FORTY YEARS OF *ALE*: MEMORIES AND REFLEXIONS OF THE FIRST GENERAL EDITOR OF ITS MAPS AND COMMENTARIES[1]

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Abstract. ALE, which has seen a continued and renewed commitment of individuals and institutions regardless of all the political changes around them, is in my experience one of the finest examples of human commitment to scientific research in a context of international cooperation. My deepest hope is that the ALE will continue as a permanent laboratory for European geolinguistics and dialectology, opening itself, at the same time, to the new interdisciplinary developments that are profoundly changing our views on human evolution, and can be expected to have a gigantic impact also on historical linguistics.

As I write these lines, in 2007, the ALE has existed, officially, for 37 years: precisely since 1970, when Toon Weijnen, at the Catholic University of Nijmegen (NL), was elected the ALE first President, after a few years of preparation, in the late Sixties, which he shared with me, working at the University of Utrecht (NL), as co-founder of the project. But counting also the years of intensive work that preceded the official birth of the project, one comes to 42 years: almost half a century!

Now, looking back at those years, it is quite easy to summarise them in this way: the few years before the official start of the project, in the Sixties, were the "foundation years"; the 13 years of Toon Weijnen's Presidency, from 1970 to 1982, were the "preparation years"; the 17 years of my own Presidency, from 1982 to 1998, were the "realisation years"; the 8 years of Prof. Viereck's Presidency, from 1998 to 2005, were the "continuation and consolidation years". And the future will tell if the years of Prof. Saramandu's Presidency, from 2005 on, will also be "continuation years", or mark, who knows, a "new course".

Leaving the future to the initiative of Prof. Saramandu, I will turn back to the years I have spent for, and with, the ALE, in the hope that my memories and my reflections might be of some use to future scholars working for the project: *historia magistra vitae*!

I spoke of two co-founders of the ALE. Actually, in the beginning there were three of us: the third co-founder was Prof. L.-E. Schmitt of the Philipp University

[1] This article was written when Toon Weijnen was still alive. His recent decease, at the age of 98 years, gives a deeper meaning to what I have written evoking his person, his vision and my work with him in the foundation years of the ALE.

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of Marburg. For each of us three had independently proposed, in the Sixties, a European linguistic project, after which we had decided to merge them, and join forces to realise a single, comprehensive project. But things did not go that way, as we soon realised that Prof. Schmitt was more of a critic than a supporter of the ALE as Toon Weijnen and I saw it. So much so that in the second year of my Presidency, during a memorable ALE meeting held at Leipzig in 1983, he was ousted from the Editorial Board and the project. It is necessary to mention this episode to explain the apparent contradiction between the repeated, official statements issued in the first preparatory ALE publications about the three-headed origin of the project, and the reality that soon emerged.

Here is, for example, how Toon Weijnen described the origin of the ALE, in his first 1975 *Introduction* to the *ALE*: “It is the coordination of three projects of recent years which has enabled our *ALE* to reach a stage where there is good reason to believe that it can be realised in the near future.” (p. 55, 1.6); and “After mutual consultation, Schmitt, Weijnen and Alinei appealed to the Committee of the Slavic Linguistic Atlas… which assured the necessary collaboration of the Slavists. With this help, the appearance of a European linguistic atlas… seems possible.” (p. 57, 1.6.4). In my own *Introduction* of 1997 (*ALE: Perspective nouvelle en géolinguistique*), the origins of the *ALE* are described in very similar words.

The reality was that only Toon Weijnen and I really put our heads together to come, slowly but surely, to the foundation of the ALE. In the beginning, Prof. Schmitt limited himself to signing official documents, such as the “historical” invitation we sent to the editors of the Pan-Slavic Linguistic Atlas (OLA) (and thus, indirectly, to the Soviet Academy of Sciences), whose positive response, with the ensured participation of the whole of Eastern Europe, marked the official beginning of the ALE in 1970.

There are two added circumstances that favoured a very close collaboration between Toon Weijnen and me in the late Sixties and in the Seventies, irrespective of Prof. Schmitt’s negative stand: one was the fact that I was working in Holland, at Utrecht, a few km away from Nijmegen. The other was that, while sharing the same “deep” vision of dialectology, namely the conviction that dialects preserved extraordinary relics of our cultural evolution, somehow we complemented each other, and were willing to listen to each other. So, for example, Toon Weijnen had no problem in accepting my proposal to change the basic map of Europe he had designed (which for some unexplainable reason left out a piece of Italy...), and I had no problem in accepting his proposal to give priority to the lexical questionnaire, and to place the historical-phonetics questions of my own project in the ALE Second Questionnaire. The ALE as it turned out would not have existed without this initial, close collaboration between us.

I have already mentioned that what marked the official beginning of the ALE in 1970 was the positive response that the Soviet Academy of Sciences gave to our invitation. For we were, let us not forget it, in the middle of the Cold War, and a “pan-European” project in the Humanities, in those years when in the Western world “peaceful coexistence” was a politically “loaded” concept, was not only extremely rare, if not unique, but also a very bold enterprise. Toon Weijnen and I shared not only the same vision of dialectology, but also this ideal of common European roots, irrespective of politics, and I would like to add that this was another decisive factor that made it possible to build the ALE.

I spoke of the close collaboration between Toon Weijnen and me, in the years before the official start, and of the way we complemented each other. This close collaboration continued, of course, also after 1970. Much time and effort were invested in the creation of the organisational structure, which had important consequences for the managing of the project. Both Toon Weijnen and I, for example, were in full agreement that National Committees would have the last word in all questions concerning their area. Even if we at times had to close our eyes at the way certain National Committees viewed their own minority problems, it was only in this way that we could ensure continued participation in our project. Also the discussions of the first Questionnaire, and the time-consuming preparations of the computerised programs for the cartography and for the databank (which were the task of Prof. Putschke of the Philipp Marburg University) went on, at least initially, without problems. All of this, however, took a long time: the first ALE preparatory publications, the *Introduction* by Toon Weijnen and the *Premier Questionnaire*, appeared, respectively, in 1975 and in 1976: it took thus six years, without even counting the years before 1970, to realise the basis for the project.

Differing opinions between Toon Weijnen and me began to emerge only in the late Seventies, as we approached the realisation of maps and commentaries for the so called essay-volume, which we were supposed to submit to the German DFG, our main financing institution.

It became clear, then, that Toon Weijnen conceived the ALE volumes that were planned for the first Questionnaire, as single fascicles, consisting only of a certain number of maps, each with its own short, list-like "technical commentary" printed on the back. A part from my objection against the extreme impracticality of the chosen format for the consultation of the ALE very large map (turning back and forth a sheet-like map to connect the various symbols of one side to the different words of the other side would have been an acrobatic work both for the arms and for the mind!), my main objection was that commentaries, far from being mere lists of etymologies, ought to be full *onomasiological* articles, illustrating in the best possible way the richness of Europe cultural heritage reflected in each notion of the Questionnaire, and thus forming a separate book, distinct from the map volume.

In an attempt to bring Toon Weijnen closer to my view, I, then, as ALE Vice-President, decided to concentrate my efforts in showing the extraordinary potential of European lexical geolinguistics. To that effect I had chosen the question 'arc-en-ciel', for I was personally very interested in research on the religious evolution of mankind, and I had good reasons to think that the question 'arc-en-ciel' would produce illuminating materials for European ideological history. And I had soon discovered, from the materials that began to come to me from the National

Committees, that they went far beyond my expectations. The map and the commentary for 'arc-en-ciel' that I prepared in the late Seventies and published in the first ALE fascicle (Alinei, 1983), with all of its extraordinary series of zoomorphic and anthropomorphic representations of the rainbow, were, I dare say, quite a novelty, and became in fact the platform for other innovations: (*1*) from the *theoretical* point of view, it introduced a new, independent level of linguistic investigation, next to phonetics, morphology, syntax and semantics (Alinei fc); (*2*) from the *methodological* point of view, it inaugurated a new type of linguistic cartography that then I called "motivational" (in my recent theoretical work [Alinei 1995, 1997a, 2000b, 2001, 2002, 2003, fc] I have replaced the much too ambiguous term *motivation* with the technical neologism *iconym*: *iconymy* being the linguistic phenomenon, *iconymic* the adjective and *iconomastic* the noun designating the new kind of linguistic record and the new level of linguistic investigation, *iconomasiology* motivational onomasiology), to distinguish it from the traditional *onomasiological* one; (*3*) from the *cultural-historical* point of view, it introduced the "three-stage ideological evolutionary theory", namely the theory according to which mankind had gone through three main stages of ideological evolution: in chronological order, the zoomorphic/totemic stage, the anthropomorphic/pre-Christian and pre-Islamic stage, and the anthropomorphic Christian and Islamic stage. All of this in accord with archaeological and anthropological evidence, namely the first stage corresponding to Paleolithic hunting and gathering societies, the second to Neolithic and Metal-Age agro-pastoral and urban, socially stratified, societies, and the third to history; (*4*) from the *technical* point of view it permitted the realisation of *multiple mapping*, whenever the richness and complexity of the iconomastic record might require it.

I presented 'arc-en-ciel' for the first time at an ALE Romance Department meeting at Toulouse, in 1978, and to my great relief it was a success. Later, Michel Contini was kind enough to say, on several occasions, that his initiative to start the *Atlas linguistique Roman* in the Eighties was basically due to the prospects that my 'arc-en-ciel' essay and the new motivational approach to geolexicology had opened. I repeated the presentation of 'arc-en-ciel at the general ALE meeting in Moscow, in 1979, and it was again a great success, despite some critical remarks from Prof. Serebrennikov, the late specialist of Finno-Ugric languages of the USSR, who objected that my method was not orthodox! Fortunately for the future of the ALE, it so happened that at the same meeting Prof. Avanesov, Prof. Ivanov and Dr. Donadze presented their own map, based on the question 'sauterelle', which was also conceived motivationally (albeit without the three-stage theory!). Prof. Avanesov's reaction to my 'arc-en-ciel' was very positive and its authority could thus balance off Prof. Serebrennikov's condemnation which, otherwise, might have had very serious, negative consequences for the ALE!

Before my official presentation of 'arc-en-ciel', of course, I had shown my map to Toon Weijnen, and he, too, was quite impressed with it. I had thus hoped that this work of mine, as well as the repeated success it had had within the ALE community, of which he was well aware, would open Weijnen's mind to my view about the size and the quality of ALE commentaries. This was not the case: preparations of the essay-volume the way he saw it continued unchanged, and finally the result was delivered to the DFG for financing.

We come then to 1982: the year when Toon Weijnen would retire and I would become the next ALE President. Shortly before the ALE meeting, which was due to take place in Copenhagen, came the bad news: the DFG had flunked the essay-volume. Main arguments: superficiality of commentaries, unprofessional handling of IE problems.

The Copenhagen meeting in 1982, then, turned out to be a dramatic one: what would the new President do in the face of the last development? The meeting was, in my experience of ALE President, the most "public" one: hundreds of people crowded the rooms; possibly, the whole Danish Academy was present. The atmosphere was at the same time festive, excited and tense. Festive because there was, in all of us, the need to express our deep gratitude to Toon Weijnen for all he had done for the ALE until then, and in some of us, satisfaction for my instalment; excited and tense because nobody knew how I would react to the news. I remember contributing to keeping the atmosphere as festive as possible, when, toasting to Weijnen, I translated the Latin "in vino veritas" in the almost equivalent Dutch "in Weijnen veritas"!

But when we came down to business, things did not go smoothly: Weijnen took the floor to break a lance for the continuation of the project as he had led it. And I, then, was forced to come out with my opposite announcement: I was proposing article-like commentaries, with a separate volume for their publication, and the installation of an IE commission that would evaluate commentaries from the point of view of IE scholarship. I still remember the loud murmur in the hall, and the expressions of shock on some faces.

Twenty five years later, I still think I did the right thing. Not only because it was, clearly, the only way out of the problem we were facing with the DFG; and because my formula, from a scientific point of view, was undoubtedly better than the one conceived by my predecessor. But also because later, Toon Weijnen himself, both in speaking and in writing, repeatedly admitted to me that I «had saved the ALE». Nothing pleased me – and still does – more than that recognition, which was and is for me, among other things, the confirmation that my deep esteem for Weijnen was well placed: Weijnen is one of those extremely rare intellectuals who are generous enough to recognise the value of others.

Yet, to my surprise, the opposition between supporters of article-like commentaries and supporters of list-like or so called "technical commentaries", did not end in Copenhagen. By far not. A few years later, in 1986, it came out, this time explosively, at the Edinburgh ALE general meeting: which was, without any doubt, the stormiest in the ALE history.

Just like the Copenhagen meeting of 1982, this meeting had been preceded by an important institutional decision that had put the ALE, again, in a critical situation. This time, however, both the decision and the critical situation that ensued had been well prepared in advance by our opponents: since the Dutch and German financings had come to their natural end, two members of the ALE Editorial Board – Prof. Hagen of the Catholic University of Nijmegen and Prof. Putschke of the Philipp University of Marburg – had presented a new plan for the continuation of the ALE, and the Dutch institution let us know that they would continue its financing only if we accepted it. The plan – as it turned out at the meeting – was supported by Toon Weijnen himself (who, however, told it to me in private with a painful expression on his face) and by the whole ALE Germanic Department, and it aimed at a radical change in the conception of the project, involving publication of maps with a short technical explanation. The main argument for this request of change was that the project advanced too slowly (in 1986 the second fascicle had just come out), and the certainty that the requested change in the format would ensure its acceleration.

This argument, in the opinion of most ALE Editors, was quite weak, as it boiled down to the well-known choice of "quantity" over "quality": a choice which raises doubts even when it concerns apples, shoes or cars, let alone a scientific project. Yet the debate was extremely vehement, and I will never forget Prof. Putschke who, after it was clear that the majority opposed his plan, screamed to us "you are murdering the ALE!".

Twenty years later, Nils Århammar, from the beginning member of the ALE Editorial Board and one of the world's best specialist of Frisian language and dialects, visiting me in Florence, apologised to me for having voted, on that occasion, "for the wrong side". It was nice to hear it!

Why so much opposition, and with such emotional intensity, to a project that after all had been quite successful *just because* of the complex and rich format it had adopted? The answer can only be one: a prestigious project such as the ALE raises not only scientific interest, but also, inevitably, personal ambitions and national pride. These are noble sentiments, of course, except when they are given priority over science. And this was obviously the case: for, from a scientific point of view, the argument that list-like commentaries would radically change the tempo of the project is not only pure nonsense but also clashes with the definition of the ALE as an *interpretive* atlas. A definition that had been decided from the very start both by Toon Weijnen and me (and even by Prof. Schmitt, for that matter).

Data listing and data interpreting are two realities of a completely different kind. Data listing can be computerised to reach maximal speed, data interpreting can also be computerised, but without reaching maximal speed.

Let me make this point as clear as possible: in Holland, during my first years at Utrecht, I was myself leader of a large computerised databank project (one of the first in the history of computational linguistics): the *Electronic Inventory of Early*

Italian (*Spogli Elettronici dell'Italiano delle Origini e del Duecento* = SEIOD). All early Italian texts, from the origins to Dante, were digitalised, in order to generate complete concordances, frequency lists, reverse lists and statistical data over words and graphemes. In 10 years, from 1968 to 1978, I was able to publish (with Il Mulino, Bologna), 20 volumes of data, averaging 500 pages each, for a total of 10,000 pages.

Why was this possible? Because the formal level of investigation, throughout the project, was the *written form*. No interpretation was needed at that level, as the computer could identify both graphemes and words. The tempo for the realisation of the project, in fact, was determined solely by the time that was spent for the digitalisation of the texts, for the funding of the costly publication and for the publication itself.

On the contrary, what was – and still is, and will always be – the chosen level of investigation for the ALE? This is the question that our good friends of the Germanic Department should have stopped to ask themselves, and unfortunately never did. And the answer is: the obligatory level of investigation for the ALE is *etymology*. There is no way to produce an *interpretive* atlas, whether onomasiological or motivational, without etymology. And etymology is hardly something that can be produced in a superficial and approximate way, and even less approached quantitatively. If this is true in general, imagine how important it becomes for an interlingual, European project, for which etymology implies not only careful, thorough and complex comparison of phonetic, morphological and lexical issues in different dialects within the *same* language area, but also of typological and motivational features in *different* language areas. Anyone who speaks of accelerated "data listing" in such case simply proves that historical linguistics is not his cup of tea, and might ask himself what is doing in the ALE.

This is why, in essence, the opposition between article-like and list-like commentaries for a project like the ALE was and is a pseudo-concept: to produce a simple list of well-searched etymologies on a European scale for the same notion not only would take almost the same time than writing an average article explaining the listed etymologies, but also – and more important – would not compensate the enormous effort put in by individual researchers.

In this regard here is how I stated my position in my last presentation of the ALE (Alinei 1997, 31):

> Only those who have contributed to the ALE as authors have experienced the enormous complexity of putting together the various etymological syntheses produced by Departments and National Committes (Celtic, Germanic, Romance, Slavic, Baltic, Greek, Albanian, Uralic, Turkic, Basque, Maltese, etc.), on one single map, giving sense to a collection of materials coming from more than 2,600 net-points. The problems to face are staggering, and go from rigour and coherence of etymology at Indoeuropean level, to the choice of levels of distinction for phonetics and morphology, to the construction of a readable legend, to the choice of good symbols for the rendering of the areal distribution. This is where ALE authors spend most of their time, and what keeps them busy for a long period of time. [...] For purposes of production, at this point, it really does not matter whether authors have written an article or a list of technicalities, while for authors having produced an article is rather a reward than a chore.

We should also consider that the ALE, as a very slow-pace project, is certainly not an exception, on the contrary: most of the vast scientific enterprises involving etymology that were started in the last century, and even in the 19th century – such as *Schweizerisches Idiotikon. Wörterbuch der Schweizerdeutschen Sprache*, first published in 1881, the *Glossaire des patois de la Suisse Romande*, first published in 1924, the *Dicziunari Rumantsch Grischun*, first published in 1939, the *Vocabolario dei dialetti della Svizzera italiana*, first published in 1952 – are far from their completion, with only a few volumes published so far. Even the FEW, first published in 1949, is now re-publishing a new version of the first few volumes, totally inadequate in comparison with the definitive format of the others. The ALE, let us not forget it, is the *first interpretive* (i.e., etymological) lexical atlas ever realised.

Of course, the desire to speed up publication was and is a perfectly legitimate one, especially if it concerns the authors of maps and commentaries, whose expectations to see their work published obviously cannot be frustrated too long. But the solution for solving this problem will never be the one so eagerly and thoughtlessly proposed by the Germanic Department in 1986. In my opinion, the most practical solution is, rather, the one cleverly inaugurated by Prof. Viereck during his presidency, in a pragmatic way and without any official announcement: to pre-publish ALE commentaries outside the ALE, in journals or other academic publications, prior – that is – to their definitive publication, with maps, in ALE volumes. Dr. Brietz (Eder)'s ALE commentaries on some family names have already been published by Peter Lang as a book in 2004 , and Prof. Viereck himself has already published partial ALE commentaries in *Festschriften* and journals. Toon Weijnen and I were not able to do this, as it would have involved a breach of contract with financers and/or publishers. But those were different years. If now over-all financing no longer exists, and if commentaries are published in a preliminary form, and without maps, there is no reason that I can think of, not to do it. The only risk, if the ALE publication stagnates, is that libraries might cancel their subscription, but this would be temporary, and the risk would be compensated by the fact that the ALE would become, in any case, a unique platform for young researchers who want to work on European onomasiology. A prospect that should have, in my opinion, high priority, especially as young linguists interested in the relationship between language and cultural history could not find a research field as rewarding as the ALE.

To conclude my reflections on my decision of 1982 and on the ensuing debate of 1986, I would like to mention another very important support to my vision of the ALE as a scientific project: the one that has come, in the last decade, from my successor in the ALE Presidency, Prof. Viereck. In his numerous illustrations of the ALE – the last of which appeared in the 14th issue of DiG in 2006 – he has become the strongest supporter of the "cultural significance" of the

ALE, identified with my “three stage theory” of European evolution, and presented with the same arguments and the same bibliography used in my four contributions to the ALE: ‘arc-en-ciel’ (1983), ‘belette’ (1986), ‘coccinelle’ (1990, with Barros Ferreira)), and ‘Noël’ (1997b), and further elaborated in my books and articles (e.g., Alinei 1984ab, 1985, 1988, 1992ab, 1993, 1994, 1997cd, 2000b, fc). He is also the only ALE author who has explicitly applied the three-stage theory to new dialect material, as for example in his article on the names of diseases (Viereck and Viereck 1999) (in which, however, my previous work on the same topic is not mentioned).

And I find it even more flattering that Prof. Viereck usually presents my theory as a sort of “received doctrine”, without referring to me or to my publications, as if I were a sort of “sacred”, Chomsky-like authority (and I apologise for my immodesty with respect to Chomsky), to whom you don’t even need to refer in presenting his established theory to the outside world.

Coming to another aspect of ALE research, but without leaving Prof. Viereck’s work, I would like to express my admiration for his sharpness of judgement: he is the first scholar, to my knowledge, who has clearly seen, and made explicit, the connection between the “three-stage theory” of my early ALE publications, and the Palaeolithic Continuity Theory (PCT), which I presented in the two volumes on the origins of European languages (Alinei 1996a, 2000a) and later works (see, for bibliographic references, www.continuitas.com).

I must confess, the three-stage theory was the way I “smuggled” unorthodox ideas into the IE field in the early Eighties: implicitly, I challenged the traditional theory to account for the co-existence, throughout Europe, of Paleolithic totemic motivations, Neolithic and Metal-Age anthropomorhic pre-Christian and pre-Islamic motivations, and historical anthropomorphic Christian and Islamic motivations: how could this have come about if this evolutionary sequence had not developed *everywhere in loco*? And how could we explain the fact that the linguistic forms of all the three-stage motivations are coherently either IE or Uralic or Altaic or Caucasian or Basque, etc., depending on whether they appear, if European ethno-linguistic groups had not been in their areas from Paleolithic on?

In the early Eighties, the traditional answer of IE scholarship was, as is well-known, twofold: (*1*) there was a gigantic invasion that at the beginning of the Metal Age brought Proto-IE into Europe, and (*2*) this invasion was followed by just as gigantic a process of “calque” translation from “Pre-IE” original motivations to IE languages.

But then came Renfrew’s book of 1987, bringing the first *tsunami* wave in the long-sleeping IE world, telling every scholar who was willing to hear it that modern archaeology had irrefutably proved that there was no mass invasion of Europe in the Chalcolithic and that, on the contrary, there was just as irrefutable evidence for cultural and demic continuity from Paleolithic to the Bronze Age in Europe. And in his book he also illustrated his own new, for me unconvincing but elegant, Neolithic Dispersal theory (NDT), according to which the Proto-IE were the first Neolithic farmers coming from the Middle East, now identified by modern archaeology as the area where agro-pastoralism first began, and from which it was introduced into Europe.

And in the Nineties the results of the new genetic research brought the second *tsunami* wave, not only among IE scholars, but also in Renfrew's camp, irrefutably proving that the genetic stock of European people goes back to Paleolithic. And at the same time came also the first articles by archaeologists and linguists such as Marcel Otte (1995, 1997ab, 1998, 199, 2000, 2003), Alexander Häusler (1996, 1998, 2003), Homer L. Thomas (1991-92), Cicerone Poghirc (1992) and others, sketching what we now call the Paleolithic Continuity Theory (PCT), as well as my two books (Alinei 1996a, 2000), which represent the first detailed linguistic illustration of the PCT, with an attempt to combine the European linguistic record with the available archeaological data.

So, my question to the growing ALE community is: what about the relationship between the ALE and the IE theory? Has the time not come to verify if the solution envisaged in 1982 is still adequate? Has the ALE scholarship, now certainly more mature than twenty years ago, faced the consequences of the crisis of the traditional IE paradigm? For nobody can doubt that there is a deep, structural and fundamental relationship between the chronology and the scenario of the PIE ethnogenesis and differentiation, and the genesis and the development of the Celtic, Italic, Germanic, Balto-Slavic, Greek and Illyrian language groups.

If I may hazard some suggestions, the question ought to be asked, first of all, if the IE Commission, the installation of which in the ALE structure was decided in 1982, can remain the same as it was conceived then. My answer would certainly be negative: even if, out of respect for the role of traditional thinking in science, we would still want to consider the Invasion Theory (IT) as a still viable one (which, in my opinion, is absolutely necessary for the morpho-phonetic formalism (except for laringalism), but absolutely impossible as far as the scenario and the chronology are concerned), should the ALE Commission not be expanded in order to have at least equal representatives for the IT, for NDT and for the PCT? And should authors of ALE commentaries, depending on their personal opinions, not be left free to elaborate their materials on the basis of one or the other of the three theories, at least as long as a new commonly accepted paradigm will crystallize?

I would also liked to suggest that young scholars interested and well read in the debate about IE and European ethnogenesis should be invited to experiment with the new different IE points of view, comparing the results with the traditional ones. For the consequences of both the NDT and the PCT for the traditional chronology, and especially for the scenario of each IE linguistic group, are simply immense. This is why I will devote the second part of this paper to the illustration of a few examples.

WHY A COMMON IE WORD FOR 'DYING', AND SO MANY FOR 'BURYING' AND 'GRAVE'?

I will begin with an example that will interest, I hope, Prof. Viereck, who has just published, in anticipation of the next ALE fascicles, a thorough etymological analysis of the designations of 'grave' (Viereck 2005). I will introduce my comments with the following question:

Why has IE a common word for 'dying', but not for 'burying' and 'grave'?

And then we will compare the three answers that are possible in the light of the three models. Let us first have a glance at the data:

	CELTIC	GERMANIC	ITALIC	GREEK	BALTO-SLAVIC
'to die'	PIE ****mer-***				
'to bury'	OIr. *adnaicim* W. *daearu*, Br. *douara* etc.	OIcel. *jarđa*, *grafa* Swed. *jorda* Germ. *begraben* Du. *begraven* Engl. *bury* etc.	Lat. *sepelīre*	Gr. *tápto*	Lith. (*pa*)*laidoti*, *pakasti* Latv. *aprakt*, *apbedìt*; OSlav. *pogreti* Serbo/Cr., Slovn. *pokopati* Serbo/Cr. *sahraniti*, Slovn. *skriti* etc.
'grave'	OIr. *fert*, *lecht* etc.	Ger. *Grab*, Engl. *grave* Dan., Swed. *grav*. Du. *graf* etc.	Lat. *sepulcrum*	Gr. *táphos*	Ru. Serbo/Cr. *grob* Cz. *hrob* Pol. *grób* etc. Lith. *kapas*, Latv. *kaps*

Let us now compare the interpretations of this picture according to the three models:

(1) Within the traditional frame of the IT, it is difficult to understand why and how this picture would develop (and this is probably why no discussion of this problem seems to be present in the literature): assuming Gimbutas' (and now Mallory's) "*Kurgan* people" invasion as a reality, should we not expect a common IE name also for 'burying' and 'grave', since precisely the *kurgan* was the typical burial of the alleged Proto-Indo-European invading warriors? Prof. Viereck's recent article does not touch this point.

(2) And also in Renfrew's NDT, why would PIE farmers coming from the Middle East, and invading or infiltrating Europe, but still speaking a common language, would have innovated, immediately after their arrival and in every single IE area, their common word for 'grave'?

(3) Only in the PCT framework the obtained explanation would be logical, and in perfect correspondence with the present knowledge of European prehistoric developments. For Europe, as is known, appears to be widely differentiated in

cultural areas already in the Upper Paleolithic, and while the notion of 'death' can go back to Middle Paleolithic, namely to *Homo loquens*' first lexical classification and articulation of the universe and of individual and social life, for the notions of 'burying' and 'grave' we must wait, indeed, for the Upper Paleolithic when, with *Homo sapiens sapiens* and a higher degree of intellectual and cultural development, religion and ritual begin, with the generalization of burial accompanied by the careful preparation of the corpse and the addition of personal ornaments and symbols (Clark and Piggott 1970, p. 50 ff.). And cemeteries (the names of which are even more numerous than those of 'grave'), begin even later, in Mesolithic (Gamble 1986, 381).

Consequently, it becomes quite simple and coherent to project the notion and the word for 'dying' onto the Middle Paleolithic, and therefore seen as belonging to the Common IE, while the notions of 'grave' and 'cemetery' – necessarily belonging to, respectively, Upper Paleolithic and Mesolithic, when IE languages, following the archaeological record, must be assumed as already differentiated –, could only be expressed by different IE words.

WHY DIFFERENT IE NAMES FOR THE 'BEAR'?

Another experiment can be made with the names of the 'bear'. As is known, the "real" name of the bear, reconstructed as Proto-IE * **rkθo-s**, or ***r̥kto-s**, or ***r̥ kso-s** (IEW 875), survives in Skt. *r̥kṣa-*, Av. *arša-*, Oss. *ars*, Arm. *arj*, Alb. *arí*, Gr *árktos*, *arkós*, *Arkádes* (> Neogr. *arkoúda*), Lat. *ursus* (> It. *orso*, Fr. *ours*, Sp. *oso*, Rum. *urs*), OIr. *art*, W. *arth*. Other IE languages have replaced it with different innovations, all clearly connected with a taboo prohibiting to pronounce the totemic animal real name, as shown by the following table:

CELTIC {good calf}[2]:	GERMANIC {brown}:	BALTIC probably {hairy}:	SLAVIC {honey eater}:
OIr. athgamain, Ir. mathghamhain	OIcel. *bjorn* Dan. *bjørn* Swed. *bjorn* Engl. *bear* Germ. *bär* Du. *beer* OE *bera* OHG. *bero* etc.	Lith. *lokys* Latv. *lacis* OPruss. *lokis*	OSlav. Serb./Cr. *medvjed*, Cz., Slovn. *medved* Pol. *Niedžwiedž* Ru. medved' Ukr. medvid

Now we know from history of religion, anthropology and ethnography that tabooing of hunted animals was born when they became "sacred" (just as other

[2] In my recent work I have introduced the convention {} to distinguish the iconym/motivation from meaning.

"sacred" aspects of life, still now, are tabooed), and thus object of a cult, within that earliest form of religion which is called *totemism*.

In both the traditional IT and in Renfrew's NDT we would have to assume something absurd: namely that this substitution process would have taken place, respectively, in the Bronze Age or in the Copper Age. Which would raise a question: why would be the bear be tabooed in the Metal Ages, long after hunting had become quite marginal to human subsistence?

More realistically, the PCT projects the common IE "real" name of the bear in the Middle Paleolithic, that is prior to the beginning of religious beliefs, and the new, noa names of the bear in the Upper Paleolithic, when IE languages would already be differentiated, religious thinking begins and, incidentally, many forms of bear cult begin to be attested. Let us dwell on the latter point.

The existence of a bear cult has been suggested by archaeologists on the basis of numerous findings of bear bones in caves dated to Musterian and Upper Paleolithic. As examples of Musterian sites can be mentioned: Regourdou in France, where a bear had been buried and its burial covered with an 850 Kg heavy stone, with bear bones scattered around. Drachenloch (an interesting, typical name!) in Switzerland, where a stone cist had been built to contain bear skulls and long bones, intentionally chosen, had been placed along the cave walls; and in another heap of bear bones, resting on two other long bones of two distinct bears; Petershohle in Bayern, where ten bear skulls had been placed on a natural platform in the cave; Wildemannlisloch (another interesting, typical name!) in Germany, where 310 bear canines had been stacked up; Les Furtins in France, where six bear skulls had been placed on stone slabs, two more on the floor, and a heap of long bones on a slab against the wall cave; Veternica in former Yugoslavia, where bear bones had been place in a crevice, later closed with stones; alignments of bear bones have been found in the Isturitz cave in French Pyrenees, and many caves in the former USSR have revealed numerous bear bones (Wymer 1982: 172). As an example of Upper Paleolithic Montespan can be cited, where a bear skull has been discovered between the paws of a headless bear sculpture, whose head is presumed to have been attached to the sculpture, probably with the skin still attached to cover the sculpture (Wymer 1982: 258, Clark & Piggott 1970: 80). Finally, in the now famous painted cave at Vallon-Pont-d'Arc, in the Ardèche, recently discovered by the French archaeologist Jean Paul Chauvet, and the paintings of which, dated to 30,000 years ago, have been considered superior even to those of Lascaux (*Archeo* X 3, 1995, 18 ff.), a bear skull has been found on a sort of 'altar'.

This is why, in the framework of the PCT, the areal distribution of the different noa names for the tabooed bear would reflect the diffusion of the new religious taboo rituals and rules, when IE languages would have already been differentiated.

WHY DIFFERENT IE NAMES FOR 'FISH'?

As is known, one of the *cruces* of the IE linguistics is the lack of an evident Proto-IE term for 'fish'. For the IT, as well as for Renfrew's NDT, it remains a puzzle why Proto-IE, still speaking the same language, respectively, at the beginning of Chalcolithic or Neolithic, would not have a common name for 'fish'. The best specialists of IE traditional linguistics have tried to solve the problem, each coming to a different solution, as is always the case when there is something wrong in the general assumptions.

Only the PCT provides a clear and realistic answer, in full concordance with the acquisitions of modern prehistorians. For archaeology places the introduction of fish in human subsistence only in Upper Paleolithic (Gamble 1986: 247). And, more important, the beginning of fishing as regular activity is first detected along the *Atlantic coast*, in the area that according the PCT has been Celtic since the beginning of IE differentiation in Upper Paleolithic. Recall also that the Baltic Sea was frozen throughout Paleolithic (with the exception of Interglacials), and acquired its modern shape only in Mesolithic times.

It would become then easy to understand why – irrespective of their etymologies – there are three different IE names for 'fish' in Europe (four, if one considers IE Eurasia, adding Indo-Iranian) (Buck § 3.65):

(1) Celtic Germanic Italic: AIr. *īask*, Goth *fisks*; Lat. *piscis*;

(2) Greek, Armenian, Baltic: Gr. *ichthýs*, Arm. *jukn*, Lith. *žuvìs*, Latv. *zuvs*, OPruss. *suckis*;

(3) Slavic *ryba*.

In Upper Paleolithic Europe, already culturally and linguistically differentiated, but still compressed to the South by the icecap covering the whole of Northern Europe, the earliest term for 'fish' would probably be the Celtic one, which would then spread, as a loanword, in the two contiguous areas, but without reaching Greece. The Greek term would be developed independently, spreading then to the East and to the North; and the Slavic word would be a later innovation.

WHY DIFFERENT IE NAMES FOR 'TAR'?

Our third experiment of comparison between the three models concerns the names of a more recent innovation: 'tar'. The production of 'tar' from trees is unanimously considered by archaeologists as a Mesolithic technological innovation. It is then quite significant that in Celtic, Germanic, Latin and Greek the name of 'tar' is not only different, but comes from the name of a different tree, while Balto-Slavic shows a different development (see Table):

CELTIC	**GERMANIC**	**LATIN**	**GREEK**	**BALTO-SLAVIC**
(Gall-Lat.) *betulla* ‘**birch’** > *bitumen* ‘tar’	PIE **ter* ‘**tree’** > Engl. *Tar* Germ. *Teer*, Du. *teer*, Dan. *tjære* Swed. *tjära*, Norw. *tjøra* ‘tar’ (> Finn. *terva*.) etc; OIcel. *tjorr* ‘wooden haft glued to a weapon’	*pinus* ‘**pine’** > *pix, picis* ‘tar’ (It. *pece*, Fr. *poix*, Sp., Pg. *pez*, Occ., Cat. *pega* etc.)	*pítys* ‘**pine’** > *píssa* ‘tar’	PIE **smel-* ‘**burn’** > Latv. *smęli* Lith. *smelà* Bulg., Cr., Cz., Ru., Serb., Slovk., Slovn. *smola* Pol., Ukr. *smoła*

Let us now compare the three models:

(1,2) In the traditional IT, as well as in Renfrew’s NDT, the semantic development and the lexical differentiation cannot be explained altogether: why would Proto-Indo-Europeans, certainly having a common word for ‘tar’ irrespective of whether they arrive in Europe in the Copper Age (IT), or in Neolithic (NDT), innovate *not only the word but also the tree*, in the Bronze or Copper Age, when tar production already belonged to the traditional technology and played no special role?

(3) Within the PCT, in Mesolithic IE languages would have already been differentiated, and at the time of the invention of ‘tar’ each ethnolinguistic group would have chosen its ‘tree’, and consequently a different word, to designate the new material.

WHY DIFFERENT IE NAMES FOR ‘BOW’?

Also the IE names of the ‘bow’, a typical Mesolithic invention, are quite differentiated:

Celtic	Germanic	Latin	Greek	Baltic	Slavic
Bret. *gwareg* < *gwar* ‘to bend’	Germ. *bogen,* Engl. *bow,* Du. *boog*, Dan. *bue*, Swed. *båge* etc., < **beuga-* ‘to bend’	*arcus* > It., Sp. *arco*, Fr., Rum. *arc*	*toxon*	Lith. *lankas* cf. *lenkti* ‘to bend’; Latv. *stuops* < *stiept* ‘stirare’	Oslav. *ląkŭ* Cr., Cz., Ru., Serb. *luk*, Pol. *łuk*

Let us read this picture with our usual three keys:

(1,2) Both the readings obtained in the framework of the IT and in that of Renfrew’s NDT are quite difficult to accept. For again, assuming the existence of a common IE name for ‘bow’ at the moment of the alleged IE invasion, either in the Copper age or in Neolithic, it is not clear what would have caused the need to innovate this name everywhere, after the alleged IE invasion, since neither in Neolithic nor in the Metal ages did the bow undergo any special development. Moreover, the motivation {bend} shown by three of the five names clearly proves that these are “original” names of the bow, and not later, modified innovations.

(3) Within the PCT, the already differentiated IE languages would have simply developed a different name for the new weapon.

WHY DIFFERENT IE NAMES FOR ‘THOUSAND’?

It is needless to recall the different names for ‘thousand’ in IE languages: Lat. *mille* (> Ir. *mile*, W. *mil,* etc.), Gr. *khílioi*, and the Balto-Slavo-Germanic isogloss OSlav. *tysešti*, *tysošti*, Pol. *tysiac* (d), Czech *tisíc*, Serbo/Cr. *tisuča*, Slovn. *tisoč*; Lith. *tukstantis* (d), Latv. *tukstuots* (d); Goth. *þusundi*, OIcel. *Þúsund,* etc. Irrespective of the etymology of these words, in most cases obscure, the first question we should ask ourselves is an anthropological, cognitive one: why would ‘thousand’ not belong to the common IE lexicon, like ‘hundred’ and the basic numbers? Let us, again, compare the three models.

The IT obliges us to assume the most absurd explanation, namely that *at the beginning of the Metal Ages PIE humans had not yet reached the intellectual capacity to count up to thousand*! Obviously, this capacity must have been reached with the new Neolithic economy, which certainly created the need to develop the high numbers associated to stock raising and to the storage of wheat and other farming products (Alinei 1996a).

This consideration, of course, is valid also for the NDT: for early Neolithic farmers coming from the Middle East would have certainly developed a common number for ‘thousand’, which they would have then introduced, with the other PIE numbers, into Europe.

The fact that ‘thousand’ has a different names in Europe can be explained only in the framework of the PCT: which assumes that by Neolithic times all IE language groups had long been differentiated, and consequently each of them would have developed a different word for the new number. The Slavo-Germanic isogloss would confirm this thesis, given the lower chronology of the Germanic Neolithic (LBK), compared to the Slavic one (Balkan complex), and the archaeologicallly well studied influences of the latter on the former.

WHY DIFFERENT IE NAMES FOR ‘WHEEL’?

As is known, there are three main word families for the ‘wheel’ in IE languages: the family of ChSl *kolo* (including Gr. *kýklos*), the family of Lat. *rota* and Gr. *trokhós*. This notwithstanding, IE specialists, following the IT, have reconstructed a PIE ****roto***- 'wheel', on the basis of Lat. *rota* (> It. *ruota*, Fr. *roue*, Sp. *rueda*, Rum. *Roată,* etc.); Ir. *roth* (m.), W. *rhod* (f.), Bret. *rod*., Gall. *Rotomagus* (*Rouen*); Germ. *Rad*, Du. *rad*; Lith. *rãtas* m. 'wheel, circle' (pl. *rãtai* 'carts, vehicle'), Latv. *rats* (Plur. *rati* 'vehicle'), (> Finn. *ratas* 'wheel'); Skr. *rátha*- (m.) 'chariot', Av. *raθa* 'idem', from PIE ****ret*(*h*)** 'run' (cf. IEW 867, Buck §10.76). The traditional, IT assumption is, of course, that the invading warriors would have had a common word for 'wheel' and that the other 'wheel' names are later innovations.

Weijnen (2002), in his excellent (but unfortunately still based on the IT and ignoring both Renfrew's NDT and the PCT) ALE commentary on 'roue' (ALE I 6), comes to a different conclusion, relying on the fundamental distinction – already made by Heyne (1901), and later resumed by Weijnen (1974) and by myself (Alinei 1996b, 2004) –, between the more primitive and earlier, disc wheel, and the more advanced and recent, spoked wheel. On this basis, the *kolo*-*kýklos* type, coming from the PIE root ***k^uel-***, simply meaning 'rotate', would probably designate the ancient solid wheel, while the *rota* type, derived from the Celtic verb for 'running', would be a recent Celtic loanword, to be compared to Ir. *roth* (m.), W. *rhod* (f.), Bret. *rod*, in turn deriving from OIr. *rethim* 'I run', W. *redheg*, Bret. *redek* 'to run', etc., while the Greek type *trokhós*, based on a motivation that would not be {run}, as traditionally thought, but (following new insights) {turn}, might correspond to the so called cross-bar wheel, an intermediate type of wheel, later than the solid but earlier than the spoked wheel. The three names would thus correspond to three completely different wheel technologies, of which the earliest would correspond to the PIE period.

This view, in comparison to the traditional one, represents a step forward: Pokorny (IEW 867) and later scholars ignored the rich archaeological record on ancient wheels, overlooked the relevance of the distinction between the ancient solid and the recent spoked wheel for the etymological problem of their name, and made consequently two major mistakes:

(1) on the linguistic level, they failed to see that the connection between a reconstructed ****roto***- 'wheel' and ****ret*(*h*)**- 'run' would imply the existence of a *fast*, spoked-wheel and horse-trained vehicle, whereas the connection between *kolo* and cognates with ***k^uel-*** 'rotate' would be more appropriate for a slow, disc-wheel and ox-trained vehicle;

(2) on the cultural level, ignoring the fact that the *spoked* wheel for fast vehicles is a "new invention" (Childe 1954, 214), dated «from hardly before the beginning of the second millennium BC in Western Asia or Europe» (Piggott 1983 27, cf. Mallory 1989, 127), they failed to see that it would be impossible, even within the lower chronology of the traditional IT, to consider the *rota* family as belonging to the common PIE stock. The word must be seen, instead, as a local innovation, and its areal distribution would be that of a borrowing.

Weijnen, however, fails to see the implications of his (and my) conclusion that the *rota* family represents a Celtic loanword. In his discussion of my article

(Alinei 1996b), though accepting my arguments enhancing the interpretation of the *rota* type as a Celtic loanword, when it comes to antedating the Celtic presence in Europe, he objects:

> Alinei muss aber gestehen, dass dies auch für die Formen in Persie und Indien gilt, und spricht letztlich von einer «peaceful anticipation of the Celtic wave of conquest of proto-historic times, which extended to Anatolia». Überdies müsste man folglich annehmen, dass die Kelten schon «in the second, third millennium» in [West-]Europa waren, was schwer zu beweisen ist.

And here, as an objection to my view, he recalls Schmidt's three "grammatische Eigentümlichkeiten", shared by Celtic with IE Eastern languages, but missing in Italic. An objection that does have some value, but only if one, following the IT, synchronizes the deepest PIE grammatical features with a recent, Metal Age technology such as the wheel's. If this synchronism falls, the objection vanishes.

More important, when stating that the Celtic presence in earlier Western Europe is "schwer zu beweisen" Weijnen makes use of the typical epistemology of the IT, in which there seems to be no space for the discoveries made by archaeology and genetics in the last decades. An epistemological model which is, obviously, unacceptable from a scientific point of view as it would prove to favor a view simply because it belongs to the traditional doctrine, at the same time ignoring the new factual discoveries that make that doctrine untenable, and thus creating the typical basis for a "dogma".

The fact is that modern archaeology has demonstrated not only the absence of any trace of mass invasions in Europe, but has also gained overwhelming evidence for uninterrupted cultural and demic continuity throughout Europe from Mesolithic (and in certain areas earlier) to the Bronze Age. As far as the Celts are concerned, consequently, what must be proved, epistemologically, is not the earlier presence of Celts in Western Europe, but their alleged "arrival"! Of which there is no trace whatsoever in the archaeological record, which, on the contrary, as I have already said, proves beyond any doubt cultural continuity in the whole of the area. The problem of the "arrival" of the Celts in Western Europe, on which there is now a steadily growing literature (cf. Alinei 1996a, 2000), is thus an unsolvable one, unless one faces archaeological reality, abandons the IT altogether, and assumes that the Celts (as well as the other IE groups) were always where they are now.

In the light of these remarks, the line of research we ought to adopt for an adequate interpretation of the three names of the wheel, and in particular of the *rota* type, is the following: (1) if the *rota*-family is a Celtic loanword, as both Weijnen and I assume, there must be evidence for a Celtic primacy in European Bronze-Age wheel technology. (2) If we succeed in finding this evidence, we must also ask ourselves what its implications are for the prehistory of the Celts and, in general, of Europe (cf. Alinei 2004). Let us address the two points separately.

THE HISTORICAL EVIDENCE FOR CELTIC PRIMACY IN WHEEL AND VEHICLE TECHNOLOGY

As to the evidence for a Celtic primacy in wheel and vehicle technology, this is simply overwhelming, and comes from both philology and archaeology:

(1) In Latin, the whole cart terminology is Celtic: *benna, cant(h)us, carpentum, carrus, carrago, carracutium, carruca, cisium, colisatum, covinnus, essedum, petorritum, pilentum, ploxenum, raeda*. *Rota* could be easily be added. The Latin record shows thus a *general* lead of the Celts in cartwright technology.

(2) For Latin writers, the most famous *spoke-wheel makers* were the Celts (Jope 1956), and their fame was especially due to their ability in creating *spoked wheels with felloes of one single piece of heat-bent wood* (Childe 1954). As is known, the felloe is the curved piece of wood on which the spokes are fitted, and is thus one of the three main components of the spoked wheel, the other two being the nave and the spoke.

(3) The archaeological record confirms that single-piece felloes, made of heat-bent wood, were characteristic of the Celtic Iron Age (La Tène) (Piggott 1983: 27), and that prior to that period the standard form was "a felloe in which the segments [we]re dowelled one to another, each carrying two spokes (*ibidem*)". Now, the earliest example of this kind comes from a second-millennium German find from Barnstorf (Oldenburg) (Piggott 1983: 27, 168). Even in that period, then, we find ourselves in a putatively Celtic area.

(4) Also Latin *cant(h)us* 'felloe, rim of the wheel', is considered to be a Celtic loanword, to be compared to W. *cant* 'rim of the wheel, tyre', and Bret. *kant* 'rim of the sieve' (DELL). The Latin form is continued by Fr. *jante* and Gallo-Romance and Occitan dial. variants (contaminated with *jambe*; FEW, without good reasons, posits *cambita* as the original form, contaminated with *canthus*), Sp. (Sanabria) *cantrelas*, Port. dial. *cantella, cantelras, cantelas* 'idem' (FEW), These latter forms can be compared to W. *cantel* and *cantell* 'a rim' (Pedersen 1909-1913: § 397), It. *canto* 'rim of the wheel', Northern-It. (Bormio) *kant* 'idem', Southeastern-It. *ianta* 'idem' (< Gallo-Rom.).

(5) The Old Irish hero-tales of the so called Ulster Cycle, which form the earliest stratum of Irish traditional literature, are centered on the epic *Táin Bó Cuailnge*, the 'Cattle-Raid of Cooley', which has been called 'the Celtic *Iliad*' (Harbison 1988: 166). In these hero-tales, of course, warfare is the dominant theme, and one of the most notable features in them is the war-chariot called *carpat*, from Celtic *carpanto*- c*arbanto*-, the word which also lies behind Latin *carpentum* (Piggott 1983 236-237).

In short, everything seems to point to both a Celtic primacy in cartwright technology in general, and to a specific Celtic origin of the new spoked wheel, characterised by bent-wood felloes, and to its diffusion, along with its name, into the Latin and Germanic area and beyond. This is why the spread eastwards of the *rota*-type name, reaching Persia and India, can then indeed be interpreted as an anticipation of the Celtic wave of conquests of proto-historic times, which extended to Anatolia. Note that the meaning of the Indo-Iranian word is not 'wheel' but '*chariot'*, namely a fast vehicle used by chieftains and their noble entourage of warriors. This has two implications, one technological and the other linguistic: on the one hand the 'chariot' as such necessarily implies a light construction, and thus horse traction and a spoked wheel (Piggott 1992, 56). On the other the change of the word meaning from the original 'wheel' to 'chariot' – while the Indo-Iranian wheel's name remained the earlier one – points to a loanword, rather than to an inner development (cf. the Baltic development, which is also that of a loanword).

THE PREHISTORIC IMPLICATIONS OF THE CELTIC ORIGIN OF THE LAT. *ROTA* FAMILY

Coming now to the implications of the Celtic primacy in spoked-wheel technology for the prehistory of the Celts and of Europe, these are several and far-reaching.

First of all, it is evident that the Celtic power of early Iron Age and protohistorical times must have rested on developments typical of Bronze Age such as horse-warfare and the cartwright technology associated with it. But the awesome problem that troubles both the traditional IT and Renfrew's NDT is, as I have already said, that in the Bronze Age or earlier the "arrival" of the Celts cannot be detected in any way in the archaeological record: within these two frameworks, the sudden appearance of the Celtic power in Europe remains thus inexplicable.

The Celtic origin of the *rota* family provides us with yet another argument to revise in an essential way traditional thinking about the formation of the Celtic power in Western Europe, as well as the traditional chronology for the differentiation and the spread of IE languages: in short, it provides us with a new confirmation of the PCT.

The fundamental question is: how did the Iron-Age Celtic primacy in spoked-wheel technology come about?

The archaeologist Stuart Piggott, in one of his major surveys of prehistoric wheel vehicles – *The Earliest Wheeled Transport From the Atlantic Coast to the Caspian See* (1983), frequently cited also by Weijnen –, devotes the 6th and last chapter of his book to Early Iron-Age spoked-wheel vehicles by him explicitly defined as "Celtic". Celtic mastery of spoked wheel technology is in fact unquestionable in La Tène (V-I cent. b.C.), as the superb wheels buried in the so called 'princely graves' abundantly testimony. However, although La Tène wheels and vehicles obviously presuppose a long period of technological growth, Piggott does not address the specific problems of the origin and formation of this Celtic primacy, the purpose of his book being more general.

Since spoked-wheel vehicles are abundantly attested also in the preceding culture of Hallstatt (end VIII cent. – VI) (cf. Piggott 1983, ch. 5), we could ask ourselves whether this culture could perhaps be considered as their ultimate origin. For the cultural continuity between Hallstatt and La Tène is unquestionable, and even traditional Celtic specialists now tend to see Hallstatt as a 'Celtic' phenomenon. But on second analysis the answer must be negative. For Hallstatt is synchronic with the cultural developments that in Italy lead to the birth (753 b.C.) and the blossoming of Rome. And since the word *rota* – and its derivation *rotundus* 'round', which must have been coined after a consistent period of existence of the spoked wheel – belong to the hardcore of Latin lexicon, it would be hard to believe that they were still unknown to the Latin-speaking founders of Rome. Unquestionably, the lead of the Celts in spoked-wheel technology must have preceded the foundation of Rome and Hallstatt.

What about Bronze Age Europe? In Early Bronze Age, European evidence for spoked-wheel vehicles is scanty, and concentrates in the East (Piggot 1983, ch. 3). This is why, among other things, a Western Asiatic origin for the invention of spoked-wheel technology, rather than a European one, is still considered as the most plausible hypothesis (Piggott 1983). But this does not create a problem for the Celtic interpretation of the *rota* type. For the solid-wheel technology was already widely diffused in Europe in the III millennium, and the earliest European solid wheel, recently discovered near Ljubljana, Slovenia, has been dated to the end of the IV millennium, in Chalcolithic. If, then, the much faster spoked-wheel vehicles, suitable to warfare, did appear first in the Near East at the beginning of the II millennium, the new, powerful Celtic elites emerging in Western Europe certainly would not have wasted time in adopting them for their own war plans and prestige, at the same time giving them a new name. Celtic technological lead could have thus been the result of a later development, just as, for example, the introduction of metallurgy into Western Europe in the III millennium was accomplished by the Bell Beaker culture, although metallurgy as such was introduced from Asia first into Eastern Europe (V millennium), and in Eastern Europe had its earliest and most important manifestation in the Balkan metallurgical complex.

In short, all of this simply means that in Western Europe there must have been some fertile ground for the new Eastern invention to take root, and to slowly develop into Hallstatt and La Tène. Could we possibly identify this focus area?

A clue to the answer emerges in Late Bronze Age Europe, when spoked wheels appear to be widely attested, as shown by Piggott in the 4th chapter of his book, and summarised in the map on p. 135: surveying the map and the data, the Bronze culture which might be a suitable predecessor of Halsstatt and a La Tène cartwright technology is the so called Rhône culture, diffused in the Swiss Valais and in the French Jura, Bourgogne and Midi regions. This culture has revealed remains of large cult, ritual or processional chariots, with spoked bronze-sheathed wheels characterised by an "exceptionally accomplished technology" (Piggott

1983: 124-125). And the archaeologist who has studied the origin and the formation of this culture, A. Gallay (1976), has concluded that its origin must be sough in the III-millennium Bell Beaker culture: the culture, incidentally, which in the second volume of my book on the origins of European languages (Alinei 2000) I have attributed to the Celts on the basis of independent arguments.

The question can then be so reworded: Could the Celtic La Tène spoked-wheel technology, via a Celtic Rhône culture, ultimately derive from a Celtic Bell Beaker culture?

Let us first recall the prerequisites, as well as the main functions, of spoked-wheels chariots, as have been lucidly illustrated by Piggott and other scholars. Piggott defines the innovation as "a complete technological revolution […] introduced by the development of the domesticated horse as a traction animal in association with a light, fast, vehicle with a pair of spoked wheels", and "in [the] form of a chariot for warfare, hunting, prestige and display" (*ibidem* 66).

Translated in etiological terms, this definition means that the new technology of the spoked wheel could be developed only where horse-riding, warfare (and thus metal weaponry), hunting, prestige and display were not only present and customary, but also had a central role in the community's way of life.

And the Bell Beaker (from now on BB) culture, which in the III millennium carried its aggressive ideology from the Atlantic coasts of Western Europe to new parts of Europe, is the only culture in which these components form its very essence. Already Gordon Childe had seen the BB as «the inevitable drinking-cup [that] symbolises beer as one source of their influence, as vodka flask or a gin bottle would disclose an instrument of European domination in Siberia and Africa respectively» (Childe 1957: 223). Sherratt, in his more recent synthesis, states: "the decorated handleless drinking-cups known as Bell-Beakers stand *pars pro toto* for a whole new way of life in the areas where they appeared, from Scotland to Sicily" (Sherratt 1994: 250). And the features of this new way of life, which in Sherratt's synthesis came to form their «martial image», was characterised, among other things, by horse-raising and riding, by warfare and hunting (daggers and archer's kit), by metallurgical skills (metal daggers), by individualism (individual burials), and by a "deliberately ostentatious personal life style" (extended burial, colourful garments, gold ornaments) (*ibidem* 250-255). Moreover, not only did the "BB Folk" practiced metallurgy, but in certain European areas it was they who introduced metallurgy (De Laet 1979, 356). And not only did they practice horse-raising and horse-riding, but in most areas where they spread (Ireland, Spain, France, Hungary and Holland), theirs are the first attestations of horse domestication (De Laet 1979: 358). They were also active and able traders, as shown by the rapid diffusion of the BB itself along the coasts, by the existence of BB trade posts along the main rivers, by the diffusion, within their area of influence, of innumerable copper, gold and amber objects (*idem*, 356). Finally, their elite character, which could be seen as anticipating the princely leadership of

La Tène, is also demonstrated by the high value that their copper or bronze dagger certainly had in the III millennium (Strahm 1994: 314).

There is then no question, in my opinion, that the BB culture of Western Europe could form an ideal context for the adoption and the independent development of the new wheel and vehicle technology, so closely associated to warfare, domination, wealth, prestige and display.

Compared to their slightly older cousins of the (stone!) Battle Axe and Corded Ware (drinking vessels as well!) of III-millennium Eastern Europe, the BB people of Western Europe had, besides the features of pastoralism, warfare, male ideology, individualism and elitism that they shared with their cousins, also some additional elements, such as technology, enterprise, trade, and the 'finesse' underlined by Sherratt (1994: 254), which were destined to change Europe in a permanent way and to bring it to its protohistorical and historical stage.

In my major work (Alinei 1996a, 2000) I have illustrated the arguments by which the BB people ought to be seen as the second manifestation, after megalithism, of a Celtic thrust to central Europe. Here, I confine myself to underline that also if one concentrates on one of the basic factors of Celtic power in protohistory, namely spoked-wheel and cartwright technology, it is again the BB that comes out as its only possible antecedent. The Celtic presence in Western Europe, in the form of the BB culture, and thus already in the III millennium, is the only satisfactory explanation of the formation and the growth of a Celtic primacy in Iron-Age cartwright and spoked-wheel technology. And this scenario is possible only within the framework of the PCT.

Only by assuming that it was the Atlantic Celts of prehistoric times who introduced important technological innovations such as metallurgy, horse domestication and horse-riding, a new type of wheel and of vehicle, as well as trade and exploitation of natural sources into Western and Central Europe, it becomes possible to understand why the Celts of Hallstatt and La Tène appear as the first «colonial power» of Europe (cf. e.g., Powell 1980: 42, 44-5, 48-49).

In the traditional view, the Celts are the first colonial power of Iron Age Europe, but no explanation is given for such a formidable development. Within the framework of the PCT, the Celts – already present in Western Europe since Palaeolithic times – become the leading force of Western Europe beginning with the expansion of Megalithism and that of the BB élite. The adoption – in the Early Bronze Age – of the spoke wheel and the development of new vehicles suitable for warfare, ritual and display belong to this coherent line of development and forms, in my view, one of the main stages of the formation of Celtic Iron Age colonialism.

In conclusion, while the IT obliges its supporters to look for a PIE word for 'wheel', opening staggering problems of various kind and leaving unexplained the other names, the PCT sees the three IE names of the wheel simply as reflections of three slightly different periods, and consequently of three different focal areas, in the development of wheel technology, when IE languages had long been differentiated. It does, however, stimulate future research on the possible correspondences between the three areas of *kolo*, *rota* and *trokhós*, and the available archaeological data.

WHICH IS THE BEST DATE FOR THE DEVELOPMENT FROM *PROTO-SLAVIC **LĘDO* 'FALLOW LAND' TO PROTO-GERMANIC **LANDA-* 'LAND'?

An interesting and never properly studied, semantic develoment is the derivation of Engl. *land*, and its other Germanic cognates (Goth. Du. Icel. Fer. Norw. Dan. etc. *land*) (Vasmer s.v., cf. Stang 1971, 33, ANEW s.v. *land*) from *Proto-Slavic **lędo* 'fallow land' (Ru. *ljadá* 'mit jungem Holz bewachsenes Feld, Neubruch, Rodeland', Ukr. *l'ado*, BRu. *lado* 'Neuland', ORu. *ljadina* ecc., Bulg. *léda ledinà* 'Aue, Bergwiese', Serbo/Cr. *lèdina*, *ledìna* 'Neuland', Slovn. *ledìna*, Cz. *lada*, *lado* 'Brache', Slovk. *lado*, Pol. *ląd* 'Land', USorb. *lado* 'Brache', LSorb. *lědo*). Among Germanic languages only Swed. *linda* 'Brachfeld' has retained the original meaning. To make this development more interesting, it must be remember that the Slavic names for 'fallow land' are also the basis of the Slavic name for 'Poland' and 'Polish' – Ru. *ljax* 'Polish', Serb./Cr. (obs.) *Leđanin* 'Polish' (> Hung. *Lengyel* 'Polish') –, Byz. Gr. *Lenzanenoi* (pl.), Crim.Tat *läh* 'Polish' (Vasmer s.v. *ljach*, cfr. EWU s.v. *lengyel*), as well as in Arab *laudzaaneh* 'polacco'. Thus the Slavic name of Poland and Pole originally meant, respectively, 'fallow land' and 'opener of new fields'.

As etymologists recognise, the specialised, farming meaning 'fallow land' of Slavic languages must have preceded the more general meaning of 'land' typical of the Germanic area, just as it preceded the development of 'Polish'.

If we now apply the three models to this complex development, it is evident that only the PCT and (much less) the NDT can provide an adequate explanation, while the IT completely fails.

(1) Beginning with the IT, it is hard to see how the Germanic people would have had to wait until the arrival en masse of the Slavs in the Middle Ages to learn the technique of fallow land and to develop the notion of 'land' from it! Slavs, incidentally, whose demographic explosion, arrival and occupation of half of Europe must have taken place in a fourth dimension, in order to escape archaeological observation!

Only the PCT and (with serious difficulties on which I will not dwell) the NDT, offer an adequate scenario: (1) first of all, it is now ascertained that the earliest European Neolithic is the one that develops, in the VII millennium b.C., in the Balkan. In both the PCT and NDT, thus, the first IE-speaking farmers are Greeks, Slavs and Illyrians. (2) In both models the LBK Neolithic culture of the V millennium (the first farming culture of Central Europe) is a Germanic-speaking culture. It is thus obvious that the Slavic Neolithic precedes the Germanic one. (3) In the general evolution of farming, the technique of 'fallow land', with the consequent rotation of cultures, reflects a mature stage of Neolithic.

Now it so happens that in Europe the first attestations of the rotation of cultures based on the fallow-land technique appear exactly in the area of the LBK and *Lengyel* (!) cultures, i.e., in the area that spans from Hungary (obviously Slavic, before the arrival of Hungarians), through former Czechoslovakia and Southern Poland, to Germany. Not only: archaeologists, underlining the extraordinary stability of the LBK culture (which, I repeat, is the first farming culture of the Germanic area), note the importance of the new fallow-land technique for its new settlements: if in Germany and contiguous areas the LBK, in spite of its stability, did not cause the formation of *tells* (the artificial hills resulting from the accumulation of debris of prehistoric villages, built on top of each other: one the most important proof of millenary continuity), typical of the whole Balkan area of the earliest European Neolithic, this is precisely due to the fundamental role of the rotation of cultures (Tringham 1971: 115).

What does all this imply for our problem? Quite clearly that the Germanic groups of the LBK, who had learned the **lędo* fallow land and rotation technique from the Slavs, adopted it systematically to "open new fields" and expand their territory, and thus came to identify the "fallow land" with "land" itself. Confirming, at the same time, the contiguity of Western Slavs and Germanic people in the V millennium b.C. in exactly the same area where they now border.

Schematically, the chronology of the development can be shown in the following table:

	GERMANIC	**SLAVIC**
MIDDLE NEOLITHIC 1		Protoslav. **lędo* 'Rodung, Neuland' Russ. *ljadá* 'mit jungem Holz bewachsenes Feld, Neubruch, Rodeland', Ukr. *l'ado*, BRuss. *lado* 'Neuland', Serbo/Cr. *lèdina*, *ledìna* 'Neuland', Slovn. *ledìna*, Czech *lada*, *lado* 'Brache', Slovk *lado*, USorb. *lado* 'Brache' etc.
MIDDLE NEOLITHIC 2		Russ. *ljax* 'Polish', Serbo/Cr. *Leđanin* 'Polish' etc. (> Hung. *Lengyel* 'Polish'),
MIDDLE NEOLITHIC 3	Germ., Engl., Du., Icel., Norw. Swed. etc. *land*	

WHICH IS THE BEST DATE FOR THE DEVELOPMENT FROM 'ENCLOSURE' TO 'VILLAGE' AND FROM 'VILLAGE' TO 'FORTIFICATION': *ZAUN TOWN DUNUM; HORTUS GARTEN GRAD*?

Another very interesting and significant semantic development is the four-stage sequence that occurs from 'enclosure', through 'garden' and 'town', to 'fortification'. To make it even more interesting, this development occurs twice in Europe, in two partially overlapping areas, and with two different lexical families: we shall call them, conventionally, the *town*-sequence and the *grad*-sequence. Here they are:

The *town*-sequence:
(A1) 'enclosure'
Germ. *Zaun* 'enclosure, hedge', dial. Engl *tine* 'enclose, enclosure', OIcel. *tún* 'enclosure', dial. Swed. ODan. *tun* 'hedge' etc.;
(A2) 'garden, orch-yard'
Du. *tuin* 'garden, orch-yard', OEngl. *tun* 'garden';
(A3) 'town'
Engl. *town*, OIcel. *tún* 'town';
(A4) 'fortification'
Gallic *dunum*, OIr. *dun* 'fortress'.

The *grad*-sequence:
(B1) 'enclosure, hedge'
Gall. *gorto*, *gortiā* , W. *garth*, Bret. *garz* 'hedge, enclosure', Alb. *garth* 'enclosure'; Gr. χόρτος 'inclosed place', Lith. *gardas* 'enclosure'; OSlav. *graždъ*, Russ. *goróža*, Ukr. BRuss. *horóža*, Bulg. *graž*, SCr. *građa*, Slovn. (acc.) *grája*, all 'enclosure', Cz. *hráze* 'garden-wall'; Slovk. *hrádza* 'idem', Pol. *grodza* 'hedge';
(B2) 'garden, orch-yard'
Lat. *hortus* 'orchard, vegetable garden'; Germ. *Garten* 'garten', Goth. -*gards*, OIcel. *garðr* etc.; OSlav. *gradŭ*, *ogradŭ*, OSlav. Bulg. *gradina* (> Rum. *grădină*), Pol. *ogród*, Cz. *zahrada*, Russ. *ogorod*, all 'garden';
(B3) 'town'
OSlav. *gradŭ*, SCr. Slovn. *grad*, Czech *hrad*, Pol. *gród*, Sorb. *grod*, Russ. *górod*, Ukr. *hórod*, all 'town'.
(B4) 'fortification, castle'
OSlav. *grad*= (> Russ. *grad*), Czech *hrad*, Pol. *gród*; SCr. *gràdina*, Slovn. *gradìna*, Bulg. *gradište*, Czech *hradište*, Russ. *gorodišče*, all 'castle, fortification'.

Notice:

(1) within the area of the *town*-sequence, there is no linguistic group that shows all the four stages: the Germanic area shows only the first three stages, while the fourth is attested only in the Celtic area;

(2) within the much wider area of the *grad*-sequence (not fully represented in the above list), only *the Slavic group shows all four stages*. All other groups (Celtic, Latin, Albanian, Greek, Germanic) only show one stage, and not the same;

(3) in both cases, the compact character of the areal distribution implies a direct, territorial continuity. In other words, the semantic develoment took place locally and spread to contiguous areas;

(4) the first part of the semantic sequence – from 'enclosure' to 'garden' and to 'village' – could be explained adequately only if placed against a Neolithic scenario; the subsequent passage of 'village' to 'fortified settlement' would then be a well-known, typical development of the Metal Ages.

Let us now apply the three models to both sequences.

In the framework of the traditional scenario of the IT, the whole semantic sequence does not make any sense, as no cultural development of this kind can be witnessed after the Copper Age. According to Pokorny (IEW, 263), Gallic *dunum* is a cognate of Engl. *down* ('dune'), while Engl. *town*, Germ. *Zaun* etc, are loanwords from Celtic! So the resulting sequence of semantic developments (left undiscussed!), is quite bizarre as it would start from 'dune'! According to Kluge-Mitzka (1975), Gallic *dunum* is 'urverwandt' with *Zaun* and its Germanic family; but no attention is given to the developmental sequence and, following Krahe, the sequence is dated to the early Middle Ages! (Apparently overseeing the implications of the fact that Gallic place-names in -*dunum* 'fortress' are pre- or protohistoric). Kluge-Seebold (1989), at least, correctly see the development of 'town' from *Zaun*, and have eliminated the Medieval dating. According to Buck (1949, 19.15), «many of the words [for 'town'] denoted first an 'inclosed and fortified place», missing the elementary distinction between the general evolutionary development from Neolithic open villages to fortified villages of the Metal Ages, and the *reverse sequence from 'castle' to 'village'*, which is typical only of the Middle Ages.

Not better is the traditional treatment of the *grad*-sequence: Kluge-Seebold (1989) do not even mention the Slavic cognates, probably accepting the (biased) view that they are loanwords from Germanic (!), and limit themselves to note the Germanic, Greek, Latin and Celtic cluster. Buck (*ibidem*) does not pronounce himself, and Pokorny groups all the relevant words but makes no attempt at putting them into a sequence. Obviously, in both cases the failure is due to the wrong chronology of the IT.

Also in Renfrew's NDT the sequence cannot be explained in any precise and satisfactory way, since in his model the formation of the Celtic group in Western Europe is entirely left to the imagination and remains just as problematic and contradictory as in the traditional model.

Only in the PCT is there is a perfect coincidence between the linguistic and the archaeological data. Let us see it in detail:

First of all, recall that only in the Slavic area the semantic sequence shows all its four stages: ‘enclosure’ > ‘garden’ > ‘town’ > ‘fortification’. This, of course, can only be explained as a consequence of the extraordinary stability and continuity of Neolithic cultures in South-Eastern Europe, the only European area characterised first by *tells* formation, and later precisely by the well-known fortified villages and “Castellieri”. These early Neolithic cultures of South-Eastern Europe would be, of course, Slavic. While the appearance of only the initial stages of the sequence in Latin *hortus* and Germanic *garten/garden* would corresponds with more turbulent developments from Neolithic to the Metal Ages in both areas. The Celtic sequence also, which “skips” the ‘village’ stage, showing only ‘enclosure’ in the *grad*-sequence, and only ‘fortification’ in the *town*-sequence, corresponds perfectly well with the archaeological record: in the Celtic area, particularly in Ireland and England, Neolithic wooden houses (IV millennium b.C.), although very similar to those of the LBK (Harbison 1988: 28-29) (from the area of which they came), did not form villages, as in Germany, but they remained isolated (Harbison 1988: 31). Which is, still today, a typical characteristic of Irish farming “villages”: actually only isolated farms, as noted Wagner (1958- 69, XI). As to the enormous importance of fortifications in the Celtic area, suffice to recall the almost four thousands *hill-forts* (ancestors of the Gallic *oppida* described by Caesar), built in England after 1000 b.C. (Dyer 1990: 124).

Schematically, if we distinguish the *grad*-sequence from the *town*-sequence with a gray background, and use a relative chronological scale, we obtain the following table (see next page):

The absolute chronology of the Greco-Slavic Neolithic area and that of the Italic one would be, of course, earlier than the Germanic, and the Celtic one would be the latest, in harmony with the archaeological data.

WHICH IS THE BEST DATE FOR THE DEVELOPMENT FROM GR. *KAKKÁBĒ* ‘TRIPOD’ AND LATIN *CACCABUS* ‘POT’ TO GERM. *KACHEL* AND DU. *KACHEL* ‘STOVE’?

While attempting to reconstruct and to date this interesting semantic development within the framework of the IT would be a useless exercise, both the NDT and the PCT would provide, on the contrary, a precise and well documented reconstruction. Let us see why.

The Greek word, which means ‘tripod’, is probably of Eastern origin (DELG). Unfortunately, I do not have any information on the possible issues of the word in modern Greek dialects. But in the area which in my work I call *Italid* or *Italoid*, or *Ibero-Dalmatian*, to distinguish it from *Italic* (Osco-Umbrian) historical languages, – that is Southern Italy, Sardinia, Corsica, France, Iberia – where the Greek word has been introduced as a loanword – the dialect developments out of the Latin *caccabus*, *caccabulus*, **caccabella,* etc. are very numerous and quite interesting.

	BALTO-SLAVIC	GREEK, ITALIC	GERM.	GERMANIC	CELTIC-
LOCAL EARLY NEOL.	Lith. *gardas* 'enclosure' OSl. *graždŭ* Bulg. *graž* Cr., Serb. *građa* Slovn. (acc.) *grája* Ru. *goróža* Ukr., Bru. *horóža* 'enclosure' Cz. *hráze* 'garden-wall' Slovk. *hrádza* 'idem' Pol. *grodza* 'hedge' OSlav. *gradŭ*, *ogradŭ* OSlav., Bulg. etc. *gradina* Pol. *ogród* Cz. *zahrada* Ru. *ogorod* all 'garden'	Grk. *chórtos* 'hedge, court' Lat. *hortus* 'orchard'	Germ. *garten* Engl. *garden* AIcel. *garđr* 'hedge' 'garden'	Germ. *zaun* 'enclosure, hedge' dial. Engl *tine* 'enclose, enclosure' OIcel. *tún* 'enclosure' dial. Swed. *tun* 'idem' etc. Du. *tuin* 'garden, orchard', OEngl. *tun* 'garden'	Gall. *gorto*, *gortiā* W. *garth* Bret. *garz* 'enclosure'
LOCAL MIDDLE NEOL.	OSlav. *gradŭ*, Cr., Serb., Slovn. *grad* Czech *hrad* Pol. *gród* Sorb. *grod* Ru. *górod* Ukr. *hórod* all 'town'			Engl. *town* OIcel. *tún* 'town'	
LOCAL CHALC.	OSlav. *gradŭ* Ru. *grad* Czech *hrad* Pol. *gród* Cr., Serb. *gràdina* Slovn. *gradìna* Bulg. *gradište* Czech *hradište* Ru. *gorodišče* all 'castle fortification'				Gallic *dunum* 'fort'

The most striking aspect of this developments is perhaps the coincidence between the original meaning of 'three-foot pot' of the Greek word, and the typological feature – quite frequent in Southern Italy and Southern Corsica – of the pot or cauldron bearing this name, which is 'with three feet' (AIS 1210, 1211, 955, 957, ALEIC 1609). In Southern Corsica, for example, the cauldron with this name is always defined ‘in bronze with three feet’. In other words, we have not only the continuity of the word, but also of the thing.

As to the Germanic area, where we have OHG. *kachala* ‘earthen pot’, Du. *kachel* ‘stove’, Germ. *kachel* (> Cz. *kachel*), Dan., Norw. *kakkel*, Swed. *kakel* ‘tile’ (> Lith. *kakalys*), and where the word, according to the FEW, «a pénétré dans les dialectes allemands de tout le front de la Mer du Nord jusqu’en Carinthie”», it becomes clear that the semantic development has been:

‘pot, cauldron’ > ‘stove of tile’ > ‘tile’

This is clearly confirmed by Dan. *kakkelovn*, Sw. *kakelugn,* etc. ‘stove’, but originally ‘stove of tile’. From the phonetic – and chronological – point of view, it is interesting to note that the introduction of the modified Latin **caccalus*, must have preceded the *Lautverschiebung*, in order to issue *kachel*. The loanword must have thus taken place very early.

Now it so happens that the tripod or three-foot pot, originally only for cooking and then also for decoration, has a well studied prehistory and an areal distribution that seem to coincide very closely with the linguistic picture. For, summarising Lilliu (1988, 91 ff.), the tripod appears in Europe first in Late Neolithic and Chalcolithic, coming from the Middle East, probably Syria. It expands first in Greece (Larissa and Thessaly, after 2600 b.C.), then in Italy – above all in Sardinia and Corsica – and finally in France and Central Europe, in particular in the Saxo-Thuringian and Bohemo-Moravian areas.

An interesting reading of the word history, which is only possible within the framework of the PCT or NDT.

WHICH IS THE BEST DATE FOR THE BALKAN *SPRACHBUND*?

Emanuele Banfi (1985) has masterly illustrated the history and the problems of the so-called Balkan *Sprachbund*, i.e., the complex of different languages (Rumanian, Bulgarian, Macedonian, Albanian, often Greek and at times Hungarian and Southern Italian dialects) belonging to four, at times six, different linguistic groups, which despite their radical difference share many linguistic features. The discovery of this anomalous linguistic complex goes back to such illustrious linguists as A. Schleicher, Fr. Miklosich, H. Schuchardt, H. Pedersen, P. Skok and others, but the merit of the scientific notion of *Sprachbund*, in the sense of complex of isoglosses shared by languages that are geographically contiguous but genetically unrelated, goes to the founders of structural linguistics, N.S. Trubeckoy and R. Jakobson. Not all isoglosses are present in the languages of the *Sprachbund*, and some of them extend to Southern Italy; other ones, especially lexical, cover the Carpatian Basin and reach as far as Ukraine (Banfi 1985, 113). The main linguistic 'balkanisms', as is known, are the following: • in *phonetics* and *phonology* (i) the presence of a *schwa* vowel (which extends to Southern Italy); • in *morphology*: (ii) coincidence of genitive and dative, (iii) analytic future with 'will', (iv) analytic comparison, (v) numbers from 11 to 19 with 'on' and 'ten' (extended to Hungarian), (vi) preservation of vocative; • in *syntax*: (vii) loss of infinitive, (viii) postposition of article and (ix) reduplication of the object; • in *lexicon* (x) circulation of scores of Greek, Latin, Slavic, Turkish and Albanian loanwords; • in *iconomastics* (motivation) (xi) scores of idioms.

The difficulties of explaining the existence of this *Sprachbund* in the light of the IT are enormous, especially given the presence of such features as the postposed article. From where could such a foreign feature come? More generally, within the extremely low chronology of the IT there is no historically documented scenario that could provide an adequate explanation for the dense network of exchanged features within the *Sprachbund*, involving Slavic and Turkic as well as Latin and Greek.

Both the NDT and the PCT, on the contrary, with their higher chronology offer a perfect scenario: the arrival from the Middle East, at the beginning of Neolithic, of the first wave of farmers, who introduce agro-pastoralism into the Balkan, and from there into Europe.

There are, however, two radical differences between the two models: (1) the NDT sees the immigrant earliest farmers as the still undivided Proto-IE, and the European autochthonous populations as Pre-IE. The PCT reverses this view, and sees the immigrant earliest farmers as intrusive Middle Easterners, and the European autochthonous populations as IE, Uralic, Altaic, Basque, Caucasian etc. (2) Consequently, the NDT is forced to see the IE differentiation process taking place in an unbelievably short time, as it identifies all major Early and Middle Neolithic cultures of Europe, depending on their areas, as already Greek, Slavic, Italic or Germanic, leaving only Celtic without a home.... Nevertheless, it is difficult to see how the Balkan *Sprachbund* could be formed immediately after the arrival of the Proto-IE, when the different IE groups supposedly do not exist yet!

The PCT, on the contrary, can see the IE differentiation process taking place very slowly, beginning from Upper Paleolithic. Therefore, by the time the Middle Easterner intrusive farmers arrive into the Balkan, the different IE languages of that area are already there.

Let us now discuss the postposed article. In the Balkan area, it appears in Bulgarian, Macedonian, Rumanian and Albanian. Recall that the postposed article in Europe exists in three different areas: besides the Balkan, in Scandinavia and in the Basque area. In the last one it is certainly an independent feature. In Scandinavia, where it involves only the IE Scandinavian languages, it is probably a local innovation. Only in the Balkan the feature is shared by languages belonging to different language groups: Slavic, Illyrian and Italic. A Slavic origin of the phenomenon can be excluded, as one of the main characteristics of Slavic languages is precisely the *lack* of article. Neolatin languages do have the article, but always preposed, and therefore Rumanian can hardly be the source of the phenomenon. Illyrian, ancestor of Albanian, has been the language of a powerful élite that in a certain period has dominated the Balkan, so, hypothetically, it could have had this role. But a part from the fact that it is very scarcely documented, and in any case does not show a postposed article, its lexical contribution to the Balkan *Sprachbund* is too modest (Banfi 1985, 106 ff.) to be combined with the powerful role that we must assume to account for the introduction of the postposed article in the whole area. The only solution is then to attribute the introduction of this feature to the intrusive Middle-Eastern farmers who brought the Neolithic economy in the area.

Now, in the light of the PCT, these Middle Eastern farmers would probably be Semitic, and would thus have spoken *one of the well-known Semitic languages which show the postposed article*.

This assumption is enhanced by another argument: one of the characteristics of the Balkan *Sprachbund* is what has been called "the *originary* Balkan lexicon" (Banfi 1985, 83-85), namely that part of the lexicon common to the Balkan languages of which specialists have not been able to trace the origin. In the PCT, obviously, this lexicon would not be "originally Balkan" but, on the contrary, Middle Eastern, and thus *peri*-IE, probably Semitic.

If we now turn to the earlier lexicon shared by the Balkan *Sprachbund*, we can first ask ourselves if such Greek terms as *keramída* 'tile', *potḗri* 'glass', and *paráthyron* 'window', diffused as they area in the whole Balkan area (Alb. *qeramidhe*, Bulg. *keramida garamida*, Serb. *ćeramida*, Rum. *caramidă*, besides Turk. *kiramit*, perhaps connected with its origin; Alb. *potir*, Bulg. *potir*, Serb. *putir*, Rum. *potir*; Bulg. *paràtir*, Alb. *parathìr*), could not be, rather than Medieval loanwords introduced from Greece, for mysterious reasons, in the area, a consequence of the fact that Greece was the earliest Neolithicised area of Europe. Recall that both ceramics and modern housing typology – which includes 'window' – begin with Neolithic.

Another example can be one of the names of the 'billy-goat' (a question that will be the object of a map and commentary in one of the next ALE volumes): Ru., Ukr., Czech, dial. Slovk., Pol. *cap*, Slovn. *càp*, Hung *cáp*, Rum. *ţap*, Alb. *cap cjap*, NGr. *tsápos*. Its already wide area expands further into Adriatic dialectal Italy: Trieste *zap*, Marche *ciappo*, Abruzzi *zappə sappə*, Sabine *sappo*, Velletri *zappo*. Irrespective of what its ultimate origin might be, we could ask ourselves, in the light of what we have already seen, if also this word could not be attributed to the early Neolithic unity of the Balkan area, ensuing from the introduction here of the first wave of Middle-Eastern farmers, with the first samples of domestic animals and plants.

To conclude, only the PCT allows us to place the beginning of the Balkan *Sprachbund* in the scenario of the introduction of Neolithic economy from Middle East into Europe. This role of "springboard" for new ideas, new techniques and new *realia*, often with their original words, must have continued also during the Chalcolithic, when the Balkan and the Carpathian Basin became, again, the cradle of European metallurgy. In the Bronze and Iron Ages, as well as in the Middle Ages, different dominating influences from the various Balkan and Carpathian linguistic groups would have continued to shape the already rich cultural picture of the area.

WAS LATIN SPOKEN IN THE MIDDLE OF THE PO VALLEY IN ITALY MUCH EARLIER THAN THE FOUNDATION OF ROME? THREE EXAMPLES

The most striking differences between the two new models of the PCT and the NDT on the one hand, and the old IT on the other, occur as far as the Romance area is concerned.

First of all, in the two new models the very notion of "Romance" acquires a profoundly different meaning from the traditional one: for in both models, following the now ascertained assumption of uninterrupted continuity from Neolithic and earlier cultures to the Metal Ages (the only Ages in which important ethnic elites migratory movements can be archaeologically detected), it becomes obligatory to identify the early Neolithic Cardial Culture of Dalmatia, coastal Italy, Southern France and coastal Iberia with the "Italid" group. The presence of neo-Latin dialects in this wide area, then, must be explained as primarily due to the continuity of languages akin to Latin spoken in the area from, respectively, the beginning of Neolithic in the NDT, and from Upper Paleolithic in the PCT, and only secondarily to Roman occupation. The Po Valley seems to provide good examples of this new vision.

1. The Italian names of the 'ploughshare'

Recall, first, that the plough is an invention of the Late Neolithic and Calcholithic (Forni 1990), typical of what archaeologists call "the secondary product Neolithic revolution", and that the prehistoric plough was entirely made of wood: a tree trunk, out of which came two branches, the longer being attached to the animals, and the shorter being the "ploughshare", sharpened in order to be able to scratch the earth. On the tree trunk itself another piece of wood was stuck, by means of which the farmer could control the angle of the short branch that functioned as 'ploughshare'. In order to have the metal ploughshare we must wait the Iron-Age, as no bronze ploughshares have been found in the European record, except ritual ones. And numerous iron ploughshares have been found in the Po Valley, all dated to the Iron Age and to the Roman times.

Now, in the Po Valley, south of the Po – an extremely flat and agriculturally rich area –, the small river Panaro in Emilia forms a well-known dialect boundary that separates, among many other linguistic features and lexical items, two different names of the 'ploughshare': East of the Panaro we have developments of the Latin name of the 'ploughshare' *vomer vomeris*; West of the boundary the name of the ploughshare continues another Latin word, **matea* (which in Latin is attested only the diminutive form *mateola*, with the meaning of 'mallet'), the Italian issue of which is *mazza* 'wooden club' (from which, for example, It. *ammazzare* 'to kill'), the French one *masse* and the English one (through French) *mace*. The motivation of this name of the 'ploughshare' was thus {wooden club}. Obviously, if in the Western part of the Po Valley the ploughshare has been called 'wooden club' it is because it must have been one!

Notice also that this surprising dialect boundary, occurring in the middle of the vast Po valley, coincides with two cultural boundaries: a prehistoric and a late medieval one. The prehistoric boundary is the one that in the Bronze Age divided the so called *Terremare* (from *Terre marne* 'calcareous lands') culture in the West from the so called *Apennine* culture in the East (from which later, in the Iron Age, developed the so-called Villanovan culture, the cradle of the Etruscan civilisation and of Rome). The late medieval boundary is the one that divided, along the Panaro, the Church State from the other states of that period. In the history of Romance traditional research, only one serious attempt has been made, by Robert Hall Jr. (1943), to explain the dialect differentiation occurring in this area on the basis of the late medieval Church State boundary. So, applying this explanation to our geolinguistic opposition between the *vomere* area and the *mazza* area can only be one: the area where the Latin word *vomer* for 'ploughshare' survives is the conservative one, while the other area is innovative, and shows a development which must be medieval.

The problem of this explanation, however, is that it clashes against the motivational evidence, combined with the archaeological record: Why would medieval peasants of the Western part of this extremely rich farming region replace the Latin name of the ploughshare with a new name meaning 'wooden club', when it is absolutely sure that in the Middle Ages they already used iron ploughshares of the best kind? Within the PCT and (with the usual difficulties) the NDT, on the contrary, which permit us to assume the presence of Latin in the area already in Neolithic times, a reading "in real time", i.e., following the prehistoric evolution of the plough, becomes possible, with a resulting reversal of the relationship between the two areas: since the primitive ploughshare was entirely in wood, and the first metal ploughshares were only produced in the Iron Age, {wooden club} will motivate the original name of the primitive ploughshare, and thus its area will be the conservative one, while the classic Latin word *vomer* will be an innovation of the Iron Age, arising from the highly innovative Villanovan culture, connected with the foundation of Rome.

If this analysis is correct (and I do not see any valid alternative to it), we have then uncovered a "pre-Roman" stage of the history of Latin, discovering that Latin was spoken in the Po Valley in Neolithic, and identifying the area where at least some "classic" Latin terms were formed. The following examples seem to confirm this conclusion.

2. **The Italian names of 'manure'**

As the plough, also the discovery of manure, that is to say of the fertilising properties of cattle excrements, does not coincide with the beginning of Neolithic, for the renewal of fields, in the early Neolithic, was obtained with other methods, mostly the so called "slash and burn" technique. In Latin, there are three different words for 'manure': *laetamen*, whence Italian *letame*, *fimus*, whence French *fumier*, and finally *stercus*, which meant 'excrements'. The first word, *laetamen*, is possibly the most interesting, for it comes from *laetus*, an adjective which before acquiring the meaning of 'happy' (as in Italian *lieto*), actually meant 'fat, fertile'. *Laetamen* meant then 'what makes the earth fertile'. Since *laetus* in the sense of 'fat, fertile' precedes the meaning of 'happy', it must be archaic, and reflect the moment of the discovery of manure, and its great importance for Neolithic farming societies. Now, in Emilia, the granary of Italy, the manure has two completely different names (AIS 1177), and – interestingly – exactly with the same distribution that we have already noticed for the ploughshare: East of the Panaro it continues the classic Latin name, in the typical Emilian dialect variant *aldàm* (< *laetamen*); West of the Panaro it comes from another Latin name, namely *rudus -eris*, which, however, in Latin does not designate manure, but 'stone ruins, remnants of buildings, debris', and – most important – the so called '*marl'*, i.e., the extremely fertile land rich in limestone, which is typical of the Western part of the Po Valley. It is not by accident that the so called *Terremare*, earlier *Terre Marne*, owe their name to *marl*, from Celtic *margila*. It is also useful to note that *marna* meaning 'marl' is already attested in Columella, the Latin writer specialised in agriculture.

Let us now come to our usual comparison of the three models.

In the traditional IT interpretation, one is forced, as usual, to see *laetamen* as the primitive phase, and *rudus* as an innovation. But this reading does not satisfy us any more than the previous one. Why would Emilian farmers have to wait until the Middle Ages to discover that their soil was calcareous, and that therefore not only did it not need any manure, but could be used as manure itself? Why wait until the Middle Ages to re-introduce a technical term already known to Columella in the same sense? It would be much more logical to assume that the word *rudus*, designating 'calcareous soil', developed originally precisely in the Northern Italian limestone area, and that in this area the word for 'marl' would have acquired the meaning of 'manure'. This development would have taken place in Chalcolithic time, whereas the later discovery of the fertilising properties of cattle excrements, expressed by the term *laetamen*, would have been made, again, within the context of the Villanovan culture, which is known as the culture which mediated between the farming cultures of the Po Valley and the pastoral culture of the Apennine. It is in fact this kind of "mixed farming", as it is now called, which modern archaeologists see as the basis for the economic success of all European stratified societies of Bronze and Iron Ages, and in particular the Etrusco-Roman (e.g., Champion et. al. (1984, chapter 6), Puglisi (1959), Torelli (1984, 1987)).

3. The Italian names of the 'hub of the wheel'

A similar picture emerges from the study of the Italian names of the 'hub of the wheel'. As we have seen, the wheel and the carriage are an invention of the Late Neolithic and Chalcolithic, attested in Central Europe and Asia starting from the IV millennium a.C., «between Rhine and Tigris» (Piggot 1983), while in Italy (precisely in the Po Valley) it appears from the III millennium a.C. In the Emilian *Terremare* the earliest example is dated to the XIII a.C. (Forni (1990).

In the area east of the Panaro, again, the 'hub' is called with a name which continues its Latin name, namely *modius*; West of the Panaro, again, the hub's name is a development of another Latin word, namely *caput* 'head'. This distribution is thus the same as in the other two cases: in the Eastern area the original Latin name is preserved, while in the Western area we have another Latin name, but with a different original meaning.

And again, if we accept the IT traditional framework, we can only see the area where the original Latin is preserved as the conservative one, while the other will be innovative, and the innovation will be medieval. But again, why would medieval cartwrights or peasants need to change the name of the hub, precisely in the middle of the Po Valley, where carriages are attested since the Bronze Age, and no technological differentiations appear in Middle Ages? And why then change it with a such a basic metaphor as {head}? Moreover, to make the hypothesis even less likely, one discovers that the hub is called {head} not only in our area, but also in the Northern Po Valley, in German dialects, in Serbo/Croatian, in Bulgarian, and in Greek, that is in four distinct language groups, forming together one large compact area (Alinei 1974). Is it really possible to explain this vast iconomastic isogloss in medieval terms, for an object like the carriage, the antiquity of which – incidentally – increases as we proceed eastward? To such questions, traditional historical linguistics and dialectology have no answers, simply because they have no proper tools to do it. And IE research usually ignores dialect evidence.

In the framework of the two new models, however, which assumes the presence of Latin and Italic languages already in prehistoric Italy (and that of other IE languages in their corresponding territories), the {head} iconomastic area would reflect the Terremare culture, connected with, and probably deriving from, Central Europe, while the *modius* area would reflect the Apennine and Villanovan cultures, directly connected with the Etruscan civilisation and Etruscan technological innovations connected with the carriage (Forni 1990: 257 ff.), the foundation of Rome and the development of the Roman civilisation. As usual, the relationship between the two areas would then be reversed: the Terremare area would be the conservative, earlier one, the Villanovan area the later, innovative. As in the two previous examples, our dialect map would reflect a 'pre-Roman', yet Latin stage, which would help us reconstruct the formation process of some words belonging to the classic Latin vocabulary.

I will stop here, though I could continue with innumerable examples of the same kind, for – as I have said – the new chronologies of IE genesis and differentiation impose a re-reading of many of our reconstructions, both general and for the single IE language groups. But I hope my choice has been representative enough.

Finally, before closing this article, I would like to pay an homage to the ALE people who are not with us anymore, and with whom I had regular and close contacts during the innumerable ALE meetings of my 27 years of vice-presidency and presidency.

Among the ALE editors with whom I became more closely acquainted, Mieczysław Szymczak, the Polish representative, was the first to leave us, in 1985. He was an enthusiastic member of our Board, and a very amiable person. I often laughed and exchanged jokes with him, and I can't help remembering the only one he did not like: I showed him a cartoon which represented Pope Woytila as a missile directed against the Eastern Blok, and he, making a gesture of rejection, said: "We are catholic!".

Boris Cazacu, the Rumanian representative, died in 1987. He was an excellent scholar and a very agreable man, and given our common background we soon became friends. I remember my embarrassment, in Holland, when – following the Mediterranean custom – he would take me arm in arm, and I would fear that that kind of "intimacy" would be mistaken by my Dutch colleagues...

Georgij Klimov, the representative of Caucasian languages, left us in 1997. He was a slightly shy but very witty, attentive and kind man, and a brilliant, original and sharp-minded scholar. It was always very pleasant to speak and exchange views with him. Unfortunately, my contacts with him, as well as with the other members of the former Soviet delegation, were not frequent enough.

Terho Itkonen was the Finnish representative in the EB and was an enthusiastic participant in the project, and an intelligent and sharp scholar who contributed a great deal to our first scientific debates. I also remember him as a very sensitive, vulnerable and tormented person, who made me often feel like protecting him. His tragic death in 1998 saddened us.

Pavle Ivic, the representative of former Yugoslavia in the ALE, died in 1999, and was the only member of the Editorial Board with whom I developed a real close, brotherly friendship, and whom I visited also socially, in Belgrade, with his wife Milka and his family. We shared many scientific, cultural and social values.

He was a world-renowned Slavic specialist and a very intelligent, open-minded and wise critic, but I admired him also for his soft and elegant diplomacy and, above all, for his unshakable, adamant faith in democracy. I will never forget, in the follow of the breakdown of Yugoslavia, his readiness in acknowledging the separation of Serbian and Croatian, and his unexpected proposal to give an independent status to Bosnian. How happily surprised he would be to see that now the ALE Board has decided to simply change Serbo-Croatian to Serbo/Croatian! He was also the only non-Italian scholar who read, still in manuscript form, my major work on the PCT. And in Belgrade, where the Academy of Science had invited me to hold a lecture on my three-stage theory, at the end of my lecture he illustrated my new PCT to the audience. With him, I really lost a piece of my life.

Jacques Allières left us in 2000. In the ALE Board he represented Basque, of which he was an eminent specialist, but he was also a renowned, extraordinary polyglot and dialectologist, an extremely learned man as well as an interesting and pleasant person, with whom I loved to discuss many issues, besides the ALE.

Finally, Gabriella Giacomelli, who left us in 2002. As a professor of dialectology at Florence University, she played a fundamental role for the ALE when, in 1987, I retired and moved from Holland to Italy. She greatly admired the ALE, of which she had always been a real “fan”, and she accepted with enthusiasm my offer to become the Director of the Editorial Secretariat in Florence. Not only. When it became clear to us that for the first year we could not count on financing from Italian Institutions (red tape in Italy is probably one of the longest in Europe), she was willing, with me, to pay the ALE secretary with our own money. She was a sweet, kind, sensitive, generous and intelligent woman, a close friend of mine and an excellent scholar. Her *Atlante Lessicale Toscano*, now consultable on the web, is a new, extremely valuable tool for the study of Tuscan and contiguous dialects, and thus for the history of that dialect – the Florentine – which has become the Italian language.

I would also like to express my gratitude to Toon Weijnen and to Prof. Viereck, for their substantial contribution, respectively, to the foundation and to the continuation of the ALE, and to wish a great success to Prof. Saramandu for his future work.

As to my own experience, I would like to repeat what I have often said and written, namely that my work for the ALE has been the highlight of my professional life. This wonderful project, which has seen a continued and renewed commitment of individuals and institutions regardless of all the political changes around them, is in my experience one of the finest examples of human commitment to scientific research in a context of international cooperation. My deepest hope is that the ALE will continue as a permanent laboratory for European geolinguistics and dialectology, opening itself, at the same time, to the new interdisciplinary developments that are profoundly changing our views on human evolution, and can be expected to have a gigantic impact also on historical linguistics.

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