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Archaeology & Language: The Puzzle of Indo-European Origins. By Colin Renfrew. New York, NY: Cambridge University Press, 1987. Pp. xiv, 346.

Reviewed by John C. Kerns, *Huber Heights, Ohio*

Although a number of reviews of Renfrew's book have already appeared, and are still doing so, some aspects of the problem of Indo-European origins seem to have escaped the notice of reviewers and even of Renfrew himself. These have to do mainly with the timing of significant developments in Indo-European prehistory. In this discussion special attention will be applied to this matter.

Renfrew's book has 290 pages of text and further sections consisting of notes, a bibliography and an index.

It happens that most of Renfrew's views on Indo-European prehistory are similar to those I have held for many years, and largely for the same reasons; accordingly, it will not be a matter for surprise that these remarks will be mostly laudatory.

In addition, there are important linguistic considerations, mostly ignored by Indo-Europeanists for the past quarter century, which likewise imply conclusions similar to most of those drawn by Renfrew. A major exception, however, is his suggestion that the earliest European speakers of Indo-European entered Greece and the Balkans from Anatolia as bearers of the first agricultural techniques to be practiced in Europe; here the linguistic considerations are against him. This will be discussed below.

In this discussion use will be made of 'IE' as short for Indo-European in general, including Anatolian, and 'IE-Proper' to subsume all the historically observable branches of IE *except* Anatolian.

In his Preface, "What Song the Sirens Sang" (1-8), Renfrew gives a preview of his intellectual approach, stating that both the archeological and the linguistic studies for the historically known post-IE speaking areas, in Europe and nearby parts of Asia, have matured to the point that some major fallacies in the presently prevailing reconstruction of IE prehistory can now be exposed. Consequently, the entire problem must be readdressed with a new methodology based on newly developed assumptions. This is indeed correct, for it has long been clear that the principal fallacy has been the common assumption that the Indo-Europeans were already in possession of domesticated horses at the time of their first entry into non-Russian Europe. This assumption is entirely unwarranted yet has seldom been questioned, and it is the cause of the collapsed chronology and consequent confusion that have constantly plagued IE prehistorical studies, resulting in a number of linguistically observable anachronisms. In history or prehistory, which is really what we are dealing with here, anachronisms and all other contradictions must not be tolerated; to resolve them, adjustments or outright replacements of our assumptions must be made as needed. This is

the essence of science, for anything less than this is a devolution to ideology whereby mistaken notions are perpetuated indefinitely.

In his first two chapters (9-19, 20-41) Renfrew summarizes the main linguistic and archeological works that have been brought to bear on the question of Indo-European origins. He mentions suggestions by Otto Schrader (1890), Gustav Kossinna (1902), V. Gordon Childe (1915 to 1958), Marija Gimbutas (1956 to 1979), and Pedro Bosch-Gimpera (1940 to 1961). Of these, Schrader, Childe and Gimbutas have favored a South Russian homeland, while Kossinna suggested a North-Central European origin. On the other hand, Bosch-Gimpera has favored a Central European origin dating from the earliest Neolithic times; this view is closest to those held by Renfrew and me. Special attention should be paid to Renfrew's concluding remarks (41), to the effect that while these studies involved thorough investigations, they all have been based on assumptions dating from the time of Kossinna and Schrader. These assumptions need to be reexamined, and if they prove to be unjustified (as they do), the conclusions based on them are likely to be mostly wrong.

In his third chapter, "Lost Languages and Forgotten Scripts" (42-74), Renfrew presents a survey of the history of Indo-European languages and scripts as elucidated by scholarship during the past two centuries. Purists and specialists can quibble on one point or another, and should; for example, I consider his Table XI (74) to be in error in some details. But in view of the complication of such multifaceted phenomena, it is hard to see how a better survey could be provided within a single chapter.

In his fifth chapter, "Language and Language Change" (99-119), Renfrew provides a concisely stated review of work done by linguists, mostly Indo-Europeanists, with special reference to the last quarter century. He discusses such matters as phonemic correspondences, the family tree model, Schmidt's Wave Theory, loan words and vocabulary invention, semantic change, word order (whether VO or OV), glottochronology and lexicostatistics, and he briefly comments on each of these. In general, his conclusions are in accordance with views held by most Indo-Europeanists, who will find them conventional. But for anyone else, this chapter is a condensed educational gem.

These two chapters seem to have been provided mainly for the benefit of archaeologists, who may be expected to be inexperienced in Indo-European linguistics. They serve the purpose well.

Chapter four, "Homelands in Question" (75-98), presents Renfrew's arguments against a South Russian homeland for the Indo-Europeans. He raises three points. First, appeals to the protolexicon for evidence of a specific place of origin and mode of life have proved to be unreliable.

Second, such widely spread (and relatively late) archeological phenomena as *Bell Beaker* and *Corded Ware* are increasingly recognized as due far more to local cultural developments than to invasions. While it is true that the wide geographical uniformity of these phenomena suggests far-reaching cultural influences, there is no reason to suppose that it was due to anything more than intertribal trading activities or visits by prominent individuals, including technical specialists. Under the circumstances, it is highly probable that the linguistic effects were superficial, consisting mainly in the borrowing of vocabulary across tribal lines with only minor displacement of the languages themselves; certainly this

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is overwhelmingly implied by the extant linguistic phenomena, at least in the Central, North, and West-Central parts of Europe -- the Eastern edge of non-Russian Europe, and nearby parts of Russia and Asia, evince late linguistic displacements on an extensive scale, mostly West-to-East and North-to-South.

Third, the facile assumption that pastoral nomads or mounted warriors from Southern Russia were preadapted for economic exploitation of Central and nearby parts of Europe, with its relatively hilly and more densely forested terrain, has been thoroughly discredited. Furthermore, the pastoral-nomadic way of life itself is dependent on continuous agricultural activity to sustain it, and indeed could not have evolved except from an agricultural society. In Southern Russia this society was clearly the *Tripolye* culture, linked by the *Cucuteni* culture to earlier *Linear Pottery* settlements on the upper Dniester in a process of adaptation toward steppe conditions. Under the circumstances, it is highly probable that the language of the Kurgan people was indeed Indo-European, but its origin was in the *Linear Pottery* culture of Central Europe, not in Southern Russia or Central Asia.

I fully agree with Renfrew on these points.

In the sixth chapter, "Language, Population and Social Organization: A Processual Approach" (120-144), Renfrew introduces a word which is new to me. The *American Heritage Dictionary* (Houghton Mifflin, 1980) does not list "processual", and in *Webster's New Universal Unabridged Dictionary* (Simon and Schuster, 1979) the meaning is given as "in law, relating to some legal proceeding or judicial process", apparently in reference to some formalized legal procedure recognized as being especially applicable in a particular legal situation. It appears that archeologists have recently adopted the word to refer to intellectual models of special social processes which are believed to have been significant in the development of certain archaeologically observable phenomena, with particular reference to the possibility of using some of these phenomena as plausible indicators of linguistic replacement in prehistoric societies.

Thus defined, processual models seem to be a generalization of a more restricted class, mathematical models, familiar to physicists and engineers. Anyone who has devised such a model is aware that it always embodies simplifying assumptions needed to make it feasible to draw conclusions from it. Consequently, no matter how rigidly these conclusions may have been implied by the model, the validity of the results as a solution to a problem in the real world is always in doubt. In the case of the more general class of processual models the doubt is even greater. Renfrew is aware of this.

Under the heading of linguistic change within a given area, Renfrew cites three situations: (1) Initial colonization of the area by human beings and their language, (2) Replacement of the current language by another brought into the area by settlers or invaders from outside, and (3) Evolution of the language into new dialects and these eventually into mutually related but separate languages. He recognizes that at least the two latter processes were operative at one time or another in the course of Indo-European prehistory. Although the area in question was not named, the region in and around Central Europe was very likely uppermost in his mind.

Renfrew then focuses on the second of these, linguistic replacement. He discusses several processual models, as follows:

1) *Demography/Subsistence.* Under this heading he includes the *Wave of Advance Model.*
2) *Elite Dominance.*
3) *System Collapse.*
4) *Constrained Population Displacement.*
5) *Sedentary/Mobile Boundary Shift.*
6) *Donor/Recipient Population Systems.*

Renfrew presents historical examples for each of these. For me, the *Wave of Advance Model* is the one which holds greatest interest because of its applicability to the *Linear Pottery* expansion, which quickly established primitive agriculture and minor pastoralism in Central and most of Northern Europe from Eastern Holland to the Western Ukraine. With a potential relative population density of some 50 to 1 over the earlier Mesolithic population, it is highly plausible that the languages of the earlier peoples were swamped by the effect of sheer numbers. This does not mean that the Mesolithic population necessarily became extinct. On the contrary, the survival of Nordic populations in Northern Europe strongly suggests that their biological ancestors willingly adopted the *Linear Pottery* culture, including its IE-Proper language, and actively participated in its penetration into their territory. Of course, this implies that the earlier languages of the Nordic peoples were not Indo-European since IE-Proper seems to have been the only dialect of IE encountered by these peoples, though their languages may possibly have been related to IE as members of some other Nostratic family, now extinct. Unfortunately, there appears to be no way we can verify this.

Thus far, Renfrew has concentrated on demonstrating the inadequacy of previous solutions to the problem of Indo-European origins. I find his arguments convincing, and as stated above, I have further arguments of my own based on linguistic considerations which lead to the same conclusions.

In his remaining chapters Renfrew offers his own suggestions for a solution, dealing with the following topics identified by chapter number and title, as follows:

7) "Early Language Dispersals in Europe" (145-177).
8) "The Early Indo-Iranian Languages and their Origins" (178-210).
9) "Ethnogenesis: Who were the Celts?" (211-249).
10) "Indo-European Mythologies" (250-262).
11) "Archaeology and Indo-European Origins: An Assessment" (263-290).

These are grouped together since they unavoidably contain much that is speculative. To discuss them in detail would be tiresome and space consuming, and my own conclusions differ from Renfrew's in some respects. In brief, his suggestions can be summarized as follows:

First, he concurs with most Indo-Europeanists in rejecting Trubetskoy's suggestion to the effect that the similarities among the conventionally recognized members of IE are due to a sustained process of convergence among languages which initially were mutually

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alien. Accordingly, Renfrew concludes that the family tree model must be acknowledged as having considerable validity for the deployment of IE in Europe, at least in its earlier stages. I agree with Renfrew, regarding Trubetskoy's suggestion as remarkably obtuse in view of the fact that although some convergences have undoubtedly occurred, they are outweighed by the many radical divergences. Furthermore, most of the resemblances are in precisely those features that have been observed to be least subject to change.

Second, Renfrew equates the Indo-Europeanization of non-Russian Europe with the introduction and spread of primitive agriculture (and minor pastoralism) into the Balkans and through Central Europe; thus he regards the *Linear Pottery* expansion as a Northern extension of the *Starchevo-Koeroes-Karanovo* (here *SKK*) culture, and the *Funnel Beaker* and the *Tripolye* cultures as later Northern and Eastern extensions of *Linear Pottery*. Again I agree, partly for the archeological reasons he cites, but also in view of important linguistic considerations.

Third, he derives the *SKK* culture of the Balkans from the earliest agricultural settlements in Greece, and these from an early farming and herding culture in Western Anatolia. I agree that this seems plausible from the evidence he cites. But then he goes on to assume that the IE language likewise had its source in Anatolia. Here I disagree since the linguistic evidence is inconsistent with this assumption. And since IE is a linguistic phenomenon, the linguistic considerations must take precedence over the apparent implications of the archeological phenomena, which in preliterate cultures are uninformative in regard to language.

Fourth, he goes to considerable length to show that the *Indus Valley* culture may well have been Indo-Aryan in speech. He bases his argument on a semantic analysis of the Rig Veda, seeking to establish that it shows no clear evidence of the commonly assumed Aryan destruction of a pre-Indo-European civilization. Still, although he may have been successful in this, it does not suffice to prove his thesis. According to Renfrew, the civilization was flourishing shortly after 3000 BC, which is rather early for the presence of Indo-Iranians in this region. On the other hand, the proposition that the civilization was Dravidian in speech has considerable plausibility in view of the fact that the early presence of Dravidian speakers in Northwest India is apparent from the many borrowings from Dravidian languages in Classical Sanskrit and some even in the Rig Veda, and by the residual presence of islands of Dravidian speakers in that general region today (Burrow, T., 1959, 373-388); hence it is apparent that the Dravidians had considerable cultural power at that time and place. Furthermore, it is doubtful that an analysis of the Rig Veda can give an accurate picture of early Indo-Aryan life. For comparison, we can consider the Homeric epics and the Greek plays of the Classical period. Although their roots go back to the Greek Dark Age and the latest phase of the preceding Mycenaean period, they give a highly romanticized picture which is very different from that presented by the Mycenaean records. Nevertheless, the matter seems irrelevant in regard to IE origins since even such a seemingly early date as 3000 BC was late in the panorama of post-IE developments.

Fifth, he regards the Celtic presence in Central and Western Europe as dating from an early period, at least as early as the beginning of the Central European Bronze Age (perhaps 2200 BC). I agree with this, in principle, provided we distinguish between Western varieties of pre-Celtic and the more particularized and developed Celtic of historical times.

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Once we recognize the bearers of the *Linear Pottery* culture (in their early phase) as the undivided speakers of the IE-Proper dialect of Indo-European, we can see immediately that it is highly probable that Western dialects of IE-Proper were spoken in the Netherlands and Northern France as early as 5000 BC. From considerations of geography and the operation of Schmidt's Wave Theory, it is very likely that these dialects were more closely related to the contemporary pre-Celtic (on the upper Danube) than to any of the other historically known branches of IE. As I see it, the later presence of Celtic languages in the West and Southwest is due to successive processes of *Elite Dominance* imposed on populations which were already somewhat similar to Celtic in speech. I believe Renfrew would agree with this.

Sixth, he takes a highly skeptical view of the attempts of certain scholars, mainly French (Joseph Vendryes, Georges Dumézil, Emile Benveniste), to show that the tripartite caste system of kings and priests, military systems, and peasants and artisans evinced by such widely separated peoples as the Indo-Aryans, the Celts and the Italic speakers, together with their mythologies and legal systems and rules of poetic meter, had a common origin dating from the Common IE period. I agree with Renfrew totally in this matter, regarding the work of these scholars as fatally marred by their bias, due to their mistaken preconceptions of early Indo-European life. In fact, it seems highly suspicious that the specific terms used in these systems were almost entirely different in the separate branches; although some of the terms can be traced to the common lexicon, their specific application in these branches is independent. As to the alleged similarities in poetic meter, these can be accounted for by the fact that the languages still had a common structure at the time the separate systems were being developed (around 3000 BC, perhaps).

In this connection, we can consider the alleged IE root *rēg-*, meaning "king" or "powerful chief". Since its reflexes are found in Latin, Celtic and Indo-Aryan, widely separated in historical times, it has been customary to regard the root as certifiably dating from the Common IE period. But to me, it does not follow. For it is clear from the extant linguistic evidence that the existence of the speakers of these languages in their historically known positions could scarcely date from earlier than 3000 BC, probably later; this is late in the pattern of post-IE developments. Prior to this, there was a period of at least 2000 years during which the linguistic ancestors of these peoples lived in or near the general Central European area, from Austria to Southeastern Poland. During this period, important developments were occurring in this *Central Region*, as I call it, and among these was a gradual intensification of intertribal conflict due to increasing population pressures here since the best land had long been occupied (Milisauskas, 1978). Because of the continually repeated encroachments, though each on a small scale, there developed a tendency for the tribes to drift away from this general area in an effort to get some relief, imposing their languages on outlying neighbors by *Elite Dominance* probably reinforced by peasant settlements. Accordingly, it is not surprising to find these peoples in their historically known positions by historical times. Contrary to common opinion, the fountainhead of post-IE aggression and migration was primarily in Central Europe, and only secondarily in outlying regions such as Southern Russia. From a processual point of view, this development can be seen as fueled by agriculture, which in the *Central Region* was relatively productive as compared to regions farther East. This was because of a relatively high rainfall and mild climate due to the influence of the Atlantic weather system.

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With this as background, we can now understand the true nature and origin of the phenomena treated by the above scholars. Because of the developments in the *Central Region* here described, gradually accelerating during the fifth, fourth and third millennia, and because the linguistic ancestors of the Celts, Latins and Indo-Iranians were still situated in or near this region during much of this period, these peoples naturally shared in a common cultural trend leading to a more intensively hierarchical social structure. As this developed, the tribal languages came to be no longer mutually intelligible, so the linguistic terms pertaining to the new structure were applied mostly independently. Nevertheless, by the operation of Schmidt's Wave Theory, there undoubtedly was some intertribal linguistic borrowing, and one example of this may well have been **rēg-*. This appears to be due to a vrddhi modification (i.e., a lengthening of the vowel) of the more ancient and widely occurring root **reg-* with reflexes in our "right", "reckon" and "reach", and with more general meanings such as to straighten or align or regulate. The vrddhi development seems to have occurred in a particular branch of post-IE-Proper in the *Central Region* in response to the need for a term for the gradually emerging institution of the *conspicuous chief* having increased powers of coercion and consequent wealth; the new term then spread to some of the other branches in this region. Although this apparently preceded the Satem development and the change of Celtic **ē* to **ī*, this is not significant since these were late and parochial in the panorama of post-IE-Proper developments. Obviously, the occurrence of **rīk-* in Common Germanic is due to late borrowing from Celtic after the Celtic vowel change had occurred, indicating the late emergence of Germanic speakers from their Northern isolation in response to late Northward expansion by the Celts.

In view of this, it is easy to see why a coincidence of roots in Indo-Iranian and Central or West European branches is by no means an adequate indication of inheritance from Common IE or even Common IE-Proper; the same remark applies in regard to a supposed similarity of traditions. I regret such a long discussion, but it seems justified in view of the characteristic confusion in discussions of this topic.

As expected, Renfrew's final chapter is a recapitulation. It also includes digressions on the evolution of several other linguistic families.

This completes my review and commentary on Renfrew's book as it stands. A short discussion will now be added, based mostly on linguistic considerations, which will confirm most of his conclusions. Unfortunately, there are many facets to be considered, and because of space limitations we cannot go into as much detail as the subject actually merits.

Renfrew's purpose is twofold: first, to show the inadequacy of the presently popular theories of Indo-European origins, represented today mainly by Gimbutas' hypothesis, by exposing the falsity of their assumptions and the circularity of reasoning generally used to sustain them, together with the shallow time depths and consequent anachronisms implied; and second and coequally, to show the high antiquity of the presence of post-IE speakers in the *Central Region* and especially in the Balkans. He is clearly right on both counts. Regardless whether we look at the matter superficially or in detail, the linguistic indications of this early presence are abundant.

Superficially, it is immediately apparent from the pattern of river names in the

Central Region and the Balkans that these together comprised the true heartland of Indo-European in Europe, for this seems to be the only region where non-IE names are not found, while such names do appear in the Southern, Western and Northern fringes of Europe (mixed with IE names) -- (Kerns, J.C., 1985, 132; Tovar, Antonio, 1977). Thus it appears that IE speakers must have dominated the heartland from an exceptionally early period, long antedating any activity by Gimbutas' Kurgan people. For comparison, we can consider the situation in England and the United States, where English has long been established, yet many river names are of non-English origin. There is a similar situation in France and Spain, where many river names date from pre-Latin times.

Investigating in greater detail, we again find that the *Central Region* was the true heartland of Indo-European expansion. It is here that we find the most ancient dialectal differences between the historically known branches, together with many indications of prolonged mutual influence between them by Schmidt's Wave Theory, suggesting profoundly ancient geographical juxtapositions in nearly the same relative positions as evinced in historical times -- with a few notable exceptions, easily accounted for. The most egregious exception is Indo-Iranian, which shows clear signs of early intimate contact with Slavic (Burrow, T., 1959, 18-23) and also with Greek and Armenian, yet practically no evidence of early contact with Tocharian -- the eventual contacts of Tocharians with Iranian traders and Indian Buddhists are irrelevant in this discussion since the late separation of Indic from Iranian had already occurred by this time.

Incidentally, if anyone believes that the early contact of Slavic with Indo-Iranian could have occurred outside of non-Russian Europe, it should be borne in mind that there are definite signs of Indo-Iranian contact with Finno-Ugric while both were still undivided (Collinder, Björn, 1977, 140-151), yet there are no signs of Slavic contact with any Finno-Ugric speaking people until the Christian era. At the time of the contact of Indo-Iranian with Slavic the Indo-Iranian leveling of the vowels *e, o, a* to *a* (long and short) had not yet occurred, but at the time of its contact with Finno-Ugric this leveling had indeed occurred or was at least well under way. This clearly shows the chronological priority of the Slavic contact and that it existed in non-Russian Europe, and it graphically illustrates the later Eastward movement of the Indo-Iranians.

Another marked exception is Tocharian itself. According to G. S. Lane (*Lane Studies*, circa 1967-70, 76-87), it seems to show its earliest connections with the Northwestern branch Celtic, and later with the Northeastern branches Baltic and Slavic (in their pre-Satem form, of course). In contrast, it shows no evidence of connections with its geographical neighbor Indo-Iranian except at the much later period mentioned above. On the other hand, Lane makes no mention of the fact that Tocharian does show a few special agreements of vocabulary with Greek, unknown elsewhere; from circumstantial considerations, I consider this significant, for I regard the Tocharians and Greeks as respectively Eastern and Southwestern extremes of relict populations of pre-Satem speakers of *Tripolye* origin, predating the Eastward Indo-Iranian movement by at least a millennium.

The major remaining exception is Anatolian, which stands apart from the rest of Indo-European (here called IE-Proper). It shows no sign of early special connections with any specific branch of IE-Proper in preference to any of the others, not even to Greek, Phrygian or Armenian, its nearest geographical post-IE speaking neighbors in historical

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times. This fact is significant, and I will refer to it later since it militates against Renfrew's suggestion that the undivided IE language was injected into the Balkans from Anatolia, for if he were right, it is very likely that there would be clear signs of close early special relationship between one or more of these three languages and Anatolian -- and no such signs exist. Here "early" refers to intimate contacts dating from the seventh millennium BC; there were indeed contacts after the third millennium, but that is irrelevant in this discussion. This means that the speakers of each of these three languages were intrusive in their historically known positions.

Although these three exceptions are the ones that spring most immediately to mind, they are by no means the only ones. Other less egregious instances are the repeated Celtic expansions to the West along the upper Danube, the Italic speakers to the South into Italy, the Greeks Southward into Greece and the Aegean area, and the Phrygians and Armenians Southeastward into or near Anatolia. What is significant is that in all cases the tribes emerged into areas where they had no close early linguistic relationship with the surrounding population. On the contrary, the earliest association of the Italic speakers was with the early Celts on the upper Danube in Austria or Hungary (Lehmann, R.P.M & W.P., 1975, 83), the Phrygians and Armenians with the Greeks in the Eastern Balkans (Mann, S.E., 1963, vi), and the Indo-Iranians with the Slavs in Eastern Europe and secondarily with the Armenians and Greeks. On the other hand, Anatolian shows no early special relationship with any branch of IE-Proper individually, but only with the entire group as a whole -- meaning that IE-Proper was still undivided at the time of its fan-like deployment in the *Linear Pottery* expansion Northward from pre-Anatolian speakers in the Balkans (5200-4800 BC).

These phenomena, considered together, cannot be explained unless we recognize the fact that the main fountainhead of post-IE-Proper expansion was the *Central Region* in the aftermath of the *Linear Pottery* expansion and its economic and cultural maturing, not Southern Russia or any place in Asia or the Near East. Also, the true source of the Anatolian speakers was in the Balkans in the *SKK* culture, not in Anatolia or the Caucasus or elsewhere in the Near East. In fact, from a processual point of view, the movement of the Anatolians from the Balkans can be seen as a Southeastward expression of the same pattern of recession from the *Central Region* as evinced by the Indo-Iranians in their Eastward movement, and for the same reason. Consequently, it is not surprising that these movements seem to have occurred at about the same time (third millennium BC), which is late in the overall pattern of post-IE expansions. Yet another example of this process may well have been Etruscan, which may have represented a Western member of the *SKK* culture, emerging in Northwest Italy as a result of a Westward movement. Unfortunately, the Etruscan language is little known, despite heroic efforts by Italian scholars and their colleagues, but if it was not an actual member of Indo-European, it was apparently more closely related to it (especially to Anatolian) than to any other Nostratic family (Kerns, J.C., 1985, 149-154).

Elsewhere, I have suggested that the IE language was first introduced into the Balkans around 6500 BC by immigrant farmers from North of the Black Sea who had been acculturated to this mode of life by Northward influences from the Fertile Crescent (Kerns, J.C., 1985, 1988), for it seemed reasonable in view of the fact that Indo-European shows

clear pronominal, morphological and structural similarities to other linguistic families to the East -- Uralic, Altaic, Eskimo-Aleut -- the whole comprising a part of a larger group of families called Nostratic, of Mesolithic origin, which also includes Afroasiatic, Elamo-Dravidian and certain Caucasian languages. However, Renfrew rejects such a source for the Indo-Europeans, preferring to identify them with farmers in Western Anatolia who proceeded to extend their economy and language into Greece, then into the Balkans and the *Central Region* in successive stages. But if this were so, the Greek language would necessarily show characteristics intermediate between Anatolian and the rest of IE, yet in no way can Greek be regarded as such an early bridge. On the contrary, Greek represents a late Southward intrusion into its historically known position, like Phrygian and Armenian, as the linguistic phenomena clearly imply. Accordingly, I disagree with Renfrew on this point.

Renfrew's suggestion is a backward projection of his *Wave of Advance Model* to include Greece and Western Anatolia as well as the Balkans and the *Central Region*. In general, this might or might not be valid, depending on circumstances. In this case, I think it is not. For in the *Central Region* the advance was aided by the riparian pattern of the settlements, which initially developed mainly along the rivers and their tributaries since it was here in these relatively lower elevations that the easily tilled and fertile loess soils had been deposited. This tended to channel the advance into some fractal dimension between one and two (disregarding variations in elevation), facilitating its general Northward movement in several directions right across the continental divide and along new rivers on the other side -- and in fact, this *Linear Pottery* deployment was the most rapid of all (Milisauskas, 1978; Kerns, J.C., 1985, 158-161). In Greece and Southern Bulgaria, however, this channeling effect was probably attenuated by the nature of the terrain with its mountainous character and transversely flowing streams. Thus the acculturation of the vigorous Mesolithic population on the lower Danube to an agricultural way of life need not have extended to an adoption of the new language, suggesting that their own language may already have been an early version of Indo-European before the farmers arrived -- in fact, I have recently come to favor this solution. I believe Renfrew himself has suggested this as a possible alternative.

It should be noted that there is no compelling reason for assuming that the first Indo-Europeans in the Balkans are to be identified with the first agriculturists there, any more than to assume that they were horse-riding pastoralists led by aggressive chiefs, which has long been the dominating assumption; as Renfrew and I have shown, there are both archeological and linguistic reasons for rejecting that assumption.

Yet in a similar manner, we could be wrong in our own assumption. True, our assumption has seemed plausible in view of the rapid spread of agriculture and minor pastoralism in the *SKK* culture and especially in its Northern extension in the *Linear Pottery* expansion, thus accounting for the unitary character of the IE-Proper division of IE despite the disparate appearance of its branches in historical times. In the latter case, I have called such a phenomenon a "focal" expansion, wherein the advance begins in a restricted region (here the *Koeroes* area in Hungary) and proceeds so swiftly that it attains its final extent in the course of a few centuries, a time sufficiently short that the original undivided language has had little occasion for change.

Thus the proposition that the first Indo-Europeans in the Balkans are to be identified with the first agriculturists in that region is indeed plausible -- but only up to a point. The difficulty arises when we try to identify that region, adjacent to Europe, whence they came. Since the dominating plants and animals of the Neolithic cultures in Europe are not native to the region, it is natural to assume that they were brought in by immigrants from outside, and Anatolia is indeed the most plausible source. But Renfrew goes on to assume that the undivided Indo-European language was also introduced by these people. I disagree with this latter assumption since it implies consequences which conflict with the linguistic facts.

But my own assumption, that agriculture and the Indo-Europeans entered the Balkans together from North of the Black Sea, seems equally unsatisfactory to me at this time. While it is plausible from a linguistic point of view, there seems to be little evidence for agriculture North of the Black Sea at a date sufficiently early, the seventh millennium BC. Milisauskas (1978) does show the *Dniester-Bug* culture in the proper position in his Fig. 4.5, but in his Fig. 4.3 he dates its beginning at about 5600 BC (calibrated radio carbon), too late to serve as a source for the *SKK* culture (begins about 6300 BC in Bulgaria). True, it is possible that the early Neolithic in the Ukraine simply has not been adequately investigated and that earlier dates may yet be found there, but it is unwise to put much faith in this.

The alternative, then, is to drop the suggestion that the first Indo-Europeans in the Balkans are to be identified with the first agriculturists there. Thus we are left with the assumption that the pre-agricultural Mesolithic people in the Balkans were already speaking early versions of Indo-European at the time the first agriculturists arrived, and that they simply became acculturated to the new technology and economy without adopting the language of the immigrants -- except, of course, to borrow new terms relating to the new way of life. This is the only suggestion I can think of that does not imply contradictions with respect to observation. Indeed, it harmonizes well with the Nostratic origin of the Indo-Europeans in the wide-ranging Mesolithic culture.

Lately there has appeared an article in *Scientific American* magazine (March 1990) by Gamkrelidze and Ivanov (here G&I) which presents a prehistory of early Indo-European which is radically different from previous suggestions. On page 111 they present a "family tree" which appears correct in some details, but grossly wrong in others since it implies anachronisms. But their most startling presentation is their figure on page 112. Here they identify the original homeland of IE with a region immediately South of the Caucasus and East of Anatolia (largely present-day Armenia), and they postulate a Westward extension through Anatolia to account for the presence of Anatolian languages there, and a further extension across the Hellespont into Eastern Macedonia to account for the Greeks in their historically known positions. In this, they seem to have influenced Renfrew since Renfrew refers to G&I in this connection, and they themselves refer to Renfrew with approbation. But as I have said, the suggestion is untenable from a linguistic and chronological point of view.

But the most egregious suggestion of G&I is to derive the remaining branches of IE from a postulated Southeastward migration from Armenia, curving in a counterclockwise direction around the Caspian Sea and Westward through Southern Russia into the Eastern Balkans, successively emitting Iranian into Iran, Indic into the Indus Valley, and Tocharian

into Central Asia, and presumably early versions of Slavic, Baltic, Germanic, Italic and Celtic into Europe via the Eastern Balkans.

This suggestion is, of course, completely wrong. It implies many anachronisms and other contradictions with respect to observable linguistic facts. It is even inconsistent with their family tree, which itself implies anachronisms.

I cannot go into details; it would require a book in itself. It would be well to sever all connections with G&I, for it is clear that they have not adequately thought the problem through. On the other hand, I have no quarrel with their reconstruction of the IE consonantal system.

I admire Renfrew deeply. More than most scholars, he thinks. He is one of the few scholars who have made a serious effort to master two disparate yet inevitably associated disciplines: European prehistoric archeology, and Indo-European comparative linguistics. His only lack is an inadequate appreciation of the seminal importance of the *Central Region* as the primary fountainhead of post-IE expansions. Unfortunately, this fault seems to be widely shared among scholars.

For many years I have hoped that other scholars would do what Renfrew has done.

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<u>COMMENTS</u> to the <u>REVISED VERSION</u> of <u>MURTONEN's COMMENTS</u> [see MT 7,8]

<u>DETAILED REMARKS</u>: <u>3</u>. M's long passage is totally irrelevant. IS's reconstruction is based on Dolg's comparison published in <u>Jazyki Afriki</u> 1966; additional cognates - in Dkl, p.62-3 (see VS in <u>MT7</u>). M simply ignores these data. M has misunderstood IS's table (p.153 of his dict.):"CC" (and not "CC-") <u>does not mean</u> that there are no cases where an additional element; appears after CaC in AfAs or Ns; M could easily see his mistake by looking through the list of IS's roots in the dict.

<u>6</u>. No point of discussion: see VS in MT7, p.15 ('blind' = secondary).

<u>8</u>. Original identity: Akk. ʔ < *ḥ in this root (as in Hebr. bḥr < *bḥr < AfAs (*bḥr) < *baḫ̣ (Dkl,p.72); IS considers ʕ secondary; Tu has a petrified prefix (Dkl: Tu a-ber < Berber *ha-bḥVr); root also present in Chadic. Cushitic phonetics is volatile; still, Saho bar 'grasp',Beja bari 'get' fit quite well. Ns should be *baḥri 'grab, take' (not *bari 'take').

<u>9</u>. See VS in MT7, p.16, about high age of some adjectives (includ.space).

<u>10</u>. The fact that a root is present in one language (here Chad.) <u>does not mean at all</u> that it was lacking in the language-ancestor (here AfAs).Still, IS provides a Sem. parallel (cf. also Dkl,p.85). Orël and Stolbova used WChad. *bar- 'give' (<*bayr-<*byar- [y=j]) to illustrate Chad.CyaC-:Ns CeC- (in our case, Ns *be[rH]u 'give', exactly as in IS's Ns dict.); cf. Hausa.

<u>11</u>. 'Descriptive' is broader than onomatopoeic;discussion: VS in MT7,p.16. As Starostin has shown in Ns and Sino-Caucasian (in <u>Explorations in Language Macrofamilies</u>, Bochum, Brockmeyer 1989), this Ns root fits well SC (= Sino-Caucas.); *same with* other IS's roots, <u>both descriptive and regular</u>.

<u>12</u>. M uses a hopelessly obsolete statement (no -ʕ,-r,etc., can be separated from Semitic tri-cons. roots: cf. also M 3;63;205;212;218).Cf.Dkl,-2,-3 Our root is presented as *beč' > *b(w)č' in Dkl, p.58 (with -k('),-ʕ,-r).- IS understood that Chad. b (=ʼb) was dependent on intervoc. glottal. consonants;Stolbova (AIJa, p.39) correctly explains ɓ-C- < *b-C'-.

<u>13</u>. M saïs that Eg. ' for AfAs (=SH) l is "at best highly irregular, probably unparallelled", but he himself cites the same correspondence in 20. For Eg. ' < *l, see also Dkl, pp. 4o,61; -l might be a splinter of **-l/r-.

<u>21</u>. What other evidence is needed beside the one presented by IS?? - As for Eg. ' < *r, this shift is highly regular, and now supported by massive evidence: Dkl, pp. 28, 29, 38, 51, 58, 84, 104, 107 etc etc etc etc.

<u>22</u>. It is not excluded that some Sem.words are different; still, our root is well represented in Sem.,Cush.,Chad.;IS correctly explains Ch. ɓ<*b--H-.

<u>23</u>. Despite "?", AfAs part fits well; note that Berb.means also 'snow'.

<u>24</u>. See above, 11 and 21 (the latter about regularity of Eg. ' <*r).

<u>27</u>. It would be highly improbable if utterly *unrelated* words showed suf.*b.

<u>28</u>. M's statement about ɣ and ʕ is obsolete: Dolg. has shown that, both in Sem. and AfAs, ɣ was different from ʕ (AfAs *ɣ < Ns *g; AfAs *ʕ < Ns *ɣ,*ʕ).

<u>31</u>. The meaning 'knee' in *brk is based, according to Dkl,p.87, on *bg 'knee,thigh'; IS considered *brk a tri-cons. root; Ns 374 (authored, as everything in vol.3, by a collective) has *brk (<*p'rk' ?) 'request, pray,bless' [AfAs; from Ns *p'irk'V 'request, ask']; 'pray, bless,curse'[Sem.] ≠ 'knee'. In any case, the meaning 'knee (,thigh)' is, no doubt, primary.

<u>32</u>. M is wrong again: AfAs *br 'son' is present not only in Sem.,<u>but also</u> in Berber (Dkl,p.86); WChad. *bara 'child' is related,of course (AIJa,p.154)

<u>33</u>. Rather it was the other way around:'split'>'rock' (as in Russ.<u>skala</u>,etc)

<u>36</u>. See VS in MT7, and above, 12 (-r = augment).

<u>37</u>. AfAs root was *sjh (not *sjḥ; both IS and Dolg. reconstruct,quite correctly, *h [<Ns(*x or)*h] as different from *ḥ [<Ns *q]; see IS,p.149 and 154: tables; actually, Ns *x is Dolg.'s reconstruction;cf.28 above, about voiced counterparts); Sem. was *sjh (in IS, misprint: *sjḥ; see *list* at the end of vol.1). This reconstruction is supported both by 3 other Ns languages and by "extra-Ns" comparison (see Starostin Ns and SC). Some examples should be eliminated, though (e.g., words for 'moon' belong to a differ.root)

*Afrikanskoe istoričeskoe jazykoznanie.Problemy rekonstrukcii, M.,Nauka,1987. -Both AIJa and VJa '88 data not just confirm AfAs sets but also "provide" AfAs vowels (<Ns!)

42. Wrong; Kartv.connection is very much valid, as corroborated by Dolg. who now reconstructs Ns *ĉV(?)ŋV 'recognize,know' > AfAs:Sem. *ŝ?n 'know' (as in Arab. š?n), *ŝny 'see' (Soq. etc.ŝny id.[y=j]): see Reconstructing Languages and Cultures, Bochum, Brockmeyer 1989, p. 95 no.24;cf.VS in MT7.

44. Correctness of IS's reconstr. recently confirmed by Orёl and Stolbova (VJa 1988,no.5,p.70): WChad. *č'ayl-'jump': Ns *ćelV (for ay:e,see 10).

50. Old Hebrew had a meaning 'liquid' as well, not just '(grape) juice'; original meaning was 'moist',and 'rotten wash' or the like;both types held.

51.I doubt that 'smoke' belongs here; as for 'smell',there are at least two root variants (one with an affricate, another one with a sibilant:this is valid for all language phyla, by the way). - As I said: don't ascribe to IS blunders he never committed: A.Faber (MT7,p.20) seems to think that IS was so bad that he would combine,in one root,'smell/odor' and 'urine'!

56. A good set.-Contrary to M, IE central meaning is 'look after'.

57. Correctness of IS's Ns and Sem.-Ham. reconstr. is excellently confirmed by Stolbova (same book as Dk3, p.87): WChad. *č'am- 'to taste (tr.)' as in Hausa c'āmē, Angas čam etc. [for phonetics, cf. WCh. *č'am, *č'an 'think' as in Hausa c'āmmāni, Angas čan etc. (Angas = 'remind')].(WCh.ā?)

59 and 60. I don't see any faults in IS's reconstr. of both "grammatical" words despite desperate attempts to discredit it. By the way, M's frequently used argument that connections between phyla (families) are "hardly likely" because of geograph.distances is absolutely irrelevant as long as we speak about genetic relationship betw. very ancient languages - and this is exactly what we do.

61. See 59 and 60. No real difference in meaning (cf. near; to near; next).

63. All wrong. Meaning 'cover' is present in Sem. (see Dk2,p.77);it is easy to show that -n,-l,-d are augments: to IS's example (Cush.:Beja dem- -im 'press') Dk's may be added now: Eg. s-dm; Ch.:Hausa damo- (id.,p.76-7); instead of checking IS's phon. table on p.151 and learn that IE -ngh-,Ur. -ŋ- fit exactly AfAs (and Sem.) -m- [a rule confirmed by many sets],M sa- is: "The IE Ural velar nasal does not tally well with the SH lab.one".

66. If this or that scholar does not have IS's roots in his/her root list, apparently,he/she has not found them yet. - As for listing Gidar dīde under md(m), - it seems,a whole lot of imagination is needed for it.

73. Again,a gross injustice to IS. 'Be silent, silence' is well represented by all 3 languages, i.e., Alt, Kartv. and AfAs (both Sem. and Cush.) all this confirmed in Dk2 (p.71-2) with additional examples.- And again,instead checking IS's table (where it is correctly stated that Alt.-ŋ- [as well as Ur.-ŋ- and IE -ngh-; see 63] fits Kartv.and AfAs -m-), M *implies* IS's phonetic negligence. Some criticism ![M simply ignored VS in MT7:63,73]

74. See 59 and 60 above.// 75. Quite valid! (cf.VS in MT7,p.18,no.75).

76. See VS in MT7. I stress: all sounds in Oromo du? find correspondences both in AfAs and IE roots. Not only Oromo du? means 'die' (see Dk2,p.87)* M ignored,in my remark,points of a real scholarly interest; he extracted only elements which he could (as he thought) use against IS. - Forms *du'-, *di'- (Ch.,Cush.) confirm IE connection *dweiH- (?~*H).By the way,IE has other meanings (besides 'die') comparable with AfAs.

81. Since Cush. *qd is secondary to *gd,IS, naturally, cited (under "?") Sem. forms of the type qdl.(Extra-Ns sets include *gVd- 'back pt!) As for -l as a possible augment, see Dk (AfAs augment -l, -r, -l/r).

82. Both Chad. and Cush have gir(-) 'fire' and 'day' (IS; add Orёl, Stolbova, Position of Cushitic,forthcoming in Protolanguages and Protocultures, Bochum).Hence AfAs 'daylight' etc. is not secondary.

87. It is beyond my understanding why should one invent implausible explanations when we already have a quite plausible one.(See VS in MT7).

88. Why is AfAs *g(w)ʕr an "artificial" combination of two roots if M himself acknowledges them being cognates? Since we have forms like gūr in Cush we should reconstruct AfAs *guʕr which is the same as IS's *gwʕr.

90. Ns *gurHa 'antelope' > AfAs *gurH id. is fine;cf.also 59 and 60.

*Add Agaw *di'- ~ CChad. *di'- 'die'; *du'- in ECush; Galla-Somali (Or,Stolб.,Position of Cush.)

A confirmation(ECush.:Sidama *gur-um-, WChad. *gur-): Orël-Stolb.,on Cush.
91.Sem. *gwr or (less likely) *gr; WCh.and Cush.*gwr show AfAs *gwr.
92.See IS's explanation of alternations at the end of this entry. It
is dangerous to explain them by primordial non-differentiation of stops.
94.AfAs *gwl (gul) is shown by Sem.and Chad.<Ns *gUlV (Krtv r < Ns l).
95.M simply ignores my comment in MT7;WCh.(=AfAs,Sem) is *gur- (AIJa).
98.All wrong about "restriction" of ɣ as opposed to ʕ (see 28 above):
both phoneme are AfAs. IS's reconstruction is precise: Ns 9 > AfAs,Krtv ɣ.
99.Quite wrong about phonetics (see above).'Dark' and 'night' is normal.
100. Unaceptab, about phonetics (see 92).Innovation ?- is restricted to
Hebrew which has h- as well. "Onomatopoeic" objections are unacceptable.
101.Both in 100 and 101 M simply ignores my remarks (from MT7).
111.IS reconstr. is excellent. Of course he does not even consider Sem.
(to say nothing about bibl. Hbr.) which is in many respects not archaic.
121.Don't ascribe to IS blunders he never committed: he calls things ex-
actly what they are - articles in Aram. and Hbr., pronouns in Berb.,etc;
his reconstr. of Ns deictic pronom.particles *?a (121) and *?e/*?i (134)
is a masterpiece of compar. linguistics; Dolg. made an independ.reconstr.
122.I don't see any reason whatsoever to change or discard IS's reconst.
123.In the revised reconstr. of this Ns root Dolg. uses only words with
concrete meaning ('food', 'fat', 'meat', 'feed' etc.). Ns = 'food'(as IS).
124.IS's reconstr. is based on Dolg.'s data (Cush. *?mr 'morning,dawn'
etc.) to which IS added Sem. *?mr 'see, be seen'; Dolg.does not accept it.
Now Dolg. reconstructs Ns *?am[o](rV) 'morn.,d-light' (Cush,Brb,IE,Krt,Ur)*
125.An exact Ns reconstr. despite descript. character;Dolg.adds Alt,Ur.
126.M simply ignores my remark in MT7.
127.Might be onomatopoeic which does not exclude an exact reconstr.;Dolg.
now reconstructs Ns *?üsV; Ur *üsV is added, but IE "*hes-" is eliminated
(which is correct: IS didn't know the difference betw. IE stable *x as in
*xas- [sic: -a-] > Hitt. has-, and unstable h and ?).Dolg.eliminates Alt.
128. See my remark in MT7. Note also that negation belongs to the stabl-
est units, which means that just one cons. (like l here) is enough to ident.
129.Wrong conception expressed several times by M.Reconstruction *?ay for
Sem. (A.Faber, MT7,p.20) seems correct. A very stable root (cf. 128);cf. 10,44.
130.If one reads attentively, one understands that the archaic meaning 'co-
me' is preserved in Hittite-Luwian (=Anat.), the most archaic IE language;
cf. also M.Kaiser, Lexical Archâisms in Slavic (forthcoming in Bochum).
132.Dolg.now reconstructs Ns *?[ä]š[o] 'settle, stay, be' >AfAs *?is-
'be, exist' > Sem. *[?]iš- 'have','exist',*'y-išu 'he/it has' > 'there is',
?Iθay 'there is'; Berb.-h₁suH 'arrive';ECush.*?Vs/š 'be,stay'>'spend day'.
134.Profound misunderstanding by M ; see also 121 above.Dolg.close to IS.
135.A good Ns root; confirmed by Militarëv (AfAs *?ayVl) ánd Dolg.
136.Dolg.nów confirms IS's reconstr.: Cush. *?it- 'eat' etc.(<Ns *?itE).
137.Dolg. reconstructs AfAs *ʕal- 'high place','ascend'> Sem. *ʕal- 'he-
ight',*ʕl(a)y 'ascend, go up';Berb.*-Hliy 'climb,ascend';Cush. *ʕal- 'moun-
tain, highland' (IS's and Dolg.'s interpret.'to cross ...'was incorrect).
138.IS's reconstr. is now supported by Dolg. (AfAs *ʕib > Šem. *ʕ[i]bb->
> *ʕubb-; Chad. *[w/?]uHb- etc. [e.g., in Lamang wuʕba; b<*Hb]). It seems,
Ns was *ʕEbU (>AfAs,Alt), *ʕUbE (>Krtv.) ["Blažek's rule"].Cf. VS in MT7.
139.In a few papers I proposed 2 Ns roots: 'water' with -a- and 'drink'
with -e-; now Dolg. reconstructs Ns *ʕ/ɣaK'u 'water' (> AfAs *ʕ[a]k'w- >
Sem. *?k'k', (?)*ʕk'w/*ʕwk'; Cush. *ʕak'ʷ- [=CCush.] etc.) and *ʕ[E]gU 'dr.'
140.M's argument for 140 ['burn (offering)'] as being related to 137 is
as follows: "a burnt offering naturally goes up in smoke";this might be a
major breakthrough in etymol. methodology but I'm still not buying it.Now
Dolg. provides additional data (Ns *ʕ[a]lV > AfAs:Sem.*ʕlw; Ural. *alV 'to
sacrifice' + Alt; but Cush.is excluded: different roots),eliminating "?".
141.I have x̣ (uvular), not x (velar) in my note.-Since Hitt. has h- in
ḫast- (usually compared with 3 other IE l-ges) I feel intrigued: Nostr. ha
a stable "laryngeal", - exactly the one which is preserved in[IE and] Ht.as ḫ

* Dolg. [without reference] is from his files: please don't use in print.

142. "Pre-Semitic time" is AfAs time, and "Pre-AfAs" time is Ns time -
or, for that matter, "pre-AfAs-Ns-SC" (if we follow Militarëv-Starostin).
But even at that time the proto-proto-language had a quite normal phono-
logical system, maybe with only 3 vowels, but certainly with a lot of con-
sonantal phonemes But for M "pre-Sem." time is, it seems, that of a primo-
rdial language (spoken, say, 100,000 y.ago, or even much earlier). M has
the same problem which many other linguists have who deal with one family
only (Helimsky correctly criticized once in VJa the Turkologist - and an-
ti-Nostraticist - Ščerbak who reconstructed for "pre-Turkic time" a kind
of monstrous "languǎge" which hardly could ever exist; in reality, Turkic
is one of several humble daughters of Altaic (<Ns<...etc.))Cf.92,100,129.

144. Any attentive comparatist will agree that AfAs/Sem. *jam (j=y) and
Ural. *jamV- have 3 phonemes in common (hence a safe Ns reconstr. *jamV).
But M has a problem here (see the previous note) and tries to impose it on IS.

146. IS correctly points out Cohen's mistake.

150; 151. For some reason, M's attack on IS become especially vicious
(and 100 o/o unfounded) when a grammatical particle (pron.,suff.,etc.) is
involved (see 111;121[where M erroneously identifies h- with a sibilant(=
a different particle); speculating in this way, M accuses IS in speculat-
ions!!!],122,128,129,134). It would be more profitable for M to try and
understand why IS managed to correctly identify grammatical sets (where,
indeed, only 1 cons. is frequently recognizable): one of the reasons is a
relative abundance of such elements and their high stability; another, "sub-
jective", reason was a stunning ability of IS (admired by Dolg.,Menges,
Ivanov, Dybo, Starostin, Helimsky et al.) to identify the right thing in
the material which is some kind of chaotic mass in the eyes of others.

154. This possibility discussed by IS (at the end of this entry).

155. IS: 'fish'>'whale' in AfAs(SAr.),Ur.(Nenets),Alt.(Tung.).-Cf.59,60.

157. A good set,now confirmed by Orël-Stolbova Position of Cush.(here al-
so WChad. *kam- 'grasp').-Agaw (=CCush.) *kam-,*kyam- is interesting: it se-
ems to reflect a root var. with *e (cf.10: ay/ya; e); adding Chad."*gm" we
may compare all this with *k/gem- in Austrones.;as for Sem. var.with,q=k',
cf. SC *k'em-, *k'im- (the latter in Nootka, one of SC l-ges in N.America).

158.A good set, indirectly confirmed by Starostin (Ns and SC) who identi-
fies a SC root 'lip',- this fits M's remark about 'lip' being primary meaning.

162.Minor changes are not excluded; the whole set (incl.Sem.)stays.

173.IS discusses this possibility, of course: end of this entry.

177.A fine set. As for "geogr. distance", see 59 and 60 above.

178.A fine set, confirmed by Chad.(WCh. *kwin-);CCush.*-kwin 'woman,wife'.

179.A quite acceptable set (including Ur.); for "geogr. dist.",see 59,60.

180.Sem. q/k (=k'/k) is not an indication that *km/*k'm is a Wanderwort
(see, e.g., 157, for q/k). Ns reconstr. is quite acceptable.-See 205 (for q/k).

190.A variation b/p' appears in some roots. As for W.Ns (AfAs,IE,Krtv.),
IS reconstructs *k'aba only (IE *ghabh-/*kap- <**kabh- [<Ns *k'aba] evolved
according to the known rule "No T-Dh [or Dh-T] in one IE root").

192.Interesting set - both ethnogr. and linguistically. IS is very care-
ful to separate Ns heritage in AfAs, Krtv.,IE,Drav. from borrowings:end of 192.

195.Berb.fits quite well; its Sem. cognate is plausible,as well as IE,Krt.

196.q/k does not prove this (see 157, 180), though descript. character is
quite possible: cf. IS's remarks on p.321. The reconstr. is still acceptable.

197.IS's ability to "predict" is astounding; cf. WChad. *k'ar- (and Tanga-
le k'er), with k'(see Orël-Stolbova VJa5,'88,p.68). -For AfAs k'/k,see 180 etc.

199.A rather precise set, despite descript. character (see end of entry).

201.Well preserved in Cush and Chad.,as well as in IE,Krtv.,Ur.,Alt. - Note
that IS does not say that AfAs shows intensif. funct. (so "don't ascribe...").

202.Even if we drop some forms (and I'm not sure that we should) we still
will have a well-preserved set (Sem. *k'wl, same in Cush.,Ch. + all other Ns).

204.Set confirmed by Dolg.(Etimol.1970,p. 364) and Orël-Stolbova (VJa '88:
p.73):WChad. *k'wal-/*k'war- [phonetically similar to *k'wal-'hear'<Ns.*q'iwlV

161.Set confirmed in V.Ja. '88(# 16): WChad. *kal[u/w]- and Cush. cognates.

205. Again we have k'/k in AfAs (cf. 157, 180, 197); Starostin compares Ns *k'Ut'V 'small' [Cf.same root in Amerind] with SC: North-Caucas.*kot'V 'short' [in Ns and SC, no.75]; I wonder if we have here one of AfAs:SC iso-forms since both languages have k- (St. considers AfAs,Ns,SC "sisters").

208. IS does not say that -y- was in AfAs (his *ql-);he explains why.

210. I wonder how 'boil' and 'sunrise' can evolve into 'rise'; it seems quite clear to me that it is the other way around (cf.'top','be high')

211.I fail to see how 'acquire' can be primary,and 'create' secondary;and I agree with IS that the original meaning was 'give birth' (same in IE,Dr.)

212.Evidence of -r/-n being a secondary element in *kpr,*kpn is precise-ly in the fact that they interchange.Note also k/k' (cf. 180,205 etc.).So I flatly disagree that Sem.should be dropped, - but even if we were left with Berb. and Chad. only, we still would have a fine Ns set.

215.AfAs seems tenable; validity of AfAs meaning seems to be indirectly confirmed by archaic Drav. 'singe' (present also in Uralic). - I fail to see how 'twist, turn' is compatible with IS's material.

216, 217. Sets are much stronger than M presents them (accepting them).

219. g might be a misprint (text shows that IS meant qass). - I have a feeling that M tends to mix this root and 196.- AfAs -a- in WCh.*-kasi, Cush.[*kas] 'bone'.

222.I flatly disagree with the idea abot *kp(p) 'bend' being related: this latter belongs to 92. For q/k, see 157,180,196,197,205,212.AfAs of 222 fits well Ural. *käppä 'paw' and IE *k̂epH- 'hoof' (as usually, palat. k̂ of IE is due to Ns front V). Seems to belong to 190; might be descriptive (& extra-Ns).

224, 229.For "geograph. remoteness" , see 59 and 60 above.

227. IE shows an ancient heterocl. *ker-u/*ker-n-; Sem. -n- > IE?

231. -h, -d are augments as in many other cases. (or)

232. An excellent reconstr.Cf. 150,151.-Ex-Ns: SC, Amerind,etc.

233.The passage about 'stay' and 'rest' is one of M's captious objections.

238. Only in part. // 239: Wrong ('call' belongs to a different root).

241.This reconstr. is confirmed (V.Ja.'88,p.73). Not every "Kulturwort" is a Wanderwort.

245.See 150,151,232 (as well as 111,121,122,128,129,134).-

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REMARKS TO M's CONCLUSIVE REMARKS: Kaiser's translation was supposed to generate constructive, and not destructive, response. - AfAs (= S-H) home-land was not in EAfrica but in the Near East (Militarёv and Shnirel'man se-em to be right in matching AfAs vocabulary [which indicates a highly deve-loped culture] with Natufians etc., - i.e., with the most developed ancient cultures of the Near East. Semites, after millennia, still stayed in the Near East; so IE's were rather close to them).

M and myself have diametrically opposed concepts, as it is clear from M's remark that modern attempts at reconstructing remote 'proto-proto-languages' are "reminicent of the 19th rather than 20th century linguistics". I consi-der M's dogmatic approach (which is typical for "narrow specialists") as obsolete. And I quite agree with V.Ivanov who wrote that IS's work had out-stripped the development of linguistics for decades to come (cf. 150,151 and 197 above,and, the whole body of my remarks: especially 142). Results of extra-deep reconstructions might be less reliable; still, because the la-nguage material increases (and not decreases) with each step back, these re-sults are frequently striking [to make my point clear: to reconstruct IE,we use n languages; to reconstruct Ns, we use these languages, plus languages which belong to - at least - 5 more families; accordingly, Ns imposes a strict control on the reconstruction of, say, IE laryngeals,or stops,or vo-wels].Linguists who are prejudiced towards broad comparisons and deep recon-structions (which is understandable: cf. disastrous results of such attempts in the U.S. in the 50's and 60's) tend to look for faults in IS's(or Dolg.'s, or Starostin's) works [of course, mistakes can be found,though very seldom]; they never try to learn from IS (the late W.Cowgill was an exception). Iva-nov correctly remarked, though, that IS's terse text can teach a lot those scholars who study "individual" l-ge families. --- Today we may reconstruct IE *th,*t,*d (see next); laryngeals *x (>Hitt.,Luw. h̥), *h (> zero),*? (stop); cf.,e.g.,VS, On Laryngeals (in:Die Laryngaltheorie, Hdb.,C.Winter,'88); vowels *a,*e,*o, unstressed *i,*u (stressed > *ai, *au or the like);this is likely.

(most)

COMMENTS on B[OMHARD]'s SUPPLEMENT to MT: "LEXICAL PARALLELS..."

Despite several correct IE-AA [=Afro-As.] cognate sets, B's book Toward
P-Ns ('84) was severely criticized by Helimsky in JAOS '87, 97-100; Dolg.
in BSL '86,2,91-97; Kaiser and VS in General Ling.'87,1,34-44, VS in Ural-
Alt.Jahrb.'89,1sqq. et al.; B's new work seems to be of the same kind, as
seen in Starostin's Ns and SC where he criticizes B's Survey of the compar.
phonology of Ns 1-ges,'87 [based on the same principles as the above supple-
ment]: "While surveying the materials of B's Survey, I did not find one si-
ngle example of PI [=IE] *t ... corresponding to PK [=Kartv.] *t or of PI
*k corresponding to PK *k. On the other hand, in one of IS's basic works
[in E = Etimologija '65], we find several good examples of PI *d (*t' in
glottalic notation) corresponding to PK *t, and PI *g (*k') correponding
to PK *k"[he cites 4 sets]. "... B. disregards completely a quite conside-
rable number of correspondences between PI *t, *k and PK *t', *k' (*q')"
[he cites 11 sets]."For these reasons, I feel it is impossible to justify
B's revision of the Ns phonological system; I feel that the original sys-
tem of correspondences established by IS is to be preserved".

We have the same problem with B's IE-AA parallels: he tries, by all means,
make IE words with *t,*k match AA words with *t,*k (though, according to
IS, AA should have *t',*k' in such cases). B also makes IE words with "*t',
*k'" (= *d,*g in tradit. transcr.) match AA *t',*k' (though, according to
IS, AA should have *t, *k in such cases). Ignoring IS's sets, B applies a
large number of tricks to make his sets "valid". A few examples follow:

B89: Additional meanings 'push, strike' are invented by B for IE *tag-
'touch' (B's "*t[h]ak'-") to make it match AA "*t[h]ǝ/ak'-".-In reality, IE
root originates from Ns *täkV 'touch' (> Alt. *täg-, Dr. [=Drav.] *takV- id.)

B90: Despite a total lack of semant. corresp., B compares IE *tem- 'dark,
darkness' with AA "*t[h]ǝ/am- 'cover over, hide'" (I can't locate such a ro-
ot). In reality, IE fits AA *t'wm, Ur. *tumV 'dark' < Ns *t'umV;AA *t'<Ns *t'.

B94: B invents 'firm, solid' for IE "*t[h]ek'-u-" (=[*tegu]> thick) to ma-
ke it match AA"*t[h]ǝ/ak'- 'be(come) established, firm, solid'. This latter
(= AA *tk'n 'put in order' in IS) fits rather IE *deḱ- 'correspond, suitable,
appropriate', Dr *tak(k)- 'suit, correspond',all < Ns *ta[k']V (IS).- By the
way, if a root is in one l-ge only,the proto-lang. root should be in brackets.

B115. Dr.root is extracted from the above set to make it match IE ⬛⬛⬛⬛⬛⬛⬛⬛
*t'e/ok[h] s - 'be fit, appropriate, suitable, proper'(in reality, this lat-
ter is rather   *teḱt- as in Greek téktōn 'carpenter'; add Alt.:Tungus*tokt-
'cut, hew; axe',Ur. *tuktV < Ns *t'uKt'V 'hew,cut; build':Dolg. in E'67,303).
The meaning of IE root is 'weave; fabricate (with ax), to make wicker or wat-
tle fabric for mud-covered house wall';this is not reflected in B's semantic
reconstruction but he does not care: in his book (p.213) he matched this ro-
ot with AA"*tsǝ/ak- 'bend, turn, twist, cover'", and in order to make it match
B invented for IE a totally different (from 115 above) meaning 'bend, turn,
twist, wind'; he gave IE as "*tǝ/ak(s)-","adding" Armen. thekhem 'bend,shape'.
This kind of "methodology" discredits not just B's work,but the Ns theory.

B81. Without any ground B invents for Kartv."*t'ep[h]-"'warm' an original
shape "*t[h]ep[h]-" to make it match the IE "*t[h]e/op[h]-"'burn, be hot'
(and AA "*t[h]ǝ/ap[h]- id."), but it is not needed: the Kartv. form was, of
course, *t'ep- from the very beginning (var.: *t'eb-), and it matches IE ex-
cellently: IE t, Kartv. t' < Ns *t', according to IS,Dolg.,Starostin,Helimsky,
Orel,Stolbova, Blažek et al. -   *t'äpV has t' in extra-Ns sets as well.

B83. B dismembers here his Ns "root" "*t[h]i/er-p[h]-"which allows him to
use it once more (as "*t[h]i/er-") in B82; as for B83, B uses here only AA
root variant *trp, omitting *t'rp, as well as Kartv. *t'rp 'delight' etc.
NOBODY WILL DOUBT THAT BOTH VARIANTS SHOULD BE ANALYSED TOGETHER - which,of
course, will lead to IS's reconstr. *t'VrpV (with a deglott. var. *tVrpV).

NOTE: 1.As soon as B violates IS's rules, his reconstr. doesn't hold [e.g.,
from the point of view of common sense] (see next page).-2.Where B's reconstr.
fits IS (e.g., AA d- = Kartv. d- = IE dh-),it's considerably better. - 3.I'm
sure,neither B nor M will accept criticism,- but what I write is not for them:
it's for younger scholars,rather - for students, able to approach our data
without prejudice. -4. IE Th - T - D [as in Ałt] is better than T-D-Dℎ [or T-T'..]

IN VERY MANY CASES B COMBINES UNRELATED WORDS (FORCING UPON THEM HIS
APRIORISTIC "RULES"), DELIBERATELY IGNORING GENUINE COGNATES, e.g.:
    B214. B compares AA "*k'ə/am- 'eat,...,crush'" [apparently 2 roots] with
"IE *k'e/omb[h]- 'chew,...,crush'", dismembering IE *g̑embh-, with an infix,
as shown by IE *g̑ebh- 'jaw,eat' etc (variants *g̑ep-, *g̑eph-): see Pokorny.
In reality, we have AA *q(w)m (=*k'um-) 'devour, eat', IE *kʷem- 'swallow,
slurp' (< Ns *K'UmV, see IS no.242): regular AA k'w: IE kʷ(<Ns *k𝑦 or *q𝑦).
    B212 and 192: AA root might have had 2 variants (with k' and k: see M157
[i.e., my comments to M, above]),the "main" one being *kam- as shown by W.
Chad.: this brilliantly confirms both IS's Ns reconstr. *kamu 'grab' etc.
and his choice of IE root with g (< Ns k before a),*gem- [not g̑- or gʷ-].
B mixes this latter with AA "*k'am-" and adds Dr. *kum- with a totally dif-
ferent vowel and meaning ('be heaped together'). In 192, B compares AA root
"*k[h]ə/am-" 'seize' etc. [i.e., our *kam-] with Dr.*kam- id. (dropping Alt.,
Ur. and IE - see IS157) with a non-existing IE "*k[h]em-t[h]- 'seize'"etc.
    B213 and his book p.240:B compares AA "*k'a/em- 'weep' [etc.]" with a non-
existing "IE *k'e/om- 'weep' [etc.]"; in reality, he uses a single Latin form
gemō ('seufzen,stöhnen'; to Gr. gémō 'bin voll' <*gem- 'greifen...bedrückt').
    B216. B compares AA "*k'ə/ar- 'cut'" [*k'ar- ?] with a non-existing IE
"*k'e/or- 'cut (into), make an incision,...'": dismembering IE [*gerbh-] (?).
    B215 and his book p.241: B compares a "AA *k'ə/aw-'make a round hole in'"
(extracted from several roots) with IE "*k'u- [etc.] id."; there is no such
root in IE: B used Pokorny's *gēu- [etc; with a laryngeal!] 'biegen, krüm-
men, wölben', from which he extracts Gr. gŭros 'round' (<*gŭ-ro-) which has
nothing to do with 'hole', and gúpē 'hole': but this latter was mistakingly
booked here by P[okorny] as shown by IS (87 Ns *gop'a 'empty'; cf. M87).
    B has 17 entries (209-225) with "Ns *k'-" from which I have just discussed
5; almost all other entries (except onomatop. 224 and 225, both "*k'ak'-)
are disfigured and/or reduced IS's entries: B209 = IS 211 (*K'anV 'give bi-
rth, be born'), with replacement (IE *gen- instead *ken-). - B210 = IS 199
(with IE added; Dr. *kīr- replaced by *kar-; Ur.,Alt.,Kartv. dropped; so,
instead of an exact reconstruction *k'erjä 'shout' [with j in AA and Ur.,
-i in Alt.,and front V in Ur.,Dr.,Alt.,Kartv. - and maybe IE *ker-: see end
of entry], we get an amorphous "*k'a/er-";k' is unjustified without Kartv.)
- B217 = IS162 (with Kartv. *kal- dropped: it contradicts B's Ns "*k'", re-
constructed solely on IE *g- ; note lack of accuracy: front ä in Ur. and
Alt., as well as g̑ (not g) in IE fits   Ns ä, hence *kälU, with k (not k')].
- B218 = IS 198 (exact IS's reconstr. *k'elV 'lack' is replaced by amorphous
"*k'i/el- 'decrease,....,small,few'", because a different root, Dr. *kil-
'small', was added by B,as well as "AA *k'ə/al-"; Ur. *kelke 'lack' is dropped)
- B219 = IS 206 (B mutilates Kartv.*k'erb- by cutting -b- away, though it is
quite clear from IS that Kartv. *k'erb- fits IE *k/kerp- [from *k/kerbh-, ac-
cording to the rule "No T--Dh in one root"], both phonetically and semantical-
ly). - B220 = IS 176 (the root begins with k-, of course, as shown by IE *gel-
or *g̑el- (g<Ns *k), and by Kartv. *kwer/l- [r fits Alt 1 in *Köl-], but B dis-
cards it as not fitting his "rule";he also drops Ur. *kūL-mä 'cold, freeze').
- B221 = IS 190 (B cites "IE *k'eb[h]-", i.e.,*gebh-,which does not exist, in-
stead of *ghabh-/*kap- < *kabh-/*kap- < Ns. *k'ab/p'a [IE *kabh- > *ghabh-,ac-
cording to the rule "No T--Dh in one root"; B cites Dr. *kapp- which contra-
dicts   Ns -b-, and lets out Dr. *kavv- which fits; see also M190 above).-
B223 = IS 159 (B drops Alt. and AA; this latter has k-;cf. IE g-,also<Ns *k-).

    B's "Ns" list is a mutilated IS's list;B dismembers roots of Ns language(s)
in accordance with his "rules" based on wrongly formulated sound correspon-
dences, such as "AA k = Kartv. k = IE k", instead of "AA k = Kartv. k = IE g",
etc. I have analysed, deliberately, two full pages of B's long list (on the
present page) to show that B's "methodology" is the same everywhere.B's tables
(pp.75-8) are mutilated IS's (=Dybo's) tables:e.g.,B's blurred table of East-Ns
vowels replaces IS's precise table,-allowing B to reconstruct practically any
vowel in his "Ns roots",fully eliminating IS's precise rule "IE KV < Ns Ka; IE
KV < Ns KE; IE KʷV < Ns KU; instead of IS's Altaic triad Tˤ : T- : D- (con-
firmed in Starostin's excellent monograph on Alt. and the origin of Japane-
se) B has obsolete T- : D- only; his blurred system of correspondences does
not allow B to say Sem.>IE borrowings (actually present in B126, B246 etc.)
from Ns inheritance (IS saw this clearly in '64),etc,etc.   -V.S.

THE RUSSIANS ARE COMING! [to Ann Arbor] *on Febr. 15*

Starostin, Helimsky and Mudrak are coming to the U.S. for 3 months (we await them in mid-Febr.) Those interested in their lectures etc., - please contact U. of Mich., Slav. Dept, A² MI 48109-1275 : Ben Stolz, Kathrine Rowenchuk, or V.S.; tel. 313-764-5355 [note my address from Apr.1 through Jul.31: c/o Prof. W.Koch, Engl.Seminar, Ruhr-Universität Bochum, D-4630 Bochum 1, W.Germany. - V.S.]

Sergei Starostin reconstructed several NC [=North Caucas.] daughter languages; (with S.Nikolaev) NWestC, NEastC, and proto-NC; he wrote an etymol. dict. of NC. He made a major reconstructive work on Yeniseian; he published (in '89) a major book on Old Chinese phonology. He and I. Peiros re-reconstructed ST (= Sino-Tibetan) and wrote an etymol. dict. of ST. In '88, in Ann Arbor, Starostin compared Ns (= Nostratic) and SC (=Sino-Caucasian); this latter was reconstructed by Starostin and Nikolaev on the basis of NC, ST and Yeniseian. Starostin wrote an innovative work on glottochronology (to appear in '90). His book on Altaic and the origin of Japanese is forthcoming in Moscow, + *Hist. Phonetics of NC.*

Eugene Helimsky is a leading expert in Uralic (his book on this subject appeared in '82). Among his many papers there are three where Helimsky shows, with immaculate logic, why "Anti-Altaicists" are wrong in their fight against Nostratic.

Oleg Mudrak is a brilliant scholar of the youngest generation, specialist in Altaic and other "Eastern" languages, as well as in writing systems. His reconstruction of Eskimo-Aleutian appeared in Reconstructing Languages and Cultures (Bochum, Brockmeyer 1989); it was followed by his reconstruction of Chukchi-Kamchatkan ("Kamchukchee") in Explorations in Language Macro-Families (ibid.).

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"BOCHUM BOOKS" in 1990: Our second book appeared in Dec.'89 (Explorations ...; see above), but the third one is somewhat late: it is expected now at the end of March: Proto-Languages and Proto-Cultures (papers by Greenberg [on IE vowels and Eurasiatic], Menges [Altaic], Tyler [Ural. and Drav.], Bengtson [Sino-Caucas.], Orël and Starostin [Etruscan], Militarёv [Afro-Asiatic, or Afrasian]; Kaiser's translation of Illich-Svitych's headings from all 3 issues of his Nostr. dict., with a Semantic Index (English to Ns) and Dybo's phonetic tables); I tried to show, in an introduction, that Salishan (and, apparently, other Almosan-Keresiouan) languages belong to SC, and not to Amerind (Salishan shows all stablest elements: pron. 'I', 'thou'; numer. '2','3'; terms for body parts, etc. as clearly belonging to SC; in particular,   being similar to the archaic NC; Salishan — SC sound correspondences are very precise).+ *Peiros on S.E.Asia.*

Our 4th book, Sino-Caucasian Languages, is expected in April; it includes long papers on SC by Starostin, Nikolaev and Bengtson, as well as a long list of (some 2,000) NC roots reconstructed by St. and Nik.

Our 5th book (tentative title: Prehistory of Cultures and Languages) will have more papers (Blažek's on Kartv., and Austral., Orël's and Stolbova's on Afro-As., Peiros's on Austric etc.) as well as Jim Parkinson's index to all 5 volumes. All 5 books carry materials from our '88 Sympo-XXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXX sium on Language and Prehistory.

Other Bochum books: M.Kaiser, Lexical Archaisms in Slavic: From Ns to Common Slavic; a collection of papers on glottochronology; a collection on global linguistic connections,-and a few more.

★ ★ ★

Many are aware, I am sure, of important papers on brode comparison and deep reconstruction (including Ns) by Václav BLAŽEK (Leningradská 342, Příbram VII, 26102 Czechoslovakia).Blažek is an excellent scholar, but he has no time to study (he is a school teacher, busy all days in school). I wrote letters to President Havel; Ac.of Sc.;Univ.- urging to provide Blažek with a more appropriate job,-and I urge you to do the same.   -V.S.

PROBLEMS CONCERNING THE COMPARISON
OF KOREAN WITH OTHER LANGUAGES

Karl Krippes

In "Lexical Parallels between Proto-Indo-European and Other Languages", Allan R. Bomhard (1989:17) writes that he does not include Korean, Japanese-Ryukyuan, and Ainu in the Altaic group because: "Looking at just the core, one is hard-pressed to find features common to all three."[1] A similar amount of skepticism is expressed by Paul J. Hopper in his foreword to Bomhard (1984:vii): Bomhard's PIE and PAA evidence "is, for example, considerably richer than the evidence adduced by Samuel Martin and Roy Andrew Miller for a Japanese-Korean-Altaic hypothesis". It happens that Koreans too are beginning to look to languages and phyla other than Altaic for possible genetic connections with their native language. For example, Janhunen and Kho (1982:7) write: "Fifth, the linguistic connections of Korean with the non-'Altaic' languages of North Asia, notably and possibly Ainu, should not be neglected in the unfortunate situation of the general overemphasis on the 'Altaic' hypothesis." Along such lines of research, we may note James Patrie (1982) who favors Street's theory (1962) of a North-Asiatic proto-language which branched off into Proto-Altaic and a theoretically possible Korean-Japanese-Ainu unity. However, Patrie (1982:8) believes that this unity, which he calls "Proto-Pelagic", forms a branch of Proto-East Altaic, together with Proto-Mongol and Proto-Tungusic. Patrie supports his hypothesis with 140 Altaic-Ainu etymologies and 226 Korean-Japanese-Ainu etymologies.

In the same year, Kil Wun Kang published his 350 Ainu-Korean lexical comparisons, though only twenty-two of these agree with Patrie's work (Krippes 1990). Like Koppelmann (1933a, b), the Korean-Ainu comparisons of both Patrie and Kang fall flat on their face because of the inadequacy of their sources. In particular, an overreliance on Batchelor's Ainu dictionary will cause any comparative attempt to fail. Other problems in Patrie's Ainu material are discussed by Dettmer (1983:332). As pointed out by Krippes (1989:149), there are serious problems with the Korean data. For example, Korean čŏl- 'to be sleepy' corresponds to Ainu *tara* 'dream' (Patrie 1982:82); this Korean word is a development from an archaic form attested in the South Hamkyoung dialect of Korean as *čapuleymi* 'sleepiness' (Ramsey 1978:35), and Ainu *ota/osa* 'sand' to Korean *mora* id. (Patrie p. 99) seems implausible in view of South Hamkyoung *molkay* 'sand' (Ramsey 1978:38).

One should not infer that Kang (1982) is superior to Patrie. Both works contain similar shortcomings because "Korean dialect studies are fragmentary" (Kim and Park 1980:159). Because Northern Korean dialects preserve a linguistic stage older than what is known as Middle Korean, Nostraticists and Koreanists will eagerly await the appearance of J. R. P. King's Ph.D. thesis (Harvard University) which examines the pre-nineteenth century Northern Korean dialect materials. This, together with Ramsey (1978) and Choi

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[1]Some Proto-Altaic reconstructions used in the larger Nostratic sense do contain Korean forms. If the Korean forms are not cognate, it must be shown how, but they cannot be ignored.

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(1982), should allow for a clearer understanding of Proto-Korean, for now an obscure and misused term.

Nostraticists like Bomhard and Hopper need no longer be dissuaded by the Proto-Japanese-Korean reconstructions of Martin (1966). These have been reworked by John B. Whitman (1985), who presents a smaller but more systematic set of cognates and correspondences with equal attention to vocalic and consonantal segments. It is hoped that this Ph.D. thesis will soon be published in order to provide a more solid foundation for the comparison of Proto-Japanese-Korean forms with reconstructions from other languages and phyla, e.g., Proto-Nostratic *p[ʰ]aHw-/*p[ʰ]eHw- 'fire, flame, spark' (Bomhard 1989:27, no. 40), Proto-Japanese-Korean *pŏr 'fire' (Whitman 1985). This comparison harks back to Eckardt's comparison of Homeric Greek πῦρ 'fire' and Korean *pul/pur* id. (1966). His Indo-European-Korean hypothesis, like Koppelmann's (1933b) comparison of Korean with Indo-European, Sumerian, Gilyak, and Ainu, has met only ridicule among academic circles in Korea and the West. In view of Bomhard's promising work (1989), we owe it to ourselves to recheck these earlier attempts.

In examining the interrelationships of Korean, Japanese, and Ainu on the one hand, and the Paleosiberian languages on the other, there exists a problem of a "common Siberian substratum" (a phrase used by Janhunen 1977:128). Street (1983:198) remarks with great insight that: "I suspect that by comparing the lexicon of Gilyak, for example, with those of Korean, Japanese, and Ainu, we might detect early loanwords (possibly involving chains of borrowing) which would eventually supply us with formal criteria for distinguishing between borrowed and inherited lexical items. As matters stand now, the lack of such criteria forms a major stumbling block for comparisons of Ainu with Japanese, Korean, or Altaic." He cites (1983:203, fn. 15) Proto-Esquimo *qajuva- 'ladle', Ainu *kasu(p)* id., Old Japanese *kasipa* 'container for liquids', Proto-Altaic *kal²buga* 'spoon' as being quite probably "involved in some chain of borrowings." Professor Street writes to me in a letter dated 11/15/89 that this is one of the reasons why he no longer deals with Japanese-Altaic connections.[2]

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[2]After reading a draft of this paper, Street (12/13/89): "Poppe (1960:13), like Ligeti -- whom you cite p. 3 end of first full paragraph -- apparently disliked Ramstedt's connection of *tuksaki* with the rabbit word.

"Though I can't remember Miller's stating this in print, it seems to me that his idea about the prefix came from (a) Ramstedt's 'rabbit/jump' connection, and (b) occurrence of the OJ pair *Fasir-u* and *ta-Fasir-u* 'run'. (Unger had *ta-* as a 'prefix' here.)

"The former OJ form is pA *pelyi- (Street 1981:646; where I didn't dare mention Ainu *pas- ~ čas-* (Patrie 1982:70)). For a while I toyed with setting up a pA *tablyV- 'run, jump, flee' ?? < *ta- + pelyi-; cf. Mo. *tauli- ~ taɣuli-* 'chase, pursue', Chag. *tavuš-* 'run, jump' (TMEN 2.616).

"My favorite rabbit form is Kirakos *thalpqa* (Pritsak Fürstenliste 56-7).

"I once thought that Korean and Japanese might have incorrectly cut off what they thought was a *ta* prefix. I.e. Koguryo *wusiga < *(ta)blyi-gan, OJ *wusagi < *(ta)blyi-gay.

"The whole thing is a mess!!"

2

Soviet specialists in Paleosiberian, such as Kreinovich (1955) and Paniflov (1973) have also published evidence of a Gilyak-type substratum in Mongol, Tungusic, and Korean. However, some of the more ancient borrowings cannot be identified because "le nombre des correspondances constituant un certain système (autrement dit probables) est trop faible" (Kara 1965:24). Herein lies the danger of uncritical usage of the comparative Altaic grammars of Ramstedt (1957) and Poppe (1960). Sanžeev (1930:676) proposed that Mongolian *qalimu* 'whale' and Tungusic **kalima* id. were cognate. Rozycki (1983:177-78) could not pinpoint the direction of borrowing, but concluded: "The phonology of Tungus indicates that this is not native stock in Tu., but it is also unlikely to be native to Mo." These, along with Gilyak *qhalm* 'whale', were studied by Kreinovich (1955:154), who believes the Tungusic words to have been borrowed from Gilyak. Judging from Rozycki's comment, it seems probable that both Mongolian and Tungusic languages borrowed the word directly from Gilyak. Although Tsintsius et al. (1975:367) proposed a connection with Korean *korey* 'whale', Proto-Japanese Korean **kudori(ra)* id. (Whitman 1985:223) makes the resemblance no more than coincidental. This demonstrates that where Korean dialect information stops short, comparative data from Japanese proves invaluable.

To simply say that all of these languages are related, however distantly, does not free us as historical linguists from the burden of devising a suitable theory which "identifies the lines of cultural transmission in which regularities of change can be sought, and laws of linguistic evolution perhaps discovered: it provides a framework against which diffusion can be traced and within which earlier cultural content can be reconstructed; and it may reveal past connections and locations of cultures for which little or no other trace may remain" (Hymes 1959:50).

Let us see how this statement applies to the Northern Asiatic languages. Kang (1988:128) suggests that Old Korean **osikam* 'rabbit' is cognate to Gilyak *osk* id. However, Naert (1962:216-17) has demonstrated that this Gilyak word is borrowed from Sakhalin Ainu *osuke* 'rabbit'. The Old Korean word is attested in a place name, in Chinese transcription, in the *Samkuk saki* (1145 A.D.), and it has been compared with Old Japanese *usagi* 'hare' by Murayama (1961, 1962), Lee (1963, 1964), and Lewin (1973). Miller (1971:116-17) believes that Middle Korean *thoski* 'hare' is a derivative of the Proto-Japanese-Korean predecessor of the above Old Korean and Old Japanese words, whose prefis /ta-/ 'wild, untamed' only occurs in Japanese *tasigi* 'snipe', but also in Proto-Altaic **tab(ī)l²gan* 'hare' (Poppe 1960:13, 44, 77, 89). Patrie (1982:48) suggests that the underived Proto-Altaic **/-bl²g-/* 'rabbit' is cognate with the following Ainu forms: *isepo/osopo*, *osukep* (Taraika dialect), *osukex* (Shiraura, Aihama, Tonnai, Tarankomari dialects) all id. This etymology is accepted by Miller (1983:448) as a "convincing example od pA **l²*" which is "solid and irrefutable". Although I tend to accept Miller's theory of a prefix in the Middle and Modern Korean word for 'rabbit', an alternate explanation may be found in the Tungusic languages. Ramstedt (1949:283) suggested a connection between the modern Korean word and Evenki *tuksaki* 'hare' (dialectal *tusaki*, *tuhaki*), which he derived from the Evenki verb *tuksa-* 'to leap, to run jumpingly'. This Korean-Tungusic comparison seems plausible to Ligeti (1959:263, fn. 17), who believes these to be completely unrelated to the Proto-Altaic form.

The formulation Evenki *tuksaki* 'hare' < *tuksa-* 'to run' was validated by Soviet Tungusic specialists according to Poppe (1972:54), cf. Evenki *tukalagda* 'wild sow' < *tukala-*

'to besmear with mud'. Nonetheless, Menges (1975:40-43) believes that the Proto-Altaic , Tungusic, Korean, and Japanese words for 'hare, rabbit' are cognate, and perhaps further related to Mayan *tūl* id. and Tamil *čevijan* id. He also adds words of unknown origin belonging to the Siberian Russian dialects: *uškan* 'Hase' (Arkhangelsk, Sibiren, Orenburg Chkalov), *ušan* id. (Pskov). Without being in a position to comment on the significance of the Mayan and Tamil forms, I should point out that a consideration of the Gilyak and Ainu data is missing in Menges' comparisons. The Siberian Russian forms are undoubtedly borrowed from Gilyak or another Paleosiberian language. Yet, we can say nothing definite about the relationship, genetic or diffusional, between Ainu *osuke(p/x)*. Old Korean **usikam*, and Old Japanese *usagi*. Whitman (1985) has found that Ainu *časi* 'stockade' is a loanword from Old Japanese, in turn a loanword from Middle Korean. The subject marker /i-/ in Korean *čas-i* 'fortress', is one of the few clues as to the ultimate origin of the word (John B. Whitman, personal communication). If the resemblance between the Ainu, Korean, and Japanese words for 'hare, rabbit' is a diffusional one, we have for the time being nothing by a hypothesis for a *possible* direction of borrowing which is based on an isolated example, 'fortress'. Here, the comparative method breaks down.

I have presented these examples in a concise manner to illustrate the problem involved in comparing Korean (and Japanese and Ainu) with other languages. More dialectal information will be needed for Korean, as with any other language, to reduce the possibility of chance resemblance (see 'whale' above). Although the inter-relationships of Korean, Japanese, and Ainu will be a promising sub-field of research, the existence of a common Siberian stratum must be carefully studied before jumping to conclusions about genetic relationships. I am not a proponent of "wholesale borrowing" to explain the Altaic theory, nor do I which to reject outright the Nostratic theory as a conglomerate of diffusional and chance resemblances. Instead, I am proposing that the Altaic and North Asiatic languages be studied more thoroughly in order to gain more insight into the prehistoric relationships of these and the Paleosiberian language.

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AN END TO SPLENDID ISOLATION:
THE MACRO-CAUCASIAN PHYLUM

by John D. Bengtson

To this day, the belief that Basque and Burushaski are totally isolated languages is widely held. For example, in the current edition of Encyclopædia Brittanica (1983), Professor Luis Michelena of Salamanca states that "Basque remains an isolated language. . . . proof of a genetic relation beyond reasonable doubt appears remote." The unsigned article on Burushaski in the same encyclopedia states that the latter language is similarly "not known to be related to any other language in the world."

In his Guide to the World's Languages (1987: 377), Merritt Ruhlen still listed Basque and Burushaski as isolates, though now he accepts their Dene-Caucasian affiliation (Ruhlen 1989). In his review of Ruhlen's volume, Harold C. Fleming (1987: 197-198) mentions the Afroasiatic hypothesis of Basque origins, versus the Dene-Caucasian hypothesis, and indicates his favor of the latter, while also including Burushaski in "Vasco-Dene". In another review of Ruhlen's Guide, Václav Blažek (1989a:16) holds that "Burušaski is probably also related [to Sino-Caucasian = Dene-Caucasian], while the Sino-Caucasian affiliation of Basque and mainly that of Sumerian is only hypothetical." So while the Dene-Caucasian hypothesis has recently won converts among serious paleo-linguists, the general tone is still, understandably, cautious, implying that any genetic ties to the "Splendid Isolates" are remote at best.

The present writer is proposing that Basque and Burushaski are, in fact, fairly closely related to one another, and, together with (North) Caucasian, form a phylum-level group (taxonomically comparable to Indo-European) which I will here designate as "Macro-Caucasian". In this view, Basque, Caucasian, and Burushaski are family-level groups diachronically comparable to, e.g., Albanian, Slavic, and Armenian, while Macro-Caucasian is subordinate to the macro-phylum Dene-Caucasian (= Sino-Caucasian), and coordinate with the other phyla (Sumerian, Sino-Tibetan, Yeniseian, Na-Dene) making up Dene-Caucasian.

The evidence for Macro-Caucasian, of which only a fraction is presented below, consists of numerous lexical isoglosses in the most basic semantic fields (pronouns, parts of the body, natural phenomena, basic descriptives, animals and plants); intimate parallels have been observed in the inflection of nouns, pronouns, and verbs. The assessment of this evidence convinces me that the relationship is at a phylum level, with a diachronic depth of no more than six millennia.

If this proposed Macro-Caucasian phylum is indeed as closely knit as is claimed here, why has it eluded general recognition for so long? Obviously, Indo-European is a very special case (Fleming 1987: 160f), and has benefited from several historical advantages, including a large number of geographically concentrated (sub-)families, many with very early records, all of which has been subjected to huge amounts of scholarly attention. Macro-Caucasian, on the other hand, consists of three

widely separated branches, only one of which (Caucasian) has the benefit of rather scanty ancient records (Hatti, Hurrian, Urartean). I am certain that if we had records of the ancient predecessors of Basque and Burushaski, the general recognition of Macro-Caucasian would long since have been a _fait accompli_.

Other difficulties had to do with Caucasian specifically. Early Dene-Caucasian researchers, such as Trombetti and Bouda, treated Caucasian as including Kartvelian as well as North Caucasian, and they were bewildered by the wide array of divergent forms from both families. Recent studies, particularly by the Soviet Nostratic school, have clearly shown the distinct origins of Kartvelian (which is placed in the Nostratic macrophylum) and North Caucasian (Sino-Caucasian = Dene-Caucasian). Any common origin for the two Caucasian families would then have to be sought in the possible kinship of the Nostratic (Eurasiatic) and Dene-Caucasian macrophyla. (Fleming 1987: 163f.)

Another problem was resolved by the formulation, by Sergei A. Starostin and Sergei L. Nikolaev, of a reconstruction of proto North Caucasian. This, and their forthcoming etymological dictionary of North Caucasian, will be of immense benefit to the further development of Macro- and Dene-Caucasian studies.

In addition, I think a real obstacle to establishing genetic kinship for Basque and Burushaski has been their legendary status as "Splendid Isolates". The idea of Basque as a lonely, even noble, isolated language acquired a life of its own in the collective mind of mainstream linguistics. Even a softening of this absolute would lead naturally to the assumption that any linguistic kinship with Basque would have to be very remote. (Cf. Ruhlen 1987: 74-75.) The possibility that these "isolates" might actually belong with one another or with other languages in a common phylum, like Greek and Celtic, was rarely entertained.

The evidence for Macro-Caucasian presented here is not intended to be a definitive statement of proof. No doubt some will be convinced, while others will demand more. This presentation is admittedly fragmentary, but I hope it will be a stimulus to my colleagues to investigate for themselves, and contribute to the discussion for or against the hypothesis proposed here.

* * * * *

The following 77 etymologies represent a fraction of the lexicon common to two or more branches (families) of the Macro-Caucasian phylum. It will be seen that vocabulary is shared by all three branches in some cases, and by pairs of branches (Basque - Caucasian, Basque-Burushaski, Caucasian-Burushaski) in others. I have not found statistically significant evidence that any of these pairs is especially close, i.e., forming a subgroup to the exclusion of the other language.

Of the 77 etymologies, about 30 appear to be restricted to Macro-Caucasian, while the rest are also shared by the higher-level grouping, Dene-Caucasian. Even among the latter category, the Macro-Caucasian forms often show peculiarities of morphology or phonology. Besides about 65 etymologies from the most basic semantic fields, several etymologies involving domestic animals

would indicate that animal husbandry was well in place before the diaspora of Macro-Caucasian speakers. Other shared lexicon points to a homeland that was hilly or mountainous, and all Macro-Caucasian peoples, to this day, are inhabitants of high altitudes.

MACRO-CAUCASIAN PHONOLOGY

The following are some of the regular correspondences of initial consonants in the Macro-Caucasian phylum:

MC	Basque:	Caucasian:	Burushaski:
*p^h	p(h)	*p(h)	ph > pf
*b	b	*b (*$\bar{p}$)	b
*t^h	t(h)	*t(h)	th
*d	d	*d (*$\bar{t}$)	d
*k^h	h	*k(h)	kh
*g	g	*g (*$\bar{k}$)	g
*x	h	*x	h
*q^h	k(h)	*q(h)	qh
*G	g	*G	g/ɣ
*X	h	*X	x
*ʔ	h	*ʔ	ɣ
*ħ	h	*ħ	h
*h	Ø/h	*h	h
*s	z	*s	s
*š	s	*š	š
*c^h	s	*c(h)	ch
*čh	tz	*č(h)	čh

SOME GRAMMATICAL EVIDENCE

The following are some of the Macro-Caucasian nominal endings:

1. Bsq -k (ergative), -ik (ablative, partitive) : Cau *-k'V (abl., part., inst.) : Bur -Ak/-ɛk (inst.).
2. Bsq -i (dative) : NECau *-i/-y (dat.) : Bur -e (genitive).
3. Bsq -ra (aditive) : Cau *-rV (loc.) : Bur -ər (allative).
4. Bsq -ta (locative) : Cau *-tV (lat.) : Bur -Atɛ/-ɛtɛ (adessive).
5. Bsq -en (genitive), -n (inessive) : Cau *-nV (gen.) : Bur -Aŋɛ/-ɛŋɛ/-Anɛ (comitative, inessive).
6. Bsq -z (inst.) : Cau *-š(ē) (inst.).
7. NECau *-cV (inst., erg.) : Bur -cɛ/-ci (inst., contactive).

MACRO-CAUCASIAN: SOME LEXICAL EVIDENCE

BASQUE:	(North) CAUCASIAN:	BURUSHASKI:	

1. ni̱ 'I, me'   *ni̱   NSC 138

2.   *zo̱ 'I'   ǰa~ǰɛ~žA-   NSC 139

3.   *u̯o 'thou'   ū-n~ū-ŋ~ū-m   NSC 142

4. hi̱ 'thou'   *Gu (Dargwa ḥu) gu-,go-,-ku-,-ko-   NSC 143

5. zu̱ 'you' (pl.)   *ẕ́ᵂV (Lak zu̱)   NSC 144

6. -haur 'self'   -khər

7. ze̱-r 'what'   *s̄a (Dargwa se̱) be-sA-n   NSC 146

8. mi̱hi̱ 'tongue'   *mēlc'c'i   mɛlǯ 'jaw'   NSC 1

9. muthur 'snout'   *marƛ'V 'nose, beak' -mUltUr 'nostril'   NSC 23

10. bu̱ru 'head',   (Udi fo̱r 'hair',   būr 'hair', -l-pūr
    be-phuru 'eyelash' Kürin fi̱ri 'mane') 'eyelash'   NSC 13

11. hortz 'tooth'   -hAšɛ(-mɛ)'molar tooth'

12. bi-ho̱tz 'heart'   (m-)ōs '(her) heart'
                                                                        NSC 48

13. poto-rro 'vulva'   *pət'i 'hole, vulva'   S 149

14. bi-ẕka-r 'back'   (Abkhaz a-zkᵂa)   -sqa 'on one's back'
                                                                        NSC 39

15. ga̱l-tzar 'side'   *ʔ̄aGᵂāɬᵂV   NSC 41

16. sa̱ihets 'side'   -sōɣUt 'bosom, side'

17. he-ga̱l 'wing, fin'   gAl-gi   NSC 32

18. esku̱ 'hand'   hɛsk~hīsk 'wrist, back
                                              of the hand' NSC 29

19. u-ka̱b-il 'fist'   *q̄q̄ᵂaɪp̱V 'paw'   kAf 'claws, talons'
                                              (Blažek 1989b)

20. hanka 'thigh'   *hīānq̄q̄V

21. hoi̱n~hu̱ñ 'foot'   *ʔᵂin(i)-qᵂV 'heel, -ɣān~-ɣāɣAn 'heel'
                             ankle'

22. u-ko̱ndo 'elbow'   *q'ᵂəIntV 'elbow, knee'   SC 1.12

23. hu̱n~hu̱ñ 'marrow, *hᵂenʔV 'blood'   ʔ hAn- 'blood'   SC 7
            brain'   (in comp.)

24. go̱rotz 'dung'   ɣurAš

25. su̱ 'fire'   *c'aji̱ (Lak c'u) ši 'fireplace',   NSC 65
                                          šu-tin 'fire-stones'

26. i-za̱r 'star'   *Ha-ʒʒᵂarʕi   lA-čhār 'morning star'
                                                                        NSC 73

	(Basque)	(Caucasian)	(Burushaski)	
27.	hil(a) 'moon'		hAlAns	NSC 72
28.		*wǝmc'o 'moon'	hɛsa ~ hisa 'month'	S 129
29.	hauts 'dust'	*X̱urtV 'foam, scum'	xurc 'dust'	NSC 77
30.	i-bar 'valley'		bǝr	NSC 81
31.		*G̱ᵂērV 'stone'	ɣoro	S 80
32.	hortz 'cloud, sky'		xurōnǯ 'cloud, fog'	
33.	in-tzig-ar 'frost'	*čowq̱IV 'sleet'	čhAɣur- 'cold'	NSC 86
34.	e-lhu-r 'snow'	*λᵂiṇV (Čečen luo)	hỉo 'hail'	T 221
35.	txitxer /čičer/ 'hail'	*c'c'VrV 'ice, to freeze'		SC 5.7
36.	e-uri 'rain', hur-alde 'flood'	*ɣwer-t̄V 'rain'	hǝr-ālt 'rain'	NSC 85
37.	arrats 'night'	*Hᵂirinƛƛi		NSC 71
38.		*p̄VsV 'night'	bAsa	
39.	biha-r 'tomorrow'	*p̄ǝk̄V 'dawn'		T 62
40.	goiz 'morning' e-gun 'day'	*G̱ᵂemtV 'day'	gunc 'day' gŪn 'dawn'	NSC 69
41.	hun ~ hon 'good'	*hᵂin-ǯV		NSC 116
42.		*/ʔI/ȫX̱ᵂV 'good'	xǎ ~ xA	
43.	zahar 'old'	*sᵂɨrHo 'year, old'	chōr- 'ancient, of former times'	BNC 36
44.	mehe 'thin'	*ʕV-mVƛ'ƛ'V		T 235
45.	txiki /čiki/ 'small'	*ǯǯi/k'k'/V 'short'	čiki 'small'	T 78
46.	zuri 'white'	*hᵂö-č'ᵂörV 'gray, yellow'	šir-Aŋo 'white-faced' (of cattle)	NSC 124
47.		*ccakkᵂV 'white, yellow'	šIk-ǝrk 'yellow'	NSC 125
48.	hori ~ holĩ 'yellow'	*q'q'VhᵂVrV 'gray, brown'		
49.	hartz 'bear'	Dagestan *XIᵂVrćV 'squirrel, marten'		NSC 90
50.		*gᵂažē 'dog, wolf'	gAlžhu 'jackal'	NSC 88
51.	hor 'dog'	Nakh-Dagestan *xwar 'dog'		BNC 72a NSC 87
52.	sagu 'rat, mouse'	*cᵂarggᵂV 'weasel, marten' (Kabard. ʒǝRᵂa 'mouse')	čhǝrge 'flying squirrel'	NSC 92
53.	khur-lo 'crane'	*q'q'ɨrɨ-q'q'ᵂV 'crane'	qǝrū-ẙo 'wading bird, heron'	NSC 96
54.	piro 'duck'		phǝriš ~ pfǝriš	
55.	igel ~ ugaraxo, (etc.) 'frog'	*q'q'ᵂVrV-q'q'V (Khinalug qurkor)	ɣUrkUn ~ ɣorkUn ~ gUrqUc	NSC 95

(Basque)	(Caucasian)	(Burushaski)
56. sahats 'willow'		šAsk
57.	*k̄alV 'stick, pole, tree'	gAl-tər 'small branch'
58.	*t'VImV 'kernel of a fruit'	tuma ~ tumAŷ 'shell of nut, stone of fruit'
59. hur 'hazelnut'	*h̄ʷɔr-ƛ'ƛ̌'V 'nut'	NSC 105
60. intzaur 'nut'	*x̣üwx̄(ʷ)o	xUnzUr 'kernel of walnut' NSC 106
61.	*ƛ'ap'V 'leaf' (Adygh tHāp)	tAp NSC 104
62. i-tzar 'old ox'	*c̄(w)ärnV 'cow' (Rutul zär)	chɪər 'young ox, steer' KL 3
63. a-bere ~ a-bel- 'cattle'	*p̄öɫV	
64. zezen 'bull'		chin-dər
65. txahal /čahal/ 'calf, heifer'	*ʔIɫć'ʷɨlV	
66. ezne ~ esne 'milk'	*šVnHV	T 97
67.	*Hinisʷu 'cheese'	hAmɛž
68. bil-dots 'lamb'	*wil-q'ərV	bēl-is ~ bel-is 'ewe; sheep over two years old'
69.	*HowohV 'ram; he-goat'	huō 'sheep'
70. (Souletin) manexina 'ewe'	*näwšE 'sheep about 2 years old'	mamuši 'lamb' KL 10
71. ilhe 'wool'	*ʔəlXIV	
72. zikhiro 'castrated goat'	*c'c'ɪ̄kV 'goat, kid' (Andi c̄'ek'ir)	chɪgir 'she-goat' KL 7
73. urde 'pig, hog'	*wāIrƛ'ƛ'ə	KL 17
74. ezti 'honey'	*hʷi-mizzū (Avar hoc'c'o, Ingush moz)	mAžhi NSC 117
75. ar 'male'	*Hir-k'ʷV 'man'	hɪr ~ hir NSC 131
76. a-tso 'old woman'	*ʒʒʷVjV 'female' (Hatti zzuwa 'woman, wife')	-ūs ~ -us 'wife' NSC 135
77.	*q(w)änV 'woman' (Dagestan qʰVmV)	qUma 'concubine' NSC 134

Abbreviations:

BNC = Basque-North-Caucasian: Čirikba 1985

KL = Kuľt urnaja leksika: Starostin 1985

NSC = Notes on Sino-Caucasian: Bengtson 1989

S = Starostin 1989

SC = Sino-Caucasian: Starostin 1984

T = Trombetti 1926

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Typology, Relationship, and Time. Edited by Vitalij V. Shevoroshkin and Thomas L. Markey. Ann Arbor, MI: Karoma Publishers, 1986. Pp. xliv, 120.

Reviewed by ALLAN R. BOMHARD, *Boston, Massachusetts*

This book came into being through a proposal by one of the editors (Markey) that the other editor (Shevoroshkin) "make a selection of what he considered the best of recent pro and con Soviet work on or about Nostratic and that the two then translate, edit, and preface the resulting collection for publication". What finally emerged in the collection is mostly a group of articles published in the Soviet Union over the past two decades discussing the (mostly posthumously published) work of V. M. Illič–Svityč (Иллич–Свитыч) on Nostratic. To a far lesser extent, the work of A. B. Dolgopol'skij (Долгопольский) is also discussed.

The book begins with a Foreword, the first part of which appears to have been written by Markey alone, and the second part by Shevoroshkin alone. In the second and longest part, Shevoroshkin begins by giving a brief history of the development of Illič–Svityč's (and Dolgopol'skij's) ideas on Nostratic and expresses strong support for the Nostratic Theory in general and for Illič–Svityč's work in particular. He then makes three proposals of his own: (A) the Proto–Indo–European system of stops should be reinterpreted as *TH*, *T*, *D* (from Nostratic *T'*, *T*, *D*, respectively), (B) Proto–Indo–European had "strong" laryngeals as well as "weak" laryngeals (the so-called "strong" laryngeals survived in Hittite/Luwian, while the so-called "weak" laryngeals were lost), and (C) the laryngeals did not affect the quality (timbre) of contiguous vowels. Let us look more closely at each of these proposals.

A.   Shevoroshkin's ideas concerning Proto–Indo–European consonantism are not all that different from the proposals made by Joseph Emonds (1972). Where he runs into trouble is in trying to derive his revised system from Proto–Nostratic. One would like to know how the glottalized series became voiceless aspirates in Proto–Indo–European without merging with the plain voiceless stops somewhere along the way. When one tries to work through various scenarios to arrive at Shevoroshkin's revised Proto–Indo–European system from its alleged Proto–Nostratic antecedent, one runs into roadblocks at every turn. In

other words, you cannot get there from here (1).

B. On the surface, Shevoroshkin's theories concerning "strong" laryngeals and "weak" laryngeals in Proto-Indo-European appear intriguing. The problem is that the data do not fit the theory (2).

C. In order to be able to judge Shevoroshkin's theories concerning whether or not laryngeals changed the quality of contiguous vowels, one would have to know what phonetic properties he would assign to the laryngeals he posits. As long as he operates with cover symbols and employs ambiguous terminology, it is not possible to form an opinion one way or the other about the validity of his proposals.

Finally, Shevoroshkin bitterly attacks the work of Bomhard (1984) in highly emotional, intemperate language that can only be described as embarrassing. The discussion of Bomhard's work is characterized by outright misrepresentation. One gets the impression that Shevoroshkin did not read Bomhard's book through carefully or that, if he did, he did not understand what he read. Rather than engage in a lengthy rebuttal, the reader is invited to look at Bomhard's book for him/herself. As for the emotional nature of Shevoroshkin's attack on Bomhard, we may quote from Bertrand Russell (1976:116) and let it go at that:

> If an opinion contrary to your own makes you angry, that is a sign that you are subconsciously aware of having no good reason for thinking as you do. If some one maintains that two and two are five, or that Iceland is on the equator, you feel pity rather than anger, unless you know so little about arithmetic or geography that his opinion shakes your own contrary conviction. The most savage controversies are those about matters as to which there is no good evidence either way. Persecution is used in theology, not in arithmetic, because in arithmetic there is knowledge, but in theology there is only opinion. So whenever you find yourself getting angry about a difference of opinion, be

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1) We would expect the developments to have been more as follows than as proposed by Shevoroshkin: Proto-Nostratic *T', *T[h], *D > Proto-Indo-European *T, *Th, *D. A typological parallel exists within Semitic, where Proto-Semitic *T', *T[h], *D have developed into *T, *Th, *D in the Neo-Aramaic dialect of Ṭūr-ʿAbdīn.

2) For an excellent survey of the Laryngeal Theory, cf. Lindeman 1987; cf. also Winter 1965.

on your guard; you will probably find, on examination, that your belief is going beyond what the evidence warrants.

We can now consider, in turn, each paper in the collection:

V. V. Ivanov: "Proto-Languages as Objects of Scientific Description." (1980).

This paper is divided into three sections. In the first section ("The Difference between a Proto-Language and a Mere System of Correspondences"), Ivanov begins by outlining the methodology by which a system of correspondences is used to reconstruct a proto-language. He notes that correspondences may be the result of borrowings. Such cases cannot be used to establish genetic relationship but, rather, result from prolonged contact between two or more languages, which may or may not be otherwise related. Ivanov then considers two examples of correspondences between grammatical systems which cannot be explained by language contact: (A) the similarity between the earliest secondary verbal endings reconstructed for Proto-Indo-European and those assumed for Proto-Kartvelian and (B) the similarity of heteroclisis in neuter (inanimate) nouns in Indo-European and Dravidian. According to Ivanov, both of these examples can be explained within the framework of the Nostratic Hypothesis. Ivanov concludes this section by expressing strong support for the Nostratic Hypothesis, particularly the version of this theory advocated by Illič-Svityč. Ivanov claims that the similarities between the various branches of Nostratic are not due to borrowing but are, on the contrary, indicative of genetic relationship. My one comment here is that I would have liked to have seen more examples and more discussion: what Ivanov has to say is extremely exciting, but he teases us by whetting our appetites and then sending us home hungry.

In the second section ("The Distinction between Proto-Languages and Intermediate States of Dialectal Evolution: The Problem of Minimizing the Number of Proto-Languages"), Ivanov seeks to answer the question of how many proto-languages can and/or should be posited for the prehistory of every single language in the world. He presents several arguments against the Indo-Hittite Hypothesis to support his view that nothing is gained by positing more proto-languages than is warranted by the evidence. While I agree in the main with the point he is trying to make, I think it necessary to mention that Ivanov's position

regarding the placing of the Anatolian languages (3) within Indo-European is by no means universally accepted. On the contrary, while recognizing that the Anatolian languages have innovated in a number of areas, there are some scholars who find in the many archaic features preserved by the Anatolian languages sufficient evidence to suggest that these languages became separated from the mainstream of Indo-European at a very early date. Next, Ivanov discusses the difficulties involved in trying to determine the exact internal boundaries that delineate a language as distinct from a dialect. He formulates a general principle that one should always attempt to minimize the number of languages and should not consider as independent languages those dialects that have become severed from the main speech community or other cases in which specific social and cultural-historical conditions did not conspire to designate a dialect as an independent language. According to Ivanov, not only should we minimize the number of languages, but we should also minimize the number of intermediary proto-languages.

In the final section ("The Descriptive Strength of a Proto-Language"), Ivanov discusses the need to include the principal proto-languages in any survey dealing with "The Languages of the World". He notes specifically that the inclusion of entries covering all of the principal proto-languages will permit one to clarify the description of individual languages substantially.

It is quite clear from the thrust of his argumentation that Ivanov belongs to the school of Linguistics that views reconstructed languages as real languages that existed at a particular point in time and not as a mere set of correspondences. This is a position that I would wholeheartedly endorse (4).

This paper, though interesting in its own right, is really not on or about Nostratic. Rather, it deals specifically with the reconstruction of proto-languages, with the question of how many proto-languages should be posited, and with how proto-languages can be utilized. No doubt, Ivanov's paper was included in this collection solely because he used Nostratic examples to illustrate the points under discussion.

Aaron B. Dolgopolsky. "A Probabilistic Hypothesis concerning the Oldest

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3) Specifically, Hittite, Palaic, (Cuneiform) Luwian, and Hieroglyphic Luwian as the oldest representatives of this branch. Lycian, Lydian, and Carian are later representatives.

4) I would like to emphasize that reconstructed languages should be thought of as real languages in every sense of the term. This means that we should be very careful not to reconstruct anything that is not characteristic of language in general: our goal should be to strive for reality in our reconstructions.

Relationship among the Language Families of Northern Eurasia." (1964).

In an Introductory Note written specifically for the English language version of this paper included in the present collection, Dolgopolsky (Dolgopol'skij) explains that this paper was written over two decades ago and that several of the examples should now be discarded in view of subsequent research. Consequently, he asks that readers regard this paper not as an etymological one but as a methodological one instead.

Dolgopolsky starts out by proposing a procedure for proving putative genetic relationship between languages. In particular, he recommends two approaches: (A) comparison of several languages and (B) statistical selection of semantic values represented by morphemes which are relatively impervious to change. He elaborates on each of these approaches, especially the latter. Dolgopolsky's second approach is reminiscent of the technique known as lexicostatistical glottochronology championed by Morris Swadesh and is thus subject to the same reservations which many linguists have expressed about glottochronology in general.

Dolgopolsky selects a list of fifteen semantic values ranked according to their degree of morphemic stability and then compares examples from Indo-European, Hamito-Semitic (now more commonly known as Afroasiatic or Afrasian), Uralic, Altaic, Chukchee-Kamchatkan (more properly, Chukchi-Kamchatkan), and Kartvelian in light of these fifteen categories. As is to be expected from a pioneering effort such as this, some of the examples are quite good, while others are best forgotten. After analyzing these data, Dolgopolsky concludes that the correspondences cannot be explained by either chance or borrowing but, on the contrary, point to genetic relationship.

This is the only paper in the collection that presents original research on the Nostratic Hypothesis. All of the other papers (except that of Gamkrelidze and Ivanov, which really does not belong in this collection), in one way or another, merely comment on the research done by others.

V. V. Ivanov. Review of Illič–Svityč, Опыт сравнения ностратических языков (семитохамитский, картвельский, индоевропейский, уральский, дравидийский, алтайский) (*An Attempt at a Comparison of the Nostratic Languages [Hamito-Semitic, Kartvelian, Indo-European, Uralic, Dravidian, Altaic]*). **Vol. I. (1971).**

V. V. Ivanov. Review of Illič–Svityč, Опыт сравнения ностратических языков (семитохамитский, картвельский, индоевропейский, уральский, дравидийский, алтайский) (*An Attempt at a Comparison of the Nostratic Languages*

[Hamito–Semitic, Kartvelian, Indo–European, Uralic, Dravidian, Altaic]). **Vol. II. (1976).**

These two papers are best considered together since both deal with V. M. Illič–Svityč's posthumously published comparative Nostratic dictionary (which, it may be mentioned, is still in the process of publication) (5).

Ivanov's reviews of Illič–Svityč's work are extremely positive. He points out that Illič–Svityč's work differs from earlier attempts (6) by the exceptional precision of his methodology, which can be seen from his scrupulous selection of material and the exhaustiveness of his preliminary investigations of data from within the language families that are compared. Ivanov notes, moreover, that Illič–Svityč's work demonstrates the explanatory power of the Nostratic Hypothesis by showing that a large number of facts which remained inexplicable within the framework of a given language family can be explained from the larger Nostratic perspective. Ivanov then backs up his assertion with several convincing examples. I endorse Ivanov's enthusiasm, though I do not necessarily agree with all of Illič–Svityč's proposals.

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5) An English language version of this monumental work is being prepared under the direction of Vitalij Shevoroshkin.

6) Ivanov (p. 2 and p. 57) faults preliminary work by Bomhard for being merely a binary comparison of Indo–European and Afroasiatic (Hamito–Semitic). It should be mentioned, however, that, in his recent book, Bomhard (1984:291) unreservedly acknowledges the need to bring in the remaining Nostratic daughter languages. In his book, Bomhard is quite explicit in noting that his goal is limited in scope and is not to reconstruct Proto–Nostratic but, rather, to apply a new approach to the comparison of Proto–Indo–European and Proto–Afroasiatic to determine whether or not there is sufficient evidence to consider the possibility that these two language families are in fact genetically related. He concludes, by the way, that the evidence points strongly to genetic relationship, albeit distant. Over the past several years, Bomhard has been gathering data from the other Nostratic daughter languages and currently has material supporting approximately 500 possible Nostratic etymologies. Analysis of these data has led Bomhard to conclude that the correspondences established by Illič–Svityč between Indo–European, Uralic, Altaic, and Dravidian are generally valid, while those between Kartvelian, Afroasiatic, and the remaining Nostratic daughter languages are in need of revision. The full corpus of lexical data (running to well over 10,000 cited forms from the various Nostratic daughter languages) to support Bomhard's views will appear in a joint monograph by Bomhard and John C. Kerns currently in preparation and tentatively entitled *The Nostratic Macrofamily.*

B. A. Serebrennikov. "On the So–Called 'Nostratic' Languages." (1983).

Serebrennikov is highly critical of Illič–Svityč's work. In spite of the fact that Serebrennikov's paper contains several factual errors (pointed out by the editors of this collection), his criticisms merit careful consideration.

Serebrennikov remarks that current linguistic investigation is attempting to establish macrophyla that include an extraordinarily large number of languages. This endeavor is based on three principles:

A.   The unification of a vast number of languages into one macrofamily broadens the framework of historical and developmental perspectives enormously.   _

B.   The greater the number of genetically related languages in a given family, the more probable the preservation of some exceedingly ancient archaisms.

C.   The discovery of large macrofamilies could contribute to a more refined definition of the geographical displacement of related languages in the distant past.

Serebrennikov then asks whether genetic relationship is ever really proven and, if so, how can one verify it.

Serebrennikov feels that the relationship of grammatical formants is more relevant for determining genetic relationship than is a comparison of lexical roots. He notes that Illič–Svityč compared not only lexical items but also grammatical formants. Serebrennikov then details the similarities and differences between selected grammatical formants in various Nostratic languages. After completing this review, he concludes that the grammatical data for Nostratic, especially as it relates to Finnish, had, in the main, been known previously, are sporadic, and are frequently not very persuasive. In a couple of footnotes, the editors take issue with Serebrennikov's conclusions at this point, and I would tend to agree with them. For my part, I fail to see how one cannot be impressed with the high quality of the grammatical correspondences uncovered by Illič–Svityč. Of course, one can quibble here and there and offer alternative interpretations, but who can deny that Illič–Svityč has gathered together an impressive amount of data from a vast and highly diverse number of languages and has presented his findings in a systematic, well–organized manner, while, at the same time, offering new perspectives on extraordinarily complex issues?

Next, Serebrennikov discusses Nostratic phonology. He finds it difficult to

believe that the Nostratic vowels were retained without essential modification from early Nostratic through Proto-Uralic and right down into Finnish. This is a good point (7). Serebrennikov doubts whether Nostratic had laryngeals. Here again the editors take issue with Serebrennikov, and I would agree with them here too.

Serebrennikov's final conclusions are that Nostratic theory does not provide anything new for the history of Finnish, that unfortunate results emerge from its application to both Turkic and Mongolian languages, that the whole system of Illič-Svityč's arguments in favor of a genetic relationship among Nostratic languages has serious drawbacks, that the Nostratic character of Hamito-Semitic (Afroasiatic) is doubtful (as is a genetic relationship between Finnish and Kartvelian), and that, as a result of these shortcomings, the genetic relationship of the so-called "Nostratic" languages is insufficiently proven.

T. V. Gamkrelidze and V. V. Ivanov. "On the Reconstruction of the Proto-Indo-European Stops: Glottalized Stops in Indo-European." (1980).

It is a mystery to me why this paper was included in the present collection: the subject matter has little to do with Nostratic proper (though there are implications for the comparison of Indo-European with the other Nostratic languages), and the views of Gamkrelidze and Ivanov are well-known and readily available elsewhere. Therefore, this paper will be discussed in only the briefest of terms.

Internal inconsistencies in the traditional reconstruction of the Proto-Indo-European stop system make that system highly improbable from a typological point of view. Reinterpretation of the traditional plain voiced stops (*b, *d, *g, *$g\mu$) as glottalized stops (that is, ejectives: *p', *t', *k', *$k'w$ respectively) accounts better for the distributional patterning of this series than does the traditional reconstruction. Furthermore, according to Gamkrelidze and Ivanov, the traditional plain voiceless stops (*p, *t, *k, *$k\mu$) are to be reinterpreted as voiceless aspirates (*ph, *th, *kh, *k^wh respectively), while the traditional voiced aspirates (*bh, *dh, *gh, *$g\mu h$) are to remain unchanged. In this revised interpretation, aspiration is viewed as a redundant feature, and the phonemes in question could also be realized as allophonic variants without aspiration.

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7) My own analysis has led me to conclude that the front rounded and back (or central) unrounded vowels characteristic of most Uralic languages are innovations and are not to be reconstructed for Proto-Nostratic, and the same can be said for the front rounded and back unrounded vowels found in Altaic languages.

Traditional Proto-Indo-European			Gamkrelidze and Ivanov		
A	B	C	A	B	C
(b)	*bh*	*p*	*(p')*	*bh/b*	*ph/p*
d	*dh*	*t*	*t'*	*dh/d*	*th/t*
g	*gh*	*k*	*k'*	*gh/g*	*kh/k*
gu̯	*gu̯h*	*ku̯*	*k'ʷ*	*gʷh/gʷ*	*kʷh/kʷ*

The revisions proposed by Gamkrelidze and Ivanov provide new insights into the underlying principles governing Grassmann's Law, Bartholomae's Law, and the Indo-European root structure constraints.

I have expressed strong support for the revisions proposed by Gamkrelidze and Ivanov (as well as Paul Hopper) elsewhere.

Concluding Remarks:

Though the editors of this collection are to be commended for making available papers on recent Soviet scholarship on Nostratic, the book is also a disappointment. Three of the papers are by Ivanov, two of which are reviews by him of Illič-Svityč's comparative Nostratic dictionary. These reviews are interesting in themselves, and Ivanov is a great linguist in his own right, but I believe that the majority of scholars would prefer to have the original source material in front of them so that they can make their own evaluation. When one considers all that could have been included in this collection (such as the many exciting papers on Nostratic published by Illič-Svityč as well as Dolgopol'skij in *Ėtimologija* [Этимология] (8)), one can only feel cheated by what the editors have chosen for us to see. Moreover, the book does not do justice to the important role played by Dolgopol'skij (who, by the way, is still making important contributions to Nostratic

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8) An indispensable reference, citing nearly everything that has ever been published on distant linguistic relationship, has recently been published by Marge E. Landsberg (1986). Here, one will find listed the articles by Illič-Svityč, Dolgopol'skij, and other Soviet (as well as non-Soviet) scholars on Nostratic.

studies). Finally, it must be noted that the book is extremely poorly edited: I counted nearly thirty typographical errors in the Foreword alone, some of which are trivial but others of which are quite egregious.

ADDENDUM

In this Addendum, I would like to make several comments of my own on recent Soviet research on Nostratic. Specifically, I will deal with this research as it has been codified in Illič–Svityč's comparative Nostratic dictionary (9). Let me begin by stating unequivocally that I have the highest admiration for what Soviet scholars have achieved. Their research has opened up new and exciting possibilities and has given Nostratic studies new respectability. However, this does not mean that I agree with everything they say. I regard their work as a pioneering effort and, as such, subject to modification in light of recent advances in linguistic theory, in light of new data from the Nostratic daughter languages (10), and in light of findings from typological studies that give us a better understanding of the kind of patterning that is found in natural languages as well as a better understanding of what is characteristic of language in general, including language change.

I agree with Illič–Svityč that, at a minimum, the following language families are likely to belong to Nostratic: Indo-European, Kartvelian, Afroasiatic, Uralic–Yukaghir, Elamo–Dravidian, and Altaic (which includes Mongolian, (Manchu–)Tungus, and (Chuvash–)Turkic but not Japanese-Ryukyuan, Korean, and

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9) I would like to thank A. B. Dolgopol'skij for his great kindness and generosity in giving me (on August 22, 1983, while he was visiting Boston) copies of his and Illič–Svityč's *Étimologija* (Этимология) articles on Nostratic as well as copies of volumes I and II of Illič–Svityč's comparative Nostratic dictionary. I would also like to thank my friend Yoël L. Arbeitman for sending me (on October 14, 1985) a copy of the first fascicle of volume III of Illič–Svityč's dictionary. Unfortunately, in spite of my best efforts to get my hands on a copy of Illič–Svityč's dictionary so that I could consult it in preparing my book *Toward Proto-Nostratic* (published in 1984), I was unsuccessful. When I finally received volumes I and II of Illič–Svityč's dictionary from Dolgopol'skij, the camera–ready manuscript of my book had already been sent to the publisher. To those who have wondered why Illič–Svityč was not given more credit in my 1984 book, the answer should now be obvious -- one cannot cite nor comment upon what one has not seen.

10) I am thinking here especially about the advances being made in Cushitic and Chadic studies within Afroasiatic.

Ainu, which must be treated separately). It is probable that several other language phyla also have genetic links to the language families just listed; these include, in particular, Chukchi-Kamchatkan, Eskimo-Aleut, and Gilyak (also called Nivkh). Recent research by Claude Boisson shows that Sumerian also has important links with the Nostratic macrofamily, though the connections are not as unambiguous as one would like.

We can begin by looking at phonology. In 1972 and 1973, the Soviet scholars T. V. Gamkrelidze and V. V. Ivanov proposed a radical reinterpretation of the Proto-Indo-European stop system (for details, see the discussion of Gamkrelidze and Ivanov's contribution to this collection above). According to their reinterpretation, the Proto-Indo-European stop system was characterized by the three-way contrast glottalized ~ voiceless (aspirated) ~ voiced (aspirated). In this revised interpretation, aspiration is viewed as a redundant feature, and the phonemes in question could also be realized as allophonic variants without aspiration. A similar proposal was made by Paul J. Hopper at about the same time.

This reinterpretation opens new possibilities for comparing Proto-Indo-European with the other Nostratic daughter languages, especially Proto-Kartvelian and Proto-Afroasiatic, each of which had a similar three-way contrast. The most natural and straightforward assumption would be that the glottalized stops posited by Gamkrelidze and Ivanov for Proto-Indo-European would correspond to glottalized stops in Proto-Kartvelian and Proto-Afroasiatic, while the voiceless stops would correspond to voiceless stops and voiced stops to voiced stops. This, however, is quite different from the correspondences proposed by Illič-Svityč. He sees the glottalized stops of Proto-Kartvelian and Proto-Afroasiatic as corresponding to the traditional plain voiceless stops of Proto-Indo-European, while the voiceless stops in the former two branches are seen as corresponding to the traditional plain voiced stops of Proto-Indo-European, and, finally, the voiced stops to the traditional voiced aspirates of Proto-Indo-European. Illič-Svityč then reconstructs Proto-Nostratic on the model of Kartvelian and Afroasiatic with the three-way contrast glottalized ~ voiceless ~ voiced, thus (Illič-Svityč's full reconstruction is given):

p'	p	b			m			w
ṭ	t	d			n	l	r	
ç	c	ʒ	s	z				
ḉ	ć	ʒ́	ś	ź (?)	ń	l'	r'	j

$\ch{c}$	$\check{c}$	$\check{z}$	$\check{s}$		$\tilde{n}$	l
			$\acute{s}$	λ		
$k̦$	k	g				
$\dot{q}$	q	g				
			$ḥ$	$ſ$		
$ʔ$			h			

The mistake that Illič–Svityč made was in trying to equate the glottalized stops of Proto–Kartvelian and Proto–Afroasiatic with the traditional plain voiceless stops of Proto–Indo–European. His reconstruction would make the glottalized stops the LEAST marked members of the Proto–Nostratic stop system. Illič–Svityč's reconstruction is thus in contradiction to typological evidence, according to which glottalized stops are uniformly the MOST highly marked members of a hierarchy (11). The reason that Illič–Svityč's reconstruction would make the glottalized stops the least marked members is as follows: Illič–Svityč posits glottalics for Proto–Nostratic on the basis of one or two seemingly solid examples in which glottalics in Proto–Afroasiatic and Proto–Kartvelian appear to correspond to traditional plain voiceless stops in Proto–Indo–European. On the basis of these examples, he assumes that, whenever there is a voiceless stop in the Proto–Indo–European examples he cites, a glottalic is to be reconstructed for Proto–Nostratic, even when there are no glottalics in the corresponding Afroasiatic and Kartvelian forms! This means that the Proto–Nostratic glottalics have the same frequency distribution as the Proto–Indo–European plain voiceless stops. Clearly, this cannot be correct. To bring the reconstruction of Proto–Nostratic into agreement with the typological evidence, the correspondences between the various branches of Nostratic should be modified as follows (using just the bilabial, dental, and velar series for illustration):

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11) For details on phonological markedness in general and on the frequency distribution of glottalized stops in particular, cf. Gamkrelidze 1978.

Proto–Nostr.	Proto–IE	Proto–Kartv.	Proto–Afroas.	Proto–Uralic	Proto–Dravidian	Proto–Altaic
b–	b[h]–	b–	b–	p–	p–	b–
-b-	-b[h]-	-b-	-b-	-w-	-pp-/-vv-	-b-
p[h]–	p[h]–	p[h]–	p[h]–	p–	p–	p–
-p[h]-	-p[h]-	-p[h]-	-p[h]-	-p-	-pp-/-v-	-p-/-b-
p'–	(p'–)	p'–	p'–	p–	p–	p–
-p'-	(-p'-)	-p'-	-p'-	-p-	-p(p)-	-b-
d–	d[h]–	d–	d–	t–	t–	d–
-d-	-d[h]-	-d-	-d-	-δ-	-t(t)-	-d-
t[h]–	t[h]–	t[h]–	t[h]–	t–	t–	t–
-t[h]-	-t[h]-	-t[h]-	-t[h]-	-t(t)-	-t(t)-	-t-
t'–	t'–	t'–	t'–	t–	t–	t–
-t'-	-t'-	-t'-	-t'-	-t-	-t(t)-	-d-
g–	g[h]–	g–	g–	k–	k–	g–
-g-	-g[h]-	-g-	-g-	-γ-	-k-	-g-
k[h]–	k[h]–	k[h]–	k[h]–	k–	k–	k–
-k[h]-	-k[h]-	-k[h]-	-k[h]-	-k(k)-	-k(k)-	-k-/-g-
k'–	k'–	k'–	k'–	k–	k–	k–
-k'-	-k'-	-k'-	-k'-	-k-	-k(k)-	-g-

The main consequence of Illič–Svityč's mistaken equation of the glottalized stops of Proto–Afroasiatic and Proto–Kartvelian with the traditional plain voiceless stops of Proto–Indo–European is that he is led to posit forms for Proto–Nostratic on the basis of theoretical considerations but for which there is absolutely no evidence in the Nostratic daughter languages. Let us look at one or two such examples:

A.   ḳ/o/ (enclitic) particle: Afroasiatic *k(w)*, Kartvelian *kwe*, Indo-European *kʷe*, Uralic *-ka/-kä*, Altaic *-ka* (Illič-Svityč 1971- .I:325-26, no. 201).

Note that, in this example, there is no evidence in any of the Nostratic daughter languages pointing to an initial glottalized stop in the Nostratic parent language. There are many more such examples.

B.   ḳaba/ḳap'a "to snatch, to seize": Afroasiatic *qb-*, Kartvelian *ḳb-*, Indo-European *ghabh-/kap-*, Uralic *kappʌ-*, Dravidian *kavv-/kapp-*, *kava-*, Altaic *k'aba-/k'apa-* (Illič-Svityč 1971- .I:313-15, no. 190).

Here, Illič-Svityč does not even follow his own sound laws. A better etymology would be:

Proto-Nostratic *$k^{[h]}ap^{[h]}$- "to snatch, to seize": Proto-Afroasiatic *$k^{[h]}ap^{[h]}$- "to take, to seize; palm of the hand" (cf. Akkadian *kappu* "hand"; Arabic *kaff* "palm of the hand, hand"; Egyptian *kp* "to seize; hollow of the hand or foot"; Ma'a *-kupurúya* "to snatch"); Proto-Indo-European *$k^{[h]}ap^{[h]}$- "to take, to seize" (cf. Latin *capiō* "to take, to seize"; Old High German *haft* "captivity"); Proto-Uralic *kappa- "to take, to seize" (cf. Finnish *kaappaus* "capture"; Mordvin (Erza dialect) *kapode-* "to grab quickly"); Proto-Altaic *kapa- "to seize, to snatch" (cf. Turkish *kapan* "one who seizes or grabs", *kapıcı* "one who seizes", *kapmak* "to snatch, to seize, to carry off, to acquire", *kapış* "manner of seizing, looting", *kapma* "act of seizing").

What about those examples adduced by Illič-Svityč which appear to support his proposed correspondences? Some of these examples admit to alternative explanations, while others are questionable from a semantic point of view and should be abandoned. Once these examples are removed, there is an extremely small number (no more than a handful) left over that appear to support his position. However, compared to the MASSIVE counter-evidence in which glottalized stops in Kartvelian and Afroasiatic correspond to similar sounds (the traditional plain voiced stops) in Indo-European, even these residual examples become suspect.
    Now we can take a look at a few counter-examples (this is but a small sampling):

A.   Proto-Nostratic *$t'ar-p^{[h]}$-/*$t'ər-p^{[h]}$- "to tear, to rend, to pluck": Proto-Indo-European *$t'r-ep^{[h]}$-/*$t'r-op^{[h]}$- "to pluck" (cf. Greek δρέπω "to pluck, to cull"); Proto-Kartvelian *$t'r-p^{[h]}$- "to enjoy" (semantic development

as in Hebrew (hif.) *ha-ṭrīφ* "to let someone enjoy (food)"; cf. Georgian *t'rp-* "to cherish"); Proto-Afroasiatic **t'ar-p[h]-/*t'ər-p[h]-* "to tear, to rend, to pluck" (cf. Hebrew *ṭāraφ* "to tear, to rend, to pluck").

B.   Proto-Nostratic **t'aħ-/*t'əħ-* "to split":   Proto-Indo-European **t'eḫḫ-* > **t'ā-* (extended form **t'eḫḫ-y/i-*) "to cleave asunder, to divide" (cf. Sanskrit *dáti, dyáti* "to cut, to divide", *dáyati* "to divide, to destroy, to cleave, to slay, to smite, to rend, to tear, to divide"; Greek δαίω "to divide"); Proto-Kartvelian **t'ex-* "to break" (cf. Georgian *t'ex-a* "to break"); Proto-Afroasiatic **t'aħ-/*t'əħ-* to break, to split, to divide" (cf. Arabic *ṭaḥana* "to grind, to mill, to pulverize, to crush, to ruin, to destory", *ṭaḥṭaḥa* "to break, to smash, to shatter something"; Geez / Ethiopic *ṭaḥama* "to split in half, to thin out plants"; Iraqw *daħ-* "to knock over, to knock down"; Dahalo *ḍaħ-* "to pound").

C.   Proto-Nostratic **k'ar-/*k'ər-* "to gather (together)":   Proto-Indo-European **k'er-/*k'or-/*k'r̥-* "to gather (together)" (cf. Greek ἀγείρω "to get together, to gather, to collect"; Latin *grex* "flock, herd"); Proto-Kartvelian **k'er-b-*, **k'r-eb-* "to gather" (cf. Georgian *k'reb-/k'rib-* "to gather", *k'erb-* "to gather"), **k'r-ep[h]-* "to gather, to pick (fruit, flowers)" (cf. Georgian *k'rep-/k'rip-* "to gather, to pick (fruit, flowers)"); Afroasiatic: Arabic *karada* "to collect, to gather, to hoard up"; Dravidian: Tamil *karrai* "collection, bundle"; Malayalam *karra* "bundle, sheaf of corn".

D.   Proto-Nostratic **k'ab-/*k'əb-* "to seize, to take hold of; to seize with the teeth, to bite":   Proto-Indo-European **k'eb[h]-/*k'ob[h]-* "to munch, to chew; jaw" (cf. Modern Irish *gob* "beak, mouth"; German *Kebe* "fish-gill"; Lithuanian *žěbeti* "to munch"); Proto-Kartvelian **k'b-in-* "to bite" (cf. Georgian *k'b-en-a* "to bite"); Proto-Afroasiatic **k'ab-/*k'əb-* "to seize, to take hold of" (cf. Arabic *kabaḍa* "to seize, to take, to grab, to grasp, to grip, to clutch, to take hold, to take possession, to hold"; Oromo *k'ab-* "to possess, to take hold of"); Dravidian: Tamil *kavvu* "to seize with the mouth, to grasp with eagerness; (n.) bite, seizing by the mouth (as dog), eating"; Koḍagu *kabb-* "to seize with wide-open mouth (of dogs, tigers, etc.)"; Tuḷu *kappuni* "to eat greedily".

E.   Proto-Nostratic **k'ap[h]-/*k'əp[h]-* "jaw, jawbone":   Proto-Indo-European **k'ep[h]-/*k'op[h]-* "jaw, mouth" (cf. Avestan *zafarə, zafan-* "mouth (of evil beings)"; Old Icelandic *kjaptr, kjöptr* "jaw"); Proto-Kartvelian **ni-k'ap[h]-* "jaw" (cf. Georgian *nik'api* "jaw"); Dravidian: Tamil *kavuḷ* "cheek, temple or jaw of elephant"; Parji *gavla* "jaw"; Malayalam *kaviḷ* "cheek".

F. Proto-Nostratic *k'ar-/*k'ər- "to turn, to bend, to wind; curved, bent, crooked": Proto-Indo-European *k'er-/*k'or-/*k'r̥- "to turn, to bend, to wind; curved, bent, crooked" (cf. Sanskrit *granth-*, *grathnā́mi* "to twist, to tie"; Armenian *kurc* "core, stump", (pl.) *kurkʰ* "breasts"; Old Church Slavic *grudь* "breast"); Proto-Kartvelian *mk'erd-* "breast" (cf. Georgian *mk'erdi* "breast"), *k'ar-/*k'r- "to bind, to tie together" (cf. Georgian *k'ar-/k'r-* "to bind, to tie together"); Dravidian: Tamil *karaṭu* "ankle, knot in wood".

G. Proto-Nostratic *k'al-/*k'əl- "to lift, to raise up, to make high, to elevate; lifted up, elevated, high; highest point, top": Proto-Indo-European *k'el-/*k'ol-/*k'l̥- "to lift, to raise up, to make high, to elevate; highest point, top" (cf. Old Icelandic *kollr* "top, summit, the head, crown"; Old English *clif* "cliff, rock, promontory, steep slope"); Kartvelian: Svan *k'əltxi* "high", *nak'lätxi* "height"); Proto-Afroasiatic *k'al-/*k'əl- "to lift, to raise up, to make high, to elevate; lifted up, elevated, high; highest point, top" (cf. Arabic *ḳalla* "to pick up, to raise, to lift", *ḳulla* "highest point, top, summit, apex"; Shilha *aḳəllal* "head").

H. Proto-Nostratic *k'aw-/*k'əw- "to make a round hole in; round; hole": Proto-Indo-European *k'ew-/*k'ow-/*k'u- "to make a round hole in" (cf. Greek γύπη "a hole", γῡρός "round", γῦρος "ring, circle"); Proto-Kartvelian *k'w-er-, *k'w-al- "round" (cf. Georgian *k'ver-* "a kind of round, flat cake or cookie"; Mingrelian *k'vark'valia-* "round"; Svan *k'urpi* "round"); Proto-Afroasiatic *k'aw-/*k'əw- "to make a round hole in" (cf. Arabic *ḳāra* (root *ḳwr*) "to make a round hole in, to gouge, to scoop, to hollow out"; Burji *k'aw-a* "a hole"); Dravidian: Tuḷu *gavi* "cave, hole, cell".

I. Proto-Nostratic *k'ʷat'-/*k'ʷət'- "to cut": Proto-Indo-European (*k'ʷet'-/*k'ʷot'- > [with regressive deglottalization]) *kʷ[h]et'-/*kʷ[h]ot'- "to whet, to sharpen" (cf. Gothic *ga-hvatjan* "to sharpen, to incite, to entice"; Old English *hwettan* "to whet, to sharpen, to incite"); Proto-Kartvelian (*k'wet'y-/*k'wat'y- >) *k'wety-/*k'waty- "to cut" (cf. Georgian *k'vet-a* "to cut"); Proto-Afroasiatic *k'ʷat'-/*k'ʷət'- "to cut" (cf. Arabic *ḳaṭṭa* "to carve, to cut, to trim, to clip"; Dullay *qatt'-* "to cut, to hoe up, to fold"); Dravidian: Tamil *katti* "knife, cutting instrument, razor, sword, sickle"; Parji *katt-* "to cut down (tree), to slaughter, to sacrifice".

J. Proto-Nostratic *k'ʷury-/*k'ʷory- "to crush, to grind" Proto-Indo-European *k'ʷerAn-/*k'ʷr̥An-, *k'ʷreAn- > *k'ʷrān- "mill, millstone" (cf. Sanskrit

grávan– "stone for pressing out the Soma"; Old Icelandic *kvern* "millstone, handmill"; Lithuanian *gìrna* "millstone"); Proto-Kartvelian **k'werc[h]ıx–* "to break, to crush (tr.); to crumble, to break (intr.)" (cf. Georgian *k'vercx–* "to heap up, to pile up"; Mingrelian *[k'ančx–]* "to crumble, to break (intr.)"; Zan *k'vančx–* "to break, to crush (tr.); to crumble, to break (intr.)"); Dravidian: Tamil *kuṟavi* "grinding pestle"; Malayalam *kuṟavi* "small rolling stone to grind with".

K.   Proto-Nostratic **q'al–/*q'əl–* "neck, throat":   Proto-Indo-European **k'el–/*k'ol–/*k'l̥–* "neck, throat; to swallow" (cf. Latin *gula* "throat, gullet"; Old High German *kela* "throat, gullet"); Proto-Kartvelian **q'eli* "neck, throat" (cf. Georgian *q'eli* "neck, throat").

L.   Proto-Nostratic **q'ʷul–/*q'ʷol–* "to strike, to hurt, to wound, to slay, to kill": Proto-Indo-European **k'ʷel–/*k'ʷol–/*k'ʷl̥–* "to strike, to kill" (cf. Old English *cwelan* "to die", *cwellan* "to kill"; Lithuanian *gélti* "to sting, to ache"; Armenian *kełem* "to torture"); Proto-Kartvelian **q'wal–* "to slay, to kill" (cf. Georgian *k'al–/k'l–* "to kill"; Zan *q'vil–* "to kill"); Proto-Afroasiatic **q'ʷal–/*q'ʷəl–* "to kill, to slaughter" (cf. Proto-Semitic **k'altall–* "to kill, to slay" > Arabic *katala* "to kill, to slay, to murder, to assassinate"; Proto-East Cushitic **k'al–* "to slaughter" > Somali *qal–*; Oromo *k'al–*); Proto-Uralic **kola–* "to die" (cf. Finnish *kuole–* "to die"); Proto-Elamo-Dravidian **kol–* "to kill" (cf. Royal and other Achaemenid Elamite *halpi* "to die, to slay"; Tamil *kol* "to kill, to murder, to destroy, to ruin, to fell, to reap, to afflict, to tease"; Tuḷu *kolè* "murder"); also note Sumerian *gul* "to destroy".

Finally, a few remarks need to be made about Illič-Svityč's proposed cognate sets in general (12). In some of his proposed etymologies, the correspondences between two or three of the branches are sound from a semantic point of view, while those adduced for the other branches are questionable. In several cases, the etymologies should be abandoned altogether. The large number of examples, however, that appear sound from both phonological and semantic points of view is truly impressive.

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12) Note also the critical comments Murtonen has made in *Mother Tongue 9* (November/December 1989) about the Afroasiatic material cited by Illič-Svityč.

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COMPUTER PROGRAM INFORMATION*

Oriental Words: New Products
for Processing and Crunching Them

by Stephen A. Kaufman, ANE Sectional Editor
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For most humanities scholars, the personal computer is just a glorified typewriter. Those who want to do more, however, do not find the road easy. For most personal computer salesmen, the academic user with special needs is a second-class user at best, and those few published guides that see the light of day are inevitably out of date before they make it through the press. (The most valuable to date is surely John J. Hughes' *Bits, Bytes & Biblical Studies* [Grand Rapids: Academie Books, 1987] and his continuing serial *Bits & Bytes Review*.) There are software producers out there, however, who are aware of our plight and, slowly but surely, coming to the rescue. As a guide to our membership, the Board of the Society has decided to publish notices on computer materials of general interest in the Newsletter, where the delay is minimal. Reviews of materials of specific value for a single discipline (such as electronic concordances or text publications) will be published in the *Journal* along with regular reviews and should be sent to the appropriate Sectional Editor.

This review discusses three software packages for the IBM PC particularly appropriate for the special requirements of many orientalists.

Multi-Lingual Scholar version 3.2

The road from the anemic "Hebrew Scribe", through the barely useful "Multi-Lingual Scribe", to the current feature laden MLS has been a long one, but it has resulted in what surely should be the word processor of choice for those who have to produce high quality output in non-Roman alphabets. The quality and variety of available fonts and the quality of the output even on 9 pin dot-matrix printers are surely unmatched in the PC world, at least as far as WYSIWYG programs go. Retail price for MLS 3.2 is $350 ($500 for the HP LaserJet II version), but substantial student and institutional discounts are available. Upgrades (from versions 2.9-3.1) are $35.

The basic package comes with several different Roman, Greek, Cyrillic, Arabic/Persian and Hebrew fonts; on request, Devanagari can replace Hebrew. With these

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and the supplied diacritics, virtually any European language can be produced. Many additional fonts are available at additional cost from the publisher and from third parties. These give you the ability to work with: Amharic (and other European languages), Armenian, Bengali, Coptic, Georgian, Gujarati, Inuktitut, IPA, Kannada, Korean, Malayalam, Old English, Old German, Punjabi, Sanskrit, Sinhalese, Syriac, Tamil, Telugu, Thai, Tibetan, Urdu, and Vietnamese. Many attractive Hebrew fonts are now available, and teachers may even use MLS to prepare exercises in such things as Ugaritic, Phoenician, Elephantine Aramaic, and Hieroglyphics. What sets MLS apart from other foreign language word processors is not just the quality and variety of fonts supported, but also its total extensibility. A font editor allows you to create your own printer and screen fonts, the keyboard may be modified at will, and you can set up to use different configurations of languages for different projects. With the proper scanning equipment, you may even scan a printed work into the font editor in order to develop a new font. To be sure, a lot of work is involved in these steps, but the power is there, and the documentation is an adequate guide.

This review assumes the reader is familiar with earlier versions of MLS and concentrates on the new features. As before, MLS requires a graphics card and is virtually impotent with much less than 640K of available RAM. Postscript laser printer support is, unfortunately, not yet available.

The most important new feature is the ability to export an image of your file to a PCX format file. This is implemented by the command "PICture", which creates an image of all or selected pages of your document, one page per file. These images can then be imported into desktop publishing programs, such as WordPerfect 5.0, Ventura Publisher and Pagemaker, where they can be combined with other materials. Of course, in their new environment they are treated as graphics objects rather than blocks of text, but, when correctly sized, the quality of the final output is nearly as good as that from MLS itself. I use MLS only sparingly, since other products such as WordPerfect and Nota Bene have so many more features and are so much easier to use. Now I anticipate using it much more often and incorporating pages and paragraphs from it into longer WP documents. (The original version of 3.2 that I received did not, in fact, prove to be compatible with WordPerfect. A call to the publisher, however, produced a revised version in a few days.)

The other major new feature will be one of interest to those who work with connected scripts such as Arabic and Syriac. Now these fonts are automatically justified the old fashioned and aesthetically pleasing way -- *keshideh*, the lengthening of the connecting line. In theory this is certainly an improvement, but in practice MLS *keshideh* seems to be limited to one or two extra dot widths, and inter-word spacing can still appear obtrusive when the words are of unusual lengths. User generated fonts can make use of this feature by including a character #255, one pixel wide, as tall as the connecting line.

Additional new features are: support for the HP Scanjet and Deskjet, an install program, an on-line tutorial, the ability to incorporate HP fonts into MLS, and changes to the configuration file utility. You must now define the default inter-character spacing for each font (obviating the need for a ";CS" command in each document) and can select the colors for the various displays. A justification parameter for footnotes may also be specified in the ";NF" command. The torpid CTIU program introduced with version 3.1, for importing

and exporting documents in multiple fonts, is unchanged.

NOTE BENE 3.0 Special Language Supplements

Nota Bene, the specially modified version of XYWrite produced by Dragonfly Software, is surely the most comprehensive word processor available for academics, even without its special "Text-Base" feature. As an add-on to version 3.0, those with appropriate hardware set-ups may purchase the Special Language Supplements.

Unlike MLS, which uses graphics screens to display foreign fonts and diacritics, Nota Bene is a character based program. In order to display and print non-standard characters with Nota Bene, then, the user must have both a video card (specifically EGA/VGA, Hercules Graphics Plus or InColor, or Everex Evergraphic Deluxe) and a printer (Epson FX-80/85/86, Toshiba 1351, P321SL, P341SL, P351, or HP LaserJet II) that support downloaded character sets. In fact, technically sophisticated Nota Bene users with such set-ups have always been able to see and print foreign fonts, but they have had to do all the preparation themselves, a task that became more and more cumbersome with each new release. The new supplements simply standardize the entire process by providing screen and printer fonts and automating the downloading and keyboard switching processes. Of course, word-wrap in right-to-left contexts is now also supported.   There are, at least in theory, five supplements:

I.   Biblical Studies: Hebrew, vocalized Hebrew, Greek, German, French, English
II.   Classical languages: Greek, Latin, transliterated Asian languages such as Sanskrit, most Western European languages.
III.   European and Slavic languages: includes Cyrillic and Old English.
IV.   Semitic languages: Hebrew, transliterated Hebrew, transliterated "mid-Eastern" languages.
V.   Transliterated languages: All transliterated languages mentioned above.

In fact, however, this system is implemented using 8 different fonts and 7 different keyboards, with different combinations activated to produce different results. Diacritics are included with the Greek vowels, but Hebrew vowel points are displayed sequentially rather than over or under their consonant, although they do print out combined correctly.

Those with EGA or VGA cards (capable of supporting two complete 256 byte fonts at one time) can only use one of these supplements at a time. (To switch from one set to the other requires a rather cumbersome re-installation process.) With the other cards, the "Complete Languages Supplement" may be used, which enables any of the characters of any of the sets to appear on screen or in a document.

Many of the familiar command-line commands have had to be changed, inasmuch as the keyboard will frequently not be in English mode when the command is to be issued. One can use the F1 menu system to make use of a new set of CTRL commands to access the standard features. CTRL-L enables language switching. When access to the Supplements is enabled, many of the other features of Nota Bene do not work as expected,

and some do not work at all, but it is possible to switch back and forth from normal Nota Bene to the Supplement version. Text-Base is not guaranteed to work properly with the early versions of the Supplement, but we are promised that it, too, will eventually be enhanced to retrieve text properly in any of the languages.

The installation process is cumbersome, and the learning curve here is steep, but the product does work. (Those with earlier versions of 3.0, however, have to order a special upgrade, and those with an EGA card must install a special version of the thesaurus program.) In the case of several commonly needed features, in particular the switching back and forth from left-to-right input on the same line is easier to use than MLS. The ability to have phrases in different languages in the phrase library is particularly attractive. The problems with this product, however, are numerous indeed:

Changing languages is a rather slow process, even with a fast hard disk. The keyboard layouts are far from intuitive. Why weren't standard keyboards, such as Hebrew, used where available? Hebrew is stored (left-to-right!) in a unique high ASCII sequence quite different from the long-since standard PC arrangement used in Israel.

The laser and 24 pin matrix fonts are reasonable, but the 9 pin fonts are quite unacceptable except for the roughest work. Unlike MLS, no font editor is provided. Although no guidance is given in the documentation, I know that it is possible to substitute or add one's own screen fonts. I have not heard of anyone who has figured out how to modify the printer fonts, however.

Not all of the special characters available in the fonts can in fact be accessed according to the provided keyboard layouts. Only by studying the chart of characters (Appendix C) and finding an equivalent in another font, can one sometimes produce the desired result. Semitic ḏ, for example, occupies the same position in font 3 that Å does in font 2. Thus, the keystrokes that normally produce the latter (shift-alt-2 A) will produce the former if the appropriate font has been activated, but I could not find this discussed in the manual.

The authors were not particularly well guided in their choice of characters, at least as far as the needs of Semiticists are concerned. The Biblical fonts include an obelus and a form of the Tiberian vowels with an etnach! I can imagine few who would have need of esoterica like this. On the other hand, ḏ and ṯ are found, but not such things as ḇ and p̱. This word processor, it seems, knows the difference between phonemic and phonetic transcription!

If you go to DOS (one of the most attractive feature of NB) and invoke a program that resets the video card, chaos greets you on your return to NB. On the other hand, if you exit the program with the normal quit command, the NB fonts remain loaded. A new quit command (ALT-ESC ENTER) has been added to avoid this possibly unpleasant situation.

There is no way, short of writing your own custom conversion program, of exporting files to other environments while preserving all of the special language information.

The Language Subset option lists for $125. The Complete Supplements option is $195. In addition, Nota Bene 3.0 is required.

WordCruncher

Many humanists have grandiose plans for inputting their textual sources into the computer and performing miraculous new kinds of analyses, but few have any idea how to go about it. Text retrieval programs such as WordCruncher are part of the solution. These programs create indexes of large bodies of textual material, enabling immediate access to all the attestations of any word or group of words, and many, WordCruncher included, have the ability to generate frequency lists and different kinds of concordances on demand. Most of these programs work as advertised with English text. WordCruncher has several unique feature, however, that make it of particular interest to Orientalists.

The program works well with alternate character sets on EGA/VGA or Hercules Graphics Plus cards as well as printers, though the user must provide the sets and download them before beginning the program. (For EGA cards the "Duke Toolkit" is recommended.) For each file, the user can specify whether right-to-left or left-to-right display is the default, but the display can be toggled on the fly. As a part of the indexing process, a unique "character file" table can be created for each file. This is maximally flexible, enabling the user to specify not only the sorting order of all 256 ASCII characters, but also to indicate which characters are parts of word, which word dividers, and which should be ignored, as well as to equate characters, both for sorting and keyboard purposes. Thus a simple vowel and its accented versions can be made to sort identically or not, and a single keystroke can be set to be equivalent to a non-Roman character.

In addition to text tiles (called .BYB or "book" files in this system), WordCruncher provides for the simultaneous access to two additional files, a .BYD "dictionary" file and a .BYT "thesaurus" file, thus enabling a kind of "hypertext" environment. While viewing any text, the F7 function key brings up the dictionary file entry corresponding to the word on which the cursor currently rests. Once open, you may also browse through the dictionary or immediately search for any other word. Naturally, the "dictionary" file need not necessarily contain definitions. Any kind of material can be included; the only requirement is that the first line of each record begin with a vertical bar character followed immediately by the key word. The thesaurus file is used to generate lists of related items for searching. In the case of a Semitic text, for example, one would list under a specific root all actual or theoretical forms. A few keystrokes generates a look-up list containing all these forms, and all the corresponding citations are instantly displayed. Naturally, as with a hypertext project, a substantial amount of preparatory work must go into developing the dictionary and thesaurus files, but if properly designed, the usefulness of the entire package is bounded only by the designer's imagination. The pedagogic potential of the system is obvious. From my perspective, this is a software package that, together with prepared textual materials, should sit prominently displayed and used in the reference room of every serious academic library.

I do have a few minor complaints. When using unusual sorting sequences, the program does tend to abort with an error message on occasion. Minor adjustments usually solve the problem. A very useful improvement would be the ability to incorporate "tags", grammatical or other information that could be searched on but that would not be displayed unless specifically requested. As it stands, two useable but less elegant approaches to tagging may be used: Tags may be incorporated into text (with their markers listed as word

dividers in the "character" file), or tags may be used as headings in a thesaurus file. The problem with the latter approach is that there seems to be a limit to the number of entries that can be subordinated to any given heading. The ability to display .BYD and BYT files in right-to-left format should also be added.

The program requires 512K of RAM (though 640 is preferable), and is virtually unusable without a hard drive. Access to a RAM disk and a fast CPU results in considerable improvement in performance. Electronic Text Corporation and other sources supply many preprocessed textual databases for use with WordCruncher, in which case the user need have only the retrieval module, "View ETC". We are also promised that a MAC version is on the way. Listed prices are $100 for View ETC alone, and $200 for the entire package. Academic and institutional discounts are available.

Products Discussed:

Multi-Lingual Scholar ver. 3.2
Gamma Productions, Inc.
710 Wilshire Blvd., Suite 609
Santa Monica, CA 90401

Nota Bene ver. 3.0 Special Language Supplements
Dragonfly Software
285 W. Broadway, Suite 600
New York, NY 10013-2204

WordCruncher
Electronic Text Corporation
5600 North University Avenue
Provo, UT 84604