

GREATER ANATOLIA
AND THE
INDO-HITTITE
LANGUAGE FAMILY

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Robert Drews

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ON THE QUESTION OF THE ANATOLIAN ORIGIN OF INDO-HITTITE

Bill J. Darden

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 fitting 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 not artificially reinforced by a government or an education 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 com-

munication 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, Kashubian 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 only 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 at 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 (*proto-* indicating the posited 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 versions 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 PIH. 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 PIH.

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 PIH is that the first division in PIH 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 protolanguage L. If a form from L shows up in A with the meaning *oak* but in B has the meaning *birch*, 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 reconstructible form and meaning must refer to an envi-

ronmental feature with broad distribution, which lessens 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 of 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 items to establish a *terminus post quem* for the first division of the protolanguage. 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 in Anatolian of a whole set of terms relating to wheeled vehicles, could place the division of PIH 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 PIH 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 PIH 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 PIH 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 PIE 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 men 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 first survey the proposals that I will evaluate.

Thomas Gamkrelidze and Viacheslav 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.

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 creolization 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 Sredny 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 items possible to trace in the archaeological record. The items demonstrably PIH are words for *wool*, *horse*, and a group of words associated with ox traction: *yoke*, *harness pole*, and possibly *harness*.

PIH Cultural Terms

(1) HwLHN- (WOOL)

Skt. *ūrṇā* Av. *varəna*, Gk. (Dor) *lānos*, L. *lāna*, Lith. *vilna*, S.C. *vūna*, OE *wulla*, Hit. *hulana*-.

All of the forms can apparently be phonetically derived from a stem *HwLHN-. There have been some questions raised about the presence of two laryngeals in the stem. Hittite is the only language that indicates a word-initial laryngeal. This raises a question about the lack of a prothetic vowel in the Greek word. Initial laryngeal-consonant clusters in Greek often result in an inserted vowel. Beekes, one of the enthusiastic proponents of this hypothesis, is only willing to say (1988: 71) "It seems that a laryngeal was vocalized before *wl*- (and *wl̥*-)." He qualifies both his example etymologies with "if." Similar problems might be identifiable with the relatives of

Hittite *hurki* (wheel). Gamkrelidze and Ivanov (1995: 623) relate this to a family of words that began with **Hw(e)r-* and meant *rotate* or *turn*, including **Hwer-t-* and *Hwer-gh-*. Pokorny (1994: 1,157) relates Greek *hratánē* (stirring spoon) to this family of words. Another possibility is Greek *rhombomai* (rove, roll about), and *rhombos* (magic wheel), which may be from an *n*-infix form of **w(e)r-g^w-*. If this is correct, then there is something yet to be explained about the relationship between initial *h* 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 **wlHn-* to **Hwlⁿ-*. He leaves the initial laryngeal in *hurki*, however.

Gamkrelidze and Ivanov (1995: 178) suggest that the original form could be **Hwlⁿ-*, with the word-initial laryngeal influencing the syllabic *l* to form the long syllabic that is reflected by the acute intonation in Lithuanian and the long *ā* in Greek and Latin. If this were a phonetic process in PIH, then we could not postulate an initial sequence **Hwl-* in the Greek words *lákhnē* (woolly hair, down), and *lákhnos* (wool), and could not relate them to other words for wool and hair. We would also expect to find the reflexes of long syllabic *r* in the zero-grade of words for (turn), which they (1995: 623) postulate as **Hwer-th* and *Hwer-gh*. We do not find this reflex in Lithuanian *virto* (turned over), or Sanskrit *vṛktá* (twisted). We would also expect a long *ū* in Sanskrit *ubhnāti* (ties), and Avestan *ub-daēna-* (made of cloth), which they (1995: 499) derive from a root **Hw-*.

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

Overall, the evidence for a laryngeal after the *l* is about as good as we can get, without an overt *h* in Hittite. (Only one laryngeal shows up as *h* in non-initial position in Hittite, and then not in this environment.) This includes the reflexes of “long syllabic *l*” 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 *h* 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 *h* in Hittite can correspond to an *o*-coloring or an *a*-coloring laryngeal. The *a*-coloring laryngeal does condition epenthesis in Greek, as in *āēsi* < **H_aweHti* (blows) compared to Hittite *huwant-* (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 **H_ow-*. Because it had a rounding effect on an adjacent vowel, *H_o* may have shared enough characteristics with *w* that it could easily be absorbed by *w*.¹

(2) *HEK̄WOS (HORSE)

Skt. *ásvas*, Av. *aspa-*, Gk. *hippos*, Mycenaean Gk. *iqo*, L. *equus*, Old Irish *ech*, OE *eoh*, Tocharian *yuk*, Old Lith. *ešva* (mare), HLuv. *d-zú-(wa/i)*. CLuv. ? *az-zu-wa-an-za* [dat. loc. pl.], Lycian *esbe*.

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

(3) *JWGO- (YOKE)

Hit. *jukan*, Skt. *yuga-*, Gk. *zugon*, L. *iugom*, OCS *igo*, OE *geoc*, Welsh *iau*, Lith. *jungas*.

In these forms the only deviation from the basic shape of the stem is Lithuanian *jungas*, which has the *-n*-infix from the verb stem. This root is well integrated into PIH morphology. There is a verb root having an aorist stem **jeug-/jug-*, a present stem **juneg-/jung-* (join, yoke) (e.g., Skt. pres. *yunákti* < **juneg-ti*, aorist mid. *dyuk-ta* < **éjug-to*). Benveniste in his (1935) discussion of PIE root structure, claimed that original roots had only two nonvocalic elements. The third was always a suffix or *élargissement*. The *-n*-infix for him was always an original suffix. In this system, the “vowels” [i] 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 **j(e)w-*. In this case, the original root is actually found in Sanskrit **ju-* (unite) in the verb *yu-té* (unites), and, with a nominal suffix *-tis*, in the Lithuanian word *jautis* (ox, 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 *jugom* 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(É)IHs- (HARNESS POLE)

Hittite *hiss(a)-*, *hess(a)-* (harness pole), Skt. *ṛṣā* (harness pole for carriage or plow), Av. *ašša-* (plow), Sln. *oje* [gen.] *ojesa* (harness pole).

We have trouble reconstructing a concrete word here. The Hittite word, considering the attested spelling with *e*, is probably best considered from **HeiHs-* and

as such is a formal match with the Avestan word, which could also come from **HeiHs-*. Pokorny (1994: 298) lists them under **ei-*, *oi-* (stange).

(5) DHWRH- (HARNESS)

Several sources, e.g., Gamkrelidze and Ivanov (1995: 624), Mallory and Adams (1997: 508), Mayrhofer (1992-: 794), suggest linking Hittite *tūrija* ([verb] harness), Sanskrit *dhūr*, *dhūram* (yoke, load, harness shaft), Tocharian *tursko* (draft ox), and Greek *thairós* (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 antler found at Aruxlo I could have been part of a primitive plow. Aruxlo I has uncalibrated carbon 14 dates of 4770 +/- 60 and 4815 +/- 60 B.C. (Kushnareva 1997: 22).

Initially, vehicles were much too heavy to be pulled by horses, and oxen were necessary. Sherratt (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 steppe, 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 3700 B.C. (1970, 1997: 82). From a grave site in the steppe north of the sea of Azov (Kamennaya 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).

However, if Sherratt is correct about the route, the spread may have taken place earlier than he thought. In the Tripol'e-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 (Gusev 1998). One of them (without oxen) is attributed to middle Tripol'e, which would be in the second half of the fifth millennium. The great majority are found in the Nebelevsk-Tomashivka, Sushkivka complex, which Gimbutas (1997: 43) labels Cucuteni B3-Tripol'e B III₃. 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. Mateescu (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 **Hekwos* referred 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 PIH societies. A survey of these 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 steppe and across much of Europe. According to Vörös (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 *Eskişehir*. Bökönyi (1987, 1991) argues that the animals found south of the Caucasus in the fourth millennium were domesticated horses introduced from the Pontic steppe. 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 Demirçi Hüyük. Groves (1986) simply says that "evidence for the presence of wild horses in Southwestern Asia is not very conclusive either, although Boessneck/von den Driesch (1976) claim to have identified wild horse bones from fourth millennium deposits in Anatolia." The bones identified by Boessneck and von den Driesch

are the ones claimed by Bökönyi 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 caballus*." 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 (1984: 70) asserts that the preponderance of young animals with milk teeth at the Neolithic-Eneolithic site of Mullino indicates domestication of the horse for purposes of meat production, but the small total number (twenty individuals) may raise doubts. Not only Gimbutas and her followers, but Sherratt (1981, 1983b), Piggott (1993: 43), and Bökönyi (1978) accept evidence from late fifth millennium steppe settlements like Dereivka to indicate horse riding and domestication. Anthony (1991a) provides a summary of the evidence and an account of how it may have happened. The evidence includes the remains of a large number of horses at a small settlement (fifty horses for three houses), ritual burials of horses, and antler pieces of a size and shape that could have been used for bridle bits. 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 stallion 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 Dereivka but sees no conclusive evidence of husbandry. Whittle (1996: 134) is even more critical, accepting Dereivka as evidence for only limited horse riding.

The doubts about domestication at Dereivka were raised by Levine's (1990) study. She examined the still-available horse bones from Dereivka and found that the ages of most of the horses represented clustered between five and eight years old. This, she argued, is too old because animals raised for food would likely have been slaughtered soon after achieving near-adult growth, roughly between one and two years. Moreover, she 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 Dereivka led her to the conclusion that the horses at that site were hunted.

Levine's conclusions may not be justified. Petrenko (1984: 69–110) found similar distributions at sites that almost certainly had domestic horses. He studied sites 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 past thirteen years, while slightly more than 70 percent died between three and nine.²

Horses appear in the upper Balkans and the Danube Valley in the late fifth millennium. Gimbutas (1979, 1997: 240–68) associates this with the spread of domestic horses into Europe with the expansion of the steppe (kurgan) culture, and therefore with the spread of Indo-Hittites. 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. Vörös (1981), however, suggests that wild horses may have been drifting west from the steppe at that time.

Fourth millennium evidence is similarly controversial. Gimbutas (1997: 273) cites a line drawing of a man on horseback, which she says is attributed to the second half of the fourth millennium. Häusler (1994) points out that this drawing cannot be securely dated. Possible bronze cheek-pieces for bridles are found in the Maikop culture of the northern Caucasus. Evidence that these may indeed be cheek pieces came from microscopic examination. Remains of skin were found on them (Munchaev 1975: 390). The dating of the Maikop culture has been controversial, but it seems to have begun early in the fourth millennium. Recent carbon dates taken from bones at Galiugai, a settlement of the Maikop culture (Kushnareva and Markovin 1994: fn. 172) indicate for Galiugai I 4,480 \pm 70 BP, for Galiugai II 4,600 \pm 80 BP, and for Galiugai III 4,930 \pm 120 BP, giving a calibrated range of 3730 \pm 131 to 3165 \pm 90 B.C. A general upper limit for the Maikop culture is provided by radio carbon dates from the burial ground at Ust'-Dzhegutinskii. These burials are supposed to relate to a period immediately following the period of the Maikop culture. The calibrated dates range from 2755 \pm 319 B.C. to 2465 \pm 335 B.C. (Kavtaradze 1983: 107–8). However, in the area north of the Kuban river, the Novotitorovka culture is thought to follow the Maikop culture, and that culture has a calibrated carbon date of c. 3100 B.C. A general range for the Maikop culture would seem to be 3750–2750 B.C., with the possibility that 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 seems 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 Dereivka, 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 (Bökönyi 1994). Toward the end of the millennium, there is written evidence for the arrival of domesticated horses in Mesopotamia (Clutton-Brock 1992: 89).

If we consider the horse to be a domesticated animal in PIH society, then in the first half of the fifth millennium the evidence would seem to limit the PIH area to the steppe east of the Dnieper. In the second half of the fifth millennium it spreads to the upper Balkans and the Danube valley. In the fourth millennium it spreads 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. Woolly sheep are first indicated at Sarab in the Kermanshah Valley of Iran. A clay figurine from that area of a sheep shows clear indication of staples (characteristic clumps of wool fleece) (Bökönyi 1977: 101). There are four carbon 14 dates from that site, all calibrating to the seventh millennium B.C. The uncalibrated dates, found in Bruenig (1987: 54) are: 6006 +/- 125 B.C., 5655 +/- 96 B.C., 5694 +/- 89 B.C., c. 5900 B.C.. Moreover, Bökönyi (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 Sarab, 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 ovicaprids 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 first in Mesopotamia. Wool eventually became an important item of trade in Mesopotamia. Workers wages were paid in wool, and exports of woollen cloth paid for the imports of raw materials (Ryder 1983: 97).

Although there is no evidence of woollen 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: 735) nevertheless suggests that the first wool textiles were felt. He points out that even in wild sheep the wool undercoat molts, 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 what might be a textile made of animal hair. In a society that had developed impressive patterns on woven flax, 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. Munchaev (1975: 161) reports a piece of cloth found at Kvaxelebi in the Kuro-Araxes culture of the southern Caucasus. He describes it as a simple weave made of wool and a plant fiber (linen). Unfortunately he does not cite a source and does not name the archaeological level. Kvaxelebi was

occupied for many hundreds of years. The levels belonging to the Kuro-Araxes culture at Kvaxelebi 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. Munchaev (1975: 161) notes the existence of pottery with impressions of cloth at Kvaxelebi and at Amiranis-Gora, from the same culture. Amiranis-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 Kvaxelebi, kept the herds of sheep near the settlement. Others, as at Amiranis-Gora, apparently practiced transhumance, moving the sheep to high pastures during the summer (Kushnareva and Chubinishvili 1970: 109; Kushnareva 1993: 223–24). Kushnareva and Chubinishvili 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 Chubinishvili identify the sheep at Kvaxelebi as of the “imeretinskii” type. These were presumably what Ryder (1983: 268) calls “imeritian,” 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 Kvaxelebi apparently were.

Remains of churns and dishes appropriate for cheese production are found at Kvaxelebi. 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 cloth are found in burial chambers (Kushnareva 1997: 213). Barber cites the discovery of a twill cloth (presumably wool) in a burial at Martkopi, which has a similar early third millennium date.

In the northern Caucasus, Barber (1991: 168–69) reports a kurgan burial at Novosvobodnaya 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 (puxovyi) yellow and black plaid. Since this cloth is contrasted to linen, we might assume that it was wool. Novosvobodnaya is either a late version of the Maikop culture or a new culture that replaced it. I mentioned above in the discussion of horses that a general range for the Maikop culture (including Novosvobodnaya) 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.

Bökönyi (1987) argues that wool sheep were introduced into Europe via the Pontic steppe 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 steppe into eastern and northern Europe is coordinated with Gimburas's second and third kurgan expansions. This would mean that wool sheep had reached the western edge of the steppe by c. 3400–3200 B.C. This agrees 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 B.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. Bökönyi (1974: 168) identifies markings on clay statuettes of sheep found in Hungary from this time as representing fleece. Ecsedy (1979: 15, 16, 19, 20, 37–39, 44) mentions the remains of blankets, spreads, and carpets 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. Ecsedy cites no carbon dates from any of the kurgans, but he does say that the early kurgan burials are contemporary with the Usatovo culture, which Gimbutas (1997: 55) 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 Gurbanești) was analyzed as camel hair (Ecsedy 1979: 39). The new sheep that moved from the steppe into Hungary had an average height at the withers of 70 cm. compared to 60 cm. for Eneolithic sheep (Bökönyi 1971). The number of sheep increased dramatically, and a larger number of these sheep were allowed to live into adulthood, indicating use for secondary products. Tsalkin (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 Spodsbjerg 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 years (Midgley 1992: 382, 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 3200/3100–2900 B.C. This is certainly consistent with Bökönyi's timetable for the introduction of wool sheep into 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, Bökönyi (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 steppe.

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 steppe. We have noted evidence for wool technology in the southern and northern Caucasus in the fourth millennium. Certainly sheep domestication existed in the Caucasus 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 swine (Munchaev 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 steppe. If wool sheep were introduced into the steppe through the Caucasus, they probably came from the northeastern area, where settlements of a culture similar to the Kuro-Araxes culture reached the steppe. In fact, sheep-based livestock economy seems to have developed in the northeastern Caucasus earlier than in the southern Caucasus. Gadzhiev (1991: 82) describes the Eneolithic settlements at Ginchi and Chinna as permanent agricultural settlements with primarily sheep as livestock, using high alpine pastures and transhumance. Temporary sites in the high altitudes confirm 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 B.C. The Eneolithic sheep in the Caucasus were similar in size to the steppe variety. His primary evidence comes from Petrenko (1984). However, Petrenko (1984: 49) preferred the area of Turkmenia near the southeast 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 earliest sheep bones found in the steppe that are intact enough to allow measurement are found at the Khvalynsk cemetery in the forest steppe zone along the lower Volga. This is the culture Gimburas claims as the parent culture of the Proto-Indo-Hittites (1997: 56). With the Sredny Stok culture in the west, the Khvalynsk culture formed the basis of the Yamnaya or pit-grave culture of the steppe. We have six carbon 14 dates from skeletons in the cemetery. Three, recently published in Anthony (1998), date it to the late sixth to early fifth millennium:

Grave 30: 6200 +/- 85 BP, 5251–5010 B.C.

Grave 18: 5985 +/- 85 BP, 4946–4783 B.C.

Grave 18: 6015 +/- 85 BP, 4994–4799 B.C.

Two of the three dates from Agapov et al. (1990: 83) roughly agree:

Grave 4: 5903 +/- 72 BP, 4863 +/- 137 B.C.

Grave 26: 5808 +/- 79 BP, 4768 +/- 136 B.C.

Grave 13, however (in the Agapov 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 anomalous 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.

Petrenko found measurable bones in the sacrificial areas—(zhertvenniki) nos. 1, 10, and 11—and in burials 101 and 115. In zhertvennik 1 he found the remains of one sheep that would have been approximately 73 cm tall at the withers. In zhertvennik 10 he found the remains of at least fourteen sheep whose range of height would have been 61.2–72.6 cm, averaging 65.77 cm. In zhertvennik 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 zhertvennik 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, Petrenko 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 later 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 steppe 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 fifth 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 PIH word for *wool*.

One could always wonder if the word for wool predates the use of wool as a product. It might conceivably have meant *hair of a sheep*. The stem for *wool*,

**HwlHn-*, 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 **HwlHn-* to a basic root for weave *H(e)w-* found in the Sanskrit, infinitive *ótum* < **au-tum* < **eu-tum* < **Hewtu-m*; *otu-* (woof), *ūta-luta* (pp. woven). The present is *váyati* < **Hwejeti*, meaning in Sanskrit *weave, interweave, braid, plait*. The simple stem **H(e)w-* is found only in Sanskrit. Baltic and Slavic have a verb with a present tense apparently cognate with Sanskrit *váyati*: Lith. *výti, veja*, Rus. *vit', v'ju*. Both mean *twist* and are used to refer to the making of nests. In other languages a variety of forms may begin with **Hew-*, or *Hwe-* and mean *weave*. One should remember that the laryngeal rarely shows up at all, and never before *w* plus a vowel, so there is multiple ambiguity. A more frequent word for *weave* is **w(e)bh-*, presumably **Hwebh-* with a *-bh-* “stem-extension”: Sanskrit *ubhnāti* (ties), Greek *huphainō* (I weave), Old English *wefan*, Tocharian B *wāp-*, Osetian *wafyn*, Persian *baf-* (weave), Avestan *ub-dažna-* (from cloth). A form with *-dh* is found in Old Norse *vād* (spinning), Gothic *gawidan* (tie), Old English *wād* (clothing), Armenian *z-audem* (tie). Lithuanian *audžia* (weaves) could have come from **Howd-* or *Howdh-*, as could the base of Russian *usló* < **oudtlo*, (starting cloth on a loom). English *wattle* may be related to 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 *weave* 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) as *w(e)l(x)-*, and then look for evidence of laryngeals where they might show up. Pokorny (1994: 1,139) suggests that the word for wool might be related to roots **wel-* which mean either *pluck* or *roll*. Meaning *pluck*, Latin *vellere* is actually used to refer to the plucking of sheep, which is the ancient method of obtaining wool. The double *ll* could come from a variety of clusters (**ld, ln, ls*). Pokorny, without citing any evidence, traces it to an original *-ls-* cluster.

Several words meaning *hair* or even *wool* are formed from the root **Hw(e)l-*. Hittite has another word for *wool*, *huliyā-*, which has a *-j-* suffix. Lithuanian *vālas* (hair from a horsetail, fishing line made from horsehair) and Sanskrit *vālas*, (hair from an animal's, esp. a horse's, tail) both may have come from *(*H*)*wol-o-s*. The length in Sanskrit is explained by Brugmann's law, which determines the lengthening of original **o* in open syllables. Slavic **wols-o-s* (OCS *vlasŭ*, R. *vólos*) (hair), is cognate with Avestan *varšas* (hair). Neither of these words should have had a laryngeal following the **l*. A laryngeal would block the lengthening of the **o* in Sanskrit *vālas* and should have conditioned acute intonation in Slavic **wolsos*, giving R. **volós* rather than actual *vólos*. Greek *lákñē* (woolly hair, down, sheep's wool) and *lákhnos* (wool) likewise lack evidence for a laryngeal. Pokorny (1,139) traces the Greek words to **włksn-*. Latin *vellus*, [gen.] *velleris* < **wellos-* (fleece) is probably a derivative from *vellere*.

Under the general rubric of Pokorny's **wel-* (roll), there are Germanic, Baltic, and Slavic verbs meaning *felt* or *full* that indicate processes applying only to animal fiber, primarily to wool. Lithuanian *vél-ti* and Latvian *vêlt* mean *full, felt, beat, roll, rumple, throw down, card* (wool). Russian *valjar'*, which is a transparent iterative from the same root as Lithuanian *vél-ti*, means *roll, full, felt, knead* (dough), *drag*, and from the same root *valit'* can mean *throw down* or *flock together*. German *walken* (full, felt, pummel) is cognate with English *walk*. In Old English *wealcan* meant *toss around, roll, move around*, but *wealcere* meant *fuller*. Barber (1991: 216) notes that in Scotland a wa(u)lking song is sung while trampling newly woven tartan cloth, soaking wet, for felting.

This array of meanings seems bewildering, but it makes more sense when we read Barber's (1991: 216) description of the process of felting: "To make fiber felt, the folk of Asia and India lay out their cleaned wool on a mat in layers corresponding in area and shape to what is desired for the finished piece. Warm water or whey is sprinkled on, and the mat is rolled and rerolled, kneaded, beaten, stamped on, dragged about, and otherwise subjected to as much friction and pressure as possible over many hours, until the fibers are thoroughly interlocked." Ryder (1983: 265) even notes that the wool may be combed before felting. Fulling is the same felting process applied to woven wool, except that it does not need rolling.

Greek has what were or had become two verbs with a present tense *eileō* < **welneō*. The verb meaning *press, squeeze, force together* has an aorist *elsa* < **welsm*, perfect *eola* < **wewola*. These clearly show that the stem was **wel-* and therefore the present has an *-n-* suffix. A second verb *eileō* (*turn, roll*) is a regular verb with aorist based on the present stem. Chantraine (1968) and Frisk (1960–70) relate the first verb to Lithuanian *vél-ti* in the meaning *felt* and the second to *vél-ti* in the meaning *turn*.

Armenian has a noun *getmn* (wool, fleece), which Fraenkel (1962–64: 1254) glosses as *felt (filz)* as well. Mann (1963: 8) relates this form to Lithuanian *vél-ti*. In this form the presence of a laryngeal after the *l* is nondistinctive. Mann gives **welmn* as the Proto-Armenian form, while Beekes (1988) treats it as possibly from PIE **HwelHmen-*.

Of all the examples, the Lithuanian verb *vél-ti* is the clearest formal match. The Lithuanian word for wool is *vilna*. Despite the graphic difference in the accent marks, the two words agree in intonation—both have acute intonation. For primary words, this normally means that there was a laryngeal following the *l*. In pre-Balto-Slavic terms, then, the verb stem had the shape **welH-*, while the word for wool was **wlH-n-eH_a*. (Any initial laryngeal would have disappeared earlier.) The only difference in the basic stem is in the ablaut grade of the root, which often changes in derivation. Given the similarities in form and the meaning of the verb, it is surprising that none of the standard etymological dictionaries even mention a direct connection between *vél-ti* and *vilna*. Pokorny (1994: 1,139), as I noted above, simply mentions the possibility that the word for *wool* is related to the roots for *roll* or *pluck*.

Under the item *vél-ti*, Fraenkel mentions words for wool as "ferner" relatives, but does not mention the possibility under *vilna*.

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 that 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: 250), divides western Eurasia and the Near East into four areas. Her southeast 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 band 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, weights 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 steppe.

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 accidental 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 steppe (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 fiber felt 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

as a large question mark. We have seen evidence of early woven wool from the southern Caucasus and reference to later burials containing felt coverings.

To return to our etymology of **HwlHn-*, we can say that there was a family of words built on an initial **Hw(e)l-* meaning *hair* or *wool*. Various suffixes, such as *-s-*, *-g-*, *-j-*, *-H-*, either form specific nouns or form verbs indicating processes applied to animal hair or wool. The stem **HwlHn-* is a tertiary derivative, indicating a particular type of animal hair, wool, which is appropriate for processing or has been processed into a textile. It is this word for wool that has the best PIH etymology, and if this explanation of its origin is approximately correct, then it is firmly tied to the secondary products revolution.

Terminus Post Quem

We are now in a position to put at least a boundary on the time of PIH unity. It has to have been a time when ox traction, horses, and woolly sheep coexisted. If we limit ourselves to hard evidence, then we would place the earliest possible time in the fourth millennium B.C. The Russian steppe had horses and putative woolly sheep in the fifth millennium but no direct evidence of ox traction until the fourth. If Shnirelman (1992) is correct in his claim that the tall sheep were introduced to the steppe from the Caucasus, then the southern Caucasus also possibly had woolly sheep and horses in the fifth millennium. Again, no direct evidence of ox traction appears before the fourth millennium. Mesopotamia did not have horses until the third millennium, and there is no evidence of horses or woolly sheep in north-western Anatolia until the fourth millennium. Northern and central Europe have no evidence of wool or ox traction before the fourth millennium.

To be safe, we could propose a *terminus post quem* for the division of PIH in the late fifth millennium B.C. At this point we can say nothing definite about the place where the first division took place. It is at least interesting, however, that the very earliest places with all three cultural items are found in the vicinity of the Black Sea, the best candidates being the Pontic-Caspian steppe and the area south of the Caucasus.

One thing we can say is that this date eliminates the proposals by Renfrew and Drews (1997). At the time of the migration of farmers from northwest Anatolia to Greece, there was no ox traction, no wool technology, and no trace of horses on either side of the Bosphorus. Even if the recent reports of horses found in south-central Anatolia prove correct, that should not matter. The Balkans have been well studied, and no horses show up until the fifth millennium. The seventh millennium farmers who migrated to Europe would have lost any word for *horse* from their language.

Proto-Indo-European and the Wheel

There is a complex of reconstructed words that are PIE, but not demonstrably PIH, and that deal with wheels and wheeled vehicles:

**k^wek^wlos* (wheel): OE *hvéol* (wheel, circle), OHG *wēl*, ONorse *hvél* (wheel), Gk. *kuklos* (wheel, circle), Skt. *cakras* (wheel, circle), Av. *čaxras* (wheel) Tocharian A: *kukäl* (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 **k^wel-*. Its attested verbal forms mean *move* (Skt. *cārati*, Gk. *pélomai*) or *turn* (Av. *čaraiti*, Albanian *siëll*, Cuneiform Luvian *ku-wa-li-i-ti*) (Melchert, 1993, 114). One might wonder whether the original meaning of the noun were *wheel* or *circle*, but the meaning *wheel* is firmly PIE.

One possible problem for the unity in the meaning is East Baltic (Lith. *kaklas*, Latv. *kakls* [neck]). This technically does not match **k^wek^wlos*, since the first vowel is the reflex of **o*, not **e*. However, there are enough examples of unexpected *a* from etymological **e* in east Baltic to make one careful. Latin *poples*, *poplitis* (knee joint, back of knee), if borrowed from an Italic language with *p* for **k^w*, could come from **k^wok^wl-et-* (Mallory and Adams 1997: 640). This enhances the possibility of a term for rotating joint with o-grade. If these words are cognate, they fit a general meaning of *something which turns*, they but do not mean *wheel*.

Other examples of forms from the same root meaning *wheel* or *things that turn* are: Sl. *kolo*(**s*), OPr. *kelan* (wheel), Latv. *ducele* (two-wheeled cart), Gr. *polos* (axis < **k^wolos*), L. *colus* (distaff).

**rot(e)H_a*: L. *rota*, Germ. *rad*, Lith. *ratas*, Irish *rath* (wheel); Skt. *rathas*, Av. *raθa-* (cart, chariot).

These words are derived from a verbal stem attested in Old Irish *rethim* (I run). The Balto-Slavic verb represented by Lithuanian *rita* (rolls), Rus. *rtut'* (an old participle < **rit-ontī* [mercury]) is almost surely cognate. The short *i* in Baltic and Slavic is a regular morphophonemic realization of zero-grade ablaut in Balto-Slavic.³ There is an apparent phonological problem with the *-ih-* in Sanskrit, *θ* in Avestan. This indicates a cluster with a laryngeal. Since this cluster has no effect in the other languages, we can simply assume that the verbal stem was **r(e)tH-*.

In this case we may doubt whether the nouns represent an original PIE stem. Deverbal derivatives 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

meaning of the Balto-Slavic verb, since the Irish is of no help. The adjective **rotH-os*, *-om -ā* could then mean *rolling*. This adjective could apply to either the wheel or to the vehicle to which the wheel is attached.

**H₄aks-* (axle): ON *öxull*, L. *axis*, Gr. *áksōn*, OCS *osi*, Lith. *ašis*, Skt. *akṣas*, Av. *aša-* (axle), Irish *aiss* (cart)

**nobh-* (naval, hub): In Skt. *nābhi-*, OP *nabis*, and Germanic (OHG *naba*, OE *nafu*, ON *naf*) 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.

weghe-* (transport by vehicle): OCS *vezq*, Lith. *vežu*, Skt. *vāhati*, Av. *vaza-iti*, Latin *vehō* (transport), OE *wegan* (carry). The o-grade derivatives from this verb typically mean wheeled vehicles: OCS *vozū* (wagon), ON *wagn*, Gk. *wákhos* (cart), Mycenaean Greek *wo-ka* (chariot). This is only one of the meanings of this verb. Although the o-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 Slavonic, 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 *vahitram*, which may be cognate (weghHtlom*) 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 *axle* or *hub* in Anatolian. The only specific disagreement is in the word for *wheel*. The Hittite word is *hurki*, which can be a derivative from a verbal stem **Hw(e)rg-* found in Sanskrit *várjati* (twist, turn). Tocharian A *wärkänt* (wheel, circle) may be formed from the same verbal stem but is not at all the same word. The word for *wagon* in Hittite is *tiyarit-*, and *huluganni-* 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. Piggott (1991: 18) dates the appearance of signs for wheeled vehicles in Mesopotamia to the late 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.

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. (Piggott 1991: 18), Sherratt 1996). A clay model of a wagon with four wheels found in the Baden culture in Hungary dates from between 3400 and 2900 B.C. (Gimbutas 1997: 255), and a burial with a two-wheeled cart found in the steppe tentatively dates from the fourth millennium (Gimbutas 1997: 83, 187). In the TRB culture, representations of apparent wheeled vehicles on pots from Bronocice and Ostrowiec Świętokrzyski are likewise dated to the last half of the fourth millennium (Midgeley 1992: 378–79).

In the area north of the Caucasus, a wagon burial is found in the Novosvobodnaya (late Maikop) culture (Kondrashov and Rezepkin 1988, Chernykh 1992: 69). Numerous wagon burials are found on the steppe north of the Caucasus in kurgans of the Novotitorovka type (Chernykh 1992: 79, Gei 1991). I noted above, in the discussion of the horse, that the approximate dates of the Maikop culture were 3750–2750 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 wagons found in burials from the Novo-Titorovka 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, but it almost surely did occur before the end of the fourth.

In the southern Caucasus, burials with wagons have been found in kurgans at Bedeni, Trialeti, and Zurtaketi (Masson 1973). As of Kushnareva and Chubinshvili (1970: 111) 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 Kavtaradze (1983: 108–9) in the second quarter of the third millennium B.C. Kavtaradze used calibrated 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 Sharak (Khachatrian 1975: 74, 77). The Trialeti kurgans with wagons are later, dated by Kavtaradze (1983: 117–8) in the second half of the third millennium. The Zurtaketi burials are considered to be part of the Trialeti 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. Kushnareva (1997:

170) asserts that model wheels “have been found at many agricultural settlements in southwestern Asia dating to the sixth to fourth millennium B.C.” Dinu (1981) identifies model wheels from the Cucuteni A and Karanovo VI cultures west of the steppe (Gimbutas 1985, 1997: 308–9; Dinu 1981). These cultures date to the mid or late fifth millennium. Munchaev (1975: 200) claims to identify model wheels in the southern Caucasus site of Tekhut, dated in the fifth millennium. These claims depend on the ability to distinguish model wheels from spindle whorls. No model wheels are found attached to model carts in the southern Caucasus until the third millennium, at roughly the same time that actual wheels are found. Dinu’s model wheels are at least large, up to 11 cm. in diameter. Jeanine Davis-Kimball, however, in a personal communication, suggested that even those wheels are not too big to be spindle whorls used for bast fibers. Gusev (1998) also discusses “model wheels” found in the Cucuteni-Tripol’e culture but offers a different argument. He argues that some discs are too light to serve as spindle whorls. The discs he studied are 1–6 cm. in diameter, 0.3–0.7 cm. thick. He suggests that they were used as wheels for vessels in the shape of oxen, found with legs and holes in their feet that could hold wooden axles for the wheels. Earlier zoomorphic vessels without these holes exist. These are found in the general area where the ceramic models of sledges are found (western Tripol’e) and at either about the same time or slightly earlier (c. 4000 B.C.). Nevertheless, Gusev himself notes that zoomorphic figures with wheels on their feet do not guarantee the existence of wheeled vehicles; wheeled animal figures are found among the Aztecs, who never used wheeled vehicles (Piggott 1983: 15).

We can now look at the effect of using the appearance of wheeled vehicles as a *terminus ante quem* for the division of PIH into PIE and PA. Candidate PIH cultures must therefore show evidence of having had wool, horses, and ox traction before the introduction of the wheel. In the Khvalynsk-Yamnaya complex in 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 cushion separates the earliest evidence for yokes, wool, and horses from the earliest attested wheels. Curiously, no early evidence of wheels has been found in the area of the western end of the Black Sea, around the Bosphorus. This could be accidental; some of the earliest evidence for wheels is found in the Danube valley. However, a mountain range separates this the area of the Bosphorus from the Baden culture, which has the early evidence for wheels. The Sherratt and Sherratt (1988) 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 wool 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 population 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 Caucasus, the specifics of the Gamkrelidze-Ivanov proposal do not fit this hypothesis. Not only do they treat the word for wheel as PIH; they have the dispersal of the PIE branch of the family from the southern Caucasus. They treat the Yamnaya culture as Indo-European, but 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 Caucasus homeland, but I do not have time to discuss that in this forum.⁵ 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 (1988) 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. Proto-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 koiné around the shores of the Black Sea (and perhaps also the Aegean . . .) in the 3rd millennium, bringing together elements from **PPIE 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

(Yamnaya) groups from the Pontic steppe to dry areas of Romania and Hungary, and the movement of the Yamnaya people into the essentially uninhabited areas of the Asian steppe. They consider trade to be a possibly more fundamental factor. This involves a change in the Bronze Age from exchange networks to directional trade. They also cite the emergence of new metallurgy and the switch to pastoral economies, with the possibility that one language group might have controlled essential resources like wool and metals.

Certainly, problems emerge in the Sherratts' view of the spread of language. Their mention of pidgins and creoles is unfortunate and unnecessary. Zvelebil (1995) also suggests that PIE was spread by creolization and pidginization. This betrays a lack of understanding of pidgins and creoles. Pidgins typically arise when a native population tries to communicate with an elite, generally intrusive, group that has no interest in learning the language of the local population. Pidgins have vocabulary, mostly that of the elite group, but essentially no inflectional grammar. Creoles may arise from pidgins, when the pidgin is adopted as the language of a group of lower status. As the pidgin becomes a true language, its complexity increases. Nevertheless, this kind of creole gathers most of its vocabulary from the elite language and manifests severely restricted inflectional grammar. Not everyone believes that creoles must go through a stage of pidginization, however. Thomason and Kaufman (1988: 48) suggest that it is enough that the language of the dominant group is so distorted by the non-native-speaking population that it is no longer recognizable as genetically related to the source language. Anyone who knows anything about an older Indo-Hittite language knows that it does not lack inflection. Nor is it true that we cannot demonstrate genetic affiliation of Indo-Hittite languages. The standard of reconstruction to which creoles are compared is the reconstruction of Indo-European. With the Thomason and Kaufman definition, if we said that Indo-Hittite languages spread as creoles, we would be saying that the reconstruction of Indo-Hittite languages does not meet the standard of reconstruction set by Indo-Hittite languages.

There are also some possible chronological problems with the Sherratts' proposal. If I interpret the evidence correctly, other articles by Andrew Sherratt fail to support the arguments of Sherratt and Sherratt (1988).

First of all, they locate the formation of the koiné in a trading network around the western end of the Black Sea in the third millennium. This is the time period of the development Mallory (1989: 239) calls the circum-Pontic interaction sphere, which he says was established around 3000 B.C. Mallory, Gimbutas, Merpert, and other scholars associate this interaction primarily with intrusion from the Russian steppe, not from influence from Anatolia. For my purposes here, it is most important that it is a third millennium phenomenon.

This chronology would mean that PIH speakers played no role in the formation of the Corded Ware culture, since that culture has its roots in the fourth millennium in northern Europe, well away from the western end of the Black Sea.

There would likewise be no possibility that PIH speakers formed part of the Yamnaya or Pit-Grave culture of the steppe, 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 populating of the nearly unpopulated Asian steppe from the Russian steppe by people of the Yamnaya culture. The other is the movement of Yamnaya people up the Danube valley. This movement is discussed in detail in Sherratt (1983a: 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 salinization 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 tumuli differ sharply from the local Baden culture. Moreover, the tumuli extend 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 koiné; it is a movement that began well before they propose that the koiné was formed.

Chernykh (1980) traces to 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 steppe 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 in Chernykh (1992). The map in Chernykh (1992: 147) shows that arsenical bronze was firmly established in the southern Caucasus, the Pontic steppe, and the Danube valley in the early Bronze Age at a time when it is only rarely found in northwestern Anatolia. The spread of arsenical bronze into this area is mainly in the middle Bronze Age, which for Chernykh begins about 2700 B.C. (cf. the map on 49). The maps on page 154 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 steppe, the Balkans, and eastern Europe in the fourth millennium, that the steppe 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 kurgan. . . . 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 Corded 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 Sherratts' 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 Sherratts' version of the spread of PIH could not have been aided by control of access to wool and arsenical 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 Sherratts' 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 Sherratts.

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 those 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.

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 tumulus 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. Chernykh (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 arsenical 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., Häusler 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 tumulus 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 temporally very close to the first. Since the

common vocabulary of PIE includes words relating to wheeled vehicles, the *terminus post quem* for the division of PIE is the appearance of the wheel. There is no good evidence of wheeled vehicles until the second half of the fourth millennium. Even if the wheel had developed earlier, the appearance of wool sheep provides another piece of evidence for the *terminus post quem*. Wool sheep do not appear in northern Europe until the second half of the fourth millennium. The Corded Ware culture dates from about 3200 B.C. In order to have Indo-European speakers on hand for the formation of the Corded Ware culture, we must look for evidence of influences from the steppe in the period between 3500 and 3200 B.C. We should look for evidence in this period for cultural contact between the steppe population and that of central and northern Europe and try to construct a scenario whereby the language of the steppe could become the dominant language. This must apparently happen without a massive, destructive invasion and large-scale population replacement.

The period 3500–3200 B.C. was the period of Gimbutas's second wave of kurgan expansion, which is the first expansion that might be tied to the cultural vocabulary reconstructed for PIE. Good evidence exists for movement of steppe pastoralists into the upper Balkans, up the Danube valley, and into northern Europe in the second half of the fourth millennium. We have seen Sherratt's evidence for people with the material culture and burial habits of the steppe who appear in areas good for pastoralism in the Hungarian plain at that time. These pastoralists apparently coexisted with the local farmers of the Baden culture. Gimbutas notes similar but less well documented phenomena along the Black Sea coast and in the north European plain, also in the second half of the fourth millennium. In the Usatova-Gorodsk-Foltesçti complex along the coast of the Black sea, farmers of the Tripol'e culture occupied wide river terraces, while kurgan people were located on surrounding hills and dunes and on spurs in the river (Gimbutas 1979, 1980, 1997: 251, 274–76).

In northern Europe, beginning about 3400 B.C., we find stock raisers of the Globular Amphora culture, which Gimbutas also attributes to her second wave of kurgan expansion. Again, these people brought the material culture and burial habits of the steppe and coexisted with indigenous farmers. While the farmers were concentrated in areas in river valleys with fertile soils, the herders inhabited areas difficult for farming that were near sources of goods important for trade, such as flint and amber (Gimbutas 1979, 1980, 1997: 254–56, 283–91).

The relationship between the Globular Amphora culture and the Yamnaya culture is considered controversial by some (Mallory 1989: 250–51; Mallory and Adams 1997: 226–27). Gimbutas, as noted above, is certain that the Globular Amphora culture is made up principally of intrusive steppe pastoralists. The similarities that she cites include a pastoral economy with limited transhumance, burial mounds with mortuary houses built of stone, cromlechs, ritual burial of horses, dogs, and cattle, and human sacrifice in burial of prominent males, including suttee.

Their distinctive item of pottery—the globular amphora 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 farther 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 (Necrasov 1980). These similarities were apparently also evident among the early kurgan burials in Hungary (Ecsedy 1979: 91–93). The similarity decreases with distance from the steppe (Schidetzky 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 Gimbutas's waves of kurgan expansion. His chart (1980: 382) of computations of morphological distance from the Drevneyamnaya (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-Donc/Sredny 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-Donc population as palaeo-europid. Compared to the Yamnaya population, the farming populations to the west and northwest are quite distinct: Tripol'e ten units, Gumelnița thirteen units, TRB nine units.

After the second kurgan expansion, the Dnieper-Donc 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. Along the Black Sea the Usatovo and the Gorodsk-Horodiștea-Foltesçti populations have a morphological distance of only about three units. Farther west, the Cernavoda culture has a distance of six units from the Yamnaya, but, interestingly, its deviation is more toward Dnieper-Donc 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

show a gradation in characteristics. The oldest area of the Corded Ware culture was essentially the same as the Globular Amphora culture in Poland and Germany. In Poland two measurements for Corded Ware populations are three units (Złota) and between five and six, while in Germany there is no difference from the original farming population (nine units).

We thus have people appearing in previously uninhabited areas with the material culture, burial habits, and physical characteristics of the people of the steppe. It is difficult not to believe that this represents population movement from the steppe into north-central Europe. Since we know the language spread, and these changes take place at the perfect time to account for that spread, we should assume that the language of the steppe appeared in north-central Europe at this time.

No good reason arises to classify the appearance of these herders as an invasion. We could call it demic diffusion, but demic diffusion of pastoralists, not farmers. A farmer's son on the frontier of the advancement of farming might have to move only a short distance along a river to find new arable land. A pastoralist would likely have to move dozens of miles to find an unused herding territory. This means that pastoral societies, diffusing into unoccupied land, move very quickly.

We thus have very good arguments that pastoralists of the Yamnaya culture moved into central and northern Europe, beginning in the second half of the fourth millennium. Certainly, they brought their language with them. There is no reason to assume that they exterminated the population of those areas and replaced them, but we need to understand why the language of the pastoralists came to dominate. We would also like to explain why the society they formed was so successful and spread so far and so fast.

We know a good bit more about the vocabulary of PIE than that of PIH, and we can get a better picture of their culture. There is reasonable evidence that PIE living units were patrilineal, patrilocal, and exogamous (Delbrück 1889). This evidence is based on ancient texts and on the vocabulary of kinship and marriage. PIE has a set of reconstructed kinship terms denoting the members of a patrilineal group but few if any solid reconstructed terms for a mother's relatives. The word for 'livestock', **peku*, may have been used for *property* and *wealth* (Mallory-Adams 1997: 23). This became the word for *money* (or the derivational base of that word) in Germanic and Latin. Thus Old English *feoh* (livestock, property, money), Latin *pekunia* (money). This makes sense in an economy based on pastoralism. After the secondary products revolution—particularly after the introduction of wool sheep—livestock did represent wealth. Domesticated animals could be used to provide labor, food, and clothing without necessarily being slaughtered. A single man tending a herd could produce much more essential produce than could a fourth-millennium farmer. That meant that the more livestock a person or a society owned, the greater its wealth. Even in Indo-Iranian, which does not show a specific meaning of **peku* as *property*, the word shows up in expressions of the wealth of a society: thus Avestan *pasu vīra* (livestock and people) is used in a formula for wealth, while Sanskrit *virapśā-* means *abundant of people and livestock*.

Some linguistic evidence suggests that the Indo-Europeans did not always respect other people's property. In Avestan, Greek, and Latin, the verbal stem **agé-* (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 *táin bo* (cattle raid < **to-ag-nai gwou-*, 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 **agé-*, although in this case it can be interpreted literally:

jó gā udājad apadhā Valāsyā . . . , sá, janāsa, Indras
(he who drove out the cows by the unclosing of Vala . . . , he, o men is
Indra) Macdonell, 1992: 46

In this passage the verb *udājad* is from **ud-é-áge-t* (out-past-drive-3rd 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 reaping 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 society, considerable intermarriage likely occurred between the pastoralists and the farming population. I noted above 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 moves 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 steppe formed good candidates for what Johanna 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 prior to 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 geared to take full advantage of the secondary products revolution.

In terms of language, we would expect that intermarriage and the fact that farmers had to deal with herders 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 dialects 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 steppe; 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 wheels 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 wheels, 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 Anatolia 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 Bosphorus, 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 northwestern Anatolia. Iarovi (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 to the third 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 Usatovo culture along the Black Sea east of the Danube, which is described as a hybrid culture containing elements from the steppe and from the Tripol'e culture (Mallory and Adams 1997: 614). Bökönyi (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 (Potakova 1979: 145). There is no direct evidence of ox traction, but since Gusev (1998) notes the existence of model sledges on runners in the Tripol'e culture as early as 4000 B.C., we can hope that ox traction existed. If Usatova was indeed a hybrid culture made up of steppe pastoralists and Tripol'e 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 choices for survival—as the farmers of central and northern Europe to adopt the pastoralists' language. The same could be said of other hybrid cultures farther west along the Black Sea, such as Cernovoda and Ezero. One of these may have produced Proto-Anatolian.

Close cultural ties across the Bosphorus were established in the fourth millennium. I mentioned earlier that Yakar attributes the metallurgy in northwestern Anatolia to influence from the Balkans, and horse bones are found at Demirçi Hüyük in the fourth millennium. Demirçi Hüyük is on the most logical route leading from the Bosphorus 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 Bosphorus is probably not the locus of the original separation between PIE and PA, although it may have been the locus 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'e farmers and the horse-oriented steppe culture was the 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 wheel, probably in the first half of the fourth millennium.

Notes

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1. An alternative explanation for the lack of a prothetic vowel in Greek is offered by Martin Peters, *Untersuchungen zur Vertretung der indogermanischen Laryngale im Griechischen*, Wien: Verlag der österreichischen Akademie der Wissenschaften, 1980, 23n. 18. Following a suggestion by Rix, he proposes that word-initial laryngeal disappeared before *w* plus a following syllabic liquid. This works not only for the word for *wool*, but for a variety of other forms.

2. To get these percents 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. *bradas* (ford), *bristi*, [past] *brido* (wade), *skradnus* (fast flying), *skristi*, [past] *skrido* (fly) (Skardžius 1943:473–74).

4. Craig Melchert, in comments on this paper, pointed out the existence of a verb meaning *drive* in Hieroglyphic Luvian of the shape PES2 *wali-za-*, which could be from **wegh-j-elo-*. While not a direct cognate of the word **wegh-elo-*, it is certainly a possible instance of the root.

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

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CRITICAL RESPONSE TO THE LAST FOUR PAPERS

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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 Carian: 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 compatible with that of an Indo-European Anatolian language, Professor Finkelberg then (and only then) turns