capacity = the number of persons per unit which the land will support at a particular level of technology.

clan = a group among which one sex considers all its members to be siblings, regardless of actual relationship.

classificatory kin terms = terminologies that merge two or more kin types.

comensal group = the group that normally shares meals.

complementary distribution = where one is, the other isn't.

compounds = two words put together as one.

contraction = making shorter.

cross cousin marriage = marriage with the child of a parent's cross sex sibling (a mother's brother, or a father's sister).

cross kindred = relatives traced through the cross (opposite) sex siblings of one's ancestors and descendants.

Crow = a type of matrilineal organization; the kinship terminology that goes with it. Cross cousins are equated with closer relatives of older or younger generations.

culture = way of life, including customs, habits, economy.

deme = the in-marrying group among foragers. Also called the marriage isolate, and dialect tribe.

derivation = the rules for building words or word stems.

Descriptive kin terms = terminologies with a separate term for each kin type.

dialect = all the forms of speech that are nearly identical. A language often has several regional dialects.

Differential Development = examination of how a feature of a protosociety developed differently under different conditions (in different societies), as a clue to its original nature.

doublets = two nearly identical words, usually the same word originating in different dialects.

Eastern Algonquian = the Algonquian languages along the Atlantic shore of the U.S. and the Canadian Maritime provinces. Once thought to be a genetic grouping, they represent 4 separate migrations from the Proto Algonquian homeland.

endemic warfare = warfare that goes on and on.

endogamy = marriage within the group.

etymologies = original, literal meanings.

"fuzzy" classes = classes without sharp boundaries, that overlap with others. They have prototypical members, and others that barely fit as members.

gender = partly natural, partly arbitrary classification of entities into broad categories. The Algonquian genders are animate and

inanimate.

glottal catch = a sound in some languages, produced by cutting off breathing for a split second.

husband centered residence = residence with husband's family.

inaccessible = entities lost, destroyed, non-functional, absent, asleep, dead, etc.

inanimate gender = a classification of nouns implying that the entities they name cannot move about of their own volition.

index = something that usually indicates the presence of something else.

inflection = the rules for ending words, usually indicating categories like number, gender, person.

initial = the first part of a word, its root.

initial change = a change in the first vowel of a verb in certain modes, making it longer, sometimes changing its quality, or sometimes adding an infix (\*-4ey/\*-4ay) before it.

inner orientation = plant orientation = a cultural focus on earth and plants.

innovation = something newly created, a change in a language.

intensive warfare = warfare aimed at destroying all enemies, or at



least all enemy warriors, rather than just a few to settle a score.

leveling = making two different things the same.

language = all the forms of speech easily mutually understandable.

linguist = person who studies language scientifically.

localized sex = the sex that has its choice of residence, i.e., that usually doesn't relocate after marriage.

marriage isolate = the unit within which nearly all marriages take place, the deme.

matrilineages = organized kin groups whose membership is traced through women only.

matrilocality = wife centered residence in a society which is matrilineal (that is, recognizes descent only through women).

medial = the second part of a word, after its root.

microword = something like part of a compound word.

morphology = having to do with the shape of words, roots, prefixes, suffixes.

Neither Dominant society = one in which neither a woman's husband nor brother dominates her.

obviation = an Algonquian grammatical category, signaling that an

entity is not the main referent or topic.

obviative = marked by obviation, not central to the discourse.

Omaha = a type of patrilineal organization; the kinship terminology that goes with it. Cross cousins are equated with closer relatives of older or younger generations.

organizational sex = the sex around which residence and descent were organized, and through which inheritance was transmitted.

outer orientation = animal orientation = a cultural focus on sky and animals.

parallel kindred = relatives traced through the same sex siblings of one's ancestors and descendants.

patrilineages = organized kin groups whose membership is traced through men only.

patrilocality = husband centered residence in a society which is patrilineal (that is, recognizes descent only through men).

person = in the grammatical sense, 'I, we' is the first person, 'you' the second, 'she, he, they' is the third.

phonetic drift = the tendency for pronunciation to change slightly as the centuries pass.

phonemic transcription = writing a word in the simplest scientific

way which gives all the needed information about its pronunciation, but no more.

phonetic transcription = writing a word scientifically, in a way which gives as much information about its sound as is practical.

phonology = having to do with the sounds of a language which are critical to conveying meaning.

prehistoric = before the invention of writing, before events were written down.

proto = first, earliest (usually reconstructed).

protohistoric times = the time at which writing was introduced, and hence a written history became possible. In the Algonquian case, this begins around A.D. 1600.

proteins = foods the body uses to grow or repair itself, found in meat and some vegetables.

prototypically = typically; the best example of.

proximate = non-obviative.

Received Ideas = ideas accepted from authority figures when one is a child, therefore believed uncritically.

regional group = a named group, often larger than a local group and smaller than a deme.

reconstruction = an account of past events put together from present evidence.

Semi-Egalitarian society = a society where men are equal, but women aren't.

sex = classification into female and male, whether natural or by social convention.

sibling set marriage = marriage by sets of same sex siblings into the same family or lineage.

suppletive = unrelated words that replace the variants expected.

survivals = once meaningful social features, which survive by passive inheritance, although no longer meaningful.

Transitive Inanimate (TI) verbs = verbs describing action on inanimate entities.

underlying = something not visible, but believed to be there at a deeper level.

uterine succession = inheritance in the female line.

uxorilocality = wife centered residence in a society which isn't matrilineal.

verb = word describing an action or state.

virilocality = husband centered residence in a society which isn't patrilineal.

wife centered residence = residence with wife's family.

winter hunting group = the largest group which stays together over the winter.

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