GREATER ANATOLIA AND THE INDO-HITTITE LANGUAGE FAMILY

edited by Robert Drews

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On the Question of the Anatolian Origin of Indo-Hittite

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In the past fifteen years several proposals have put the homeland of the original speakers of the Indo-Hittite (IH) languages in Anatolia. I propose in this paper to evaluate these proposals. Any solution to the problem of the homeland of Proto-Indo-Hittite (PIH) must involve both linguistics and archaeology. As a linguist, I start from the linguistic data. It is well to remember, however, that the problem is posed by linguistic data, and any solution not facing the linguistic data is unacceptable. We need to account for the linguistic facts of the extraordinary spread of Indo-Hittite languages, the reconstructed vocabulary, and the relative chronology of the division of the language family.

At the dawn of recorded history, the languages of this family were found over most of Eurasia. They extended from Ireland to India and western China, from the Baltic Sea to the Mediterranean and across into Anatolia. While history dawned at different times in different places, this distribution is certainly ancient. The fact that these languages have been demonstrated to belong to a genetic language family indicates that all these languages evolved from a single fairly coherent language. In the absence of mass education and government policy, the area that can be covered by a coherent language without substantial differentiation is limited. That means that the IH family underwent an extraordinary expansion across Eurasia at a time before the existence of large cities and empires and, at least initially, when trade was conducted through exchange networks rather than through established trade routes. Any prehistory of Eurasia that does not account for that expansion is woefully inadequate.

Ideally, we should try to discover the time and place of the beginning of the expansion and to offer some explanation for that expansion. The original area certainly has to have been much smaller than Eurasia. The amount of dialectal variation that we find in a language is not artificially reinforced by a government or an educational system varies, depending on the topography. Mountainous areas have intense variation, as do coastlines (Nichols 1992: 233). Forested areas have less but still substantial variation across areas not connected by rivers. Open plains have better communication and less variation. Nichols (1992: 15) notes that throughout known history the Eurasian steppe has had one basic language at a time. She does not, however, exclude the possibility of dialectal variation in that basic language. While we would like to reconstruct PIH as a pure dialect without variation, this is very unlikely. We should certainly not look for any area larger than the Russian steppe, and if it is that large, it should be an open plain. If we place the homeland of PIH in a mountainous region, it could be quite small. James Mallory (1989: 64) estimates that to have a coherent language without significant dialects, we should not aim for a territory larger than Germany or Poland. Actually both Germany and Poland have old and significant dialectal variation. In fact, Kastubhan in northern Poland is best considered an independent language. However, we are unlikely to get any closer than an area that large.

In discussing the vocabulary of the parent language, the operative word is reconstructed, not shared. In the process of proving that two languages are genetically related, linguists must demonstrate that there are regular relationships between the sound segments (vowels and consonants) of the two languages such that regular sound changes can derive both languages from the forms of a parent language. To be a properly reconstructed item in the parent language, a word must have cognates in both languages, and it must be demonstrated to have undergone all the regular sound changes differentiating the two languages. Such a word must also have similar enough meaning in the two languages to warrant belief that the two meanings evolved from a single vocabulary item. Linguists typically start with semantically stable items to establish the formal relationships and then look at words with formal agreement but varying meanings. For Indo-Hittite, there are hundreds of words or roots for which we are reasonably sure of both form and meaning.

When we apply the comparative method, we compare items in daughter languages. We reconstruct items based on agreement among those languages. This means that the items we reconstruct had to be in the parent language at the time and place of the separation. It also means that we cannot go back any further than that time and that place—at least not systematically. The reconstructed items in the language are those that existed in the last place and time when the speakers of the protolanguage were together in a single area and constituted a reasonably unified linguistic community. That, for a linguist, is the homeland. If we go back any further in history, we are talking about the history of culture and hoping to relate that to the history of language.

The question of the relative chronology of the division of protolanguage brings us to the Indo-Hittite hypothesis. In its weakest version, the Indo-Hittite hypothesis says that the first division in Proto-Indo-Hittite (PIE) indicating the postulated parent language of the family was between Proto-Anatolian (PA) and the rest of the family. Originally the whole family was called Indo-European. For people who use the term Indo-Hittite, the non-Anatolian branch is called Indo-European (IE), and the reconstructed parent of that branch is called Proto-Indo-European (PIE).
Stronger variants of the Indo-Hittite hypothesis claim that there were a significant number of common innovations in Proto-Indo-European before it began to break up into the various subfamilies of Indo-European. This would mean that reconstructed PIE would be a slightly different language from PIE. It would also allow for the possibility that a considerable amount of time elapsed between the initial division and the break up of PIE into its subdivisions. Because the time of the division is different, the place of the division may also be different. Thus, arguments for the homeland of PIE, based on PIE data, may not directly apply to the discussion of the homeland of PIE.

My approach in this paper differs from previous approaches in two ways. First, I take the Indo-Hittite hypothesis, at least in its weaker variant, very seriously. I will make a systematic distinction between vocabulary items reconstructible for Proto-Indo-Hittite and those reconstructible for Proto-Indo-European alone. This is a reasonable precaution when we are discussing Anatolia as a possible homeland. The second differing aspect of this study arises from the fact that I will use no environmental terms for my arguments. I will limit myself to cultural terms. This is because environmental terms are systematically unreliable at the level of Indo-Hittite. For the level of Proto-Indo-European such terms might theoretically be useful, but they have proven not to be so.

The reason that environmental terms are systematically useless for PIE is that the first division in PIE is binary. Suppose we have a language family L with two branches, A and B, which together occupy more territory than we could reasonably expect to have only a single language. We might try to use environmental terms to select one of the two territories as the original homeland. If both A and B have a word for oak tree, and this word can be derived from a proto-form in L, then we can assume that the original homeland had oak trees. However, since A and B have an inherited word which means oak tree, oak trees must exist in the territory of both A and B, so the term is of no use. If a term for oak tree exists in A, but not in B, then we cannot determine whether the word was in the proto-language L. If a form from L shows up in A with the meaning oak but in B has the meaning fir, we cannot decide the meaning in L, again, it is of no use. Similar arguments would apply to any environmental term.

Because there is no guarantee that the initial division of the PIE branch was binary, we might have better luck finding environmental terms in PIE. A systematic problem still exists, however, but perhaps not a systematic impossibility. If there were an animal or plant unique to the original homeland, we would be guaranteed to miss it. When the language expanded to areas where the plant or animal did not exist, then the term would either go out of use or be applied to some other plant or animal. Thus, we either could not prove that the word was PIE, or we could not arrive at a reconstructed meaning. To prove that a term is PIE, a reconstructible form must occur with essentially the same meaning in several IE languages. This means that a term with a reconstructible form and meaning must refer to an environmenal feature with broad distribution, which lends its value for locating a precise place of origin. At best, we could expect only that a given term which is arguably PIE could be used to eliminate part of the territory where IE languages are found. A large set of terms might allow us to eliminate a large part of the territory occupied by historical IE languages. In practice this is not the case. James Mallory (1989: 117, 163) is one the most careful scholars who has applied this methodology. In Europe, the only places that he could eliminate on the basis of environmental terms alone were the treeless parts of the Russian steppe and Greece.

If we limit ourselves to cultural terms, then we obviously cannot make any direct arguments for the place of origin of Indo-Hittite. We can identify candidate cultures, and we can use datable cultural terms to limit the time of the first dispersal of the Indo-Hittites. We use positive evidence for cultural terms to establish a terminus a quo for the first division of the proto-language. With considerably more danger of error, we can use the absence of important cultural terms to establish a terminus ante quem for the division. For instance, I will argue that the existence of a reconstructed term for yoke, combined with the absence of an Anatolian of a whole set of terms relating to wheeled vehicles, could place the division of PIE into PA and PIE between the introduction of ox traction and the introduction of the wheel. Since languages can and do spontaneously lose or replace words, positive evidence is always much more reliable than negative evidence. The positive evidence indicates that the division between PA and PIE took place after the introduction of ox traction (c. fifth millennium B.C.) and that the dissolution of PIE took place after the introduction of wheeled vehicles (c. late fourth millennium B.C.). It is negative evidence (the absence of terms for wheeled transport) which indicates that the division of PIE took place before the introduction of the wheel. We should look for evidence from other sources to confirm that. I will argue that we may find such other evidence in the development of putative IE cultures in Europe. It is broadly accepted that IE speakers played a prominent role in the Yamnaya and Corded Ware cultures. These cultures occupied a territory much too large to allow for a unified language; thus, as the cultures differentiated, so would have their language. This would establish a division within the PIE branch of PIE and entails an earlier differentiation between PIE and PA. The younger of these cultures, the Corded Ware culture, began in the late fourth millennium. We thus have a second line of evidence that could place the terminus ante quem for the division of PIE in the second half of the fourth millennium.

Are we limited merely to proposing candidate cultures for the original homeland? In the general case, I think we are. There is no one-to-one correlation between culture and language, and all we have are cultural artifacts. In the case of Indo-Hittite, however, we may be able to make reasonable suggestions. Luckily, the number of candidate cultures is restricted, even in the broad area where Indo-Hittite is found. The fact of the extraordinary spread of Indo-Hittite languages adds to our good fortune in this search. While culture can spread without language, language is
an extremely important cultural artifact unlikely to spread by itself. We should be able to detect in the archaeology the kind of cultural contacts that would allow for the spread of language. Furthermore, the extraordinary spread of Indo-Hittite should be associated with some set of important changes for which we can expect to find evidence in the archaeological record.

Vocabulary items that are reconstructed for PIH and PIIE reflect what Sherratt (1981) called the secondary products revolution. This involves the use of animals for labor, for transportation, and for such items as milk and wool. The use of animals to increase production, and in particular the use of animals to produce food and clothing without the necessity of killing them, had the potential to add a considerable amount of wealth to human society. The use of ox-drawn plows could allow for the use of land that was previously unusable. Because a few oxen could take care of a large number of animals, the more animals the greater the wealth. A society that adopted this system ahead of its neighbors would have an advantage both in wealth and mobility. It could expand rapidly into both unexploited geographic and economic areas.

All this will, I hope, become clearer as we look at concrete data I will survey the proposals that I will evaluate.

Thomas Gamkrelidze and Vlachkov Ivanov (1995), in Indo-European and Indo-Europeans (the English translation of a 1984 book in Russian), use archaeological and linguistic evidence to place the original homeland south of the Caucasus in Anatolia, in the fourth or fifth millennium B.C. They describe the steppe north of the Caucasus and the Black Sea as a "secondary" homeland of Indo-Europeans (1995: 836). However instead of postulating a geographically simple migration of most of the Indo-Europeans through the Caucasus, leaving the Anatolian Indo-Europeans behind, they present a picture of the migration patterns of the various tribes that can fairly be described as confusing (cf. their map on 850-51).

Colin Renfrew (1987), in Archaeology and Language, uses only archaeological data for prehistory. He starts from the premise that most of the population spread within Europe can be traced to the spread of farming, which supported an increase in population. Farming was introduced into Europe in Greece in the seventh millennium B.C.—almost certainly from Anatolia. He suggests that the language of these initial farmers spread as they spread, through what Ammerman and Cavalli-Sforza (1973) call demic diffusion. As children of farmers grow up, some may stay on their parents' farm, but others will move in random directions to find new usable farmland. This can eventually result in the spread of population over an entire continent.

Robert Drews, in his 1989 book The Coming of the Greeks, places the homeland of the Proto-Indo-Europeans in the southern Caucasus, but much later than do Gamkrelidze and Ivanov. He associates the spread of Indo-Europeans with the spread of chariot warfare and puts PIE unity as late as 1900 B.C. In a later article (1997) he differentiates between Proto-Indo-European and Proto-Indo-Hittite.

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Using this distinction, Drews presents a variant of Renfrew's proposal. He suggests that the farmers who crossed from Anatolia to Greece in the seventh millennium B.C. were speakers of an Anatolian Indo-Hittite dialect.

Sherratt and Sherratt (1988) provide an interesting variant on the Renfrew proposal. They suggest that a pre-Indo-Hittite language did originate in the farming belt of the Middle East and spread into Anatolia, as well as perhaps into southeastern Europe. They then suggest that a koiné developed as a trading language around the western part of the Black Sea in the third millennium B.C. and that this language spread by means of civilization and as a trade language.

I will consider only one alternative to the Anatolian solutions. James Mallory's 1989 In Search of the Indo-Europeans uses an impressive array of archaeological material, integrated with a sound knowledge of Indo-European scholarship, to place the homeland of the Proto-Indo-Europeans in the steppe north of the Black and Caspian seas, sometime between the late fifth and early third millennium B.C. The specific people in the steppe were of the Pit-Grave (Yamnaya) culture or its immediate predecessors, the Khvalynsk and Sredni Stok cultures. Mallory presents archaeological evidence for the possible spread of people and cultures from that area. Mallory's work is really a continuation of the work of his professor, Marija Gimbutas. Her collected works on this problem have been published as The Kurgan Culture and the Indo-Europeanization of Europe (1997). (The label "kurgan" comes from the Russian word for the characteristic burial mound in this culture.) I think that the Mallory-Gimbutas hypothesis provides the best fit for the PIE data, but neither of them seriously considers the possibility of a difference between PIE and PIH.

I will concentrate in this paper on a few cultural terms possible to trace in the archaeological record. The terms demonstrably present in words for wood, horse, and a group of words associated with ox traction: *xwoc, xwore, hore, and possibly xarnos."

PIH Cultural Terms

(i) *xwoc- (wool)

Skts. *gmpa Av. sarana, Gk. (Dor.) lýs, L. lana, Lith. vėna, S.C. sîna.
OE suðil, Hr. hulena.-

All of the forms can apparently be phonetically derived from a stem *Hwoc-. There have been some questions raised about the presence of two larynxes in the stem. Hittite is the only language that indicates a word-initial laryngeal. This raises a question about the lack of a protogenic vowel in the Greek word. Initial laryngeal-consonant clusters in Greek often result in an inserted vowel. Indeed, one of the enthusiastic proponents of this hypothesis, is only willing to say (1988: 71) "It seems that a laryngeal was vocalized in Early PIE.*
Hittite ḫuruk (wheal). Gamkerleider and Ivanov (1995: 623) relate this to a family of words that began with *Hus(t)-, and meant relate or turn, including *Huer-t- and Huer-g-. Pokorny (1994: 1,157) relates Greek  houtaná (visiting) spoon to this family of words. Another possibility is Greek ἱμβρούν (rose, roll about), and ἱμπέν (magic wheel), which may be from an n-infused form of *uveling-. If this is correct, then there is something yet to be explained about the relationship between initial ḫ in Hittite and Greek prothetic vowels.

Adams (Mallory and Adams 1997: 648) solves the problem by postulating a metathesis to change a Proto-Hittite form *uvelen- to *uvelone-. He leaves the initial laryngeal in ḫarki, however.

Gamkerleider and Ivanov (1995: 178) suggest that the original form could be *Huvelon-, with the word-initial laryngeal influencing the syllabic Ḫ to form the long syllabic that is reflected by the acute intonation in Lithuanian and the long a in Greek and Latin. If this were a phonic process in PIH, then we could not postulate an initial sequence *Huvelon- in the Greek words lakhto (woolly hair, down), and laktone (wool), and could not relate them to other words for wool and hair. We would also expect to find the reflexes of long syllabic v in the zero-grade words for (turn), which they (1995: 623) positulate as *Huer-th and Huer-g-. We do not find this reflex in Lithuanian evito (turned over), or Sanskrit yadha (twisted). We would also expect a long a in Sanskrit adhatras (cin), and Avestan ad-danaya (made of cloth), which they (1995: 499) derive from a root *Hw.

Tischler (1984-1: 279) likewise argues that there was no second laryngeal after the Ḫ but provides no evidence. Melchert (1994: 55-56) argues for the presence of the Ḫ after the i at the time of an assimilation of *Hw > 埒, since this assimilation was blocked in ḫdane. He is unsure whether the vowel after the Ḫ is purely orthographic or a later insertion. This obviously serves as an argument against both Tischler and Gamkerleider-Ivanov.

Overall, the evidence for a laryngeal after the Ḫ is about as good as we can get, without an overt ḫ in Hittite. (Only one laryngeal shows up as ḫ in non-initial position in Hittite; and then not in this environment.) This includes the reflexes of *long syllabic ħ in Greek, Latin, Sanskrit, and Balto-Slavic, as well as Melchert’s argument for Anatolian.

The best overall solution to the problem of the presence of an initial ḫ in Hittite coupled with the absence of a prothetic vowel in Greek is to accept the possibility that laryngeal plus *w did not always condition a prothetic vowel in Greek. Initial ḫ in Hittite can correspond to an a-coloring or an a-coloring laryngeal. The a-coloring laryngeal does condition epenthesis in Greek, as in dîís < *ṂuwHis (blow) compared to Hittite buwıwı - (wind). There is, as far as I have been able to determine, no reliable example of a prothetic vowel in Greek that can be traced to an initial *Hw. Because it had a rounding effect on an adjacent vowel, H, may have shared enough characteristics with w that it could easily be absorbed by w.

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(2) *Herwos (horse)


This is a concrete word. In this set the only phonological problem is the initial i- of Greek hipsos. The possibility that the i might be phonologically explainable in Mycenaean is offered in Mallory and Adams (1997: 274).

(3) *Ywgo- (yoke)

Hit. juka, Skt., yaga-, Gk. augeon, L. tector, OCS yug, OE geoc, Welsh ieg, Lith. jungas.

In these forms the only deviation from the basic shape of the stem is Lithuanian jungas, which has the -w- infix from the verb stem. This root is well integrated into PIH morphology. There is a verb root having an aorist stem *yung-yung, a present stem *yung-yung (boil, yoke) (e.g., Skt. pṛṣṭaṃ yuṅkta < *yung-yung, aorist mid. ayukt- < *yung-m-) Bremmer(in his 1935) discussion of PIE root structure, claimed that original roots had only two nonvoicing elements. The third was always a suffix or clausiement. The -a-inflect for him was always an original suffix. In this system, the "vowels" [e] and [u] are always the vocalic realizations of the glides /j/ and /w/, which count as consonants. The primary root in this case would be *ya-w-. In this case, the original root is actually found in Sanskrit yu- (unite) in the verb yu-ya- (unites), and, with a nominal suffix -ti, in the Lithuanian word janta- (en, i.e., the yoked animal).

I include this here to indicate just how sound the PIH status of this word is. It is not a borrowing.

The fact that jagan was a native word, however, does not imply that the PIH speakers invented the yoke. It merely indicates that in borrowing an instrument they applied descriptive terms from their own language to name it. We will see the same thing for the words for wheel.

(4) H(e)H(e)- (harness pole)

Hittite h,E,-, b,E,- (harness pole), Skt. h,ā (harness pole for carriage or plow), Ae.  a,ā (plow), Slav. o, (groom) o,na (harness pole).

We have trouble reconstructing a concrete word here. The Hittite word, considering the attested spelling with e, is probably best considered from *Het?H- and
as such is a formal match with the Avianian word, which could also come from *Hesov-. Pokorny (1994: 298) lists them under *vé-, av- (stake).

(5) dwrrH- (harness)

Several sources, e.g., Gamkrelidze and Ivanov (1995: 624), Mallory and Adams (1997: 508), Mairhofer (1992: 794), suggest linking Hittite *nāšja (fleed) harness), Sanskrit abhāya (yoke, load, harness shaft), Tocharian naška (draft ox), and Greek thairon (pivot of a door or gate, axle of a chariot). Multiple ambiguities arise in these forms. The Sanskrit and Greek could come from *dhwrH, while the Hittite and Tocharian could begin with any dental stop.

Ox Traction

I have not found any study dedicated to the invention and dissemination of the yoke. However, the combination of words for yoke, harness pole, and harness indicate the existence of animal traction. This should involve the traction of plows or vehicles. In both the south of Russia and in Mesopotamia, there is evidence that sledges were used as vehicles before the introduction of wheels (Piggott 1992: 16), but there is no evidence to place these vehicles before the fifth millennium B.C. There is no unambiguous evidence for plows pulled by oxen before the fourth millennium B.C., but they almost certainly go back to the fifth. Kushnareva (1997: 172) even suggests a sixth millennium date for plows in the Southern Caucasus and in the Near East in general. Along with indirect evidence from sophisticated agriculture, she suggests that a piece of amber found at Azrolo I could have been part of a primitive plow. Azrolo I has uncalibrated carbon 14 dates of 4770 ± 60 and 4813 ± 60 B.C. (Kushnareva 1997: 22).

Initially, vehicles were much too heavy to be pulled by horses, and even were necessary. Sherzer (1981: 288) presents a map of the distribution of ox traction as of 4000 B.C., with its subsequent expansion. He shows the early area of ox traction to stretch from Mesopotamia to the area south of the Caucasus by 4000 B.C. It then spread through the Caucasus, across the Russian steppes, and into the upper Balkans and central Europe. This conforms to what Gimbutas has to say about the spread of the yoke and the plow. She cites the existence of copper figurines of yoked oxen in the southern Caucasus dated to about 5700 B.C. (1970: 197, 1997: 82). From a grave site in the steppe north of the sea of Azov (Karrenaya Mogila, near Melitopol’), which she dates from the second half of the fourth millennium B.C., she presents (1980, 1997: 273) a line engraving that seems to represent yoked oxen pulling a vehicle (without indication of wheels).

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However, if Sherzer is correct about the route, the spread may have taken place earlier than he thought. In the Tripolit-Cucuteni culture at the western end of the Russian steppe, archaeologists have found more than fifty ceramic vessels in the shape of sledges, some of which have the heads of paired oxen attached to the front (Guzev 1998). One of these (without oxen) is attributed to middle Tripol’s, which would be in the second half of the fifth millennium. The great majority are found in the Nebeleroko-Tomashivka, Sushkivka complex, which Gimbutas (1997: 43) labels Cucuteni B3-Tripol’s B III. She gives no carbon 14 dates for this complex, but dates on both sides (1997: 47) would make it around 4000 B.C. The earliest indirect indication of ox traction is changes in the bone structure of the legs of oxen attributed to the stress of traction. Mateuška (1975) found such evidence in ox bones from the early fifth millennium B.C. on the lower Danube. Davis (1987: 162) withholds judgment on this evidence, citing the lack of experimental controls.

The Horse

No internal linguistic evidence is available to determine whether the word *Hesov- refers to domestic or wild horse. The argument that the Proto-Indo-Hittites used domesticated horses is based on the fact that the domesticated horse is extremely important in the mythology and ritual of the oldest PITH Societies. A survey of three arguments is found in Gamkrelidze and Ivanov (1995: 465–75). Although the field of comparative mythology does not have very rigid standards of proof, this argument is as solid as any it offers.

Throughout all the period that interests us, wild horses were found in the Eurasian steppes and across much of Europe. According to Vértes (1981) there are no traces of wild horses in the Balkans or in the Danube valley in the Neolithic. They had begun to filter into the Carpathian plain from the northwest and begin to appear in the upper Balkans in the Eneolithic (fifth millennium).

Ample evidence exists of horses in the Kuro-Araxes culture of the southern Caucasus in the fourth millennium B.C., and they are found at two sites in the fifth millennium (Kushnareva 1997: 173). Piggott (1992: 38) reports the existence of wild horses in the fourth millennium B.C. south of the Caucasus and in northwest Anatolia around Diyvakhir, Bokhni (1987, 1991) argues that the animals found south of the Caucasus in the fourth millennium were domesticated horses introduced from the Pontic steppes. Piggott’s report about the find in northwest Anatolia is apparently based on a personal communication from von den Driesch to Juris Zarins (Zarins 1986: 180). Zarins identifies the specific site in western Anatolia as Demirci Hüyük. Grove (1986) simply says that “Evidence for the presence of wild horses in Southwestern Asia is not very conclusive either, although Boesecksteck and von den Driesch(1976) claim to have identified wild horse bones from fourth millennium deposits in Anatolia.” The bones identified by Boesecksteck and von den Driesch
are the ones claimed by Biskriy to be domesticated horses. Russell and Martin (1998) report the presence at Çatalhöyük West (c. 5000 B.C.) of several bones "belonging to large equids, which is likely to mean Equus *balkas.*" Colin Renfrew, at this conference, verbally confirmed the existence of horses at Çatalhöyük from as early as the seventh millennium.

Nevertheless, all claims for early domestication of horses are controversial. Petrenko (1994: 70) asserts that the preponderance of young animals with milk teeth at the Neolithic-Eneolithic site of Mollino indicates domestication of the horse for purposes of meat production, but the small total number (twenty individuals) may raise doubts. Not only Gubíttas and her followers, but Sherratt (1981, 1983b), Piggott (1993: 43), and Biskriy to accept evidence from late fifth millennium steppe settlements like Derevka to indicate horse riding and domestication. Anthony (1991a) provides a summary of the evidence and account of how it may have happened. The evidence includes the remains of a large number of horses at a small settlement (fifty bones for three houses), ritual burials of horses, and antler pieces of a site and shape that could have been used for bridles. But Anthony's demonstration of wear on a stallion's teeth that could be the result of using the antler bits has recently been called into question by the well-known skull's radio carbon date of c. 2900 B.c. (Mallory and Adams 1997: 276). Levine (1996) is careful to distinguish between horse taming and riding, as opposed to husbandry, which is true domestication. She accepts the evidence for horse riding from Derevka but sees no conclusive evidence of husbandry. Whittle (1996: 134) is even more critical, accepting Derevka as evidence for only limited horse riding.

The doubts about domestication at Derevka were raised by Levine's (1990) study. She examined the still-available horse bones from Derevka and found that the ages of most of the horses represented clustered between five and eight years old. Thus, she argued, is too old because animals raised for food would likely have been slaughtered sooner after reaching an adult growth, roughly between one and two years. Moreover, the points out that working horses in modern Mongolian societies are kept until they are fifteen to sixteen years old. The lack of older horses at Derevka led her to the conclusion that the bones at that site were marital.

Levine's conclusions may not be unjustified. Petrenko (1984: 69-110) found similar distributions at sites that almost certainly had domesticated horses. He studied these along the middle Volga and west of the Urals. At Iron Age sites and at sites from the first millennium a.d., Petrenko's data show that less than 2 percent of the horses with determinable age lived over thirteen years, while slightly more than 70 percent died between three and nine.

Horses appear in the upper Balkans and the Danube Valley in the late fifth millennium. Gubíttas (1957, 1997: 240-68) associates this with the spread of domesticated horses into Europe with the expansion of the steppe (kurgan) culture, and therefore with the spread of Indo-Hittite. If indeed the bearers of the steppe culture brought horses with them in these expansions, then the horse was at least domesticated, if not ridden, before the beginning of the fourth millennium. Viets (1981), however, suggests wild horses may have been drifting west from the steppes at that time.

Fourth millennium evidence is similarly controversial. Gubíttas (1997: 273) cites a line drawing of a man on horseback, which she says is attributed to the second half of the fourth millennium. Hübner (1994) points out that this drawing cannot be securely dated. Possible bronze cheek-pieces for bridles are found in the Makop culture of the northern Caucasus. Evidence that these may indeed be cheek pieces came from microscopic examination. Remains of skin were found on them (Munach 1975: 390). The dating of the Makop culture has been controversial, but it seems to have begun early in the fourth millennium. Recent carbon dates taken from bones at Galgughi, a settlement of the Makop culture (Kushnareva and Markovkin 1994: fig. 172) indicate for Galgugi I 14,480 ± 78 B.C. for Galgugi II 4,600 ± 80 B.C. and for Galgugi II 4,930 ± 120 B.C. giving a calibrated range of 3750 ± 130 to 3105 ± 90 B.C. A general upper limit for the Makop culture is provided by radio carbon dates from the burial ground at Ush-Drozhginskii. These burials are supposed to relate to a period immediately following the period of the Makop culture. The calibrated dates range from 2755 ± 310 B.C. to 2465 ± 335 B.C. (Kavtaradze 1983: 107-8). However, in the area north of the Kuban river, the Novsutorovka culture is thought to follow the Makop culture, and that culture has a calibrated carbon date of c. 3100 B.C. A general range for the Makop culture would seem to be 3750-2750 B.C., with the possibility this it ended earlier in some parts of the region. No specific dates are connected with the cheek pieces. The horses in the Kura-Araxes culture are widely considered to have been domesticated. Kushnareva (1997: 193) argues that they were used as pack animals, but the only evidence strong to be that they appear where they were not found before, associated with sites developing transhumance of sheep.

In the third millennium we have Anthony's evidence of bit wear on the skull from Derevka, with a carbon 14 date of c. 2900 B.C. in central and western Europe the Bell Beaker culture, dated by Mallory and Adams (1997: 53) at c. 2600-1900 B.C., is thought to provide the first good evidence of the raising of horses outside the steppe (Bokovenko 1994). Toward the end of the millennium, there is written evidence for the arrival of domesticated horses in Novotornia (Clayton-Bruck 1992: 89).

If we consider the horse to be a domesticated animal in FHH society, then in the first half of the fifth millennium the evidence would seem to limit the PHH area to the steppes east of the Dnieper. In the second half of the fifth millennium it spread to the upper Balkans and the Danube Valley. In the fourth millennium it spread to include the area south of the Caucasus and into northern central Europe. In the third millennium the area is spread over much of Europe, the Near East, and the Asian Steppe.
Wool

Sheep may have been domesticated as early as 9000 B.C. (Ryder 1983: 22), but the originally domesticated sheep were used for meat only. Wild sheep are hairy, rather than woolly, although they have a wool undercoat. Wooly sheep are first indicated at Sarat in the Kermanshah Valley of Iran. A clay figurine from that area of a sheep shows clear indication of staples (characteristic clumps of wool floss) (Bokonyi 1977: 101). There are four carbon 14 dates from that site, all calibrating to the seventh millennium B.C. The uncalibrated dates, found in Braeoun (1987: 54) are: 6006 +/− 125 B.C., 5655 +/− 96 B.C., 5094 +/− 89 B.C., c. 5900 B.C. Moreover, Bokonyi (1977: 24) notes that 73–93 percent of the sheep bones belong to adult animals, indicating a use of the sheep for secondary products. Preservation of adults of both sexes should indicate use for wool as well as milk products. Davis (1984), however, basing his work on a 1978 excavation in the Kermanshah valley including Sarat, argued that no evidence of secondary use of sheep shows up until the beginning of the sixth millennium. Outside this area there is no indication of use of wool. Slaughter patterns for sheep and goats in eastern Anatolia and Greece do not indicate use of oviscardis for secondary products in that area in the Neolithic (Davis 1987: 159). Significant indications of wool technology do not occur anywhere until the fourth millennium, occurring in Mesopotamia. Wool eventually became an important item of trade in Mesopotamia. Worker wages were paid in wool, and exports of woolen cloth paid for the imports of raw materials (Ryder 1983: 97).

Although there is no evidence of woolen textiles existing before the fourth millennium, we have to entertain the possibility that wool felt may have existed earlier. Felting requires no equipment; so if the textiles themselves did not survive, we are left with no direct evidence. Ryder (1983: 75) nevertheless suggests that the first wool textiles were felt. He points out that even in wild sheep the wool undercoat melts, a process resulting in natural felting. The earliest possible attestation of felted material is, according to Barber (1991: 216–17), in Anatolia at Çatalhöyük, from 6000 B.C. This is the remains of who might be a textile made of animal hair. In a society that had developed impressive patterns on woven fabrics, this artifact shows no pattern. She suggests that this may be the accidental felting of hair used as matting, rather than an intentionally produced textile. It is possible but unlikely that felt textiles were produced without the presence of woolly sheep. Since many woolly sheep are required to produce enough wool for felt, it would take many more hairy sheep to produce the same amount. I will return to this issue below. For now, I will limit myself to the actually attested evidence.

In the fourth millennium, weaving of wool textiles spread from Mesopotamia to Egypt, and apparently to the Caucasus. Munchave (1975: 161) reports a piece of cloth found at Kvacelebi in the Kuro-Araxes culture of the southern Caucasus. He describes it as a simple weave made of wool and a plant fiber (flax). Unfortunately he does not cite a source and does not name the archaeological level. Kvacelebi was occupied for many hundreds of years. The levels belonging to the Kuro-Araxes culture at Kvacelebi are labeled B and C. A calibrated carbon 14 date for the top of the C layer is 3568 +/− 277 B.C. (Kavtaradze 1983: 83). Kavtaradze (1983: 97) suggests that the top of the B layer should be dated to the beginning of the third millennium, a date he further argues should represent the end of the Kuro-Araxes culture in Georgia. This would seem to locate the cloth in the fourth millennium. Other evidence seems to support this conclusion. Munchave (1975: 161) notes the existence of pottery with impressions of clot at Kvacelebi and at Amirjanis-Gora, from the same culture. Amirjanis-Gora has calibrated carbon 14 dates of 3654 +/− 402 and 3404 +/− 416 B.C. (Kavtaradze 1983: 83). While there can be no indication of the fiber, this at least indicates the existence of some woven cloth in this area in the fourth millennium.

Furthermore, many sites in the Kuro-Araxes culture show a change in livestock breeding from an early focus on cattle to a clear emphasis on sheep. Some, as at Kvacelebi, kept the herds of sheep near the settlement. Others, as at Amirjanis-Gora, apparently practiced transhumance, moving the sheep to high pastures during the summer (Kushnareva and Chuchinidze 1970: 109; Kushnareva 1993: 223–24). Kushnareva and Chuchinidze see as evidence of transhumance the existence of large buildings for the winter housing of sheep, as well as enclosures for stock in high pastures.

Kushnareva and Chuchinidze identify the sheep at Kvacelebi as of the "imeretinski" type. These were presumably what Ryder (1983: 268) calls "imeritians," a Caucasian fat-tailed breed used for milk products, meat, and wool. Ryder notes that a similar breed, the Karachaev (which he observed), produces a coarse wool. These sheep were kept outside the year round, as the sheep at Kvacelebi apparently were.

Remains of sheets and dishes appropriate for cheese production are found at Kvacelebi. It may well be that milk was more important than wool for settlements of this type, but the cloth indicates that wool was used. Although there are no indications of looms, spindle whorls are found throughout the Kuro-Araxes culture. In kurgans of the Bedeni culture, which follows the Kuro-Araxes culture, felt and woven wool cloths are found in burial chambers (Kushnareva 1993: 213). Barber cites the discovery of a twill cloth (presumably wool) in a burial at Markhops, which has a similar early third millennium date.

In the northern Caucasus, Barber (1991: 168–69) reports a kurgan burial at Novosobodnaya in which were found the remains of clothing on a male skeleton. There was an undergarment of a linen-like fabric and an outer garment of a fluffy (pseudoy) yellow and black plaid. Since this cloth is contrasted in linen, we might assume that it was wool. Novosobodnaya is either a later version of the Maikop culture or a new culture that replaced it. I mentioned above in the discussion of horse that a general range for the Maikop culture (including Novosobodnaya) would be c. 3750–2750. Barber (1991: 169–70) also notes a burial of similar age in the steppe along the lower Volga that has the trace pattern of a rug used in the burial.
Bölönyi (1987) argues that wool sheep were introduced into Europe via the Pontic steppes in what he calls the third millennium. He explicitly says that he is not using calibrated dates yet says that the introduction of sheep from the steppes into eastern and northern Europe is coordinated with Gilgalrepas's second and third Kurgan expansions. This would mean that wool sheep had reached the western edge of the steppe by c. 3400–3200 n.c. This agree with Sherratt (1983b), who says that wool sheep are found in the Pontic steppe, the Balkans, and eastern Europe in the fourth millennium and that the steppe route is the most likely for the introduction of wool sheep. He also says that the Corded Ware culture dated by Mallory and Adams (1997: 127) as beginning about 3200 n.c.was the vehicle by which wool sheep spread throughout northern Europe.

The best evidence for the introduction of wool sheep into central Europe comes from the influx of kurgan burials in the Danube valley, beginning in the second half of the fourth millennium. Bölönyi (1974: 168) identifies markings on clay statuettes of sheep found in Hungary from this time as representing fleece. Eecedy (1979: 15, 16, 19, 20, 37–39, 44) mentions the remains of blankets, spreads, and carpers made of organic material in early kurgan burials in the Carpathian basin and elsewhere. In general, the material itself does not survive, only the pattern in the earth. Eecedy cites no carbon dates from any of the kurgans, but he does say that the early kurgan burial are contemporary with the Ulascavovi culture, which Gimburt (1997: 95) dates from the second half of the fourth millennium. Unfortunately, the only surviving textile that was analyzed (a piece of a tent from an undated kurgan burial at Kutanjati) was analyzed as camed hair (Eecedy 1979: 39). The new sheep that moved from the steppes into Hungary had an average height at the withers of 70 cm. compared to 60 cm. for Aelasolitic sheep (Bölönyi 1971). The number of sheep increased dramatically, and a large number of these sheep were allowed to live into adulthood, indicating use for secondary products. Tjallingii (1972) found that by the late Bronze Age, the mean height of sheep throughout eastern Europe was 70 cm.

In the Trichterbecherkultur (TRB, German for Funnel-necked Beaker Culture) of north-central Europe, the first indications that sheep are being raised for anything other than meat is very late, near the end of the fourth millennium. At Spöderberg on Langeland more than half the ovicaprids found had lived past adulthood, and most had died after their sixth year. The data from Schalkenberg in the Bernburg culture indicate that about 20 percent lived past four months (Midgley 1992: 832, 384). This is the earliest indication of use of sheep for secondary products, but it does not differentiate between use for milk and use for wool. The Bernburg culture is dated by Midgley (1992: 227) from 2900/2100–2500 n.c. This is certainly consistent with Bölönyi's timetable for the introduction of wool sheep in this area.

Sherratt (1983b) associates wool with the Corded Ware culture (normally considered to contain IE speakers as a major component) and suggests that it is reasonable to think that wool was introduced into northern Europe by this culture. Interestingly, Bölönyi (1987) finds evidence for the spread of Caucasian sheep along the southern coast of the Black Sea at the same time that they were spreading across the Pontic steppes.

The main problem tracing the origin of this new variety of sheep is not their movement into Europe but how and when they arrived in the steppes. We have noted evidence for wool technology in the southern and northern Caucasus in the fourth millennium. Certainly sheep domestication existed in the Caucasian in the fifth and sixth millennia, but there is no direct indication that they were used for or were useful for wool. Sheep were there but not as an important feature in the Maikop culture of the northern Caucasus. The primary focus of their livestock economy was ovine (Munch et al. 1975: 382). In population, sheep were in third place, after cattle. The Maikop culture never occupied the high altitudes best suited for sheep raising. This does not necessarily mean that the people of the Maikop culture did not know about wool, but it makes it less likely that they produced much. It also means that the Maikop culture is not a good candidate for the vehicle whereby wool sheep were introduced into the steppes. If wool sheep were introduced into the steppes through the Caucasus, they probably came from the northeastern area, where settlements of a culture similar to the Kuro-Araxes culture reached the steppes. In fact, sheep-based livestock economy seems to have developed in the northeastern Caucasus earlier than in the southern Caucasus. Gadjiev (1991: 82) describes the Eneolithic settlements at Ginoschi and Chirana as permanent agricultural settlements with primarily sheep as livestock, using high alpine pastures and transhumance. Temporary sites in the high altitudes conform this.

Shnirelman (1992) suggests that the tall sheep that moved from the steppe into central and northern Europe had been in the steppe since the Eneolithic, and that these sheep were introduced from the Caucasus c. 5800–4800 n.c. The Eneolithic sheep in the Caucasus were similar in size to the steppes variety. His primary evidence comes from Petrenko (1984). However, Petrenko (1984: 46) preferred the area of Turkmenia near the southeastern corner of the Caspian as a source. This is near the area where the earliest evidence of domestication is found, and the Eneolithic sheep from that area are also tall.

It is certainly worth examining Petrenko's data in detail. The earlier sheep bones found in the steppe that are intact enough to show ornamentation are found at the Khvynsky cemetery in the eastern steppes zone along the lower Volga. This is the culture that Gimburt claims as the parent culture of the Proto-Indo-Hittites (1997: 56). With the Sredny Stok culture in the west, the Khvynsky culture formed the basis of the Yamnaya or pit-grave culture of the steppes. We have six carbon 14 dates from skeletons in the cemetery. Three, recently published in Anthony (1998), date to the late sixth to early fifth millennium.

Grave 30: 6200 +/- 85 BP; 5251–5010 n.c.
Grave 18: 5985 +/- 85 BP; 4936–4783 n.c.
Grave 18: 6015 +/- 85 BP; 4994–4799 n.c.
Two of the three dates from Agropov et al. (1990: 83) roughly agree:

Grave 4: 5903 ± 72 BP: 4863 ± 137 B.C.
Grave 26: 5698 ± 79 BP: 4768 ± 136 B.C.

Grave 18, however (in the Agropov study), had a date of 4026 ± 57 BP, which would calibrate to the middle of the third millennium, if correct. If the sample itself is not contaminated, this anomalously late date likely represents a later intrusive burial. It is probably safe to stick to the earlier dates and to date the animal remains at the site to the early fifth millennium.

Perenken found measurable bones in the sacrificial areas (chzhervenniki) nos. 1, 10, and 11—and in burial 101 and 115. In chzhervennik 1 he found the remains of one sheep that would have been approximately 73 cm tall at the withers. In chzhervennik 10 he found the remains of at least fourteen sheep whose range of height would have been 61.2-71.5 cm, averaging 65.77 cm. In chzhervennik 11 he found the remains of four adult sheep that would have been about 78 cm tall. In burial 115 he found the remains of twenty-two sheep with approximately the same range and average as those from chzhervennik 10. He does not give measurements from burial 101.

This seems to provide good evidence for the existence of tall sheep in the steppe in the fifth millennium, but it says nothing about any earlier period, and the data leave me with some questions. Clearly, Perenken has found at least five tall individual sheep. There are thirty-six sheep in two groups of fourteen and twenty-two whose average height (c. 66 cm) is closer to the 62.5 cm he cites as the average height of east European Eneolithic sheep than to the height of the tall sheep. It would be very useful to have radio carbon dates for the bones of the tall sheep. If the tall individuals belong to a late period, then we could be documenting the arrival of tall sheep during the Khvalynsk culture, rather than demonstrating that domesticated sheep on the steppe had always been of the tall variety.

Nevertheless, it is likely that the tall variety of sheep introduced from the steppe into central Europe existed in the early fifth millennium. It is also reasonable to speculate that these sheep produced usable wool in the fifth millennium and must surely have produced usable wool by the second half of the fourth millennium. Because sheep can be tall and hairy and, therefore, producing no or little usable wool, and because we have no evidence of woven cloth or looms, we do not have unambiguous evidence one way or the other for the existence of wool-producing sheep in the steppe before the fifty millennium.

This brings us to the question of the use of felt, which would have left little or no archaeological trace. Before discussing that, however, I would like to look seriously at the PHL word for wool.

One could almost wonder if the word for wool predicates the use of wool as a product. It might conceivably have meant hair of a sheep. The stem for wool, *HuevH⁻inx, if not a borrowed word, is much too long not to have been derived. We might well find candidates for a root or stem from which it derived.

Gamkrelidze and Ivanov (1995: 498-99) relate *HuevH⁻inx to a basic root for weaver Hep-me found in the Sakhrin, infinitive stem < *me-tum < *me-tum < *HuevH⁻me, atur- (wool-), atur-tum (pp. woolen). The present is vepxati < *Hepxati, meaning in Sakhrin weaver, interwoven, braided, plaited. The simple stem *Hev⁻inx is found only in Sakhrin. Baltic and Slavic have a verb with a present tense apparently cognate with Sakhrin vepxati: Lith. eptis, epti, Rus. evi, ievi. Both mean to twist and are used to refer to the making of nets. In other languages a variety of forms may begin with *He-, or Hue- and mean weaver. One should remember that the laryngeal root usually shows up at all, and never before a plus a vowel, so there is multiple ambiguity. A more frequent word for weaver is *tukhvx, presumably *HuevH⁻hx with a -hx- stem-extension. Sakhrin adhehxata (ice), Greek aphthoxtos (I weave), Old English wifman, Todzarian B wip-, Ustrian wifon, Persian kav (weaver), Avestan ad-úapata- (from cloth). A form with -st is found in Old Norse satið (spinning), Gothic geisatida (wV). Old English wæld (clothing), Armenian tarandak (tie). Lithuanian stuktis (wool) could have come from *Hepxat- or *HuevH⁻at-, as could the base of Russian voolas = *evel—(starting cloth on a loom). English wattle may be related in the same root.

One may or may not believe that all these words come ultimately from a single root. If they did, the meaning of the root was probably much more primitive than weaver in the technical sense of make cloth on a loom.

What we would like to find is a related word which simply lacks the final -n, which is a common suffix in Indo-Hittite. Practically speaking this means that we look for roots listed in Pokorny (1994: 1,139) suggests that the root for wool might be related to sots- wol- which means either pluck or roll. Meaning pluck. Latin sullum is actually used to refer to the plucking of sheep, which is the ancient method of obtaining wool. The double s could come from a variety of clusters (*Ad- tis). Pokorny, without citing any evidence, traces it to an original *A-cluster.

Several words meaning hair or even wool are formed from the root *HuevH⁻inx. Hittite has another word for wool, huljigi, which has a -gi suffix. Lithuanian sulas (hair from a horsehair, fishing line made from horsehair) and Sakhrin sulas, (hair from an animal's, esp. a horse's) tail both may have come from *HuevH⁻ot-. The length in Sakhrin is explained by Brugmann's law, which determines the lengthening of original *e in open syllables. Slavic vool- (OCS vule, R. sula) (hair), is cognate with Avestan auras (hair). Neither of these words should have had a laryngeal following the si. A laryngeal would block the lengthening of the si in Sakhrin sulas and should have conditioned acute ionization in Slavic *sulov, giving R. *sulo rather than avus- al sulas. Greek leukos (woolly hair, down, sheep's wool) and leptomus (wool) likewise lack evidence for a laryngeal. Pokorny (1,139) traces the Greek words to *HuevH⁻ot.- Latin sullus, (gen.) seratia s *sullas (Hecate) is probably a derivation from sullus.
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Under the item veli, Fraenkel mentions words for wool as "femur" relates, but does not mention the possibility under veli.

The indication of an ancient connection between the word for wool and the process of felting raises an obvious question. How old is felting? The answer, unfortunately, is both in general and for the area of Eurasia that most interests us, we do not have the faintest idea. Barber, in summarizing her views on the history of textiles (1991: 256), divides western Eurasia and the Near East into four areas. Her northwest and southwest areas center in Mesopotamia and Egypt, respectively. Those cultures had weaving very early and used ground looms. Of immediate interest to us are her northwest and northeast zones.

The northwest zone includes Anatolia, the Balkans, Italy, the Danube Valley, and areas north and west of that. This area could be identified with the farming communities that moved from Anatolia to the Balkans and spread north and west from there. In this area we find flax from the very beginnings of farming, with wool introduced much later. The weavers presumably used hand looms (small looms with tension supplied by a band around the weaver's back) for making narrow strips of cloth and what she calls warp-weighted looms for making larger textiles. Even where textiles were rarely preserved, weight serve as probable indicators of weaving. Significantly, none were found before the second millennium B.C. north of southern Poland, and none were found in the Russian steppes.

The northeast zone is an area that used primarily wool. Barber postulates that large cloth was made by felting but that small band looms were used for making narrow strips of cloth. For early periods, however, these assumptions must remain only assumptions. Except for the area of the northern Caucasus, there is no evidence from the early periods. Barber assumes that this area produced felt because there are no traces of large looms, and from the time people of this area appear in history, they produce felt. The problem is that they do not appear until the first millennium B.C. In northern Europe, evidence of felting or fulling occurs as soon as wool is found. (Barber 1991: 217). She dates this as late Neolithic or early Bronze Age.

The earliest possible attestation of felted material is, according to Barber, in Anatolia at Çatalhöyük, from 6000 B.C. As noted above, she suggests that this may be the incidental felting of hair used as matting rather than an intentionally produced textile. According to Barber, the first firmly attested felt is found in Anatolia in the third millennium. Barber suggests that this is in a culture transplanted from the steppes (251). Even if this is true, these Anatolians had probably lived in the upper Balkans for a long time before crossing to Anatolia, so they had plenty of contact with weighted warp looms. More intensive evidence of felt fabric is found in Gordion, the Phrygian capital, in c. 700 B.C. The Phrygians were Indo-Europeans who likewise moved from the Balkans into Anatolia, but it is a stretch to call them steppe people at the time of the move.

Barber identifies the Proto-Indo-Europeans as inhabitants of the northeast felting zone (1991: 254-55). Significantly, however, she leaves the area of the Caucasus
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*peh₁₁as (wheel); OE. houtl (wheel), circle; OHG xutl, ONorth germ. *hekul (wheel), Gk. ἱκέλα (wheel, circle), Skt. cakra (wheel, circle). Av. cāro (wheel), Tocharian A. ḫakal (wagon).

This is a clearly reconstructible PIE word, not just a root. It is a reduplicated noun, which is unproductive in all the language families—not that unusual in the oldest vocabulary. It is from the verbal root *peh₁₁-. Its attested verbal forms mean move (Skt. cāra, Gk. πεμεῖν) or turn (Av. cāravati, Albanian ujeil, Canaanite Luwian ku-wa-wa-d-t-m) (Melchert, 1993, 114). One might wonder whether the original meaning of the noun was wheel or circle, but the meaning wheel is firmly PIE.

One possible problem for the unity in the meaning is East Baltic Lith. kēkās, Latv. kēks (rock). This technically does not match *peh₁₁as, since the first vowel is the reflex of *e₁, not *a₁. However, there are enough examples of unexpected a from etymological *e₁ in East Baltic to make one careful. Latin populus, popōlis (knee joint, back of knee), if borrowed from an Italic language with p for *q₁, could come from *peh₁₁e₁-s. (Mallory and Adams 1997: 640). This enhances the possibility of a term for rotating joints with o-grade. If these words are cognate, they fit a general meaning of something which turns, but they do not mean wheel.

Other examples of forms from the same root meaning wheel or things that turn are: Sc. hóla*(e), OPr. hōlian (wheel), Latv. skirdes (two-wheeled cart), Gr. pole (axis < *peh₁₁-e₁), L. cultus (disarticulate).

*roth₁₁H₁, L. rotus, Germ. rot, Lith. rutas, Irish roth (wheel); Skt. rathas, Av. rathau (cart, chariot).

These words are derived from a verbal stem attested in Old Irish rithim (I run). The Baltic-Slavic verbs represent by Lithuanian rita (rolls). Russ. rata—an old participle of rotat ‘to roll (mercury)’ is almost surely cognate. The short t in Baltic and Slavic is a regular morphophonemic realization of zero-grade ablaut in Baltic-Slavic.* There is an apparent phonological problem with the -th- in Sanskrit, *th in Avestan. This indicates a cluster with a long dental. Since this cluster has no effect in the other languages, we can simply assume that the verbal stem was *roth₁₁-. In this case we may doubt whether the nouns represent an original PIE stem. Derival derivative with o-grade ablaut remained productive for millennia in the Indo-European languages, and these words, therefore, could have been formed in the various languages at any time. However, the nouns are supported by better evidence and are more consistent in meaning than the verb. They should be reasonably old. If we wish to make this a PIE stem, we should consider it an adjectival derivative, with gender varying according to the gender of the modified noun. This can account for the fact that the resultant noun is masculine in Indo-Iranian, feminine in Latin, and neuter in Germanic. Substantivization of adjectives is a normal process in Indo-European languages. For the verbal meaning, we might as well believe the

Proto-Indo-European and the Wheel

There is a complex of reconstructed words that are PIE, but not demonstrably PIE, and that deal with wheels and wheeled vehicles:
meaning of the Balto-Slavic verb, since this is is of no help. The adjective *știh- at, *um-2 could then mean rolling. This adjective could apply to either the wheel or to the vehicle to which the wheel is attached.

*șf, șf (n.) ON șf, L. axi, Gr. ăkhis, OCS aš, Lith. ašis, Skt. ăsates, Ar. ašla (n.), Irish aṣa (n.)

*nābh- (n.) On Nābh, OP ṇābhi, and Germanic (OHG nāba, OE nafe, ON naŋ) the word for naval is used for the hub of a wheel. This may be accidental convergence in meaning in Germanic and Sanskrit. Old Prussian was subject to strong influence from German.

*āvekg- (transport by vehicle): OCS reņq, Lith. tėfž, Skt. nākṣat, Ar. nāfan-āla, Latin nēve (transport), OE rungon (carry). The e-grade derivatives from this verb typically in-āvā wheeled vehicles: OCS reņq-wagon), ON augen, Gl. wēgkhe (crt.), Mycenaean Greek 3aν(ηκ άt (chariot). This is only one of the meanings of this verb. Although the e-grade derivatives support the claim that this term was used for transport with wheeled vehicles before the breakup of PIE, it is clear that the vehicles in question did not have to have wheels. In Lithuanian, Russian, Old Church Slavic, Latin, and Sanskrit, the verb could apply to transport in ships, and in Lithuanian and Russian it can apply to sleighs. Latin vehiculum and Sanskrit svahīnem, which may be cognate (*āvekgāsām) or may be independent derivatives with a productive suffix, both mean either wheeled vehicle or boat.⁴

None of these words have cognates in Anatolian languages. Unfortunately we do not know the words for ac통 or hub in Anatolian. The only specific disagreement is in the word for wheel. The Hittite word is hursi, which can be a derivative from a verbal stem *Hur-series found in Sanskrit surjati (swine, turn). Tocharian A avukhts (wheel, circle) may be formed from the same verbal stem but is not at all the same word. The word for augen in Hittite is ḫuriga, and ḫaligenes is some other kind of vehicle.

Despite the fact that we can never be sure that the absence of this set of cognates in Anatolian is significant, it should be useful to try to find out whether it makes sense to talk of a split between Proto-Indo-European and Proto-Anatolian after the time of the introduction of the horse, wool sheep, and the yoke, but before the introduction of the wheel.

The date and place of the invention of the wheel is open to question, assuming there was only one event of this nature. Pigott (1991: 18) dates the appearance of signs for wheeled vehicles in Mesopotamia to the last fourth millennium. Actual wheels are not found there before the third millennium, a fact that must be considered alongside the fact that in the rest of the world we do not have a written record.

On the Question of the Anatolian Origin of Indo-Hittite

All we have are the first attestations of wheels or models of putative wheels in archaeological finds. Actual wheels are found in various places in Europe in the second half of the fourth millennium. An axle and wheels unit found in Switzerland dates from c. 3400 B.C. (Pigott 1991: 18), Sherratt 1996). A clay model of a wagon with four wheels found in the Baden culture in Hungary dates from between 5400 and 2900 B.C. (Gimbutas 1997: 255), and a burial with a two-wheeled cart found in the steppe tentatively dated from the fourth millennium (Gimbutas 1997: 83, 187). In the TRB culture, representations of apparent wheeled vehicles on pots from Bronocice and Ostrówiec Świętokrzyski are likewise dated to the last half of the fourth millennium (Medley 1992: 378-79).

In the area north of the Caucasus, a wagon burial is found in the Novovodobnaya (late Maikop) culture (Konstantinov and Reznik 1988, Chernykh 1992: 69). Numerous wagon burials are found on the steppe north of the Caucasus in kurgans of the Novoisotnitsa type (Chernykh 1992: 79, Gei 1993). I noted above, in the discussion of the horse, that the approximate dates of the Maikop culture were 2700-2750 B.C. There is no radio carbon date associated with a wheel or wagon found in the Maikop culture. Two carbon 14 dates have been assigned to wagon burials found in kurgans of the Novo-Tinorovka culture immediately north of the Maikop culture. One has a carbon 14 date of 4440 ± 40 BP calibrated to c. 3100 B.C. (Gei 1991). Gei points out that this burial is typologically younger than other wagon burials in the same culture. However, a typologically older burial has a date of 4270 ± 40 BP, c. 2900 B.C. One of the two sites must have had some contamination, and since the younger date was found closer to the surface, he thinks that that date is most suspect. Overall, we have no evidence to place the appearance of the wheel in the steppe or the northern Caucasus before the beginning of the second half of the fourth millennium, and it is almost surely did occur before the end of the fourth.

In the southern Caucasus, burial with wagons have been found in kurgans at Bedeni, Tsotai, and Zoraketsi (Manon 1973). As of Khashnev and Chabanislovii (1970: 113) a wagon found at Bedeni was the earliest known wheeled vehicle from the southern Caucasus. Chernykh (1992: 98-102) locates the Bedeni culture in the middle Bronze Age, beginning in the period 2700-2500 B.C. The kurgans on the Bedeni plateau are dated by Kvaradze (1983: 108-9) in the second quarter of the third millennium B.C. Kvaradze used carbon dating, but the actual kurgans for which dates are available are in the Alazan valley, considered contemporaneous with the Bedeni culture. Dating from about the same time are models of wheeled vehicles found at Shakar (Khachaturian 1975: 76, 77). The Tsotai kurgans with wagons are later, dated by Kvaradze (1983: 117-48) in the second half of the third millennium. The Zoraketsi burials are considered to be part of the Tivedi culture.

Because disc-shaped clay objects with holes in their middles have been found in a variety of cultures, some archaeologists, seeing model wheels among these objects, push the origin of the wheel much more deeply into the past. Khashnev (1997: 1995).
170) asserts that model wheels "have been found at many archaeological sites in southwestern Asia dating to the sixth to fourth millennium B.C." Dies (1981) identifies model wheels from the Cushan A and Karanovo VI cultures as "of the steppe" (Gimbutas 1985, 1997: 308-9; Dies 1981). These cultures date to the mid or late fifth millennium. Munchmayer (1975: 200) claims to identify model wheels in the southern Caucausan site of Títík, dated in the fifth millennium. These claims depend on the ability to distinguish model wheels from spindle wheels. No model wheels are found associated with model cars in the southern Caucasus until the third millennium, at roughly the same time that actual wheels are found. Dies's model wheels are at least large, up to 11 cm. in diameter. Jeannine Davies-Kimball, however, in a personal communication, suggested that even these wheels are not too big to be spindle wheels used for barb fibers. Green (1998) also discusses "model wheels" found in the Cushan-Tripol'tye culture but offers a different argument. He argues that some discs are too light to serve as spindle wheels. The discs he modeled are 1-6 cm. in diameter, 0.3-0.7 cm. thick. He suggests that they were used as wheels for vehicles in the shape of cones, found with legs and hoofs in their feet that could hold wooden axles for the wheels. Earlier zoological vessels without these hooves exist. These are found in the general area where ceramic models of sledges are found (western Tripol'tye) and at or after the same time or slightly earlier (c. 4000 B.C.). Nevertheless, Guev himself notes that zoological figures with wheels on their feet do not guarantee the existence of wheeled vehicles; wheeled animal figures are found among the Abashe, who never used wheeled vehicles (Piggott 1983: 19).

We can now look at the effect of using the appearance of wheeled vehicles as a *terminus ante quem* for the division of PIE into PIE and PA. Candidate PIE cultures must therefore show evidence of having had wool, horses, and ox traction before the introduction of the wheel. In the Khabdzhyk-Yamnaya complex of the Russian steppe, evidence of wool and horses clearly precedes evidence for wheels. Since there is early evidence for sledges, it seems reasonable to assume that ox traction with sledges preceded wheeled vehicles. In the southern Caucasus, we must ignore the arguments for model wheels as evidence for wheeled vehicles. If we do that, then a thousand-year chasm separates the earliest evidence for yokes, wool, and horses from the earliest attestations. Curiously, no early evidence of wheels has been found in the area of the western part of the Black Sea, around the Bosporus. This could be accidental; some of the earliest evidence for wheels is found in the Danube valley. However, a mountain range separates this area of the Bosporus from the Abashe culture, which has the earliest evidence for wheeled vehicles. The Sherratt and Sherratt (1998) proposal then remains viable.

Not only are these areas viable possibilities, they seem to be among the best possible candidates in the ancient world. Mesopotamia proper had the wheel before the appearance of horses. When we look at central and northern Europe, we find it difficult to argue that "no technology appeared before wheeled vehicles. In the Asian steppe, Indo-European-like cultures appear after the appearance of such cultures west of the Urals, and there are reasonable arguments for an expansion of the steppe cultures east from the Russian steppe. In this view, the movement involved initial populations of an area that had previously been essentially unusable for human habitation. Little doubt exists, therefore, that the population would have brought its language with it.

Despite the general viability of the area south of the Caucausan, the specifics of the Gamkrelidze-Tronov proposal do not fit this hypothesis. Not only do they treat the word for wheel as PIE; they have the dispersal of the PIE branch of the family from the southern Caucasus. They treat the Yamnaya culture as Indo-European, not Proto-Indo-European (it is, according to them, the homeland of the north-west European branch of PIE (Balto-Slavic, Germanic, Celtic, Italic). Since the Yamnaya culture existed for several centuries before evidence for the appearance of wheeled vehicles in the steppe, this hypothesis runs into trouble. It becomes very difficult to explain why the Indic languages, which spread directly from the southern Caucasus, have the same word for wheel as NW PIE languages, which apparently did not get the wheel until they got to the steppe. There may be ways to salvage a southern Caucasian homeland, but I do not have time to discuss them in this forum. I will spend the rest of my time contrasting the Sherratt and Sherratt proposal with the Mallory-Gimbutas proposal.

The Sherratt and Sherratt Proposal

Like Renfrew the Sherratts (1998) attribute the origin and initial spread of the ancestors of the Indo-Europeans to the development and spread of farming from the Near East. They, however, attribute the initial stages, which involves the spread of the ancestral language into western Anatolia and the Balkans, to what they call preProto-Indo-European. Pre-Indo-European itself, according to them, was formed by convergence in the trading network around the western end of the Black Sea.

One of the phenomena noted by linguists in areas such as the Caribbean or Melanesia is the formation of pidgins and creoles in connection with the growth of trading networks, especially those based on coastal contacts. If we postulate a coastal link between the shores of the Black Sea (and perhaps also the Aegean... in the 3rd millennium, bringing together elements from *...PIE languages already existing in Anatolia and perhaps in the Balkans and western Pontic area, these requirements are fulfilled. After all, boats—both large and small—are attested in the protolexicon as well as horses and carts, and the wealth of Troy II indicates the potential scale of trading at this time.

They admit that population movement might have played a role in the spread of the language. The examples they cite are the movement of the Pit Grave
There would likewise be no possibility that PIH speakers formed part of the Yarmaya or Piro-Grave culture of the steppes, which begins even earlier. This is important for a variety of reasons.

Sherratt and Sherratt mention two instances of population movement connected with the spread of PIH. One is the colonizing of the newly unpopulated Asian steppes from the Russian steppes by people of the Yarmaya culture. The other is the movement of Yarmaya people up the Danube valley. This movement is discussed in detail in Sherratt (1988a:37–38). He provides the best evidence of which I am aware for population movement into a populated area of Europe. He notes a drastic reduction in the number of populated sites in the early fourth millennium, which he interprets as due to salinizations of the soil. Then, in the period 3500–2900 B.C., burial mounds of the steppe type occur in the Hungarian plain. The material culture and burial rite represented by these mounds differ sharply from the local Baden culture. Moreover, the tumuli extended eastward to the Pontic steppe, which is the center of their distribution, while the Baden culture extends farther to the west than the tumuli. In general, the tumuli are found in areas unsuitable for farming but suitable for stock raising. Sherratt suggests that steppe population moved into vacant or abandoned land in the Hungarian and Romanian plain. This is not only movement by a population that should not have spoken Sherratt and Sherratt’s proposed kievan; it is a movement that began well before they propose that the kievan was formed.

Chernykh (1980) traces the Kuro-Araxes culture in the southern Caucasus, the source of the new metallurgy using arsenical bronze and bivalve molds for characteristic shaft-hole axes. Beginning in the second half of the fourth millennium, this new metallurgy spread across the Russian steppes into the Balkans and up the Danube valley. He says that these characteristic axes do not show up in any quantity in Anatolia until the late third millennium. This is confirmed and better illustrated by Chernykh (1992). The map in Chernykh (1992:147) shows that arsenic bronze was firmly established in the southern Caucasus, the Pontic steppes, and the Danube valley in the early Bronze Age at a time when it is only rarely found in northwestern Anatolia. The spread of arsenic bronze into this area is mainly in the middle Bronze Age, which for Chernykh begins about 2700 B.C. (cf. the map on p. 49). The maps on pages 156 show a similar pattern in the distribution of shaft-hole axes. Yakar (1985:383–84) also argues that the metallurgy of northwestern Anatolia in the early Bronze Age is to be traced to the Ezero-Cernavoda-Cotofeni cultures of southeastern Europe.

I also noted earlier that Sherratt (1983b) says that wool sheep are found in the Pontic steppes, the Balkans, and eastern Europe in the fourth millennium, that the steppes route is the most likely vehicle for the introduction of wool sheep, and that the Corded Ware culture was the vehicle by which wool sheep spread throughout northern Europe. Apparently, Sherratt still believed this in 1994 (1994:187): "A powerful new culture developed, characterized by pit graves covered by wooden beams and marked by a round mound or lorgar. . . . Small groups of these people
penetrated along the Danube into northern Bulgaria and up into eastern Hungary. It was through such eastward connections that ultimately Near Eastern features found their way into Europe other than via the Aegean: wool-bearing breeds of sheep, and a metal casting technology using the two-piece mold"; (1994: 171): "In Northern Europe, fundamental changes which incorporated the full potential of plough farming and pastoralism only took place after 3000 B.C. with the spread of the Cored Ware complex."

It is also true that an economy based on pastoralism appeared first in the Pontic steppe and then spread into central and northern Europe. Sherratt (1982a) documents the arrival of pastoralists in the Hungarian plain in the second half of the fourth millennium.

We thus have evidence that the population movement from the steppe and the spread of wool sheep, new metallurgy, and an economic system based on pastoralism began in the fourth millennium, well before the formation of the Sherratt's version of PIH. Moreover, the source of this movement, as far as central and northern Europe are concerned, is the Russian steppe. This clearly indicates that the Sherratt's version of the spread of PIH could not have been aided by control of access to wool and aneculic bronze, cannot be supported by any known population movement, and followed rather than accompanied the spread of the new, pastorally based economy. All this makes it appear that the Sherratt's cultural arguments for the spread of PIH (really PIE) would work much better if they had located the beginning of the spread in the fourth millennium and if they had identified the geographical source as the Russian steppe, rather than the western end of the Black Sea.

However, to argue that the Yamnaya culture was actually the source of the spread of Indo-European in Europe, we need a better scenario for language spread than that offered by the Sherratt.

I will now turn to a more detailed discussion of the spread of Indo-European into Europe. Indo-European is the proper term here since we are discussing a time after the separation of the Proto-Anatolians from Proto-Indo-Hittite. I will focus on the spread into north-central Europe because it was in northern Europe that the Corded Ware culture was formed. No one has yet figured out a coherent linguistic history of Europe without assuming that both the Corded Ware and the Yamnaya cultures were predominantly Indo-European speaking, and yet there is no general agreement about the relationship between these cultures.

I noted in the introduction that the spread of Indo-European is an important fact about the prehistory of Europe and that prehistorians ignore it at their peril. One can always argue that similarities in culture are due to parallel developments or exchange of information among ruling elites. One cannot apply that argument to language. If two culturally distinct areas, distant from each other, speak the same language or languages that are genetically related, then the language of at least one of them did not develop independently in place. This fact can and should color our interpretation of other developments.

On the Question of the Anatolian Origin of Indo-European

The relationship between the Corded Ware and the Yamnaya cultures is controversial. There are many similarities between the two cultures and also significant differences, or they would not be different cultures. All the items of similarity, with the possible exception of wheeled vehicles, appear first in the Yamnaya culture, later in north-central Europe. These similarities include an economy based on livestock breeding without long-term settlements, the use of tumuli burial (less consistent in the Corded Ware), evidence of social hierarchy, cord decoration on pottery, the presence of domesticated horses, wool sheep, and wheeled vehicles. Chernyh (1992: 138–39) suggests that the Corded Ware culture used the casting techniques and bivalve molds associated with the metallurgy that spread from the Caucasus through the steppe. However, it initially used unalloyed copper rather than aneculic bronze.

We thus see a variety of examples of cultural flow from the southeast to the northwest. When we look for evidence of a flow in the opposite direction, we find little or nothing. No case can be made for any cultural spread from north-central Europe into the steppe at any time relevant to the spread of Indo-European.

One might still argue that the Corded Ware culture developed with little contribution from the culture of the Pontic steppe, and respected scholars have (e.g., Hauser 1981, 1983). The economy of north-central Europe was ready for the addition of a mobile pastoral component and could have developed one on its own. A more stratified society may also be the result of internal evolution. The idea of tumuli burial might spread from elite to elite without much movement. Horses and wool sheep might have been acquired by trade, and metallurgical technology could have spread with the movement of very few people.

In that case, however, we must ask why the two areas ended up speaking Indo-European. Again, no case can be made for a cultural spread from north-central Europe into the steppe at any time relevant to the spread of Indo-European. The Corded Ware culture was an economically robust, aggressive, expanding complex, which spread in almost every direction except back into the steppe. All indications are that it had good trade (or exchange) networks with rapid communication and spread of innovations. There is no obvious case where it came in contact with any group in northern or western Europe that was economically, culturally, or militarily superior to it. It seems highly unlikely that the Corded Ware culture, once formed, would have adopted anyone else's language. To account for both the linguistic and the archaeological facts, we must assume that Indo-European speakers played a role in the formation of the Corded Ware culture. This does not necessarily mean that the Indo-European speakers were new emigrants from the steppe, nor does it mean that the Corded Ware culture must have been formed on or near the steppe and then spread throughout northern Europe. It means that Indo-European speakers were on hand at the time and place where the Corded Ware culture was formed.

The Corded Ware culture is apparently the last and best candidate for the kind of influence from the steppe that could have led to the spread of Indo-European into north-central Europe. It was also temporarily very close to the first. Since the
Their distinctive items of pottery—the globular amphorae with corded decoration, is very similar to amphorae found at Mikhailovka I on the Black Sea. Moreover, the Globular Amphora culture covers areas with two previous cultures—the Tripol’e and the TRB cultures, and the TRB extends further to the west than does the Globular Amphora culture.

The genetic, anthropological features of the males (not the females, a point that becomes important later) in burials associated with steppe expansions, including the Globular Amphora culture, are initially similar to the features of males of the steppe population (Necrozov 1980). These similarities were apparently also evident among the early kurgan burials in Hungary (Eczy 1979: 91–93). The similarity decreases with distance from the steppe (Schindler 1980, Menk 1980).

Menk (1980) performed a multidimensional computer analysis of cranial measurements of many different populations of eastern and central Europe in the Eneolithic and early Bronze ages. He used the computer analysis to compute morphological distances between populations. While his measurements are obviously limited to skull shapes, his figures show significant differences in the relevant areas of Europe before and after Gimbutar's waves of kurgan expansion. His chart (1980: 382) of computations of morphological distance from the Dniepr/Yamnaya (oldest Yamnaya) population shows the most interesting results.

The chart has arbitrary units, with the maximum difference between populations being fourteen units. In the east, the Yamnaya population is closest (only one unit of distance) to the pit-comb culture, which covered a large area of European Russia from the Baltic to the Urals. On the other side of the Urals, the Siberian Neolithic population is more distant (six units). The Dnieper-Dniester/Subboty Stok population is quite distinct from everything else, being a distance of eleven units from the Yamnaya population and fourteen units from the Tripol’e farmers immediately west of them (cf. chart in Menk 1980: 380). Menk characterizes the Dnieper-Dniester population as palus-europe. Compared to the Yamnaya population, the farming populations to the west and northwest are quite distinct: Tripol’e ten units, Gumelnita thirteen units, TRB nine units.

After the second kurgan expansion, the Dnieper-Dniester population essentially disappeared, according to Menk. In the areas where the Mediterranean-type farmers had lived, we find populations that were much closer to the Yamnaya population. Among the Black Sea the Ustovo and the Gornodok-Horodiatska-Fedchenki populations have a morphological distance of only about three units. Farther west, the Cernești culture has a distance of six units from the Yamnaya, but, interestingly, its deviation is more toward Dniester-Dniester characteristics than toward those of the Mediterranean-type farmers who preceded this culture (cf. chart in Menk 1980: 373). In the northwest, the Globular Amphora culture in Romania and Poland has a distance of only three units from the Yamnaya. By the time the Globular Amphora culture reaches the territory of modern Germany, however, the Yamnaya influence has disappeared. If more data were available from Poland, we would expect it to
Some linguistic evidence suggests that the Indo-Europeanans did not always respect other people's property. In Avestan, Greek, and Latin, the verbal stem *ą�e- (drive), when it takes a domestic animal as object, can mean to drive off that animal as loot. That the same construction was possible in Celtic is shown by the Old Irish expression cainn mou (steal cattle e "taing nas goine", literally a drive of cattle). Bruce Lincoln (1981) reconstructed a "Myth of the First Cattle Raid" for Indo-Iranian and found traces of a similar myth in European IE languages. Interestingly, a Vedic hymn relating to this myth uses the verb *ą�e-, although in this case it can be interpreted literally:

*jī ḫuk uddāj apāddīk Vādāya . . . . . sū, saṁbūs, Indra

(he who drove out the cows by the wounding of Yla . . . . . . he, a man is Indra) Macdonell, 1992: 40

In this passage the verb uddāj is from *uiddāj (3rd-pl-st-decl-nom sing.).

Lincoln points out similarities between the myth and practice of the Indo-Iranians and those of tribes in Africa with cattle-based economies. This does and should raise questions about the naive assumption that similarities in the mythology of Germanic and Indo-Iranian justify a reconstruction of that myth in a common ancestral society. Nevertheless, the argument remains as a typological argument, if such mythology and practice arise naturally in relatively primitive stock raising economies, then we should not be surprised to find evidence for such practices and mythology in early IE society.

Bogucki (1988) paints a fairly grim picture of the economy of the farmers in northern Europe before the arrival of the pastoral society. There was no shortage of land; the problem was labor, since the production of food through farming was labor intensive. Farmers had small herds of cattle as a hedge against crop failure and for food supplies to get them from one crop to another. The herds were too small to be stable, and any loss of livestock could have been catastrophic. Wild animals did not constitute a significant source of food, probably because game animals had simply moved away from the areas undergoing concentrated farming. Evidence exists of the use of wheeled vehicles shortly before the arrival of the steppe pastoralists, but there was little open space between fields in the loess belts where farming was concentrated.

Bogucki expresses doubt that the wagons were used for more than transporting supplies and grain to and from the fields in the immediate area.

The arrival of steppe pastoralists in his area would have provided both a threat and an opportunity for a farmer. On the one hand, a raid on his herd of livestock could lead to starvation before the end of the next growing season, even if none of his family were hurt in the raid. In any conflict, the pastoralists had several advantages. They were more mobile, having horses, so it was easy for them to select targets where they had immediate superiority in numbers and to get away quickly. Moreover, they did not have a fixed base to defend. They could therefore intimidate small farming communities even without attacking them. A modern-day parallel is
found in Asia. Zvelebil (1995) notes that in Asia, when semi-nomadic pastoralists come into contact with farmers, they tend to oppress the farmers.

On the other hand, the pastoralists offered the farmer an easy source of new livestock that he could get in exchange for farm produce. The pastoralists also brought with them a new and valuable product—wool. The new technology in metals production would also have been valuable. The herders themselves did not have any real motive to exterminate all the farmers in the area, since they needed grain for their diet. At the frontier of the expansion, the choices offered the local farmers would likely have been rather sharply differentiated: the risk of ruin by suffering raids on their livestock or alliance with the herders, perhaps raping some of the benefits of raids on farmers farther along the path of pastoralist expansion. This could have led to the rapid expansion of a combined herding-farming society.

Because economic ties are established through marriage in most primitive societies, considerable intermarriage likely occurred between the pastoralists and the farming population. In the same way that the kinship structure of PIE suggests a society that practiced exogamy, if the leading edge of the diffusion of pastoralists was made up of single clans, or of single males striking out on their own, the intermarriage at the leading edge of the expansion could have been almost complete. I also noted above that while the males in the kurgan burials typically have the physical traits of steppe populations, the females do not. This could explain why the genetic traits of the steppe population diluted rapidly as the pastoral society moved farther into northern Europe.

We could argue that in the second half of the fourth millennium—at least temporarily—the areas of Europe adjacent to the steppes formed good candidates for what Johannich Nichols (1998) has called linguistic spread zones, that is, large open areas with low population density. Controversy continues, however, over whether the depopulation of the upper Balkans was due to an invasion from the steppe (Gimbutas's first wave) or to internal events such as change in climate or salinization of the soil. For our purposes it does not matter. The depopulation had occurred, so there was plenty of land with very little population. Thanks to Sherratt (1981, 1982a), we know that this is the appearance of the steppe herders in the Hungarian plain a massive reduction in the number of farming sites occurred—almost certainly due to salinization of the soil. In northern Europe there is no similar research to indicate significant abandonment of sites due to over-farming, although there must have been some. Huge areas in the north European plain were empty, however, except for sparse populations of hunter-gatherers. Pastoralists could have moved in without any initial obstacles. Both the pastoralists and their language would have found these areas convenient spread zones.

Consequently, we have a picture of a rapidly expanding pastoral society, which could combine and intermarry with the local population and introduce an economic system considerably wealthier than the preceding culture. As Sherratt says, this society was granted to take full advantage of the secondary products revolution.

On the Question of the Anatolian Origin of Indo-Hittite

In terms of language, we would expect that intermarriage and the fact that farmers had to deal with herdsmen for essential items like wool and metal products would lead to considerable bilingualism. Since the pastoralists had spread far and fast, while the farmers had been in place for a long time, we would expect that the pastoralists' language would be more consistent over a large area, while the farmers' languages would be broken up into many distinct and distinct languages. Since wool sheep, new metallurgy, and domesticated horses were introduced from the steppe, we have reason to suspect that the new arrivals could have maintained control over access to these goods. Thus, the language of the steppe people would have been a much more useful language to know. The exchange networks that brought wool sheep and horses to the area were initially controlled by speakers of the language of the steppes; so we should expect that, when exchange networks evolved into trade routes, the language of trade would be the language of the steppe pastoralists.

What this means is that instead of spreading because it was a trade language, PIE became a trade language because it had spread. The facts that PIE was spoken over broad areas with less variation and that it was associated with a society both militarily and economically superior to the competing systems made PIE a logical candidate for a regional language, when the need for such a regional language developed.

If this scenario for the spread of PIE into northern and central Europe is correct, then we are in a position to offer some justification for the claim that the division between PIE and PA occurred before the arrival of wheeled vehicles. The physical and cultural separation of the PIE speakers in the Yamnaya and the Corded Ware cultures provides the prerequisites for linguistic differentiation within the PIE branch of PIH. Because cultural differences had been established by 3200 B.C., the time of the beginning of the Corded Ware culture, we can assume that linguistic differences were also developing. Because any differentiation in the PIE branch must follow the establishment of PIE by the division between PIE and PA (including time for any common PIE changes), we have a second line of reasoning that posits a terminus ante quem for the division between PIE and PA in the second half of the fourth millennium B.C. And because evidence of wheeled vehicles in the steppe also occurs in the second half of the fourth millennium, we can assume that dispersal of PIE speakers started shortly after they acquired wheeled vehicles.

Finally, we can return to the issue of the time and place of the division between PIE and PA. The Yamnaya culture is dated by Mallory (Mallory and Adams 1997:651) from 3600–2200 B.C., and there is broad agreement that it developed from the Khvalynsk and Sredny Stok cultures in the same area. Because these cultures existed without strong external cultural influences for several centuries before the occurrence of evidence for wheeled vehicles, there should be a strong presumption that the Khvalynsk and Sredny Stok cultures are strong candidates for the home of PIH.

Do we have evidence for expansion of the steppe culture toward western Anatoia before the introduction of the wheel? Yes. The sequence of depopulation...
followed by the appearance of people with the economy and burial habits of the steppe began in the fifth millennium in the Balkans, and the subsequent close cultural interaction across the Bosporus, which Sherratt and Sherratt use to justify the formation of PIE, could be evidence of the kind of contact that we would need to explain the spread of PA into northeastern Anatolia. Jasanoff (1985: 106-12) documents steppe-type burials in the area of the Black Sea east of the Danube from the time of about 3500 B.C. (uncalibrated). Wheels and wheeled vehicles occur in groups that he attributes to the second half of the fourth millennium. His dates are not based on carbon 14 measurements but on comparison to burials with uncalibrated dates elsewhere. In calibrated dates, this would indicate the appearance of steppe burials about 4400 B.C., with the appearance of wheeled vehicles about 3200 B.C. As late as 3500 B.C. (calibrated) we find the Usturun culture along the Black Sea east of the Danube, which is described as a hybrid culture containing elements from the steppes and from the Tripol's culture (Mallory and Adams 1997: 614). Noktiniy (1987) describes this culture as having had both the tall sheep, which he associates with woolly coats, and the shorter Neolithic-type sheep. They definitely had horses as well (Ponikova 1979: 145). There is no direct evidence of ox traction, but since Gesev (1998) notes the existence of model sledges on shtrems in the Tripol's culture as early as 4000 B.C., we can hope that ox traction existed. If Usturun was indeed a hybrid culture made up of steppe pastoralists and Tripol's farmers, the language of the steppe group could easily have already become separate and differentiated from that of the rest of the steppe. The local farmers would have had the same motivation—the same choice for survival—as the farmers of central and northern Europe to adopt the pastoral language. The same could be said of other hybrid cultures farther west along the Black Sea, such as Cornovoda and Ezeren. One of these may have produced Proto-Anatolian.

Close cultural ties across the Bosporus were established in the fourth millennium. I mentioned earlier that Yaker attributes the metalurgy in northeastern Anatolia to influence from the Balkans, and bronze bones are found at Demirci Hoyuk in the fourth millennium. Demirci Hoyuk is on the most logical route leading from the Bosporus to the central Anatolian plateau, where Hittite was historically located. A third millennium sequence of destruction and depopulation, followed by a switch to a more pastoral economy, covers the area where Luvian was historically distributed. (Mallory 1989: 28-29).

The Bosporus is probably not the focus of the original separation between PIE and PA, although it may have been the focus of the final separation. The linguistic separation was probably started with the establishment of hybrid cultures in the Balkans. Anthony (1991b) argues that the original boundary between the Tripol farmers and the horse-oriented steppe culture was at Dnieper. On that basis we could speculate that the place of the division between PIE and PA was the lower Dnieper, and the time was between the arrival of ox traction and the arrival of the wheeled, probably in the first half of the fourth millennium.

On the Question of the Anatolian Origin of Indo-Hittite

Notes

Most of the research for this article was done while in residence at the University of Chicago Humanities Institute, now the Franke Institute for the Humanities. I would like to express my gratitude to that institution for the facilities and the time that enabled me to work on this project.

1. An alternative explanation for the lack of a prosthetic vowel in Greek is offered by Martin Peters, Untersuchungen zur Verbreitung der indogermanischen Laryngale im Griechischen, Wiesbaden: Verlag der akademischen Druck- und Buchhandlung, 1980, 236. 18. Following a suggestion by Rix, he proposes that word-initial laryngal disappeared before m plus a following syllabic liquid. This works not only for the word for > xwe, but for a variety of other forms.

2. To get these percentages I combined the data from tables on pages 75 and 85, weighting the percentages by the number of individuals given in appendices 5 and 7. pp. 157-63.

3. Parallel examples are Lith. bėtės (bird), krišt (kiss), kryž (wreath, wreathing), (fast flying) eršt, (fast) kriš (fly) (Gardiner 1943: 73-74).

4. Craig Melchert, in comments on this paper, pointed out the existence of a verb meaning "drive" in Hieroglyphic Luwian of the shape PE52 well-e-i-a., which could be from *"ne:kyl-eh*-. While not a direct cognate of the word *"ne:kyl-eh*-, it is certainly a possible instance of the root.

5. I discussed this in detail in an article, "Indo-Hittite and the Caucans," which will appear in the proceedings of the Chicago Conference on Caucans, May 1999.

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1985 The Late Prehistory of Anatolia: The Late Chalcolithic and Early-Bronze Age. BAR International Series 208. Oxford: British Archaeological Reports.
I am grateful to Robert Drews for inviting me to play a role in this Colloquium. I particularly welcome this opportunity because I feel that the issues addressed here have not always received, from what I have termed the "Indo-European establishment," the full attention and respect that they deserve. I trust that all will understand that I cannot do full justice to these four data-rich papers in the time available to me. I can only give a reasoned critical evaluation of the analyses and arguments presented—within the limitations of my own expertise—citing enough specific points to justify my assessment.

I begin with the paper of Margalit Finkelberg on the language of Linear A, which stands somewhat apart from the others in both subject matter and methodology. A marked strength of her presentation is the logical and linear progression of the argumentation. She first presents evidence (such as apparent matching word pairs between Linear A and B) to support the generally accepted phonetic values of the syllabic signs of Linear A. While a full decipherment might bring some refinements, it seems that we must take the established interpretation of the syllabic signs as essentially correct. This point is of fundamental importance, since it effectively precludes any solution to the problem of the language of Linear A along the lines recently successfully advanced for Cretan: i.e., a radical revision of the sound values assigned to the individual signs. We must essentially deal with the language we have.

Professor Finkelberg next tries to establish what she terms a phonological and morphological "profile" of the language of Linear A. She then compares this with the profiles of various languages of the Eastern Mediterranean that have been or might be proposed as candidates for the language of Linear A, very effectively reducing the number of reasonable possibilities. While one must always allow for the possibility of under-representation of linguistic distinctions in a writing system (and even for the existence of a typologically unusual member of a given language family), her arguments against Linear A as representing a form of Greek, Semitic, or Hurrian are convincing.

Having shown that the "profile" of "Minoan" is incompatible with that of an Indo-European Anatolian language, Professor Finkelberg then (and only then) turns