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ARCHAEOLOGY AND LANGUAGE I

Theoretical and methodological orientations

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ROUTLEDGE

London and New York

7 *The homelands of the Indo-Europeans*

JAMES P. MALLORY

INTRODUCTION

After over 150 years the search for the homeland of the Indo-Europeans (IE) has still not achieved the type of solution that commands more than either partisan support or acceptance (often uninformed) by those wishing to defer to whatever 'experts' with whose works they have come into contact. That the homeland has been discovered there can be no doubt as it has been sought anywhere from the North to the South poles and from the Atlantic to the Pacific (Mallory 1973). Temporally, it has been located anywhere between 80,000 BC (Neanderthals) and *c.* 1600 BC (the expansion of chariot warfare from eastern Anatolia).

The underlying premise of the IE homeland problem is that the differentiation of Proto-Indo-European (PIE) into its various stocks (Celtic, Italic, Germanic, Baltic, Slavic, Albanian, Greek, Armenian, Anatolian, Indo-Iranian, Tocharian, etc. – Figure 7.1) was a factor of both time and space. All languages change through time, and hence differentiation between different speakers of any language can be expected to occur over time. As speakers of any particular language are increasingly dispersed, mutual interaction and communication will diminish such that the course and direction of language change will vary from one region to another. It should be emphasized that both factors themselves are liable to other constraints which will affect both the speed and the intensity of language change (Fodor 1965) – for example, economic systems, the nature of the terrain separating peoples, the existence of written standards or prestige dialects, foreign substrates/adstrates/superstrates, etc. But no matter how these may specifically influence language change, it is commonly accepted that at one time there existed a PIE language (or languages) whose dialects were far more closely related or similar than may be found among the different IE stocks known from their earliest entrance into the written record of the Late Bronze Age or Iron Age. The location of the IE-speakers before major differentiation into the various stocks is the IE homeland.

Figure 7.1 Distribution of the major stocks of the Indo-European languages.

SOME PRINCIPLES AND ASSUMPTIONS

A problem that has been so resistant to solution owes as much of its intractability to methodological matters (Dressler 1965) as it does to empirical data. Consequently, a preamble to any discussion of the homeland problem should lay down some basic principles.

Linguistic primacy

As the IE homeland problem involves a spatial definition of a prehistoric *linguistic* construct, the utility of any other discipline, such as archaeology, depends on whether a linguistic entity can be translated into something discernible in the archaeological record. In short, any solution not *purely* linguistic must involve some form of indirect inference whose own premises are usually, if not invariably, far from demonstrated.

Linguistic limitations

There is no purely linguistic method which resolves the problem of IE origins. Time does not permit a rehearsal of all the various linguistic solutions nor, probably, could the tolerance of any reader (insomniacs are directed to Mallory 1975 for a broad sample). We must deny ourselves the pleasure of marvelling at such works as that of Alexander Jóhanesson (1943) who argued that the proliferation of velars in PIE roots was imitative of the harsh cries of sea birds (hence a Baltic homeland), and, like Dr Watson's readers, the world is probably still not ready to entertain the consequences of Georg Schwidetsky's

(1932) comparison of Proto-Indo-European, Mongolian and Chimpanzee! But even after a judicious application of Pinkerton's principle – to confute absolute nonsense is surely as ridiculous as to write it – there are certain linguistic techniques that are widely recognized and these do warrant serious consideration.

CENTRE OF GRAVITY PRINCIPLE

The centre of linguistic dispersal is held to lie where there is the greatest degree of linguistic variation, while the peripheries are marked by the greatest degree of homogeneity (age–area hypothesis). This principle (beginning with Latham 1851, 1862) has been employed in locating the origin of many language families, e.g. Athabaskan (Sapir 1936: 234), Numic (Lamb 1958: 98), Salish (Jorgensen 1969: 105), etc. With respect to the IE homeland problem, it has often led to the conclusion that the centre of dispersion should be somewhere in south-eastern Europe since this region would embrace

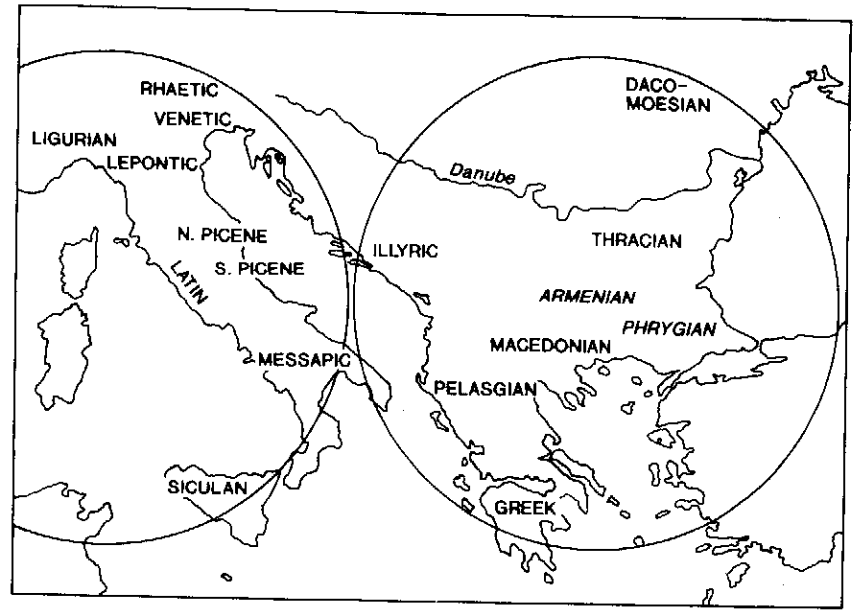

Figure 7.2 Centres of gravity. The Balkan centre of gravity proposed by Dolgopolsky consists of one language (Greek) which is well known and a series of languages such as Pelasgian, Macedonian, Illyric, Thracian and Daco-Moesian which are minimally (and generally only secondarily) attested, or languages putatively derived from the Balkans (Armenian, Phrygian) from an earlier temporal horizon than the other languages. An alternative homeland might just as well be set to Italy where there are even more (minimally attested) languages. Obviously, any employment of the 'centre of gravity' principle must be based on well known languages attested from the same temporal plane.

the greatest number of IE stocks (Mallory 1989: 152–3). It is worth taking a close look at this argument by citing its recent use by Aron Dolgopolsky:

> The Balkans constitute the area of the highest genetic diversity within the entire IE field: among the 12 (or 13) known branches of Late IE, 7 (or 8) were found in (or originated from) the Balkans: Greek, Macedonian, Phrygian, Armenian (originated from the Balkans according to ancient authors and the linguistic evidence), Thracian, probably Daco-Moesian (if it is not a branch of Thracian), Illyric (or different Illyric branches?), Pelasgian (one of several branches of IE preserved in a substratal lexical layer of Greek and in the few Philistine terms in Hebrew).
>
> (Dolgopolsky 1988: 12)

But what is the actual nucleus of this centre of gravity (Figure 7.2)? In the Balkans it basically consists of one language of which we know a great deal (i.e. Greek), and a series of ethnonyms that have been served up as distinct languages when the hard evidence is almost entirely confined to personal and place-names (which need not derive from the native languages in the first place) and a series of glosses. As we cannot decline a single noun, conjugate a single verb, count or even make it to square one of a Swadesh basic word list with these 'languages', i.e. Illyrian, Thracian, Macedonian (if not simply a Greek dialect (Brixhe and Panayotou 1994a)), Daco-Moesian (if not a Thracian dialect (Brixhe and Panayotou 1994b)), Pelasgian (lexical items in Greek believed to be remnants of an earlier language), it is extraordinary that we can pretend that they have different atomic weights, all contributing to an IE nucleus. We could just as easily place the homeland in Italy (Figure 7.2) and cite Ligurian, Lepontic, Rhaetic, Venetic, north Picenian, south Picenian, Messapic, Osco-Umbrian, Latin-Faliscan and Siculan (nine – one more than the Balkans). And as for Dolgopolsky's two additional languages – Armenian and Phrygian – which seemed to have escaped the orbit of the nucleus, even if we admit that they did have a Balkan origin, it was clearly at a date long before we have any evidence for our other various Balkan languages (similarly with the 'Pelasgian' remnants). Packing one's nucleus on the basis of 'privileged information' (i.e. assigning to the nuclear area languages which derive from different temporal horizons), is methodologically unsound unless, of course, we are also to be permitted to augment the Italian homeland argued above with Greek, Albanian, Sardinian, and all the modern dialects of Italy as well.

A final reason to be suspicious of employing the centre of gravity principle to argue a Balkan homeland is the dialectal position of Tocharian. For along with the diversity expected at the centre, one also expects peripheral homogeneity which seems to be breached in Indo-European as Tocharian is so markedly different from its neighbours of the Indo-Iranian superstock and is invariably associated on morphological and lexical grounds with IE stocks far to its west (Adams 1984). Whatever the path of IE dispersions, its trajectory does not seem to be transparently reflected in the position of the IE stocks. The (Late) IE homeland may have been in the Balkans, but it will require better evidence than the centre of gravity principle to demonstrate this.

CONSERVATISM PRINCIPLE

The centre of linguistic dispersal is held to lie where there is the least linguistic change from the proto-language, as this area is most likely to have been the least affected by foreign substrates (e.g. Poesche 1878: 66; Bender 1922: 53, etc.). This principle has been employed in a variety of ways, ranging from counting cognate clusters by stock to scoring (intuitively) the retention of PIE features – stress, accent, morphological features, etc. – across the various IE stocks. It is most often invoked to support a homeland in the Baltic region on the basis of the conservatism of Lithuanian (Hirt 1895: 653; Bender 1922: 55; Pisani 1940: 278; Georgiev 1966: 351) or the retention of alleged PIE river names in the Baltic region (Schmid 1972). The principle itself (which incidentally appears to contradict the assumptions of the 'centre of gravity' principle) is suspect because:

1 it presumes that the single or main cause of linguistic change is contact with other languages;
2 it requires one to 'measure' the degree of linguistic change between different languages on a commensurate scale; e.g. one might argue that Celtic retains the PIE stops better than Germanic but Germanic retains the differences between series much better than Celtic – how are such differences to be weighted?;
3 the principle leads to egregiously false conclusions – i.e. on an intuitive basis, Icelandic might well be regarded the most conservative of the Scandinavian languages but derivation of the rest of the Scandinavian languages from Iceland is historically absurd.

EXTRAFAMILIAL CONTACTS

The homeland is held to lie adjacent to the language family that shares the greatest similarity with the Indo-European family. This has been variously argued for Semitic (Hommel 1879; Schott 1936; Levin 1971, Hodge 1981; Dolgopolsky 1993), with a presumed Anatolian/Near Eastern homeland; Kartvelian (Gamkrelidze and Ivanov 1995), with an east Anatolian homeland; North-west Caucasian (Colarusso 1994), with a north Pontic homeland; and Uralic (Anderson 1879; Uesson 1970, Napolskikh 1995), with homelands ranging from northern and central Europe to the Urals or beyond.

Distinguishing between the level of contact between these other language families and the nature of the contacts (e.g. deep genetic contact between proto-languages, contacts between stocks of one family with the proto-language of another, etc.) is, as Aron Dolgopolsky (1993: 241–4) rightly argues, critical to the construction of a valid argument concerning geographical location. But to argue that 'unlike archaeological evidence, linguistic evidence (especially that of loanwords) is not liable to conflicting historical interpretations and is therefore decisive [in demonstrating the superiority of PIE-Semitic contacts]' is a remarkable statement indeed. If this essentially linguistic problem could have been resolved in the cold logic of linguists of good will, then one might well wonder what they have been up to this past century and a half. Take, for example, a set of well-known cultural words (boar, slave, pig, bee, honey, seven, etc.) that were borrowed into Finno-Ugric from (some form of) Indo-European. According to Dolgopolsky (1993: 242) they are Indo-Iranian and 'suggest that proto-Indo-Iranian . . . was once spoken in the vicinity of Finno-Ugric'. When we turn to the writings of Thomas Gamkrelidze and Vyacheslav Ivanov (1995: 815) we read that 'these [same] Finno-Ugric words are specifically early Iranian, not Aryan [i.e. Indo-Iranian] and above all not Sanskrit'. But then a review of J. Harmatta's (1981) detailed chronological ordering of these loanwords by Satya Misra (1992: 24) 'reveals that most of the loan words and the corresponding phonological changes ascribed to Indo-Iranian or Proto-Iranian are Indo-Aryan'. Some of these very words elicited as deriving from some stage of the Indo-Iranian superstock have also been regarded as '(quasi-) Tocharian' (Napolskikh 1994). Will the 'real' linguist please stand up?

It should be obvious that linguists have as much difficulty in establishing the chronological relationships between loanwords as any other 'historical science'. That there has been a loan of some type may be indisputable, and one may also find comparative agreement concerning both the direction of the borrowing and the relative chronological period in which the loan should have taken place. But there is no criteria where one can take a number of loanwords and read off distance and direction in kilometres. Furthermore, as the homelands of the other language families putatively in contact with Proto-Indo-European are no more securely located than that of the Indo-Europeans (although often argued as if they were), dead reckoning one unknown from the position of another unknown can hardly inspire confidence.

The conclusion here is that linguistics *alone* is unable to determine the prehistoric location of the PIE language irrespective of the confidence of individual linguists to make such claims.

The IE homeland is time factored

It is impossible to determine the geographical location of a prehistoric entity unless its chronological position is also determined. For example, in tracing the spread of English across the world today, the suggestion that it spread from England *c.* AD 1100–1900 is valid, while tracing it to the bearers of the Beaker 'culture' (*c.* 2500–1800 BC) and then on to North America, Australia or India is patently absurd. Yet we find a similar range of dates among the various proposals for an IE homeland in Anatolia which begin with the Early Neolithic in the 7th millennium BC (Renfrew 1987; Safronov 1989), then range on to the 5th millennium BC (Gamkrelidze and Ivanov 1995) to finish with the 2nd millenium BC (Drews 1988). Although all look to the same general homeland, they are not identifying the same people nor tracing the same expansions, and, given the fact that the 5,000-year gap between these theories could not help but result in massive linguistic evolution, there are no grounds to assume that they are talking about the same language either. Hence, the chronological position of PIE must be (approximately) determined before one may even hope to apply some form of indirect inference and determine its geographical location by some other means such as archaeology.

Linguistic date of Proto-Indo-European

Although PIE is a linguistic construct, there are no purely linguistic methods for providing absolute dates for a prehistoric language that are *verifiable* (Mallory forthcoming a). In the history of the IE homeland problem, three purely linguistic techniques have been employed: external contact dating, glottochronology and estimation.

EXTERNAL CONTACT DATING

There have been a few attempts to place PIE in real time by anchoring its lexicon to datable texts in another language family. As only Semitic, Sumerian and Elamite texts are likely to pre-date the earliest appearance of the IE stocks, it requires that one identify loanwords in PIE whose base form is precisely dated in an early literate language. Attempts to link words such as PIE $*h_2estar$ 'star' with Akkadian *astar* (Ipsen 1923) are controversial in the first place (Diakonoff 1985; Dolgopolsky 1993) and appear to lack the chronological shading once imputed them (indeed the word itself may well derive from the PIE verbal root $*h_2eh_x$ – 'burn' – and require no Semitic explanation). As external contact dating must be anchored to datable written texts in other languages, it is unlikely that there can be any suggestion of creditable matches before the 3rd millennium BC, a time by which most scholars would assume that PIE had already begun differentiating into its various stocks. The method is, therefore, so far useless if intended to associate the IE *proto-language* with a written language.

GLOTTOCHRONOLOGY

The dating of language separations on the basis of either the theoretical premise that there is a constant decay rate in the basic vocabulary of a language or on the basis of observed language splits in the written record (Swadesh 1960; Tischler 1973) is currently disregarded by most linguists *involved in IE studies* (*pace* Ehret 1988) who find neither the premise nor its implementation valid in an IE context. Since this technique is still applied in other linguistic regions (to the astonishment of some Indo-Europeanists) and attempts are even now being made to refine the technique (V. Blažek, pers. comm.; but see also Coleman 1994), the ball is clearly in the court of those who support glottochronology to demonstrate why anyone should accept their findings.

ESTIMATION

The technique whereby linguists 'estimate' the time-depth necessary for separation between languages, especially between the earliest appearance of IE languages in the Bronze Age (Anatolian, Greek, Indo-Iranian), generally leads to estimates that fall somewhere between 5,000 and 2,500 BC (e.g. Milewski 1968: 39; Cowgill and Mayrhofer 1986: 69–70; Zimmer 1988; sometimes still earlier, e.g. Dolgopolsky 1993: 238–9). This forms an extensive body of 'informed speculation', but it offers neither means of testing nor, indeed, is the reasoning for such estimates usually made explicit. While the 'weight of linguistic opinion' does require serious consideration, it is sobering to reflect that prior to the invention of radiocarbon dating and its calibration, European archaeologists grossly underestimated the antiquity of the European Neolithic.

CONCLUSION

Linguists generally date the initial separation of the IE stocks to the period *c.* 5,000–2,500 BC but these dates are provided largely on the basis of estimation rather than the product of an empirically validated methodology.

Linguistic-cultural date of PIE

The application of the comparative method to reconstruct the cultural vocabulary of a proto-language (e.g. Gamkrelidze and Ivanov 1995), coupled with archaeological evidence, may provide very broad dates for the separation of a proto-language (Mallory 1976). Reconstructed PIE reflects a vocabulary which unequivocally exhibits an economy based on domesticated plants (grain) and animals (cattle, sheep, goat, pig, dog), and associated technology (grinding stone, sickle) indicating that the separation of the IE stocks was unlikely to have occurred anywhere before *c.* 7000 BC and later, depending on its geographical location. It also contains a number of items such as plough, yoke, wheeled vehicles, wool, possibly silver, which are not generally attested earlier than *c.* 5000–3000 BC (Mallory 1989). It should be emphasized that the time-depth of these reconstructions is valid for *all* IE languages, and Dolgopolsky's (1993: 240–1) suggestion that Anatolian preserves an especially archaic vocabulary, untouched by subsequent later cultural developments reflected in the other IE languages which moved away from an IE homeland in Anatolia, is very difficult to sustain. While terms are not abundant, Anatolian does reflect many of the latest IE technological or cultural items and even supporters of same Anatolian homeland, e.g. Gamkrelidze and Ivanov (1995: 623–7), employ Anatolian cognates in their reconstruction of Proto-Indo-European words for 'yoke', 'wheel', 'harness', 'thill' and (less convincingly) even 'ride (in a vehicle)'. To this list may also be appended other items such as 'wool'. On lexico-cultural grounds alone, there are no convincing reasons to separate Anatolian from the other IE languages with respect to time-depth. On the other hand, it is necessary to emphasize that the utility of such dates is limited by the following factors:

1 The reconstructed vocabulary (assuming that it meets all linguistic criteria) indicates only the cultural items known to IE speakers before differences between them became so great that loanwords might be marked. It does not necessarily (although it could) reflect the vocabulary within the specific borders of the 'homeland' itself.
2 Mobile items in the reconstructed vocabulary (e.g. horses, wheeled vehicles, etc.) may have entered any region later than the initial spread of PIE provided that they arrived before the IE language of the particular area had differentiated sufficiently to permit the detection of loanwords (Mallory forthcoming a).

It should be noted that the actual utility of the reconstructed lexicon is very much tied to the construction of test cases. For example, one may wish to propose that the Proto-Greeks (or Proto-Indo-Europeans whose language would eventually evolve into Greek) occupied Greece since the beginning of the Neolithic, i.e. *c.* 7000–6500 BC. If one grants to the IE lexicon a word for 'horse', a reconstruction that is attested in so many IE stocks that it can hardly be denied (Hamp 1990a) and whose cognate forms are to be found in both Mycenaean and Classical Greek (Plath 1994), then one must explain the fact that the earliest evidence for the horse in Greece only dates to the Bronze Age, *c.* 3500 years *after* its occupation by our putative Proto-Greeks. To explain this by a later loanword is not particularly convincing since it requires us to believe that the Proto-Greek language remained essentially unchanged from the beginning of the Neolithic to the Bronze Age to accommodate the borrowing of the word for 'horse' in a way undetectable from all of the otherwise inherited items of the PIE vocabulary. The same would go for later technological items such as wheeled vehicles.

CONCLUSION

Linguistic-cultural analysis suggests broad dates for PIE running from *c.* 7000–2500 BC; the most recent items suggest *termini* for the separation of the IE stocks at around 4000–3000 BC but they do not necessarily reflect the earliest date for the actual expansion of PIE.

Non-conformity of disciplines

Attempts to date PIE result in broad bands of (unquantifiable) probability that generally fall between *c.* 7000 BC, the earliest appearance of the Neolithic economy reflected in the reconstructed lexicon, and *c.* 2000 BC, the dawn of the appearance of historically attested IE stocks in Anatolia. Even the more precise dating frameworks that include both 'informed estimate' and the cultural vocabulary, i.e. *c.* 5000–2500 BC, envisage a process of IE divergence over a period of 2,500 years and may allow for its continuance over many more centuries.

A prehistoric linguistic construct must by its very nature be temporally less precise and, if the borders of a language family are dynamic, geographically

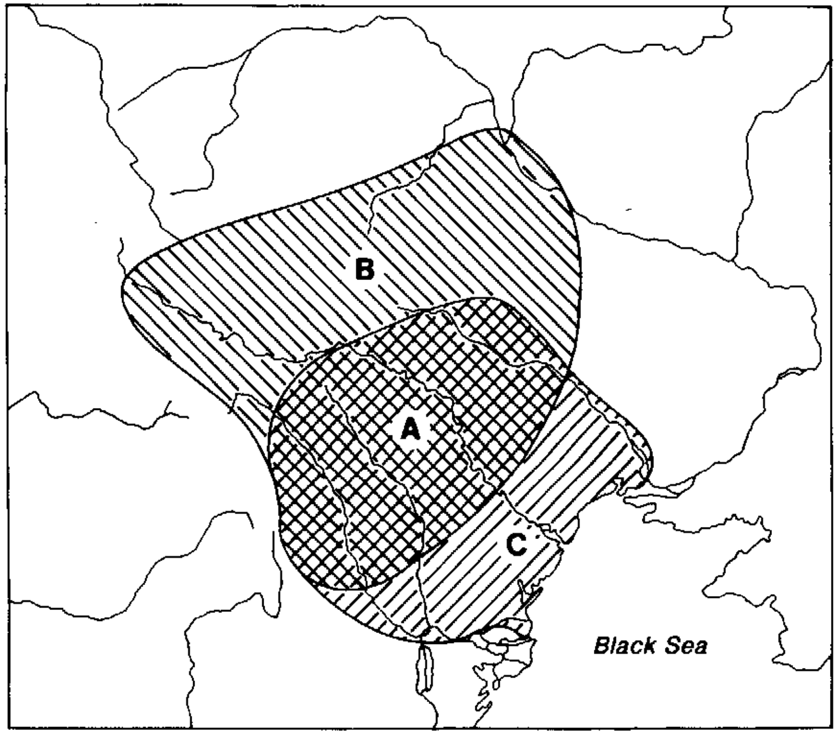

Figure 7.3 Non-conformity of disciplines: a hypothetical Indo-European homeland set to the region of the Tripolye culture. The area marked A comprises the earlier distribution which later expanded into B and finally included area C. Although temporal and spatial borders of the culture may be defined with some precision by archaeologists, there is no way that a linguist could determine at what stage the proto-language had begun to diverge; e.g. had palatalization of the velars begun in area A, or B or C or even later?, and how could one possibly know? Most solutions to the homeland problem involve even larger-scale entities with even greater imprecision.

less precise than an archaeological construct e.g. – the temporal band of a radiocarbon date at two standard deviations or the geographical distribution of archaeological culture(s). For example, even if we were somehow divinely inspired with the knowledge that the Tripolye culture of the north-west Black Sea region was '(Proto-) Indo-European', the nature of the archaeological entity does not accommodate a clear linguistic description through time (Figure 7.3). The culture existed for some 1500 years over a variety of periods and with dynamic cultural borders. There is no point in its existence (temporal or spatial) in which a linguist might confidently declare that the velars had not yet been palatalized, the feminine had been formed, or heteroclitics had ceased to be productive; i.e. there is no chronological or geographical slice

of this culture which might be declared pre-IE, proto-IE, or already differentiated IE. Thus, a solution expressed in terms of archaeological cultures with narrow dates occupying well-defined territories theoretically exceeds the precision that can be given for the temporal definition of Proto-Indo-European. The quest for the IE homeland in archaeology bears an awful similarity to seeking the day, month and year of the Renaissance or Industrial Revolution.

CONCLUSION

Even ignoring the problems of translating the PIE speech community into a phenomenon reflected in the archaeological record, no solution to the homeland problem can reflect with archaeological precision either the exact temporal or geographical borders of the IE homeland.

Linguistic relationship principle

A minimum requirement of any solution to the IE homeland problem must be congruence with the most basic linguistic relationships between the various IE stocks (i.e. major isoglosses). While there may be dispute over the internal relationships between some of the stocks, there is broad linguistic agreement (e.g. Adrados 1982; Hamp 1990b; Gamkrelidze and Ivanov 1995) that envisages the following relationships:

1 A separation of Anatolian from the other IE stocks.
2 A central area involving Greek, Armenian, Indo-Iranian.
3 A north(western) area involving Germanic, Baltic and Slavic in some late linguistic relationship.
4 A western area (perhaps) involving Celtic and Italic, which was perhaps more closely linked to 3 than 2.
5 The position of Tocharian is disputed but does not include an early close relationship with the Indo-Iranian superstock.

A viable solution to the IE homeland problem must somehow accommodate these relationships or, at least, not propose vectors of expansion that would obviously transgress such relationships.

Total distribution principle

A valid solution to the IE homeland problem must explain the distribution of *all* the IE languages. The nature of any 'solution' for one region of Eurasia has implications (chronological, contact) for others that cannot be ignored. Hence, an explanation for one region, no matter how satisfying, is valueless unless it is articulated with explanations for all IE-speaking regions.

While the principle is clear enough, its violation is so frequent that it demands some additional comment, especially as it often involves largely archaeological solutions to the homeland problem. For the early nineteenth century, it was regularly claimed (and demonstrated to the satisfaction of its proponents) that the Germanic peoples/languages had been, as Tacitus observed, autochthonous in northern Europe. Any archaeologists could then trace, through retrograde reconstruction, cultural continuity from the Nordic Iron Age cultures (Jastorf, etc.) back through the Late Bronze Age to the Early Bronze Age (no essential break in continuity) to the Later Neolithic (Corded Ware/Battle-axe horizon) to the Neolithic TRB and then Ertebølle and still earlier Mesolithic cultures. If the Germans could not be explained by migrations from the outside then they must have originated in northern Europe, and if the Indo-Europeans occupied a confined homeland, and we know that the Germans hadn't moved, then it must have been the rest of the IE-speaking peoples who had left a north European homeland. This argument of archaeological continuity could probably be supported for every IE-speaking region of Eurasia where any archaeologist can effortlessly pen such statements as 'while there may be some evidence for the diffusion of ideas, there is no evidence in the archaeological record for a population movement' or 'while admitting some minor population intrusions, these hardly suffice to explain a shift in language'.

The current penchant for demanding local continuity is by no means confined to proponents of European homelands as it now also appears to be a product of certain post-colonial Indian archaeologists and linguists who assert that Indo-Aryan has always been spoken in the north-west of the subcontinent and that all other IE peoples have migrated from there (Misra 1992; Deo and Kamanth, 1993, but see also the more balanced works of others, e.g. Sharma 1993, 1995). The ramifications of such theories are seldom if ever contemplated and after decisively demonstrating (to their satisfaction) that all Indo-Europeans originated in north-west India, the remaining IE stocks are left very much to their own devices (unaided by archaeological evidence or linguistic credibility) to find their own way to their historical seats. In any event, India has not been rejected as a potential homeland of the Indo-Europeans (*pace* Misra 1992: 41) only because 'it is a nice place to live and people would not move outside it'!

Non-IE periphery

The periphery of the IE world at its earliest reflection in the written record indicates the current or previous existence of non-IE languages, i.e. Iberia–south France (Tartessian and Iberian, Basque), central Italy (Etruscan, ?etc.), Crete (?), central and eastern Anatolia (Hattic, Hurrio-Urartian), Iran (Elamite), and India (Munda, Dravidian/Brahui). It is less likely (although not entirely impossible) that the IE homeland was situated within a territory occupied by non-IE languages at the time of the earliest written records.

ASSESSMENT PRINCIPLES

In assessing the plausibility (rather than validity) of any solution to the homeland problem, there should at least be a series of consistent principles employed.

Drawn from the above discussion, the following minimum criteria should be satisfied by any potential solution.

Temporal plausibility

The homeland should be identified at a time consistent with the broad parameters of linguistic estimation of time-depth and the evidence of lexico-cultural seriation – i.e. the Neolithic through Early Bronze Age. Any date later than the Early Bronze Age (i.e. later than *c.* 2500 BC), is too late to accommodate the degree of linguistic divergence that we encounter in the Later Bronze Age when the earliest IE stocks are recorded (Anatolian, Indo-Aryan, Mycenaean Greek). Any date earlier than the Neolithic requires us to reconstruct a PIE speech community lacking most of its culturally diagnostic vocabulary. That the Proto-Indo-Europeans had linguistic predecessors is self-evident, but the term 'Proto-Indo-European' must have chronological parameters like any other linguistic entity and can no more be applied to a population of speakers who predate the reconstructed IE lexicon than we can assign a Bronze Age date to 'proto-French'.

Exclusion principle

The homeland should lie in a territory not demonstrably non-IE at a time proximate to IE expansions; i.e. those territories of Europe or the Near East with non-IE populations from the earliest historical (Bronze Age) testimony are unlikely to have been the IE homeland. Obviously, it is just possible that already by the earliest written records IE-speaking populations may have been displaced by non-IE populations. But given the fact that we are attempting to describe the dispersion of a truly enormous language family and there are no apparent directional patterns of IE dialectology that might conform to a model of an external 'push', it seems unlikely that already by the earliest written records the Indo-Europeans had been displaced from their original area of distribution.

Relationship principle

Expansions from the homeland should conform with the broad interrelationships between the IE stocks; i.e. they should not require an entirely different series of movements to explain the major morphological and lexical isoglosses between the various IE stocks. Given that the inter-stock relationships within IE are not particularly precise, there is wide room for latitude here although some theories do seem to transgress any obvious relationships.

Total distribution principle

A homeland solution must provide a plausible case for the dispersion of *all* IE languages. While it will be readily admitted that there are many areas in which it is extremely difficult to date or trace the movements of IE-speakers into a particular region, there are also cases that can be made and arguments that need to be addressed. This principle will be applied to any theory in which major sections or stocks of the IE language family have been left to their own devices to explain their existence.

Archaeological plausibility

The archaeological expression of an IE homeland and dispersions should be supported by evidence which at least *might* be argued to reflect culturally similar, related or historically associated cultures or culture groups or the vectors by which language may have spread. While it must be granted that there is no direct correlation between archaeological culture and language group, the lack of a total correlation does not mean that no correlation exists at all (Clarke 1978: 302–5). While there have been some attempts to discuss the nature of relationships between both archaeology and migration, e.g. Rouse (1986), Anthony (1990), Kristiansen (1989), and archaeology and language (Mallory 1992), none of these have yet created the sought for 'middle range theory' that might tie hard archaeological evidence to the movement of linguistic entities. Nevertheless, a solution to the IE problem that explains the dispersion of all the various IE stocks must rest on something stronger than its proponent's ability to wield a felt-tip pen. In short, there must be some supporting archaeological evidence for the arrows drawn on a map that suggest the course of IE dispersals.

INDO-EUROPEAN HOMELANDS

Although there is a wide range of solutions to the IE homeland problem, current discussion is largely confined to about four basic models, albeit with many variations (Figure 7.4). These are arranged here in chronological order. In presenting each case, an attempt has been made to indicate why such a theory is attractive, critically evaluate it according to the criteria laid down above, and, where possible, suggest modifications that might enhance the theory's credibility.

Model 1: Baltic-Pontic(-Caspian)

TIME: MESOLITHIC (e.g. Killian 1983; Häusler 1985)

This solution embraces two regions which later exhibit population movements and/or widespread cultural homogeneity that may be associated with linguistic entities ancestral to many of the IE stocks. The north European component of this solution embraces a presumed continuum that runs from the TRB Culture through the Globular Amphora and Corded Ware cultures of the Late Neolithic to the Bronze Age sequence of Unetice > Tumulus > Urnfield. This comprises a vast area of northern and central Europe which can underlie the later emergence of Celts, Germans, Balts and Slavs; possibly also Italic and Illyrians. The eastern territory of this solution also includes the steppe and forest-steppe regions of the Pontic-Caspian – i.e. the Sredni

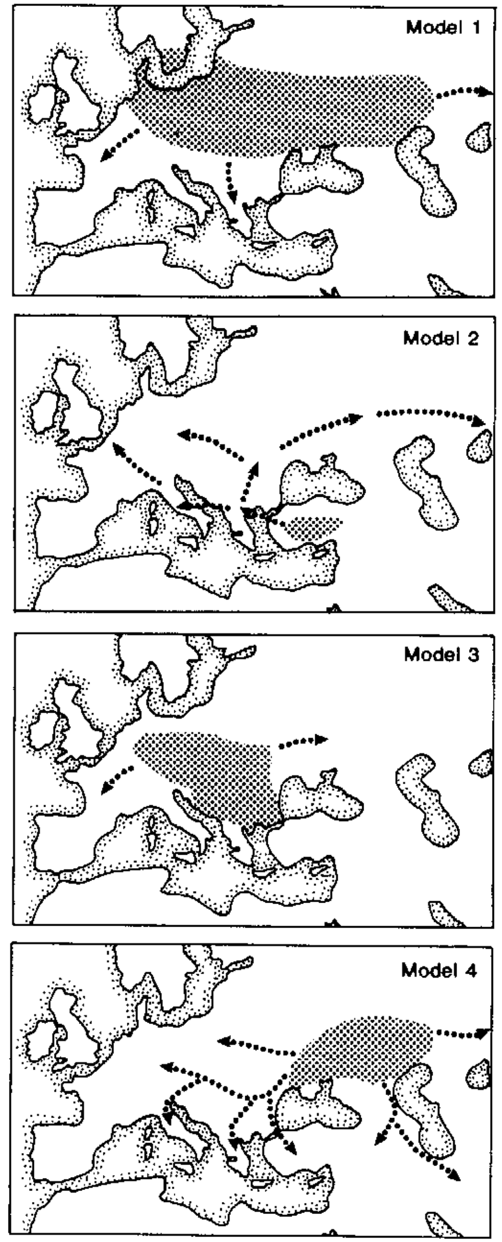

Figure 7.4 Four main models of the homeland and dispersion of the Indo-European languages.

Stog–Khvalynsk–Botai cultures with the later Yamna, Catacomb/Poltavka, and, by the Later Bronze Age, the Srubna and Andronovo cultures which would involve expansions eastwards from the European steppe through Kazakhstan and on into central Asia and beyond. Developments in this region could be seen to underlie the later Indo-Iranians, possibly the Thracians, Dacians, Tocharians, and, if Eneolithic and Early Bronze Age expansion from the steppe into SE Europe/Anatolia is allowed, then Anatolians, Phrygians, Armenians, and Greeks might be arrayed in this solution. This model provides a convenient homeland for those who argue either genetic or early contact relations between Proto-Indo-Europeans and the Uralic language family (and North Caucasian).

EVALUATION

1 The time-depth of the solution is usually taken to represent the Mesolithic and it is nearly impossible to envisage why such populations, stretching from the Baltic to the Black Sea or Caspian, might share at this time a common vocabulary for domestic plants, animals and items of late Neolithic/Early Bronze Age technology such as wheeled vehicles, or gain this vocabulary without evidence of borrowings, thousands of years after the Mesolithic period.
2 The territory was not demonstrably occupied by non-IE populations at any time although there is a considerable literature devoted to non-IE substrate lexical items from northern and central Europe (e.g. Hamp 1990b; Huld 1990; Polomé 1990).
3 Expansions from the general area indicated can be seen to accommodate most linguistic models of the dialectal relationships between the IE stocks; i.e. one may at least propose a series of movements centrifugal from this region which would not be incongruent with the major dialect groupings of the IE stocks.
4 This solution provides a convenient ancestry to the IE languages of much of Europe and those of Asia since it requires minimum movements of population from areas where IE-speaking populations are later encountered. However, its explanation of IE stocks in the Balkans, Greece and Anatolia requires it to subsume elements of the Pontic-Caspian solution (see pp. 112–13).
5 Archaeologically, the homeland area is an artificial construct and consists of at least two distinct cultural regions from the Mesolithic period onwards; i.e. there is no underlying cultural reason why one might impute the same language to Mesolithic populations from the Baltic to the Caspian as there is no reason to suggest a major interaction sphere in this region, similarity of physical type, or any other phenomena which might lead one to conclude that we are dealing with a common linguistic region.

MODIFICATION

The time-depth of this solution may be altered to the Neolithic which mitigates the criticism stemming from the first criterion. On the other hand, the territories of central Europe and those of the steppe region are so culturally distinct (and agreed so even by the supporters of this solution) that it still seems to argue against a common linguistic basis. Indeed, a modification to the Neolithic raises almost as many problems as it resolves. It is, for example, difficult to separate the origins of the Neolithic in Danubian Europe from Neolithic cultures of south-eastern Europe and, by extension, Anatolia and western Asia, while altering the time-depth of this solution to a later period transforms it largely into the Model 2 solution (see next section). Resistance to moving the dates of PIE to the Neolithic are necessitated by the recognition (among supporters of this particular solution) that the steppe-region is culturally distinct, and it is extremely difficult to build any archaeological model that supports strong cultural links between central Europe (or the Balkans) and the steppe region either at the initiation of or during the Neolithic.

Model 2: Anatolia

TIME: EARLY NEOLITHIC, *c.* 7000–6000 BC (Renfrew 1987; Safronov 1989; Cavalli-Sforza *et al.* 1994)

This solution provides IE expansions with a substantial vector in demic diffusion (i.e. a 'wave of advance'), that carried speakers of IE over a vast area of Europe from an area (Anatolia) where IE speakers are later found in the Bronze Age. The mechanism for expansion is seen in the spread of a new subsistence economy based on domestic plants and animals. This would have stimulated population growth such that random budding off would have resulted in the expansion of people from Anatolia across Europe and Asia and provided the immigrant farmers with selective advantages whereby they would have assimilated local populations of hunter-gatherers. The solution also unites populations from the Balkans (and probably Danubian Europe) and Anatolia which, in the Neolithic, are both physically and culturally related (Schwidetsky and Rosing 1990), and which could argue for a common linguistic area. The solution provides a convenient geographical relationship for those who suggest extra-linguistic connections between Indo-European, Semitic and Kartvelian.

EVALUATION

1 The solution requires IE expansions from the beginning of the Neolithic and postulates the existence of a series of IE-speaking cultures across Eurasia in areas which, *at the time postulated*, do not reflect temporally critical items of the reconstructed PIE vocabulary. For example, the same word for horse is attested in almost all IE stocks (from Ireland to Chinese Turkestan) and yet the animal is not known in Anatolia

until the 4th millennium BC (and *pace* Dolgopolsky 1993: 240, the Luvian and Lycian cognates of the PIE word for horse are as likely to be inherited as borrowed from an Indo-Iranian language) or Greece until the Bronze Age (and can only be explained away by presuming long-distant acquaintance with the horse). Even more serious are items such as wheeled vehicles which have no archaeological expression at such an early date yet are attested in the inherited PIE vocabulary of Anatolian. Linguistic diffusion at *c.* 7000 BC is considerably earlier than is usually accepted by linguists on their admittedly intuitive estimations of PIE time-depth.

2 With the emergence of the earliest written records an Anatolian homeland falls either within or on the immediate periphery of non-IE populations; i.e. the Hatti, whose territory the Hittites occupied, or the Hurrians of east Anatolia. Both languages are radically different from IE and provide no grounds whatsoever for suggesting that they developed together among related dialects at the beginning of the Neolithic. Anatolian scholars (e.g. Singer 1981; Steiner 1981; Puhvel 1994), on both linguistic grounds and the self-testimony of Hittite documents, regard the Hittites as intruders into their territory and not autochthons. On the other hand, linguistic records are 'mute' for prehistoric western Anatolia where one might prefer to anchor the PIE homeland.

3 The course of Neolithic expansion proposed, for example, by Colin Renfrew, violates the interrelationships of the IE languages, i.e. the proposed population movements are not always congruent with the isoglosses within the IE family and it proposes historical connections and lines of development contradicted by the linguistic evidence; e.g. close relationships between Anatolian and Greek, between Greek and Italic, etc. Other configurations involving a secondary IE homeland for the non-Anatolian stocks in south-east Europe, e.g. Dolgopolsky (1988, 1993), can be made to accommodate better the dialectal relations of the different IE stocks.

4 The theory provides a plausible archaeological vector for the Europeans but it has serious problems explaining the IE-speakers of Asia. Renfrew's Plan A, for example, derives them from the Near East at the spread of the Neolithic, against the fact that between his 'homeland' and the historical seats of the Indo-Iranians are historically attested non-IE populations (Elamites, Sumerians, Dravidians) who demonstrably or circumstantially should have preceded the movement of Indo-Europeans. His Plan B requires a demic diffusion from the Balkans across the Pontic-Caspian steppe which is regarded by many (supporters of the Model 1 and Model 4 theories) as contradicted by the archaeological evidence that suggests two very different cultural and physical worlds with no demic diffusion (or if there is, it was moving from east to west).

5 Archaeological plausibility is tied to the vector of demic diffusion which is certainly 'arguable' for large parts of Europe and, if acculturation be allowed, for most of Europe. To this have been added genetic arguments where an analysis of the first principle component involving the constellation of ninety-five gene frequencies of modern European populations suggests a movement from south-west Asia towards Atlantic Europe (Cavalli-Sforza *et al.* 1994: 291). What is not necessarily demonstrated is that this represents a movement coincident with the origins and dispersal of agriculture rather than something else, e.g. the spread of modern human populations through south-west Asia from an African 'Eve'. Moreover, it is questionable whether such genetic maps actually provide independent support for archaeological migrations when the map prepared from the second principal component would, in the absence of negative archaeological evidence, support a migration from Finland to Iberia (or the reverse)! Evaluation of the genetic map against the patterns of migrations and linguistic affinities proposed for this theory have been challenged by statistical analysis (Sokal *et al.* 1992).

MODIFICATION

The theory can be (and has been) modified (Zvelebil and Zvelebil 1988, 1990; Sherratt and Sherratt 1988) which can ameliorate some of the objections. If demic diffusion be limited to western Anatolia, the Balkans and Danubian Europe, and this be regarded as a linguistically interactive zone from the 7th to the 4th millennium BC, then it may be possible to envisage the dissemination of cultural terms within this region that are reconstructed to the proto-language. The homeland territory, if placed further west in Anatolia (where the archaeological evidence is admittedly quite sparse), helps obviate the need to derive the earliest attested Indo-Europeans from a region occupied by non-IE peoples (e.g. Hatti or Hurrian territory). If the theory avoids attempting to explain the spread of all IE stocks in the Neolithic but allows for their spread later in the Bronze and Iron Ages, then it permits different trajectories for the distribution of the IE stocks that may accommodate their linguistic interrelationships; e.g. Celtic and Italic could then derive from Danubian Europe and not Greece > Italy > France. The revised model might also include the establishment of a sphere of interaction in the north Pontic to avoid the difficulties of deriving the Eurasian steppe Neolithic/Eneolithic solely from (unattested) demic diffusion from the Balkans (Sherratt and Sherratt 1988).

Model 3: Central Europe–Balkans

TIME: EARLY NEOLITHIC *c.* 5000 BC (Gornung 1964; Diakonoff 1985)

A homeland in central Europe to the Balkans is situated in a region which would satisfy those linguists who adhere to the use of the centre of gravity principle. It also provides a reasonably close contact zone between IE and the Uralic or North Caucasian languages suggested by some linguistic models. From an archaeological standpoint, this is the territory of the *Linearbandkeramik* which occupied a broad area of Europe, Atlantic to the Ukraine, and shows remarkable uniformity which is suggestive (at least to some) of a common linguistic horizon, e.g. Bosch-Gimpera (1960), Devoto (1962), Makkay (1991, 1992). Later developments within the Danubian and Balkan regions see the emergence of all the later cultural elements ascribed to PIE by the 4th millennium BC.

EVALUATION

1 The time perspective of this solution can generally accommodate most temporal models of PIE.
2 The proposed homeland solution is devoid of any evidence other than IE-speakers from the earliest historical records. But, as with the Model 1 solution, arguments for non-IE substrates exist also for central Europe and the Balkans.
3 An expansion from this region can be made to conform with the dialectal relations of the IE stocks.
4 The model is critically deficient in explaining the IE stocks of Asia and in general does not satisfactorily project IE-speakers east of the Dnieper unless it adopts elements of the Model 1 solution which, consequently, then fail the test of chronology and archaeological plausibility.
5 The archaeological evidence can be pressed to service over much of northern and western Europe but it is not easy (although not impossible) to support an expansion of Danubian or Balkan cultures either into Anatolia or across the steppe into the staging areas – much less the historical seats of the Indo-Iranian and Tocharians. Inherent in this solution is also a rejection of the Model 2 solution which is problematic since it excludes that which the Anatolian solution supports most strongly, i.e. cultural (and linguistic) links between Anatolia and the Balkans. It must be admitted, however, that this model has a great many variations among which many would support the model of the *Linearbandkeramik* culture as an autochthonous development in the Neolithic, ethnically and linguistically unrelated to the neolithic cultures of south-east Europe although influenced by them with respect to subsistence agriculture and technology.

Model 4: Pontic-Caspian

TIME: ENEOLITHIC *c.* 4500–3000 BC (Gimbutas 1977, 1980, 1991; Anthony 1991)

The solution accommodates the widespread belief that the earliest IE-speakers were primarily mobile pastoralists rather than settled agriculturalists, and the location satisfies those who believe that the closest extrafamilial links with

PIE are to be found in Uralic and North Caucasian. The Pontic-Caspian steppe and forest steppe region produces a number of broad cultural entities which may be seen as interaction spheres within a single language family, and there is archaeological evidence of expansion from this region both westwards into the Balkans and eastwards into Kazakhstan. The vector for the initial expansion is seen in a highly mobile economic strategy with a social organization capable of absorbing and dominating various different ethnolinguistic groups.

EVALUATION

1 The time-depth for a PIE homeland in the Eneolithic (i.e. 5th and 4th millennia BC), has no difficulties in satisfying the general expectations of linguists for PIE time-depth nor for accounting for the lexico-cultural elements ascribed to PIE – e.g. wheeled vehicles, horse (riding), secondary products, etc.
2 The area, only attested in historical records since the Iron Age, has never been shown to be demonstrably non-IE in the prehistoric period, although it has seen more recent intrusions from Turkic-speaking peoples.
3 Centrifugal expansion of IE from a Pontic-Caspian centre can accommodate the basic interrelationships of the IE stocks and has been accepted by a number of linguists (e.g. Adrados 1982).
4 The model proposes to explain the later historical position of all IE languages, both those in Europe and Asia, i.e. this solution 'bridges' the cultural (Dniester–Dnieper) gap envisaged in the Model 1 and Model 3 solutions and not convincingly resolved in Model 2 (see pp. 106–12).
5 There is solid archaeological evidence for population movements from the steppe region but only as far west as the Tisza river and the Balkans. All further expansions, either to the west or the north (i.e. the presumed staging areas for Celtic, Germanic, Baltic and perhaps Slavic), are founded on evidence which is frequently dismissed as too generic to compel belief (e.g. spread of domestic horse, wheeled vehicles, defensive architecture, tumulus burial, battle-axes, animal burial, etc.). The evidence for expansions southwards into Anatolia is similarly questionable. While a good case can be made for an expansion of Pontic-Caspian pastoralists onto the Asiatic steppe, and perhaps also into the belt of central Asian urban centres (Parpola 1988), it is still difficult to demonstrate movements from the steppe into the historical seats of the Indo-Aryans and the Iranians of Iran itself (Mallory forthcoming b).

CONCLUSIONS

The temptation to conclude with the last solution as the most probable must be resisted and, in the limitations of this chapter, it is perhaps advantageous to set aside all non-archaeological aspects of the problem as peripheral and focus on one of the key archaeological elements of any solution. The various archaeological models proposed here must each confront a common problem which Harold Peake and Herbert Fleure, perhaps simplistically but still neatly, described as the difference between the 'steppe and the sown'. This dichotomy is probably overdrawn (Mallory forthcoming c) but there is, nevertheless, widespread recognition that the cultural trajectories of the European Neolithic and Copper Age cultures and those of the Pontic-Caspian region would appear to be extremely different. Yet by the end of the Neolithic or during the Bronze Age, both regions must be accommodated within a single covering model that explains why they should both belong to the same language family and be ancestral to most of the known IE stocks. A cultural trip line or watershed is then proposed, somewhere between the Dniester and the Dnieper rivers (cf. Kosko 1991), which must somehow be bridged (Figure 7.5). Two

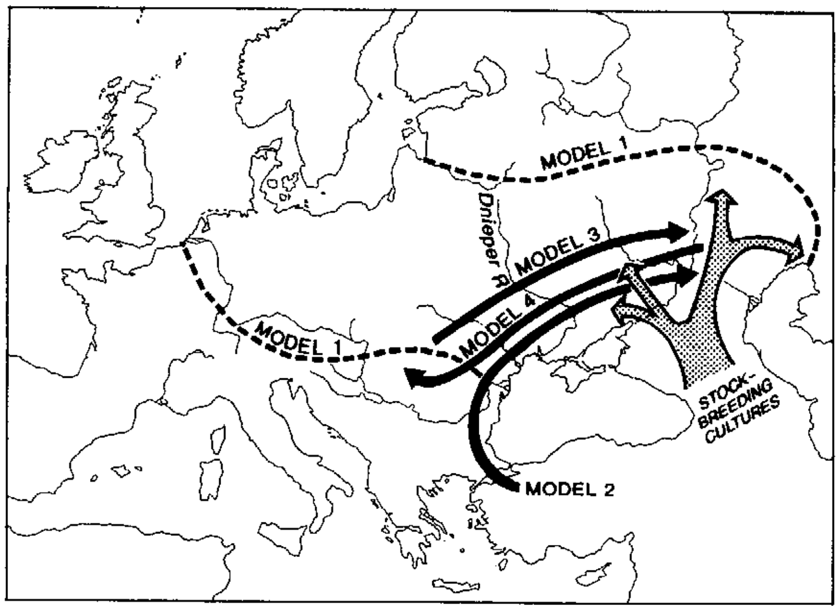

Figure 7.5 The Dnieper 'border'. Model 1 embraces both sides of the Dnieper border, while Models 2 and 3 require population movements in a west–east direction associated with the spread of agriculture from Anatolia through the Balkans; Model 4 suggests that the movement went from east to west and finds a more convenient source of the neolithic economy in the Caucasus.

of the solutions, the Anatolian (Model 2) and Central European/Balkan (Model 3) attempt to bridge from the west; the Pontic-Caspian (Model 4) attempts to bridge from the east, and supporters of the Baltic-Pontic solution (Model 1), finding the pitch already too cluttered with opposed cultural trajectories in the Neolithic, move the goal posts back to the Mesolithic. None of these solutions is entirely convincing, and I would emphasize that I do not personally think that the Pontic-Caspian solution is the best solution but merely the 'least bad' solution. But preferences aside, I would like to conclude with a look at some of the critical problems involved.

If we examine the various theories in reverse chronological order, then the Pontic-Caspian solution marks the most recent 'breach' of the Dniester–Dnieper barrier. This 'breach' occurs by *c.* 4000 BC (Manzura *et al.* 1995), and it should be emphasized that the days when one envisaged purely a movement of Yamna tribes to the west – i.e. a phenomenon of the 3rd or late 2nd millennium BC (e.g. Alekshin 1994) – are long since passed. That there was population movement into the Balkans seems difficult to fault unless one wishes to deny all evidence of migration in the archaeological record; i.e. it comes fairly close to the same type of evidence we would employ to identify Anglo-Saxon burials in Britain. But such evidence is only solid as far as the Tisza river (Ecsedy 1979), and beyond that the entire construct of 'Kurgan invasions' is far more problematic and insecure (Mallory 1989: 243–57). What is most disturbing perhaps is that the evidence of the steppe-type burials occurs generally in those environments similar to that encountered on the steppe and, consequently, suggests that there were ecological limits to such intrusions. Moreover, the movement of steppe populations into the rest of Europe is also historically attested, most visibly in the migrations and settlement of Sarmatian tribes during the Iron Age, but these most certainly did not lead to the permanent extension of the Iranian language into areas beyond the Pontic steppe – much less an Iranian domination of eastern Europe.

If Anthony (1990) is correct in seeing the adaptation of horse-riding not only as a stimulus to expansion but also one that may have definite environmental limitations, then we can see all too clearly that the so-called Kurgan solution does not do enough to resolve the problem of origins for most of the IE stocks beyond the Balkans. But this weakness applies only if one presumes that the Pontic-Caspian solution relies entirely on a mobile horse-riding model. The origins of cultures such as the Globular Amphora, initially derived directly from the steppe in the early writings of Marija Gimbutas, are now sought elsewhere. Polish archaeologists are increasingly coming to accept a local (Lengyel) origin for the Globular Amphora (e.g. Cofta-Broniewska 1991). Now such a model is not incongruent with the latter writings of the late Marija Gimbutas who argued for a much more diffuse spread of 'Kurgan traits' among a variety of east and central European Late Neolithic cultures. Her most recent models (e.g. Gimbutas 1991) had moved considerably from the simple concept of a wave of horse-riding warriors, a fact that her critics often appear to forget. But her models moved into an area of more subtle cultural interrelationships, a 'cumulative kurganization' to steal a phrase from Christopher Hawkes, which will require both far greater evidence and argument to be properly discussed or dismissed.

Two of the other theories attempt to bridge the Dniester–Dnieper gap from the west. Of the two, it is most difficult to credit the *Linearbandkeramik* solution since the number of attempts to propose cultural movements during the Middle or Later Neolithic from the west on to the steppe are few indeed (e.g. Klein (1963) and Shevchenko (1986)), and little regarded. Even if we should see the Cucuteni-Tripolye culture as an eastern extension of the *Linearbandkeramik* under the influence of Balkan cultures, we can track its settlements spatially only as far as the Middle Dnieper and, worse, temporally only as *synchronous with* rather than *ancestral to* the Pontic-Caspian cultures that emerged many hundreds of kilometres to its east. If the Tisza marks the western border of Pontic-Caspian incursions, so the Dnieper marks the eastern border of central European or Balkan movements.

Can the Dniester–Dnieper gap be bridged during the inception of the Neolithic? This has been presumed on occasion, but in reality the case for demic expansion ends with the Tripolye culture, and it is exceedingly difficult to make a solid case for a population movement at the beginning of the Neolithic crossing the Pontic-Caspian region and then moving into Kazakhstan by the 4th or 3rd millennium BC. Even the origin of domestic livestock and plants in the Pontic-Caspian region appears to have had staging areas different from south-east Europe, and both central Asia (Merpert 1974; Matyushin 1986) and (more plausibly), the Caucasus (Shnirelman 1992) have been proposed. Given that mixed agriculture in the Caucasus was as early as that in the Balkans, and more proximate to the earliest neolithic sites in the lower Don region, it would provide a source for a strain of domestic sheep most similar to that of the Pontic-Caspian, and is thus clearly the preferred hypothesis.

If a case for linguistic expansion from the Balkans across the steppe and forest-steppe is to be sustained, it is more likely to hang on some form of elite dominance. In considering the possible evidence, one could point to the circulation of Tripolye wares and figurines in Sredny Stog burials (Telegin 1973) or, more extensively, the movement of copper and copper artefacts of Balkan provenance across the steppe as far as the Volga (Chernykh 1992: 42–8). But even here it is difficult to warrant these as a mechanism for prestige social relationships between Balkan traders(?) and local Pontic-Caspian populations. The very mechanism of these contacts has been credited to the highly mobile steppe populations who themselves produced this interaction sphere by pushing into the territory of the west (Chernykh 1992: 44). Moreover, there is at least some evidence that the steppe and forest-steppe populations worked the imported copper themselves and founded their own metallurgical tradition. The other possible prestige metal – silver – also circulated through this interaction sphere in the 4th and 3rd millennia (Mallory and Huld 1984; Jovanovic 1994), but here again we are hardly talking about

the expansion of a prestige Balkan dialect across the steppe. The most obvious Near Eastern impact on the Pontic-Caspian region is evident in the Maykop culture and, given the location of the Maykop remains, we might better expect the expansion of north-west Caucasian than Indo-European into the steppe region. In short, putative IE homelands set to the west of the Dniester–Dnieper border are even less likely to produce a model of cultural much less linguistic transgression across this border than those who place the homeland on the steppe and forest-steppe.

The attraction of somehow combining at least two-thirds of the disparate geographical elements of the homeland puzzle in the Baltic-Pontic solution (Model 1) are obvious since such a homeland pre-empts any necessity for devising demic or elite movements crossing the Dniester–Dnieper border, as this region sits in the middle of the homeland territory. But other than by an act of cartographic fiat, what is there to justify drawing a circle around the various cultures from the Baltic to the Pontic or Caspian and declaring them all to be Indo-European? Not only is there no common Mesolithic tradition or techno-complex underlying this entire area but even appeals to similarities in physical anthropology, the so-called robust proto-Europoids, have been dismissed with reference to the Dnieper–Donets region (Jacobs 1993). The reconstructed cultural vocabulary of PIE makes it clear that if this solution is to be validated it must show how central and northern Europe, as well as the Pontic-Caspian, shared the same inception of the Neolithic economy and subsequent technological developments. As this is the very stumbling block of the other solutions, it can hardly be applied here.

In sum then, we have different sub-regions of an early IE world, scattered in space from the Baltic to Anatolia and east across the European steppe, and set in time somewhere between what we would recognize as PIE unity and the emergence of the major IE stocks. To unite these disparate geographical elements together into a single 'unified field theory' seems to be as distant to those seeking such a goal in Indo-European studies as it is for physicists.

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