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Master's Thesis

Modern Indo-European Homeland Hypotheses: A critical examination of linguistic arguments

Completed under the supervision of Prof. Dr. E. F. Konrad Koerner

September 1994

Department of Linguistics

Faculty of Arts

University of Ottawa

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PREFACE

The objective of this study will be to provide a succinct and critical overview of the linguistic arguments behind the most important homeland hypotheses to have been developed over the last three or four decades. In other words, I will present what I consider to be 'modern' theories which have been generated by post-war scholarly research, and assess the linguistic foundations on which these theories are grounded. Earlier work on the Indo-European homeland problem will only be referred to briefly in the introductory section, and when its relevance to modern theories makes it necessary. This particular time-frame restriction was chosen firstly for reasons of length limitations, and secondly for reasons of interest, as very few of the arguments which were being used until the 1940s are still considered valid today.

The domain of Indo-European studies has typically been one of inter-disciplinary research to which linguists, archaeologists, anthropologists and other experts have contributed a great deal. In the context of the present study, however, I will focus specifically on the linguistic discussion, drawing attention to arguments from other fields only in those instances where the linguistic arguments stand to gain from it.

I would like to sincerely thank Prof. Dr. E. F. Konrad Koerner for his invaluable guidance, his constant support and his selfless generosity for which I am truly endebted and without which this study might never have been undertaken. I would also like to thank Prof. Dr. Max Krell, Erika Krell, and Michael Bussmann for their unconditional support and encouragement. Without it, the project might never have been brought to completion.

0. *Introduction*

The problem of Indo-European origins has intrigued scholars and students of language generally for over two centuries. Proto-Indo-European (henceforth PIE) is the posited ancestor of all Indo-European languages; since the 19th century, linguists have gradually begun to reconstruct the grammar and vocabulary of PIE through comparative methodologies. From the beginnings of Indo-European research, scholars from various disciplines, inspired by the contents of the reconstructed PIE lexicon, have attempted to situate a PIE culture in time and space. As the fields of historical linguistics and archaeology evolved, so did educated opinions on the location of the prehistoric homeland of PIE speakers.

Linguistic historiographers like to credit Sir William Jones with having been the founder of Indo-European historical-comparative studies. Jones had been serving as a British judge in colonial India when he presented his famous "Third Anniversary Discourse" on the Hindus in 1786.[1] In it, he commented on the surprising lexical and grammatical similarities which he had noticed between Latin, ancient Greek and Sanskrit, suggesting that the similarities between these seemingly distant languages might not be of a fortuitous nature at all, but rather the result of an ancestral link, "a

[1] Sir Jones' *Third Anniversary Discourse* was published two years later, in 1788, as the first volume of the *Asiatick Researches* series. It has since been reprinted in Lehmann (1967:7-20).

common source, which, perhaps, no longer exists". Moreover, in the same line of thought, he hypothesized that Gothic, Celtic, old Persian "might be added to the same family".

In reality, Jones was not the first to notice these similarities. As Droixhe (1978:86ff) and Koerner (1981:151) have pointed out, various types of links between European and Asian languages had been proposed since the Renaissance, and the topic continued to be discussed in the18th century by scholars such as G. W. Leibniz (1710) and P. S. Pallas (1786/87-1789).

Nevertheless, it was Jones' remarks that finally fell on fertile ground when Friedrich Schlegel, in his *Über die Sprache und Weisheit der Inder* of 1808, made a series of forceful proposals concerning the reconstruction of the original Indo-European proto-language which subsequent generations, from Franz Bopp onwards, developed into what is called the "comparative grammar" of the Indo-European languages (cf. Koerner 1989:269-290 for details). The problem of the geographic location of the elusive ancestral speakers of the Indo-European *Ursprache* (from German, meaning the "original language") has captured the imagination of philologists, historical linguists, and archaeologists both in Europe and North America ever since.

Over the decades and even centuries, as research into the matter evolved, many different theories based on a wide and sometimes wild range of arguments, and most were eventually rejected. Earlier this century, in particular, the problem of the Indo-European homeland was notoriously misused by some scholars to promote certain socio-political ideologies (cf. Section 1), and thus has had to free itself of the stigma it received, both from within the historical linguistics community and from concerned other fields such as archaeology and anthropology.

In this paper, I will look at three competing PIE homeland hypotheses which are the product of modern, post-World-War-II scholarship. These hypotheses each utilize different lines of

argumentation, sometimes emphasizing linguistic data, sometimes archaeological data. Here, I will be examining primarily the linguistic argumentation in each hypothesis with the purpose of assessing the strength of this argumentation both within the context of a given hypothesis, and within the scope of modern linguistics in general. Archaeological evidence will be addressed only insofar as it provides additional support to a set of linguistic arguments which are thought by their author(s) to already stand on their own.

In Section 1, for example, I will demonstrate how linguistic research *alone* has, in my view, been able to make a legitimate case for a North-Central European hypothesis with little help (nor any serious challenge) from the archaeological community. It remains to be seen whether material evidence can be found to either refute or support linguistic findings. In the case of the Eurasian Steppe (or 'Kurgan') hypothesis (Section 2), which seems to be founded largely on archaeological arguments and uses selective linguistic data merely for corroboration, the situation is practically reversed. I do not feel that I am in a position to challenge Gimbutas' elaborate *archaeological* claims, nor is it my intention to do so. I will, however, evaluate the strength of the linguistic corroboration which is implied throughout her work. Section 3 presents another major hypothesis which has been erected independently by a couple of Russian linguists and treats archaeological corroboration as secondary. However, some archaeological research has recently surfaced which may actually support an Anatolian Hypothesis of one type or another and will therefore be briefly discussed.

1. *The North-Central European Hypothesis*

The North-Central European Hypothesis may be called the 'veteran' of modern PIE homeland theories, as it has existed in various versions since the 19th century when Indo-European comparative linguistics was establishing itself as a separate discipline. Perhaps not surprisingly, its greatest support has traditionally come from Western European scholarship. The argumentation for a North-Central European *Urheimat* (i.e., "original homeland"), however, has undergone a significant evolution over the course of the present century. Arguments which were being used fifty years ago have fallen into obsolescence and have been replaced by new ones founded on sounder theoretical priciples and better empirical data. Nevertheless, the hypothesis has steadily lost its popularity to more recent competitors such as Gimbutas' 'Eurasian Steppe Hypothesis' (cf. Section 2) or Gamkrelidze & Ivanov's 'Anatolian Hypothesis' (cf. Section 3).

The most notorious argumentation in support of a European genesis of PIE was ideologically motivated and represents a trend which first flourished in 19th-century scholarship. I refer of course to the Aryan myth which was originally promoted by authors such as Hirt (1892) and Kossina (1902), only to be picked up a generation later, during the Nazi era, by Kühn (1932), Schulz (1935),

Neckel (1935), and many others.[2] Each of them expounded his own variation of a North-European or Central-European[3] *Urheimat* theory involving a so-called 'Aryan race' which was thought to be superior to other peoples. Their work was typified by elitist ethnological claims, subjective anthropological assumptions, archaeological ignorance, and a very selective use of linguistic data, has long since been put to rest, its theoretical foundations having been thoroughly discredited by modern scholarship. To this day, there is an undeniable stigma associated with North-Central Europe as a possible PIE homeland, a stigma which, as Kitson remarks in a forthcoming article, seems to manifest itself as a "prejudice in the English-speaking world against it" (p.3).

Despite a past riddled with unscientific approaches and ideological biases, the North Central European Hypothesis has not completely disappeared from current discussions on PIE origins. In this first main section, I will focus on the some of the modern (i.e., post-World-War-II) scholarship supporting a North-Central European *Urheimat*. I will argue that the linguistic evidence which has been put forward by various proponents in recent decades, and most importantly by Schmid (e.g., 1968, 1976, 1982, 1987), cannot be disregarded. Other Indo-Europeanists need to account for this evidence before dismissing the possibility of a European glottogenesis for PIE, as many have done. Finally, I will briefly comment on the archaeological support which has been provided for a North-Central European homeland.

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[2] A thorough list of these early authors is provided by J. P. Mallory in "A Short History of the Indo-European Problem" and included in the 1976 reprint of Victor Hehn's *Cultivated Plants and Domesticated Animals* (xxi-lxxv). It should be mentioned that Indo-European scholarship in the '30s did not unanimously promote a model of Central-European genesis. Polish archaeologist Tadeusz Sulimirski (1933), for example, argued in favour of an Asian origin. He was supported in his theory by the French scholar George Poisson (1934). Dutch scholar C. C. Uhlenbeck suggested that the *Urheimat* was to be found in the Aral-Caspian steppes (1937), while Austrian scholar Wilhelm Koppers (1934) pointed to an area just west of Turkestan.

[3] The *Urheimat* in question was usually identified with either all or part of the Baltic region, or with the area roughly corresponding to modern-day Germany, though some authors' versions extended across both.

1.1 *Modern linguistic argumentation for the hypothesis*

Although European homeland theories lost favour in the eyes of many scholars both inside and outside German academic circles after 1945, those who did argue for a European *Urheimat* did so with considerably more rigour and caution than their predecessors had. They dutifully emphasized the crucial distinctions between the concepts of 'race' and 'culture' on the one hand, and 'language' on the other (Meyer 1948, Thieme 1954), in some cases bringing forth new lines of argumentation based almost entirely on *linguistic* data (e.g., Krahe 1951) and little corroborating evidence from other fields.

Perhaps the most convincing arguments in favour of North-Central European origins have been generated since the late 1960s, though many scholars have chosen either to ignore them or brush them off without proper discussion. In this subsection, I will survey some of these modern linguistic arguments to try to assess their significance within the wider scope of Indo-European studies. First, I will argue that Schmid's 'Old European' hydronomy theory merits serious consideration, and must be accounted for by scholars who attribute PIE to origins to a homeland outside Europe. Then, I will survey the contributions of some other contemporary scholars who have supported and even added to Schmid's arguments for a North-Central European Urheimat.

1.1.1 *Schmid's theoretical concerns.* The most important proponent of a North-Central European *Urheimat* today is perhaps Wolfgang P. Schmid, a German linguist who has been researching the question since the 1960s. Schmid belongs to the school which treats the methodological use of a reconstructed PIE lexicon with more than just initial caution. He points out (1987:322-323) that the term *Indo-European* has been used in the literature in three distinct ways:

> 1) It refers to the association of a particular language with a group of languages, the features of which differ in phonological, morphological, syntactical and lexical domains from those of other groups, such as those of Semitic and Finno-Ugric; 2) it refers to those distinguishing features; and 3) serves to designate the postulation of a protolanguage and its speakers, e.g. the Indo-Europeans, the Proto-Germanic peoples, etc.

The author then states that the first two interpretations are quite acceptable, as they involve nothing more than descriptive assessments, and that 2) is actually the pivotal concept since it constitutes the prerequisite for the formulation of 1). The third interpretation, however, he considers to be highly speculative on the grounds that it simply cannot be deduced from the first two.

Schmid stresses that in reality the notion 'Indo-European' involves no more (but no less) than a set of correspondences (phonological, morphological, or otherwise) which have been discovered between attested languages as far apart as four millenia (e.g., Hittite and Modern English). Thus, he argues, the "comparison and the results thereof [i.e., Indo-European correspondence rules] are completely independent of the time and location of the languages being compared" (1987:323). Furthermore, he points out that since these correpondences only apply to particular fragments of the sampled languages (phonemes, morphemes, etc.) and never whole systems, the notion of 'Indo-European' should not be extrapolated to a cover a wider scope than the data on which it is founded.

Schmid does not deny that the remarkable corrrespondences which have been found between languages spanning all the way from Ireland to Sri Lanka warrant some kind of an explanation. Since they are observable at all levels of grammar, he admits that they are most likely indicators of a common linguistic source. However, the author apparently objects to the idea that this source can be recovered and represented through specific grammatical constructs, be they semantic, syntactic, morphological or simply phonemic. He seems to favour a more abstract approach whereby each correspondence is labelled with an arbitrary symbol representing the source-element from which the

attested, inter-linguistic similarities were derived. The exact nature of the source-element must remain speculative since, "if one compares the prehistoric extrapolation with the historically observable intrapolation, it becomes immediately clear what an enormous oversimplification such a simulation represents", and thus, the reconstructions which have been proposed for PIE at all levels of grammar may be seen as "merely an abstract inventory of selected correspondences between a given number of compared languages" (Schmid 1987:325).

Thus, Schmid's perspective on Indo-European studies is quite different from that of most other scholars, in that the reconstruction of an Indo-European proto-language is viewed with considerable scepticism, and therefore, that the use of PIE to deduce the geographic location of the linguistic source-elements is *not* considered to be a valid methodological approach. As we will see, this theoretical concern expressed by Schmid sharply contrasts with the *a priori* acceptance of PIE as a valid tool for prehistorical investigation by proponents of the hypotheses represented in Sections 2 and 3.

1.1.2 *The evidence of 'Old European' hydronomy.* Schmid finds an alternative approach to the exploration of Indo-European prehistory through the study of onomastics, i.e., the study of proper names, most often place names, and in this case, river names in particular. He refers to the work of his teacher Hans Krahe (1954, 1957, 1962) who uncovered a veritable network of archaic Indo-European hydronyms.[4] Since this network seems to be concentrated within North-Central Europe

[4] Hydronyms are the proper names of bodies of water. In Krahe and Schmid's work, they most often involve the names of rivers, though they may also refer to lakes or seas.

and ends rather abruptly east of the Baltic regions, the suggestion is made that the distribution of these so-called 'Old European' river names points to the center of Indo-European dispersal.

The theoretical premise behind both Krahe's and Schmid's findings is that the proper names of places or bodies of water are least likely to be affected by semantic and phonological shifts in the course of regular linguistic change. The phonological form of a hydronym, for example, may remain essentially unchanged long after the language which generated it has evolved to later stage. Original hydronyms and toponyms are also more likely to be preserved even when the local speech community is completely replaced by an incoming group, as has often been the case in Western Europe.[5]

Schmid's studies of European hydronomy seem to indicate that there is a unique region, namely the Baltic region, where river names show particularly archaic Indo-European characteristics and apparently lack the non-Indo-European sub-strata which are so common all over the rest of Europe. The author uses six specific characteristics to identify these 'Old European' river names (1972:4), as he calls them: 1) The origin of the rivers' names cannot be directly traced to the language of the population living alongside them. 2) The phonological structure of their roots follow a C_1eHC_2-pattern,[6] and their semantic content relates to the concepts of 'water', 'flowing' or adjectives of colour which can be associated with water. 3) The root lexemes may be supplemented by various

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[5] Indeed, this explains the preservation of strong non-Indo-European substrata in place and river names all over Europe. Another excellent example is the complete or partial phonological preservation of numerous native North American lexemes in toponyms such as *Toronto*, *Québec*, *Saskatchewan*, *Ontario*, *Ottawa*, *Shawinigan*, *Chicoutimi*, *Chibougamau*, *Maniwaki*, *Niagara*, *Missisaugua*, *Rimouski*, and *Winnipeg*, as well as hydronyms such as *Nipissing*, *Athabaska*, *Temiscouata*, *Muskoka*, *Magnelawan*, and *Mississippi*. Admittedly, in the North American context, the original meaning of such lexemes has sometimes little to do with their present referent.

[6] In this structure, C_1 stands for a given root-initial consonant, H stands for a laryngeal phoneme, and C_2 stands for a root-final consonant different from C_1.

suffixes, the antiquity of which can be determined both through linguistic means and through their geographic distribution. 4) Suffix inflexions, which occur according to the grammatical distinctions in a given language (e.g., distictions of gender), may have been assimilated to subsequent, locally spoken dialects. 5) Regular phonological correspondences can be found between these hydronyms, whether they be located in the east, west, north or south of Europe. Schmid refers to this as a *Namenlandschaft* (i.e., a 'landscape of names'). 6) The roots and suffixes of these hydronyms are directly traceable to the body of reconstructed PIE lexemes, and their composition is in agreement with the rules of PIE morphology.

So for example, the (southern) German river *Etsch*, which used to be called *At(h)esis* in Antiquity, finds the corresponding hydronyms *Atesẽ* and *Atesỹs* in Lithuania, and *Ata* (without a suffix) in Latvia. Similarly, there is a correspondence between German *Drau*, Greek *Δράβος*, Serbo-Craotian *Dráva*, Welsh *Drewen*, and Lithuanian *Drãvinė*. Furthermore, Norwegian *Eisa* finds a parallel in Bielo-Russian *Jesá*, Dutch *Ijssel*, Galician *Isla*, and Lithuanian *Jieslà*. In the 9th century A.D., the German river *Lek* was known by the older form of *Laca* (which later developed into *Lecca*); the same form can be found in Latvian *Laka*, in Lithuanian *Lãkesas*, and *Lakajaĩ*, and even in Finnish *Lakajà*. A Latin reflex can be found in *lacus*, "lake". In Antiquity, the river *Moráva* was referred to as *Margus* or *Márγος*. These forms correspond not only to Celtic, German, and Polish hydronyms (which are not expressly named in Schmid 1972), but also to Latvian *Mārgava* and *Mērdze*, as well as Lithuanian *Margà* and *Meřgė*. The German river *Nahe* is referred to as *Nava* by Tacitus and can be related to German *Nuhne*, Lithuanian *Nóva* and *Novenà*. The French river *Aisne* can be traced back to the form **Apsōna*, which finds near perfect correspondence in the Lithuanian *Apsuonà*.

Moreover, all the above examples are used by Schmid (1972:7-10) show that the common denominator in the distribution of Old European river names is consistently the Baltic region. The author also points out, such Old European river names are conspicuously absent from the area east of the Baltics, or more specifically, from the area which is delimited by the upper San river to the west, by the Pripjet to the north, and by the Goryn to the east (see Appendix, Map I). This is precisely the area which is often identified as the ancient homeland of the Slavs.

Schmid hypothesizes that the PIE dialectal groups were originally located in the Baltic region and that their initial dispersal began from that region (once again, see Appendix, Map I). According to him, the eastern PIE groups (i.e., the Slavic, the Anatolian, the Greek, and the Indo-Iranian groups) were the first to leave the Baltic *Urheimat* and move their separate ways. Subsequently, the western groups began to spread out all over Europe, to the south and to the west of the Baltic region into their historic locations. This scenario would account for the particular distribution of 'Old European' hydronyms within Europe, as well as their abscence from adjacent regions to the east.

Schmid's hypothesis has gone largely uncommented by his colleagues, at least in comparison to their active discussions which are brewing around the more popular hypothesis of Marija Gimbutas (cf. Section 2) and even such a controversial hypothesis as that of Gamkrelidze & Ivanov (cf. Section 3). However, it is my opinion that the evidence of ancient European hydronomy cannot be ignored if the solution to the PIE homeland question is to be sought in a scientific and unbiased fashion.

1.1.3 *Other proponents of a North-Central European homeland.* Schmid has not worked in isolation and does not represent the only voice in favour of a North-Central European hypothesis. His

traditional line of onomastic arguments has found a solid proponent in British scholar P. R. Kitson, who also takes a position in favour of a PIE cradle located in Europe, though he assigns the PIE homeland to a slightly more western location, i.e., roughly to the area covered by modern Poland, Germany and Czekoslovakia. His assessment of the archaeological research up to date is that it has failed to make a convincing case for any one hypothesis over another, and that therefore, in his words, "the linguist has to take the bull by the horns and say that to square with *linguistic* evidence the western origin must be the right one" (Kitson p.19; my italics: KSK).

Kitson points specifically to evidence from river names on the British Isles. He describes how, contrary to the high proportion of pre-Celtic[7] or even Celtic hydronyms one would expect to find, English and Scottish river names are remarkably consistent in their 'Old European' character (Ekwall 1928, Nicolaisen 1976). This is quite baffling if one accepts both Schmid's (i.e., Krahe's) premise about the phonological stability of hydronyms, and the mainstream view among Celtologists that the Celts did not arrive on the Isles until the first millennium B.C. Kitson, however, takes the position of a "dissenting minority of Celticists" (p.23) who have argued that the Celts arrived in Britain as early as the third millennium B.C., and replaces the term 'Celts' with 'Old Europeans' (of a sort). In other words, the Celts who arrived in the first millennium found a pre-established speech community using the Old European river names which it had brought there two thousand years earlier. These river names were then taken over and preserved by the Celtic newcomers.

From this scenario, so goes Kitso'sn argument, it follows that the center of PIE dispersal could not have been too far east because it seems very unlikely that Old European river names could have

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[7] The term 'pre-Celtic' refers to those non-Indo-European languages which were spoken by the Picts and other tribes who inhabited the British Isles before the arrival of the Celts. These languages have long since disappeared, leaving no trace other than the sub-strates which can be distinguished in the historic Celtic languages.

spread all the way to the British Isles otherwise, given the approximate timeframe for PIE "unity" (i.e., around the 4th millennium B.C.). The solution seems to be a more western homeland from which some of a group of speakers (though not necerssarily Indo-European in origin) was able to export the Old European hydronyms north-west to Britain by the 3rd millennium B.C.

Kitson also underlines evidence of proto-Iranian loan words in Finno-Ugrian (Harmatta 1978) which suggests that there was probably linguistic contact of some sort between these two distinct speech communities. Because the loan words in question include some early examples of palatalization of certain plosive consonants, a process typical for the so-called 'satem' languages, the period of contact can be dated to a time when the PIE dialects were still in relative proximity of one another, yet were already roughly divided into eastern and western groups.[8] Since the Finno-Ugric homeland is generally thought to have been located somewhere in northern Russia, and Harmatta's data suggests that the easternmost dialectal groups of PIE must have been located in an adjacent area to the west, Kitson concludes that a PIE homeland in North-Central Europe "makes perfect sense" (p.13).

1.2 *Archaeological support for the hypothesis*

1961 saw the publication of one of the most ground-breaking and influencial monographs on the topic of Indo-European origins, *Les Indo-Européens: Problèmes archéologiques* by Pedro Bosch-

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[8] The *eastern* dialectal groups of PIE roughly correspond to the 'satem' languages, in which, under the traditional model, the set of PIE velar plosives (*g, gh, k*) has undergone palatalization (*g', gh', k'*), followed by spirantization (*ź, ž, š*), and in some cases affrication (*ǰ, ǰ, č*). The western dialectal groups of PIE are referred to as 'centum' languages; their first-series plosives did not undergo the very early process of satemization described above, though individual centum laguages may have gone through a similar process later in their development. English, for instance, shows affricates instead of original Germanic velar plosives. Thus we obtain cognates such as English *cheese* versus German *Käse*.

Gimpera, a Spanish-born archaeologist working at the University of Mexico. Bosch-Gimpera described the speakers of PIE as a Mesolithic people based in Northern, Central and Pontic Europe. Only two years later, Archaeologist Marija Gimbutas (main representative of the Eurasian Steppe Hypothesis described in section 2) criticized Bosch-Gimpera's monograph because, in her view, it did "not include the most recently accumulated archaeological data from eastern Europe and Anatolia. Hence [the book is] entirely outdated from the archaeological point of view" (Gimbutas 1963:85).

Even though the archaeological arguments on which most of Bosch-Gimpera's theory is based may have been largely rendered obsolete by the new evidence from modern radiocarbon dating techniques, *Les Indo-Européens* is still one of the most comprehensive volumes ever written on the PIE problem. No new case has been made to associate material data on European prehistory with a centre of dispersal for Indo-European languages. Perhaps, with the popularization of Gimbutas' theories, few scholars of European archaeology have thought it worth the effort to swim against the current of mainstream assumptions, only to argue an alternative scenario which may be possible but never provable.

Nevertheless, Kitson points out (pp.23-24) that even in the absence of specific archaeological corroboration, the available data on the spread of "Beaker Folk" throughout Europe in the late third millennium happens to fit almost exactly the distribution and probable chronology of Old European hydronyms. He suggests that this may not be a coincidence and that the two phenomena were related. Thus, with further research into this relationship, archaeological support may yet be provided for the North-Central European Hypothesis.

1.4 *Conclusions*

Modern arguments in favour of the European genesis of PIE are very different from the ones of the early twentieth century. The use of onomastics can, in my opinion, serve as a legitimate tool to the historical linguist because it provides a valuable glimpse of the achaic distribution of a language. In fact, given their apparent phonological stability on the one hand, and their semantic stability on the other, hydronyms may well represent the most useful type lexical fossils for the reconstruction of prehistoric speech communities.

The distribution of Old European river names is certainly intriguing if nothing else. To be considered relevant in general discussions on the PIE homeland question, however, Schmid's findings will first have to gain recognition among historical linguists and be followed up with further comparative research. Such research has yet to be seriously conducted in the context of non-Indo-European language families, and it would very interesting to see if ancient *Namenlandschaften* could be uncovered for other linguistic families as well. If this were possible, it would probably lend more weight to onomastic studies and, used in combination with other linguistic approaches, the answer to the PIE question might become more attainable than ever before.

In my view, the lack of archaeological proponents for a North-Central European homeland may be due to the fact that past involvements with it have left a bad taste in many scholars' mouths. Archaeologists may prefer to stay away from such inter-disciplinary discussions altogether, even if their findings may contribute to one or the other position. It is almost up to the linguist to seek out and make sense of relevant archaeological literature, a rather challenging task for someone not familiar with its theoretical foundations. Perhaps as onomastics become more widely accepted, and as inter-disciplinary studies improve, new archaeological also support will emerge.

2. *The Eurasian Steppe Hypothesis*

The Eurasian Steppe Hypothesis, perhaps better known as the 'Kurgan' hypothesis, was first proposed in 1956, and subsequently developed over a period of three decades, by the Lithuanian-born scholar Marija Gimbutas;[9] not surprisingly, it draws its principal arguments from archaeological research. However, this hypothesis has been quite influential in the fields of historical-comparative linguistics and comparative mythology, as well as archaeology.

It will be argued here that, although the hypothesis claims to have answered the question about the origins of PIE, an inherently *linguistic* construct, there are some serious problems with this hypothesis from a linguist's point of view. The general criticism is that in constructing her Kurgan hypothesis, Gimbutas places far too much emphasis on archaeological data and on her particular interpretation thereof, while failing to relate that data to the current linguistic research on PIE. In particular, it will be shown here that the vocabulary items commonly reconstructed for PIE hardly form the striking correlation with Kurgan archaeological data which Gimbutas claims it does.

Another inherent weakness of the Kurgan hypothesis is that it relies quite heavily on certain questionable presuppositions. For example, Gimbutas assumes that language spreads and behaves

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[9] Professor of European Archaeology at the U.C.L.A. from 1963 until her retirement in 1990, Marija Alseikaité Gimbutas died from cancer at the age of 73 on 2 February 1994 in Los Angeles.

in a way parallel to the material aspects of a given archaeological culture; in this particular context, she simply supposes that all the early Kurgan-type cultures spoke the same language (or at least closely related dialects). A further assumption crucial to her hypothesis is that language, in this case late PIE, can be (and was indeed) imposed on entire local populations by a small conquering warrior élite. Finally, she seems to assume that the PIE vocabulary reconstructed to date can in fact be used at face value to adequately represent a PIE culture and environment at a specific point in prehistory.

As will be shown, Gimbutas' avoidance of the actual PIE data, coupled with her unfounded presuppositions concerning language change in general, poses some serious questions as to the credibility of her overall argumentation.

2.1 *General overview of the hypothesis*

Gimbutas first identified the Indo-European people with what she coined the 'Kurgan' archaeological culture in a 1956 monograph, *The Prehistory of Eastern Europe, Part I.* The term 'Kurgan' is actually the Russian word for "barrow", and was chosen by Gimbutas as a cover term for particular groups of prehistoric cultures which, according to her, attest to a common cultural tradition. Gimbutas chose the term 'Kurgan' because the principal trademark identifying this tradition is said to be the construction of earthen barrows as part of the burial ritual (Gimbutas 1963:821).

According to Gimbutas' research, the chalcolithic[10] Kurgan culture first developed into its characteristic form sometime during the third millennium B.C. in the Eurasian steppes; more precisely, in the steppe-lands which extend from the lower Volga basin north of the Caspian Sea,

[10] I other words, of the copper-stone or post-neolithic stage.

around the Sea of Aral, through much of (northern) present-day Kazakhstan, all the way to the Altai mountains, and as far east as the Sayan mountains and the upper Yenisey river. Thus it is quite a vast territory which Gimbutas has posited as the PIE homeland (Appendix, Map II).

This geographically enormous homeland is justified, under Gimbutas' theory, by the fact that the early Kurgan culture appear to have been a semi-nomadic, pastoral people who depended on large pasture-lands to sustain their sheep- and cattle-based economy. Furthermore, she claims that the Eurasian steppes constitute "the area where horse-breeding must have first occurred" (Gimbutas 1963:820), presumably by the Kurgan people, for whom the domesticated horse would certainly have had many advantages.

According to Gimbutas, by the time the Kurgan people started to expand westward and southward from their Eurasian steppes, they were already divided into several dialectal subgroups, a view which most linguists would probably agree with, given the vastness of the posited Kurgan homeland. However, she does assume that, originally, all Kurgan-type settlements were occupied by speakers of one common language.

Gimbutas' chronology of the Kurgan culture was somewhat tentative in the theory's early years, and even with the advent of carbon-dating technology in the 1960s, she kept reworking the original chronology, as C-14 testing was slowly being perfected. Ultimately, however, she divides the Kurgan tradition into three stages (Gimbutas 1970:177):

> 1. EARLY KURGAN OR KURGAN I north of the Black Sea precedes the infiltration into (Danubian) Europe and Near East;
> 2. MIDDLE KURGAN is the period of continuous waves of expansion, subdivisible into early and late phases, the *Kurgan II* and *Kurgan III* periods.
> 3. LATE KURGAN OR KURGAN IV is the last period of a relatively uniform culture before the crystallization of Bronze Age cultures in Europe.

Gimbutas dates the Early Kurgan period back to the fifth millennium B.C., the Middle Kurgan period to the fourth millennium B.C., and the Late Kurgan period to the third millennium B.C. (Gimbutas 1970:177-181).

Furthermore, she identifies *three separate waves* of westward Kurgan migrations instead of a single one, as she had previously thought (Gimbutas 1965), giving them the following preliminary chronology:

> a. Expansion to the Balkano-Danubian area in the first half of the fourth millennia B.C. [...] and subsequently to northern Europe.
> b. Expansion to Transcaucasia, Iran, and parts of Anatolia during the second half of the fourth millennium B.C.
> c. Raids and/or expansion to the Aegean and Adriatic area and to Syro-Palestine and possibly Egypt around 2500-2200 B.C. (Gimbutas 1970:191)

In her 1977 article, she pushes back this chronology, settling on the following dates for the Kurgan waves of incursion into Europe:

> Wave No. 1 at c.4400-4300 B.C.;
> Wave No. 2, c.3400-3200 B.C.; and
> Wave No. 3, c.3000-2800 B.C. (Gimbutas 1977:277)

This is the chronology which is upheld in her subsequent articles (Gimbutas 1978, 1980, 1985). For the purposes at hand, the primary focus will be on the Early Kurgan culture and on the significance of its attributed date and location, not only as a concrete prehistoric milieu for PIE speakers, but also as the source of the earliest dispersal (Gimbutas' Wave No. 1) of PIE as a language.

2.2 *Gimbutas' archaeological data versus the linguistic data*

One of the most relevant types of archaeological information in trying to solve the question of the PIE original homeland is that data which relates to the physical environment, be it natural or man-made. Gimbutas claims that the archaeological findings which she has gathered with respect

to the Kurgan culture correlate with the linguistic picture which has been sketched of PIE. Her confidence on this point is expressed in no uncertain terms:

> Constantly accumulating archaeological discoveries have effectively eliminated the earlier theories of Indo-European homelands in central or northern Europe and in the Balkans. The Kurgan culture seems the only remaining candidate for being Proto-Indo-European: there was no other culture in the Neolithic and Chalcolithic periods which would correspond with the hypothetical mother culture of the Indo-Europeans as reconstructed with the help of common words [...]. (Gimbutas 1970:156)

In order to evaluate how well the PIE lexicon actually reflects Gimbutas' Kurgan data, we will examine the items she can account for archaeologically alongside those items which can be reconstructed most safely by historical-comparative linguistics. This linguistic data is taken from four authoritative sources available to scholars today,[11] and each PIE form reconstructed for a given concept is accompanied by some of its attested reflexes found in ancient Indo-European languages.[12]

In examining the gist of Gimbutas' archaeological findings, the relevant data seems to revolve principally around the following themes: the Kurgan fauna and flora, a Kurgan pastoral economy (with the domestication of the horse as its revolutionary pivot), and the use of wheeled transport. Gimbutas claims that the Kurgan cultural tradition which she has been able to piece together archaeologically is perfectly reflected in the vocabulary commonly reconstructed by linguists for PIE. Indeed, this is her primary argument for associating the two in the first place.

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[11] Buck's 1949 *Dictionary of Selected Synonyms in the Principal Indo-European Languages*, Pokorny's 1959 two-volume *Indogermanisches Etymologisches Wörterbuch*, Mann's 1984 *An Indo-European Comparative Dictionary*, and Watkins' 1992 IE roots in the *America Heritage Dictionary*.

[12] Reflexes are not synonyms; they are lexical items which share some sort of semantic link with the proto-form, but most importantly, they can also be demonstrated as being phonological derivatives of that proto-form. Whenever a reflex is not provided in one or the other cell of a table, it is because the given language possesses no item which is phonologically and semantically traceable to the reconstructed PIE form in question. Of course, this does not mean that the concept itself has no label in that language; it may have some other label, but one which cannot be phonologically traced to that particular PIE root.

2.2.1 *Fauna*. The animal remains found by Gimbutas in Kurgan I and Kurgan II type excavations are primarily the bones of (in her opinion) domesticated animals: horses, cattle, pigs, sheep, goats, and dogs. Most of all, however, she stresses the importance of the domesticated horse, which represents no less than 65 percent of the animal bones found in any given Early Kurgan settlement, and therefore, in her view, must have been central to the Kurgan culture and economy. She also finds evidence of wild animals (e.g., Gimbutas 1970:157-160), but although wild animal bones show a greater diversity of species than those of domesticated ones, they typically only account for about a fifth of all the bone remains (p.157). Nevertheless, Gimbutas implies that all these species are accounted for by the proto-vocabulary.

However, after verifying her claim in Table I below,[13] we can see that the correlation between early Kurgan fauna and the PIE fauna is not quite as tight as Gimbutas claims. The donkey, for instance, is not accounted for by any of the PIE sources. On the other hand, there are several well-attested PIE forms, such the ones for 'duck' and 'goose', which are not represented in the Kurgan data. Admittedly, given the remoteness of the period in question and the relative aloofness of linguistic change, one could hardly have expected a perfect match in any case; nevertheless, a link between the Kurgan and PIE fauna already seems less clear cut.

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[13] The following symbols and abbreviations have are used in this and subsequent tables:

ə:	the phoneme shwa				
m—dash:	not found in the data	OBr.:	Old Brythonic	ON:	Old Norse
(?):	uncertain	OCS:	Old Church Slavonic	OPr.:	Old Prussian
Goth.:	Gothic	OE:	Old English	Toch.:	Tocharian
Ir.:	Irish	OHG:	Old High German	Ved.:	Vedic Sanskrit
MIr.:	Middle Irish	OIr.:	Old Irish	W:	Welsh

Kurgan fauna	Latin reflexes	Greek reflexes	Other European reflexes	Sanskrit (or Toch.) reflexes	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)
horse	*equus*	*ἵππος*	MIr. *ech*	*açva-*	*ekwo-	*ekuo-s	*ekuos	*ekwo-
cow	*bōs*	*βους*	OE *cu*	*go-*	*g^{w}ou-	*g^{u}ou-	*guōus	*g^{w}ou
cattle	*pecus*	*πέκω*	OHG *fihu*	*paçu*	*peku	*pek-	*peku	*peku
pig	*sūs*	*σύς*	OE *sū*	*sūkara*	*su-	*sū-s	*sūs	*sū-
young pig	*porcus*	*πόρκος*	OE *fearh*	—	*porko-	*porko-s	*porkos	*porko-
sheep/ewe	*ovis*	*οἰς*	Ir. *ōi*	*avi-*	*owi	*oui-s	*ouis	*owi-
lamb	*agnus*	*ἄμνος*	OCS *agne*	—	*ag^{w}(h)no-	*ag^{u}h-no-s	*agunos	*ag^{w}h-no-
goat	*(capra)*	*αἰξ*	OE *gāt*	*ajā-*	—	*aiĝ-	*aiĝis	—
dog	*canis*	*κὺων*	Ir. *cū*	*çvan-*	*kuon-	*kuon-	*kuō	*kwon-
auroch	—	—	—	—	—	—	—	—
deer/elk	—	*ἔλλός*	Lit. élnis	Toch. *yäl*	*el-	*elen-	*elənis	—
boar	*aper*	*κάπρος*	OHG *ebur*	*varāha-*	*epero- (?)	*epero-	—	—
wolf	*lupus*	*λυκος*	Goth. *wulfs*	*urka-*	*wlk^{w}o-	*ulk^{u}os	*ulk^{u}os	*wlk^{w}o-
fox	*vulpēs*	*ἀλώπηξ*	Ir. *sinnach*	*lopāça*	—	*ulp-, lup-	—	*wlp-ē-
beaver	—	—	—	—	—	—	—	*bhi-bhru-
squirrel	—	—	—	—	—	—	—	—
badger	—	—	—	—	—	—	—	—
hare	*cānus*	*- κασία*	OE *hasu*	*śaśah*	—	*kas	*kas-	*kas-
marten	—	—	—	—	—	—	—	—
bear	*ursus*	*ἄρκτος*	Ir. *art*	*rksa-*	*rk^{w}o-	*ḱto-s	*ərkt-	*rtko-
donkey	—	—	—	—	—	—	—	—
unsp. fish	—	—	—	—	—	—	—	*dhghū-

Not in Gimbutas	Latin reflexes	Greek reflexes	European reflexes	Sanskrit reflexes	Buck's PIE forms	Pokorny's PIE forms	Mann's PIE forms	Watkins' PIE forms
duck	*anas*	*νῆσσα*	OE *ened*	*ātī-*	*anəti-	*anət-	*anətis	—
goose	*anser*	*χήν*	ON *gās*	*hansa-*	*ĝhans-	*ĝhans-	*ghansis	*ghans-
salmon	—	*λακάω*	ON *lax*	*lasāmi*	—	*lak-	*lakəsos	*laks-
eel/snake	*anguis*	*ὄφις*	Li. *angis*	*áhih*	—	*ang^{w}(h)i-	*anguhis	*ang^{w}i-
crane	*grūs*	*γῆρανος*	OE *cranoc*	—	—	*ger-	*gerənos	*gerə-
eagle/ large bird	*vulturis*	*ὄρνις*	OHG *aro*	—	—	*er-/or-	*oros	*or-
bee	*opis*	—	OE *bēo*	—	*bhī-	*bhei	—	*bhei-

Table I: *Gimbutas' Kurgan fauna vs. PIE fauna*

Kurgan flora	Latin reflexes	Greek reflexes	Other European reflexes	Sanskrit reflexes	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)
oak	*dūrus*	*δόρυ*	W *derwen*	*dāru-*	*derwo-	*deru-	*deru-	(*perkʷu-)
birch	*farnus*	—	OE *beorc*	*bhūrja-*	*bherə̆ĝ-	*bherə̆g-	*bherĝos	*bherəg-
fir	—	—	—	—	—	—	—	—
beech	*fagus*	*θηγος*	ON *bōk*	—	*bhāgo-s	*bhāgo-s	*bhāĝos	*bhāgo-
elder	—	—	—	—	—	—	—	—
elm	*ulmus*	—	Ir. *ailm*	—	—	*elem-	*ə̆lm-	—
aspen	—	—	—	—	—	—	—	—
willow	—	—	—	—	—	—	—	—
cherry	—	—	—	—	—	—	—	—
melon	—	—	—	—	—	—	—	—
apple	*abulus*	—	MIr. *uball*	—	—	*ābel-	*ābəlos	—
spelt/grass (or wheat)	—	*πυρός*	OCS *pyro*	*pūrah*	—	*pū-ro-	*pūros	*pūro-
millet (cf. 'to grind')	*milium*	*μελίνη*	OBr. *mell*	—	—	*m(e)lēi-	*meli	—

Not in Gimbutas	Latin reflexes	Greek reflexes	European reflexes	Sanskrit reflexes	Buck's PIE forms	Pokorny's PIE forms	Mann's PIE forms	Watkins' PIE forms
ash tree	*ornus*	*-ώνίδος*	Ir. *unnius*	*asanah*	—	*ōs-en-	*ōsnos	—
barley (1)	*hordeum*	*κριθή*	OHG *gersta*	—	*ĝherzd(h)	*ĝherzd(h)	—	—
barley (2)	*farris*	*φαρ-*	ON *barr*	*bâr*	—	*bhar-	*bhars-	*bhares
legume/ broad bean	*faba*	*φακός*	OCS *bobŭ*	—	—	*bhabhā	*bhabos	*bha-bhā-
flax	*līnum*	*λίνον*	OE *līn*	—	*līno-	*lī-no-	*līnos	*līno-
maple tree	*acer*	*ἄκαρνα*	OHG *ahorn*	Ved. *akráh*	—	*aker-	*akeros	—

Table II: *Gimbutas' Kurgan flora vs. PIE flora*

2.2.2 *Flora.* Gimbutas places somewhat lesser emphasis on the correlation between flora attested within the Kurgan homeland and that of the PIE proto-vocabulary. On the one hand, Kurgan agriculture was "not highly developed" (Gimbutas 1970:161), so as to exclude any reference to a variety of grain crops. On the other hand, the local forests were home to diverse species of trees (p.160). In this respect, once again, the Kurgan and PIE data do not quite overlap, as shown in Table II above. Not only is there a discrepancy with respect to the trees which can be accounted for, it also seems that PIE discerns at least three or four different types of grain, not to mention other cultivatable plants. This brings us directly to our next point.

2.2.3 *Economy.* In Gimbutas' archaeological interpretation of early Kurgan culture (and, by extension, PIE culture), much weight is given to the Kurgan economy being primarily pastoral in nature, with very little agricultural activity in comparison to neighbouring cultures to the south-west. Gimbutas claims that this lack of agricultural activity is precisely reflected in PIE vocabulary. Yet, we have seen that the PIE terminology for a variety of crop-plants gives us no reason to discriminate against an equally (or even more) advanced agricultural society. Moreover, it can be shown, as in Table III below, that PIE lexical items referring to agricultural activities are not lacking in comparison with items of a pastoral nature. Thus, one can hardly argue, based on the linguistic data, that Gimbutas' Kurgan economy is unmistakably reflected in PIE.

2.2.4 *Technology.* Finally, Gimbutas claims that there is very solid archaeological evidence for early Kurgan use of wheeled transportation. This is not disputed; there are indeed various reconstructed PIE forms relating to wheel-technology (Table IV, below). However, there are also some very solid

Pastoral terms	Latin reflexes	Greek reflexes	Other European reflexes	Sanskrit reflexes	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)
to break in a horse	*domō*	*δαμάω*	ON temja	*damayāmi*	—	*(demə-), domə-	*domāiō	*demə-
to ride	*verēdus*	*-ριθμός*	OE *rīdan*	—	—	*reidh-	*reidh-	*reidh-
to milk	*mulgeō*	*μήλγω*	ON *mjólka*	*mṛjámi*	*melĝ-	*mēlĝ-	*melĝō	*melg-
wool	*lāna*	*οὖλνος*	ON *ull*	*ūṃā*	*wl̥nā	*ul̥-nā	*uln-	—
to protect, feed	*pāscere, pāstor*	*πατέομαι*	OE *fōda*	*pāsyāmi*	—	*pā-	*pāsĸō	*pā-
Agricultural terms	**Latin reflexes**	**Greek reflexes**	**European reflexes**	**Sk./Toch. reflexes**	**Buck's PIE forms**	**Pokorny's PIE forms**	**Mann's PIE forms**	**Watkins' PIE forms**
field for cultivation	*ager*	*ἀγρός*	Goth. *akrs*	*ajra-*	*aĝro-	*aĝ-ro-s	*aĝros	*agro-
grain	—	*ζειαί*	Lith. *java*	*yava-*	*yewo-	*ieuo-	*ieuos	*yewo-
kernel	*granum*	—	Ir. *grān*	—	*ĝr̥no-	*ĝrē-no-	*ĝr̥nóm	*grə-no-
hand mill	*gravis*	*γύρις*	ON. *kvern*	*gurú-h*	*gʷer-	*gʷr̥-nu-	*guernu-	*gʷerə-nā
to sow, seed	*serere*	—	Goth. *saian*	*senā-*	*sē-	*sē(i)-	*sēiō	* sē-
to mow, to reap	*metere*	*ἀμάω*	Ir. *meitheal*	—	*mē-	*mē-	*mēiō	*mē-
to gather, to pluck	*carpere*	*καπρός*	Li. *kerpù*	*kṛpāṇa*	*(s)ker	*(s)ker-	*kerpō	*kerp-
to grind	*molοere*	Myc. *meretrija(i)*	ON. *mola*	*mṇáni*	*mel-	*mel-	*melō	*melə-
to grind	*frendere*	—	OE *grindan*	—	—	*ghren-	*gʰrendhō	*ghrendh-
to plow	*arare*	*ἀρόν*	Goth. *arjan*	Toch. *āre-*	*ar-	*ar(ə)-	*arō	*arə-

Table III: *Gimbutas' Kurgan pastoral economy vs. PIE agricultural terminology*

Kurgan technology	Latin reflexes	Greek reflexes	Other European reflexes	Sanskrit reflexes	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)
axe	*ascia*	*ἀξίνη*	OE *æx*	—	—	*agu(e)sī	*aksios	—
sickle	—	—	—	—	—	—	—	—
copper	*aes*	—	Goth. *aiz*	*àyas*	*ayes-	*aios-	*aios-	*ayes-
wheel	*pabillius*	*κύκλος*	OE *hweohl*	*cakra-*	*k^{w}elo-s	*k^{u}ek^{u}lo-	*quequolos	*k^{w}(e)-k^{w}l-o
to go or transport in a vehicle	*vehere*	*ὀχέω*	OE *wegan*	*váhāmi*	*weǧh-	*ueǧh-	*ueǧh	*wegh-
wheel hub/ navel	*umbilicus*	*ὀμφαλός*	OE *nafu*	*nábhyam*	—	*enebh-	*nobh-	*nobh-
yoke	*iugum*	*ζυγόν*	ON *ok*	*yuga-*	*yugo-m	*iu-go-m	*iugóm	—
cart	—	—	—	—	—	—	—	—
axel	*axis*	*ἄξων*	OE *eax*	*ákṣaḥ*	*aks-	*aǧes-, aks	*aksis	—
wagon	—	—	—	—	—	—	—	—
Metallurgy	**Latin reflexes**	**Greek reflexes**	**European reflexes**	**Sanskrit reflexes**	**Buck's PIE forms**	**Pokorny's PIE forms**	**Mann's PIE forms**	**Watkins' PIE forms**
ore	*aurum*	—	OE *ōra*	*ā́raḥ*	—	—	*ōros	—
gold	—	*Τελχίς*	Li. geležís	—	—	—	*ghelǧh-	*ghel-
silver	*argentum*	*ἄργυρος*	OIr. *argat*	*rajatám*	—	—	*arĝntom	*arg-
Terms of navigation	**Latin reflexes**	**Greek relexes**	**European reflexes**	**Sanskrit reflexes**	**Buck's PIE forms**	**Pokorny's PIE forms**	**Mann's PIE forms**	**Watkins' PIE forms**
ship	*navis*	*ναῦς*	Ir. *nau*	*nāu-*	*nāu-	*nāu-	*nāu	* nāu-
oar/to row	*remus*	*ἐρετμόν*	ON *ār*	*aritra-*	*erə-	*erə-	*erət-	*erə-
mast	*malus*	—	OE *mæst*	—	*mazdos	*mazdo-s	—	—

Table IV: *Gimbutas' Kurgan technology vs. PIE technology*

reconstructions suggesting PIE knowledge of navigation, a technology quite atypical of Gimbutas' Kurgan society until after the third wave of migrations. Furthermore, Gimbutas describes the Early Kurgans as a copper-age people, unfamiliar with other metals until the first wave of migration into the Balkano-Danubian basin. How applicable this picture is to PIE is subject to debate; lexical roots have in fact been reconstructed (though tentatively) which refer to gold, silver, and the somewhat vaguer concept of ore.

2.2.5 *Conclusion.* I have thus suggested, through a comparison of Gimbutas' archaeological data and the actual vocabulary reconstructed for PIE, that an identification of the Early Kurgan people as the original PIE speakers cannot be empirically validated beyond a reasonable doubt.

2.3 *Gimbutas' linguistic presuppositions*

Having examined some internal problems with Gimbutas' hypothesis, I will now briefly discuss a few of its basic *a priori* assumptions concerning the relationship of language and culture, language spread, and linguistic reconstruction.

2.3.1 *The question of Early Kurgan linguistic homogeneity.* As mentioned above, Gimbutas assumes at the outset that because Early Kurgan-type findings (mostly tombs) share certain material characteristics, they must also reflect a culture which shared one common language (Gimbutas 1975:298-299). Although such a scenario is certainly possible, it is a speculative one at best, and to accept it without question is a leap of faith which can be challenged by many counter-examples, not only in the modern age, but throughout history.

Hungary is a case in point: surrounded by Indo-European-speaking neighbours on all sides, it has preserved a Uralic speech-community over millennia. Culturally, however, it shares many of the material traits which are characteristic of central-eastern Europe as a whole. Written records notwithstanding, would archaeologists from Mars assume, five thousand years from now, that 20th-century Hungarians spoke the same basic language as their neighbours?

Let us not forget that when talking about the Early Kurgan period, we are dealing with a group *prehistoric* peoples who inhabited a relatively vast area of land, and who left no written records of their language(s). It is conceivable that the Eurasian steppe was home to more than one linguistic group and that these groups borrowed elements from each other's material culture.

2.3.2 *Linguistic spread through a conquering élite?* Gimbutas seems to argue that what is lacking in written records can be compensated for by the evidence of tribal migrations; she claims that "there were no other great expansions and conquests affecting whole territories where earliest historic sources and a cultural continuum prove the existence of Indo-European speakers" (Gimbutas 1970:156). Here, Gimbutas is referring to the three posited waves of Kurgan migrations, which she uses to account for the dispersal of Indo-European dialects throughout Europe and the Near East.

Indications of cultural spread through time and space may indeed be relevant to the problem of tracing the spread of Indo-European dialects, whether it involved actual population migrations or not. However, Gimbutas' interpretation of the data describes the Kurgan migrations not as a mass movement of populations, but as a series of conquests of neighbouring cultures by a Kurgan warrior élite. According to her scenario, it is presumed that the vanquished populations gradually assimilated

the language of their conquerors, in the same way and for the same practical reasons that most historical empires have had a *lingua franca.*

There are several problems with such a picture. Firstly, it is difficult to imagine how a small élite, powerful as they may have been, could have caused such a dramatic shift in the linguistic make-up of an entire continent. Secondly, even the use of a *lingua franca* for economic transactions does not necessarily entail such a language gradually replacing the native tongue of local populations; at best, it becomes a second language spoken only by those whose livelihood depends on it. Thirdly, the mode of diffusion for a *lingua franca* in prehistory may have been quite different from that of our modern experience. A foreign language is much more likely to be assimilated by a population if it is taught as part of a standard education, used in various social institutions,[14] and most importantly, popularized through one or more mass media, including the written word.[15] In a prehistorical context, such conditions would have been rare or even non-existent (in the case of written transmission).

2.3.3 *The use of PIE as an archaeological tool.* Last but not least, it should be remembered that PIE is no more and no less than an abstract linguistic construct and should be treated as such. As Stefan Zimmer points out in two recent articles (1988, 1990), the reconstruction of a PIE lexicon and

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[14] These first two conditions apply, for instance, to the role of Latin throughout medieval Europe. I am not referring here to the earlier spread of Latin, which did involve the relocation of Latin-speaking populations and from which the Romance languages subsequently evolved, but to the later use of Latin, even in non-Romance countries, as the language of the Church and of the scholarly community; but as we know, in this context Latin did not come to replace the native languages of its users.

Another example would be the role of French in 17th, 18th and 19th-century Europe. Although it was the language of the aristocracy and of diplomacy, the general use of French certainly never spread beyond previously-francophone populations.

[15] Examples of this in a modern-day context might be the use of Swahili in many parts of eastern Africa, the use of French in Zaire and other equatorial African countries, or the use of English in post-colonial India.

grammar does not necessarily give us the linguistic picture of a group of PIE speakers at one point in time, or even in one location in space. For one, the information we have on PIE may well represent a linguistic continuum of several millennia into which different lexical items were introduced at different stages. Unfortunately, an internal chronology of the PIE lexicon simply cannot be established with any degree of certainty.

Furthermore, we must stress the difficulty of even approximately reconstructing the *semantic* content of each PIE form. Historical linguists know only too well how dramatically the semantics of a given word can shift in the course of a few centuries, let alone several millennia. Any given PIE form may have had different meanings at different points in time within the time-frame posited for the PIE language. Gimbutas, however, does not allow for such an eventuality in her Kurgan–PIE scenario. Yet it is imperative, in working on the problem of Indo-European origins, that the contents of the PIE lexicon not be treated too literally, but instead, that it be taken with a sizable grain of salt.

2.4 *Conclusions*

Given all the internal and external problems with Gimbutas' Kurgan–PIE hypothesis which I have been able to raise in a paper as short as this one, I would like to suggest that her arguments should be scrutinized more thoroughly by the linguistic community, the archaeological community, as well as the popular press who have contributed to making the Kurgan–PIE correlation the popular hypothesis that it is. The problem of the PIE homeland (and that of any other proto-language) is certainly a complex one; in my opinion, its solution may only be sought in a better understanding and cooperation between archaeologists, linguists, and scholars from other related disciplines.

3. *The Anatolian Hypothesis*

The most recent and probably the most controversial addition to the list of PIE homeland theories is the Anatolian Hypothesis put forward by the linguists T. V. Gamkrelidze and V. V. Ivanov. Their theory made its first major appearance on the western academic scene in 1985 when three articles, which had been published in the former Soviet Union a few years earlier, were translated from Russian into English and published in the Spring/Summer volume of *The Journal of Indo-European Studies*. Gamkrelidze & Ivanov's theory is characterized by an overwhelming emphasis on linguistic argumentation, based their own reconstruction of PIE phonemes, a relatively exotic interpretation of the PIE lexicon, and supposed evidence of close linguistic contacts with other language groups in the Near East. Their theory has certainly been the subject of much debate, not only within the ranks of linguists, but also among archaeologists (e.g., Gimbutas 1985, Mallory 1989) whose general criticism has been the difficulty in corroborating Gamkrelidze & Ivanov's claims with any significant amount of archaeological data.

In this third and final principal section, I will focus primarily on the various levels of Gamkrelidze & Ivanov's linguistic argumentation. It will be shown that throughout their research on PIE phonology, lexical items and linguistic contacts, the authors tend to make a series of questionable assumptions, and then draw sometimes surprising conclusions which they treat as if it

were hard evidence. Furthermore, I will examine certain criticisms which have been raised by other contemporary scholars. In particular, I will address the comments of K. M. Hayward (1989) in his extensive review of their work, and those of I. M. Diakonov (1985), who not only points out some obvious flaws in their reasoning but also takes the opportunity to propose an alternative hypothesis. Finally, I will briefly compare Gamkrelidze & Ivanov's research with the work of British archaeologist Colin Renfrew on the same subject, as he also proposes a Near Eastern origin of PIE culture. It will be demonstrated, however, that Renfrew's theory cannot be used effectively to support Gamkrelidze & Ivanov's Anatolian hypothesis.

3.1 *Gamkrelidze & Ivanov's Theories*

The linguistic arguments most relevant to Gamkrelidze & Ivanov's (henceforth: G & I) homeland hypothesis may be divided into two general categories. First, there is the lexical inventory of PIE which they use in and of itself to deduce the natural environment of the speakers of PIE. This involves the authors' particular version of the PIE lexicon and their semantic interpretation thereof. As we shall see, not only do they reconstruct lexical items which are not usually attributed to PIE, but they also use the PIE lexicon to draw conclusions which are, at times, rather daring.

Second, the authors emphasize the linguistic similarities which PIE supposedly shares with certain other languages groups, most importantly Semitic and Kartvelian. This second line of argumentation occurs at two levels: one phonological and the other lexical. More specifically, the first type of argument involves the system of plosive consonants which the authors have reconstructed for PIE. This proposal has caused somewhat of a stir in Indo-Europeanist circles since it calls for a significant revision of the system of plosives established by August Schleicher and his

followers, and refined by subsequent generations of scholars, particularly Saussure (1879), Möller (1917), Kuryłowicz (1927), and others, who contributed to the development of the Laryngeal Theory which has become widely accepted in recent years. Nevertheless, the 'Glottalic Theory' as it has come to be known, has found a number of proponents other than G & I.

At the lexical level, the authors emphasize what they see as ample evidence for very close and prolonged contact between PIE and other language families which are traceable to Near Eastern origins — or so we are told. I will argue that the semantic qualities and variety of these lexical similarities are such that they cannot easily be explained by usual processes of linguistic borrowing, and that the possibility of genetic affiliation should be considered.

3.1.1 *Evidence from the PIE lexical inventory.* To narrow down the geographic area from which the speakers of PIE might have originated, G & I seek recourse in their own reconstruction of the PIE lexicon, one which differs not only phonologically from traditional reconstructions, but which also includes lexical items which have not been acknowledged as being of Indo-European origin by most other linguists. Nevertheless, they make significant use of this inventory of lexical items in order to eliminate other homeland candidates:

> The conclusion that the original homeland could not have been in central and eastern (excepting southeastern) Europe as deduced on the basis of evidence about the landscape and ecological habitat concurs with cultural and historical data on domestic animals and cultivated plants with which the ancient Indo-Europeans must have been acquainted. (1985a:11)

There are several categories of lexical items which they consider to be relevant in this process of deduction, though it is noteworthy that PIE forms referring to climate and topography are given as

much emphasis as forms concerning fauna, flora, economy and technology. These will be the semantic fields under scrutiny in the present section.

In Tables VII to XII below I have juxtaposed G & I's lexical reconstructions with the ones of Buck (1949), Pokorny (1959), Mann (1984) and Watkins (1992), for the sake of comparison. Most of these forms have already been presented in section 2. on the Kurgan Hypothesis, however I would like to present them again here in order to better illustrate the semantic and lexical particularities of G & I's version of the lexicon. The phonological differences will be ignored for the moment, as I will discuss the authors' revisions of PIE phonology in subsection 3.2.1.

3.1.1.1 *Topography.* Given the remoteness of PIE on the one hand, and the wide range of concepts which can be observed in any human language on the other, it is indeed surprising that G & I choose to make particular claims such as: "The first point which may be stated with sufficient assurance with regard to the homeland of the Indo-European language is that it was a region with a mountainous landscape" (1985a:9). This claim, though not necessarily false, is based solely on a limited set of PIE forms which could certainly be interpreted in a variety of different ways. They are presented in Table V below.

G & I's PIE reconstructions	Meaning according to G & I
*ont'-/nt'-	mountain, cliff, rock
*m-(e)n-t^{h}-	mountain, height
*k^{h}ol-	height
*b^{h}erg^{h}-	high
*or-u-n	large lake, sea
*sal-	[salt] sea or lake
*Hap^{h}-	river, stream
*t^{h}ek^{h}-	to flow rapidly

Table V: *G & I's PIE lexemes of topography*

All this list suggests to me is that PIE speakers knew of areas which were mountainous, or perhaps only hilly to one degree or another, and that they were familiar with rivers and large bodies of water. This strikes me as neither surprising nor very revealing and, in my opinion, it does not entail that the speakers of PIE were living in particularly mountainous surroundings. Moreover, it is not clear to me why the authors interpret **or-u-n* as referring specifically to a *mountain* lake simply because it may describe a body of fresh water as opposed to salt water. Neither do the authors explain how the mere existence of the commonly accepted reconstruction **t^{h}ek^{h}-* results in the sweeping qualification of **Hap^{h}-* as a *rapidly* flowing river (1985a:9). To claim that the above list "naturally rules out those flat regions of Europe where there are no large mountain ranges" (1985a:9-10) seems at least speculative. The authors' argument that PIE originated in a mountainous region is therefore suspect.

3.1.1.2 *Climate.* G & I also point to PIE climatic concepts (see Table VI below) which, in their opinion, constitute further "evidence for a high mountain climate with overcast skies and frequent thunderstorms" (1985a:9). I would suggest that temperate regions in Eurasia where cloudy and stormy weather never occurs are probably quite rare. Nevertheless, the authors pursue this argument by pointing to the presence of forms describing the concept of 'heat' or 'warmth' which supposedly "rules out situating the original Indo-European homeland in the northern regions of Eurasia" (1985a:9). This is a rather incredible statement; even Inuit peoples of the North American Arctic regions have words for 'heat', and I would be surprised to find a language that didn't, given the fact that temperature contrasts are a fundamental part of the human sensory experience.

G & I's PIE reconstructions	**Meaning according to G & I**
*neb^{h}-es-	cloud, storm cloud, sky
*Huent^{h}-	wind
*Hk'or-	mountain wind, northern wind
*seu-/su-	rain
*sneig^{h}o-	snow
*g^{h}eim-	winter
*(i)ek'- & *eis-/is-	cold, ice
*g^{h}oer-m- & *t^{h}ep^{h}-	heat, warm

Table VI: *G & I's PIE lexemes for climatic conditions*

3.1.1.3 *Flora.* With regard to the plant life known to speakers of PIE, G & I do not diverge significantly from the traditional list of concepts, except for terms such as 'cherry', 'grape', 'yew', 'pine', 'walnut', 'heather', and 'rose'. These terms, they argue, narrow down the possible location of the PIE homeland even further, pointing to "the comparatively more southern regions of the

Mediterranean area in the broad sense, which includes the Balkans and the northern part of the Near East" (1985a:10). However, it has been argued by most other linguists that these key forms which G & I attribute to PIE are attested only in certain southern groups of Indo-European languages, and consequently, cannot be reconstructed beyond a reasonable doubt for PIE as a whole. Table VII (below) illustrates that none of these particular forms find corroboration in the four other lexical inventories. For that reason, I would rather take the side of caution and dispute, for the time being, the legitimacy of these contentious forms.

Furthermore, even if there were better support for the reconstruction of certain roots, I cannot agree with the argument that the presence of crop and fruit tree names *per se* indicates the practice of advanced agriculture and viticulture as G & I claim (1985a:11). These appellations may simply refer to wild species which only later became associated with similar, cultivated varieties.[16]

Next, I would like to point out the difference in meaning between G & I's **ieuo-* and the that of the other corresponding forms. The authors give this root the very specific meaning of 'barley' whereas the meaning given by others is usually the more general concept of 'grain'. This may seem like a harmless discrepancy; however, G & I claim that barley does not appear as a predominant crop in the northern regions of Europe "until the end of the second to the first millennium B.C." (1985a:12) and that this restricts those peoples who were acquainted with this cereal much earlier to a more southern location. The problem with such an argument is that it takes the proto-vocabulary at face value, leaving absolutely no room for possible (even likely) but unrecoverable semantic shifts.

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[16] I would like to thank Dr. Roger Blench of the University of Cambridge for drawing my attention to this point. His example involved certain African tribes who harvest cereals which grow in the wild, and have evidently harvested them in this way for millennia. Another example can be found in the harvesting of wild rice by native North American tribes. In both cases, cereal became a source of food without being cultivated for this purpose.

Meaning according to G & I	G & I's PIE forms (1985)	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)	Generally accepted meaning
barley	*ieuo-	*yewo-	*ieuo-	*ieuos	*yewo-	grain
wheat	*p^{h}ur-	—	*pū-ro-	*pūros	*pūro-	wheat
flax	*lino-	*līno-	*lī-no-	*līnos	*līno-	flax
apple/apple tree	*sam(a)lu-	—	*ābel-	*ābəlos	—	apple
Cornelian cherry/cherry	*k^{h}rno-	—	—	—	—	—
mulberry tree	*moro-	—	—	—	—	—
grape, wine	*uo/ei-no-	—	—	—	—	—
(Greek) walnut	*g^{h}ar-	—	—	—	—	—
tree, oak	*te/oru-	*derwo-	*deru-	*deru-	—	oak
oak, cliff	*p^{h}er(k^{h})-	—	—	—	*perk^{w}u-	oak
mountain oak	*aik'-	—	—	—	—	—
acorn	*k'oelH-	—	*g^{w}el-	*g^{u}el-	—	acorn
birch	*b^{h}erHk-	*b^{h}erəĝ-	*b^{h}erəg-	*b^{h}erĝos	*b^{h}erəg-	birch
beech	*b^{h}aHk'o-	*b^{h}āgo-s	*b^{h}āgo-s	*b^{h}āĝos	*b^{h}āgo-	beech
hornbeam	*(s)k'rob^{h}o-	—	—	—	—	—
ash	*Hos-	—	*ōs-en-	*ōsnos	—	ash tree
asp/aspen	*Hosp^{h}-	—	*apsā	*apsā	—	aspen
willow, white willow	*so(e)lik^{h}-	—	—	—	—	—
yew	*ei-/oi-	—	—	—	—	—
pine/fir	*p^{h}it^{h}-	—	—	—	—	—
heather	*uer-	—	—	—	—	—
rose	*urot-/urt'-	—	—	—	—	—
moss	*m(e)us	—	—	—	—	—
Cf. Table XVI		—	*b^{h}ar-	*b^{h}ars-	*b^{h}ares	barley
Not mentioned in G & I		*ĝherzd(h)	*ĝherzd(h)	—	—	barley
		—	*m(e)lēi-	*meli	—	millet
		—	*b^{h}ab^{h}ā	*b^{h}abos	*b^{h}a-b^{h}ā-	legume
		—	*elem-	*əlm-	—	elm
		—	*aker-	*akeros	—	maple tree

Table VII: *G & I's PIE lexemes for flora*

In other words, we simply cannot be sure that **ieuo-* originally referred to the plant which we now refer to as *barley*, and therefore, it seems imprudent to limit the possible *Urheimat* to only those regions in which barley can be attested for the 4th (or any other) millennium B.C.

A similar observation also applies to other species, most notoriously the oak and the beech, which have been used since the late 19th century (e.g., Hehn 1976 [1885], Schrader 1890) by most linguists to date, including G & I (1985a:10), as an essential tool for more precisely defining the boundaries of the PIE homeland. In my opinion, such argumentation is rendered obsolete by what we have learned from both modern research in sociolinguistics with regard to language dynamics in the present, as well as from the field of historical linguistics concerning the evolution of specific languages within historical times.

For example in Canadian French, "maize" is called *blé d'Inde*, which translates as "wheat from India" or "Indian wheat", because the first French explorers who arrived in the New World, thinking they had reached the Asian continent, saw that maize was a staple crop for the natives and named it after the staple with which they were most familiar. Similarly, among English-speaking North Americans, *corn* now specifically refers to the maize plant, whereas in Europe it has maintained the generic meaning of "cereal crop", possibly referring to wheat, barley, rye, or oats, depending on the region. What is noteworthy is that there is a relatively large difference in appearance between, for instance, wheat grass and American maize, and yet the former generated the more popular name for the latter. Therefore, it is perfectly conceivable that a tree rather different from the beech might have, for any number of reasons, eventually lent its name the beech nevertheless.

Other incongruities between the lexical inventory of G & I and those of others include the absence of some commonly reconstructed forms such as **meli-*, **b^{h}ars-*, **g^{h}erzd^{h}-*, **b^{h}ab^{h}a*, **elem-*,

and **aker-* (roughly reconstructed). Whether it is because the authors do not recognize them as legitimate PIE lexemes or whether there is some other reason is not clear to me; in the case of **bʰars-*, at least, they claim that the root was borrowed from a non-Indo-European source, thus it was presumably not part of the original PIE lexicon. The question of loan words is discussed further in sub-section 3.2.2.

3.1.1.4 *Fauna*. Like most other scholars, the authors assume that in PIE society, all the animals in Table VIII (below) had already been domesticated, whereas Table IX represents wild animals. From a strictly linguistic point of view, I would argue that this cannot necessarily be assumed, just as I have argued earlier that the presence of forms designating grain crops and fruit trees does not in itself entail the practice of organized agriculture in a given society. We should not presume that in prehistoric times words like 'horse', 'cow' or 'sheep' carried the same semantic content as they have in historic times; intrinsic connotations of domestication may be more recent than the period in which PIE was still spoken.

Meaning according to G & I	G & I's PIE forms (1985)	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)	Generally accepted meaning
horse	*ek^{h}uo-	*eR^{w}o-	*eRuo-s	*eRuos	*ek^{w}o-	horse
ass	*osono-	—	—	—	—	—
sheep	*Houi-	*owi	*oui-s	*ouis	*owi-	sheep
goat	*qhok'-	—	*aiĝ-	*aiĝis	—	goat
dog	*k^{h}uon-	*Ruon-	*kuon-	*kuō	*k^{w}on-	dog
swine	*su-	*su-	*sū-s	*sūs	*sū-	pig
piglet	*p^{h}ork^{h}-	*porRo-	*porRo-s	*porRos	*porko-	young pig
Cf. Tables XV & XVI		*g^{w}ou-	*g^{u}ou-	*guōus	*g^{w}ou	cow
		*peRu	*peR-	*peRu	*peku	cattle
		*ag^{w}(h)no-	*ag^{u}h-no-s	*agunos	*ag^{w}h-no-	lamb

Table VIII: *G & I's PIE lexemes for domesticated animals*

It is mainly in the PIE designations for wild animals (see Table IX below) that G & I seek further support for a southern ecology. For instance, they assure us:

> Some of these animals [i.e., 'panther', 'lion', 'elephant', 'crab'] are specific to the southern geographic region, which rules out central Europe as a possible original territory of habitation of the Indo-European tribes [...]. (1985a:11)

However it is precisely the reconstruction of this group of animals which is not supported by the other four sources listed in Table VIII. Apparently, these glosses cannot legitimately be assigned to the PIE lexicon, some due to an unjustified semantic reconstruction, others on the grounds that they are probably loan words from non-Indo-European sources. In fact, even G & I admit that the form they have reconstructed for 'ape' (cf. Table XVI) had most likely been borrowed from Semitic. However, it is not clear how they were able to determine that this loan word, as well as number of other loans

Meaning according to G & I	G & I's PIE forms (1985)	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)	Generally accepted meaning
wolf	*ulk^{h}o-/*ulp^h-	*wl^{kw}o-	*ulk^{u}os	*ulk^{u}os	*wl^{kw}o-	wolf
bear	*Hrt^{h}k^h-	*rk$^{'p}$o-	*ṙkto-s	*∂rkt-	*rtko-	bear
lion	*leu-	—	—	—	—	—
lynx	*leuk^h	—	—	—	—	—
fox, jackal	*ulop^h(e)k^h-	—	*ulp-, lup-	—	*wlp-ē-	fox
wild boar	*qhuep^h-	—	—	—	—	—
dear, antelope	*el-(e)n-/ *el-k^h-	*el-	*elen-	*el∂nis	—	deer/elk
wild ox, aurochs, bison	*t^{h}auro-	—	—	—	—	—
hare/rabbit	*k^{h}as-(no-)	—	*ṙas	*ṙas-	*kas-	hare
elephant, ivory	*ieb^h-/*Heb^{h}u- + *leb^h-ont^h- /*leHb^{h}o-	—	—	—	—	—
snake	*og^{h}oi-/*ang^{h}oi-	—	*ang^u(h)i-	*anguhis	*ang^{w}i-	eel/snake
crab	*k^{h}ark^{h}ar-	—	—	—	—	—
bee	*b^{h}ei-	*b^{h}ī-	*b^{h}ei	—	*b^{h}ei-	bee
fish	*d^{h}g^{h}u-	—	—	—	*d^{h}g^{h}ū-	fish
bird	*Huei-	—	—	—	—	—
eagle	*Heor-	—	*er-/or-	*oros	*or-	eagle/ large bird
water bird, goose, swan	*g^{h}ans-	*ĝhans-	*ĝhans-	*g^{h}ansis	*g^{h}ans-	goose
crane	*k'er-	—	*ger-	*ger ənos	*gerə-	crane
raven/crow	*k^{h}er-	—	—	—	—	—
black grouse	*t^{h}et^h(e)r-	—	—	—	—	—
woodpecker, small bird, finch	*(s)p^{h}ik^{h}o-	—	—	—	—	—
Not mentioned in G & I		*epero-	*epero-	—	—	boar
		—	—	—	*b^{h}ib^{h}ru	beaver
		*anəti-	*anət-	*anətis	—	duck
		—	*lak-	*lakəsos	*laks-	salmon

Table IX: *G & I's PIE lexemes for wild animals*

words, were adopted into PIE at such an early stage. Critics have pointed out that this reconstruction, just as the ones I have already mentioned relating to southern flora, can only be attested through a select few Indo-European languages, mostly of the southern regions. Thus it is certainly possible, if not likely, that these words appeared somewhat later and were not part of the original proto-vocabulary. On the other hand, what is surprisingly missing, in comparison with other inventories, are the terms for 'duck' and for 'salmon'.

3.1.1.5 *Economy*. As already mentioned above, I see no reason to assume that the speakers of PIE had domesticated cows, pigs, sheep or horses simply because names for these animals can be reconstructed. This same point was made quite early on by Fraser (1926:266-267). Similarly, I do not believe that the mere presence of grain-crop designations is sufficient to reconstruct the scenario of an agriculturally advanced society (as I have already indicated).

However, it is the case for PIE that we can also reconstruct terms such as 'to plow', 'to sow', and 'furrow' (see Table X below). These terms are generally accepted as valid reconstructions of the concepts. It seems reasonable, then, to deduce that speakers of PIE were familiar with the basic techniques of agriculture and did indeed grow certain crops for their own consumption.

Moreover, the reconstruction of terms meaning "butter" and "wool" (Table XI, below) suggests that PIE societies had access to *live* animal by-products, or in other words, that they had domesticated (at least to some degree) animals such as sheep, and possibly even cattle. As for the reconstruction of a word for 'honey', in my opinion, this hardly proves that the speakers of PIE had developed the practice of beekeeping (as suggested by G&I, 1985a:11). With a bit of skill, honey may also be retrieved from bee colonies in the wild.

Meaning according to G & I	G & I's PIE forms (1985)	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)	Generally accepted meaning
to plow	*Har-	*ar-	*ar(∂)-	*arō	*arə-	to plow
plow	*Har-H-t^{h}ro-m	—	—	—	—	—
to sow	*seHi-	*sē-	*sē(i)-	*sēiō	* sē-	to sow
furrow	*solk^{h}u-	—	—	—	—	—
furrow	*p^{h}erk^{h}-	—	—	—	—	—
sickle	*serp^{h}-	—	—	—	—	—
time of harvest	*(e)s-en-	—	—	—	—	—
time of ripening	*Ham-	—	—	—	—	—
to ripen, to gather the harvest	*meH(i)-	*mē-	*mē-	*mēiō	*mē-	to mow, to reap
to treat barley grains by fire	*b^{h}erek'-	—	*b^{h}er-	—	—	to roast, to bake
to pound, crush [grain]	*p^{h}eis-	—	(*peis- ?)	—	—	to trample
to grind	*mel-	*mel-	*mel-	*melō	*melə-	to grind
millstone	*k'orau-	—	—	—	—	—
honey	*mel-it^{h}-		*meli-t			honey
Cf. Table XVI		*g^{w}er-	*g^{w}r̄-nu-	*guernu-	*g^{w}erə-nā	hand mill
		—	*g^{h}ren-	*gwhrend^{h}ō	*g^{h}rend^{h}-	to grind
		*ĝrno-	*ĝrē-no-	*ĝrnóm	*grə-no-	kernel

Table X: *G & I's PIE lexemes for an agricultural economy*

Meaning according to G & I	G & I's PIE forms (1985)	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)	Generally accepted meaning
pasture	*Hak'ro-	*aĝro-	*aĝ-ro-s	*aĝros	*agro-	field for cultivation
to pasture	*p^{h}aH-	—	*pā-	*pāsĸō	*pā-	to protect, feed
herdsman	*uest^{h}er-	—	—	—	—	—
oil/butter	*selp^h-	—	*selp-	—	—	fat/butter
oil/butter	*ong^{h}o-	—	—	—	—	—
wool	*Huln-	*wl̥nā	*ul̥-nā	*uln-	—	wool
to spin	*sneH-	—	*(s)nē-	—	—	to twist threads together
to cart wool	*k^{h}es-	—	*kes-	—	—	to scratch, to comb
to cart wool	*p^{h}ek^h-	—	*peĸ-	—	—	to pluck wool or hair
Not mentioned in G & I		—	*(demə-), domə-	*domāiō	*demə-	to break in a horse
		—	*reid^h-	*reid^h-	*reid^h-	to ride
		*melĝ-	*mēlĝ-	*melĝō	*melg-	to milk

Table XI: *G & I's PIE lexemes for a pastoral economy*

There are relatively few correspondences between G & I's reconstructions and the traditional ones, but the ones that exist share virtually identical semantic interpretations ('to plow', 'to sow', 'to gather the harvest' or 'to reap', 'to grind', and 'honey', Table X). As for the well-attested traditional reconstructions *$g^{\mu}\bar{r}$-nu, *g^{h}ren- and *$\hat{g}r\bar{e}$-no-, G & I attribute them to a single loan word from Semitic, *g-r-n (cf. subsection 3.2.2, Table XV), and thus, I have not included them in this list of original PIE forms.

G & I further argue that during the 4th millennium B.C. "livestock breeding (like agriculture) in Central Europe was in its embryonic stages" and particularly that "in the region north of the Black Sea and the Volga steppes, such developed livestock breeding [...] is almost completely absent up until the first millennium B.C." (1985a:11). According to the authors, the only possible area remaining for an agriculturally and pastorally developed society in the 4th millennium B.C. is south-eastern Europe and the Near East.

However, archaeological sources indicate that by the 4th millenium B.C., agriculture had already spread from the Near East to many other areas, including Greece, the Danube basin, and the whole of central Europe (Zohary 1990:38). As for the breeding of livestock, Renfrew (1987) describes it as spreading more or less in parallel with agriculture, most domestic animal species having originated in the same area as the early cultivated plant crops.

It is interesting that *Hak'ro- is interpreted as "pasture" instead of the usual "field for cultivation". It is also curious that G & I fail to mention such well-attested forms as *domə-, *$reid^{h}$-, *melĝō, and *$u\bar{r}$-nā, even though one would expect them to be present, given the meaning of the other roots which are reconstructed in Table XI above.

3.1.1.6 *Technology*. G & I emphasize that PIE 'evidence' for wheeled transport, in particular, further supports their hypothesis of a south-eastern *Urheimat* since, according to their archaeological sources,[17]

> the manufacture of wheeled vehicles dates from around the fourth millennium B.C. The original area of their distribution is accepted to be the area from the Transcaucasus to Upper Mesopotamia. Wheeled vehicles spread from the place of their inception in the Near East to the Volga-Ural region, the region north of the Black Sea, the Balkans, and Central Europe [...]. (1985a:13)

In their view, the diffusion of wheeled transport also neatly coincides with the spread of agriculture, and thus with the spread of Indo-European dialects. The authors speak of an "extensive Proto-Indo-European terminology for transport vehicles" (1985a:13), yet they themselves provide no reconstructions to support this.

As in preceding tables, a comparison of G & I's data with that of other scholars reveals several incongruities (Table XII below). At the semantic level, the authors' interpretation of **Haios* as "bronze" instead of the usual "copper" is most noteworthy because, contrary to other hypotheses which tend to date PIE back to the chalcolithic period, this implies that PIE society had already entered the Bronze Age. However in such a scenario, the relevant archaeological data would seem to be incompatible with G & I's chronological claims (Renfrew 1987:31), though the authors never actually address this particular problem.

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[17] These sources referres to by G & I cover a time span from Gordon Childe's 1954 "The Diffusion of Wheeled Vehicles" to a 1977 article by G. V. Gening entitled "Mogil'nik sintashta i problema rannikh indoiranskikh plemen".

Meaning according to G & I	G & I's PIE forms (1985)	Buck's PIE forms (1949)	Pokorny's PIE forms (1959)	Mann's PIE forms (1984)	Watkins' PIE forms (1992)	Generally accepted meaning
sword	*(e)nsi-	—	—	—	—	—
battle-axe	*o/ad-hi-es-	—	—	—	—	—
to cut, to shear, to cut out	*sk^{h}er-	*(s)ker	*(s)ker-	*kerpō	*kerp-	to gather, to pluck
wheel, wheeled vehicle, chariot	*k^{h}oel-, *k^{h}oek^{h}lo-	*^{kw}elo-s	*k^{u}ek^{u}lo-	*quequolos	* k^{w}(e)k^{w}lo	wheel
wheel	*rot^{h}o-	—	—	—	—	—
to turn [a wheel, circle, vehicle]	*Huer-t^{h}-, *Huer-g^{h}-	—	*uer-	—	—	to turn
harness	*d^{h}ur-	—	*d^{h}er-	—	—	to hold
axle	*Hak^{h}s-	*aĸs-	*aĝes-, aĸs	*aĸsis	—	axel
yoke	*iuk'om-	*yugo-m	*iu-go-m	*iugóm	—	yoke
to carry [in a vehicle], to pull	*ueg^{h}-	*weĝh-	*ueĝh-	*ueĝh	*weg^{h}-	to go or transport in a vehicle
ride in a vehicle	*iaH	—	—	—	—	—
bronze	*Haios-	*ayes-	*aios-	*aios-	*ayes-	copper
to navigate in a boat or ship using oars	*erH-/*reH-	*erə-	*erə-	*erət-	*erə-	oar/to row
to navigate (in a vessel)	*p^{h}leu-	—	*pleu	—	—	to run, to flow
Cf. Table XVI		*nāu-	*nāu-	*nāu	* nāu-	ship
Not mentioned in G & I		*mazdos	*mazdo-s	—	—	mast
		—	*eneb^{h}-	*nob^{h}-	*nob^{h}-	wheel hub/ navel
		—	—	*ōros	—	ore
		—	—	*g^{h}elĝh-	*g^{h}el-	gold
		—	—	*arĝntom	*arg-	silver

Table XII: *G & I's PIE technological lexemes*

3.2 *Similarities between PIE and other linguistic groups.*

In addition to the emphasis which G & I place on the *semantic* value of PIE lexical data to support their Anatolian Hypothesis, they point to another type of 'evidence' which is supposedly inherent in the structure of PIE and which, in their opinion, clearly points to a Near Eastern origin. This evidence consists of certain similarities which G & I claim to observe at two different levels, phonological and lexical, between PIE and other language groups such as Semitic, Kartvelian, and languages of the Caucasus (e.g., Sumerian).

The supposed phonological similarities described by G & I are generated from the authors' significant revision of the classical PIE phonological system. This revision is motivated by some rather strong concerns (which are reemphasized by Gamkrelidze in a forthcoming article) over the plausibility in terms of linguistic universals, in general, and of the typological consistency, in particular, of linguistic reconstructions. A variety of lexical similarities which G & I also notice between PIE and other languages of reportedly 'Near Eastern' origin are interpreted as the multilateral borrowing of words within a so-called *Sprachbund*, a close coexistence over a long period of time of a group of unrelated languages. For the purpose at hand, I will focus my attention on those forms which, according to G & I, have been borrowed into PIE from non-Indo-European sources. The authors also give numerous examples of PIE loan words which made their way into non-Indo-European languages, particularly into Semitic and Kartvelian. However, I will not be discussing those examples in any detail, as do not feel that I have sufficient knowledge of Semitic or Kartvelian to comment on the significance of such data.

3.2.1 *Phonological similarities.* As early as 1973, after having reexamined PIE with regard to certain typological concerns, G & I put forward a new theory of the PIE phonological system. In this section I will focus specifically on the set of PIE plosive consonants which they have reconstructed, as this is perhaps the most pertinent aspect of their theory with regard to the present discussion about the Anatolian Hypothesis.

For the sake of comparison, we first present the traditionally accepted set of PIE plosives.

	Set 1	Set 2	Set 3	Set 4
Series I	(b)	*d	*g^{w}	*g'
Series II	*b^{h}	*d^{h}	*g^{wh}	*g'^{h}
Series III	*p	*t	*k^{w}	*k'

Table XIII: *Traditional reconstruction of the PIE plosive system*

This we contrast with the set of PIE plosives reconstructed by G & I (1980a, b).

	Set 1	Set 2	Set 3	Set 4	Set 5	
Series I	(*p')	*t'	*k'^{w}	*k'	*k''	
Series II	*b^{h}	*d^{h}	*g^{wh}	*g^{h}	*g'^{h}	
Series III	*p^{h}	*t^{h}	*k^{wh}	*k^{h}	*k'^{h}	*q

Table XIV: *G & I's reconstruction of the PIE plosive system*
N.B.: ′ following a plosive indicates palatalization, while ' indicates glottalization.

We notice three fundamental differences. Firstly, G & I admitted a set of *non-palatal, non-labial* velar plosives, *k', *g^{h}, *k^{h} (G & I's Set 4), in addition to the usual sets of labio-velar plosives *g^{w}, *g^{wh}, *k^{wh} (Traditional Set 3) and the of palatal velar plosives *g', *g^{h} *k^{h} (Traditional Set 4). So, for example, what has been traditionally reconstructed as *$ulk^{w}os$ or *$wlk^{w}o$ ("wolf") by most other

linguists is now reconstructed as **ulk^{h}o*, with a non-labial velar plosive, by G & I (cf. Table IX above).

Secondly, G & I have replaced the series of *unaspirated* voiceless plosives **p*, **t*, **k^{w}*, **k'* (Series III) with a series of *aspirated* voiceless plosives, **p^{h}*, **t^{h}*, **k^{wh}*, **k^{h}*. The reasoning behind this additional feature of change in aspiration is the general acknowledgement that, from a typological standpoint, languages which have a series of voiced aspirates such as **b^{h}*, **d^{h}*, **g^{h}* (as everyone seems to agree PIE did), usually also have a parallel series of voiceless aspirates.[18] Hence, G & I for instance reconstruct **k^{h}as-* for "hare, rabbit" instead of the traditional **kas-* (cf. Table IX above).

Thirdly, and most notably, they have replaced the series of voiced unaspirated plosives *(b), d, g, g^{w}* (Series I) with homorganic voiceless, glottalized (also referred to as 'ejective') counterparts *p', t', k', k^{w}', k''*. This particular reconstruction of PIE velar plosives has come to be known as the Glottalic Theory. Again, the motivation for this revision has to do with the authors' concern for the typological 'realism' of linguistic reconstructions. Under the traditional system of PIE plosives, Series I has always been particularly problematic in that the voiced labial **b* almost never occurs in reconstructed PIE lexical forms; this rarity of **b* not only defies linguists' intuitive expectations (Pedersen 1951, for instance), but it also directly contradicts typological predictions. Studies in linguistic typology (e.g., Melikišvili 1972) have observed the relatively frequent occurrence of */h/* in languages which have a series of voiced plosives, on the one hand, and a tendency for */p/* to be

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[18] In their 1973 article, G & I refer to Jakobson (1958), who had already noticed and discussed this particular typological inconsistency in the traditional reconstruction of PIE plosives. More recently, Polomé, who is known for his opposition to the G & I phonological revisions, also admits in a footnote that the absence of voiceless aspirates in the classical reconstruction of PIE presents a typological incongruity; he refers to Martinet (1955:115) who, even before Jakobson, "points out that voiced aspirates occur only in languages that have also voiceless aspirates: therefore, the system reconstructed for Indo-European can only be a 'transitory' system" (Polomé 1994:30). In the same footnote, he goes on to mention Jakobson (1958:23) and Szemerenyi (1967:65-99), both of whom express the same concern over the absence of PIE voiceless aspirates despite the presence of voiced ones.

the phoneme occurring least frequently in series of voiceless plosives, on the other. To explain the rarity of the PIE phoneme of Series I/Set 1 in the PIE lexicon, G & I suggest that we must be dealing not with */b/*, but with a different phoneme whose typological frequency matches the frequency of its distribution in PIE. Thus, based on the typological data presented in Jucquois (1966) and in Melikišvili (1972), the authors propose this phoneme to be a voiceless labial, just as Pedersen had already suggested in 1951.

Furthermore, to account for the fact that the plosives in Series I occur relatively seldom in comparison to the plosives of Series III, which are also voiceless, G & I propose that Series I plosives must be more marked than Series III plosives. By postulating glottalic plosives for Series I, G & I claim that their phonological reconstruction best reflects 'real' typological data in terms of the frequency of occurrence which is typical for the plosives in Series I and Series III, respectively. Thus, they reconstruct forms such as **b^haHk'o-* "beech", instead of **b^hago-*, **k'er-* "crane", instead of **ger-*, and **Hak'ro-* "field", instead of **agro-* (cf. Tables VII, IX, and XI respectively).

G & I repeatedly underline the importance of verifying that a reconstructed language not only agree with linguistic universals, as far as they can be determined, but that it be typologically plausible, if the reconstruction is to be at all useful in historical studies:

> Among diverse theoretical patterns of linguistic reconstruction arrived at with the aid of genetic comparison of related dialects typological criteria must give preference to only one of them considered linguistically most plausible and realistic (Gamkrelidze, forthcoming).

With these criteria in mind, the authors are remarkably confident of the 'realistic' nature of the sound system which they have reconstructed for PIE; according to them, their particular reconstruction of PIE phonological system also shows definite typological similarities as well as regular sound correspondences with certain Near Eastern languages:

> Characteristic of the Proto-Indo-European, Kartvelian, and Semitic languages is an isomorphic structure in the consonant system, with three series of stops: glottalized (or pharangealized), voiced, and voiceless. [...] Such a similarity, even to the point of isomorphism in the way linguistic structures are formed, could have been the result of prolonged interaction of these languages and of their entering into allogenetic relations with one another. (1985a:14)

Thus, the argument goes, speakers of PIE may be traced back to a location adjacent to the homelands of these other languages, homelands which, presumably, can be dated to the same time frame as PIE and were located in the Near East, though G & I never refer to any other scholarly evidence supporting this claim. The non-specialist, it seems, must simply take the authors' word that Semitic, Kartvelian, and South Caucasian languages were all originally spoken in the Near East around the same time as PIE, that is, during the 4th millennium B.C. or so.

It seems interesting that even in the light of recent proposals of a 'Nostratic' theory, G & I never address the possibility (be they for or against it) that the similar phonological systems of a supposed PIE-Semitic- Kartvelian-South Caucasian *Sprachbund* may actually be due to genetic affiliations, as opposed to inter-linguistic contact. Moreover, the absence from their work of a discussion about the validity a Nostratic-type model becomes even more conspicuous given that they pursue their observation of linguistic similarities between PIE and 'Near Eastern' languages even further, at the lexical level.

3.2.2 *Lexical similarities.* As I have indicated in section 3.2.1 above, G & I attribute non-PIE origins to a variety of items in the PIE vocabulary. Moreover, they find an even greater number of forms in Semitic, Kartvelian, Sumerian (and Caucasian, Hattic, Elamite, Hurrian, Urartian) which, they claim, were borrowed from PIE (1985a:14-24). In this section, however, I will examine only those forms which G & I qualify as loans from non-Indo-European sources into PIE. The two

primary sources of loan words, according to the authors, are Semitic and Sumerian. I have represented the loan words borrowed from these sources (1985a:15-19) in Tables XV and XVI (below), respectively, along with the original, non-Indo-European forms from which the PIE forms were supposedly derived. In examining these tables, there are, in my opinion, a few curiosities worth noting: firstly, among the list of loan words, G & I include forms which have always been thought of as part and parcel of the original PIE lexicon.[19] Some of these forms were conspicuously absent from Tables VII–XII (above), where G & I's reconstructions were compared to those of other scholars. Secondly, the loan words do not seem to follow a particular thematic pattern; except for the numeral 'seven', they are all noun or verb roots as opposed to prepositions or adjectives for instance, but they span across a relatively wide range of semantic categories. This range is even wider in the body of words which, according to G & I, were borrowed from PIE into Semitic, Kartvelian, Hattic, Elamite, Hurrian, and Urartian (1985a:19-24). However, for reasons of economy, my observations will relate only to those forms which are assumed to be borrowings into PIE.

Meaning according to G & I	**PIE**	**Supposedly from Sumerian**	
ox, cow	*k'o(o)u-	nu(d) [also *ng'ow* (Egyptian)]	a large type of ox
field, arable plot	*ak'ro-	agar	irrigated territory, arable plot
yard	*d^{h}uer-	tur	yard, cattle pen
peak of a mountain	*He/ok'or	kur	mountain, mountainous country
ore, copper	*r(e)ud^{h}-	urud	red
gold	*(a)ues(k^{h})-	guškin	gold

Table XV: *Supposed PIE borrowings from Sumerian*

[19] In Tables XV and XVI, the rows containing these original PIE forms have been shaded.

Meaning according to G & I	PIE	Supposedly from Semitic	
seven	*septm	*s-b-'-tu	seven (fem.)
sacrifice	*tap^h-	*d-b-h	to sacrifice, to slaughter
ox	*t^{h}auro-	*tawru	ox
goat	*g^{h}ait'-	*gadju	kid, (she-)goat
lamb	*ag^{h}no-	*iglu	lamb, young animal
ape	*qeop^h-	ukupu (Akkadian) qop (Anc. Egyptian)	ape
grain, groats	*b^{h}ar(s)	*b-r(r)	grain, threshed grain
grain, bread	*d^{h}oHna-	*d^{h}unu	millet, sorghum
plants used for food	*Hand^h-	*h-n-t	to gather buds, to ripen (wheat, grain)
millstone	*k'oern-	*g-r-n	to mill, to grind
intoxicating drink, beer	*alu(t^h)-	*h-l-w-	sweet, sweet drink
honey, honey drink	*med^{h}u-	*m-t-k-	sweet
to buy, to trade	*krhor(e)i-	*k-r-	to trade
battle-axe, axe	*p^{h}elek^{h}u-	*p-l-k-	to cleave/chop, axe
axe, battle-axe	*sek^{h}ur-	sukurru (Akkadian)	axe
to lock, key	*k^{h}laHu-	*k-l-	to lock
vessel or boat, container	*naHu-	*'-n-w	container, (nautical) vessel

Table XVI: *Supposed PIE borrowings from Semitic*

Even assuming that all of the PIE forms in Tables XV and XVI can indeed be reconstructed,[20] and admitting that there are some visible parallels between the PIE forms and the corresponding non-Indo-European forms, I am nevertheless tempted to question whether these parallels can be uniformly ascribed to linguistic borrowing. The dynamics of linguistic borrowing are still not fully understood. However, Diakonov (1985:116-122) remarks that it is far from occurring in an arbitrary fashion, and indeed, there are particular patterns of borrowing which seem to emerge, the elements of which are certainly interrelated.

First of all, loan words are most often adopted in conjunction with the introduction of significant technological or cultural innovations. In other words, there is a motivation to borrow words for concepts which are quite new to the speakers of the adoptive language, whereas there is rarely motivation to tag new labels onto wide-spread or even universal referents. For example, the word *tobacco* was adopted from native North Americans in the 16th century, and subsequently spread throughout Europe along with its referent. In much more recent times, the English term *computer* has made its way into languages all over the world as electronic technology has become the most important means of global communication.

As is well established, linguistic borrowing does not usually occur within certain semantic categories such as names for body parts, lower numerals, common elements of the natural environment, etc., which tend to form a stable core within the lexicon of a given language.[21] So, for example, it is difficult to explain why PIE speakers would feel the need to borrow the Semitic designation (in a feminine declension, no less!) for the numeral 'seven', or a Sumerian lexeme to

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[20] This, in itself, constitutes a matter of dispute (cf. Diakonov 1985).

[21] These semantic fields have been described in the well-known list compiled by Morris Swadesh.

describe the 'peak of a mountain', a concept which is unlikely to have been lacking in the original PIE vocabulary, given the supposedly mountainous character of the *Urheimat*.

This brings me to the next point. One might be justified in working on the premise, be it arrived at intuitively or empirically, that loan words are chronological late-comers to an adoptive lexicon. If they were not, the whole notion of an 'original' PIE vocabulary versus a distinctly borrowed set of lexemes would become nonsensical. The presence of recognizable loan words within a given lexicon can, in my opinion, provide a perhaps rudimentary but crucial relative internal chronology for the lexicon as a whole. Therefore I find it puzzling, for instance, that PIE speakers would be motivated to borrow a Sumerian word to derive an appellation for 'copper' when, according to G & I, they had already arrived at the level of a Bronze Age economy[22] and, thus, must have already been familiar with copper for some time. It is also unclear why they needed to borrow the Semitic word for 'boat' (becoming **naHu* in PIE) despite indications that they were already familiar with seafaring technology (PIE **erH-* "to navigate using oars", **p^{h}leu-* "to navigate"). Such chronological incongruities are quite numerous in G & I's list of loan words, and certainly undermine their claims.

In the literature on lexical borrowing, one may also find examples where a foreign language was perceived as having a higher level of social prestige by indigenous speakers and loan words were eventually introduced for their stylishness, and not because they were filling a semantic lacuna. The adoption of words like *venison*, *pork*, *mutton* and *veal* from French into the language of the conquered Britons, even though the English words *game*, *pig*, *sheep* and *calf* were already available, illustrates such a scenario. It is important to note that in cases like this, lexemes tend to be adopted

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[22] Let us recall that G & I's semantic interpretation of the PIE root **Haios* is "bronze"; most other scholars reconstruct this same root as meaning "copper".

not in isolation, but rather as semantically definable groups. It can be argued, in my view, that such patterns of borrowing are recognizable in the tables above. The words which G & I claim were borrowed into PIE span across several semantic categories:[23] aside from the improbable borrowed numeral and topographic term, there are a few animal names, some agricultural concepts, and a scattering of technological indicators such as 'gold', 'boat', 'key', and 'axe'. It is my opinion, however, that in every case, the forms lack the characteristic cohesion of prestige-motivated borrowings. By this I mean that even within each of these semantic fields, forms do not share a semantic feature consistent enough to explain why PIE speakers would have chosen to superimpose them onto their own corresponding forms.

In conclusion, I would argue that the lexemes in Tables XV and XVI represent unlikely examples of linguistic borrowing, either because they are chronologically inconsistent with original PIE forms of the same category, or because they show no patterns consistent enough to suggest prestige-influenced borrowing, or any other pattern for that matter, within a semantic category. Most questionable of all is the claim that lower numerals and other generally stable lexemes were for some reason adopted into PIE from other sources, and even vice-versa.

An alternative explanation which G & I do not seem to consider at all is that these unlikely similarities, at least the ones involving generally accepted PIE forms, may not the product of linguistic borrowings at all, but that they may represent cognates from genetically related languages. However, such an explanation is potentially incompatible with the authors' conviction that PIE

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[23] The body of words which are supposed to have been borrowed from PIE into Kartvelian, Semitic, and other languages is much larger and contains an even greater variety of semantic types, including some of the ones least likely to be borrowed; a few extreme examples are: PIE **ok^{h}t^{h}-a-* "four fingers" into Kartvelian **(o)st^{h}x(o)* "four", PIE **soek^{h}s-* "six" into Katvelian **ek^{h}sw-* "id.", and PIE **k^{h}ert'-* "breast/chest" into Kartvelian **m-k'erd-* "id.".

speakers must have been in physical contact with the speakers of Semitic, Kartvelian, Sumerian, and others, in the context of a *Sprachbund*, since the formation of separate dialects usually involves a gradual geographic distancing of one degree or another.

3.3 *Gamkrelidze & Ivanov's conclusions*

The area which G & I ultimately propose as the prehistoric cradle of the speakers of PIE includes the eastern part of modern Turkey, northern Iran, and the former Soviet republics of Georgia, Armenia and Azerbaijan (see Appendix, Map III). The authors presume that only such a location could accommodate all the lexical items they list in the PIE lexicon, as well as accounting for the phonological and lexical correspondences which they note between PIE and other language families of Near Eastern origin.

The relative chronology of the initial IE dialectal dispersal, as described by G & I (1985a, b), is schematized in Table XVII below. The authors estimate the latest possible date for the use of PIE at around the turn of the 3rd millennium to the 2nd millennium B.C., when the Anatolian group, the Greco-Aryan group and the Balto-Slavic-Germanic group had already separated, and the remaining dialectal groups were on the verge of doing the same. The authors describe the Indo-European peoples as migrating westward, some of them making their way around the Caspian Sea, and eventually into Europe (G & I 1985b). Here, however, I am not so much concerned with the routes of PIE dispersal as with its catalyst, because it establishes a chronological limit for the original unity of PIE speakers and it also determines what type of archaeological evidence is relevant in identifying the *Urheimat*.

4th milennium B.C.		*3rd millennium B.C.*		*2nd millennium B.C.*	
4000	*3500*	*3000*	*2500*	*2000*	*1500 B.C.*
PIE	**>Anatolian**	> Anatolian	> Anatolian	> *Hittite*	
				> *Luwian*	
	> **Tocharo-Celto-Italic**	> Tocharian			
		> Italo-Celtic	> Italic		
			> Celtic		
		> **Greco-Armeno-Aryan**	> Greek	> Eastern Greek	> *Arcadian-Cypriot*
					> *Ionian*
					> *Aeolian*
				> Western Greek	> *Dorian, etc.*
			> Armeno-Aryan	> Armenian	
				> Indo-Iranian	
		> **Balto-Slavic-Germanic**	> Balto-Slavic		
			> Germanic		

Table XVII: *G & I's chronology of the Indo-European dispersal*
Legend: **Proto-language** > **dialectal group** (> sub-group > sub-group) > *attested language(s)*, where the symbol > means "developed into".

The motivation given by G & I for the gradual dispersal of Indo-European speakers, starting with the Anatolian group sometime during the 4th millennium B.C., is quite different from the one proposed in Gimbutas' 'Kurgan Hypothesis'. According to the authors, the spread of Indo-European dialects is directly related to the spread of agricultural technology from the Fertile Crescent.[24] This, they say, resulted in an unprecedented food surplus and a subsequent demographic explosion which pushed people to seek out new pasture land and arable soil for cultivation. As we will see in the final section, this is also the position independently held by Renfrew based on his archaeological research (cf. section 3.5). However, I will show that his explanation is incompatible with the chronological time-frame which G & I set out for PIE.

For the moment, I would simply underline the fact that this scenario defines PIE speakers as belonging to a Near Eastern culture which was both pastorally and agriculturally developed, as opposed to the semi-nomadic, pastoralist steppe culture depicted by Gimbutas.

3.4 *Reception of G & I's theories by the scholarly community*

Criticisms of the Anatolian hypothesis as a whole, and of the specific claims on which it is founded, have been numerous and varied. On the linguistic front, for instance, Diakonov (1985) has commented very disapprovingly on his colleagues' use of lexical data, both in terms of 'original' PIE forms and of supposed loan words. He takes the time to demonstrate that most of their pivotal examples (Diakonov 1985:110-137) either involve forms which cannot be properly attested for PIE,

[24] This region extended from the Nile Valley up to Northern Syria, and along the region of Mesopotamia, between the Tigris and the Euphrates rivers.

or that they suffer from semantic speculation, or even misinterpretation, usually in favour of a southern origin.

However, most scholars have concentrated their efforts on scrutinizing G & I's reconstruction of PIE phonology, rather than their lexical inventory or their semantic interpretations. This is perhaps due to the fact that few comparative linguists are in a position to confidently assess the authors' claims regarding loan words, as they would have to possess at least some knowledge of Semitic and Kartvelian as well as Indo-European languages.

In this section, I will assess the reception of G & I's phonological theories by the linguistic community in general, giving particular attention to Diakonov's (1985) criticisms.

3.4.1 *The mixed reviews of the Glottalic Theory.* Apart from Hopper (1973, 1982), who himself proposed a very similar[25] theory independently of G & I, the notion of the PIE phonological system including glottalic consonants (interchangeably referred to as 'ejectives' in the literature) has found a number of proponents. These include Bomhard (1979, 1984, 1988), Vennemann (1983, 1984), and Diakonov (1985), who argues however that the set of glottalized plosives should be voiced, as we will see in section 3.4.2 below.

Some of the more established scholars, such as Mayrhofer (1983, 1986), Martinet (1986), Lehmann (1983, 1986) and Polomé (1982) have approached the theory more cautiously, but do not entirely reject it. In fact, in a recent article Polomé seems relatively sympathetic to the Glottalic Theory, perhaps more so than in the past. This may have something to do with the fact that

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[25] Although Hopper reconstructs the glottalized stops **p'*, **t'*, **k'* instead of the traditional **b*, **d*, **g*, just as G & I do, his system differs from theirs in that he reconstructs neither voiced nor voiceless aspirates at the phonemic level.

accounting for Germanic consonant shifts has always been problematic under the classical system, whereas under the Glottalic Theory, the task is rendered much easier. Thus he diplomatically concludes that

> a number of issues remain unsatisfactorily solved under the reformulation of the old pattern of the pre-Germanic consonantal system, but the need to reevaluate long-accepted conclusions is clearly illustrated by the sweeping reinterpretation of the Germanic consonant shift the new approach entails [...]. (Polomé 1994:17)

Other scholars, such as Dunkel (1981), Haider (1985), Pisowicz (1988), and most notably Szemerenyi (1985) have, for various reasons, taken a firm position against it.

In 1988, K. M. Hayward published a rather extensive review article of G & I's 1984 publication, a two-volume monograph in Russian, *Indojevropeiskij jazyk i indojevropejcy*,[26] which brought together all their theories on PIE origins. In particular, Hayward disputes the relevance of Jucquois's (1966) typological data on which the Glottalic Theory hinges, suggesting it is seriously misinterpreted by G & I.

3.4.2 *Diakonov's modification of G & I's PIE theory.* In his 1985 review of G & I (1985a, b), Diakonov not only criticizes the basic phonological foundations of the Anatolian Hypothesis, but he also proposes an alternative theory which is commonly referred to as the 'Balkan Hypothesis'. His position in favour of a PIE homeland located in the Balkan-Carpathian area (ca. 5th–4th millennium B.C.) is supported by relatively balanced amounts of linguistic and archaeological argumentation, though Diakonov's archaeological authority is open for discussion. His linguistic

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[26] The titled translates into *Indo-European and the Indo-Europeans*; an English edition is still under way.

argumentation, on the other hand, strikes me as an interesting contribution to the study of PIE origins as it is largely derived from his own research, rather than a reinterpretation of the analisys of others.

The evidence he presents is essentially three-fold. First, he re-examines the system of PIE plosives posited by G & I and modifies it in a way that, according to him, eventually points the linguist in the direction of the Balkan regions. Second, he tackles specific categories within the reconstructed PIE lexicon which relate to the physical and cultural environment of its speakers. Third, he evaluates their claims with regards to the borrowing of PIE forms into other languages and vice versa. On the whole, Diakonov argues not so much for overwhelming positive evidence in favour of the Balkan Hypothesis, but rather that there no real negative evidence against it.

Diakonov begins by revising both the traditional as well as G & I's non-traditional theories of PIE phonology. Though he seems to agree with the basic principals of G & I's reconstruction rather than the traditional one (which he considers to reflect a later stage of phonological development), he draws attention to the fact that G & I themselves had observed how "in one of the groups [he does not say which] of Proto-Indo-European dialects there were only two sets of velar stops" (Diakonov 1987:95): $*k^{w}$, $*g^{hw}$, $*k^{hw}$ (Set 3), and $*k'$, $*g^{h}$, $*k^{h}$ (Set 4). They had explained the loss of the third set of velar stops, i.e., Set 5 ($*k^{\circ}$, $*g^{\circ h}$, $*k^{\circ h}$), as being the result of its merger with the second set (Set 4). Diakonov suggests that, consequently, their original table of PIE plosives should be simplified as follows:

	Set 1	Set 2	Set 3	Set 4
Series I	(*p')	*t'	*k'^{w}	*k' / *k''
Series II	*b^{h}	*d^{h}	*g^{wh}	*g^{h} / *g''^{h}
Series III	*p^{h}	*t^{h}	*k^{w}h	*k^{h} / *k''^{h}

Table XVIII: *Diakonov's simplification of G & I's PIE plosive system*
(What happens to G & I's reconstructed phoneme *q remains unclear to me.)

Thus, non-labialized velar stops of the same series are reinterpreted as allophones which would appear in mutually exclusive phonetic environments. This reinterpretation then prompts Diakonov to go even further and suggest that, in fact, it is those IE dialects that do *not* have the third set of velar plosives (i.e., the palatalized ones) which have retained the "archaic state of affairs" (1985:97), because otherwise it becomes difficult to derive the 'satemized' reflexes of eastern Indo-European dialects from Set 4 in a systematic way. Originally, palatalization would have been an unnecessary feature to distinguish Set 3 from Set 4; but when the eastern dialects started palatalizing the phonemes in Set 4, the feature of labialization in Set 3 became superfluous and was eventually dropped. In time, the palatalized velars of Set 4 became affricated, and later still, the affricates were spirantized (i.e., 'satemized'). So, for example, the PIE phoneme *k'' would have been subjected to the following changes: *k' > *k' > *č > š, while PIE *k'^{w} would have lost its labial (and apparently also its glottalic) feature: *k'^{w} > k.

With this in mind, Diakonov redefines the table of PIE plosives in the following manner:

	Set 1	Set 2	Set 3	Set 4
Series I	(*p')	*t'	*k'^{w}	*k'
Series II	*b^{h}	*d^{h}	*g^{wh}	*g^{h}
Series III	*p^{h}	*t^{h}	*k^{wh}	*k^{h}

Table XIX: *Diakonov's reconstruction of the PIE plosive system*

Diakonov's model, however, has inherent implications with regard to the initial process of PIE dispersal which are incongruent with G & I's scenario. It is based on the premise that all the dialects where satemization eventually occurred (e.g., Balto-Slavic, Armenian, and Indo-Iranian) can be traced back to a common Eastern Indo-European group which also included Anatolian and Greek,[27] dialects which did not undergo satemization. Anatolian would have separated from the group first, moving further east. The Greek group would have separated next, migrating south. The remaining groups would have undergone the process of satemization together, and then separated as well. Obviously this scenario cannot be reconciled with G & I chronology where satemizetion is thought to have begun before the PIE dispersal,[28] even though, on the one hand, Armeno-Aryan is in the same sub-group as Greek, whereas on the other hand, Balto-Slavic in a separate sub-group with non-satem Germanic.

If Diakonov's description of the satemization process is correct, it seems that G & I's account of the PIE dispersal must be seriously reevaluated, both for its internal chronology and for the

[27] Diakonov makes this connection on the grouds of "numerous lexical and especially grammatical (morphological) isoglosses" between these groups (1985:98).

[28] They even ascribe some degree of satemization to Anatolian, though Anatolian is generally classified as a 'centum' group.

direction in which the various dialects spread, since their version seriously hinders the movement of linguistic groups to their historic locations.

3.5 *The problem of archaeological support*

The hypothesis has received mostly sceptical reviews as regards its archaeological argumentation. Not surprisingly, some of the harshest criticism has come from none other than Marija Gimbutas (1985). She rightly points out that "the authors' usage of archaeological materials is too superficial, the chronology used in many cases is incorrect, and that much of the published information is unknown" (Gimbutas, p.185). It is true that, particularly in their early publications, G & I seemed not to have felt much need to back up their claims with supporting references from other scholars. Only recently has independent work by Colin Renfrew (1987, 1990) shown some ability to substantiate G & I's hypothesis, though there are certainly many conflicting areas with regards to their interpretations of PIE chronology. Here, I will briefly assess the impact of Renfrew's theory on the overall credibility of G & I's hypothesis.

In his 1987 monograph entitled *Archaeology and Language*, Renfrew discusses anew the problem of Indo-European origins by trying to explain the spread Indo-European through the 'wave-advance' model proposed by Ammerman & Cavalli-Sforza (1973, 1979), rather than through the migrationist model associated with *Stammbaum* scenario. He associates the PIE dialectal dispersal with the spread of the farming economy which can be traced from the Near East into Europe and western Asia. He argrees that this dispersal started gradually around the 7th millennium B.C.

Of course Gamkrelidze wasted no time in enthusiastically welcoming Renfrew's theory, declaring that his own linguistically-based hypothesis for a *Urheimat* in the Near East had finally

"been fully corroborated and enhanced by solid archaeological evidence" (Gamkrelidze 1990:9). The linguist fails to recognize, however, that there is a very crucial discrepancy between his hypothesis and that of Renfrew's, namely that Gamkrelidze dates the beginning of the break-up of PIE at ca. the 4th millennium B.C., whereas Renfrew estimates it to have begun approximately three thousand years earlier. This massive discrepancy must certainly be reduced if the two theories are to support each other. For now, both authors rely so heavily on their respective absolute chronologies that is difficult to see how Renfrew's theory on the spread of agriculture could be seen as properly supporting G & I's Anatolian Hypothesis.

3.6 *Conclusions*

The Anatolian Hypothesis as proposed by G & I has yet to find proper archeological support. Given that their chronology of PIE dispersal seems to show inconsistencies even from a purely linguistic point of view, a reconsideration of that part of their theory may prove to be most fruitful. In principle, Renfrew's association of the 'wave-advance' spread of farming with the spread of Indo-European dialects seems rather attractive; it seems to provide an accurate picture of independent communities which were acquainted both with agriculture and stockbreeding. If the dating of PIE could be reconciled with this scenario, it would make for a most attractive alternative to other *Urheimat* hypotheses, which tend to rely on unconvincing migrationist explanations. I this were the case, even G & I's more exotic reconstructions would become unnecessary in order to argue in favour of a Near Eastern genesis. Unfortunately, this reconciliation has not yet been achieved. Presumably, the only other way to validate G & I's hypothesis would be to positively identify the PIE

speakers with a Near Eastern archaeological culture of the 4th millennium B.C.; yet even the authors themselves admit defeat on this front.

It appears to me that much linguistic verification is still required, and much archaeological evidence still needs to be provided, in order for the Anatolian Hypothesis to be taken seriously by the mainstream of PIE scholars.

4. *General Conclusions*

Clearly, the problem of the PIE homeland is far from solved. I have presented only the three most important modern hypotheses on the topic, and yet there is very little consensus among their respective proponents. The only point they seem to agree upon is the approximate date of initial PIE dispersal, which is generally estimated to the turn of the third millenium B.C. As regards their other arguments, be they linguistic or archaeological, there is very little or no overlap in their discussions.

To summarize my examination of the hypotheses in Sections 1-3, I will briefly address some issues of methodology and of inter-disciplinary collaboration. Finally, I will conclude with a few thoughts on the theoretical considerations which should be involved in the use of PIE as a tool for prehistoric studies.

4.1 *The competition of methodologies*

I have demonstrated in the previous sections how the problem of the PIE *Urheimat* has been approached from three very different angles. The variations of the North-Central European Hypothesis presented by Schmid and Kitson both rest almost exclusively on evidence from onomastic studies. They make practically no use of so-called 'linguistic paleontology', i.e., the interpretation of historico-cultural data which may be contained in a reconstructed proto-lexicon.

This latter method has always been much more popular (though, in my opinion, also more subjective) than the onomastic approach. I find it puzzling that no major efforts have been made to use the two methods in conjunction with each other, instead of applying them in mutually exclusive contexts.

The use of linguistic paleontology as a methodological tool is perhaps the only thing which the last two hypotheses have in common. However, because linguistic paleontology is a fruitless pursuit in isolation, and depends on the parallel use of other methodologies to arrive at any conclusions, the Eurasian Steppe Hypothesis and the Anatolian hypothesis must both seek recourse in adjacent types of research. It seems to me that, in both cases, the end result is one where linguistic paleontology is conveniently used to corroborate independent findings from the parallel research.

For example, in Section 2 on the Eurasian Steppe Hypothesis, Gimbutas seems to establish a Kurgan Hypothesis *first*, based on purely archaeological observations, and then proceeds to create a picture of the PIE homeland and subsequent dispersal which fits neatly over her archaeological findings. The problem is that in order to do this, she has had to be rather selective in her use of linguistic data, as well as in her interpretation of that data. In other words, she has put the cart before the horse. In my view, such an unsystematic approach should have given her linguistic proponents real cause for questioning the relevance of her theory, especially if one considers that, by virtue of its nature, the study PIE is first and foremost a matter of *linguistic*, not archaeological investigation.

In the case of the Anatolian Hypothesis (Section 3), linguistic paleontology has also been made to support the results of other studies, but this time, linguistically motivated ones. I refer to Gamkrelidze & Ivanov's preoccupation with typological data at the level of PIE phonology which, in their view, is almost sufficient to place the *Urheimat* in the Near East. Thus, to support their

typological data, they fall back on an appropriate historico-cultural interpretation of the PIE vocabulary, not to mention the addtional argument of loan words which they propose. Here again, it appears to me that data from the PIE lexicon has been successfully adapted to fit a previously-defined theory. I would argue that, for this reason, linguistic paleontology should be carefully re-evaluated as a valid tool for the scholar of PIE, or any other proto-language.

Since other linguistic methodologies, such as onomastic and typological studies, seem to yield less ambiguous results, and since these methodologies seem to be providing most of the primary arguments already, I suggest that the practitioners of historical linguistic research would benefit from the fine-tuning of these newer approaches. The old, pliable crutch of linguistic paleontology should be abandoned, at least until the theoretical uses and limitations of the PIE lexicon have been more precisely defined.

4.2 *The problems of inter-disciplinary studies*

I have already pointed out that the problem of Indo-European origins is an inherently linguistic one and, thus, that it must be approached primarily through linguistic methods. The problem of many homeland theories in the past was that the concept of the 'speech community' was consistently associated with the concept of an 'individual culture', or even with that of a particular 'race'. Though the concern for racial identification has fortunately disappeared from Indo-European studies, the relationship between archaeologically attested, material cultures and linguistic groups is still viewed in different ways by different scholars. It is perhaps this lingering ambiguity which has permitted archaeologists such as Gimbutas and Renfrew to formulate elaborate hypotheses on the PIE question,

quite independently of the complex theoretical considerations to which of historical linguists must attend.

In a 1990 article, Renfrew himself recognizes this pit-fall after having participated in the inter-disciplinary Bellagio Conference on "Indo-Europeans and Pre-Indo-Europeans" of 1988:[29] "Certainly I came to see more clearly some of the difficulties in the linguistic field for the alternative thesis which I have been propounding" (p.15). Furthermore, the author rightly points out that there is a great need for linguists, archaeologists, and anthropologists to collaborate in the development of some new models for the dynamics of language dispersal and replacement.[30]

> It is my argument that the use of this kind of reasoning is the only way by which the archaeologist may reach toward the kind of historical reality which could also be intelligible in linguistic terms. Instead, I would go so far as to say that without some such reasoning of this kind, some clear awareness of the kind of archaeological evidence which we may be seeking, there can be no hope of arriving at a coherent view of Indo-European origins. (Renfrew 1990:22)

It is my suggestion that the necessary models might be successfully developed through a sufficient number of case studies of language dispersal in historic times. These would have include as many examples as possible from non-literate speech communities in order to best relate the data back to a prehistoric context.

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[29] The conference was held February 8-13, 1988, at the Rockefeller Foundation's Bellagio Study and Conference Center on Lake Como, in Italy. It was attended by some of the most prominent names in Indo-European studies: Thomas V. Gamkrelidze, Eric P. Hamp, Edgar C. Polomé, Karl Horst Schmidt, John A. C. Greppin, Thomas L. Markey, and others. Papers from these conference participants, and from other scholars such as I. M. Diakonov and Marija Gimbutas, were later brought together by Markey & Greppin (1990).

[30] In my view, the lack of such a model is part of the crux of Gimbutas' Kurgan-PIE hypothesis, as I have already stated above in subsection 2.3.2. Thus, it is perhaps no surprise that supporters of her hypothesis — particularly her archaeological colleagues such as Mallory (1989:158-164) — typically continue to treat linguistic paleontology as the most reliable means of uncovering the geographic source of Indo-European languages, as it is perhaps still the technique most accessible to linguists and non-linguists alike.

4.3 *Theoretical considerations for the study of PIE*

Last but certainly not least, I would like to reemphasize the point I had briefly made in sub-section 2.3.3, namely, that PIE is a purely theoretical construct of Comparative Linguistics, and though the approach to reconstructing proto-languages was inspired by methods from the pure sciences of the 19th century, such as anatomy and biology, the end product cannot be considered in the same scientific light. Pure science involves the independent verification of experiments through a reduplication of their results. Obviously, this has not been achieved by the comparative method in historical linguistics, nor was it to be expected.

I have shown in the tables on PIE vocabualry items how the same Indo-European cognates have been used by different scholars to reconstruct significantly different proto-forms. This is a telling indication of the imperfect conditions under which historical (and especially pre-historical) linguists must work, and nevertheless formulate their theories. For both the scholar and the amateur, it is always advisable to keep in mind the inherent limitations of PIE studies.

Concerning the idea of an absolute chronology of PIE, for example, Zimmer (1988:372) offers the following commentary:

> Every attempt, then to give absolute dates for 'PIE' (or dates for alleged different stages of 'PIE') is either based on the speculative identification of an archaeological culture with the speakers of the 'language of the PIEs' (e.g. Gimbutas, Renfrew, Mallory) or on what may be called 'intelligent guesses', deliberations of probability and feelings of appropriateness (e.g. Meid, Gamkrelidze-Ivanov). The first type of proposal is usually contested by fellow archaeologists and doubted by linguists, the second, being purely subjective because objective arguments simply do not exist, is bound to remain noncommital. As easily to be seen, many dates of both types have found their way to an often far too skeptical public, and produced strange assertions.

A similar verdict can be given, in my opinion, to the attempt at *precisely* identifying the original referents of reconstructed lexical items.

In conclusion, I am cautiously optimistic that with the development of new theoretical models, further research on language change, both at the structural and at the sociolinguistic level, and better interdisciplinary understanding, the debate over the origins of PIE may some day be resolved.

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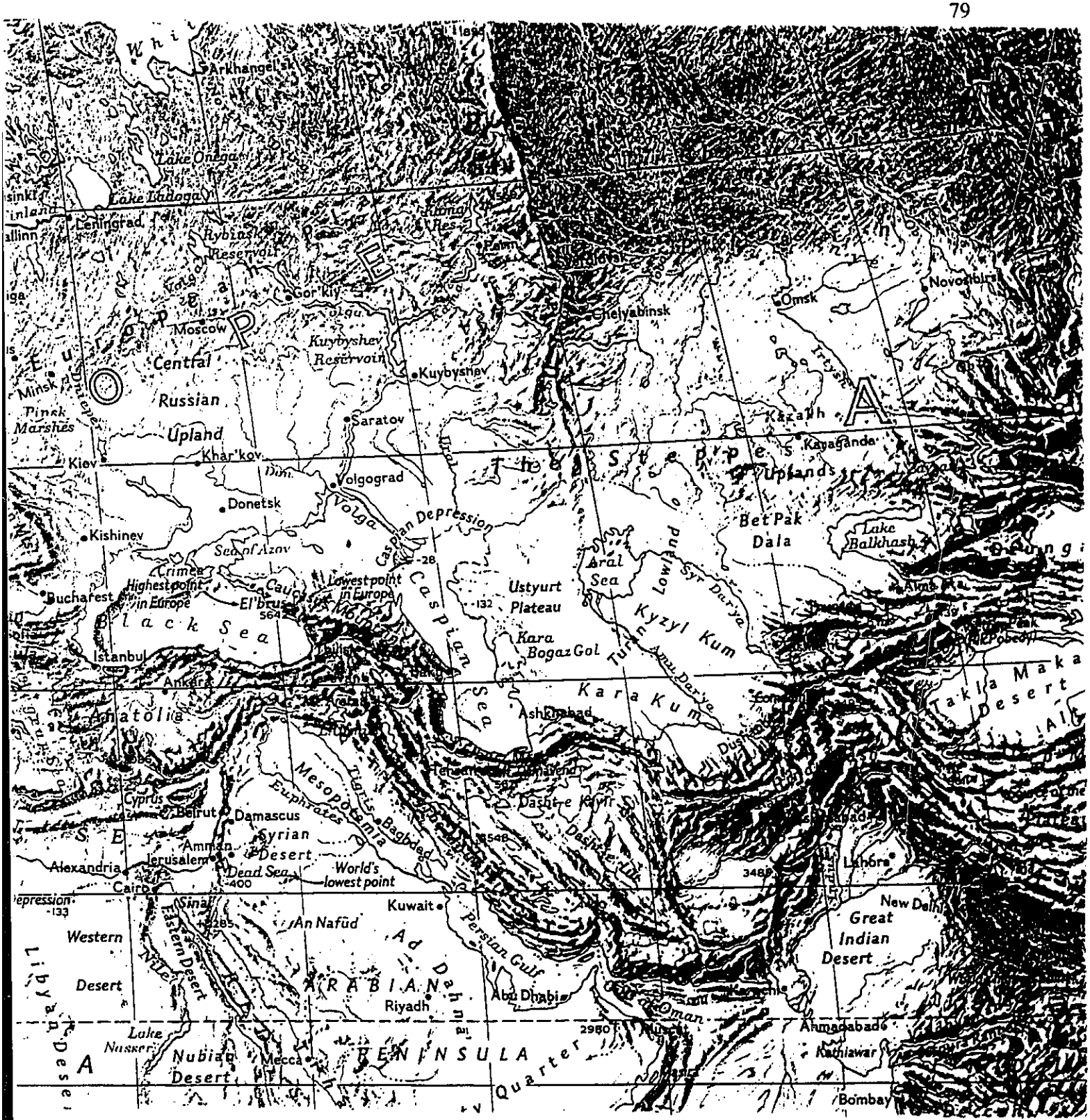
Map I: *The North-Central European homeland*



Map II: *The Eurasian Steppe homeland*

Warsaw
Marshes
Upland
Saratov
Prague
Kiev
Khar'kov
Ural
Dnepr
Dniester
Dniester
Dniester

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