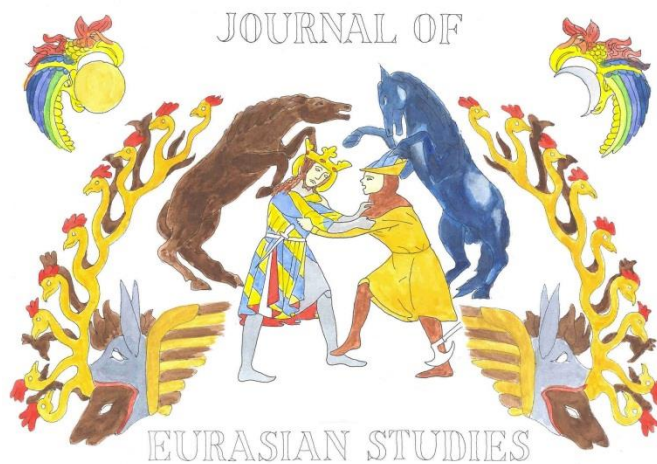


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1. An analytical article should have at least 5,000 words including notes,
2. The author(s) must elaborate the theme of the article logically,
3. References must be uniform and clear (the author(s) should follow consistently a particular pattern, like Chicago style, or Harvard style),
4. Author(s) must take all care to develop their ideas on their own; there should be no cases of plagiarism,
5. Wikipedia is not a scientifically authoritative source; referencing it must be avoided, unless Wikipedia or its usage/influence is the topic of the paper,
6. The article can be written in any language. In case it is written in a language other than English, an English summary of at least A4 length is required,
7. A brief (max. 10 sentences long) professional CV in English.

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DEAR READER,

HARVEST TIME

It seems that book publishing follows the passing of seasons; autumn is not only harvest time in the fields but also for publishers in releasing new titles. Of the rich crops that we got on the market during the autumn of 2014 I would like to highlight two that provided me great pleasure during the cold winter months: *Papegaai vloog over de IJssel* (*Parrot Flew over the IJssel* by Kader Abdolah, and *The Architect's Apprentice* by Elif Shafak.

Papegaai vloog over de IJssel is the latest novel of Kader Abdolah, which is the pen name of Hossein Sadjadi Ghaemmaghani Farahani, was published in September 2014 by the publisher Prometheus in the Netherlands. Kader Abdolah In his mid-30s he fled to the Netherlands as a political refugee in 1988. There he learned Dutch in a record time and in order to overcome the trauma of fleeing and exile he started to write in this language and publish columns, short stories and then novels. Soon he became the Netherlands' number one writer. *Het huis van de moskee* (*The House of the Mosque*) catapulted Abdolah onto the Dutch bestseller lists. It was voted second best Dutch novel ever in the Netherlands. His works have been translated into most of the major languages. It seems that his destiny was to become a literary spokesman in the West for the Persian literature in specific and for the Oriental culture in general.

Having read and liked all his volumes I was reading his latest novel with much anticipation. The main topic of this novel is the fate of immigrants in the Netherlands, who arrived during the past 25 years or so, mainly from countries with Muslim background. It is a relatively new voice in the European literature and it tackles the historic process of the arrival of some 30 million immigrants to Europe during the past few decades, of which about 1 million landed in the Netherlands. This process affected not only their lives, but mostly unexpectedly also the lives of the old inhabitants of their new homeland. Abdolah paints this historic process, undoubtedly using his own personal experiences too, with a fine, highly emotional and humanistic literary brush. This is literature of the highest order, neither plain documentary, nor a romanticized mass literary product. Through the parallel, intertwining and intriguing personal stories of many immigrants and of Dutchmen, we are presented with deep reflections on the human affairs at the turning of the 21st century both in Oriental lands and in the West, in this case specifically of the Netherlands. *Papegaai vloog over de IJssel* is another literary delicacy presented by this highly talented writer of Persian origin, now living and writing in the historic Dutch city of Delft, which I highly recommend to those who can read it in its original, and just let us hope that translation in major languages will follow soon.

The Architect's Apprentice is Elif Shafak's (Elif Şafak) ninth novel, this one also written originally in English and released in November 2014 by Viking. Being Turkey's bestselling female writer, her main themes are centered on cosmopolitanism and sophisticated feminism. In this novel she pays homage to

Mimar Sinan (1489-1588), the greatest Ottoman architect, and one of the greatest ones of mankind. Like Orhan Pamuk's *My Name Is Red*, the plot of *The Architect's Apprentice* is taking place in 16th century Istanbul, the Golden Century of the Ottoman Empire, which is always a fascinating epoch to read about. Elif Shafak created a fictional story based on historical events. The style of the novel exhibits all the mature elements of her writing skills like deep humanity, Sufism, and cosmopolitanism. Both those familiar with the 16th century Ottoman history and those who are not can enjoy and learn from the historical framework and the fictional story. Next to that she peppers the narrative with her usual wise ideas, comments which make the reading a real intellectual pleasure. I can only highly recommend this book for every book lover.

Flórián Farkas

Editor-in-Chief

The Hague, April 19, 2015

ARTS

BÉRCZI, Szaniszló

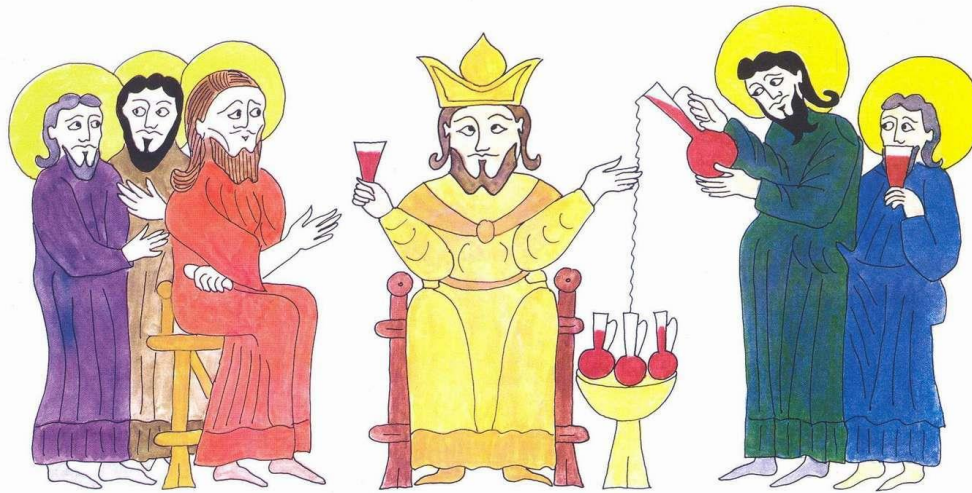
The Bible Art of the Armenians

Armenia is a country in the Caucasus Mountains. Once in the past the country was larger and the population had been decimated by the wars attacking the country at the crossroads of migrations. We get acquainted with the Armenian art in the booklet about the arts of the Caucasus. There we exhibited the wonderful Saint Cross Church of Aktamar (Agdamar) which played a key role in the Romanesque art of Armenian architecture.



*Fig. 1. The front cover of the booklet *The Bible Art of Armenians: Nativity and the visit of the three kings from the East (Epiphany)*.*

In this new booklet we introduce another famous field of the Armenian art which was preserved as paintings in the codices. In these pages we follow the most well known episodes of the New Testament. The role of the small images in the codices - the so called initiales - served as much as paintings and sculptures on the walls of churches: instead of writing, the imaging helped teaching the people the right way of life. Let us overview shortly the Armenian history because this helps understanding how the cultures stratified in the Armenian thinking and life are also represented in the images of the Armenian Bible Codices.



Ezt a munkámat édesanyámnak, Magyar Györgyének ajánlom és tisztelettel emlékezem meg az e munkához kapcsolódó nagyszerű muzsikáról, a Messiás oratóriumról, és annak szerzőjéről, G. F. Händelről is.

Hálával és szeretettel gondolok mindazokra, akik valamilyen formában segítettek az Eurázsiai Művészetek kifestőkönyv-sorozat napvilágra hozását: Bánsági Tamás, Bánsági Mária, Bérezi Alajos, Bérezi István, Bérezi Katalin, Bérezi Zsófia, Biró Mária, Budaházy György, Cseh Vilmos, Czene Valéria, Csíkszentgyörgyi-Ficzus Margit, Csuri Károlyné, Deákné Demjén Ilona, Deák Gyula, Dobos László, Fabriczy Anikó, Földi Tivadar, Greskovits Dávid, Gulács Balázs, Gulács Pálné, Jeppesen Henrik, Kabai Sándor, Kiss Ulrich, Kiszél Vilmos, Kolczonay Katalin, László Gyula, Lukács Béla, Magyar Györgyi, Magyar Jolán, Matyasovich Henrik, Miura Yasunori, Muzslay István, Nagy Dénes, Nyers Csaba, Ogawa Tohru, Ostorházy László, Pap Gábor, Péter Júlia, Péter Borbála, Pongor Gábor, Sára Endréné, Szabó Béla, Szabó László, Szili István, Takahito Mikasa hg., Takaki Ryuji, Tóth Albert, Tordai Magdolna, Veszeclovsky Endréné.

Fig. 2. The back cover of the booklet *The Bible Art of Armenians: The wedding of Kana*.

The state of Urartu (the kingdom of Ararat in the Bible) had been founded circa 3000 years ago in the triangle stretched by the three lakes south of the Caucasus: the Lake Van, the Lake Sevan and the Lake Urmia. In the 7th century B.C. the invasion of the Scythians debilitated the Urartu state. After three centuries of standing of Urartu first the Assyrians, then the Medes and finally the Persians occupied the region. The next change came when the Greeks defeated the Persians, and Alexander the Great organized the new states. Armenia became part of the Seleucid Empire until 190 B.C. when the Armenians won their independence. The Armenian king extended the boundaries of the country to the three seas: to the Caspian Sea in the East, to the Black Sea in the Northwest and in the Southwest region, at the Cilician region, the Mediterranean Sea. (Later this region had the name of Little Armenia.)

A century before Christ, during the rule of Tigran the Great, Armenia became the strongest state of Asia Minor. After his death the country debilitated again. In the wars between Parthia and Rome, Armenians tried to maintain the alliance with the Parthians. This was a successful effort and strengthened by the royal house of the Arsakide Empire. For two centuries the alliance with the Parthians resulted in a peaceful development for the country. In 251 A.D. the rule from Parthians was taken by Sassanids in the Persian Empire. The Persians wanted to degrade the alliance and Armenia became a province. The Persians wanted to further debilitate the Armenians by ordering the taking up of the Zoroastrian religion. Against this enforced obligation the Armenian king Tiridates the III took up Christianity as state religion in 301 A.D. (12 years later the Roman Emperor also took up Christianity.)



Fig. 3. Virgin Mary at the beginning and at the final scene of the booklet. At Angelus Archangel Gabriel reports the Message from the Heavens. At Pentacoste the Holy Spirit arrives to Virgin Mary and the Apostles.

Until the beginning of the 5th century A.D. the Eastern-Roman Empire and the Persian Empire divided Armenia. The Persians enforced the Zoroastrianism religion, but the Armenians rebelled against it under the leadership of Vartan Mamikonian and they succeeded in preserving Christianity.

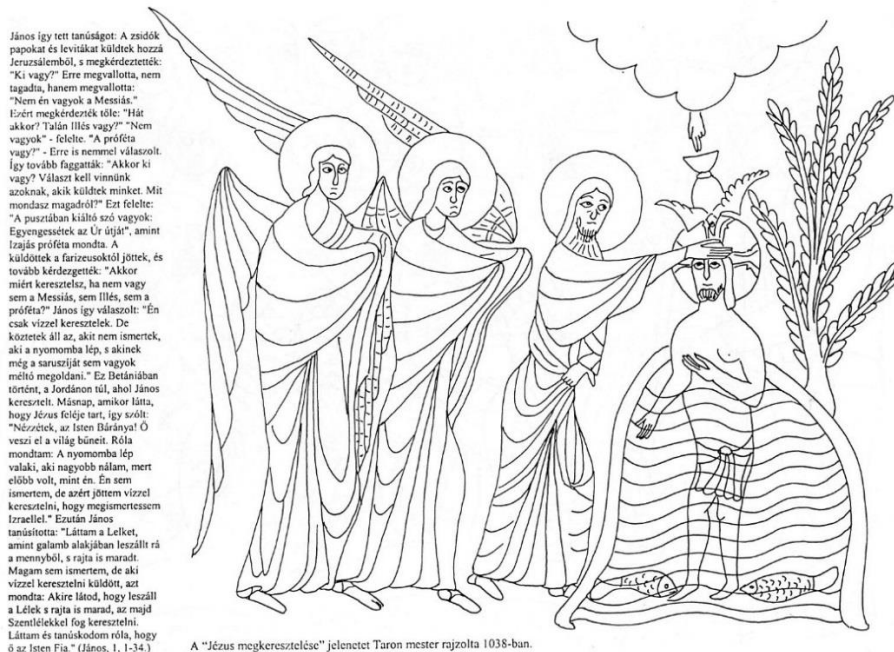
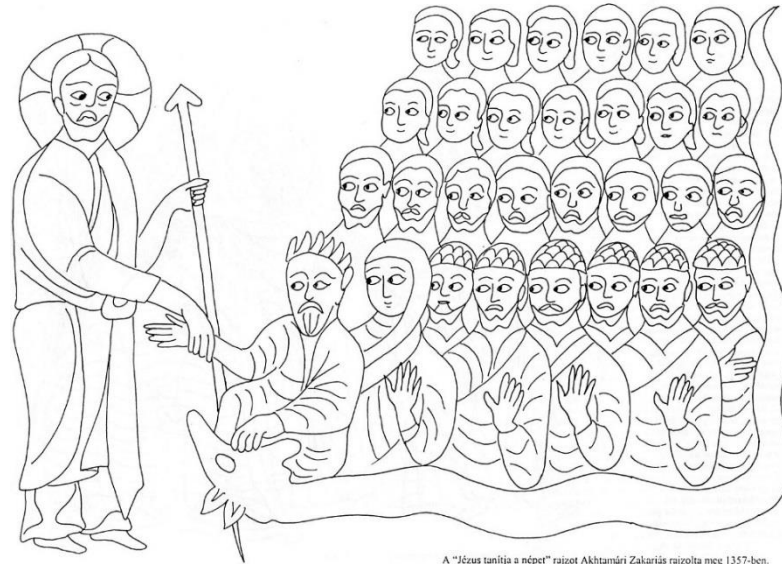


Fig. 4. Baptism of Jesus in the Jordan River by John the Baptist.

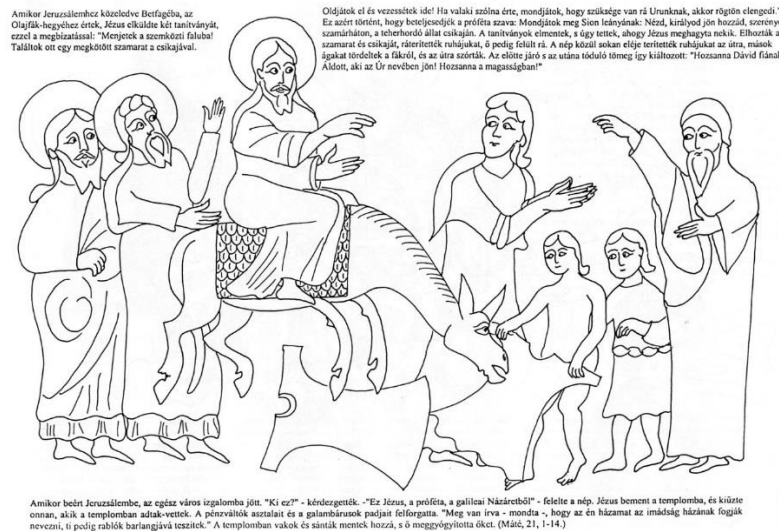
From the beginning of the 7th century the Arab invasion threatened Armenia. With the leadership of the Bagratida family (during the period when the Árpád-people were coming into the Carpathian Basin) Armenia won independence again. The capital became Ani, the town of thousand and one churches.



A "Jézus tanítja a népet" rajzot Akhtamári Zakariás rajzolta meg 1357-ben.

Fig. 5. Jesus teaching the people.

The flourishing of the third Armenian kingdom came to an end by the attacks from the Byzantine Empire and until the middle of the 11th century the country lost independence again. At the same time the attacks from the nomad Seljuq Turks also destroyed the country for 50 years. One part of the Armenians fled to Cilicia and founded Little-Armenia which stood for 300 years.



Amikor Jeruzsálemhez közelítve Betfagéba, az Olajfák-hegyhez értek, Jézus elküldte két tanítványát, ezzel a megbízatással: "Menjenek a szemközti faluba! Találjanak ott egy megkötött szamarat a csikájával.

Oldjátok el és vezessétek ide! Ha valaki szólna érte, mondjátok, hogy szükségé van rá Urunknak, akkor rögtön elengedi." Ez azért történt, hogy beteljesedjék a próféta szava: Mondjátok meg Sion leányának: Nézd, királyod jön hozzád, szentély számarányos, a véherberő állat csikáján. A tanítványok elmentek, s úgy tettek, ahogy Jézus megbízta őket. Elhozták a szamarat és csikáját, ráterítették ruhájukat, ő pedig felült rá. A nép közül sokan előre terítették ruhójukat az útra, mások ágakat törtöltek a fákér, és az útra szórták. Az előtte járó s az utána tördülő tömeg így kiáltozott: "Hosszanna Dávid fiának Állott, aki az Úr nevében jön! Hosszanna a magasságban!"

Amikor beér Jeruzsálembe, az egész város izgalomba jött. "Ki ez?" - kérdezték. "Ez Jézus, a próféta, a galileai Názáretből" - felelte a nép. Jézus bement a templomba, és kiűzte onnan, akik a templomban adóskodtak. A pászvillók szentelét és a galambárusok padját felborította. "Meg van írva - mondta -, hogy az én házámat az imádság házána fogjátok nevezni, ti pedig rablók barlangjává teszitek." A templomban vakok és sértettek mentek hozzá, s ő meggyógyította őket. (Máté, 21, 1-14.)

Fig. 6. Jesus marches into Jerusalem.

The century after the Seljuk invasion brought peaceful development for the country. After that century the nomad peoples from Central Asia (Mongolians, Golden Horde) defeated the country and great communities of Armenians fled to the north: to the Crimean Peninsula, to Poland, to Hungary (to Transylvania during the rule of Princeps Apafi).

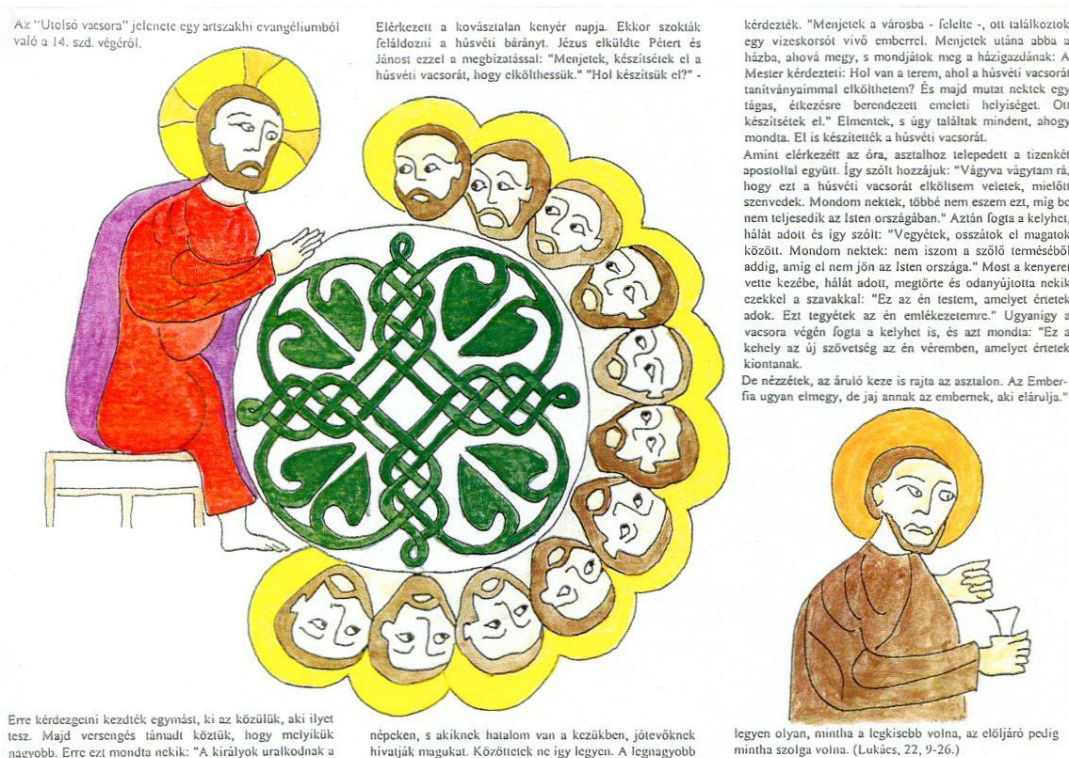


Fig. 7. The Last Supper.

The motherland of the Armenians became divided between the Ottoman Empire and the Persian Empire. The successful victories of Shah Abbas the Great of Persia brought the possibility for Armenians to become accepted as more-or-less equals into the reorganized Persian State in the 16th century. Great communities of Armenians had been settled in the vicinity of the capital, Isfahan. The monasteries of the Apostle Church of Armenia were founded at this time.

We should also review the beginnings of the codices paintings. It probably goes back to the Coptic (Egypt) papyrus paintings and writings. In these texts adornations were taken in and between the rows. The text had been written by hand, and the middle-text figures were colored, too, in order to make beautiful visual effects on the pages.

In Roman art it became popular to place miniatures into the text. The copying of the Biblical Books appeared in the Coptic, Syrian, Armenian and Byzantine old churches as early as the 5th century. Among the European codices art the famous Book of Kells from the 7th century is probably the most well known. There animal figures, plants and flowers occur in and between the rows of the text. Some of them have

been shown in the Eurasian Art Series, in the Celtic Art booklet. During the Caroling renaissance the ornamentation of codices spread to all of Western Europe.



Fig. 8. The capture of Jesus.

The Bible-Art of Armenians developed in the more peaceful periods occurring between the wartime ones. The first images in time which occur in our booklet had been made after the times after the Seljuq attacks.

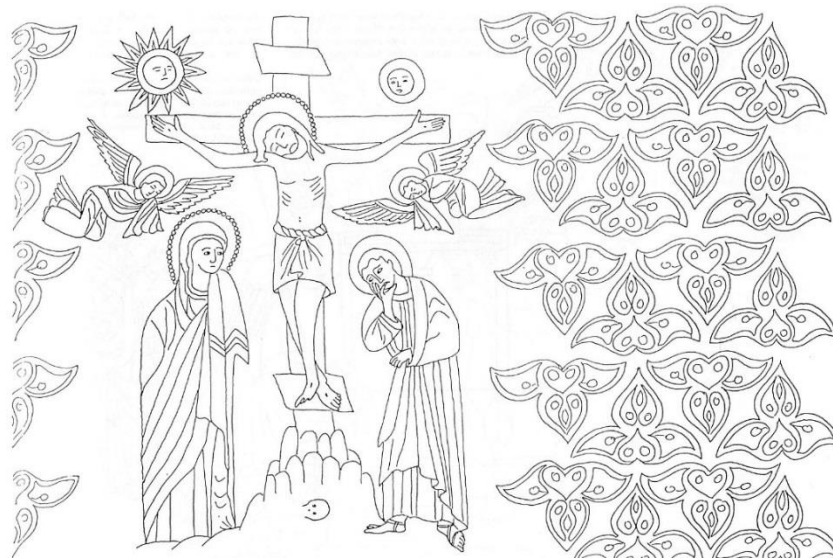


Fig. 9. On the Golgota: Jesus Christ on the Cross.

The heyday of this art happened in the 13-14th centuries when several composer communities came into existence in the historical provinces of the Armenians. From the products of the codice painting schools we selected pieces from the Vaspurakan Province (South, in the vicinity of the Lake Van), the Artsakhi Province (North, in the vicinity of the Lake Sevan), and from Cilicia Province. The clear view and drawing and the embracing imaging was a characteristic feature of these schools.



„A tizenkettő közül az egyik, Tamás, vagy melléknévén Didimosz, nem volt velük, amikor megjelent nekik Jézus. A tanítványok elmondták: „Láttuk az Urat!” De ő kételkedett: „Hacsak nem látom kezén a szegkek nyomát, és oldalába nem teszem a kezem, nem hiszem!” Nyolc nap múlva ismét együtt voltak a tanítványok, és Tamás is ott volt velük. Ekkor újra megjelent Jézus, bár az ajtó zárva volt. Belépett, és köszöntötte őket: „Békesség nektek!” Aztán Tamáshoz fordult: „Nyújtsd ki a kezedet, és tedd oldalamba. S ne légy hitetlen, hanem hívő.” Tamás felkiáltott: „En Uram, én Isteneim!” Jézus csak annyit mondott: „Hittél, mert láttál. Boldogok, akik nem látnak, mégis hisznek!” (János, 20, 24-29).

Fig. 10. After resurrection of Jesus Christ, Apostle Thomas does not believe.

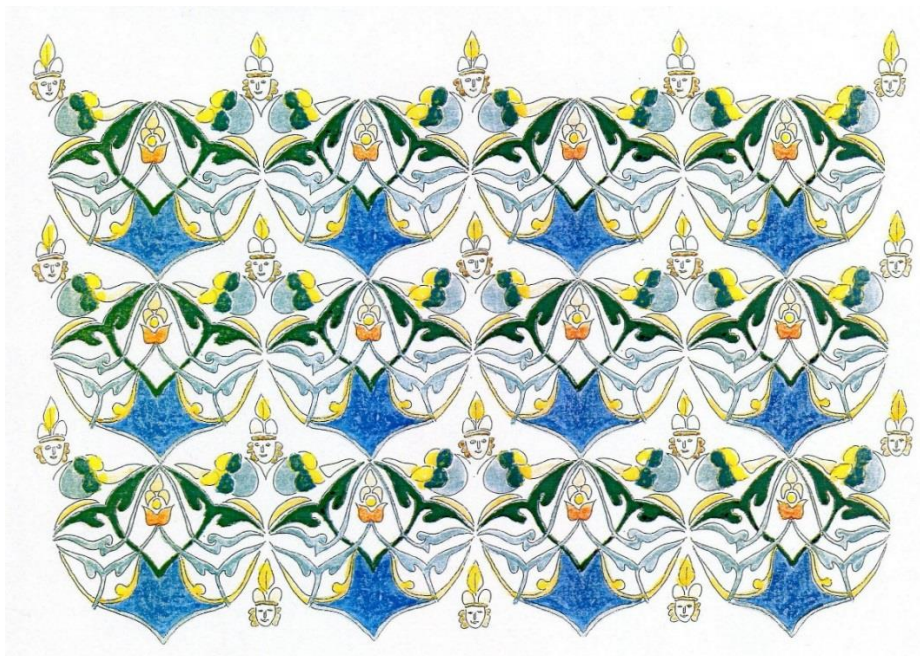


Fig. 11. Ornamental art in the booklet: *The Bible Art of the Armenians*.

We hope that the drawings of the Armenian Bible Art booklet gives joy and pleasure for the readers and to those who like crafts and painting activity.

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HISTORY

ERŐS, Vilmos**Ethnohistory in Hungary (Elemér Mályusz and István Szabó)****Abstract**

The above study offers an overview of one of the most renowned Hungarian medievalists in the 20th century and of his school of „ethnic history“ („népiségtörténet“). In the first part the author describes the roots, program, aims and most important institutions of this ethnic history, as well as the major works it produced. Yet Mályusz dealt not only with ethnic history, but also with ecclesiastical, intellectual and social history, or the history of the institutions, the study treats these briefly as well. Alongside Mályusz, the other major representative of ethnic history was the Debrecen-based historian István Szabó, who after World War II deepened this kind of research with a view mainly to the history of the peasantry. In this he was related to the movement of the so-called „populist writers“, whose „third road“ approach, as the study demonstrates, exerted a considerable influence on his interpretation. Finally, the study portrays the important source publication activities of the two historians (which reinforced the social historical character of their oeuvre), and also the politico-ideological implications of their works.

Historical method as we know it today was established in the nineteenth century, when the professionalism pioneered by Leopold von Ranke was adopted first by German historians, and then spread to France, England, the United States, and even Russia and Italy.¹ The gist of Ranke's methodological reform was to apply the techniques of textual criticism to the writing of history. What counted as historical evidence from that time on was documentary sources—deeds, grants, and charters. Verifying the authenticity of these sources and establishing what exactly they meant came to be considered the historian's most important task, and the single best guarantee of historical objectivity, the historian's duty being, in Ranke's words, to tell his tale *wie es eigentlich gewesen ist*. Essentially, all the contemporary advances in the teaching of history served to promote the new methodology. The departments of history being set up at the major European universities for the first time ever offered a new kind of professional training, one which included exercises in source criticism, and the study of auxiliary disciplines such as diplomacy, paleography, heraldry, epigraphy, and so on. There were also other indications of the growing emphasis on professionalism. Vast source collections were published (most of them modeled on the

¹ Cf. E. Fueter, *Geschichte der neueren Historiographie*, (Oldenbourg:München, 1911); H.E. Barnes, *A History of Historical Writing*, (Dover Publications: New York, 1963); E. Breisach, *Historiography*, (Chicago-London, 1983); and Georg G. Iggers, *New Directions in European Historiography*, (Wesleyan University Press: Middletown, 1975); *Atlas of European Historiography. [The Making of a Profession 1800-2005.]* Edited by Ilaria Porciani and Lutz Raphael. The European Science Foundation. (Palgrave Macmillan: New York. 2010).

Monumenta Germaniae Historica, which went back to 1819), and historical journals sprang up all over Europe, as did historical societies dedicated to the coordinated research of the particular nation's history.

Reformist that he was in the sphere of methodology, Leopold von Ranke was thoroughly conservative in his philosophy of history. Ranke put the weight of his immense authority behind the established practice of identifying "history" with affairs of state and foreign policy, expressly formulating the doctrine of the primacy of foreign affairs (*Primat der Außenpolitik*). He held that historians, like politicians, must focus not on social issues or a nation's internal conditions, but on the problem of power and the shifts in the balance of power. The struggle of the various nations to maintain what power positions they had, Ranke argued, or to extend their sway at the expense of the others, was the very driving force of history. Due in no small part to Ranke's immense prestige, historians continued to focus on narrative political history, and on the lives of statesmen and military leaders, "great personalities" who shaped their times. This entire approach—called "historicism" by some authors—took a modern turn with the advent of New History in the United States, the *Annales* in France, and the new social history that started in Germany after the Second World War. What all these schools had in common was the determination to establish a "scientific" history writing. Reassessing the role of the historian, they emphasized not so much the critical evaluation of the sources, but the need to analyze the law-like regularities behind all phenomena, and the main trends of development. These law-like regularities, they held, were most evident in a society's material culture and the patterns of social and economic development. To reconstruct them, one needed to study not documentary sources, but new types of historical evidence: maps, censuses, church registers (for births and deaths), tools, foodstuffs, and so on. To help investigate this source material, the "scientific" schools turned to the insights and techniques of the "other" social sciences: ethnography, geography, linguistics, anthropology, archeology, sociology, and economics. The change was reflected also in the training recommended for would-be historians. Rather than focusing on the auxiliary sciences, as their nineteenth-century counterparts did, historians were encouraged to acquire competence in all the social sciences. All the above schools concurred in their repudiation of Ranke's *Primat der Außenpolitik*. They concurred also in their belief in the *Primat der Innenpolitik*, i.e., that the main responsibility of the historian lies in fostering initiatives aimed at improving society.

The modernization of historiography under the impact of New History and the *Annales* began in the inter-war years, but it was only after the Second World War that the "scientific" trend really came into its own. The Rankean type of narrative political history, however, has more than managed to linger on, as the *Historikerstreit* of the 1980s so spectacularly demonstrated.

In Hungary, it was not until the post-1867 dualist era that historians came to identify with the professionalism advocated by Ranke.² The landmarks of this development were similar to those marking the progress of historicism elsewhere—source publications, reliance on the auxiliary sciences, and the establishment of historical societies and journals. And while few historians were as rigorous as Ranke in their sifting of the "historical evidence", narrative political history was the focus of most history writing.

² For the development of Hungarian historiography, cf. Emma Léderer, *A magyar polgári történetírás rövid története* [A Short History of Hungarian Bourgeois Historiography], (Budapest, 1969); Ágnes R. Várkonyi, *A pozitívista történetírási élet a magyar történetírásban* [Positivism in Hungarian Historiography], I-II, Budapest, 1973; Bela Vardy, *Modern Hungarian Historiography*, (New York: Columbia University Press, 1976). For Hungarian historiography between the two world wars, see also Ferenc Glatz, *Nemzeti kultúra-kultúra/nemzet* [National Culture a Nation of Culture], (Budapest, 1988.)

There were, of course, initiatives that went counter to the prevailing trend. Gyula Pauler, for instance, who had high praise for Comte's positivism, advocated probing for the universal features of human progress, and urged the investigation of collective, mass phenomena, and aspects of life generally subsumed under the heading of cultural history.³

Between the two world wars, the dominant trend in Hungarian history writing was *Geistesgeschichte* [spiritual history] as represented by the works of Gyula Szekfű, Bálint Hóman, Gyula Kornis, Tibor Joó, József Deér, and Péter Vaczy. Fully versed in the works of Ranke, Meinecke, Dilthey and Lamprecht, Gyula Szekfű, the most outstanding of these historians, was also the one to conclude that Hungarian history would lend itself admirably to a consistent synthesis.⁴ In his *A magyar állam életrajza* (1918), and in his *Bethlen Gábor* (1929), Szekfű expressly models his approach on Meinecke's⁵, and tells the entire story from the vantage point of *raison d'état* and the national point of view. This meant that for him, the central issue of Hungarian history was the territorial integrity of historic Hungary, the Hungary of St. Stephen. This particular outlook is even more evident in Szekfű's *Három nemzedék* (1920), the veritable Bible of the period. Here, he depicts the nineteenth-century Hungarian liberals responsible for the disintegration effected by Trianon. Blinded by the political tradition of the nobility's struggle for Hungarian independence throughout the sixteenth and seventeenth centuries—ran Szekfű's indictment of the liberals—they construed the word "freedom" to mean "independence from the Habsburgs", and failed to realize that the territorial integrity of historic Hungary (i.e., Hungarian rule over the nationalities) could be maintained only with the support of an outside great power, namely, the Habsburg Empire. (This correlation was something that Széchenyi had recognized, and Szekfű, accordingly, esteemed him as by far the greatest Hungarian.)

One finds the same train of thought in all the sections that Szekfű wrote of *Magyar Történet* (Hungarian History, 1929-1933), a seven-volume synthesis he published together with Bálint Hóman. (Szekfű authored the period stretching from King Matthias Corvinus and the Renaissance to the date of publication). In the final analysis, at every stage of Hungary's history, we find him dividing the leading politicians into two groups: those who believed in "Small Hungary" and those who believed in "Greater Hungary". The "small Hungarians" were those whose primary goal was national independence from the Habsburgs. But this aspiration of theirs, he maintained, was motivated not by some lofty ideal, the love of freedom, for example, but by selfish "class interest" (the nobility's determination to protect its privileges), coupled with a passion for dissension and upheaval inherited from their Eastern ancestors. Another name for this

³ Cf. Gyula Pauler, 'A pozitivizmus hatása a történetíráásra' [The Influence of Positivism on the Writing of History], *Századok*, 1871(5): 527-645.; 624-641; and Gyula Pauler, 'Comte Ágost és a történelem' [Auguste Comte and History], *Századok*, 1873 (7): 225-241; 391-406; 462-481.

⁴ Szekfű's major works, in chronological order: *A Magyar állam életrajza* [A Biography of the Hungarian State] (Budapest: Dick Manó, 1918); *Három nemzedék* [Three Generations], (Budapest, 1920); *Történetpolitikai tanulmányok* [Historical-political Studies], (Budapest: Magyar Irodalmi Társaság 1924); *Bethlen Gábor*, (Budapest: Magyar Szemle Társaság, 1929), *Magyar történet* [Hungarian History] (Budapest: Magyar Történelmi Társulat, 1929-33), and *Állam és nemzet* [State and Nation], (Budapest: Magyar Szemle Társaság, 1942).

⁵ For Meinecke, see Walter Hofer, *Geschichtsschreibung und Weltanschauung*, (München: Oldenbourg, 1950); Ernst Schulin, 'Das Problem der Individualität' (Eine kritische Betrachtung des Historismus-Werkes von Friedrich Meinecke) [The problem of individuality. A critical analyses of the historicism-work by Friedrich Meinecke], *Historische Zeitschrift*, 1963(197): 102-133; Hans Klueping, "'Vernunftrepublikanismus' und 'Vertrauensdiktatur'", Friedrich Meinecke in der Weimarer Republik", *Historische Zeitschrift*, 1986 (242): 69-98.

"passion" was Protestantism, which, as Szekfű saw it, was *ab ovo* inspired by the resolve to spark denominational conflict and create disorder.

The "great Hungarians", on the other hand, had always appreciated that the great power status of the Habsburg Empire was a historical necessity. They recognized the need for political compromise, and strove to promote social reform, and the nation's material improvement and intellectual progress (naturally, with Habsburg support). Szekfű's synthesis presents the Baroque culture of the eighteenth century as the zenith of Hungarian history, a time when the country's territorial integrity had been more or less restored, when religious (Protestant vs. Catholic) and political (Estates vs. absolutism) in-fighting no longer undermined the unity of the nation, when the country's economic and cultural development picked up momentum, and its resettlement began.

Even in the late '30s, Szekfű was very much preoccupied by matters of external politics and national sovereignty. In his *Állam és nemzet* (State and Nation, 1942), he rejected both the French notion of a political nation and the German "ethnic nation" concept, and presented a uniquely Hungarian notion, one rooted in St. Stephen's tolerance toward the "foreigners". It was a nation concept which guaranteed the country's minorities a high degree of autonomy, while its *raison d'être* was to safeguard, and/or to restore Hungary's territorial integrity.

One historian who strongly and openly opposed Szekfű's views right from beginning of his own professional career was Elemér Mályusz. The first tilt in his intellectual and ideological jousts with Szekfű was his "A reformkor nemzedéke" (The Reform Generation)⁶. In this study Mályusz refutes Szekfű's claim that the middle nobility of the Reform Era was prompt-ed to armed confrontation with the Habsburgs only by its obsessive determination to redress the Court's encroachment on its political privileges and argues that its goal was the country's embourgeoisement. To substantiate his interpretation, Mályusz points to the reports of the various county committees appointed by the 1791-92 Diet, which already contained the outlines of a program of modernization and "bourgeois transformation". As for the anti-government posture of the uneducated lesser nobility, that, Mályusz maintained, was a consequence of their deteriorating social status, and their resentment of attempts by the great landowners and the central government alike to curtail their customary rights through enclosure.

Mályusz also rejected the interpretation advanced by Szekfű in *Magyar Történet*, his main objection being to the inconsistency of Szekfű's vision of the country's cultural development.⁷ Szekfű saw the Hungarian Renaissance a confined to the reign of Matthias Corvinus, and gave no explanation for the subsequent "immobility" that set in up to what he considered to be the beginning of the Baroque in the eighteenth century. Mályusz, on the other hand, held that "the Renaissance" was applicable to the Hungarian culture of the entire sixteenth century, and that the seventeenth century was already the time of the Baroque in Hungary. In other words - and this is Mályusz's main thesis -Hungary's early modern cultural development kept pace with the intellectual and cultural trends of Western Europe, and had kept abreast even in earlier times for-as he demonstrated with an analysis of the legend of Blessed Margaret of

⁶ Elemér Mályusz, 'A reformkor nemzedéke'[The Generation of the Reform Period], *Századok*, 1923(57-58): 17-75.

⁷ Elemér Mályusz, "Magyar reneszánsz, magyar barokk" [Hungarian Renaissance-Hungarian Baroque], *Budapesti Szemle*, 1936 (241):159-179, 293-318; (242): 86-104, 154-174.

the House of Árpád⁸-as early as the thirteenth century, Hungary had been able to absorb the Gothic, the most modern cultural trend of that time. Mályusz also took exception to Szekfű's views on Transylvania and the Transylvanian Reformation. As he saw it, both the Transylvanian educational system, with its emphasis on the natural sciences, and the Transylvanian Reformed/Puritan denominations, with their gospel and practice of tolerance were veritable harbingers of the Enlightenment. (In short, Hungarian cultural development at the time was on a par with that of England and the Netherlands).

Mályusz considered the tolerant religious policies of Ferenc II. Rákóczi to be the culmination of this development, and proof that, left on its own, Hungary would have been capable of embourgeoisement and modernization. One of the gravest tragedies of Hungarian history, he maintained, was the period of Habsburg reaction that set in following Rákóczi's defeat-a time of resurgent religious fanaticism and subverted national sentiment, a time when Hungarian Protestants were driven off their lands, and foreigners were brought in and were settled all over the country.

In essence, it was on a political and ideological plane that Mályusz attacked Szekfű's *Geistesgeschichte*[spiritual history]-inspired interpretation of history. The most serious shortcoming of this representation of Hungarian history, as Mályusz saw it, was that Szekfű attributed far too positive a role to the Habsburgs, and seemed to have no sense of Hungary as a sovereign and autonomous culture. A dangerous attitude, given that Hungary could depend on nothing but its own strength in the pursuit of its national aspirations-and here Mályusz, too, was thinking of Trianon. Thence his eagerness to see ethnohistorical research start up; it was, he believed, the only way to demonstrate the sovereignty of Hungarian culture. (This was an issue he would return to time and time again⁹. In other words, Mályusz realized that to win his battle against *Geistesgeschichte*, he needed not only to refute its ideology, but also to transcend its methodology.

The roots of Mályusz's ethnohistory¹⁰ go back to the early '20s. His own doctoral thesis, *Turóc megye kialakulása* [The Formation of Turoc County] published in 1922¹¹, deals with a topic that anticipated the theses his students were to write ten years later. That all this-though not called ethnohistory at the time-was part of a full-fledged historiographic program is illustrated by Mályusz's 1924 study¹² on the challenges of doing local history.

After describing the work of Dezső Csánki and Károly Tagányi, two late nineteenth-century pioneers of local history and historical geography, he goes on to urge historians to follow the lead of the German *Territorialgeschichte*[territorial history], and focus more on local history. The study of "non-documentary" sources (land registers, church registers, place names, etc.) would facilitate the clarification of questions of

⁸ Elemér Mályusz, 'Árpádházi Boldog Margit' [Blessed Margaret of the House of Árpád], in *Károlyi Árpád emlékkönyv*, (Budapest, 1933), pp. 341-384.

⁹ Elemér Mályusz, "A történettudomány mai kérdései" [The Problems of Doing History Today-A Lecture], (Kecskemét, 1936); Elemér Mályusz, *A magyar történettudomány* [Hungarian Historiography], (Budapest: Bolyai Könyvek, 1942).

¹⁰ Cf. Elemér Mályusz, *Népiségtörténet* [Ethnohistory]. Edited by Soós István. (MTA Történettudományi Intézete: Budapest, 1994.)

¹¹ Cf. Mályusz's "Turóctól Thuróczyig", where he recalls that in the early '20s, he had wanted to write up the settlement history of all of Upper Hungary: "Turóctól Thuróczyig", (prepared for publication by Istvan Soós), *Sic Itur ad Astra*, 1990/1-2: 128-138.

¹² Elemér Mályusz, "A helytörténeti kutatás feladatai" [The Tasks Facing Research into Local History], *Századok*, 1924(57-58): 538-66.

settlement history, public administration, property relations, and genealogy, and would lend a sociological dimension to Hungarian historiography.

The importance of the sociological approach to the study of local history remained a key concept also in "A népiség története" (Ethnohistory) written in 1931¹³, and the most comprehensive formulation Mályusz would ever give of his program. The study starts with a definition of the notion of "the ethnic". As opposed to "the national", the *conscious* expression of a people's cultural and political aspirations, "the ethnic" was shorthand for the *spontaneous/unconscious* ways and cultural preferences of a particular people. The best way to get started in ethnohistorical research, he went on to say, was to write "synthetic" local and/or county histories. By "synthetic" he meant just the opposite of the village by village approach of the prewar county histories: the historian was to focus on the small, organically related historic-geographical units-estates, valleys, plains, and so on-units he would later call "cultural regions", and whose study he expected to reveal an entire network of Southern, Eastern and Northern cultural contacts.

Mályusz honed his theory by clashing swords with proponents of the most powerful historical ideology of his time.¹⁴ Taking a direct stab at *Geistesgeschichte* [spiritual history], its preoccupation with Western cultural influences and its exclusive reliance on the evidence of the written word, he set ethnohistory the task of concentrating on "spontaneous" cultural elements such as roads, means of transportation, architecture, settlements, systems of local political and administrative organization, and "anthropological" data of every kind that might serve to give an accurate picture of the day-to-day life of the people.

Mályusz's views on the nature and techniques of ethnohistory, were thus fully developed by the time he came to give his "Introduction to Ethnohistory" course in the 1936-37 academic year. One of the main issues addressed in the lectures was the matter of the "auxiliary disciplines" which Mályusz proposed to "modify" with a view to making them integral parts of the science of ethnohistory. He was particularly enthusiastic about the potential of ethnography and of linguistics, attaching great importance to the study of dialects (and their exact geographic mapping), and to tracing the origins of place names and personal names. He was also keen to have his students learn to use questionnaires, and to set up the institutional framework of ethnohistorical research.¹⁵

The last of Mályusz's theoretical works on ethnohistory was the series of articles collected and published as *A magyar történettudomány* [Hungarian Historiography] in 1942. In these articles, he called upon the most prestigious of the country's scholarly bodies - the Academy of Sciences, the Budapest Pázmány Péter University, the Historical Society - to shift the focus of their activities to ethnohistory. The Academy, he suggested, should offer bursaries to students of ethnohistory, which he wanted to see

¹³ Elemér Mályusz, 'A népiség története' [Ethnohistory], in *A magyar történetírás új útjai* [New Trends in the Writing of History in Hungary], Edited by Bálint Hóman (Magyar Szemle Társaság: Budapest, 1931), pp. 237-69.

¹⁴ Elemér Mályusz, "Három folyóirat" [Three Journals], *Századok* (68), 1934: 45-65.

¹⁵ The lecture dealing with anthropography has not been included in the edition of *Népiségtörténet* in 1994. Mályusz had given three reasons for attaching importance to anthropography: its methodology lent itself to a degree of objectivity equal to that of the natural sciences; it helped to reconstruct ancient history; it could be crucial to determining the origin of certain ethnic groups, e.g., the Székelys. Unlike Székfű who, held that the concept of race had no place in history, Mályusz approved of research aimed at establishing the racial origins of peoples. He rejected, however, the identification of "race" with "ethnic group".

introduced as one of the subjects in which prospective secondary school teachers could major at the university. Mályusz also called upon his fellow historians to chart the layout of all the towns in Hungary, to do research on the question of assimilation, and to introduce the notions "ethnic ground" [Volksboden] and "cultural ground"[Kulturboden] among the accepted terms of historical geography. The program carried explicit political overtones as well: the Historical Society, Malyusz submitted, would do well to set up an institute for the study of the Jewish question.

It was this book that cost Mályusz his job at the university after the war, when he was also stripped of his membership in the Academy of Sciences.

Mályusz was not just a theoretician; first and foremost, he was a practicing historian. His first attempt to put his program of ethnohistory into practice was his doctoral dissertation, in which he examined how, thanks to a consistent policy on the part of the exchequer, the crown land of Zólyom evolved in time into the noble county of Turóc. His next work of ethnohistory was written ten years later at Pál Teleki's behest. *Geschichte des ungarischen Volkstums* (finally published in 1940)¹⁶ tells the story of the peoples of Hungary focusing on the Magyars' internal colonization of Pannonia in the decades following the Conquest, the progressive consolidation of their rule over the entire area, the settlement of the region by successive waves of immigrant peoples, and the pattern of social development that evolved in the region up to Werbőczy's time.

Mályusz's next major works with an ethnohistorical slant grew out of the lectures he gave in the latter half of the '30s on "the ethnic ground"[Volksboden] and "the cultural ground"[Kulturboden] of the Magyars in medieval times. "A magyarság es a nemzetiségek Mohács előtt" and "A középkori magyar nemzetiségi politika" both appeared in 1939¹⁷, the latter giving rise to considerable controversy, and not just in academic circles. In the study on the country's ethnic composition prior to Mohács, Mályusz argued that in respect of the ethnic composition of the population, fifteenth-century Hungary fell into three major areas: 22 counties inhabited only by Magyars, 26 counties where Magyars comprised 80 percent of the population, and 9 counties where Magyars were a minority, Le., comprised 20 percent of the population. From all this, he concluded that the medieval Kingdom of Hungary was Magyar in character not primarily because of its Magyar political institutions, but because of its predominantly Magyar population. Mályusz had made much the same point in his lecture series on of the Magyars in medieval times, where he demonstrated that the House of Árpád had pursued a deliberate settlement policy in establishing villages in the Military Frontier Zone for the protection of the Magyar population.¹⁸ Addressing Szekfű in his "A középkori magyar nemzetiségi politika", Mályusz presents yet further evidence to support his contention that there was nothing arbitrary in the immigration policies pursued by Hungary's medieval kings. A close study of place names of medieval origin, he points out, indicates that the immigrant peoples were not

¹⁶ Cf. Elemér Mályusz, *Geschichte des ungarischen Volkstums von der Landnahme bis zum Ausgang des Mittelalters*. [Hungarian Ethnohistory from the Conquest until the end of the Middle Ages] (Budapest: Pannonia-Bücher 1940), 120 pp.

¹⁷ Elemér Mályusz, 'A magyarság és a nemzetiségek Mohács előtt' [The Magyars and the Minority Nationalities prior to Mohacs], *Magyar Művelődéstörténet II*, (Budapest, 1939), pp. 105-124; and 'A középkori magyar nemzetiségi politika' [Nationality Policy in Hungary in the Middle Ages], *Századok* (73), 1939: 257-94, and 385-448.

¹⁸ Cf. Elemér Mályusz, *A középkori magyarság település- és nemzetiségi politikája*. [Settlement - and nationality policy in Hungary in the Middle Ages.] Edited by Soós István. (: Budapest: Lucidus, 2002)

settled on large, contiguous tracts of land, but interspersed among the Magyar population, obviously with a view to accelerating their assimilation.

Mályusz does not moot the reality of a tolerant, "democratic" nationality policy, one that respected the autonomy of the minorities, but he dates it not to the time of St. Stephen, but to the fifteenth century, a time of growing influence for every one of the three estates, a development which tended to strengthen the local organs of self-government. In other words, unlike Szekfű who, by way of providing the Kingdom of St. Stephen with moral legitimacy, posited a spirit of tolerance toward the national minorities going back to the "Catholic spirituality" of St. Stephen, Malyusz insisted that tolerance was a product of social development. His purpose was to prove the strength and autonomy of Hungarian culture. The spirit of ethnic tolerance, he claimed, was not the legacy of some foreign priest-the author of the *Libellus de institutione morem* (written in the name of St. Stephen for the instruction of his son Imre) it was something that the Hungarian nation achieved through mobilizing spiritual resources of its own.

As the first step to providing ethnohistory with an institutional framework, in 1932, Mályusz, working under the auspices of the National Archives, started a seminar, rather a working group on the settlement history of Upper Hungary. The aim was to establish the exact border between the Hungarian and Slovak linguistic zones; the tangible outcome was the publication of the "A magyarság és a nemzetiségek" (The Magyars and the National Minorities) series.

Another milestone in the institutionalization of ethnohistory came in 1937, when the Institute for Ethnohistory and Settlement History was set up at the Pázmány Péter University. The institute would be to publish "Település és Népiségtörténeti Értekezések" [Studies in Settlement History and Ethnohistory], the series in which the doctoral dissertations submitted by Mályusz's students would appear.¹⁹

As indicated earlier, other important researches of Mályusz's can be referred too. I have already mentioned some of his social historical studies, but to them can be added f.e "A patrimoniális királyság", "A karizmatikus királyság", "A magyar köznemesség kialakulása", "A magyar társadalom a Hunyadiak korában", "A Rákóczi kor társadalma".²⁰

In these studies Mályusz depicts/outlines a sketch about the whole development of the Hungarian society, from its beginnings till the 19-th century and even further.²¹ One of the most striking features of this panorama is the central position of the nobility, which - following Mályusz - possessed always a higher elite imbued with European culture and political capability. This social rank was in Hungary the leading force of the social reforms and modernization, even that of embourgeoisement in opposition to the

¹⁹ As we know from Elemér Mályusz's memoirs, a total of eight dissertations appeared in the "Település és Népiségtörténeti Értekezések" series. See Vardy, *Modern Hungarian Historiography*, pp. 248-49.

²⁰ Cf. Elemér Mályusz, 'A patrimoniális királyság.' [Patrimonial Kingship.] *Társadalomtudomány*, 1933/1-2: 37- 49; Elemér Mályusz, 'A karizmatikus királyság'. [Charismatic Kingship.] *Társadalomtudomány*, 1934/3: 153-178; Elemér Mályusz, 'A magyar társadalom a Hunyadiak korában. A hűbériség és rendiség problémája'. [Hungarian Society in the Age of the Hunyadis. The Problem of Feudalism and Estatism.] In Lukinich Imre (ed.), *Mátyás király emlékkönyv születésének ötszázéves évfordulójára. I.* (Budapest, 1940.) 309-433. pp. Elemér Mályusz, 'A magyar köznemesség kialakulása', [The Development of the Hungarian Nobility.] *Századok* (76) 1942: 272-305, 407-434.

²¹ Cf. Elemér Mályusz, 'Kossuth működésének társadalomtörténeti háttere.' [The Social Historical Background to Kossuth's Activities.] *Napkelet*, 1928(6), 11: 166-183.

Western countries where the "third estate" fulfilled this task. The bourgeoisie in Hungary could have played the same role since it was of German origin and analyzing the self government policy of the towns - turns out, that they had an aristocratic constitution.²²

Other important directions of Mályusz's researches were his partly already also mentioned ecclesiastical and spiritual history (*Geistesgeschichte*) studies. The most outstanding of them were in this respect (partly already mentioned) - "Árpádházi Boldog Margit" [Blessed Margaret of the House of Árpád], "A türelmi rendelet", "A pálosrend a középkor végén" [The Paulist Order at the End of the Middle Ages.], "Az egyházi társadalom a középkori Magyarországon" [Ecclesiastical society in Hungary in the Middle Ages.], and - respectively - "A gótika Magyarországon" (The Gothic in Hungary), "Magyar renaissance - magyar barokk" [Hungarian Renaissance, Hungarian Baroque], "A felvilágosodás Magyarországon" [The History of Hungary in the Age of Enlightenment.] and his chronicle-studies ("Thuróczy János krónikája" [The Chronicle of Thuróczy and its Sources.], "V. István-kori geszta" [The Gesta of the Age of Stephen V.] 23, etc.)

From these studies is obvious that Mályusz did not reject unanimously the *Geistesgeschichte* tradition, only that sort of it represented by Hóman and Szekfű - they eulogized/glorified namely the line of the Middle Ages, Baroque, Romantic Period - embodying/epitomizing much more the values of Catholicism and neglecting/defying the significance of the other line, of these sequels of ideas/*Weltanschauungen*, the Gothic, Renaissance, Enlightenment being more favorable to the Protestants.

Similar ideas can be detected in the field of Mályusz's ecclesiastical researches. For example. in his "Egyházi társadalom a középkori Magyarországon" (the roots of which go back to the thirties, to his lectures at the University of Budapest, entitled "A gótika Magyarországon"²⁴) he attempted to prove that the paramount feature of the social development in Hungary was the "secularization" process, the formation of a certain secular intellectual rank within the society. This prepared the (also secular) ideas of reformation/Protestantism which stemmed/arose of a deep social and cultural desire and by this motive can be explained its rush spreading all over the country at the beginning of the 16-th century.]

²² Cf. Elemér Mályusz, 'Geschichte des Bürgertums in Ungarn.' [The history of bourgeoisie in Hungary] *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 1927 (20): 356-407. The self government of the cities - so Mályusz - consisted of the inner (small) council and the outer (big) council, the members of the later (originally all of the citizens) elected the representatives /deputies of the former body. After a certain time the members of the big council were not elected, but nominated by the older families, because they wanted to keep away the immigrating (mostly non German, Hungarian) inhabitants from the leading positions of these communities.

²³ Cf. Elemér Mályusz, *Magyarország története a felvilágosodás korában*. [The History of Hungary in the Age of Enlightenment.] Edited by István Soós. (Budapest: Osiris, 2002.) Elemér Mályusz, *A Thuróczy-krónika és forrásai*. [The Chronicle of Thuróczy and its Sources.] (Budapest: Akadémiai Kiadó 1967.) Elemér Mályusz, *Az V. István-kori gesta*. [The Gesta of the Age of Stephen V.] (Budapest: Akadémiai Kiadó, 1971.) Elemér Mályusz, 'A pálosrend a középkor végén.' [The Paulist Order at the End of the Middle Ages.] *Egyháztörténet*, 1945 (3): 1-53.

²⁴ Cf. Elemér Mályusz, *Egyházi társadalom a középkori Magyarországon*. [Ecclesiastical society in Hungary in the Middle Ages.] (Budapest: Akadémiai Kiadó, 1971.)

Another important figure of the Hungarian "ethno history" was István Szabó²⁵, whose synthesis/summary about the development of the history of the Hungarian population was already mentioned.²⁶ According to the literature dealing with the historian from Debrecen²⁷ there are three main fields of his historical research/activity:

Firstly his researches referring to the history of his native city, Debrecen including his studies about the history of the town during the revolution in 1848-1849, when it became for the first time the capital of the country²⁸. After the Second World War he returned to this theme when on the occasion of the centenary of the revolution he edited with the well-known protestant bishop and church historian, Imre Révész Jr. the book with the title "Debrecen, the capital of the independence war in Hungary"²⁹. The book stirred up heavy discussions, and provoked fierce criticism on the side of communist historians, on which details I'm going to come back later. Other important studies of Szabó concerning the history of his native city and of its surroundings (The Great Hungarian Plain) include: "A debreceni tanyarendszer kialakulása." [The making of the settlement/hamlet system around Debrecen], "A tokaji rév és Debrecen" [The ford of Tokaj and Debrecen], "A debreceni közösség" [The community of Debrecen], "Debrecen a történelemben" ["Debrecen in the history of Hungary"] etc.³⁰

The most important/striking feature of these studies that he started originally with the political aspects of the history of the city ("histoire evenementielle", "drum and trumpet" history), then gradually deepened this problematic towards the direction of social historical aspects. His main concern became (cf. his studies later about the Haiduks, market towns, etc.³¹) the possibilities of a special Hungarian way of modernization/bourgeois development mainly based on the peasantry. According to him the situation of the peasantry even in the Middle Ages was improving, and even e.g. the phenomena of "deserting" at the end of the Middle Ages, the decline can be explained not by the deteriorating/aggravating situation,

²⁵ Literature about Szabó cf. Vilmos Erős, 'Szabó István körül.' [About István Szabó.] *Aetas*, 2000/3: 110–126; Vilmos Erős, 'Szabó István és 1848/49.' [István Szabó and 1848-1849.] *Valóság*, 2003/7: 94–108.; Vilmos Erős, *Asszimiláció és retorika.* [Assimilation and rhetorics.] (Debrecen: Csokonai Kiadó, 2005); Vilmos Erős(ed.), *A harmadik út felé. Szabó István történész cikkeiben és dokumentumokban.* [Towards the Third Way. The Historian István Szabó in articles and documents.] (Budapest, Lucidus Kiadó 2006); Vilmos Erős, *A szellemtörténettől a népiségtörténetig. (Tanulmányok a két világháború közötti magyar történetírásról.)* [From Spiritual History to Ethnohistory. Studies about the Hungarian Historiography between the two World Wars.] (Debrecen: Debrecen University Press/Egyetemi Kiadó, 2012.)

²⁶ Cf. István Szabó, *A magyarság életrajza.* [The Biography of the Hungarian People.].(MTA: Budapest, 1941.)

²⁷ Cf. Erős, *A szellemtörténettől a népiségtörténetig. (Tanulmányok a két világháború közötti magyar történetírásról.)* [From Spiritual History to Ethnohistory. Studies about the Hungarian Historiography between the two World Wars.]

²⁸ Cf his doctoral dissertation Szabó István, *Debrecen 1848/49-ben.* [Debrecen in 1848-1949] (Debrecen, 1928).

²⁹ Cf. István Szabó (ed.), *A szabadságharc fővárosa, Debrecen, 1849. január-május.* [Debrecen, the capital of independence war in 1849.] (Debrecen, 1948.)

³⁰ Cf. István Szabó, 'A debreceni tanyarendszer kialakulása.' [The making of the settlement/hamlet system around Debrecen] *Föld és Ember*, 1929/5: 214–244. István Szabó, 'A tokaji rév és Debrecen 1565–67-ben.' [The ford at Tokaj and Debrecen in 1565-67.] *Debreceni Képes Kalendárium*, 1934: 89–97; István Szabó, 'A debreceni uradalom a mohácsi vész korában.' [The estate of Debrecen at the age of Mohács.] *Debreceni Képes Kalendárium*, 1935: 85–90; István Szabó, 'A debreceni közösség'. [The community of Debrecen] *Debreceni Képes Kalendárium*, 1940: 73-77.

³¹ Cf. István Szabó, 'A hajdúk 1514-ben.' *Századok*, 1950 (84): 178-198., 478-488.

exploitation of the peasantry, but much more by the "sweeping/attracting effect" of the market towns, offering the possibility of higher standard of living, respectively culture within their walls.³²

Another important field of research of Szabó's was his before mentioned "ethno"- or "population" history studies. His main works in this respect include the "Ugozca county"(1937), "The biography of the Hungarian People"(1941)³³, 'Az asszimiláció a magyarság történetében' [The Assimilation in the History of Hungary] (1942), 'A magyarországi nemzetiségek településtörténete' [The Settlement History of the Nationalities in Hungary]³⁴, 'A középkori magyar falu'[The Hungarian Village in the Middle Ages] (1966), 'A falurendszer kialakulása Magyarországon'[The Making of the Hungarian Village System] (1969)³⁵.

As also mentioned above, in these studies Szabó explored meticulously e.g. the proportion of the Hungarians and other/Slavic people during the 9-th and 10-th centuries (Conquest), the questions of assimilation in the Middle Ages, the devastations of the Turkish occupation, the new settlement in the 18-th century, the developments (migration-immigration) of the 19-th century. These studies (like that of Mályusz's) can be evaluated by two different points of view: by the methodological one they strengthened /underlined the social historical aspect of his orientation. In contrast to Szekfű³⁶, he concentrated much more on the social, population aspects of Hungarian History ("Biography of the Hungarian People"³⁷), applying/leaning vastly/widely on the also mentioned/touched methodological innovations of Mályusz (new, non-written sources, cooperation with allied sciences, linguistics, statistics, geography, ethnography, etc., carrying through the geographical point of view/regional history). From the ideological aspect he represented the "ethnic" nation concept, in contrast to Szekfű's "political" nation theory. That meant for example³⁸ that he analyzed the history of the questions of assimilation in the Hungarian history from its beginnings, stating from the time when the Hungarian "ethnic" character took shape even at the time of the occupation of the Carpathian Basin. Basically this character (in spite of the different stages and phenomena of assimilation, settlement of other nationalities, etc.) didn't change during the later development. Or, if it changed, e.g. in the 18-th century, with the settlements of the Germans and other "foreigner"s in the administrative apparatus, Szabó evaluates it as a detriment to the Hungarians.³⁹

³² Cf. István Szabó, 'Hanyatló jobbágyság a középkor végén'. [Declining Serfdom at The End of the Middle Ages.] *Századok*, 1938(72):10-59.

³³ Cf. István Szabó, *Ugozca megye*. (Budapest: Magyar Tudományos Akadémia, 1937.)

³⁴ Cf. István Szabó: *Népiségtörténeti tanulmányok*. [Studies in Ethnohistory.] Edited by Vilmos Erős. (Budapest: Lucidus Kiadó, 2005.)

³⁵ Cf. István Szabó: *A falurendszer kialakulása Magyarországon (X–XV. század.)* [The Evolution of the Village System in Hungary in the 10-th to the 15-th Century.] (Budapest: Akadémiai Kiadó, 1966); István Szabó: *A középkori magyar falu*. [The Medieval Hungarian Village.] (Budapest: Akadémiai Kiadó, 1969.)

³⁶ Cf. Szekfű, Julius: *Der Staat Ungarn. Eine Geschichtsstudie*. (Stuttgart-Berlin: D.V.A., 1918.)

³⁷ Cf. István Szabó, *A magyarság élet rajza*. [The Biography of Magyarodom.] (Budapest: Magyar Történelmi Társulat 1941.)

³⁸ Cf. Erős, *Asszimiláció és retorika*. [Assimilation and rhetorics.]

³⁹ It should be remarked that after 1945 - like Mályusz - Szabó was not able to pursue further these population history studies. Despite this he published some smaller essays concerning these questions and two major monographs about the settlement history, village system in the Middle Ages. In these masterpieces he deepened in many respects the social historical aspects of his former researches concentrating - beside the historical demography, social-, and settlement history - on the historical-anthropological aspects of the problem. (Feasts, church going, plays/games, housing, furnitures, utensils, plot system etc.) But

The third important direction of the historical researches of Szabó's was the history of the Hungarian peasantry (in fact his main field of research). His best known works/studies in this respect comprise "A magyar parasztság története"[The history of the Hungarian peasantry](1940), (the first synthesis of the[history of this important social class, apart from the book of Acsády), "A jobbágy birtoklása az örökös jobbágyság korában" [The possession of the serfs in the era of second serfdom] (1946), "Tanulmányok a magyar parasztság történetéből" [Studies on the history of the Hungarian peasantry] (1948), "Tanulmányok a parasztság történetéhez a kapitalizmus korában" [Studies about the peasantry in Hungary in the Age of Capitalism"] (I-II. (Edition, 1966).⁴⁰

The best way to analyze the standpoints and evaluate the theories/ideas developed by Szabó in these works seems to be to focus on the heavy debates/controversies that these works evoked. One of them was about (with Gyula Kristó f.e.) the level of the Hungarian culture/civilization during the conquest of the Carpathian Basin and later in the early Middle Ages.⁴¹ In his famous book ("The making of the Village System in Hungary") Szabó held the view that the so called winter settlements/dwellings("téli szállás") can be regarded as the forerunners/antecedents of the Hungarian village system. This interpretation meant at the same time, that the Hungarians could be called/were not nomadic, but half nomadic people in that period, that is to say that they had a much higher level of civilization and standard of living even before the conquest of these territories and further in the Middle Ages than e.g. Otto von Freisingen (who despised them with very harsh words.) described them in his chronicle.

Another important tenet of the thoughts/results of Szabó's was the persuasion, that the situation of the Hungarian serfs (peasants improved during the Middle Ages.⁴² In 1954 e.g. he published a study, launching a discussion with the "Young Turkish" representative/spokesman of the Marxist historiography, György Székely, about the significance and interpretation of the serf laws, issued/enacted in 1951.⁴³ In this study he attempted to prove that these laws mirror not the aggravating/deteriorating situation of the peasantry, because they regulate not the paying but the levying-in obligation of the taxes (nona, ninth) so they represented an obligation/burden not for the peasantry but that of the aristocracy/lords. In the preceding years a heavy/grave epidemic(pests) swept over the country (and Europe, 1948-49) after which aroused a wide shortage of manpower and the feudal lords attempted to attract the serfs to their demesnes with the promise of not levying the taxes for a certain period. This noble gesture could have been afforded

here he let prevailed/materialized ideological, "rhetorical" points of view also in the vein of the earlier heavily attacked "ethno/population" history: he applied the "community" notion of Ferdinand Tönnies, in portraying the relation between the peasants/serfs/tenants and their lords (and other members of the community) which was based on mutuality, aid, and other bounding/linking sides of common work and culture.

⁴⁰ Cf. István Szabó, *A magyar parasztság története. [The History of the Hungarian Peasantry.]* (Budapest: Magyar Szemle Társaság) 1940; István Szabó, *A jobbágy birtoklása az örökös jobbágyság korában.* [The possession of the the serfs in the age of the perpetual serfdom.] (Budapest: MTA, 1947.); István Szabó, *Tanulmányok a magyar parasztság történetéből.* ["Studies about the history of the Hungarian Peasantry] (Budapest: Teleki Pál Tudományos Intézet, 1948.) István Szabó (ed.), *A parasztság Magyarországon a kapitalizmus korában (1849–1914) I–II.* [Studies about the peasantry in Hungary in the Age of Capitalism (1849–1914)] (Budapest: Akadémiai Kiadó, 1965)

⁴¹ Cf. for example Gyula Kristó, 'A honfoglaló magyarok életmódjáról'. [About the Standard of Living of Hungarians in the 10-th Century.] *Századok* (129) 1995/1. 3-62.

⁴² Cf. Szabó, *A magyar parasztság története.* [The History of the Hungarian Peasantry.]

⁴³ Cf. István Szabó, 'Az 1351. évi jobbágy törvények.' [The serf laws from the year 1351.] *Századok*, 1954 (88): 497–527.

by the big proprietors/the lords, and barons the lower nobility could not allow it (they lived exclusively from these revenues) and that's why they enacted the law about the obligation of levying in the nona(ninth) on the Diet of 1351. (According to Szabó - which was already of course a common place among the Hungarian historians - this laws was in tune with the other laws of that Assembly, they represented the interests of the nobility anyway.)

Szabó's third paramount/main debate with the Marxist historians touched(comprised) the so called "second serfdom" theory, which became one of the fundamental tenets of the Marxist historiography after the second world war.⁴⁴ The roots of Szabó's ideas go back to the researches of the famous agricultural history school led by Sándor Domanovszky, the students of whom analyzed/explored mainly the big estate structures in the Hungarian economy and society in the early modern period.⁴⁵ The theoretical and ideological basis of these studies was intended against Gyula Szekfű's "Geistesgeschichte" school, according to which the Hungarian historical evolution is part of Western Europe, and for instance Transylvania was the last bastion of the European culture, renaissance, baroque, enlightenment, Protestantism, etc. in contrast to the culturally underdeveloped/inferior Balcan and East-European territories. Domanovszky and his followers contested this theses and wanted to emphasize (instead of the cultural superiority of the Hungarians) much more the common features/the similarities of historical development of these "small nations".⁴⁶ and they found these parallel motifs in the circumstances of social historical developments/circumstances. Applying/using the notions/models of the German agricultural history they distinguished the terms "Grundherrschaft"(demesne) and "Gutsherrschaft"(estate). According to these theses the East-European (among the Hungarian) development "took a curb"/took a turn from the Western one at about the beginning of the 16-th century, when instead of the "Gutsherrschaft" a new form of big estates, the "Grundherrschaft"(demesne) came to the fore in these territories, which meant/had the consequence, that the nobility and the lords took the agricultural production (instead of making the free peasant hiring plots/freeholder system as in Western Europe, which become a direct forerunner of the modern capitalist system) in their hands. The consequence of this "turn" was the aggravating/deteriorating situation of the peasantry/serfs in these territories, the modernization process came to a standstill, the bourgeoisie remained weak, the phenomena of the so called "refeudalization" process strengthened in a striking degree causing other political problems later, for example the failure of bourgeois revolutions, etc.⁴⁷

After the Second World War and with the communist takeover, the new Marxist historiography capitalized linked together (for his own political/ideological sakes) with the ideas of Lenin about the "Prussian" way of capitalistic development in Eastern Europe (to the East from Elbe) which had the

⁴⁴ Cf. Pál Pach Zsigmond, *Der Bauernaufstand vom Jahre 1514 und die 'zweite Leibeigenschaft'*[The peasant uprising in 1514 and the 'second serfdom'] in Pach, Hungary and the European economy in Early Modern times, (Budapest 1994). pp. 275-301.

⁴⁵ Cf. Erős, *A szellemtörténettől a népiségtörténetig.* [From Spiritual History to Ethnohistory.]

⁴⁶ There are many parallels in this respect with the views of István Hajnal. About him cf. László Lakatos, *Az élet és a formák. Hajnal István történelemszociológiája.* [The Life and its Forms. The Historical Sociology of István Hajnal] (Budapest: Új Mandátum, 1996.)

⁴⁷ Cf. Vilmos Erős, 'Szabó István "ellentörténelme"-i az 1950-es években.' [István Szabó's "counter-histories" in the 1950-s.] *Magyar Szemle*, 2012/3: 98-119.

function to deliver a legitimizing ideology of the Soviet occupation of this region (and justify the political decisions of Yalta dividing Europe, and rendering Eastern Europe to the Soviet interest sphere.)⁴⁸

The starting points for Szabó's studies/ideas (expressed/developed in his studies partly before and during the Second World War, but mainly after 1945, cf. "Studies about the history of the Hungarian peasantry"1948, and "The possession of the serfs..1946)⁴⁹ were the results of the Domanovszky school, that means the similarities of the East European development (instead of stressing the one sided Western/German orientation of the Hungarian culture and history respectively.) But - according to/so Szabó - the Hungarian development neither belongs exclusively to the Eastern phenomena (East European), because – e.g. - the Hungarian serf never was a "holop", a slave who could possess any personal rights. Therefore we cannot speak of a "second serfdom" in Hungary, not even of a first one in the Middle Ages, as we could observe in the former studies (cf. his debate with György Székely) the situation of the peasants were improving even at that period (they could freely move to another place, or flee to market towns, they could even elevate themselves among the ranks of nobility, etc.). This tendency continued after the revolution of Dózsa too⁵⁰, when after a short/certain time the serfs could move freely again and opened many possibilities to improve their situation, they could move to market towns (so become bourgeois), they could become members of the military garrisons ("végvári vitézek"), they could become "Haidu", and in the 18-th century the German peasants could not have been attracted to repopulate these territories with the promise of becoming serfs, deprived of all personal rights and possessions.⁵¹

One of the most significant studies in this respect written by Szabó is "The possession of the serf⁵²"-1946, in which he explores/analyses meticulously/in its details the rights of the peasants for possessing vineyards, for cleared forests, etc.). We have to add to the above mentioned interests his special interest for the phenomena of "hamlet"-s/settlement (tanya), which from the end of 18-th century (with the market towns) became a special feature of the Hungarian development (first of all in the Great Plain), proving that not all of the peasantry belonged to the superiority of lords/nobles so there are many signs/signals of a bourgeois development in Hungary that could have been based on these free peasantry.

All in all - according to Szabó's ideas - the Hungarian development can be placed between Eastern and Western Europe (it is a Central European, transitional phenomenon), and this idea meant at that time a direct and fierce opposition to the official, Marxist ideology, embodied in many respects a "third way/road" theory (conceived between the two World Wars by the famous populist writer Németh László⁵³) and with

⁴⁸ Ibidem.

⁴⁹ Cf. Szabó, *A jobbágy birtoklása az örökös jobbágyság korában*. [The possession of the the serfs in the age of the perpetual serfdom.]; Szabó, *Tanulmányok a magyar parasztság történetéből*. ["Studies about the history of the Hungarian Peasantry"]

⁵⁰ Cf. Szabó, 'Hanyatló jobbágyság a középkor végén'. [Declining Serfdom at The End of the Middle Ages.]

⁵¹ Cf. Szabó, Stefan, 'Das Ungarntum des Tieflandes im 18. Jahrhundert.[The Hungarians of the Great Hungarian Plain in the 18-th century.] *Ungarn*, 1943: 369-375.

⁵² Cf Szabó, *A jobbágy birtoklása az örökös jobbágyság korában*. [The possession of the the serfs in the age of the perpetual serfdom.]

⁵³ Cf. László Németh, *Sorskérdések*. [Fate questions.] (Budapest: Magvető Kiadó 1989.) There are some similarities with the ideas of the Romanian writer and thinker E. Lovinescu. Cf. Ibidem: *A modern román civilizáció története*. [The history of the modern Romanian civilization.] (Palamart Kiadó, 2002.)

this Szabó was, in a certain sense, a forerunner of Jenő Szűcs's well-known theory about the three regions of Europe (written/conceived in the 1980-s.⁵⁴)

In the historical activity of both historians (Mályusz and Szabó) played a very important role in the field of editing sources. Mályusz's work in this respect includes "The Papers of Palatinate Archduke Alexander Leopold, 1170-1795", 1926, "Documents Concerning the Toleration Edict", 1939, "Documents Concerning the Age of Sigismund", 1951-1958? etc. That of István Szabó's are the followings "Ugocsa County", (1937) "Papers for the History of the Hungarian Peasantry", "The decima/tax-rolls of Bács Bodrog " etc.⁵⁵

The first remark that can be noted to the interpretation of these activities is that they secured a very solid scientific basis for their researches and theories, partly from which their high scientific authority(credibility) originates and that's why, for instance, that Mályusz's criticism were taken more seriously against Szekfű, than that of the populists, or of National Romantic School (Jenő Csuday, István R. Kiss , Jenő Zoványi .⁵⁶)

This also resulted in the fact, that, based on the very profound source collections about the "Documents Concerning the Age of Sigismund", the Hungarian scientific audience has quite a Clear picture about Sigismund. In the literature published prior to Mályusz he was portrayed as a non-national ruler not being interested at all in the problem of the Hungarian nation that's why he was either very often despised or neglected. Mályusz - in tune with his source editions - discovers him as the initiator of many modernization tendencies of the country (c.f. his laws concerning/supporting the towns, the institutional system, etc.) and in this way he was placed between Róbert Károly and King Matthias, as the three outstanding pledger of the social and institutional reforms/modernization of Hungary in the second half of the Middle Ages.⁵⁷ (About Matthias such kind of a source collection is not available that's why we don't possess such a solid/balanced picture about him than about Sigismund.)

The other important feature of these source collections is that they represent the social historical approach to the Hungarian history of both historians. From one side they treat questions of social history (with Mályusz's great introduction e.g. in the cases of "The Papers of Palatinate Archduke Alexander Leopold, 1170-1795", 1926, "Documents Concerning the Toleration Edict", 1939, in which he depicts the social and cultural background of the periods analyzed. From the other side these collections contain in many respects so called non written and non-charter sources which reflect much more the social history of wider historical ranks of the society (upper classes, even that of the peasantry in the case of Szabó). These new sources include: settlement names, person-names, letters of peasants, village laws, peasant letters, tax polls, tribunal papers, town maps, municipal papers, etc. In their cases e.g. the cooperation with

⁵⁴ Cf. Jenő Szűcs, *Vázlat Európa három történelmi régiójáról*. [About the three historical regions of Europe.] (Budapest: Magvető, 1983.)

⁵⁵ Cf. *Iratok a türelmi rendelet történetéhez*. [Documents Concerning the Toleration Edict], Edited by Elemér Mályusz. (Budapest: Magyar Protestáns Irodalmi Társaság 1940.) *Sándor Lipót főherceg nádor iratai, 1790-1795*. [The Papers of Palatinate Archduke Alexander Leopold, 1790-1795], (Budapest: Magyar Történelmi Társulat, 1926.) *Zsigmondkori ok levéltár* [Documents Concerning the Age of Sigismund], I. 1387-1399, II/1. 1400-1406, II/2.1407-1410. Edited by Elemér Mályusz. (Budapest: Akadémiai Kiadó, 1951-1958.) István Szabó, *Bács, Bodrog és Csongrád megye dézsmalajstromai 1522-ből*. [Tax rolls from the counties Bács, Bodrog and Csongrád from the year 1522.] (Budapest: Akadémiai Kiadó, 1954.)

⁵⁶ About the National romantic School cf. Vardy, *Modern Hungarian Historiography*.

⁵⁷ Cf. Elemér Mályusz, *Kaiser Sigismund in Ungarn 1387-1437*. (Budapest: Corvina 1990.)

other social sciences (interdisciplinary approach) - Anthropology, Ethnography, Linguistics, Geography, etc. - was necessary in order to explore the ways of their application in solving historical problems.

In this respect a supplementary remark can be made, for example in the case of Szabó these activities included also ideological/"rhetorical" aspects. After the Second World War he initiated (for the centenary of the revolution of 1848-49) a series of source editions pertaining to the history of the Hungarian peasantry.⁵⁸ For this undertaking (supported warmly by another outstanding social historian, István Hajnal⁵⁹) Szabó elaborated the strict "scientific" method, collected the collaborators (Imre Wellmann, Jenő Berlász, Kálmán Guoth⁶⁰, Bálin Ila...etc., - all old fashioned, "bourgeois" historian). But the leadership of this undertaking was taken out from his hands accusing Szabó that in these collections He portrayed the relation between the serfs and their lords as too "patriarchal" too idyllic, neglecting the class war the conflict the fight between these social classes.⁶¹

Finally we have to refer briefly to the political aspects of Mályusz's and Szabó's historical writing. Mályusz's main work in this respect is his pamphlet "The Fugitive Bolsheviks"⁶²; Count Pál Teleki entrusted him (with Szekfű) with completing the text in 1927.⁶³ In the end Mályusz wrote the work alone, because Szekfű took over at that time the editorship of *Magyar Szemle* (Hungarian Review.) C. A. Macartney evaluated the book as a genuine political pamphlet (being full of invectives) because in the book Mályusz despised the most important participants of the revolutions in 1918-19 (already in emigration at that time/in the 20-s) as traitors of the Hungarian nation whose behavior during the revolution and emigration could be explained by their egoistic, anarchic "emigrant/revolutionary" "soul", which was epitomized/embodied first of all by the Jewish (Max Stirner). In this political/ideological respect another stone of astonishment in Mályusz's carrier is his also widely known book entitled "A magyar történettudomány" [The Hungarian Historical Scholarship, 1942], published originally in a form of a series of articles in the extreme right oriented journal of Béla Imrédy, "Egyedül Vagyunk" [We Are Alone], in which our historian claimed the restructuring of the whole historical scholarships (including Academy, Archives, Universities, Hungarian Historical Association, etc.) according to the principles of Volksgeschichte/Ethnohistory. (So he regarded the "ethnohistory" not as one of the many disciplines of history, but - following his intentions - all other disciplines should have been "gleichgeschaltet"(statalized)/reconstructed in order that they pursue exclusively "ethnohistorical" researches.)

⁵⁸ Cf. Erős (Ed.): *A harmadik út felé. Szabó István történész cikkekben és dokumentumokban*. [Towards the Third Way. The Historian István Szabó in articles and documents.]

⁵⁹ Ibidem.

⁶⁰ Already dead at that time.

⁶¹ Erős (ed.): *A harmadik út felé. Szabó István történész cikkekben és dokumentumokban*. [Towards the Third Way. The Historian István Szabó in articles and documents.]

⁶² In German Cf. Elemér Mályusz, *Volkskommissare und Genossen im Auslande*. [The Fugitive Bolsheviks] (Munich, 1931) Cf. Note 67.

⁶³ Cf. István Soós, 'Szekfű Gyula és Mályusz Elemér A vörös emigráció című műve.' [The work of "The Fugitive Bolsheviks" by Gyula Szekfű and Elemér Mályusz.] In: *A negyedik nemzedék és ami utána következik. Szekfű Gyula és a magyar történetírás a 20. század első felében*. [The fourth generation and what follows after. Gyula Szekfű and the Hungarian historiography in the first half of the 20th century. Edited by Gábor Ujváry.] (Budapest: Ráció Kiadó, 2011.) pp. 238–250.

In the introduction of this book Mályusz conceived his ideas about the "political" and "ethnic" nation proposing the breaking/carrying through of the latter, which comprised the purging of the Hungarian nation from the foreigners, its enemies (Jews first of all). Mályusz even claimed the establishment of an institution for the research of the - "negative" - role of the Jews in the Hungarian history, but - to tell the truth - his proposal has not been materialized.)

The political consequences of István Szabó's view about the Hungarian historical development can be grouped around the fierce debates about his books, "Studies about the Hungarian peasantry" and "Debrecen as the capital of the independence war 1848-49".⁶⁴ The later work was heavily disputed/attacked by the Marxist/communist historians, pointing out/alleging that the authors were too comprehensive toward the attitude of the so called "Peace Party" during the revolution in 1949-49, they eulogized the role of the "cíviss" (burghers of Debrecen instead of the working class) they were not tough enough in the fight against the traitor', enemies of the revolutions which caused finally its failure. The most striking reproach/criticism against Szabó was that he developed the idea about Kossuth's peasant policy. Szabó justified him in the case of the policy of "free soil possession" being right at that time instead of the "land distributing" policy of the lefts (Vasvári, Táncsics, etc.). Szabó stuck to the rightness of the policy of Kossuth (approved it), which leaned first of all on the nobility, claiming that the land distributing policy would have alienated the nobility from the goals of the revolution and the independence war. Meanwhile the nobility was the leading force of the rebellions against the Habsburgs even in the previous centuries and the idea of the social/bourgeois reform was not far from them either (in tune with Mályusz's ideas.)⁶⁵

The officials/the representatives of the reigning power didn't dare to touch Szabó personally, although he was persecuted to a certain extent, but two of his collaborators/pupils were sentenced to prison, in Recsk (with the accusation of a planned uprising/uproar against the regime) and many followers/students of him took an active part in the Hungarian Revolution in 1956, when he was elected to a co-president of the revolutionary committee at the university of Debrecen in that October-November days of the uprising against the Stalinist-communist power/system.

Perhaps it is time, at this point, to venture some evaluation of Mályusz's and Szabó's "ethnohistory"'s contribution to Hungarian historiography.

Mályusz's ethnohistory was the revival of the positivist traditions of the nineteenth century.

The legacy of positivism, as his contemporaries were quick to point out, was evident in his preoccupation with the collective, and with the law-like regularities of development, and in his concentration on cultural history. But ethnohistory proposed to give an account of cultural development with full regard to its grounding in economic history and historical geography. Instead of political and administrative units, it took organically related historical and/or geographic regions for its units of analysis, and investigated them at all levels and with all the tools that we have come to associate with micro-history and micro-geography.

⁶⁴ Cf. Szabó (ed.), *A szabadságharc fővárosa, Debrecen, 1849. január-május*. [Debrecen, the capital of independence war in 1849.]

⁶⁵ Cf. Cf. Erős, *A szellemtörténettől a népiségtörténetig*. [From Spiritual History to Ethnohistory.]

So far, so good. The picture is tainted, however, by the fact that the contemporary inspiration of Mályusz's ethnohistory was the *Volkstumskunde* associated with Aubin, Kötzschke, Keyser, and Spamer in the inter-war years. *Volkstumskunde* itself harked back to the nation concept espoused by Herder, Arndt, Fichte and the brothers Grimm, which posited race and ethnicity as the basis of nationhood, and defined national affiliation in terms of a community of descent, language and culture. It was an approach humanist in inspiration, but wide open to racist exploitation. Thus it was that by the turn of the century, the pan-German movement had made it into an ideology of world domination, one serving to substantiate their doctrine of the Germans' racial superiority over the Slavs. Allied with *Ostforschung*, another *fin-de-siecle* intellectual trend, *Volkstumskunde* came to present German history as essentially a crusade to spread German culture (the German "cultural ground") and to extend the area of German settlement (the German "ethnic ground" [Volksboden]), principally toward the east. Empire building and "civilizing"-founding cities, introducing the German legal system, organizing churches-was, on this view, at the very heart of German history, as was the struggle for pan-German unification. (Paradoxically, for all its chauvinism, *Volkstumskunde* proved to be a highly fruitful trend in German historiography. As opposed to the tradition represented by Troeltsch, Meinecke, and Below-concentrating on the state, the history of ideas and "great personalities"*Volkstumskunde* explored collective phenomena and material culture for sources of historical evidence, and encouraged a basically interdisciplinary approach.⁶⁶

Considered purely as a methodology, *Volkstumskunde*, like Mályusz's ethnohistory, would have had the potential for providing relatively impartial, indepth depictions of particular segments of the past. There is, however, no way to disregard their political and ideological thrust. Mályusz's introductory lecture to the second semester of his course on ethnohistory leaves absolutely no doubt as to his explicitly political agenda. His studies of the early 1930s on the new German nationalism bear this out. Post-war Europe, he noted (and would continue to reiterate for another decade), had given rise to a new kind of nationalism, one predicated not on state formations, but on ethnicity.

Perhaps the most problematic aspect of Mályusz's concept of an "ethnic nation" was that it necessitated his precluding the country's Jews from the body politic. "Let us exclude the Jewry from our nation", he wrote; "let us dismiss, in amicable accord, all those who do not, in their heart of hearts, feel that they are thoroughly Hungarian"⁶⁷

⁶⁶ From the latest literature of "Volkstumskunde" cf. Oberkrome, Willi, 1993. *Volksgeschichte: methodische Innovation und völkische Ideologisierung in der deutschen Geschichtswissenschaft 1918-1945*. (Göttingen: Vandenhoeck-Ruprecht, 1993); Fahlbusch, Michael, *Wissenschaft im Dienst der nationalsozialistischen Politik? „Die Volksdeutschen Forschungsgemeinschaften“ von 1931-1945*. (Baden-Baden: Nomos, 1999); Karen Schönwaelder, *The Fascination of Power: Historical Scholarship in Nazi Germany*. *History Workshop Journal*, 1997/43: 133-154; *Volksgeschichten im Europa der Zwischenkriegszeit*. Edited by Hettling, Manfred (Göttingen: Vandenhoeck&Ruprecht, 2003.)

⁶⁷ Mályusz's anti-Semitism goes back to the '20s. In his "Kossuth működésének társadalmi háttere" [The Social Background of Kossuth's Political Activity], he puts the blame for the lull in Hungary's modernization squarely on the Jews, arguing that it was the amoral selfishness of the post-Compromise Jewish immigrants that shattered the two social classes which had the potential of becoming the backbone of a democratic bourgeoisie: the urban middle class, and the middle nobility. He makes the same kind of argument in "A vörös emigráció" [The Fugitive Bolsheviks] a notorious series of articles that appeared in *Napkelet* in 1931. Here, responsibility for the revolutions of 1918-19 is laid at the door of the selfish and anarchic "personality type" identified by Max Stirner as most common among Jews.

Admittedly, Mályusz was not a racist: he did not believe that history was, in essence, the struggle of the various races for *Lebensraum*, with the superior races winning. In fact, in his "A népiség története" of 1931, he criticized German historians for identifying "culture" with German culture. The task facing Hungarian historians, he insisted, was to preserve for posterity what the Magyars had achieved jointly with the Slavs in the way of culture.

Mályusz's (and Szabó's) cultural nationalism was anti-German in several respects. For one thing, his very emphasis on the autonomy of Hungarian culture implied resistance to Hitler's attempts at expansionism. But there was also another side to it. Mályusz's cultural nationalism-as he himself admitted⁶⁸ was meant to lay the groundwork for revisionism. His resolute underscoring of the strength and autonomy of Hungarian culture was meant to provide an alternative to Szekfű's vision of a Hungary whose fortunes were irrevocably tied to that of the Habsburgs⁶⁹. Given the opportunity, Mályusz was suggesting, Hungary would be capable of carrying through a territorial revision on its own. All in all, however, Mályusz might most equitably be judged as having posited-as opposed to Szekfű's concept of *nation as state*-the concept of *nation as culture*.⁷⁰ For all its manifest ideological and political bias, in respect of methodology, ethnohistory anticipated some current approaches to social history.

The lesson might prove as timely as the German revisitation of *Volkstumskunde* has proved to be.



⁶⁸ Cf. his manuscript memorandum to Domanovszky of 1928, in the Manuscript Archives of the Library of the Hungarian Academy of Sciences. The manuscript was published by Vilmos Erős with the title 'Mályusz Elemér feljegyzése egy Magyar Történeti Intézet felállításáról'. [Elemér Mályusz's memorandum about the set up of a Hungarian Historical Institut.] *Történelmi Szemle* 1998/1-2: 113-126.

⁶⁹ I would like to remark at the same time, that not even the school of Szekfű represented an exclusively state centered, political history. The most important difference between them was rather, that *Geistesgeschichte* focused more on the higher, elite culture, meanwhile "ethnohistory" on the material, even everyday life, notwithstanding the political/ideological implications of these differing conceptions. But both were at the same platform, in opposing the narrow political, "historie evenementielle" historiography of the Dualist Age, rather that of National Romantic School.

⁷⁰ On the other hand we should remark, that with the studies of Szabó, in applying much more on the lower ranks of the society, the progressive message, the sociological aspects of the Hungarian "ethnohistory" became much stronger, even paramount which was able to offer a real alternative against the reigning *Geistesgeschichte* orientation between the two world wars. On top of all that, with his striving for applying the so called "third way/third road" theory it became one of the most important opponents of the dominating Marxist/Communist historiography after 1945, respectively 1948. This idea can be evaluated as a "scientific" protest against the Soviet system and occupation with which gains a tantamount ethical/moral character. On the other (methodological) side it strengthens even the comparative aspects of the Hungarian historical understandings, with which epitomizes the overpassing of the one-sided Hungarian-centered view of this scholarships and breaks a way towards a comparative, East-Central European History.

LINGUISTICS

MARCANTONIO, Angela
The Indo-European Language Family: Questions About Its Status
— Introduction —

This paper is the introduction to the volume *The Indo-European Language Family: Questions About Its Status*, edited by Angela Marcantonio and published by the Institute for the Study of Man in 2009, Washington D.C., as the Journal for Indo-European Studies Monograph Series, Monograph 55. (www.jies.org). Republished with permission. – Ed. Journal of Eurasian Studies

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1. 1. The purpose of the present volume is to survey the current state of the Indo-European ('IE') theory in the light of modern linguistic knowledge. Included in the survey is also extra-linguistic evidence, such as recent archaeological, genetic and palaeo-anthropological findings. Its ultimate purpose is to revisit the validity of the various tenets of the theory. In fact, when scholars refer to the "IE theory", they may be referring to one of a number of competing, and sometimes contradictory, models. For example, some regard IE as purely a linguistic classification, whilst others regard it as an attempt to reconstruct a 'real' pre-historical language. To take another example, some scholars hold that the original IE protolanguage was a morphologically complex language, similar to Sanskrit, whilst others argue it was morphologically simple.

The volume intends to achieve its purpose by approaching the IE theory from various perspectives and areas of expertise, thanks to a collection of articles by scholars specialized in IE and historical linguistics, and also by Sanskritists, Dravidianists and archaeologists, whose field of study have been heavily affected by the IE linguistic classification. It is only by bringing together, in a single volume, these various approaches and views about IE that it clearly and explicitly emerges how surprisingly different and, often, even deeply contradictory, these views and approaches may be. Thus, the IE theory is widely accepted despite the fact that opinions differ enormously on what the theory actually comprises. Opinions may clash even as to the very nature and validity of many of the underlying tenets of the theory – whether explicitly stated or quietly assumed. For example, as mentioned, some scholars regard the subject a 'pure theory', which helps to describe correlations between languages, whilst others regard it as a valid means to reconstruct pre-historical facts. Although there seems to be a widely shared assumption that at least a 'hard core' of the correlations among the IE languages are 'compelling' – that is, too striking to be the result of chance – there may be deep divergences on how to interpret these correlations, as well as on other, less central but equally important aspects of the theory. In other words, contrary to what one would expect, the wide acceptance of the IE linguistic classification does not appear to be accompanied by a parallel

acceptance of a coherent and equally agreed set of tenets, a coherent common denominator, consisting of what one could call the 'hard linguistic evidence' and the 'fundamental principles' upon which the theory, supposedly, is based. This being the case, it is appropriate to dedicate a volume to the precise task of addressing the fundamental question of why this should be the case. This, ultimately, amounts to the task of re-assessing the founding principles of the theory. In particular, one might reasonably ask the following questions:

1. How is it possible that such widely differing and often contradictory views are still unresolved today? Is this a consequence of the fact that the adopted methods of analysis are not rigorous enough, or alternatively is it the linguistic and extra-linguistic evidence that is problematic – for example, is it ambiguous, insufficient, or simply highly 'malleable'?

2. Is the IE theory a 'scientific' theory, that is, has it been established through "methods of analysis which produce results that are subject to invalidation"¹? If so, what precisely is the evidence counter to the model? This evidence does exist, although it is usually minimized, in various ways, but: is it really so insignificant?

3. As an alternative, some scholars believe that the theory cannot be invalidated and therefore is not a 'scientific' theory; nevertheless its validity is simply 'compelling'. What is the basis for this claim?

The points of views and perspectives presented in this volume, although might have surfaced in professional publications before, have hardly been dealt with and confronted with one another within a single volume, despite being inextricably interdependent. As a consequence, the questions raised in the points (1)-(3) above have hardly been addressed in a targeted and systematic way. For example, there are plenty of publications dealing with the issue of the strengths and weaknesses of the methods of historical linguistics (see Birnbaum (1977); Jones (1993); Fox (1995); Aikhenvald & Dixon (eds, 2001); Nichols (1992); Lass (1997), etc., just to pick up some names at random). However, these publications hardly ever ponder on whether the acknowledged weaknesses may have a negative impact on the IE theory and, if yes, to what extent. Similarly, there are many publications revolving around "how real(ist) are reconstructions" (Lass 1993), but their scope hardly ever extends to encompass the consequent issue of how to best interpret the IE reconstructed, comparative corpus. On the other hand, there are plenty of publications which (ignoring the debates mentioned above), revolve only around the question of the whereabouts of the (assumed) IE proto-community. Finally, textbooks of IE linguistics (and, often, specialist publications too), hardly ever mention any of these ongoing debates. It is true to say that the purpose and scope of a textbook is not that of reporting controversies, but just that of presenting and illustrating the received wisdom. Nevertheless, the fact remains that textbooks typically present a highly idealized, monolithic picture of IE: a paradigmatic, problem-free language family, where everything works (especially sound laws, lexical and morphological correspondences), where there are hardly any contradictions or ambiguities in the linguistic or extra-linguistic evidence, or even any significant divergence of views among scholars. This is even more surprising since this idealized picture is in stark contrast with the messy reality – in terms of

¹ I have used here the words by H. Andersen, personal communication; 2006.

variation, high level of exception, contradictory evidence, etc. – found in practically all the other language families of the world, as well, admittedly, within branches of IE (such as the Romance and Germanic languages, or the Balto-Slavic continuum, etc.). This in turn has led several scholars (such as Grace 1990 & 1996) to come up with the rather ‘aberrant’ idea that there must exist in the world two basic types of language families: the ‘the exemplary ones’ (IE and just few others) and the ‘aberrant’ ones (all the rest).

There are, of course, textbooks that present a more realistic picture of the state of the IE family, by pointing out the many exceptions to the stated laws, the difficulties encountered in the process of phonological or morphological reconstruction, or the poor quality and /or quantity of the evidence in support of otherwise widely accepted theories. One may compare, for example, Szemerényi’s (1973 & 1996:122 ff.) and Gusmani’s (1979) account of the slippery evidence on which the laryngeal theory is typically based on the Hittite side, or Sihler’s (1995:144 ff.) account of the factual and methodological obscurities found in Verner’s Law, one of the most revered IE sound laws. However, in most cases, the dissident opinions or the problematic data and analyses – if mentioned at all – are typically minimized through various means (for example, by being reported in footnotes and / or by the absence of the due references), so that it is difficult to evaluate their potential impact on the validity of the theory, or simply just to acknowledge them. It could be objected that no theory can be or ever has been invalidated by the discovery of one or more pieces of evidence counter to the model. This is true; however, if, at some stage in the history of a theory the amount of evidence counter to the model reaches what is usually called a ‘point of critical mass’, then a revision of the theory might be in order, whether to modify or update its tenets, or, if necessary, to reject it altogether. In other words, minimizing, re-interpreting or adjusting any evidence found to be inconsistent with the model might be misleading, and therefore not desirable. In fact, scholars identifying problems in their area of research may wrongly assume that the matter has been settled beyond doubt in other areas of study, and may therefore in turn decide (understandably) to minimize or even ignore his / her own item(s) of counter-evidence, in this way, unwillingly, and maybe wrongly, contributing to reinforce the validity of the theory in question.

1. 2. The scholars contributing to this volume will argue in favor or against some of the major tenets embedded in the variegated IE theory on the basis of both specific corpora of data and / or methodological considerations. We believe that a fair and healthy debate may be of help in attempting to sort out, if necessary, what appear to be proper ‘facts’ and valid analyses from what appear to be instead (questionable) interpretations, (unfounded) speculations, or even sheer myths. If it turns out from this debate that the different and at times contradictory views about IE can, after all, be reconciled, then the IE theory will gain in rigor and therefore in credibility. If, on the contrary, it turns out that these views cannot be reconciled, then, maybe, some or all of the tenets and assumptions that lie at the foundation of the theory will have to be revisited, or called into question.

This volume sets out to bring together points of view and analyses that deal with issues within the following, main pillars of IE studies: a) the sound laws and the reconstructed phonology and lexicon; b) the reconstructed grammar; c) the traditional family tree model and related concept of ‘proto-language’ *vs* other models of language formation and diffusion; c) the thesis that the Sanskrit language /culture did not originate in India, but represents an intrusion from the west; d) the thesis that Sanskrit is (arguably) the oldest language within IE, and therefore one of the most, if not the most important one for the purpose of

reconstruction. In addition, some attention will also be dedicated to the state of the archaeological, palaeo-anthropological and (to a lesser extent) genetic research, to verify whether or not the 'extra-linguistic evidence' lends support to the traditional model (although this is a linguistic volume and its focus remains the linguistic analysis). As to the specific areas of debate, we will concentrate on the following: 1) the strengths and weaknesses of the comparative method, and, therefore, the reliability of the conventionally established sound laws and reconstructions; 2) the role of morphology in assessing and reconstructing language families in general and the IE family, with particular attention being paid to the IE verbal system; 3) the debate of whether it is possible to tell apart genetic from areal correlations; 4A) the 'conventionalist' vs 'realist' approach to reconstruction; 4B) the issues of the IE homeland and related migrations of the (assumed) IE proto-community.

2. The comparative method and the sound laws

2.1. The strengths and weaknesses of the comparative method

The debate revolving around the issue of the strengths and weaknesses of the comparative method is an intense, long standing debate. In fact, establishing regular sound correspondences is considered by several (many / most?) scholars to be a crucial part of the process of establishing language families (see for example Campbell (1998:315)). However, as mentioned, this debate has hardly ever been associated with a targeted, extensive investigation of the possible impact the weaknesses of the comparative method may have on the validity of IE as a linguistic classification. In particular, the long standing "*Lautgesetz* controversy" (for which see Wilbur 1977) subsided without resolution, and despite its recent resurfacing in publications dealing with several linguistic areas / families in the world (see for example Ross & Durie (eds, 1996); Blust (1996)², Aikhenvald & Dixon (eds, 2001), etc.), it is rarely referred to in textbooks of IE. In fact, these usually assume, whether explicitly or implicitly, that the 'regularity principle' and the related family tree model have been established in IE beyond doubt, through the support of an extensive amount of data derived from the various IE languages. However, this is not necessarily the case (as illustrated in this volume in the chapter by Andersen and Marcantonio). As a consequence, the reader, including general linguists or even experts in historical linguistics who are not acquainted with the details of IE, may be excused if they are confused as to the actual 'quality' and 'quantity' of the phonological / lexical correspondences conventionally established for IE. Indeed, within IE too irregularity and exceptions do occur – it would be odd otherwise. However, the vital questions are:

² See the following quote by Blust (1996:137) with regard to the Austronesian languages of island Southeast Asia: "Pandemic irregularity resist a plausible explanation through borrowing, analogy or other mechanisms of secondary change and hence are particularly difficult to reconcile with the Neo-grammarians position that all apparent diachronic rule violations are due to secondary factors which interfere with the perfect regularity of primary change".

A) How pervasive is this irregularity?

B) Is it really true that the encountered irregularities can, in most cases, be justified through 'genuine' linguistic processes, that is, without stretching the explanatory system up to the point of dangerous 'circularity', by 'appending' *ad-hoc* justifications?

The reader who would expect an answer to these questions which is coherent, unanimous, and, most importantly, decisive – one way or the other – might be disappointed. To show that this is the case, here is a list of the most common justifications put forward to answer question (A):

1. Only the ordinary nouns in IE, particularly those referring to objects and concepts of everyday life, display a high degree of irregularity (much higher in any case than verbal roots), but this is only because they belong to the lower level of speech, the lower level stratum of the IE population (Meillet 1934: 396 ff.; Benveniste 1935:175 ff.).

2. The irregularities are only, in most cases apparent, in the sense that linguists have not yet found the appropriate explanation to account for them.

3. There are some / many / plenty of irregularities (according to interpretations), but there is nothing to be surprised about. We know that sound changes do not proceed so regularly after all, but this does not have a negative impact on the validity of IE, whose establishment, in fact, has *not* been based (only) on the phonological / lexical correlations, but on the morphological ones. Such a view is embraced, among others, by Harrison (2003:214 ff), Greenberg (1987:18; see also Croft 2005) and, in this volume, by Kazanas. For example, Harrison (2003: 217) states that: "If one can *prove* that even one single cognate pair holds over two languages, one has proven those languages genetically related". The (implicit) claim here appears to be that proper lexical 'correspondences' are generally hard, if not impossible to attain. Therefore, linguists must come to terms with the fact that also within IE (like within other language families or across macro-families) the correlations among the assumed cognates are not, in the main, as 'regular and systematic' as generally claimed.

4. Sound changes do, in the main, proceed regularly, but the encountered irregularities are a natural effect of the great antiquity of the IE family, although its precise degree of antiquity is impossible to assess (see in this regard the chapters by Kazanas, Bryant and Schmitt in this volume).

One may further observe that sound changes (such as phonemic mergers) may not be necessarily that useful for the purpose of identifying innovations, and therefore the internal sub-grouping of families. Most of these sound changes are in fact so natural that they can be easily repeated in different lines of descent³. Besides, these natural sound changes may independently occur in totally different language families (see

³ According to Ringe et al. (2002), the same would apply to morphology, where, for example, often it is impossible to discover which inflectional markers are ancestral and which may represent innovation.

for example Ringe et al. (2002:63 ff.); see also Blenvins (2004) for a detailed account of word-wild frequent sound patterns and sound changes⁴).

As to the answer to question (B), we are not aware of any research carried out with the specific purpose of ascertaining whether or not the circularity issue has had a negative impact on the soundness of IE. In the year 1998 Morpurgo Davies (1998: 254) wrote:

A final agreement about the nature and validity of sound-laws was never reached. It was generally accepted (by the neogrammarians and everyone else) that testing any sound law against the data was bound to reveal a number of exceptions; in other words, there could not be an immediate empirical demonstration of the regularity principle [...]. The neogrammarians did of course maintain that all the exceptions could be explained away by re-defining the law, or by identifying a different starting-point, or by recognizing the interference with analogical process, but they were immediately accused of circularity [...]. We can say that the sound laws have no exceptions only because when we find an exception we eliminate it saying that there has been analogical interference. On the other hand, we also say that the only way in which we can prove that a form [...] is analogical is by pointing out that otherwise it would be an exception to the sound laws. There seemed no obvious counter-objection and the problem remained open [...]. Meanwhile, however, the practicing historical or comparative linguist continued to, or began to, operate in terms of sound laws not too remote from those pleaded by the neo-grammarians.

On the issue of the reliability (or otherwise) of sound laws, compare also the following statement made by Clackson (2007: 60-61) with specific reference to the laryngeal theory: "The comparative method does not rely on absolute regularity, and the PIE laryngeals may provide an example of where reconstruction is possible without the assumption of rigid sound-laws". This statement begs the question of why, when and where, and on the basis of which criteria, scholars may – or may not – assume the existence of "rigid sound-laws".

2. 2. The circularity issue

If – as it seems – the circularity issue has not been solved (yet?), scholars could attempt to set up some sort of qualitative and / or quantitative constraint to the number of the defining parameters a given law may consist of. In practice, however, as far as we are aware, there has never been any such attempt. On the contrary, in the every-day, painstaking practice of establishing sound laws and correspondences, any mismatch in the evidence (ambiguity or absence of the expected outcome, exceptions, etc.) can always "be explained away" through a range of procedures, a range of 'adjustable parameters', to be added to the original definition of the law. In other words, instead of casting doubts on the validity of an assumed law (and, if necessary, dropping it) when faced with exceptions and difficulties, typically the practitioner tries

⁴ As pointed out by Hock (1986:633) and Belardi (2002:307), the neo-grammarians worked on sound changes only *ex post facto*; therefore they were unable to observe particular changes in progress; see Kazanas in this volume.

to 'rescue' the stated law, even at a cost of making recourse to a (virtually unlimited) number of (often *ad-hoc*) adjustments, such as:

1. re-defining the law
2. identifying a different starting-point of the law
3. assuming borrowing, from other languages, or 'transitional' dialects, or even from unknown, extinct languages /dialects⁵
4. assuming analogical processes
5. re-arranging the stated sequence of rules in a different order
6. postulating a (/another) laryngeal segment
7. stating that the mismatches in the expected outcomes of the law are not significant for calling into question a theory as well established as IE.

The obvious consequence of this circularity deeply embedded in the comparative method is that the explanatory system runs into the risk of becoming so powerful, so flexible, that it can be stretched to match almost any data, in this way making it impossible to compare the results it yields against the predictions of the model. In other words, although each single 'adjustable parameter' as listed above may in itself reflect a plausible, genuine linguistic process, the overall cumulative effect of many adjustable parameters added to the definition of a given law may endanger the 'cumulative effect', the 'statistical significance' any established 'law', or even 'tendency', should display to deserve these names. This is an issue that has hardly ever been properly and systematically addressed (as far as we know). Although the supposedly rigorous, 'scientific' nature of the comparative method has often been called into question, and more objective quantitative methods of analysis have been at times adopted within historical linguistics, the statistical significance of the IE comparative corpus itself (both the phonological (/lexical) corpus and the morphological one) has never been tested. For example, Collinge (1985) raises the general issue of the lack of 'cumulative effect' in most of the conventionally established 'Laws of Indo-European', but does not provide any detailed, systematic (qualitative and / or quantitative) analysis of the IE comparative corpus on the whole. Ringe (1992, 1995, 1998a, 1999a) and Ringe et al. (2002) apply their statistical analyses basically to the correlations between IE and other, supposedly related language families, in order to prove that macro-families lack statistical significance. Other authors, such as McMahon & McMahon (2003 & 2005), or McMahon (2005), apply quantitative analyses to estimate the 'correct phylogeny' of the IE family, whilst also emphasizing the need of integrating the traditional methods of historical linguistics with quantitative methods (at this regard see the chapter by Drinka, who also discusses the pros and cons of the latest cladistic models). Thus, these Authors have used the IE family, whose validity is taken for granted, basically as a 'control case' for various kinds of statistical tests within historical linguistics, but

⁵ For example, Sihler (1995:157) says: "Much effort has been devoted to trying to discover the conditions under which $*k^w > f$ might be a regular Germanic development, but without success. The probable explanation is that these forms are dialect borrowings from an otherwise un-attested *P*-dialects of Germanic".

have not tested the statistical significance of the IE comparative corpus itself. See in this regard Brady & Marcantonio (2003) and Marcantonio (2002/5). See also Marcantonio in this volume, who argues that the great majority of the conventionally stated IE sound laws lack statistical significance and that, therefore, most of the conventionally established correspondences (within the LIV corpus) are simply similarities, most probably in the given sense of ‘chance resemblances’.

2. 3. Borrowing *vs* inheritance

In addition to the issues listed above, there is also to consider the possibility that the established ‘cognates’ – be they ‘similarities’ or proper ‘correspondences’ – may be due to the common processes of borrowing, diffusion, convergence, or even chance resemblances. As is known, borrowed words tend to integrate into the sound pattern of the receiving language, as well as undergoing the same (more or less regular) changes that inherited words would undergo. Thus, the identification of borrowed elements on the basis of internal, linguistic clues only might not always be easy. Therefore, sound correspondences, whilst fundamental to most approaches in assessing language families, “can be misused” (to use Campbell’s (1998: 315) words). The difficulties (and related long standing debate) of telling apart genetic *vs* areal correlations are clearly illustrated in this volume in the chapter by Andersen, who closely examines certain pathways of development of the Balto-Slavic accentual system.

It is also worth mentioning in this context the well known fact that several semantic fields within the IE basic lexicon (including the terms designating ‘extended family’ and ‘the family of the woman / wife’), in addition to being mainly irregular, typically lack a wide distribution across the IE area, being often confined to just two or three contiguous languages (as pointed out by Schmidt (1872) and Meillet [1934]). In contrast, the cognate terms for ‘mother’, ‘father’, ‘brother’, ‘daughter’, etc., display a much wider distribution, and a higher degree of regularity. This factor has correctly raised the suspicion that processes of loss (and consequent replacement) of original words, or even processes of chance resemblance, may have been involved in this area. The issue of the wide *vs* restricted distribution and the (degree of) irregularity of many basic cognates within IE is interpreted differently by different scholars (see for example discussion at point (1) in paragraph 2.1. above). On this topic the reader may compare the contrasting views held by Kazanas and Marcantonio in this volume. Kazanas⁶ argues that a great deal of fundamental cognate terms have actually been individuated across the IE languages, more than enough to warrant their inherited nature – even if they are not, in the main, regular and systematic correspondences. Marcantonio, on the other hand, argues that there is a great deal of chance resemblances involved. Finally, one could also assess these data and related interpretation in the light of recent research in the field of historical, contact linguistics, for which see Matras (2002) and Matras & Bakker (eds, 2003). The results of this research appear to support the ‘loss’ interpretation. In fact, Matras (2002) investigates two typical mixed languages, Romani and Domari (descendent of Central Indo-Aryan languages) and shows how patterns of original words are typically retained, and are therefore easily identifiable even within a context of extensive borrowing. For example, the system of kinship terms of Romani generally retains original, Indo-Aryan terms that refer to “the first-level kin of the same generation” (such as the

⁶ Kazanas reports a detailed and exhaustive list of widely diffused cognates, which include, for example, the term for ‘blood’, ‘daughter’, ‘arm’, ‘daughter-in-law’, ‘house’, ‘metal’, ‘widow’, etc.

terms for 'sister' and 'brother'), or the "first-level and lateral kin one generation older" (such as the terms for 'father', 'mother', 'uncle', etc.), or the "first level kin one generation younger" (such as the terms for 'son' and 'daughter'). In contrast, the terms for less close relatives tend to be more easily borrowed (such as the terms designating 'cousins', 'nephews', 'grandchildren', etc.). This is, by and large, the situation encountered within IE. Thus, one could argue that, over time, the original IE vocabulary dwindled, whereby 'more basic kinship terms' – so to speak – have been retained, whilst the 'less basic' ones (and others) have been replaced by borrowed terms (compare discussion below and Matras in this volume for the results of historical, contact linguistics applied to morphology).

2. 4. Is the phonological evidence malleable?

Judging from the debates outlined above, one could certainly argue that the conventional, phonological / lexical evidence on which the IE theory is based, to a closer scrutiny, appears to be rather 'malleable' – it is certainly not as decisive (one way or the other) as generally claimed. As a matter of fact, it is always possible to find a plausible, although not always testable, justification to any intervening piece of evidence counter to a stated rule or tendency. Similarly, it is also always possible to provide at least two equally plausible, equally well founded explanations for any given linguistic phenomenon. This is also the case within non-controversial areas of IE linguistics, such as the postulation of the so-called 'Indo-Iranian branch / unity'. One can in fact compare the different interpretations given to this unity – (also) on the basis of phonological /lexical evidence – in the relevant literature, for which see Lazzeroni (1998), Sims-Williams (1998) and Schmitt (1987). Compare also Schmitt in this volume, who argues that the data from both Vedic and Sanskrit are not with absolute necessity genuinely antique and that, therefore, Old Indo-Aryan is not as close to PIE as still believed by some scholars (see below).

At this point one could object that all these methodological and factual difficulties do not, after all, matter, even if they *did* impact negatively on the validity of the conventionally established sound laws (and related correspondences and reconstructions). This is because, as mentioned above, the lexicon is often considered to be the level of language less (or not at all?) relevant for the purpose of assessing genetic relationships. This would be sound and acceptable if there were indeed a consensus among Indo-Europeanists as well as comparativists in general on the principle that it is morphology the level of language (mostly?) relevant in this context. But, do we find that consensus? This brings us to the second, specific area of debate dealt with in this volume.

3. The role of morphology

3.1. Is morphology the most reliable indicator of genetic inheritance?

Since the very times of the establishment of IE the prevailing opinion appears to have been that grammar can offer the most reliable evidence for assessing genetic relatedness. Grammar has typically been considered to be a rather stable level of language and, often, totally resistant to borrowing – in contrast to the volatility of the lexicon. These properties have made morphological correlations quite popular among many historical linguists, even if it has always been (more or less openly) recognized that

it is not clear what the criteria are, if any, on the basis of which to identify and evaluate morphological similarities. In fact, not even the regularity principle is expected to consistently operate at this level, due to the overwhelming interference of the analogical principle. In addition, there has never been (to our knowledge) any systematic attempt to define a possible measure, a 'unit of similarity' (in form and / or function) – as it were – to be applied in the practice of comparing morphemes. This measure of similarity would work as a common denominator against which to evaluate the at times vaguely similar, at times very similar and at times identical morphemes occurring among (most / some) IE languages. Thus, the problem is that the morphological correlations are typically observed and established by intuitive, visual inspection (often by single scholars), whereby considerable latitude may be allowed when it comes to phonological forms as well as to similarity of functions. For the morphological (and morpho-phonological) correlations to be rigorous, to be proper 'correspondences', one should certainly require regular phonological correspondences between morphs which also indicate similar (but how similar?) meanings and /or functions –condition which is hardly ever met. Indeed, one often reads in the literature that the grammatical correlations within IE are (still nowadays) simply and purely 'obvious' to the 'naked eyes' of the trained philologist (see Nichols 1996a), exactly as they appeared to the first scholars who dealt with them a couple of centuries ago.

3. 2. The degree of 'borrowability' of grammar

In recent years a mounting body of evidence has been accumulating according to which not only grammar is found to be 'borrowable', but, given the appropriate historical and social context, it may rate quite high on the scale of borrowability. It could therefore be difficult to determine whether shared grammatical innovations are the result of genetic inheritance or of areal convergence. For example, in connection with some Australian linguistic areas the following question has often been posed: is grammar borrowable to such an extent that historical reconstruction becomes impossible? (See Dench (2001); Austin (1989); Austin (ed. 1990) & Dixon [2002]).

The issue of the (degree of) borrowability of grammar is addressed in this volume by Matras, through his detailed investigation of the grammatical borrowing that has taken place in the 'mixed' languages: Romani and Domari (see also discussion above (par. 2.3.) with regard to their lexical borrowing). In particular, the Author shows that in these languages grammatical borrowing has been extensive, occurring even in the domain of bound morphology. Nevertheless, genetically inherited morphological elements and patterns *can* be identified and kept distinct from the borrowed ones. In other words, even in the context of extreme contact and mixing, borrowing does not appear to be random but "tends to be structured in a hierarchical manner, and so it is at least to some extent predictable". This in turn means that "some components of grammar offer more reliable indicators than others about shared historical-genetic inheritance. For example, Romani shows "unique morphological conservatism in its nominal and verbal inflection", as illustrated by the fact that it preserves to a considerable degree the original endings of the Old /Middle Indo-Aryan present tense conjugation⁷. On the other hand, Matras also presents evidence that nominal case endings and verbal inflections, if borrowed at all, tend to be borrowed "wholesale" (see also

⁷ Compare for the singular: OIA *-āmi* > Romani *-av*; OIA *-asi* > Romani *-es*; OIA *-ati* > Romani *-el*. For the plural: 1st OIA *-āmas* > possibly Romani *-as*; for the 3rd person OIA *-anti* > Romani *-en*, spreading also to the 2nd person by analogy (Matras 2002: 43).

Levin (1995), who presents evidence of this sort in the borrowing of verbal and nominal paradigms from IE into Semitic). These findings can certainly assist linguists in the delicate task of identifying original morphemes / morphological paradigms and reconstructing the morphological structure of proto-languages in general, and of the IE proto-language in particular. As a matter of fact, within IE studies there is still an open question regarding the nature of the original morphological structure of PIE: was PIE rich in morphology (as is the case, mainly, of Greek and Indo-Iranian), which has then been ‘reduced’ or ‘lost’ in the other languages, or was it rather poor in morphology, in which case the complex morphology of Indo-Iranian and Greek is the result of parallel, shared innovations, rather than of genetic inheritance? This issue is dealt with in this volume by Drinka, Kazanas and Di Giovine, who all hold different, at times contradictory views on the topic, as discussed in more details below.

This already intricate debate is further complicated by the connected issue of the ‘Indo-Hittite theory’ as proposed by Sturtevant (1933) – although Sturtevant’s position as such has now been abandoned. In fact, although scholars appear to agree on the fact that the Anatolian languages have a special status among the IE languages, they disagree on how to interpret and justify this special status, that is, the numerous acknowledged differences existing between the Anatolian languages and all the rest of the IE languages, (particularly) at the morphological level⁸. Regarding this issue, see Carruba (1997 & 2001a⁹), Luraghi (1998); Zeilfelder (1994/2001); Marazzi et al. (1990), Drews (ed. 2001)¹⁰, Clackson (2007: 129 ff.), etc.). For a possible interpretation of the status of Hittite within the family compare the chapter by Di Giovine, who addresses the issue of the verbal morphology, and Carruba, who addresses the peculiarities of the Hittite *Ablaut*.

Still on the issue of the ‘borrowability’ of grammar, it has been claimed at times that the IE morphological correlations are, on the whole, similar enough to be considered valid correlations but different enough so as not to raise the suspicion that borrowing might have been involved. However, certain IE grammatical forms typically reported in textbooks as ‘obvious’ examples of genetic inheritance – such as the paradigm of the verb ‘to be’, or ‘to bear, carry’ – are so similar, if not in some forms identical across the area, that the suspicion of borrowing may indeed arise. In fact, one would normally expect much more divergence from a long process of inheritance and development. These observations have been made, for example, by Croft (2005: XIX) and Greenberg (2005), and are discussed in more details in the next paragraph. In addition to this, one should take into consideration the numerous morphological correlations which supposedly connect the IE family with other contiguous, but different language families, as argued for by the supporters of the so-called macro-families (see for example Greenberg (1987

⁸ there are basically two contrasting theories: a) the ‘Schwundhypothese’, according to which the features and categories present in most/all IE languages but missing in Anatolian (feminine gender, aorist, dual, etc.), have been lost; b) the “Herkunftshypothese”, according to which those features and categories absent from Anatolian have been formed only after Anatolian split off from the rest of the family, in which case Anatolian would be particularly archaic. Either of these analyses is void of problems; see Zeilfelder (1994 / 2001: 9-20).

⁹ Carruba (2001a:6) states that there is still one knot to untangle: the Indo-Hittite knot, although this should take place in ‘a more modern, less polemical manner’, than in the twenties or thirties.

¹⁰ Among the various articles contained in this volume see the one by Lehrman, who observes the following (Lehrman 2001:107): “What I find in 2000 is that many scholars have accepted the Indo-Hittite theory in some form – either in name as well as in essence, like Don Ringe (e.g., Ringe 1998b) and Anna Morpurgo Davies (private communication, January 28, 2000, Berkeley, Calif.), or in essence if not in name, as Craig Melchert has done [(2001)]”.

& 2005) and Comrie [1998]). Furthermore, it has recently been observed that several grammatical endings (case and personal endings), at first believed to be exclusive to IE or the Eurasiatic macro-family, actually occur also in non-contiguous areas, such the Americas, so that both the assumption of 'long distance' genetic inheritance and that of 'borrowing' have to be excluded. In other words, in addition to the traditional dichotomy: 'inherited *vs* borrowed', there is now evidence that there may also be a third, (a priori) equally possible explanation for the correlations individuated among languages at the morphological level (as well as at the phonological / lexical level): 'chance similarities', as clearly illustrated in Campbell (1995 and 2003a)¹¹.

Finally, it is worth reflecting on the following remarks made by Harrison (2003:223 ff.), who sums up some of the criticisms raised against the privileged role often attributed to the grammatical correlations (see also Koch (1996: 218-20) & (2001) for similar concerns):

Grammatical objects fare poorly as evidence for genetic relatedness [...]. There can be no regularity assumption for grammatical objects to provide a measure of similarity, because grammatical objects are unique. [...]. Cases like those of morphological person-number paradigms are of particular interest because, although not universal in any absolute sense [...] linguists are surprised neither by their occurrence nor by their non-occurrence, in the verb or noun morphology, not even in closely related languages, or by the occurrence of similar paradigms in non-related languages, or *vice versa*

3. 3. Is the morphological evidence malleable?

At this point one could object that the risk of reconstructing false matches within IE grammar is rather low, since one can rely on a wealth of shared (inflectional and derivational) morphology in a great variety of areas. Furthermore, although much of the grammatical evidence put forward by traditional IE studies is certainly rather intuitive and subjective, as it happens, this evidence turns out to fall within the range of what Nichols (1996a: 49 & 64) calls "diagnostic evidence":

Traditionally linguistic kinship was identified on the basis of diagnostic evidence which is grammatical and combines structural paradigmaticity [...] and syntagmaticity with concrete morphological forms. The Indo-Europeanists' *intuitive feel* for what was diagnostic evidence of relatedness corresponds to a computable threshold of probability of occurrence [...]. A grouping can be regarded as established by the comparative method if and only if it rests on individual-identifying evidence

The idea is that one can compute the probability of occurrence in the languages of the world of certain features, certain patterns, such as consonant sequences (in the dimension of syntagmaticity), or the co-occurrence of some grammatical categories and their morphological indicators (in the dimension of

¹¹ Campbell (2003a:11) states that: "Highly recommended though such grammatical evidence is, caution in its interpretation is necessary. There are impressive cases of apparent, idiosyncratic grammatical correspondences which, in fact, have non-genetic explanation (accident or borrowing)".

paradigmaticity), etc., whereby the Author assumes that a probability of occurrence of a given phenomenon of 1 in 100.000 is “individual-identifying”. Within IE one finds several, arguably a statistically significant number of instances of individual-identifying diagnostic evidence. The following data, as proposed by Nichols herself (1996a: 47), are among the most quoted ones for the purpose of this discussion (notice that to the Latin and Greek endings reported in this table at least the corresponding Sanskrit endings should be added to complete and strengthen the picture: M. *-as, -am*; F. *-ā, -ām*; N. *-am, -a*):

Table (I)

	<i>Masculine</i>	<i>Feminine</i>	<i>Neuter</i>
<u>Latin</u>			
Nominative	<i>-us</i>	<i>-a</i>	<i>-um</i>
Accusative	<i>-um</i>	<i>-am</i>	<i>-um</i>
<u>Greek</u>			
Nominative	<i>-os</i>	<i>(*)-ā</i>	<i>-on</i>
Accusative	<i>-on</i>	<i>(*)-ān</i>	<i>-on</i>

Here there are two dimensions of paradigmaticity: case and gender on the one hand, with ‘cognates’ endings combined together with identical distribution on the other. These dimensions of paradigmaticity are also quite productive: there are for example numerous items in the masculine declension: Skt. *-as vs* Gk. *-os vs* Lat. *-us* (and not just nouns but also adjectives and participles: compare Lat. *amatus*, etc.). The same holds true, for example, for the feminine declension, although other classes of nouns may be less numerous, coming down to 5-6 items only. In addition, in many cases these morphological paradigms also occur in connection with lexical items (verbs and nouns) that are typically quite similar, and are in fact considered to be cognates (see for example the various IE terms and related paradigms for ‘name’: Skt. *nāma-*, Lat. *nōmen*¹² etc., as well as the various cognates and paradigms of the verb ‘to be’ and ‘to bear, carry’, etc.). Several, striking examples of diagnostic evidence of this type are reported and discussed in this volume by Kazanas, who, in line with mainstream doctrine, argues that these highly complex, and at times unique patterns of lexical and morphological correspondences cannot be explained either through chance or through borrowing. This is certainly a plausible model of explanation; however, alternative explanations are also possible. First of all (as mentioned in the previous paragraph) there is plenty of evidence that wholesale (nominal and verbal) paradigms of the type under discussion *can* also be the effect of borrowing, as shown by Matras’ contribution to this volume. Second, not all the IE languages do possess those rich, (more or less) consistent, paradigmatic morphological systems we would need to establish a

¹² Compare for, example, the following case endings: Locative *nām(a)ni* in Sanskrit and *nōmine* in Latin; for fuller paradigms and other examples of this type compare Kazanas in this volume (par. 3c.).

wealth of 'individual-identifying' grammatical evidence. In fact, this kind of evidence is found mainly among Indo-Iranian and Greek (the so-called *Greco-Aryan* model), and, to a much lesser extent, in Latin, Germanic, Balto-Slavic, Armenian and Celtic. As to Hittite¹³ and the other Anatolian languages, there appears to be consensus on the fact that the "Greco-Aryan model does not work well as an explanation for the Anatolian verb" (to use Clackson's (2007:115) words). As mentioned above, many scholars explain this situation by assuming that the less morphologically rich languages have 'lost' / 'reduced' the original, complex morphological / paradigmatic systems present in the proto-language¹⁴ (this model is in line with the original views by Bopp, Schleicher, Brugmann, etc., who reconstructed indeed a nominal and verbal system very similar to that of Greek and, particularly, of Sanskrit). In this volume this view is embraced by Kazanas as well as (with some reservations) by Di Giovine, who also puts forwards several proposals on how to account for the peculiarities found in the Hittite verbal system. However, other scholars think that the opposite holds¹⁵, that is: the special bond existing between Greek and Indo-Iranian in their morphological (particularly verbal) system appears to point (mainly) to widespread processes of possibly late contacts and areal diffusion¹⁶, rather than to archaism. This position is defended in this volume by Drinka, and, to a certain extent, implicitly, by Schmitt, when he claims that (Vedic) Sanskrit is not necessarily the oldest language within IE (see below). In particular, Drinka proposes a "stratified model of PIE, one which takes into account the systematic, layered morphological correspondences of Greek and Indo-Iranian". This in turn means that those "languages which share only archaic elements, such as Hittite and Germanic, are presumed to have separated from other IE languages at an earlier time". Thus, Drinka relies on the "stratification of data" – innovative layers building on more archaic layer – to account for the morphological correlations observed among the IE languages (for a 'stratified' interpretation of IE see also Andersen's chapter).

The idea of the existence of some sort of 'Indo-Greek branch / unity' is not totally new, having already been proposed, for example, by Pisani (1933, 1940 & 1974). As is known, this idea was mostly rejected at the time, since there are several other grammatical (as well as phonological) isoglosses that cut across such an assumed branch (compare for example, the reduplicated perfect of Germanic, or the future ending in -s of Lithuanian, shared with Sanskrit and Greek¹⁷). Some scholars still nowadays continue to reject the thesis of the Indo-Greek unity, arguing that it is a rather naïve notion, based on selected, partial evidence.

¹³ Hittite has no aorist; it has one preterit in opposition to the present tense; it has only indicative and imperative, no reduplicated perfect and no separate thematic and a-thematic conjugations. The verbs either follow the *-mi* conjugation or the *-qi* conjugation, although there are claimed to be a few, good matches to stems, suffixes and even whole paradigms with other IE languages.

¹⁴ It could be argued that the thesis of the 'loss' of rich morphology is a typically 'circular' explanation, even if (one could counter-argue) this explanation appears to be supported by the fact that the modern IE languages have all manifestly lost the original inflected structure, as shown by the modern Indo-Iranian languages, the Romance languages as well as the case of Romani and Domari, for which see again the chapter by Matras in this volume.

¹⁵ The issue of the presence *vs* absence of a rich (mainly inflectional) morphology has long been known; see for example Fortson (2004:224-5); (Meillet 1908); Birwé (1955); Meid (1975); (Neu 1976); Polomé (1981 & 1985); Drinka (1993/95, 1995, 2001 & 2003); etc.

¹⁶ There appears to be evidence, from archaeology and palaeo-anthropology, of pre-historical, as well as, of course, historical contacts between Asia Minor and the Indus Valley. See Sedlar (1980); Tucci (1982); Rawlinson (1916 & 1975) and Basham (ed. 1975).

¹⁷ Compare also the isoglosses that separate Hittite and Italic from Sanskrit and Greek (such as augment in past tense and prohibitive Skt. *mā* and Gk. *mē*), or the morphological isogloss of the locative plural ending *-su* / *-si*, which encompasses Sanskrit, Greek and Slavic. On the other hand, palatalization separates Sanskrit from Hittite, Greek and Italic. Notice also that palatalization is not at all a clear-cut isogloss, especially in Baltic and Slavic, for which see Miller (1976).

This is clearly stated in Kazanas (2004) and claimed by Kazanas in this volume. However, Drinka (in her contribution to the volume), on the basis of new data and new methods of analysis, argues that this unity does indeed exist, and uses the definition: “eastern languages” to refer to the Greek and Indo-Iranian special morphological bond. On the other hand, Di Giovine (in this volume) argues that looking at the issue in simple, binary terms of ‘archaism’ *vs* ‘innovation’ does not lead anywhere, since the status of the IE verbal system is much more intricate and subtle than this dichotomy would lead us to believe. All this, once again, ties in with, and lends support to, the well known fact that a clear-cut IE family tree is still difficult to draw, both at the phonological and morphological level¹⁸. This is why Drinka insists that areal / contact models of interpretation have to be adopted in addition to, or, actually, incorporated into, the traditional paradigm: only an integrated, three dimensional model (“an amalgamation of family tree and wave models”) can “explain” the nature and distribution of the correlations found among the IE languages (as also pointed by Andersen (1998: 420) and in this volume).

Whatever the case, one has to admit and reflect on the fact that the morphological reconstructions are essentially based on a few core languages, whose morphological correlations – whether the result of archaism, or innovation, or borrowing, or convergence – are nevertheless well attested, whilst “the attribution of the other languages to the family is necessarily done on partial evidence” (as stated by Morpurgo Davies; Cambridge Seminar¹⁹ [2005]).

3. 4. Are morphological reconstructions predictive?

It is reasonable to ask at this point whether the circularity issue (as discussed above with regard to phonology and lexicon) could have a negative impact also on the morphological reconstructions, including the area of diagnostic evidence. In fact, here too scholars can increase the power of the comparative method by making recourse to a number of ‘adjustable parameters’. Actually, the risk of circularity is higher at the morphological level, because of the (supposed) pervasive operation of analogy and the lack of a rigorous definition of ‘morphological correlation / similarity’ (as mentioned above). One can compare at this regard the many difficulties, obscurities and exceptions encountered in the reconstruction of the IE verbal and nominal paradigms. For example, with regard to the (thematic) *o*-stem²⁰ class of nouns referred to in Table (I) above (certainly a highly productive class of nouns, displaying rather consistent paradigmatic patterns), one can nevertheless find several, more or less serious difficulties. For example, some case endings cannot be reconstructed in an economic, straightforward way, or cannot be reconstructed at all, so that a number of ‘adjustable parameters’ have to be introduced in order to overcome the observed mismatches. The adjustable parameters in question include:

¹⁸ This problem emerges again in the various IE ‘phylogenies’ attained through ‘cladistic’ techniques. See Gray & Atkinson (2003); Ringe, Warnow & Taylor (2002); Garrett (2006), Forster & Renfrew (2006), and again Drinka in this volume.

¹⁹ Between October and December 2005 the Faculty of Classics of the University of Cambridge hosted a series of seminars on issues in IE linguistics, organized by A. Marcantonio.

²⁰ As an example of a well known noun belonging to this class (masculine), compare: Gk. λύκος, Lat. *lupus*, Goth. *wulfs*, OCS *vlikŭ*, Lith. *vilkas* ‘wolf’; etc.

1. a chain of (at times unverifiable) assumptions, including syncretism and reshaping through analogy;
2. a chain of (at times unverifiable) intermediate reconstructions and alternating forms;
3. a chain of minor but frequent (and, at time unverifiable) language-specific sound changes,
4. if any of these procedures fail, there is always the possibility of giving different interpretations to the internal structure, function and origin of the case endings themselves.

As concrete examples of the complications involved in the reconstruction of several case endings in this class of nouns compare the following²¹:

1. The instrumental singular, whose reconstruction requires the help of a laryngeal segment and vowel alternation: $*-e-H_1 \sim *-o-H_1$, whereby the ending is unmistakable attested only in Indo-Iranian $-\bar{a}$ ²² (although here too only in a dozen or so forms in the Rigveda, the usual ending being, even in the Rigveda, $-en\bar{a}$); no o -stem form is quotable from Mycenaean.

2. The genitive singular, whose overall picture is not clear, and for which at least two alternative reconstructions have to be postulated: a) the alternating type $*-os(y)o \sim *-es(y)o$, attested in Indo-Iranian, earliest Greek and Italic; possibly also in Germanic and Balto-Slavic, although with some obscurities in its development, and, possibly, in the Hieroglyphic Hittite $-asi$; b) the type $*-i$ (Italic), found in Latin and Celtic (whereby scholars appear to be still divided as to whether the two types of endings do or do not have an ultimate, common origin; see also Hock [1986:453]).

3. The locative singular $*-ey \sim *-oy$; here there are some doubts as to which of the two variants may have been the original form.

4. The ablative singular, variously reconstructed as $*-ad \sim *-at \sim *-h_2et$ (whence $-\bar{a}t$ in Vedic and $*-\bar{o}t$ in Italic). This does not appear to have the expected reflex in Lithuanian and does have some other, minor oddities in Latin adverbs in $-\bar{e}$.

5. The nominative plural $*-\bar{o}s$ in nouns and $*-\bar{o}y$ in pronouns, whose original distribution (Indo-Iranian, Germanic, Osco-Umbrian, and, with a bit of remodeling, Hittite [$at-te-e\check{s}$]), was otherwise "disturbed" (to use Sihler's (1995:261) words), as shown in Old Latin *poploe*. In the Rigveda fully a third of the endings of nominative plural are represented by another ending: $-\bar{a}sas$, which also occurs in Avestan.

6. The dative / ablative / instrumental / locative plural, which are expressed by various endings: a) $*-\bar{o}ys$ (attested in Vedic, Greek and Italic; see Old Latin *poplois*); b) $-ebhis$, which is the instrumental plural, occurring in Vedic with almost the same frequency as the ending $-\bar{a}is$, derived from $*-\bar{o}ys$ (it is questionable whether an ending of instrumental case can be equated to that of a dative/ablative/locative); c) Old Indian

²¹ See Mayrhofer (1973:38-9); Szemerényi (1996:183-7); Sihler (1995: 257-265); Meier-Brügger et al. (2003:198-9); Rix (1992:135 ff.); Fortson (2004:113-7); Clackson (2007: 9-10), etc.

²² This ending is ambiguous as to its grade.

-bhyas, to which Latin *-bus* in the other declensions corresponds; however, in Germanic and Balto-Slavic there is an ending **-m-* (**-mis*); d) locative **-oi-su*, present in Vedic, Homeric Greek, etc.

7. Finally, notice the Genitive plural *-ē* of Gothic, “still an unsolved problem” (according to Szemerényi [1996:185]).

Accounting for these morphological difficulties (however minor they may look) requires a number of justifications which all contribute to build up the complexity and, most likely, the ‘post-dictive’ nature of the explanatory apparatus.

3. 5. Counter evidence within morphology

It has been argued above that the morphological evidence on which the IE theory is founded appears to be rather ‘malleable’, this problem being caused by the general lack of criteria and guidelines on how to evaluate morphological correlations. This in turn means that it is equally difficult, if not impossible, to identify evidence potentially counter to the model (with perhaps the only exception of the evidence from languages in contact that wholesale morphological paradigms *can* be borrowed). For example, the absence of the fundamental IE category of feminine gender in Anatolian²³ could be considered a paradigmatic example of the inability on behalf of the IE theory (and the methods of historical linguistics in general) to make very clear-cut, testable predictions. There have been no claims (as far as we know) that the absence of this ‘diagnostic’ category should be classified and set apart as potential evidence counter to the IE origin of Anatolian²⁴. Quite the contrary, efforts have been devoted only to finding traces of the feminine gender here and there or to justify its absence in the most plausible, natural way (see for example Oettinger (1987); Melchert (1992); Justus²⁵ (2002), Clackson (2007:104ff.), etc). The same could be argued with regard to one of the most puzzling aspects of Hittite verbal morphology, the ‘*qi*-conjugation’, which, admittedly, does not easily slot into any reconstructed category of IE (although there are claimed to be some good lexical correspondences between verbs adopting this conjugation and verbal roots in other IE languages; see Jasanoff (2003); Neu & Meid (eds, 1979); Clackson (2007:139 ff.) and Carruba in this volume). The situation does not appear to be much different at the level of morpho-phonology, as is evident from the state of the research regarding the IE *Ablaut* (vowel alternation / apophony). Here, the scanty and irregular distribution of vowel alternations across the IE area has generated an array of complex, sophisticated explanations, none of which however appears to be either testable or satisfactory, as clearly illustrated in the chapter by Carruba in this volume. Carruba suggests that the original function of *Ablaut* is to be sought

²³ Hittite, for example, has no nominal declension corresponding to the feminine stems in **-eh₂* or **-i-h₂* (as these are currently reconstructed).

²⁴ The same could be said about the many difficulties and mismatches encountered at the phonological level between Anatolian and the other IE languages (as clearly pointed out by Melchert (1994); Kimball (1999) and Hawkins & Morpurgo Davies [1998]). Compare also Zeilfelder’s view (1994/2001:3 ff.), according to which the phonology (and lexicon) of the Anatolian languages is not of much help for the purpose of verifying either the thesis of the archaic or that of the innovative nature of the Anatolian branch.

²⁵ Justus’s article (2002:121-123) offers a summary of “contemporary views” on the problem of ‘gender’ in Hittite and related attempts to solutions.

in the 'deixis' – a rather archaic, elementary function, but a very productive one within IE. He ascribes the poor attestation of coherent patterns of vowel alternations across the IE area to the very ancient origin of phenomenon itself. This is certainly a plausible explanation; however, again, there may be alternative explanations. For example, it could be argued that the *Ablaut* present in Sanskrit (and partially in Greek), does not represent a phenomenon to be traced back to the IE proto-language, but on the contrary, a language-specific phenomenon. This is, in fact, basically Kazanas' opinion, as expressed in his assessment of *Ablaut* in this volume: Sanskrit would be the only IE language that has roots and a "proper vowel gradation". Compare also Marcantonio's contribution, which investigates the 'statistical significance' (or lack of it) of the present *vs* perfect vowel alternation. Finally, compare Di Giovine (1995) and Di Giovine in this volume, who instead embraces a more conventional approach to IE *Ablaut*.

3. 6. Is there hard core evidence at the foundation of Indo-European?

Bearing in mind the shortcomings of the morphological correlations as discussed above, it is understandable how several scholars, such as Campbell (2003a & b) and Morpurgo Davies (Cambridge Seminar 2005; see note [19]) have reiterated and emphasized the role of the phonological (/lexical) correspondences for the purpose of assessing genetic relations. One could compare also the case of Proto-Dravidian, where it is the lexicon that has seen the greatest amount of relevant, comparative work, as illustrated by Steever (1996). As a matter of fact, phonology has at times offered us the possibility of testing the validity of the comparative method, in the sense that some predictions made by the comparative analysis have subsequently been proven correct in connection with the discovery of new linguistic material. For example, new evidence from Hittite and Linear B Mycenaean has confirmed the validity of earlier reconstructions, in this way also allowing us to redefine the processes which lead to the attested languages. The evidence in question includes:

1. The laryngeal segments found in Hittite, which would fill in and confirm the existence of the 'coefficient sonantique' predicted by Saussure in his *Mémoire*.

2. The so-called 'r/n-stems' (the best known class of 'heteroclitic' stems), an archaic group of nouns²⁶ displaying a different final consonant in the direct *vs* oblique cases. This alternating stem had been reconstructed (through the means of both internal and external comparison) long before further evidence for the alternation became available with the discovery of Hittite, where, in fact, this class of nouns is somewhat productive (contrary to what happens in the other languages, where it is a relict; compare Latin *iter ~ itineris*, or Greek *ἵδωρ ~ ἵδατοζ*).

²⁶ Famous heteroclitic r/n-stem, neuter nouns include: 1) the term for 'water': *wod-r/n-, with alternating stem *wed-n-, preserved almost exactly in Hitt. *wātar* → Gen. *witenaš*; 2) the term for 'fire': *peh₂-w₁, continued most faithfully in Hitt. *paqqur* → Gen. *paqqenaš*; 3) the term for 'blood': *ēšqar* → Gen. *ešqanaš*. Compare also: 4) Lat. *femur ~ femin-is* 'thigh'; 5) *iecur ~ iecineris* (from earlier *iecor ~ iecinis*), the latter corresponding to Skt. *yakṣ ~ yakanas* 'liver'. Contrary to what happens in Anatolian, the original mode of inflection is scantily attested elsewhere, in which case 'conflation of paradigms' is typically assumed (see Szemerényi (1996:173); Fortson (2004:110-2) & Clackson [2007: 94-5]).

3. A series of separate sounds in Mycenaean, testifying in favor of the postulated IE labio-velars stops, which were lost by the time of alphabetic Greek (having merged with other stops according to specific rules and dialects). This distinct series of stops, transliterated as *q*, not only are attested in Linear B, but are also still preserved exactly where predicted²⁷, as shown, for example, by *qe* 'and', with *q* as a reflex of labio-velar (< **k^we*).

These are undoubtedly remarkable discoveries, and fine analyses. However, the data and analyses under discussion are not void of difficulties. For example, with regard to the Mycenaean material and the postulation of labio-velar segments, one should bear in mind that, given the Mycenaean spelling system, we do not really have direct proof that the segment in question is indeed a labio-velar, although this represents, without doubt, "a strong contender" (to use McMahon & McMahon's (2005:14) words²⁸). As to the heteroclitic stems, it is recognized that the Hittite evidence is contradictory, or at least unsatisfactory, since not all the *r/n*-stem Hittite nouns available appear to have IE counterparts, as clearly pointed out by Sihler (1995: 299-300). Furthermore, one could observe that the more linguistic material, the more *comparanda* one brings into the equation whilst doing comparison, the easier it is to find a phonological /lexical (as well as morphological) match with a given form or reconstruction. As Campbell (1998: 277) puts it, referring to the issue of mass-comparison and macro-families: "The potential for accidental matching increases dramatically ... when one increases the pool of words from which potential cognates are sought or when one permits the semantics of compared forms to vary even slightly" (for similar remarks see Hock (1993)²⁹; compare also the chapter by Marcantonio in this volume). Finally, one could always argue that a few instances of fulfilled predictions – the occurrence of the right, predicted reflex at the right place – are not enough to prove the supposed predictive nature of all the conventionally established IE laws.

As to the laryngeal theory, it is undeniable that it represents one of the most outstanding examples of a successful theory within historical linguistics. As Andersen (2007)³⁰ puts it: "From a purely algebraic theory in Saussure; given putative phonetic justification by Hermann Moeller; then actually justified by

²⁷ Compare Fortson (2004:227): "Before the decipherment of Mycenaean, it had been assumed that the labiovelars were still intact in Proto-Greek [...] even though none of the dialects then known actually preserved them as labiovelars. This assumption had been made because it was the easiest way to explain certain consonant alternations within the dialects [...]. Mycenaean proved this hypothesis correct: it still preserved the labiovelars intact, or at least preserved them systematically as something different from labials, dentals, or plain velars".

²⁸ Since we do not have a segment-by-segment spelling system for Mycenaean, we cannot directly prove that the segment at the beginning of those syllables (where we would reconstruct a labio-velar) is indeed a labio-velar. All one can safely state is that the symbol in question is not the one used in syllables which have /p/, /t/ or /k/ in other Greek dialects; therefore, the initial consonant must be something else.

²⁹ Hock (1993:221ff.) observes the following: "Preliminary results [of an experiment in which English, Finnish and Hindi are compared] suggest that enlarging the data base does not improve the reliability of the method. In fact, if there is any change at all it may consist in a slight increase of false friends". On the other hand, Hock (*ibidem*) also recognizes that working through a comparison of only a small number of languages (three in the specific case), may be equally misleading.

³⁰ Personal communication; 2007.

the discovery of some of the posited segments in Anatolian; found to correlate with the 'prothetic³¹ vowels' and 'Attic reduplication' in Greek, and of 'final lengthening' in Homer; and then with the Baltic and Slavic distinction between acute and circumflex long vowels and diphthongs". Nevertheless, the laryngeal theory is not uncontroversial, at least with regard to the number of laryngeal segments to be postulated. Compare for example the divergent views by Martinet (1953), Szemerényi (1973 & 1996:121-6), Meier-Brügger et al. (2003:107), Lindeman (1982), etc. Compare also, for example, Beekes (1989:1 ff.), who argues in favor of three laryngeals, whose number would be required by Greek, although Hittite "certainly points to two laryngeals", and Di Giovine (2005), who, in line with other scholars, calls into question the postulation of /h₁/, since this segment does not have direct reflexes in any of the IE languages, including Anatolian³². However, the main problem associated with the laryngeal is that the advantages attained by making recourse to the laryngeal theory are counterbalanced by a number of disadvantages. For example, Winter (1990: 20-1) referring to Saussure's original idea of the 'coefficient sonantique' and subsequent developments, describes one of the known 'disadvantages' of the laryngeal theory as follows:

We attempt to simplify our statements by subsuming overtly differing phenomena under one common formula which may or may not require positing directly unattested elements conditioning the differences [...]. This analysis had the tremendous advantage that now all subtypes of ablaut in Proto-Indo-European could be treated alike and that canonical forms of roots could be set up; along with this, however, went the disadvantage that the Proto-Indo-European system of vowels apparently had to be reduced in an unreasonable way

Sihler (1995:111 ff.), who reconstructs an *Ablaut* system "considerably leaner" than others thanks to the support of the laryngeal theory, appears to (implicitly at least) identify another, major 'disadvantage'³³. In fact, he states that the economy of his model is achieved by "a complication elsewhere in the system", because his reconstruction also requires "a number of sound laws" applying to the postulated laryngeals³⁴. Here, the fundamental questions (which have not been asked yet, to our knowledge) are: a) how many more sound laws do we need to make the laryngeal system work? b) is this extra number of segments and

³¹ The claim that prothetic vowels can be explained through laryngeals in Greek and Armenian has been contested by Wyatt (1972) and Beekes (1969:18-98).

³² Compare also the volume edited by Vennemann (1989), where the 'new sound of Indo-European' appears to have had triggered some reshaping in the definition of the laryngeal theory. Furthermore, it is now recognized that Hittite does not really lend much support to the laryngeal theory, since the evidence available from this language is, admittedly, 'fragmentary', 'contradictory', 'disappointing' (see Lehmann (1952: 25 ff. & 1993:125); Szemerényi (1996: 121-126 & 137-8); Mayrhofer (1986:123ff), Clackson (2007:58 ff); etc.). The absence of satisfactory evidence for laryngeals from the Anatolian languages seems to have been compensated, in recent years, through data drawn from Greek, which is now claimed to show an unparalleled 'triple reflex', that is, a distinct outcome for each of *h₁*, *h₂* and *h₃*, when they occur within a few specific contexts (see Clackson 2007:58-60). This analysis appears to be gaining acceptance, even though laryngeals nowhere survive as consonants in Greek.

³³ Sihler's reconstruction is much leaner than others because it works with only "a SINGLE pattern of alternations, **e* ~ **o* ~ Ø", together with "three new consonants of obscure phonetics", the laryngeals *H₁, *H₂ and *H₃. Note however that Sihler does not appear to consider this extra number of rules as a 'disadvantage', that has a negative impact on the 'economicity' of the system. This is, in fact, an interpretation of the Author (A. M.) of the present *Introduction*.

³⁴ Similar remarks can be found also in Collinge (1985) and Gusmani (1979).

required operating rules added to the system as high as to nullify the otherwise attained benefits? In this regard, Marcantonio (in this volume) argues that adding laryngeal segments to reconstructions does indeed increase the explanatory power of the comparative method³⁵.

Thus, if we accept the analyses, objections and counter objections expounded thus far, it is still not clear what exactly the uncontroversial, hard core evidence that lies at the foundation of the IE theory is supposed to be³⁶.

4. Is there a pre-historical reality behind reconstructed Indo-European?

4.1. Conventionalism *vs* realism

The 'conventionalist' *vs* 'realist' approach to linguistic reconstruction, and related concept of proto-language (in general as well as within IE), is another long standing debate within historical linguistics, and one for which, yet again, there does not appear to be much of a consensus. Compare Koerner (1989) for an overview of the debate; compare also Bonfante (1945); Pulgram (1959 & 1961); Meid³⁷ (1975) and Clackson (2007:16 ff.). It is true that between the extreme conventionalist approach on the one side and the full realistic approach on the other there are plenty of more moderate, intermediate positions. Nevertheless, it appears that the fully realistic approach, which in turn is based on the controversial method of palaeo-linguistics³⁸, is the one that has so far attracted many supporters, and not only among archaeologists (such as Mallory (1989 & 2001) and Renfrew [1987, 1990]), but also among linguists. For example, in his criticism to the thesis by Gimbutas (1970) regarding the location of the IE home land, Schmitt³⁹ (1974: 283) observes that "[with] the methods of linguistic paleontology anything can be proved as Proto-Indo-European, but it can *not* be proved as *typically* Proto-Indo-European". Thus, Schmitt appears to have confidence in the validity of the realist approach, although he has doubts regarding the factual interpretation of single reconstructions and the hasty conclusions reached about the reconstruction of single 'pre-historical facts'. It could be said that in this case too (like in the case of the 'regularity principle') it is not always made explicit what the motivations are, what the evidence is, that provides the basis for the choice of one

³⁵ For an up-to-date state-of-the-art report on the laryngeal theory & essential bibliography see: Polomé (1987); Mayrhofer (2004:17-39); Meier-Brügger et al. (2003:106 ff.); Di Giovine (2005) & Clackson (2007); see also Sturtevant (1942).

³⁶ Last, but not least, one should take into consideration also those further limitations of the comparative method as pointed out in recent years by scholars such as Dixon (1997), Aikhenvald & Dixon (2001) and Nichols (1992, 1993/1995, 1996b, 1997 & 1998); see also Andersen (2006). These are: a) the inability of the method to get at a more remote linguistic prehistory than the generally assumed 8000/6000 years); b) its inability to account for the actual distribution of the degree of linguistic 'diversity' *vs* linguistic 'uniformity' found in the world. These issues are not dealt with in this volume.

³⁷ For example, Meid (1975), in proposing his 'Space-Time' model, claims that the spoken IE parent language did exist, and that there must have been a corresponding very small, tight-knit community which slowly expanded over a vast area, and through a long stretch of time. On the contrary, Clackson (2007:16) states that PIE "is not a real language" and that: "Reconstructed IE is a construct which does not have any existence at a particular time and place".

³⁸ Among the criticisms raised in the literature against the methods of palaeo-linguistics see Renfrew (1987:77-86, 103-104, 109-110) and Marcantonio (2002/5); compare however Polomé (1990) for a rather positive evaluation.

³⁹ Please, note that although R. Schmitt contributes with a chapter in this volume, his contribution by no account deals with the issue of how real(istic) reconstructions may be.

approach, or the other. In this volume, this issue is dealt with, or touched upon, by Andersen, Bryant, Drinka and Di Giovine. The chapter by Andersen deals (also) with the ontological status of proto-languages and the relation between such a 'language' and community languages. The chapter by Bryant (also) discusses the 'malleability' of the palaeo-linguistic evidence relating to the origin of the Sanskrit language and culture. The chapter by Drinka draws attention (among others) to the fact that the linguistic evidence (in particular, specific morphological correlations) provides more complete information as to the relatedness of population and to historical /cultural reconstruction than archaeological records. Di Giovine, on the basis of the analysis of the IE verb inflectional system presented in his contribution, argues that PIE was not a compact language or not even a community language at all. Compare also the chapter by Häusler, as discussed in the following paragraph.

4. 2. The Aryan debate

Within the camp of the realist approach the debate has revolved mainly around the issue of the whereabouts of the (assumed) IE proto-community. A strand of this debate has recently re-surfaced under the definition of: 'the Aryan Debate', for which see Bryant (2001), Bryant & Patton (eds, 2005), Kazanas (2001, 2002, 2003 & 2004); Polomé (1985); Tripathi (ed., 2002/2005); Masica (1991:32-60), as well as the two debate-volumes on the origin of complex societies in Central Eurasia, edited by Jones-Bley & Zdanovich (2002). On this topic, compare the chapter by Bryant, Drinka, Häusler and Kazanas (and, to a certain extent, Schmitt) in this volume. The Aryan debate centers on the following, major interconnected issues:

1. Was the assumed homeland located in the west, somewhere in Europe, or in the east, somewhere in North India /Pakistan? In other words, were the bearers of the Old Indo-Aryan (Vedic and Sanskrit) culture indigenous or intrusive in North India?
2. Why, how, when and in which direction did the original proto-community disintegrate and migrate / spread, so as to bring about the distribution of the IE languages as we found them in historical times?
3. How old is Old Indo-Aryan? Is it as old as the traditional IE theory claims (about 1700/1500 BC), in line with the 'migrationist' model, or is it much older than that, in line with the 'indigenist' model? In turn, these questions are connected with the following major issue (already touched upon above):
4. Is Old Indo-Aryan the most archaic, and therefore, the most important language for the purpose of reconstruction?

The issues raised in points (3) and (4) are of particular relevance, not only because they are strictly connected to the home land issue, but also because the assumption of the great antiquity of (Vedic) Sanskrit is (or, at least, it has been) one of the pillar tenets at the foundation of the IE theory, and related traditional reconstructions. As we have seen above (par. 3. 3.), this tenet is now under attack, since several scholars believe that the *Greco-Aryan* morphological paradigm represents in fact innovation, and not archaism. As to the more specific issue of the archaicity of Sanskrit, Schmitt in this volume calls into question this

traditional view, arguing that: “the archaism particularly of the Old Avestan language makes it only too clear that, despite the old age of the earliest Vedic texts, the Old Indo-Aryan language is not the only fundament of the IE proto-language [...] and that its data are not with absolute necessity genuinely antique; therefore Old Indo-Aryan (both Vedic and Sanskrit) is not so close to PIE as many people think”. On the other hand, Kazanas (quoted, and in this volume) supports the traditional way of thinking as well as the indigenist model claiming exactly the opposite: Old-Indo-Aryan is indeed the oldest language within the family (actually, much older than conventionally assumed), as well as the closest language to PIE, even though a PIE language cannot be reconstructed. In turn Drinka, in her contribution to this volume, rejects the indigenist / ‘Out-of-India’ model, arguing against it (mainly) on the basis of those very “archaic” morphological isoglosses Indo-Aryan shares with some IE languages and those very “innovative features” it shares instead only with Iranian and Greek.

4. 3. Sanskrit and the South Asia linguistic area

This already articulated debate is in turn connected with the other, equally tangled issue of how to interpret the lexical, phonological, structural and even morphological correlations that have been identified as existing between the Old (and Modern) Indo-Aryan languages and the other non IE languages of India, such as (mainly) Dravidian and Munda. In fact, these (and other) various languages / language families of India are widely claimed to form what is usually referred to as the ‘South Asia linguistic area’, for which see Steever (1993:10 & 1996:11 ff.); Masica (1976) and Emeneau (1956, 1971 & 1980). In particular, on the basis of (supposed) lexical borrowing from Dravidian into Indo-Aryan and from the modern geographical distribution of the Dravidian languages, it is often claimed that Dravidian and Indo-Aryan must have been in contact – since prehistoric times– in those extreme northwestern areas of India first inhabited by the Indo-Aryans (but see Hock (1996:38) and discussion below for different points of view⁴⁰). Thus, the relevant question here, the question which has been hotly debated and whose resolution, if ever attained, could help to break the deadlock of the indigenist *vs* migrationist model – together with the issue of the degree of antiquity of Sanskrit – is the following: Are the non IE features present in Old-Indo-Aryan the result of sub-stratum, super-stratum, ad-stratum, convergence, or even genetic inheritance? At this regard, compare the (at times clashing) views by scholars such as: Burrow (1946, 1973 & 1975); Emeneau (1980); Hock (1975, 1986, 1996 & 1999); Kuiper (1955 & 1991); Witzel (1999 & 2000); Kuzmina (2003); Misra (2005); Kazanas (2004) and Kazanas in this volume. Several scholars, such as Southworth (1979), Kuiper (1955, 1991 & 1992) and Burrow (1973), have compiled long lists of (Rig-)Vedic words believed to be of Dravidian or, more generally of non-Aryan origin. Other scholars, particularly Hock (1996: 36ff.), have drawn attention to the fact that the situation is not that clear-cut after all. In fact, in many instances it is difficult to trace back the origin of the lexical as well as structural similarities observed in this intricate linguistic area, and to sort out whether they are attributable to borrowing, convergence, or chance resemblances⁴¹. On the other hand, there are Indian scholars (such as

⁴⁰ Hock (1996:38) states that: “given the multiply different possible explanations, no explanation can be considered certain enough to be used as evidence for or against prehistoric Dravidian/Indo-Aryan contacts”.

⁴¹ Hock (1993) observes that within well established families too, such as IE, a significant number of chance resemblances may occur, or, conversely, a number of cognates are very hard or even impossible to detect because of the changes they have undergone through time.

Misra (2005) and Aiyar [1975]) who challenge the traditional classification of Sanskrit as an IE language, observing that under a different socio-politico-cultural context, the similarities under discussion could have been interpreted differently. For example, they could have been ascribed to the postulation of a shared, common ancestor for Sanskrit and Dravidian, and the studies of the South Asian linguistic area, as we know them today, might have taken a totally different course, generating a different production of knowledge, and, possibly, a totally different proto-language as the ancestor of Sanskrit. In turn, these scholars are accused to be strongly biased in their research, being driven by nationalistic feelings and political motivations. Certainly – it has to be said – there are elements of truth in this statement, since the main concerns (mostly unexpressed) of these Indian scholars are for the cultural unity of India under a Sanskritic proto-banner, whereby Dravidian would be some sort of Sanskritic prakrit (on these issues see the chapter by Bryant and Annamalai & Steever in this volume). Nevertheless, it is also fair to recognize that had the events, the accidents of History been different, in other words, had Sanskrit not been brought to the attention of the western scholars (as a consequence of the English colonization of India, for which see Bryant [2001]), the cultural scenario of historical linguistics might well have been totally different indeed, and one can easily imagine how a different paradigm might have been created to account for the origin of Sanskrit and the other languages (/language families) of India. Actually, the fact that the initial choice of comparing a certain pair / set of languages, rather than others, is by its own nature ‘intuitive’ and may be also influenced⁴², or even dictated by circumstances external to linguistics, can increase the risk of circularity embedded in the traditional methods of analysis, as discussed above (for similar remarks see Clackson⁴³ [2007: 3]). It would therefore be desirable to be able to ‘demonstrate’ that the correlations shared by Sanskrit and Dravidian, for example, are the genuine effect of a typical process of *Sprachbund* convergence, rather than simply ‘labeling’ them as ‘contact-induced’ just because it is widely accepted that the languages in question are not genetically related. The thesis that the correlations shared by Sanskrit and Dravidians are the genuine effect of *Sprachbund* is embraced by Annamalai & Steever. In their chapter the Authors do not directly address the issue of the origin of the non IE features present in Old (and Modern) Indo-Aryan languages, but argue that Dravidian is a clearly distinct linguistic family from Indo-Aryan, although the latter does indeed display Dravidian features. In other words, according to Annamalai & Steever, “some of the four genetically distinct language stocks in South Asia have clear genetic linguistic relations outside of the subcontinent”. As a matter of fact, a comparison of two classical languages of India, Sanskrit and Old Tamil, as carried out by the Authors, would reveal and illustrate a typical *Sprachbund* situation, where “two languages may be genetically distinct, yet grammatically related”.

At this point it is interesting to observe that there is at least one aspect within this intermingled debate about which there appears to be consensus among all scholars: there is no archaeological evidence for the assumed migrations of (part of the) original IE community, either from a supposed western homeland eastward into North India, or the other way round, as pointed out by Bryant (2001), Chapman & Hamerow

⁴² The classification of Sanskrit is not the only known instance of linguistic classification which has been influenced by historical /political events. There is a clear, well documented example of this process: Hungarian, for which see Marcantonio 2002.

⁴³ Clackson (2007: 3) says: “The operation of the comparative method does not guarantee a language’s place in the family; only the initial recognition that two or more languages are related can do that [...]. When does a linguist decide that there is enough material to relate a language to the IE family? There is no absolute set of criteria beyond the general rule that the evidence must convince both the individual linguist and the majority of the scholar community”.

(eds, 1997) and Häusler (2002, 2003a, b& c, & 2004). Compare also the contribution by Häusler and Kazanas in this volume. Häusler (2004:1) points out that the explanation of the spread of the Indo-European languages as “the spread of the culture of a concrete group of people from a cradle [...] has not been proved by anthropology and archaeology as yet”. The Author (*ibidem*) also believes that “all attempts to reconstruct the old culture of the Indo-Europeans as existing in a concrete cradle, by the means of ‘linguistic paleontology’ are wrong”. On the other hand, the objection could certainly be raised that the ‘absence of evidence is not evidence of absence’, so that scholars are faced here with yet another area of IE studies where the ‘evidence’ (or lack of it) is malleable and un-decisive. As a matter of fact, Häusler himself believes that “the IE linguistic community must have existed at some point in ancient times, since the linguistic classification has been fairly safely established, even if this (assumed) linguistic community is not at all retrievable by the means of archaeological and anthropological research” (as stated in his contribution to the volume). Whatever the case, the fact remains that, thus far, the required, supporting (one way or the other) archaeological and palaeo-anthropological evidence has not been found.

As to the palaeo-linguistic evidence – place and river names, terms for flora & fauna, names of deities, etc. – mainly from Sanskrit, some scholars claim that this evidence is malleable, and therefore inconclusive when it comes to trying to establish the indigenous or intrusive character of Old Indo-Aryan; see Bryant (2001) and Bryant in this volume. In contrast, other scholars claim that the palaeo-linguistic evidence clearly supports the indigenist / ‘Out-of-India’ model; see for example Kazanas (2004) and Kazanas in this volume. Drinka, on the other hand, argues that the thesis of late contacts between Greek and Indo-Iranian – as put forward in her contribution to the volume – constitutes one more piece of evidence that the ‘Out of India’ theory is untenable.

Finally, it is worth mentioning that recent genetic evidence (whatever its relevance may be in this context) appears to support the indigenist hypothesis of the origin of the Sanskrit speaking peoples and culture; see Kazanas (2002 & 2004), as well as Kazanas’ and Bryant’s chapter in this volume.

4. 4. The conventionalist approach

Those scholars who adopt the conventionalist approach to reconstruction claim that linguistic classifications do not necessarily imply and guarantee the existence of the corresponding speech community. Therefore, searching for the original IE community, and the pre-historical, highly intertwined linguistic and extra-linguistic processes that would have brought about the IE languages, is a pointless task. This remains the case even if linguistics is assisted by other disciplines whose methods of analysis are claimed to have recently achieved high levels of reliability, such as archaeology and genetics. In other words, comparative historical linguistics cannot in any way shed light onto pre-historical ‘facts’ (homelands, migrations, institutions etc.). Historical linguistics was not meant to be, it is not and cannot be used as a ‘branch of prehistory’. As Harrison forcefully (2003:231) puts it:

Too many comparative historical linguists want to dig up Troy, linguistically speaking. They consider it more important that comparative historical linguistics shed light on prehistoric migrations than that it shed light on the nature of language change [...]. I do not consider comparative historical linguistics a

branch of prehistory, and I sincerely believe that if we cared less about dates, maps, and trees, and more about language change, there'd be more real progress in the field

5. Introduction to the chapters (in alphabetical order)

Chapter II. Henning Andersen's chapter: *The satem languages of the Indo-European Northwest. First contacts?* focuses on some of the key concepts of comparative linguistics: *Stammbaum*, proto-language, the relation between stemmas and the '(re)construction' of language histories, diffusion, Schmidtian dialectal areas, etc. The Author's specific field of research is the Slavic and Baltic languages – a very good starting point for the debate in question. In fact, these languages show obvious similarities, underpinned by regular and systematic correspondences on the one hand, and "significant irregularities in phonological development, divergences in morphology and deep differences in vocabulary" on the other hand. This in turn means that a coherent, pre-historical account of their origin and mutual relationships proves difficult to draw. In particular, Andersen observes that even some of the regular correspondences holding within and among the two branches, such as instances of regular developments of their respective accent (/accent system), may be deceiving, in the sense that it is difficult to ascertain whether these developments are the result of shared innovation or, instead, of parallel but independent developments, triggered by language contact (including contact with pre-IE languages). Actually, the Author argues, some of the earliest accentual features shared by Baltic and Slavic, traditionally interpreted as shared innovation and, therefore, as inherited, are in fact most likely to be the result of separate development. The Author further remarks how the laws conventionally established to account for the Balto-Slavic accentology cannot really be considered 'law', since they are merely plain, "laconic restatement" of the observed phenomena, often relying upon massive recourse to unjustified (and atypical) analogical changes. A new, more rigorous interpretation of the relevant data and processes, a "paradigm shift", is then proposed. The Balto-Slavic issue offers Andersen the opportunity to re-affirm his thesis of the "stratification of the IE Northwest", according to which the substantial differences existing between the two language groups as well as within Baltic itself can be accounted for by positing various, distinct substrata, distinct IE language traditions underlying the later pre-Baltic and pre-Slavic dialects. In other words, the European Northwest would be one of those areas where "admigrations" took place, i.e., migrations staggered at different times – just as there must have been other admigrations from the IE core area. It could be said that Andersen criticizes IE/comparative linguistics from within – as it were – accepting the basic paradigm and traditional procedures whilst focusing on appropriate and inappropriate ways of interpreting the established correspondences and tenets, in an attempt to attain realistic and more broadly acceptable results. At this regard the reader might want to compare the chapter by Marcantonio, who calls into question the (supposed) 'scientific' nature of the comparative method and, as a consequence, of the IE family. Compare also the chapter by Drinka, who shares with Andersen the view that a "stratified model of IE relationship" can better account for the relevant evidence. Kazanas, on the other hand, lines up with both Andersen and Drinka in reaffirming the validity of the IE theory in general, but he has no faith in the conventional IE reconstructions.

Chapter III. The chapter by E. Annamalai & S. B. Steever: *Ideology, the Indian homeland hypothesis and the comparative method*, focuses on a wide range comparison of two classical languages of India: Old Tamil and Sanskrit. By paying particular attention to the phonological, morphological and morpho-phonological components of their respective grammar, the Authors point out where and how these languages converge and where and how they diverge. After evaluating the nature of the observed similarities and differences, the Authors argue that Old Tamil and Sanskrit belong to two distinct language families (Dravidian and IE, respectively), according to mainstream view. They also reaffirm the conventional thesis that the shared similarities of these languages clearly bear the diagnostic marks of a *Sprachbund* convergence. Thus, according to Annamalai & Steever, the application of the comparative method to the Indo-Aryan languages continues to uphold the traditional IE hypothesis, by means of the well known, systematic correspondences existing among the various IE languages. On the contrary, those scholars who put forward claims about the historical relation of the South Asian languages to an Indian homeland – also by calling the methods of historical linguistics into question – find themselves at a loss to economically explain the systematic correlations that hold within IE. More specifically, the proposed comparison of the selected structural features of Sanskrit and Old Tamil is claimed to show how these two languages do not incorporate reference to a so-called ‘geographic index’ – an index that would point toward a historical origin of the languages in question in South Asia. As a consequence, the hypothesis of an Indian homeland, as well the criticisms raised against the comparative method, would owe more to ideological currents of thought than to scientific, linguistic analysis. At this regard, the reader may want to compare the views expounded by Annamalai & Steever with those put forward by Bryant and Kazanas. For example, Bryant points out how major areas of the conventional (linguistic and extra-linguistic) evidence on which the (Indo-)European origin of Sanskrit is based, can actually lead to a circular reading. Kazanas instead supports the Indigenist hypothesis for the origin of Sanskrit (/Indo-Iranian) and reaffirms its IE classification, but denies that the IE languages are held together by regular and systematic correspondences.

Chapter IV. Edwin Bryant, in his chapter: *The Indo-Aryan migration debate* summarizes the debate relating to the origin of the Vedic culture and language. The numerous, intricate facets of this debate include: the underlying ideological background, some of the more prominent aspects of the data brought forward by either camp (migrationists *vs* indigenists), the methodological shortcomings underpinning the relevant evidence, including a widespread, embedded circularity, etc. The main claim made by the Author is that the relevant palaeo-linguistic evidence, as well as some aspects of the extra-linguistic evidence, is so flexible, so ‘malleable’ that it does not permit a determination of whether the Vedic culture is of local origin or is instead intrusive into North West India. More specifically, much of the evidence associated with the Indo-Aryans, whether philological, linguistic or even archaeological (such as the absence of horse and chariot remains in the relevant archaeological sites), can be construed in sometimes diametrically opposed ways by scholars approaching the issue from differing angles. With little or nothing in the data itself to favor either models of interpretation, marshalling the data in support of one or the other position is clearly a consequence of the starting up assumptions. Then, typically, the selected reading of the data is configured to support the initial assumption, in this way generating an obvious circularity. This circularity becomes particularly acute in the case of the vexed issue of the (supposed) borrowed elements in Sanskrit.

Here again, on many instances the data itself would not allow to trace back the source and /or direction of the borrowing. Thus, if one did not accept that the IE classification is well founded, the overall borrowing issue, the connected substratum *vs* ad-stratum issue, and, ultimately, the conventional classification of the Indian languages involved (including Dravidian and Munda) would certainly demand revisiting. Bryant's views are at odds with the views of those scholars (in general and in this volume) who argue instead that there is enough linguistic and extra-linguistic evidence to allow linguists to support either the indigenist model (see Kazanas) or, conversely, the migrationist model (see Drinka, Annamalai & Steever and Schmitt). Bryant's chapter can be read also in connection with the chapter by Marcantonio, who challenges the normative model of IE historical reconstruction. Finally, Bryant points out that there is at least one clear, undeniable 'fact' amongst so much malleable evidence: no trace of migration (in either direction) can be found in the field of archaeological, paleo-anthropological and historical research (as also claimed by Häusler), whilst the genetic evidence appears to suggest a local origin for the bearers of the Vedic culture (as also pointed out by Kazanas).

Chapter V. Onofrio Carruba, in his chapter: *Indo-European vowel alternations: (Ablaut/ apophony)* intends first to review the state of the research regarding the IE vowel alternations, and then to put forward his own views regarding the origin and nature of this phenomenon. In fact, the Author argues, despite intense research and long standing debates, neither the origin nor the original function of the apophony have really been identified. More specifically, Carruba intends to investigate the connection between the "mobility" of position of the accent and the alternating vowels – and related "mobility of meaning" – both at the PIE and the "pre-PIE" stage of development. To do so, one should depart from the traditional schemes of analysis and investigate instead the relationship between the stem of the present in *-e* and that of the perfect in *-o vs* the vowels in question. It is here in fact that one can clearly observe the 'deictic' value and origin of the apophony, both in the sense of its 'temporal' value ('vicinity' (/presence) *vs* 'distance'), and in the sense of its 'markedness' value (*-o* being marked and *-e* unmarked). This would mean that the accent – now such a vital element for the apophony – must not have been that vital, or must have been a pitch type of accent, in an earlier phase of IE. To trace back the ablauting structure of this pre-PIE phase it is not only necessary to take into account the morpho-phonemic schemes of the actual IE languages, but also to make recourse to linguistic notions, linguistic elements that are proven to be really archaic, as is indeed the 'deixis' (a rather elementary notion, but a very productive one within IE). Having identified the 'deixis' as the founding factor of the (pre-*proto*-)IE apophony, Carruba focuses his attention onto the Anatolian languages. Here we would observe a clear example of the "breaking down" of the typical IE ablauting schemes (that are in any way well preserved only in Old Indian, and, to a lesser extent, in Greek). In fact, Hittite typically displays several new ablauting forms (such as *sāk(ah)hi* 'to know'), although it also preserves some old, known ones. Carruba concludes that: "There are still numerous difficulties that hinder scholars from fully understanding the nature and behavior of the apophony [...]. It appears to be lost in the depth of time, at the point that it is practically impossible to achieve an un-equivocal and coherent reconstruction". In line with mainstream doctrine, Carruba ascribes these difficulties of reconstruction to the 'loss' of the relevant *comparanda*, due to the great time depth involved. An alternative explanation for the poor occurrence of the perfect *vs* present *Ablaut* in most IE languages is proposed by Marcantonio. In her opinion, both the perfect *vs* present alternation and the IE *Ablaut* in general may not be an ancient,

residual IE phenomenon, but simply an artefact of the method of analysis, as is suggested by the clear lack of 'statistical significance' in the relevant data. Compare also Kazanas' view, according to which only Sanskrit possesses a proper *Ablaut*.

Chapter VI. Paolo Di Giovine, in his chapter: *Verbal inflection from "Proto-Indo-European" to the Indo-European languages: A matter of coherence?* intends to re-visit that coherent bundle of grammatical features which join together Old Indian, Avestan, Ancient Greek, Gothic, Old Church Slavic, Latin and Hittite, and on which the reconstruction of a PIE verbal system is traditionally based. Against this background, the Author observes, several other grammatical features, several 'independent innovations' can also be identified, whose close scrutiny would show to what extent a different feature "springs up as an independent innovation and to what extent it points to different archetypes" - the latter case in turn would suggest that PIE was not a "compact language, or not even a language at all". In particular, Di Giovine focuses on the still hotly debated issue of the innovative *vs* conservative character of the verbal system of the various IE languages, ranging from the Greco-Aryan verbal system at one end of the spectrum to the Hittite one, at the other. The Author comes to the conclusion that looking at this issue in terms of the usual, simple opposition of: 'archaism *vs* innovation', can be misleading. Similarly, the schematic, binary family tree model is not of much help to properly account for the actual distribution and nature of the morphological (particularly verbal) correlations among the IE languages (as indeed recognized nowadays by many scholars, also in this volume). The type of stratified model of IE put forward in this volume by Drinka and Andersen, according to Di Giovine, does not appear to be satisfactory either. Instead, the model of interpretation that would allow us to 'explain' the morphological correlations in a more realistic way - although by no means in a totally satisfactory way - is the one that depicts IE as a pool of dialectal varieties, a pool of languages that progressively develop and differentiate from one another according to each language internal, specific mechanisms, specific 'drift'. For example, it would be inadequate to consider the Greco-Aryan verbal system purely and simply as an innovation, given that those languages that possess a less complex verbal morphology actually show relics of ancient verbal categories that are no longer productive, or are extinct. This is the case of the Latin *perfectum* (the result of the convergence of aorist and perfect), or the Latin conjunctive of the type: *sīm* and *velim* (formally a continuation of the optative suffix, which is no longer attested in this language). Thus, a detailed investigation of the history of the single languages /language groups would be required in order to achieve "coherent" and reliable results. Such an investigation would reveal how it is the 'simplification' of the inflectional system that represents innovation, rather than the other way round. This view is held in this volume also by Kazanas, although he reaches this conclusion through different arguments and a different approach to reconstruction.

Chapter VII. Bridget Drinka, in her contribution: *Stratified reconstruction and a new view of the family tree model*, focuses on the debate of whether and how we can sort out genetic *vs* areal correlations, and connected issues of how to interpret the conventional concepts of IE proto-language and family tree. Drinka starts from the assumption that linguistic evidence is an indispensable resource in cultural reconstruction, one which provides more complete information as to the relatedness of particular groups than the archeological record can furnish. The validity of IE as a construct is not contested here, nor is the

importance of comparative reconstruction as a valuable tool. What is contested, however, is the oversimplistic characterization of PIE (still maintained by a number of scholars) as a single unified entity, or as an abstraction which serves only to represent the relationship of the daughter languages to one another (not a language which was truly spoken). Drinka presents a critical interpretation of the comparative data, with an eye to constructing a more realistic picture of the development of the IE languages, including stratification of layers and areal contact as important considerations. Specifically, Drinka claims that the conventional genetic model is only endorsed in her analysis as a 'descriptive' model of the ancient relationship of IE languages, but its usefulness as an *explanatory* model is questioned. What the family tree represents is a skeleton, providing a mapping of common ancestry, but no information as to how or why the languages diverged. As it has been amply illustrated since the time of Schmidt (1872), linguistic change occurs through contact, through the spread of innovation across populations. Hence, some similarities among languages will represent inherited features whilst others will represent shared innovations, acquired through contact (on this topic compare also the chapter by Andersen and Matras). Recent work on languages in contact are claimed (by the Author) to help linguists to sort out the data as archaic or innovative, and to construct a stratified view of PIE accordingly. An updated model which attempts to combine the 'family tree' and the 'wave' models is then proposed. The Author argues that, adopting this new integrated model, it is possible to sort out those morphological similarities (such as iterative, inchoative and intensive affixes) that represent an archaic relationship between the IE languages, from those similarities (such as the development of the imperfect) that represent instead later, shared innovations. The stratification of archaic *vs* innovative structures would allow us to recognize not only that the IE languages are genetically related, but also that some IE languages, like Indo-Iranian and Greek, must have remained in contact longer than others, like Hittite and Germanic. This is testified by the fact that, for example, Indo-Iranian and Greek possess an extremely similar temporal-aspectual system, whilst both Hittite and Germanic have extremely simple verb morphologies. Thus, the shared morphological complexity of Indo-Iranian and Greek would not represent archaism, as traditionally assumed, but rather shared innovation, due to late contacts in the "eastern" areas of IE. In this regard, please compare the chapter by Di Giovine and Kazanas, who support instead the traditional model, although through different corpora of data and different arguments. Compare also the chapter by Schmitt, who argues that (Vedic) Sanskrit is not necessarily the oldest language within IE. Finally, the thesis of late contacts between Greek and Indo-Iranian, according to Drinka, constitutes evidence that the 'Out of India' theory, put forward by Kazanas in this volume, is untenable. However, both Kazanas and Drinka concord in recognizing the validity of IE not only as a linguistic construct, but also as the expression of a real, pre-historical speech community.

Chapter VIII. In his contribution: *The origin and spread of the Indo-Germanic people*, Alexander Häusler states that IE has only a linguistic meaning, and therefore it is methodologically wrong to identify IE or the Indo-Europeans with archaeological cultures, or anthropological types or races. The Author backs his claim through the results of recent archaeological and anthropological research. As a matter of fact, Häusler argues, there is clearly no archaeological or anthropological evidence in favour either of the popular thesis of the existence of a restricted, ancient *Urheimat* or of an *Urkultur* of the Indo-Europeans – see however the chapter by Kazanas, who has a different stand regarding these issues. Similarly, there is

no extra-linguistic evidence in favour of the mainstream, migrationist-type of theories, according to which the Indo-Europeans would have dispersed from their *Urheimat* located in the area of the South Russian or Pontic Steppe. On the contrary, at least with regard to the region extending between the North Sea and the Caspian Sea, there is only evidence for a continual, progressive development of the old-established population and culture – undisturbed by migration and conquest – since the Mesolithic / Neolithic times. On the other hand, the Author observes, the so-called ‘diffusionist’ theses, according to which the old IE population would have diffused peacefully and slowly, due to their attained economical advantages, do not appear to contradict the archaeological evidence. Even so, the (assumed) IE linguistic community is not at all identifiable by the means of archaeological or anthropological research, although it must have existed, since the corresponding linguistic classification has been fairly safely established. This chapter interacts mainly with the chapter by Bryant, Kazanas and Drinka, all dealing (also) with the issue of the IE home land(s) and related migrations, and all holding (more or less) different views.

Chapter IX. Nicholas Kazanas, in his: *Indo-European linguistics and Indo-Aryan indigenism*, argues in favor of the indigenist, ‘Out-of-India’ hypothesis – as against the Aryan Invasion / Immigration Theory – although he also holds the possibility that the IE home land might have been a very wide area, stretching from North West India up to the Pontic Steppe. The Author bases his claim on data and argumentations drawn from linguistics (particularly from the lexical, morpho-phonological and morphological structure of Sanskrit), archaeology and genetics, as well as from the system of cultural and ideological values found in the Vedic Hymns. The Indigenist hypothesis supported in this chapter is in contrast with the mainstream doctrine of the European origin of Sanskrit, which is re-proposed and defended in this volume by Drinka and Schmitt. On this issue please compare also the chapter by Bryant, who argues that there is nothing in Indian archaeology “that in any way ...supports a migration of peoples into the Indian subcontinent during even the broadest of time frames associated with the post-PIE dispersions”. Kazanas also claims that Sanskrit represents the oldest language within the family, in this respect holding a view which is line with the traditional model, even if, in Kazanas’ opinion, the Vedic language and civilization is actually much older than typically assumed within the conventional framework of IE studies: Sanskrit is as old as to even predate the Harappan civilization (but see Schmitt in this volume, who argues instead that there is no sufficient evidence to uphold this claim). Thus, Kazanas believes that the IE theory is well founded, and that an IE language family /speech community did exist in pre-historical times, in this being again in line with the mainstream (and realist) approach to IE. However, Kazanas distances himself from the mainstream doctrine when he argues the following: a) PIE cannot be reconstructed, not only because the widely assumed ‘regular and systematic’ sound laws have not really been established, but also because reconstructions are, by their very nature, highly conjectural and non-verifiable; b) most of the conventionally established correspondences are actually ‘similarities’, that is, they do not, strictly speaking, correspond to each others, but are nevertheless ‘similar’ in sound and meaning. In this respect, Kazanas’ overall assessment of reconstructed IE is in line with that of Marcantonio in this volume, although the interpretation of the relevant data and, consequently, the conclusions drawn by the two Authors differ altogether. In fact, Marcantonio interprets the similarities in question mainly as ‘chance resemblances’, which in turn would be an indication that the IE theory is not, after all, as well founded as widely claimed. In contrast, Kazanas interprets these similarities as ‘cognates’, because the possibility of

chance resemblances or of borrowing is to be excluded given the wide distribution of most of these similarities across Eurasia – but not, significantly, in the Near East. Similarly, according to Kazanas, the well known, extensive similarities of morphological elements and paradigms would be difficult to account for through chance, or borrowing (although there are attested instances of borrowed, wholesale morphological paradigms, as reported in the chapter by Matras).

Chapter X. The main thesis of Angela Marcantonio's chapter: *Evidence that most Indo-European lexical reconstructions are artefacts of the linguistic method of analysis*, is that the methods of historical linguistics, including the comparative method, can be so flexible – by their very nature – that they can be stretched to account for almost any data. This means that the explanatory system runs the risk of becoming dangerously circular, and, therefore, of yielding misleading results – in this case within the field of IE studies. Over the course of about two hundred years of everyday practice of reconstruction within IE the encountered counter-evidence has been typically 'explained away' through all sorts of (often ad-hoc) justifications, so that today one can find hardly any evidence counter to the model. The Author examines this 'explaining away' process critically, by asking the question: have we fitted the IE data to the model, or have we made the model so flexible that it can fit almost any data, including potential counter evidence? Marcantonio analyzes the full comparative corpus of the verbal root reconstructions contained in the most recent IE etymological dictionary (LIV), by adopting simple, quantitative methods of analysis. She argues that the great majority of these reconstructed roots lack the required 'statistical significance', the required 'cumulative effect'. As a consequence, the cognates relating to these roots are to be interpreted as 'similarities', rather than 'correspondences'; in turn, these similarities can be interpreted as instances of 'chance resemblances'. Marcantonio also argues that adding laryngeal segments to the process of reconstruction (whatever the rights and wrongs of the laryngeal theory may be), dangerously increases the explanatory power of the comparative method, in this way further contributing to the flexibility of the overall explanatory model. This chapter interacts mainly with the chapter by Andersen and Kazanas. For example, Kazanas objects to Marcantonio that the scientific method is not applicable to this field of studies – actually, the proper 'scientific' approach would be to ignore rigidity, regularity and uniformity, since the IE linguistic and cultural changes occurred in diverse ways and certainly under no observable laws. Similarly, Andersen believes that scientific proves cannot really be delivered in a field such as this, where scholars cannot observe the objects of investigation directly. Also Drinka and Di Giovine (in this volume) defend the validity of the IE theory, despite their criticism of the one or the other traditional IE tenets. Finally, the reader may want to compare the analysis of the present *vs* perfect *Ablaut* offered in this chapter as against the one proposed by Carruba in his contribution. Both Carruba and Marcantonio point out the scanty and irregular distribution of the vowel alternations across the IE area (with the exception of Old Indian and, partially, Greek). However, Carruba interprets this fact as the fading away through time of a very archaic, IE phenomenon, whilst Marcantonio emphasizes the lack of 'statistical significance' of the phenomenon itself and interprets it as linguistic artefact.

Chapter XI. In: *Defining the limits of grammatical borrowing*, Yaron Matras examines two 'mixed' languages – Romani and Domari – whose populations are known to have come into contact with several other populations and languages in historical times, and it is therefore possible to trace the borrowing that

has occurred. These dialects show indeed extensive borrowing, both at the lexical and grammatical level. Close examination of the borrowed grammatical elements shows that some categories are more likely to be borrowed: discourse markers, connectors, focus particles, indefinite expressions, indefinite markers, and expressions of modality; whilst other categories tend to be borrowed less easily, such as: nominal derivation, plural formation, prepositions, lower numerals, negation and interrogatives. Despite these patterns, no areas or categories of lexicon or of grammar appear to be completely immune from borrowing. Matras presents evidence that case and verbal inflections tend to be borrowed wholesale, if they are borrowed at all. For example, the Greek-derived tense markers in Romani are characterized by a “wholesale import of the entire paradigm, rather than of individual markers, and, in all cases, the imported verb paradigm exists in complementary distribution alongside the inherited inflection paradigm”. This evidence illuminates one of the most striking aspects of IE: how does one explain the ‘wholesale correlations’, spanning entire paradigms, which are observed across some IE languages? For example, textbooks frequently cite tables illustrating the verb ‘to carry, to bear’, which shows amazing correspondences across Sanskrit, Greek, Latin, Gothic, O. Irish, etc., in most forms of the declension of the present tense, as well as in the lexical forms (compare Sanskrit. *bhārāmi*, Latin *ferō*, Greek *φέρω*, Gothic *baira*, O Irish **berū* ‘I carry’, etc.). The verb ‘to be’ also shows similar – lexical and morphological – correlations across the entire paradigm. There has been an underlying assumption, upon which the IE theory was founded, that these patterns cannot be borrowed and therefore they must be inherited. For example, Nichols (1996a) says that these patterns are “individual-identifying evidence”, that is, evidence “obvious to the naked eye of a trained scholar” that the languages are genetically related (this interpretation is supported in this volume by Kazanas). However, there is a minority view, in which Authors question this assumption. For example, Greenberg (2005) and Croft (2005: XIX) remark that these correlations are too good to be the result of inheritance, because of the language change that would have occurred in the millennia involved. Matras’ study provides attested, concrete evidence that the borrowing of entire verbal forms – lexical item + paradigm – does indeed occur. This suggests that the familiar examples of verbal paradigms cited in textbooks could equally well be accounted for through borrowing, and this, in turn, calls into question one of the main, traditional tenets of the IE theory.

Chapter XII. Rüdiger Schmitt, in his: *Iranian archaisms vs. Vedic innovations - and the Indo-Iranian unity*, argues that with regard to the comparative study of the IE languages altogether and to the reconstruction of the IE proto-language it is not sufficient and adequate to refer to the (Old) Indo-Aryan data alone. The wealth and diversity of the textual evidence written in younger Iranian languages as well as the archaism particularly of the Old Avestan language make it only too clear that despite the old age of the earliest Vedic texts the Old Indo-Aryan language is not the only fundament of the IE proto-language, that its data are not with absolute necessity genuinely antique and therefore Old Indo-Aryan (both Vedic and Sanskrit) is not as close to PIE as several people still think. Rather, scholars should take into account the “(proto)-Indo-Iranian linguistic unity”, which is indeed “defined by a large number of phonological, morphological, syntactical and phraseological isoglosses found exclusively in both these language families, including the speakers’ self-designation as *Aryan*”. The Author puts forward a long list of data and argumentations (mainly from the level of phonology), which would show how the Iranian branch often reveals more archaic traits than the Indo-Aryan one. In particular, Schmitt sets himself the task of

drawing the list of those Iranian archaisms “that are faced with secondary changes, and are therefore obvious innovations, on the Vedic side”. This list, it seems, has never been drawn thus far, despite its obvious relevance for the issues under discussion. Schmitt’s findings are at odds with one of the major, traditional tenets of IE studies, that is, that Vedic Sanskrit is the oldest language within the family, and therefore the most important for the process of reconstruction. This tenet is supported in this volume by Kazanas and, to a certain extent, by Di Giovine – the reader might want to compare also Drinka’s chapter regarding the status of the Indo-Iranian branch. In particular, Kazanas draws a list of data and features which, in contrast to Schmitt’s list, would demonstrate how Vedic is actually more archaic than (Old) Iranian, and the most archaic language within IE. Kazanas also points out that Schmitt’s conclusions rely entirely on linguistic data – amenable to a reverse interpretation – and ignore other aspects relevant to the debate, such as evidence drawn from mythology, archaeology and genetics. The claimed archaism of Old Iranian also gives Schmitt the opportunity to consider as correct the mainstream doctrine (re-affirmed in this volume by Drinka and Annamalai & Steever), according to which the Indo-Aryan language must have immigrated into India, rather than having originated there. For an alternative point of view, please compare again the conclusions reached by Kazanas, who, whilst not denying the existence of an Indo-Iranian linguistic unity, argues that this unity must have formed and existed for some time in the Saptasindhu area, whence later on the Iranians left.

6. Conclusion

The reader has seen in this book a variety of views about IE, ranging from the belief that it represents the language of a real pre-historical community; through the thesis that it is only a model to embody linguistic correlations; all the way to statistical evidence that (many) linguistic correlations themselves may be merely an artefact of the method of analysis. In fact, when the various components of the theory are brought together so that they can be seen holistically, it is hard to pin down what the foundations of the theory are actually supposed to be. For example, one of the founding principles of the traditional version of the theory was the assumption that morphological paradigms cannot be borrowed, and therefore it is possible to trace genetic inheritance through them. However, we have seen evidence of wholesale paradigm borrowing, based on studies of languages in contact. In any case, some scholars now hold that morphology is less relevant than other factors – but it is at present unclear whether, or how, these other factors may be verified or falsified. It has been the purpose of this book to bring to the fore these contradictions and open questions associated with the theory. It is for the reader and the linguistic community to decide the way forward.

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TRAVELOGUE

MIRABILE, Paul

Pilgrimage to Tūrkestan over the Three Seas
and Central Asian Steppes



'Les hommes marchent par des chemins divers, qui les suit et les compare verra naître les étranges figures.'

Novalis

*'My dreams grow long, atop Burak, en route to the bazaar,
The world bazaar into which the tormented servant rides;
My head weary, my life oozing, my blood thin,
My name is Ahmet, my country, Tūrkestan.'*

Hodja Ahmet Yesevî

Pilgrimage may strike some readers as an odd word, especially nowadays in our brave new modern world of rapid communications, lightning transfers of money, arms and other miscellaneous toys, giant

screens and speed reading courses. To undertake a pilgrimage assumes a voyage of some divine destination, of some sacred site of a religious nature. It assumes a long voyage! And in fact, the etymon 'voiaig' in Old French meant 'pilgrimage'...

For me, thus, pilgrimage and voyage are inseparable: a bi-unity that traces the Road to one's destination, be it sacred, divine or religious. One may or may not reach this destination, but this unfulfilled desire should not be subject to scorn or to grief : it is the effort of the pilgrim more than the result that the Creator considers ; and this consideration ranks much loftier than any prize or privilege accorded by committees, boards or panels to someone for his or her 'achievements'. I reached Timbuktoo...Phuktal...but failed in Northern Laos. I must happily acclaim that my fourth pilgrimage, a forty-five day voyage over three seas and three countries to Tūrkestan, to the *turbeh* of Hodja Ahmet Yesevî, was accomplished far beyond what I ever imagined ; that is, far beyond my expectations ! Not only did I reach the holy site, but did so in the slowest and longest and most inspiring fashion. I meditated upon the strophes of the Sufi sage's *hikmets*, or strophic wisdom, ruminated and assimilated them as the Road bore me over the sparkling Black Sea, into Georgian mediaeval churches and monasteries, over the oily black Caspian, the steppes of Kazakhstan, the arid sands of the Aral Sea, then into the marvelous mausoleum of the Sufi sage. For it was these bezels of wisdom, these strophes of sagacity that enlightened my mind, kindled my creative imagination, in short, that inspired me to undertake the pilgrimage to the Hodja's final resting home in Kazakhstan.

Who is Hodja Ahmet Yesevî, *au juste*? Also called Pir-i Tūrkestan, the sage of Tūrkestan, he is the founder of Turkic Sufism. Hodja Ahmet of Yese, the former name of the small town of today's Tūrkestan in Eastern Kazakhstan, is the Sheikh, the Master Sufi of all the Turkic branches of Sufism from Central Asia to India, to Anatolia and throughout North Africa. His disciples number in the hundreds: Shams i-Tabrazi, Mevlana, Hacı Bektaş, Yunus Emre, Pir Sultan Abdal, etc. His *hikmets*, or as I coin them, strophes of sagacity, are read by millions of Muslims throughout the world, both Shia and Sunna. As I relate my voyage to the holy site, I shall present Ahmet Yesevî to my readers so that you may partake, too, of this nourishing repast, savoured both in his poetry and legends connected to his life. A repast that carried me theophanically to his *turbeh*, whilst boats, buses, trains and at times my legs, bore me there physically. This bi-unity is indeed what constitutes a pilgrimage, a voyage in all its etymological, historical and spiritual dimensions...

The theophanic region is as vast as the mundane one: fasting (*oruç*), Koranic interpretation (*tafsîr*), dancing and spiritual auditions (*samâ*), silence (*süküt*) and reclusion (*halet*) all form part of the Sufi's mental make-up; it is the 'tassavuf yolu' 'the Sufi Road'. It became mine, momentarily, because Hodja Ahmet Yesevî became my Road Companion, ever since I had begun working in Istanbul...

My pilgrimage required a spirit of resistance, given the fact that the pilgrim in question was over sixty years of age; fit in mind and body, more or less, but nevertheless, over sixty years of age! I make no pretense to virtue, but a pilgrimage is accomplished on one's own, and this in spite of what Chaucer would have thought of that principle! For the Road also becomes one's Companion; it is the pilgrim's intermediary between the Creator and Hodja Ahmet. The Road is the pilgrim's Friend, etymologically from the Old

English verbal form *freond*, which meant 'to love'! In the Hodja's strophes 'friend', 'Dost' in Turkic, is our Creator...He is our Friend in love and thus our interlocutor as we dialogue with Him...His love is unflinching, unflagging...unlike many of our mundane friendships based on common interests, whose commonness may fail and flag, and thus transform into enmity or indifference...that scrouge of the mediocre...

Now my readers may query whether that Creator be the Muslim, Christian, Jewish, Buddhist or Hindu one! Ahmet Yesevî was a Sunna Muslim. And the pilgrim? He is Christian. This discrepancy poses unfathomable questions, and at times sweeps in on me like so many sands of doubt in the desert of Self-Accomplishment. Am I like Odysseus who visited many a city and came to know many a different people? Or the rougish denizen of the world who will tell you a golden tale for a copper? Perhaps I am the Eternal Pilgrim, tramping from one holy site to another, disregardless of religion or creed, quenching my inexhaustible (*intarissable* is better!) thirst from the fountains of all and sundry? I here must assure my readers that never have I been subjected to any spiritual crisis : no galvanic conversion at the fourth pillar of Notre Dame of Paris, no mystical charge of light at fifty-eight ; to have explored and lived Christianity (in many of its denominations), Islam, Judaism, Buddhism and Hinduism especially Sivaism, has never stirred hidden emotions that may trigger a conversion, however exotic these religions may appear to some, whatever esoteric delights their recesses may contain. The Creator is that One for everyone: His form appears different to the Hindu Sivaite and to the Sufi Muslim, yet It is His form that He imagines by means of the pilgrim's Imagination. This is the Creative Imagination, Ibn Arabî's method of communion.

These pilgrimage-voyages have borne witness to my Self-Accomplishing: a commingling of adolescent enthusiasm, existential nourishment and ontic imperativeness towards identities to which I can respond cheerfully, acknowledge confidently, but not necessarily to which I will abide! All religions are founded on Faith in a Creator or a Creation ; a Being whose supremacy obliges us, His bondsmen and companions, to seek Him out wherever Faith is most intensive ; to seek Him out means to emulate Him. And this emulation has its most drawing powers at the holy sites of mankind's traces and vestiges of His deeds, or the deeds of those for whom He interceded : prophets, saints, *veli*, *wali*...*erenler*...No doubt there is empathy on my part, I have no qualms against this. However, there can be no ontic identification with the Pir-i Türkistan, or if one wishes a more technical word, intropathy, on which the pilgrim projects his or her desires (imagination?) upon his or her hero, if he or she does not take that 'ontic leap'! For the pilgrim emulates not the holy seer, but his deeds, in the same way that the Orthodox Christian penetrates, by way of his or her Creative Imagination, the ikon of Christ or of the Virgin Mary and not the paint and its wooden support.

My translation of Ahmet Yesevî's *hikmets* were done en route to his *turbeh*. They follow a line of ontic conduct, and at times intercede in my account to attenuate the gravity of my interior monologue concerning daily activities or very unpleasant worldly events. They also provided a refreshing distraction for a few of my fellow travellers who were tired of reading the rather staid information written in their Lonely Planet guide books or other such literature that cyclists or foot travellers tend to carry with them on such long journeys. I shall print out the original *hikmets* in an annex from my edition.¹ As to the Hodja's

¹ *Ahmed-i Yesevî Divânı, Hikmet'ten Seçmeler*, Professor Dr. Kemal Erastan, Kültür ve Turizm Bakanlığı Yayınları, İstanbul 1983.

life, or what is known of it, my stories have been gleaned from that same edition of his *hikmets*, and from the Atatürk Revue, *Erdem*.² These were my two reading companions, plus the Second Testament or the Evangiles if you please, where I reread Paul's Letters, not due to any geographic conjunction, but rather from his steadfast movement from one town and country to another, an extraordinary élan motivated and generated by Faith. Besides these three books, I read no other during my forty-five day pilgrimage. The languages that I needed to communicate with in these countries, in the order of their daily usage, were: Turkish, Russian, English and French.

I have always adhered to the belief of or in parallel worlds, of or in the multiple existences within one's lifetime upon our Earth. This belief lies not in fantasy, but in Reality. Since childhood, no one has yet deterred me from this belief. And there are many, too, who evolve within these parallel worlds; I have met quite a few, and several on this fourth pilgrimage. I failed to note our lively conversations, indeed failed to keep any daily record of my adventure on paper at all. I set off with five kilos of clothes, those aforesaid books and a German map of Central Asia all stuffed in my backpack. But no travelogue! My translation of the Hodja's *hikmets* was written out on the photocopied edition of that sagacity, consequently, all my meetings with travellers, pilgrims, shopkeepers, hostel-owners, restaurant workers, street-cleaners, railroad and bus civil servants, sailors, etc, etc, have been taken from memory ; none, however, is fictitious...And yet, is it not fiction that opens the wide horizons of Reality ? Hodja Ahmet's *hikmets* and life lie at the threshold of these two realms, *entre chien et loup*...at those parallel worlds of which I spoke and will speak again so praisingly...

Thirteen years in Turkey, the majority of which spent in teaching, have taught me that at the university, at high schools or translating at the Inkilap publishing house of Istanbul, History in this country, in this city is never an object of the Past but a subject of and for the Present! Those who study History must contemplate the Present circumstances: the Past will never be partially understood without a firm grip of the Present, without the philologists' eye for detecting the variantes in order to unite analogically the commonalities into one flexible dynamic Whole. The Philologist-Historian wanders in the shades of the twilight hour, *entre chien et loup*, in which the Present and the Past slide and glide inseparably, silently within one another, shift from one to the other like the sinking sun over Aya Sophia and the Blue Mosque.

The Hodja also slides and glides within the framework of Present and Past, within the contours of History and Legend, for History is an ensemble of striking and coincidental contrasts. Antagonistic contrasts, sable colours that brighten one's Path, flamboyant colours that darken it, too. The discovery of Hodja Ahmet Yesevî painted itself into those stark and subtle colours, out of whose flow the obsession of my pilgrimage took an undulating but affirmed shape. A slow but steady flux which had begun with the study and translation of the *Book of Dede Korkut*, with the readings of Yunus Emre, Mevlana, Eşrefoğlu Rumi, Pir Sultan Abdal, Hacı Bektaş, the Sheikh Bedreddîn, Evliya Çelebi, and of course the Master of all those aforesaid Masters, Hodja Ahmet Yesevî.

Hodja Ahmet Yesevî, or Ahmet of Yese (the suffix -vî being the locative in Old Turkic, the modern suffix being -ile), was born in 1150 in modern Kazakhstan, in today's Tūrkestan, called in medieaval times, Yese.

² N° 21, Nov. Ankara 1995.

Others put that date at 1093. His Sufi Master was Arslan Baba, under whom he studied in Bukhara. He left that pious city and returned to Tūrkestan where he effected his 'earthly' death at the age of 63, Muhammad's age of death, by digging an underground cell where he lived until 1200. But this date is conjective; some doughty historians put his death age (earthly or divinely) at 120! Be that as it may, Kocka Axmet Яcayy in Kazak, Хорка Ахмега Яcави in Russian or Hodja Ahmet Yasawi in Latin letters is the Dawn of all Sufi lights that enlumine the shades of our most arcane nooks and crannies. It is his life and writings that fortify our Faith in mankind, that shape a defense against fanatical Wahabism and Salafism, that sharpen our wits and deepen our religious Faith against those zealots who have deliberately discontinued Islamic growth, have ignominiously suspended Islamic beauty, theology, philosophy and law, framing Islam into a golden age of the immaculate commencement, which has been sullied and fouled by the deceit and duperie of Shia heretics, by Jewish and Christian Islamophobes, and worst of all, by zandaqa atheists !.. It is Islamic Sufism that will challenge and defeat the tyrannical bigots of Saudia Arabia and Pakistan, and their vassalized henchmen round the world, and that challenge will emerge from Central Asia, from the Turkic countries of Central Asia, from the flourishing universities and schools of thought that defend the universal message of Hodja Ahmet Yesevî, his high places of sainthood (ziyade), his very much present geneology of disciples (ocak) or (silâsila), that Sheikh-chain of heritage. I believe in this Sufi revival, and for this belief I ventured to his tomb (mazar) at Tūrkestan, out of which these timeless strophes of sagacity slide and glide between the spaces of the crepuscular:

*'Hey friends, lend an ear to what I say,
Why ever at sixty-three did I enter the earth?
Fair Mustafa upon his ascension saw my spirit
For that reason at sixty-three did I enter the earth.*

*Fair Mustafa asked of Gabriel
How can this spirit find God without entering the body?
With tearful eyes the circle was cleft, the crescent neck bent;
For this reason at sixty-three did I enter the earth.*

*Gabriel said: "the Umma is the just one for you;
Take lessons from the angels that descend from the firmament;
The seven layers of the firmament with groaning groans,
For that reason at sixty-three did I enter the earth.*

Indeed at Mohammad the Prophet's age of death Hodja Ahmet, too, left this earthly world to dwell in *barzakh* -the isthmus- in the intermediary world between God and the human, a subterranean world that wafted his body, soul and intelligence to the Highest Heavens, *Arş* ! His burial rite, seemingly his own, was ordained and accompanied by the Creator. Likewise his ascension, perhaps some sixty years later at the age of 120, the age of Abraham, the Father of all Jews, Christians and Muslims...

Accompanied by these strophes of sagacity, the *hikmets*, I resolved to voyage as slowly as possible, as passively as possible, exposing my Self to the hazards of the wide Road and to whatever the encounter or obstacle be, find a suitable solution, be it visas, transport, disagreeable or unhealthy encounters with fellow travellers or the indigenous, health reasons...And I must confess now that none of those aforesaid hazards of the Road brought me ill-fortune, quite the converse occurred : my passive persistence and full acceptance to my surrounding circumstances of exposure favoured a florilege of delightful events ; the very first being a marvelous sea-crossing on the Black Sea from Istanbul and Varna to Poti...

My ship this time was the Bulgarian freight carrier Героите на Севастопол 'Geroite na Sevastopol', built in Norway in 1979. It cruised at 19 knots when fully loaded with train cars, lorries and passenger cars. Once loaded after a day and a half of patient waiting, the Geroite na Sebastopol left port after noon, and slid gently out of the Belaslava channel into the Black Sea. To our left, the beaches of the sea-side resort where I had spent most of my time, the sand being soft, the sun hot, the water refreshing and clean, the food, especially the prawns and salads, excellent and rather cheap. At the bow I observed the sailors gesticulating to their wives and children who were aligned on the banks as our ship left the calm waters of the channel behind and penetrated those more agitated ones of the Black Sea. The wind suddenly picked up and the movement of the ship with it...

Yes, with cargo ships one must practice the art of Patience. Patience is that principal catalyser of a Voyage. Indeed, even before you board your ship you become immersed within the intense flux that bears you towards that patiently awaiting sea-companion. The port personal, the bustling crew, the colourful cargo loaded or unloaded, the engines dormant or churning, the smoke abellowing or wafting from the twin stacks in thin wisps of curly white ; the meals and of course the sea, either in port or during navigation. These events or should I say, this poetry, act as a series of successive links on a chain: the anticipation and the actual crossing; the rolling and pitch, the conversations and the salty silence. The seagulls on the wing or in gaggles on the rotting moles, the sparrows romping about the bow, the dolphins at play, scratching their backs on the hull of the ship on the high seas! The sun, then the moon and the stars...The azures of a sky that merge into the blueness of the placid waters: the hues of this scene exhilarate the spirit, stimulate the imagination, they remind the pilgrim that whatever the delay, Nature soothes and redeems any regrets or misgivings...There is nothing more elating than leaning on the ship-rail, inhaling and exhaling the briny and robust air of the open sea, inhaling and exhaling the tension of our creative élan...

The Geroite na Sebastapol's daily menu: bread, butter, jam, boloney or salami, eggs and tea for breakfast. Sausages, boloney or salami, mackerel soup, potatoes, carrots and tea for lunch. Vegetable soup, sausages, salades, pork or beef for supper...but no tea, and never any coffee! Odd really. Perhaps because the Bulgarians drink only 'Turkish coffee'...A very undietetic menu, I must admit, but Eastern Europeans, Soviet-orientated or not, have never been fervent admirers of the 'new cuisine'. The sailors were all overweight, much too much ruddy-cheeked. Too much red meat, cold cuts and potatoes. Too much bread. Every dish lay smothered under layers of salt, that burner of body cells! Now and then apples, pears and oranges were carefully placed at the left of our plates; a refreshing alternative to the rubbery salami and boloney. But these are mere observations, not complaints!

Four days and nights of drifting upon the still waters of the first sea to be crossed. Conversations were rythmed by the droning of the engines, measured by the activity of the crew or the inactivity of the seven passengers: Armenians, Azeri, Bulgarians and French...On the fifth morning of the crossing the port of Poti rose slowly into sight. We glided in as smoothly as we glided out of the Bosphorous Straights in Istanbul and the channel of Belaslava near Varna. The Getroite na Sebastpol was moored to the wharf round noon...

Georgia is a country of republics, now independant, now annexed to the capital Tblissi; some severed completely, others unshakeable in their adherence to the Mother City. The Christian Orthodox and Islamic problematic has torn this country apart since the arrival of the Seldjuks in the XII century.

It is also a land of endless nationalities: Jason's Greek argonauts crossed the Bosphorous (the Clashing Rocks?), ploughed the Black Seas and arrived at the tenebrous shores of the Colchis in pursuit of the Golden Fleece, which they shamelessly stole from Medea's father. The roguish Greeks payed dearly for that shameful theft, but some of the crew must have stayed on because Greek is still spoken in Abkhazia, presently a new region in Putin's ever-expanding Federation. This break-away republic in the North-West of Georgia is today prohibited to Georgians! But not to tourists, especially Russians! As to the former Republic of Adjaria, of which Batum is the capital and largest port, it was ruled by a Muslim dictator until 2004 who has since fled and is currently *en cavale*. The population of 400.000 has suffered many a Persian, Turkic, Russian or Soviet invader...

Upon arrival at Tblissi, I secured my bedding in a non-descript private home transformed into a guesthouse when the fine woman owner needed extra money, located in the back lanes of Old Tblissi. Once unpacked and settled, I set out immediately to deal with my visa formalities, no easy task when one must enter Azerbaijan without an invitation...In fact, I needed only a few days in Baku to get a cargo out to the port of Kazakhstan, Aktau...Would they oblige me to procure this ridiculous invitation ?..

Indeed, during the day, when I was not running between the Kazakh and Azerbaijan embassies, I would read or meditate in the charming, peaceful garden at the Metekhi Church, perched high on a hill overlooking the Mtkvari River and the Maidan. The cool shade, excellent drinking water from the fountains, the beautiful cross-cupola mediaeval church (although at that time its façade was under restoration), built by King Demitrius in the XIII century, whose habitués came and went quietly, either alone or in small groups, and the quaint wooden café at the back entrance of the garden, granted me hours of tranquillity. I especially became attached to three different sized campellas hanging from a wooden portico at the front of the gardens, which overlooked the modern section of the city. To look through them out into the blueness of the Tblissi sky gave me the impression of an indefinite suspension of Time and Space. At night, seated in the alleyway in front of my little room, I would listen to my neighbour play Chopin and Bartok on her untuned piano, or squabble politics and economics with Sergo, the owner's cantankerous, university-graduated son.

One bright, blue-skied morning I took the metro at Independent Square to Didube Station, and there at the huge minibus terminal for two *lari* caught a *mashruka* to the mediaeval capital of the kingdom of Georgia, Mtskheta, about 15 or 20 kilometres from Tblissi. There is no doubt that Mtskheta emits a Sense of the Past : the prodigious Cathedral of Svetitskhoveli is a mediaeval gem, true, surrounded by western-styled cafés and souvenir shops, as could be unfortunately expected, but nevertheless, lovely to behold, enclosed like a ruby in its bezel, within its restored mediaeval walls. To penetrate the hollowness of the cathedral, shoulders must not be bare, nor should the visitor wear shorts. A woman's hair must be covered, although the priest at the door, a dwarf, did not implement this last breach of decorum. The stone basilica foundation dates back to the fifth century: the actual cathedral today was built in the eleventh by King George the First. The legend narrates that King Mirian, the first Christian king of Georgia, had a stone church built on recommendation of Saint Nino in 360 (incertum) because it was on that very spot that a woman named Sidonia had been supposedly interred along with Christ's robe, bought by her Jewish brother from a Romain guard at Golgotha and given to her. Another legend reports that a Lebanese tree was cut down to make room for the basilica from which seven pillars were hewn to support the awe-inspiring structure, and whose biggest pillar was endowed with divine properties due to Saint Nino's ardent prayers; it emitted a special light and heavenly aroma, and when bathed in this divine light or when touched, cured many maladies. This became Sveti Tskhoveli; that is, the 'Pillar of Life', the name then adopted for the holiest of churches. Then I turned my efforts to explore the Jvari Monastery some fifteen kilometres from Mtskheta, but which can be seen clearly from the cathedral...

The approach to the hilltop monastery is an awe-inspiring experience: No wonder this monastery, and the Svetitskhoveli Cathedral nestled below it, have always been the sacred heart of Georgia: mediaeval and modern...I read out these dullcient strophes to commemorate my arrival:

*'At sixteen all the spirits answered:
"May you be blessed" said Adam arriving;
"My son!" he said; he hugged my neck and enveloped my heart,
At seventeen, thus, I found myself in Tūrkestan.*

*'At eighteen I drank water with the Forty,
I invoked God, I pierced my bosom, stopped and made ready;
Sought my fate, strolled through Paradise, hugged the houris,
Thus I saw the beautiful face of Mohammad the Just?'*

Another bright morning, after I had secured my visas for Azerbaijan (a five-day transit visa) and Kazakhstan (a full month), I hitch-hiked to King David's Lavra Monastery, a forty-five minute ride from Tblissi to Sarejevo. From there, a sixty kilometre road, twenty of which being a mélange of broken, pot-holed asphalt and earth-packed one, winding round and over barren hills and ochre-coloured fields leads you to the monastery. The landscape reminded me of the veld-lands of South Africa. I would never have imagined that the mountainous and forest infested Georgia possessed such a solitary, desert-like countryside. Indeed, this extraordinary monastery is located on the Azerbaijan border in the middle of a desolate landscape where neither tree nor plant grows, where the rudeness of the monastery surrounding

walls, towers, stairways, chapels, gates and cells blended so nicely into the surrounding barrenness of the wasteland. It is said that the majority of the Georgian monasteries were founded in this southern region during the VI century, then restored or rebuilt during the IX century because the monks desired absolute solitude, far away from the marketplace of daily life. Hearty souls those maiden preachers, and only the tears of the praying King David, which transformed the rocky bed of a low-lying depression into a brook for the monks to drink from, offered solace to them...

I have always approached holy lieus either by circling them first, then slowly penetrating their sacred walls, or from above if the relief of the landscape allowed such a panoramic approach. Afterwards I descend into the vortex of mediaeval sensations...What are these sensations? The Sense of the Past? An escape from it? An exercise in empathy? A parenthetical dive into one's awareness of his or hers utter mediocre existence and the nostalgic thrill that may be therein wrenched and disgressively savoured? To touch and to be touched: that is a genuine sensation wrought from empathy, wrought from a Voyage of Penetration into the vortex of mediaeval sensations. For are we not all sentient beings?..

My last three days in Tblissi were spent visiting the Catholic and Orthodox churches, the synagogue, and the mosque with its adjoining hamam and gardens. I bought my ticket, too, for Baku at the bus terminal in order to secure a place in the *mashruka* that would deposit us in a small village near the border before hopping on the bigger bus for the Azeri capital. And so when I finally sped off to the Azerbaijan border, I felt rather melancholic at the sight of the churches, bridges, art gallery and parks and gardens that slid by my glazed window. I had grown used to Tblissi. I had found it to be a refuge from the car-riddled and horde-infested hell of Istanbul, of all those formless or deformed populated metropolises of the Orient that twenty or thirty years ago had been livable and enjoyable cities. True, certain sections of Tblissi pampered to mass tourism, and have suffered the gross setbacks of feigned façades of cosmetic face-lifting and rouged window dressing. Yet, Tblissi breathes a certain charm, a certain continuity of poetic prowess, be it mediaeval or modern...

I arrived in Baku at one o'clock in the morning and slept in the bus terminal. At the crack of dawn, I went in search of the port from which I would catch my boat to Kazakhstan. I was not in Azerbaijan for more than five days, for indeed if the tourist overstayed his five-day visa he or she was fined five-hundred dollars...

There are two ports of the oil-rich city of Baku: a large, bright, modern one and the small, dilapidated and archaic one that lay a kilometre or two from the first. That's where I presently bent my steps...I galloped across a lovely park which hugged the Caspian coast, then darted into a narrow lane or alley where rusting railroad tracks littered with cans and rotten food and out of whose fissures moss and lichen grew wildly, led me directly into a small square. Two hangers, both made of metal, stood silent in the cool air of the white dawning. There was no one in sight! Just then a guard shot out from his diminutive guardhouse and pointed a finger to the hanger to my left, knowing perfectly well why I had stumbled into that square at six o'clock in the morning. I pushed open the creaking metal door and found myself in a vast, warehouse of some sort. The cement walls were naked and the cement floor without carpet or kilim. In the far left hand corner of this ghastly hole an elderly woman was hunched over a hackney metal table,

alone. I mechanically shuffled over to her across the void; she hardly lifted her head from an open logbook in which she was penning something down. When I began to speak to her very politely in Turkish, her head suddenly bobbed up like an apple in a water-filled barrel. Thrilled that I spoke Turkish, she informed me that the *Shah Dagh* was now in port and about to leave. My ticket secured, I raced to the port a half kilometre down the road, alongside the railway tracks. Alas, the customs officials would not let me through since my visa for Kazakhstan was to begin in two days...I would thus have to wait for another boat. The kind policeman, noting my anguish at this idea, pointed to a strip of embrowned trees and told me I could procure a bed there instead of walking back into the centre of Baku, some three or four kilometres from the port. This is exactly what I did...

The long narrow strip of huts and shacks lay between the railway tracks at the foot of the moles. It was divided into plots or enclosures of more or less five or six metal- or wood-made huts, all connected to one another by a earth-packed track. Withering trees and shrubs provided some shade from the blazing sun, whilst parasols, too, had been placed over broken picnic tables, stools and ripped upholstery armchairs or canopes that lined the dirt track. Some proprietors had built bowers and arbours which arched over their own enclosures, and there they sat in conversation, or in reverie of a better world. I dipped into the first enclosure where the woman owner said that no bed was vacant. She pointed to the second enclosure, two or three metres away to my right. There the woman, who spoke excellent Turkish, agreed to give me a bed with three other men if I gave her seven *manats* : I agreed and she showed me my bed : a squeaking iron-framed thing with a lumpy mattress whose sheets and pillow-cases dissimulated not hardened yellow stains and numerous holes, girt with frayed, brown edges, probably made from cigarettes. Two men were sleeping soundly; the other bed, identical to mine, lay empty. The air in the shack was sultry, neither ventilator nor fan to clear it out. -What would the night herald with its clouds of mosquitoes?- I wondered, mirthlessly.

And mirthlessly I sweated it out for two days with the Azeri refugees from the Karabagh war and with the immigrant workers from Uzbekistan and Turkmenistan. Finally, on my second night, informed by the old woman in the hanger, I boarded the *Karabagh* at two o'clock in the morning...not a sailor in sight! It was a huge ship, and I felt as if I were atop some skyscraper. At the prow, however, a small, bright light guided me along the upper deck. To my right, the Flaming Towers of Baku burned away glibly like two midnight lamps amidst a hollowed, black, humid void...I stepped into the pilot's cabin without ceremony, where three sailors were eating olives and bread, drinking large cups of tea.

«Selamaylekum» I bellowed. They all shot me a surprised look, and responded accordingly. One of them, short and pudgy, offered me a seat and olives. Another poured me a cup of tea. The third, an unshaven middle-aged man with jet black hair and eyes asked for my ticket, all crumbled up in my vest pocket. They hadn't expected a passenger aboard before dawn. Nevertheless, the second in command, the short, pudgy one, asked me to follow him to his cabin where he wrote down my name in a log-book, then showed me my cabin. Before leaving me, he sullenly explained that meals were included on board, and told me the hours. I would be shown the dining hall in the morning at breakfast time. I had a stroke of luck, many Caspian crossings require the traveller to bring aboard his or her own food; I had a few sandwiches and some fruit...

The *Karabagh* left port about three o'clock in the afternoon. It steered a North-eastern course amongst the dozens and dozens of oil-platforms that pecked the placid, nacreous waters, rising high in overweening pride. My cabinmate, a Dutchman named Gustav, was en route to Indonesia on bicycle. Aboard, too, was a young German couple hitch-hiking to India. We took our meals together in the canteen and discussed travelling in general. On our second day out at sea something quite odd occurred: In the middle of the Caspian, more or less, on a northerly route where no oil-platforms could be seen, the crew had opened the valves at the bow and Caspian Sea water was rushing onto the deck, there forming a huge swimming pool! Several of the crew members were already knee-deep in the swirling, clean waters, titillating with excitement, either in their underwear or bare-chested, trousers sagging low round their loins. They were soaping themselves up in front of the valves where powerful gushes of water proffered a wonderful massage, whilst rinsing off the sweaty skin. I stripped down to my shalvar, climbed down the few rungs of the ladder into the swishing currents: the second-in-command threw me some soap. This proved an excellent occasion to wash and bathe, for at the port of Baku there was no shower, and the one in our cabin reeked of a powerful stench much too strong for my nostrils; I dared not step barefoot onto the rust filled, grimy floor of the shower, nor touch the walls, thick with slime and muck. The captain jumped into the rising waters in his underwear; he waved to me to fetch the other passengers. I climbed out, darted to my cabin where Gustav was fast asleep; he had forgotten to remove his boots! He opened a sleepy eye and shook his head at my beckoning...I then found the young German couple, who tentatively followed me, but when they inspected the 'swimming pool' (I think they believed that the ship actually possessed a real swimming pool), their faces became fraught with that conundrum expression that wed's the wary with the awe ! They smiled perfunctorily and simply watched us, although at one point the boy ventured in, trousing up his trousers to the knees. But when the crew mischievously began to splash him with water, which they had been doing to me, he climbed out...

As the sun sank, setting the skies and the sea ablaze, at the ship's railing I explained to Gustav that to the North of us lies the Volga River Delta before it transforms into the Caspian Sea, and that it was by way of the Volga and the Delta that the XV century Russian merchant from Tver, Afanasya Nikitin, had voyaged to Persia and then on to India. His four year voyage *Over the Three Seas*, recounts his Oriental encounters, at times garnished with minute detail, and his probable conversion to Islam whilst sojourning in Western India. His ships upon the Caspian had been pursued by the Tartars of Saray (Astrakhan), and were accosted by them, who attacked the Russian crew with arrows. Then his smaller boat was pillaged, but our adventurer managed to escape thanks to a small bark, sailing quickly out of the mouth of the Delta into the Caspian, or the Siroan as it was called in the Middle Ages. Alas, the bigger boat, too, was accosted, and the Tartars made the crew land near a wooden barrage. As soon as the Tartars released the two smaller boats, Afanasya and his crew steered for Derbent. Again misfortune befell the luckless Tver merchant: a storm broke out and smashed the smaller of the two boats into splinters on the shores of Daghestan. After much hardship and irksome negotiations, Afanasya reached Derbent where he arranged for supplies and means of transport to continue his voyage to Baku, and there set sail for Persia...

At the outset of Afanasya's voyage, his account lacks geographic, ethnic and even factual precision. But once in Persia and in India his journal thickens with vivid details and savoury anecdotes. This little known

voyageur appealed to my instincts for the ontic and existential movement that transforms the voyageur as he or she travels on the Road of Life. For this reason I dedicated my pilgrimage to him...

The second-in-charge touched my shoulder and pointed East; there lay Aktau, the Caspian port of Kazakhstan. We would moor in two or three hours if a pier was available. It wasn't...

For six or seven hours we waited on the open sea. The fine yellow line of the shore stretched out before us. The only relief was Aktau proper and the tiny port. Aktau means 'white mountain' in Kazakh, and it was the Soviets who transformed it into a thriving port which linked the oil-filled Azerbaijan, the caviare canning factories of Astrakhan and the petrol refineries at Krasnovodsk (*kızıl su* 'red water') in Turkmenistan, today, however, called *Türkmenbaşy* the 'Grand Turk', which apparently refers to the actual dictator, Niyazov.

Indeed, Aktau holds no magical interest for the visitor, even one with a strong imagination. Its housing and buildings are very Soviet in appearance, besides the centre of the town, more transparent than opaque! The streets are very straight and clean. Even in the older districts, near the market, nothing 'ancient' or 'hoary' transpires from the dull frontages. I was not disappointed, mind you. In fact, I expected nothing since I had not read one line about this port town. The 'old' mosque was indeed older than the bright new one, which I had entered to pray and give thanks on two occasions since no church seemed to exist at Aktau, albeit I never really investigated. On the other hand, one day I left the main avenue in the centre of the town, swung left towards the Caspian and happily discovered a promenade with cafés and small restaurants, and most of all, beautiful sandy beaches which were free of charge. Many Kazakhs and Russians strolled on the boardwalks or drank beer or soft drinks at the make-shift cafés. The breeze off the Caspian lowered the temperature, and the colours of the unruffled sea dazzled bright. Loafing about the promenade, I worked my way back inland, and at the centre found an agency that sold train tickets. No ticket for Aral'sk was procurable for the following day, nor for the next four of five days. What was I to do in this boring port town: loiter about the impeccably clean streets for five or six days? The woman noted my anguish and proposed that I buy a ticket for Oktjabr'sk, in Northern Kazakhstan, then three hours later, catch the train from Aktjubinsk to Alma-Ata.

These formalities terminated, my last night in Aktau was quite a sleepless one, smashing the myriad mosquitoes upon the blood-splattered plasterboard walls of my room at the Kiremet Hotel. Groggy-eyed the next morning, I left for the railway station, ten kilometres from the Kiremet, caught my train, passed a relatively pleasant day and night on it with some very delightful passengers, then caught the second train to Aral'sk town, and again as luck will have it, slept in a couchette and conversed with passengers and the woman in charge of the car the whole day, and half the night...

The heat was already on the rise when I stepped out of the train and walked the twenty minutes necessary to reach the tiny centre of Aral'sk, where the only hotel in town, the unimaginative Aral Hotel, stood on the edge of the once shimmering waters of the Aral Sea. The woman at the reception (there always seemed to be women at reception desks), pulled a sour face when she noticed my presence in the dank and damp hall so early in the morning. The large hall bore that mass Soviet demean about it, and yet something heavier, more oppressive (is that possible?) seemed to bear witness to other ideological or

ethnic strata; it might have once been a classy hotel for the Alma-Ata or Moscow officials in the heydays of Aral'sk; that is, when fisherman and factories combined to enrich this town in the 1920s, 30s and 40s. To this effect, there is an interesting fresco at the Aral'sk train station cafeteria which depicts Lenin saluting and thanking the fisherman of Aral'sk who are brawny and cheerfully casting their loop nets and lines into the bright, fish-jumping waters. Lenin's arm extends to those bare-armed, barrel-chested men in comradely gratitude...

Today, the sea lies practically empty besides a few large stretches of lake-like waters, huge puddles and sinewy rivelets that creep languidly into welters of reeds and groves of Willows. Rusty cranes and dilapidated canneries hang limply off the eroded and barren banks where I stood. To walk upon the muddy soil and examine the few trawlers stuck in the mire, bows upright, instills a sort of fear or awe as if one were awaiting an ineluctable disaster, or a survivor of a world catastrophe... Within the lush green gardens that sloped into the sandy sea there was a museum which boasted a perfectly intact trawler that had been left stranded in the drying sea, and which the authorities have since transformed into a showpiece. To reach the top deck, I climbed a ladder in the interior of the museum, and once at the top found myself just under the pilot cabin, high enough to be able to gaze out far over the forsaken sea. The sight, although tragic, triggered a rush of energy: had I not gained the third sea? Had I not equaled those three seas of Afanasya? Had I not bent my step over his, more or less? Transfixed to the marooned boat, from afar, apparitions emerged amongst the groves of Willows, muddy marshes and hillocks. What were they? Real figures romping about the swamplands...or mirages? The oppressive air bleached the sky a blinding white; a train whistle screamed. This sea no longer could be called a sea, but a graveyard of iron dinosaurs and bashed economic systems that ruined an industry and the lives of thousands. Who will pay for this ecologic and human crime?

I spent two days in Aral'sk, then on a slow train arrived to Tūrkestan in the very early morning of the 44th day of my pilgrimage. I walked briskly the few kilometres that separated me from the holy site, ignoring the cars that offered, for a meagre price, to drop me off there; as I've always said holy sites must be gained on foot...and only on foot! And there it was in all its majestic glory as I ran along the campus ground of the Ahmet Yesevî University and past the very expensive and modern university hotel...

A huge weedy lot separated me from the crown; a crown that outshines all others in Central Asia. A spectacle to behold, even from behind, because indeed I attained the *turbeh* from behind. Even the Soviet atheists were subdued by its stunning beauty! *Quelle merveille! Quelle merveille!* I cried out: «Labayk! Labayk! Labayk!», that Mansur al-Hallaj cry as he penetrated town and pilgrimage site, prohibited to ordinary Muslims because pronounced only by Mohammad, the last prophet. I was not a Muslim...so...»Labayak! Labayak! Labayak!» wafted my cry into the azure which girt the ever approaching *turbeh*. *Labayak*, by the way, means 'Here I am!' I personally prefer the French translation: *Me voilà*. Whatever be the better translation, there I was...finally, overwhelmed by joy and fatigue, by awe, delight and relief...Truly, the gleam of the double-domed *turbeh* filled my eyes with tears, my mind with wild confusion. I think I had gone a bit hysterical for a few moments; my dusty sandals and frayed trousers cut a poor sight, methinks, because passing pilgrims glanced at me oddly. I shouted out the Hodja's name two or three times to them. I touched the walls when I reached the *turbeh*, raced to the majestic front

entrance, and there stood in ecstasy at the portal, presently uncluttered by clumps of tourists or pilgrims, yet teeming with religious fury: *furor poeticus*? Was it the portal or me? I rationalized not at such inept solipsismes, and progressively penetrated the space of the holy...The façade, constructed of fired bricks mixed with mortar and clay, soars skywards from a square base which acts as that ante-chamber, that narthex or jamatoun, before one penetrates a sacred lieu. The depth of the entrance itself, from the front court to the threshold of the wooden portal studded with huge iron nailheads measures no less than twelve metres. It would have been an admirable spot to scrutinize the slithering ochre forms which delimited the façade laterally, coiling up in swirls of geometric relief in guise of pillars, if it hadn't been for the hundreds of pigeons perched on rotting beams jutting out from the mud brickwork, cooingly bespoiling the entrance with their mindless droppings, the rancid odour of which pressed the most perseverant pilgrim to enter or exit! The oblong protruding beams gave the impression that Timur's inspired labourers had forgotten to dismantle their scaffolding. Six buttresses on each side of the *turbeh* bolster the inlaid Persian blue and vermilion glazed tiles, which depicted vegetal motifs and geometric patterns. Girdling the uppermost part of the ribbed dome were Kufic inscriptions, grand and glossy. The back entrance that led into the Hodja's chamber of eternal rest was shut. One must step back a few metres from the *turbeh* to encompass wholly the immense central dome made of brick, which crowns the main hall. It measures eighteen metres in diameter and is the largest in Central Asia. The smaller ribbed dome which bedecks the prayer room where the Hodja's sarcophagus lies, is resplendent with golden and apple green enameled tiles, whilst the larger dome scintillates lapis-lazuli and turquoise ones...

I stepped into the saint of saints, the sacrophagus of the Pious One over whom the enjewelled ribbed dome rose in glorious triumph! Pilgrims prayed in union, whispered confidences to the Imam; both men and women had covered their hair, men with skullcaps, women with scarves. I quickly covered mine with the hood of my soiled jacket. I took a seat, ever so self-conscious of my ungainly weeds, pulled out my *tesbih* -rosary beads- and murmured:

*Here lies the Amânah, the divine Deposit,
the sacred Heart that bridges Light and Darkness.*

*Here lies the hidden Treasure of the Ocean depths,
lieu of the myriad Epiphanies.*

*Here lies the Summit of the Mountain,
where all Ways converge, and none dare proclaim the highest.
Here lies the untainted, the unruined, the untarnished
Mirror that reflects the Face of God.*

Here lies the Heart of the Universe...

When I had finished, I stepped outside into the blazing white of the front courtyard. The Imam of the Hodja's chamber stopped me, and for a while we chatted in Turkish. He had studied at the Ahmet Yesevî

University. He invited me to break the fast *-iftar-* that night with him, his best friend Ahmet, who also graduated from the Yesevî University, and other guests at his home if I so desired. I agreed without hesitation, and we made a rendez-vous at the *turbeh* portal in the late afternoon.

Thus, I was met by the Imam and his friend, Ahmet, at the portal before sunset. We crossed the front gardens of the enormous park that girt the *turbeh*, exiting through a gate of the enclosure walls, very recently rebuilt, and lit by a series of multi-coloured spotlights. We carried on for quite some time, passing the street where my hotel was located, then into a maze of side-streets until reaching a courtyard. On the verandah of a rather large one-storey house, we removed our shoes and penetrated the imam's home, lieu of the *iftar* on that particular night. I was immediately ushered into a huge room, flanked by Ahmet and the Imam, who presented me to the forty or fifty guests, the majority of whom were men, seated upon cushions on both sides of a very long *sofra* -low table-, set with a bright white tablecloth upon whose sheen bowls of fruit and fried dishes had been lain. The few women present, all gathered at the far end of the table near the kitchen, and the two or three screaming children, the Imam's, I presumed, greeted me with smiles, whilst the men welcomed me with ceremonial formulae. After the formal presentation, I was seated opposite Ahmet, and alongside another Imam, who only spoke Kazakh. Ahmet thus acted as my official translator. At the head of the table was the chief Imam of Tūrkestan; he was tugging at a tangled beard as he listened to the men gathered around him. Another Imam was sprawled out on a divan just to the right of the long table. I noticed that there was not one chair in the room...

The ladies of the home swarmed out of the adjacent kitchen, bearing vessels of fresh fritters, steaming hot. As they lay the vessels at different points of the table, I took cursory glances at the walls : completely bare, save a clock with Arabic writing (those that are sold cheaply at the Mecca!) and which read : 'Allah be praised.', a formula written on all clocks from Istanbul to Saudi Arabia...To my left, propped against a wall whose paper was neglectingly peeling, a long bookcase overflowed with books, pamphlets, photo albums, from which dangled Persian-blue enamelled evil eyes to ward off either illiterary or coyish djinns, and a small, square, thread-bare carpet depicting the Ka'ba. There was absolutely nothing else. Suddenly a silence befell the guests; the tall, sinewy Imam, host of the house, rushed into the hall to announce that the bell had been sounded, breaking the fast for that day...

Now *iftar* in Turkey and in Kazakhstan, as I was soon to learn, are very distinct. Restive fingers plunged into the fruit bowls and vessels of honeyed fritters. Bowls of soup followed abundantly. Then a sudden silence froze everyone in their gestures, and one by one the munching or slurping guests filed out of the hall into a corridor, methinks, to the left of the house entrance. Ahmet motioned for me to remain seated whilst they all went to pray. Indeed they practiced a genuine *iftar*: after nibbling fritters or fruit, slurping soup, all Muslims must go to pray before indulging in the eating of meat. In Turkey, this custom is hardly respected, the families gobble down their meals, either praying afterwards or not at all! Only in the villages and smaller towns is the traditional *iftar* respected...When the prayers returned, steaming mutton, vegetables and salads were heaped upon the table, followed a half hour later by tea and kumiss ! Two hours later, fingers and mouths dripping with mutton fat, nothing remained...The Imam and Ahmet offered to drive me to my hotel; I thanked the assembly, slipped out with my friends and they drove me back to the Sabina Hotel; I would see them every day for my remaining days in Tūrkestan...

Every morning for five days at the call of the prayer, I would briskly walk to the *turbeh*, and with my *hikmets* opened, recite seven strophes as I turned slowly seven times round the Hodja's eternal dwelling place. I recited the same strophe every morning and evening until I had memorized them:

*'At fifty-seven, my life has passed as the wind,
hey friends, deedless am I, my head in a spin ;
May Allah be praised, the Guide of Love held my hand,
By my Oneness and Existence, am I to see His Face?'*

*'I reached fifty-eight, without news am I,
My soul accomplished the sixth degree, my Rabb al-Qahhaar;
By your zeal, I strike my inauspicious soul with a battle-axe,
By my Oneness and Existence, am I to see His Face?'*

*'I attained fifty-nine, help and lamentation,
When I gave my soul I remembered not;
I remain humble before Your Face, you free me from world constraint,
By my Oneness and Existence, am I to see His Face?'*

Like the planets circle the sun so too does the pilgrim circle the lieu of sacredness. Like the earth spins on its axis, so too does the pilgrim spin on his uprightness. Like the dervish turns and turns round the *samâ*, so too does the pilgrim turn and turn round his Self. All circular movements, all circuitous courses, all mental and spiritual convolution lead the pilgrim back to the same point of non-commencement ; the mystery of the womb ; that point in time and space when and where the ontic vertical and the existential transversal unite in transient harmony, in communal awareness. Be it circumnavigating our World or circumambulating our Heart, the pilgrim circles, spins and turns in order to circumscribe his circumstances, and in doing so, situate him or herself on the long and narrow Road of Self-Accomplishment, the sole Road of and for the pilgrim, the Voyageur of the World and the Heart, without circumlocution or circumvention...

And every day for five days I would pray with the Imam in front of the sacrophagus, would meet him and Ahmet in the gardens where we chatted about this and that. Through my prayers and our conversations I came to regard Hodja Ahmet Yesevî as one of the *Rabi el-Alamîne* -le Maître des Mondes-, second only to Christ, our Saviour. This is blasphemous in the ears of Muslims, but not in mine. The Hodja is certainly not a prophet, yet he is a seer, a sage, a wise man who has come to earth to deliver us...all or us...whatever be his or her creed or religion...like the Christ, albeit the Hodja's mission was an earthly one and not divine...Yet, both were men of peace who never brandished the sword...

Hodja Ahmet Yesevî's place in our World and in our Hearts can be gauged by tracing his teachings, writings and influence to the other Shiekhs of the Sufi Path that were his direct disciples, or those who

form part of the Sufi Chain of Transmission. The effects in words and deeds of his *hikmets* have spread eastwardly and westwardly, have offered insights and perspicacious analyses of Koranic interpretation and of the hadiths, have softened the stringent demands of an oftentimes intolerant Sunnism. The Art of the *samâ*, of dancing and chanting, is one inspired by Ahmet Yesevî's *hikmets*. The Sufi's daily communion with his Rabb is the Hodja's daily communion with us all. The sage is he who obeys both God and men, who willingly bears upon his shoulders the load of Humanity, the toil and hardships of paving a Way for all and sundry to God, again, disregardless of religion and creed.

On one occasion I asked Ahmet and the Imam whether the Hodja had contemplated the Face of God. My question aroused them out of the tedious routine of small talk. The imam answered without demur: «No one contemplates His Face whilst in this life; it is we who are the objects of God's vision.» This answer brooked no further inquiry or immediate contrariety...

*'Bondsman Hace Ahmet, I rejected the self, I rejected the self,
Then I sought and found My Self;
Before death I suffered for not giving my Self,
By my Oneness and Existence, am I to see His Face?'*

asks the Hodja at the threshold of Selfhood ; namely, the Accomplishing of the Self with God as one's Companion. Every prophet and sage followed this Road or Path, be they Jew, Christian, Muslim, Hindu or Buddhist. The multiple and chaotic existences of this earthly life gradually rose into the lofty summit of the awareness of the ontic Self, that one embued with the divine light of God's refreshing void. Yes, void ! The velvety solace of the very Instant, that very *Augenblink* when the sleeper awakens after having striven and toiled, after having experienced a myriad of existences, after so many dreams and arcane desires have fashioned those existences...The awakening into the sounds of Selfhood is the end of the Voyage, of transversal labouring...of the futile, unremitting alterations of masks or rôles...So many blinks of the eyes until the eyes finally **see**...So many descents of the Rabb until that One Face is **seen** ! Bountiful is the Void...

*'Şibli danced the dance of Love and saw the Light,
He readied himself, asked a question and saw Mohammad;
He closed his eyes, abandoning the other world,
Friends, chant and danse such as servants do.'*

*'Şibli said, crying with Love: «Hey, Prophet,
I am exhausted, if I dance I shall grieve;
The Prophet said: «Hopefully He will accept your action.»
He longed for permission to chant and dance, friends.'*

Awake! Awake! Into the Light of Reality, awake! Every existence has been a dreaming until the ontic Self bursts upon the scene, tears away the curtains and reveals He who does not applaud or hoot!

*'Those who habituate themselves to the poverty of this world,
Who find consolation in torment;
Who render service to good; servant Hodja Ahmet,
Such people will be sultans on the Day of Judgement.'*

The Companions of Torment : the Bedreddîns, Börklüces, Torlaks, Mansur al-Hallajs and Sohrawardis ; the Saints Paul and Peter, the Etiennes and Sebastians, Blandines and Joans of Arc ; the Giordanni Brunos and Wycliffs and Rabbi Akibas and Eti Hilsoms, and the other six million... They attained Selfhood and now rest in peace. Their toilsome Voyage upon the face of our Earth provided them the requisite to behold His Face. They became what Humanity harbours deep within its Heart: the quintessential Being. Are not the Hodja's *hikmets* those theosophic strophes that he composed and recited so as to understand his link, his bond, his vassality to God? To grasp the cause of his Being and Existence begot by Him who caused all things? The wisdom of the Hodja lies in this quest, in this forging of a Way to Him, the Mover of all that moves...

Around the *turbeh*, the World, the Heart, I turned and spun and danced the Cosmic Dance, murmuring prayers for those mighty heroic Companions, those noble souls of the long and rocky Road of Self-Accomplishment ; they who have divested the cloak of mortality, clad only in the shroud of purity : the joyous reascent... Their pilgrimages ended now in joyful tears now in abominable abjection.

And here, seated behind the locked door of the Hodja's chamber of rest, reading aloud his bezels of sagacity, my pilgrimage, too, has come to an end yet, neither in joyful tears nor in abominable abjection, but rather in a state of expectancy of further calls that will conduct me ever nearer towards the lofty whorling summit of Selfhood...

Yes...the unveiling of arcane things and the actualizing of thoughts that deform not their essentiality through the independant comprehension of outer circumstances has entralled and lured this pilgrim to

such formidable delights that not once has the veracity of the Road ever been overshadowed by inane pettifoggery or notorious acclaim...

*'Ô day, rise! The atoms dance,
Souls, lost in ecstasy, dance,
In your ear I shall tell you where the dance leads,
All the atoms in the air and in the desert,
Heed well, are swirling in madness,
Each atom, sad or happy,
Is amorous of the sun whose essence brooks no degree.'*

Mevlanâ

Hommage à Afanasya Nikitin

The unabridged version of this travelogue is published as a supplement. – *Ed. Journal of Eurasian Studies.*



EXHIBITIONS

FARKAS, Flórián

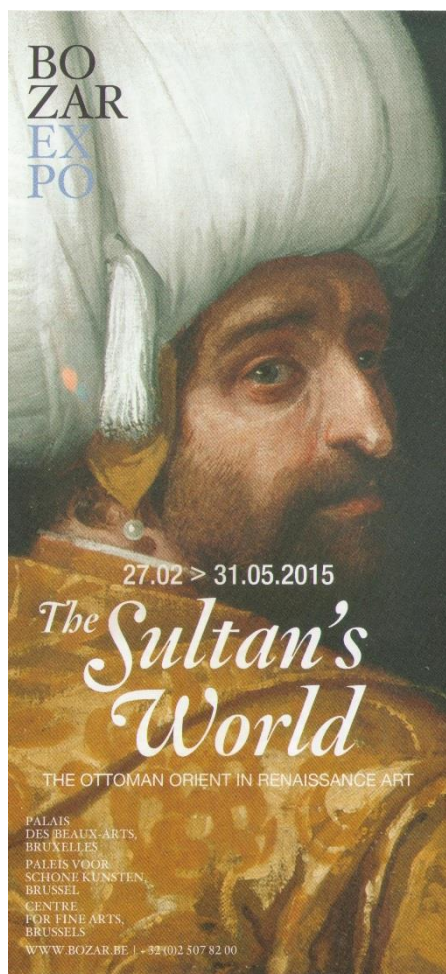
Magnificent Cultural Treasures in Brussels

During the spring of this year two extraordinary exhibitions are held in Brussels: *THE SULTAN'S WORLD – The Ottoman Orient in Renaissance Art* in BOZAR and *GAO XINGJIAN. Retrospective* in Museum of Ixelles. These two exhibitions are presented below.

* * *

THE SULTAN'S WORLD

– *The Ottoman Orient in Renaissance Art* –

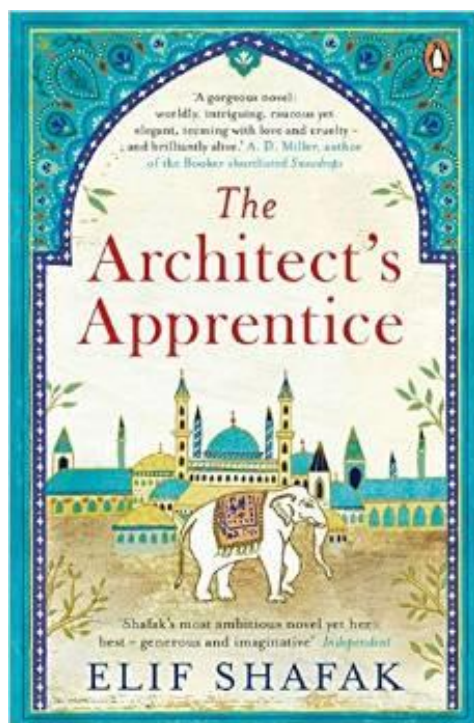


This exhibition is held in [BOZAR](#) (Palais des Beaux-Arts, Bruxelles / Paleis voor Schone Kunsten, Brussel / Centre for Fine Arts, Brussels) between 27 February and 31 May 2015. The exhibition will move to the [National Museum in Kraków](#) from 26 June until 27 September 2015 with the title *Ottomania – The Ottoman Orient in Renaissance Art* – where it will get a greater Polish focus.

This exhibition is organized in the context of the international project “[Ottomans&Europeans – Reflecting on Five Centuries of Cultural Relations](#)”, which puts the cultural ties between Europe and the Ottoman Empire/Turkey in the spotlight.

Many aspects of arts have a fairly direct relationship to the politics of the day. The exhibition *The Sultan's World* is a prime example of this relationship. During the past 15 years or so Turkey has become a regional (economic) power and the Central-Eastern European countries which joined the European Union in the same period have asserted their position as full members of the European family or nations. The sole existence of the exhibition is a testimony to the first and the widening of the European context, including the Central-Eastern European countries into the mainstream of it is a testimony to the second.

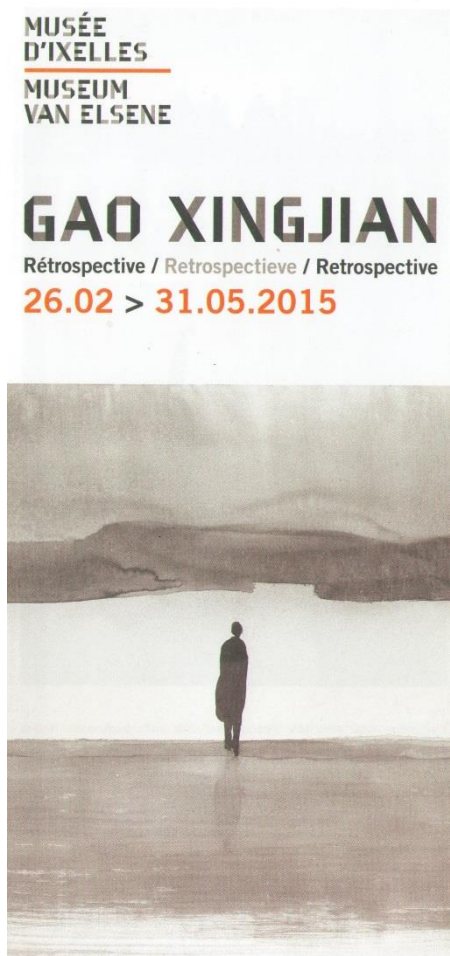
The Ottoman Empire came into existence at the end of the 12th century in Anatolia and achieved its zenith in the 15th. During this period it became the leading Islamic polity, it made deep inroads into Europe, Asia and Africa. A conflict with Europe became inevitable, and due to its different religious background it was easy to paint it as the arch-enemy of Christian Europe. The centuries-long relationship between the Ottoman Empire and Europe though was a complex one and this fact is perfectly reflected in this superb exhibition. Although it tackles the issue of political propaganda in renaissance art it goes beyond the clichés used in it during the period in question and delves into the complex web of Ottoman-European relationships. If one wishes to summarize in one key sentence the main message of the exhibition: it is the unearthing of the long process of discovering the 'Other'. Much attention is paid to the Renaissance portraiture of the Sultans, the discovery of mostly Venetian painters of Istanbul and the everyday life of the Ottoman Empire, the lure of the Orient, trade, the exchange of books, ideas, and many more. Superbly exhibited in the spacious rooms of BOZAR, visiting this exhibition is a real pleasure and a great cultural adventure.



The exhibition opened shortly after the release of Elif Shafak's latest novel *'The Architect's Apprentice'*, which is an ode to the greatest Ottoman architect Mimar Sinan. The author, one of Turkey's leading novelists, is the ambassador to this exhibition who not only wrote an introduction for the hardback exhibition catalogue (available in English, French and Dutch) but she also put together a literary visitor's guide *'Beyond East and West: The Language of Tulips'*. She chose eight exhibited works and wrote a personal commentary to them. One of the selected works is Melchior Lorck's *Sultan Süleyman the Magnificent* (Full-length portrait with a view to the Süleymaniye Mosque), 1573, London, The British Museum.

According to the commentary this picture was the main inspiration for her latest novel. In the forefront a full-length portrait of Sultan Süleyman, and in the background there is an elephant with a mahout and a banner, before the Süleymaniye Mosque. Its unusual imagery captivated her imagination and ultimately led to a beautiful novel. A nice conclusion to the exhibition is a literary evening with Elif Shafak on the 28 May 2015 at 20:00 in BOZAR.

Exhibitions like *The Sultan's World* are invaluable. Paintings, scrolls, coins, carpets, and other artifacts were lent by several dozen museums. It would be close to impossible to see all these in their original places. Therefore it is a great possibility to visit this exhibition, either in Brussels or in Krawków, to better understand our past and to have another view of the 'Other'.

GAO XINGJIAN. Retrospective

This exhibition is held in [Museum of Ixelles](#) (Musée d'Ixelles / Museum van Elsene) between 26 February and 31 May 2015. Usually there are no retrospective exhibitions for living artists. This exhibition is thus a rarity from this perspective, which the artists acknowledges much; he is very thankful to the Museum of Ixelles, which holds the largest retrospective exhibitions devoted to him.

Gao Xingjian was born in 1940 and though he attended the Foreign Language Institute in Peking in order to study French, he pursued simultaneously literature and painting, which was always a long tradition in China, contrary to European tendencies where there was an insistence on concentrating on one area or the other. Actually it was painting that brought him to Europe on a scholarship in 1987 and in 1989 after the events on the Tiananmen Square he sought political asylum in France.

While keeping up with painting he also continued to write in several genres: novels like *Soul Mountain*, *One Man's Bible*, short stories and drama (e.g. *Snow in August*). In 2000 he was awarded the Nobel Prize for Literature, becoming the first Chinese person to receive this famous prize.

Gao Xingjian's writing and painting exhibit the same characteristics: humanistically subjective, sublime, soul-searching for freedom, independent from any school, free of any constraint of doctrines or -isms. Furthermore, he combines Western and Eastern techniques, traditions in his own artistic pursuit, which is finding the light with an inner vision. For him the dividing line between abstract and figurative is irrelevant, his paintings are suggestive, in the best traditions of pictorial art.

In a brief interview given on the occasion of this retrospective exhibition he reiterated his choice for material usage: he uses Indian ink, but not to make it specifically Chinese. Next to that he paints on canvas, with a frame, which is quite Western. In this sense his paintings are a nod to both the East and the West.

Gao Xingjian. Retrospective is a beautiful and rich exhibition, which allows every visitor to delve in his or her inner self. It is accompanied by a hardback exhibition catalogue in French, in which the exhibited paintings are complemented by beautiful literary commentary.



NEW BOOKS

CZEGLÉDI, Katalin

Russian Grammar for Advanced Students

Katalin Czeglédi's Russian Grammar for Advanced Students, written in Russian, is published as a supplement to this issue. You can read the brief foreword below in English, Russian and Hungarian. — *Ed. Journal of Eurasian Studies*

Foreword

This book is written for those, who want to study Russian grammar as advanced students at home or at school. On the one hand it is useful for students studying Russian at universities where they have to study Russian grammar in Russian and on the hand for those who simply want to study it in Russian. This book is written in order to have a Russian grammar which contains all of the essential elements of an up-to-date Russian grammar and at the same time to give a comprehensive systematised grammar helping the students. I included many examples that can enhance the study. At the end of the book a list of publications is included; using those works can further deepen the knowledge acquired from this book. On request of my students I made also a separate, more detailed plan of Russian grammar, collection of grammatical terminology and a list of irregular verbs.

Предисловие

Эта книга написана для тех, кто хотят заниматься грамматикой русского языка на русском языке либо сами дома либо при помощи учителей. По другому эта грамматика является полезной для студентов занимающихся русским языком в разных институтах и либо они сами хотят либо им надо учиться русской грамматикой по-русски. Своя цель дать студентам грамматику содержаемую все важных, т.е. дать студентам универсальную, систематизированную картину о грамматике русского языка с многими примерами в одной книге чтобы помогать студентам в учёбе. Материалы находящиеся в литературе могут также подтвердить знание о русской грамматике. В этой книге написано также отдельный более подробный план о грамматике, даже словарь русских грамматических терминологий и лист неправильных глаголов по просьбе моих студентов.

Előszó

Ez a könyv azok számára készült, akik haladó szinten orosz nyelven kívánják az orosz nyelvtant tanulni akár egyénileg otthon, vagy akár tanár segítségével. Másrészt hasznos olyan felsőoktatási intézményekben orosz nyelvet tanulók számára, akik igénylik, ill. akiknek oroszul kell az orosz nyelvtant tanulni. A célom az, hogy a tanulók olyan anyaghoz jussanak hozzá, amelyben minden lényeges benne van, ugyanakkor olyan átfogó, rendszerezett képet kapjanak a mai orosz nyelv grammatikájáról, amely egy helyen megtalálható és sok példa segíti a tanulást. Az irodalomban megadott anyag pedig megerősítheti az így megszerzett tudást.

A tanítványaim kérésére külön tervet készítettem, más szóval egy részletesebb vázlatot az anyagról, valamint egy nyelvtani kifejezések tárá, amelyekkel a megértést, átláthatóságot és könnyebb tanulást kívánom elősegíteni. Csatoltam továbbá a rendhagyó igék listáját.



OUR AUTHORS

BÉRCZI, Szaniszló

Physicist-astronomer who made a new synthesis of evolution of matter according to the material hierarchy versus great structure building periods. This model is a part of his Lecture Note Series Book on the Eötvös University. He also organized a research group on evolution of matter in the Geonomy Scientific Committee of the Hungarian Academy of Science (with Béla Lukács). He wrote the first book in Hungary about planetary science *From Crystals to Planetary Bodies* (also he was the first candidate of earth sciences in topics planetology). He built with colleagues on the Eötvös University the Hungarian University Surveyor (Hunveyor) experimental space probe model for teachers for training purposes and development of new constructions in measuring technologies.

CZEGLÉDI, Katalin

Studied Hungarian-Russian-Altaic languages and literatures at the University 'József Attila' in Szeged, Hungary. She was given the title 'dr. univ' at the same University, too. As a teacher Ms. Czeglédi taught foreign languages at all type of state schools, and linguistics at state universities. Her major research topics cover linguistic prehistory in general and applied linguistics. Currently she teaches linguistic prehistory at Private Universities called 'Nagy Lajos király' in Miskolc, Hungary and 'Kőrösi Csoma' in Budapest, Hungary. She regularly delivers scientific lectures at conferences and meetings. Ms. Czeglédi published about 80 essays and two books: 'History of Scythian-Hunnish languages 1. Phonetics 2. Presyntaxe'. Currently she is working on the third volume of this series '3. Accidence (1. System of roots of words 2. System of forming of words.)'. Her major aim is to learn the history of our language and our people in the best possible way and to convey this knowledge to as many people as possible.

ERŐS, Vilmos

Mr. Erős is an Associate Professor at the University of Debrecen (Historical Institute), where he teaches Hungarian and European historiography, the Theory of History and the History of Political Ideas. Now he is working on a general historiographical synthesis (in Hungarian) and on a summary about the Hungarian historical writing (in English.) His earlier books include: *A Szekfű-Mályusz vita*. [The Szekfű-Mályusz Debate] "Csokonai Új História Könyvek". Csokonai Kiadó, Debrecen. 2000; *Asszimiláció és retorika*. (Szabó István: A magyar asszimiláció című tanulmányának rekonstrukciója.) [Assimilation and Rhetorics. The Reconstruction of Study by István Szabó, "The Hungarian Assimilation".] Csokonai Disputa Könyvek". Csokonai Kiadó, Debrecen. 2005; *A harmadik út felé*. (Szabó István történész cikkekben és dokumentumokban.)

[Towards the Third Way. The Historian István Szabó in Articles and Documents.] „Kisebbségkutatás Könyvek.” *Lucidus Kiadó*, Budapest. 2006; A szellemtörténetől a népiségtörténetig. (Tanulmányok a két világháború közötti magyar történetírásról. 2. kiadás.) [From Spiritual History to Ethnohistory. Studies About The Historical Writing in Hungary between the two World Wars.] *Egyetemi Kiadó*, Debrecen, 2012; "Tudomány és ideológia között. Tanulmányok az 1945 utáni magyar történetírásról. [Between Ideology and Scholarship. Studies about the Historical Writing in Hungary after 1945.] (Szerk., Takács Ádámmal közösen.) ELTE *Eötvös Kiadó*, Budapest. 2012.

FARKAS, Flórián

Mr. Farkas was born in 1967 in Kolozsvár/Cluj/Klausenburg. He holds a M.Sc. degree from Technical University of Budapest, Hungary and Ecole Normale Supérieure de Cachan, France and an MBA degree from Henley Management College, UK. Since 1992 he is living in the Netherlands. He cofounded the Foundation Mikes International in 2001 in The Hague, the Netherlands.

MARCANTONIO, Angela

Associated Professor of 'Historical Linguistics' and 'Uralic Studies' at the University of Rome "La Sapienza". She is a founder of the so-called 'revolutionary school' of Finno-Ugric/Uralic studies. The results of her research are controversial, because she challenges the foundation of the field, that is, the validity of the conventional Finno-Ugric/Uralic theory and related family tree. She is the author of several books and numerous articles (e.g.: *The Uralic Language Family: Facts, Myths and Statistics*, 2002; *A történelmi nyelvészet és a magyar nyelv eredete. Angela Marcantonio válogatott tanulmányai* ('Historical Linguistics and the Origin of Hungarian: Selected Papers by A. Marcantonio', 2006). Next to this, she is working on the origin and prehistory of Hungarian in close cooperation with colleagues from the Universities of Budapest and Amsterdam, and she is also publishing in Hungarian periodicals in the Netherlands like *Amsterdam Studies* and *Mikes International*.

MIRABILE, Paul

After having travelled and worked for many years in Africa and in Europe, Mr. Mirabile enrolled at the University of Vincennes-Saint-Denis, Paris VIII where he obtained his doctoral thesis in 1986 in mediaeval History, literature and linguistics: *La Genèse de la Chanson de Roland: la Théorie de l'Entonnoir* under the direction of Bernard Cerquiglini. Since then he has taught languages and literature, philology and History either at universities or secondary schools in Turkey, South India, China, Ireland and Russia whilst doing research on the Mediaeval Eurasian Koine. He has contributed articles and essays on mediaeval History, religions and philosophy in *Stratégique* (F.E.D.N), *Contrastes*, *Liber Mirabilis*, *Nietzsche-Studien*, *Journal of Armenian Studies*, *Journal of Dravidic*

Studies, Armenian International Reporter, La Chine au Présent, Al Amanecer (Istanbul: Judio-Spanish journal), *Chasse-Marée* and in university reviews in China and Russia. He is currently teaching in Istanbul.