

**“Somewhere in Asia,’ and No More”  
Response to ‘Indigenous Indo-Aryans and the  
R̥gveda’ by N. Kazanas**

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The Indo-Aryan problem has, of late, become one of the most contentious issues in the field of Indology. While the Indological consensus of an Indo-Aryan invasion or migration into the Indian subcontinent has long been taken for granted, at least in western academic circles, it has been ferociously contested amongst Indian scholars over the last decade or so (and on the margins of academia in India for well over a century), and the debate has now become an unavoidable issue which all scholars dealing with the early history of the Indian subcontinent must confront. Unavoidable too, unfortunately, seems to be the emotion that the issue tends to generate from all sides, with discussions on the net soon degenerating into polemical exchanges, and even published articles on the topic adopting a surprisingly acrimonious tone.

Kazanas rightly wonders why respected academics adopt derisive attitudes when critiquing the work of those who differ from them on this subject, although one might point out that he himself hardly displays cordiality at times in his own paper, such as when he dismisses the reconfiguring of the Indo-Aryan ‘invasion’ to a ‘migration’ as a “preposterous proposition” revealing a “shameless frame of mind.” The emotionalism the Indo-Aryan problem generates on both sides of the issue not only manifests in the adoption of *ad hominem* attacks and condescending rhetoric but, even more so, in the tendency of disputants to simply highlight and ridicule the most outlandish aspects of opposing arguments while conveniently ignoring any coherent points made that might counter their own position. Such emotional involvement understandably raises questions as to the personal investment of the participants themselves: what is it that causes scholars to become so inflamed and invested about whether the Indo-Aryan speakers and their Indo-

European predecessors originated in the Steppes of South Russia, North-West India, or, for that matter, Timbuktu?

Much has been written about the appeal of indigenous origins to insurgent nationalisms, and there can be no doubt that the theme of autochthonous Vedic pedigree surfaces in much right-wing Hindu discourse. However, as I have attempted to point out in the past, it is too simplistic to lump *all* recalcitrant voices opposing the consensus of Indo-Aryan invasions/migrations into a nationalistic or fundamentalist mold, even while there is no dearth of ideologically strident voices who have coopted this issue in support of their agendas. Anyone unfamiliar with this aspect of South Asian studies can gain a sense of such tendencies to immediately stereotype dissident views on this issue by the fact that Kazanas sees fit to immediately state disclaimers as to his own possible predispositions.

One can, of course, understand the concern over the extremes of Hindu nationalism given Europe's own bitter history of Aryanism, although the fact is that much of the impetus fueling the 'revisionism' of the Indo-Aryan issue stems from distrust of the motives and agendas underpinning the entire construction and pursuit of the Indo-European homeland quest by Europeans in the 19th and 20th centuries in the first place. Anyone who has at all dabbled with the history of this enterprise, rooted as it is in European racisms, nationalisms and quests for biblical origins, can hardly blame such *a priori* suspicion in the post-colonial climate of Indian historiography. After all, much of the scholarship on the history of the Indian subcontinent was formulated during the colonial and imperial heyday of the 19th century. We thus have a complex situation where, on the one hand, there are valid and serious grounds for concern over nationalistic appropriation of myths of origin in present day India, and, on the other, equally valid grounds for submitting the entire Indo-European/Indo-Aryan locating enterprise to post-colonial scrutiny. In any event, at face value, Kazanas is a vigorous opponent of the Indo-Aryan migration/invasion theory who cannot be categorized and pigeonholed as a Hindu nationalist or fundamentalist of some sort.

In terms of content, a good deal of Kazanas' paper is, to a certain extent, a reworking of a by now more-or-less standard corpus of material. Kazanas' most distinctive contribution to

the discussion of Indo-Aryan origins is the table of deities he presents in section VI. His premise is that the Vedic deities have widespread cognates in other Indo-European languages, while non-Vedic deities from other non-Indo-Aryan Indo-European languages have barely one or two cognates in other languages. I will leave it up to specialists in Indo-European mythology to evaluate the specifics of Kazanas’ table and, allowing for some quibbling over details, assume his basic premise to be generally correct. Kazanas’ next assumption, that “the people or culture that has preserved most *ceteris paribus* has moved least,” needs more attention. This principle, which has also been referred to as the ‘conservation principle,’ has a long history in the Indo-European homeland quest in the field of linguistics, and is still used by various homeland proponents. Kortland (1990), for example, promoted the Ukraine as the homeland partly on the grounds of the archaic character of the Lithuanian language, and Gamkrelidze and Ivanov (1983, 1985, 1995), Anatolia, on the same grounds but in relation to the Hittite group of languages. One corollary of this view, that emigrating languages innovate as the result of substratum influence, has also found supporters, albeit promoting other homelands (e.g. Feist, 1932, Pissani, 1974).

However, although the conservation or preservation principle has a respectable history in linguistics, and is a perfectly adequate way of accounting for this type of data, so does another linguistic principle, which would seem to oppose it. In fact, as early as 1862, Robert Latham was the first to challenge the then established notion of an Eastern homeland by proposing that languages are analogous to species: the geographical center of origin exhibits the greatest variety of features. Or, as Dyen (1965) pointed out, it is more likely that an isolated language or two, such as Indo-Iranian and Tocharian, moved out from a matrix that had become heterogeneous in time, such as East Europe/South Russia, than that many linguistically distinct groups moved out from a relatively homogeneous area such as India. Hock (1999) raises similar arguments specifically against an Indian homeland. Since linguistic variegatedness suggests innovation and thus loss of archaisms, the heterogeneity-as-locus-of-origin principle would seem to at least partially contradict the conservatism-as-locus-of-origin, or preservation, principle. Peripheral or emigrating languages can just as well preserve archaisms where

central ones have innovated, and one would need to provide convincing reasons as to why the same could not have been the case with mythology.

I say this not in support of either view — especially since Nichols (1997) has rejected the heterogeneity argument and provided a radically new way of accounting for the homogeneity of Indo-Iranian — but to further underscore the vulnerability of predicating homeland hypotheses on linguistics, which has given us two acceptable models that produce opposing ways of interpreting the same evidence. Although Kazanas does not hide his disdain for linguistics in general, he does see fit to invoke the conservation/preservation principle to argue that not only has Vedic mythology retained the Indo-European character most completely, but the language has as well. Perhaps, in his response, Kazanas could outline his own rationale for opting for the conservation principle rather than the alternative outlined above.

Apart from the arguments mentioned above, Kazanas, relies on Burrow (1973) in support of the position that Sanskrit is the most conservative Indo-European language. I would also be interested in his response to the view (e.g. Polomé, 1985) that the features in languages such as Sanskrit and Greek that have historically been considered the most conservative or archaic, such as the reduplicated perfect, in fact represent late innovations. There has, since Burrow, developed an Indo-Hittite school which argues that the existence of the laryngeals and various morphological features of Hittite, especially those features it shares with Germanic, point to its greater archaicism in comparison to Sanskrit. From this perspective, the absence of supposedly archaic Indo-European features in Hittite, such as masculine and feminine gender, actually indicate its greater antiquity, since these features are in fact later innovations and not archaic at all; thus languages such as Sanskrit that exhibit such features are more recent. Accordingly, which Indo-European language can best lay claim to being the most archaic, or best preserved, viz-a-viz the mother tongue, no longer has the consensus it once had, further problematizing the preservation principle. Kazanas' observations about the speculative and inconclusive nature of linguistics would thus seem to be applicable to his own central arguments of preservation on more than one count.

Nonetheless, I agree that, in the absence of dateable

written records, and given the history of criticisms against linguistic palaeontology, linguistics can only establish relative chronology (although it seems unfair to make the sweeping charge that "linguists continue to arrogate themselves the competence of other disciplines," particularly since Kazanas, who hastens to establish his credentials as a Sanskritist, does not hesitate to make pronouncements on archaeology and astronomy). I also agree that, given the problems associated with all homeland theories, it does appear rather precarious to single out NW India/Pakistan/Afghanistan as definitively ineligible for the dubious privilege of being the homeland, especially given the criticisms that can and have been raised against migrationist interpretations and other homeland theories. I will only comment, here, given Kazanas' view that "philology has little competence in this field," that he dedicates the bulk of his paper to utilizing philological data gleaned from the texts — the Vedic deities and comparative mythology, Sarasvatī, the horse, chariot, and astroarchaeology — to argue for the possibility of an Indian homeland.

Be that as it may, the argument that the Vedic texts, despite their antiquity and volume, preserve no mention of an overland trek is an old one, going back at least to Dayananda Saraswati at the end of the nineteenth century. It is, of course, an *argumentum ex silentio*, but it is nonetheless one valid ingredient in a complex picture. However, Kazanas might be too forceful in pushing his case, since the Anatolian languages also preserve no such mention. Although he states that "all IE branches other than the Indo-Aryans have demonstrably moved to their historical habitats from some homelands: there is evidence of this of one kind or another," he had early noted that the Hittite records also preserve no mention of a migration to their historical habitat despite their antiquity. Kazanas compensates for this by noting that eminent Hittitologists consider them intrusive on other grounds; needless to say, whatever might be Kazanas' opinion of their arguments, eminent Vedicists likewise consider the Indo-Aryans to be intrusive on a variety of other grounds despite a similar absence of migratory records. Deferral to eminent specialists thus runs the risk of becoming a selective and therefore potentially problematic procedure. Also, other equally eminent specialists of Hittite, such as the linguists Gramkrelidze and Ivanov, consider the Anatolian languages to be, if not precisely in their

proposed East Anatolian homeland, then immediately adjacent to it. Moreover, it is interesting that they use very similar arguments as Kazanas to argue their case (such as the preservation principle noted above, and the dismissal of the value of the horse evidence, etc).

While on the subject of linguistics, given that Kazanas chastises Witzel so vigorously for, amongst other things, misrepresentation of opponents, I feel obligated to point out that he has quoted Hock as finding "no obstacle or difficulty presented by the isoglosses with regard to the Out-of-India scenario." In actual fact, (even though Nichols' position does offer a radical new model that accounts for his concerns) Hock's further statement in his 1999 article with regard to the Indo-European isoglosses is that a PIE-in-India position "while...not in itself improbable...has consequences which, to put it mildly, border on the improbable and certainly violate basic principles of simplicity" (16).

As for the substratum evidence, here, too, I cannot ultimately fault Kazanas' rejection of this evidence, although I find him to be somewhat cavalier in his dismissal of it. No one can deny that there are many sound changes occurring in historical linguistics that do not seem to correspond to any known law, but neither can it be denied that there are phonemes, words and grammatical features that are completely at odds with the basic Indo-European pattern and thus are conspicuous as foreign linguistic features. There is thus no doubt (and Kazanas himself begrudgingly accepts) that the Vedic texts themselves attest to the existence of Dravidian and Munda, and, in all probability, an unknown non-Indo-European language, in the form of loan words in the early period (therefore, it is not quite correct to state that "Vedic was an intruder when no other language of equal age was attested").

To my mind, the issue is not whether Vedic exhibits non-Indo-European linguistic features, but whether such features must only be explained in terms of substratum influence. As I have argued elsewhere (Bryant, 2001), in addition to the difficulties noted by Kazanas with regards to removing all doubt that such features might not have obscured Indo-European origins, those arguing for a substratum need to eliminate a number of other interpretative possibilities. For example, phonemic features unique to Indo-Aryan from the I-E language

group that the former shares with Dravidian and Munda, such as retroflexion, may have been spontaneous post-Indo-European developments in any of the languages on the subcontinent (including Indo-Aryan) that then became South Asian areal features. Non-Indo-Aryan words evidenced in the texts could have simply been introduced as the result of trade or other types of social interactions, or as adstratum influence, rather than as the result of intruding Indo-Aryans imposing their language on indigenous non-Indo-Aryan speakers. And, even if migration were a factor, how do we know it was not Dravidian, Munda or ‘language x’ speakers migrating into Indo-Aryan territory and influencing Indo-Aryan as a fleeting superstratum rather than substratum? All these along with other possibilities have, in fact, been considered by scholars, and they combine to considerably diminish the persuasiveness of the substratum theory.

I agree that a plausible explanation has yet to be given as to how, if there were indeed no actual invasion of Indo-Aryans but only the migratory ‘trickle’ into which it has been reconfigured, the newcomers could have completely eradicated the pre-existing language of the entire North of the subcontinent in the short interval normally allotted between their arrival and the composition of the *R̥gveda*, in which the local topography is Indo-Aryan. When one considers that, in these proposed two or three centuries, such hypothetical migrants managed even to Sanskritize practically all of the names of rivers and places, the most conservative aspect of a substratum, in the N.W. of the subcontinent, but yet failed to do so in the East of the subcontinent despite Aryanizing it for well over two millennia, and the accomplishment is remarkable (and, of course, they failed to even displace the Dravidian languages in the South despite extensive interaction for almost as long). Add to this that their predecessors were the highly sophisticated and urbanized residents of the Indus Valley Civilization, and the incoming Indo-Aryans typically construed as pastoralist nomads, and Kazanas has a right to wonder how and why would the Indus Valley dwellers have so thoroughly and completely adopted the language of these illiterate herdsmen if the latter were not invaders – a status denied them by archaeology?

As for astroarchaeology, I will reserve a full analysis of Achar’s paper for a work in progress, but will simply note here

that it is unwise, in my opinion, to reject the astronomical evidence in too hasty a fashion. As has long and continuously been pointed out by numerous scholars, the Brāhmaṇa texts have a significant number of references that might point to the position of the sun in *Kṛttikā* (Pleiades) at the vernal equinox some time in the first half of the 3rd millennium BCE (e.g., Jacobi 1909; Law 1965; Bryant 2001 for overview).

Granted, as Whitney (1895), Thibaut (1985) and more recently Pingree (1973) point out, the zodiac may not have been divided in the exact same way in the ancient period as in the historic, and the computing skills of the ancient Indo-Aryans may not have been as accurate as they became. But, at the same time, there are actually no demonstrable grounds to assert that they were not. The astronomical treatise, the *Vedāṅgajyotiṣa*, albeit open to similar objections, gives very precise astronomical information on all four solstitial and equinoctial points corresponding to the late 2nd millennium B.C.E., and this text is much later than the Brāhmaṇas. These data are as valid evidence for an early date for the *Rgveda* (which long preceded all these texts), as any evidence brought forward to promote a later date. The evidence of *kṛṣṇa ayas*, iron (literally 'black metal') in the Brāhmaṇas fails to conclude the issue since, although smelted iron does not surface in the subcontinent until the late 2nd millennium BCE, objects made of black iron ore have been discovered in Harappan sites going back to 2600 BCE (Possehl 1999). There is no way, to my knowledge, of asserting that *kṛṣṇa ayas* refers to smelted iron in the earlier texts (as it did in the later ones), rather than iron ore or even, as Kazanas speculates, blackened copper. While legitimate counter-arguments can be raised against the astronomical evidence (primordial memories from outside of South Asia, for example, could have been preserved in texts written in the subcontinent in much later times), it should not be brushed aside, particularly given the counter-arguments that can be raised against the evidence brought forward to argue for a later date.

The discussions on the horse and chariot are exhausting and I have nothing further to add to what I and many others have already written on this issue, except that I was only made aware of the reference to the 34 ribbed horse in the *Rgveda* after I wrote my book. This does seem to further complicate attempts to connect it with the already highly problematically



reconstructed proto-Indo-European steed from the Russian Steppes. As for the chariot, I will let Witzel defend his views that the *ratha* known to the *R̥gveda* is the light spoked-wheel one and not a heavier one from an earlier historical period as Kazanas seems to be suggesting it might be. Likewise for the interminable discussion on the Sarasvatī, although I will note, here, that proposals correlating her with other rivers in Afghanistan or elsewhere are unconvincing to my mind, as are attempts to argue that she ended in a terminal lake rather than the ocean. Kazanas has provided additional philological arguments to support the least complicated opinion, that Sarasvatī as known in the *R̥gveda* was a mighty river that flowed to the sea. One can always engage in special pleading to avoid this conclusion, but the real problem, to my mind, is not the correlation between Sarasvatī and the Hakra/Ghaggar river bed, but determining when this river became dry or even significantly diminished. To my knowledge there is not a clear consensus of opinion on this date, and until this is established, it is difficult to correlate the composers of the *R̥gveda* with this aspect of the archaeological record. If it can be established with some degree of rigor that the river went dry before 2000 BCE, then, to my mind, this provides significant grounds to suppose that the Indo-Aryans must have been in this area prior to this date.

I trust I may be allowed to take this opportunity to respond to Kazanas' charges against my own work, namely that "it makes the issue seem far more complicated than (in my view) it is by giving undue weight to arguments that do not deserve it and by ascribing undue importance to the linguistic side." I had hoped to make it clear both in my introduction (p. 9) and conclusion (p. 308) that my goal was not to offer a solution to the Indo-Aryan problem, but to lay out the *history* of 'The quest for the Origins of Vedic culture,' particularly those aspects that have been contested by Indian scholars. As such, my task was specifically to lay out *all* the arguments, linguistic or other, that have been influential in this regard over the decades, even while I have attempted to point out the problems associated with them. I approach this material as an historian of ideas. It cannot have escaped Kazanas' attention that I am as skeptical of the value of most of the linguistic evidence involved in the Indo-European homeland quest as he himself obviously is.

With regards to the two examples he offers in a footnote,

that I take Hock's view too lightly, I do, on pg. 151, critique it by outlining Nichols' model which specifically addresses and invalidates his concern. True, in a discussion that has become all-too-polemical of late, I attempted to treat Hock's view respectfully and gave it fair airing as a thoughtful observation on Indo-Aryan and its relationship with the other Indo-European languages by a dedicated linguist. As for Kazanas' second complaint, I stand corrected in my statement that the Greeks and Scandinavians do not preserve record of their early movements if, as he claims, they unambiguously do.

All in all, I cannot fault Kazanas for feeling the need to undertake a critique of the evidence supporting the Aryan Migration hypothesis. In my view, the Indo-Aryan invasion/migration theory, at least in its present forms, as well as the dating of the Vedic texts, remain unresolved issues that invite unbiased fresh scrutiny. And he does, in my view, have a point that we owe it to the people of India to conduct a thorough review of all the evidence. I might repeat, in this regard, that it is not helpful to *a priori* assume that all dissident voices from India are Hindu nationalists or fundamentalists of some sort (nor is it helpful, in our post-colonial academic culture, if we adopt a tone presuming to correct their erroneous views rather than courteously differing from our colleagues who have offered alternative interpretations to our own). Having said this, I do, however, feel the rhetoric of Kazanas article to be on the polemical side, even if he is by no means unique in this regard. The Indo-Aryan problem is likely to remain unresolved for the foreseeable future, so we might as well attempt to address it in a cordial fashion. Kazanas takes it for granted that "mainstream philologists will react unfavourably" to his thesis; his somewhat contentious tone, in places, has unfortunately not made it any easier even for those otherwise sympathetic to critiques of the Aryan Migration theory to try to lend a favourable ear to his views.

In any event, it is one thing to undermine the scholarship supporting an Indo-Aryan immigration into the subcontinent, it is quite another to promote India as a homeland from which all the other Indo-Europeans emigrated. To the extent that one argues that, in so far as the presently available data is concerned, NW South Asia *may just as well* have been the IE homeland in theory since all other homeland proposals suffer from setbacks that are arguably just as problematic as those

associated with a South Asian homeland, one may have a point. But in so far as one attempts to argue that NW South Asia has the *best* claim to being the proto-homeland one is, in my view, being as selective and partial in one’s appropriation of the evidence as Indigenists typically charge other homeland theorists to be. In my view, the homeland quest has not advanced much further than Max Müller’s assessment in 1847: “if an answer must be given as to the place where our Aryan ancestors dwelt before their separation....I should say, as I said forty years ago, ‘Somewhere in Asia’ and no more.”

### References

- Bryant, Edwin  
 2001 *In Quest of the Origins of Vedic Culture: The Indo-Aryan Invasion Debate*. New York, Oxford University Press.
- Bryant, Edwin, Patton, Laurie (eds)  
 forthcoming *The Indo-Aryan Controversy: Evidence and Inference in Indian History*. Richmond, Curzon Press.
- Dyen, I.  
 1965 A lexicostatistical classification of the Austronesian languages. *International Journal of American Linguistics*. Supplement 31.1.
- Feist, Sigmund  
 1932 The origin of the Germanic languages and the Indo-Europeanising of North Europe. *Language* 8: 245-254.
- Gamkrelidze Thomas V & Ivanov, Vjaceslav, V.  
 1983 The migration of tribes speaking the Indo-European dialects from their original homeland in the Near East to their habitations in Eurasia. *Soviet Studies in History* 22, 1-2, 53-95.  
 1985 The problem of the original homeland of the speakers of Indo-European languages. *Journal of Indo-European Studies* 13: 174-184.  
 1995 *The Indo-European and the Indo-Europeans*. Trends in Linguistics Studies and Monographs 80 Berlin, Mouton and Gruyter.
- Hock, Hans Henrich  
 1999 Out of India? The linguistic evidence. *Aryan and Non-Aryan in South Asia*. *Harvard Oriental Series Opera Minora* 3: 1-18.
- Jacobi, H.  
 1909 On the antiquity of Vedic culture. *Journal of the Royal Asiatic Society* 1909: 721-726.  
 1910 The antiquity of Vedic culture. *Journal of the Royal Asiatic Society* 1910: 456-67.
- Kortlandt, Frederik

- 1990 The spread of the Indo-Europeans. *Journal of Indo-European Studies* 18: 131-140.
- Latham, R. G.  
1862 *Elements of Comparative Philology*. London: Walton and Maberly.
- Law, N. N.  
1965 *Age of the Rgveda*. Calcutta: Firma K.L. Mukhopadhyay.
- Müller, F. Max.  
1887 *Biographies of Words and the Home of the Aryas*. Reprint, New Delhi, Gayatri, 1985.
- Nichols, Johanna  
1997 The epicentre of the Indo-European linguistic spread, in Roger Blench and Matthew Spriggs (eds) *Archaeology and Language I*. London, Routledge, 122-148.  
1998 The Eurasian spread zone and the Indo-European dispersal, in Roger Blench and Matthew Spriggs (eds) *Archaeology and Language II*. London, Routledge, 220-266.
- Pingree, David  
1973 The Mesopotamian origin of early Indian mathematical astronomy. *Journal for the History of Astronomy* 4: 1-12.
- Pissani, Vittore  
1949 *Glottologia Indoeuropea*. Torino, Rosenberg & Sellier.
- Polomé, Edgar, C.  
1985 How archaic is Old Indic?, in Werner Winter (ed) *Studia Linguistica Diachronica et Synchronica*. Berlin, Mouton de Gruyter, 670-683.
- Possehl, Gregory  
1999 The Early Iron Age in South Asia, in V. Pigott (ed) *The Archometallurgy of the Asian World*, MASCA Research Papers on Science and Archaeology 16, Philadelphia, University of Pennsylvania, 153-175.
- Thibaut, G.  
1895 On some recent attempts to determine the antiquity of Vedic civilization. *Indian Antiquary* 24: 85-100.
- Tilak, Lokamanya.  
*n.d.* *The Orion*. Poona, Messrs Tilak Bros.  
*n.d.* *The Arctic Home in the Vedas*. Poona, Messrs Tilak Bros.
- Whitney, W. D.  
1895 On a recent attempt by Jacobi and Tilak to determine on astronomical evidence the date of the earliest Vedic period as 4000 BC. *Indian Antiquary* 24: 361-369.