



AKADÉMIAI KIADÓ

Gyula László's theory of the "two-time conquest of the Magyars" and the archaeology of the Avars

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ABSTRACT

Gyula László's theory, published in 1970, was virtually ignored and received with tacit dismissal by the Hungarian archaeological scholarship and international archaeological community was largely unaware of it. This paper aims to provide clarity for the latter research. Not a single element of the theory was accepted or was acceptable even at the time of its birth: distribution of the late Avar and the Conquest-era sites do not complement each other; István Kniezsa's map is highly discussed and is not suitable for proving that the eighth century Avars were Hungarians; Byzantine sources record the immigration of a military group and not of a people, who later moved on; the "Ugri Bjelii" mentioned in the *Russian Primary Chronicle* cannot be applicable to this immigration; the so-called of "griffin-tendrill" population is about 30 years later as the supposed immigration; there was not a migration from the Káma region in the seventh century) connecting the "*Uuangariorum marcha*" with the "Onogurs" is highly uncertain; there is no trace of any immigration in the anthropological material of the Avar period.

Errare humanum est.

KEYWORDS

so-called two-times conquest of the Magyars, archaeology of the Avars, Byzantine and Russian sources, Kama-region, "*Uuangariorum marcha*", physical anthropology

Revisited in this study is a theory proposed some half a century ago.¹ Although seemingly no longer of any relevance, not merely because of the many decades that have elapsed since, but rather because Gyula László's theory was, with a few exceptions, virtually ignored and received with tacit dismissal in Hungarian archaeological scholarship at the time, while the international archaeological community was largely unaware of it owing to the language of the publications. Yet, this national-romantic theory continues to resonate in Hungary, even though its historical, archaeological, linguistic and physical anthropological assertions were neither accepted, nor acceptable at the time. The relevant literature on this theory is predominantly in Hungarian; at the same time, several elements of the theory do have a relevance for the period's broader Central European research. Discussed below will be the research of the Avar period between 1950 and 1980, Gyula László's long-lasting impact on the archaeology of the early medieval period in Hungary, alongside a look at the sources on which his theory rested.²

The main elements of the theory are as follows:

- the distribution of late Avar and Conquest-period sites complement each other;
- the distribution of late Avar sites corresponds to the eleventh–twelfth-century Hungarian settlement territory as reconstructed by István Kniezsa, a scholar of Slavic studies;
- Byzantine sources record the arrival and settlement of the Onogurs around 670/680;

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¹The expression "two-time conquest" is Gyula László designation of his theory, which is how it referred to in the academic literature and in public discourse. (In Hungarian: "*kettős honfoglalás*".) However, Imre Boba, a half-Hungarian scholar living in Seattle pointed out that "two-fold" conquest would be more accurate: Boba (1984); his arguments were also accepted by Makkay (2009) 70. For a history of research, cf. Csiki (2010); Farkas (2011).

²Fodor (2001); Langó (2007) 117–124; Lezsák (2021).

- tendril-ornamented belt fittings resembling the ones of the late Avar period are known from the Kama region;
- the twelfth-century *Russian Primary Chronicle* speaks of “white Ugrians” who marched under Kiev during the reign of the Byzantine emperor Heraclius (r. 610–641);
- two dialects can be distinguished in the early Hungarian linguistic relics;
- the anthropological traits of late Avar and early Árpáadian Age skull types resemble each other, but differ from the ones of the Conquest period;
- a ninth-century charter features an “Onogur” toponym in Lower Austria.

Based on the above, Gyula László concluded that the ancient Hungarians had actually conquered the Carpathian Basin twice: the first Hungarian-speaking population groups arrived to the Carpathian Basin around 670/680, while the second conquest was the arrival of the tribal alliance led by Árpád in 895.

THE PATH TO THE THEORY

Gyula László first noted that the settlement territories outlined by the (late) Avar- and Conquest-period cemetery sites complement each other in 1944.³ In 1955, striving to demonstrate the early arrival of the Finno-Ugrian-speaking Magyars to the Carpathian Basin, he added his theory of migration from the Kama region around 670–680,⁴ which József Deér, the first to react to the idea, designated as a *Katastrophen*theorie.⁵ Yet, both the idea of a migration around 670/680 and the focus on the Kama region in relation to this population movement were to some extent grounded in previous Avar scholarship. Gyula László solely cited Géza Nagy, who pursued both archaeological and historical studies at the turn of the nineteenth and twentieth centuries. However, regarding the story of Kuber and the mid-seventh-century disintegration of Magna Bolgaria, a polity variously located to the Kuban, Don or Dnieper region, Géza Nagy merely noted that “it seems unlikely that there would not have been Magyars among these Bulgars”, to which he later added that “the Magyar element had settled in Hungary already during the Avar period”.⁶ Nándor Fettich was the first to suggest, in 1935, the possible links between the Avar finds and an archaeological culture in the Kama region, although he only cited a single burial of the Nevolino cemetery, whose grave goods seemed good parallels to the Avar material. At the same time, there was nothing to underpin his belief that a larger Avar population

had once moved to the Kama region.⁷ Gyula László interpreted this as the *return* of the Avar population,⁸ since he was well aware that any movement of the Avars was only conceivable on the Eastern European steppe. As a matter of fact, the arrival and settlement of Kuber and his people was first linked to the mid-seventh century Avar finds by Dezső Csallány, specifically to the Szeged-Átokháza burials representing one of the period’s key assemblages, in a study devoted to the burial’s chronological position⁹ – which, however, Gyula László never quoted.

Gyula László first presented his ideas in a preprint distributed during the Fourth Slavic Archaeological Seminary in 1963, which he did not personally attend;¹⁰ by 1965, he described his idea as a “certainty”,¹¹ and by 1970, he had expanded it into a broad theory.¹² The first academic debate took place in 1969, during which historian Péter Váczy, orientalist Károly Czeglédy and archaeologist István Bóna, as keynote speakers, voiced their detailed critique and rejected Gyula László’s ideas with varying degrees of vehemence.¹³ Gyula László’s response to the criticism was evasive and he insistently maintained that a Hungarian-speaking population had arrived and settled in the Carpathian Basin around 670/680 and that the traditional date of 895 in fact marked a second Hungarian Conquest. In a small booklet published in 1978, he politely responded to the objections that had been raised,¹⁴ but did not revise the main elements of his theory either then, or in his later studies, in which he merely changed the rhetoric.

During the 1950s and the 1960s, both Hungarian, and Central and Eastern European archaeological scholarship was largely oblivious of the issues that preoccupied modern research: the problems of reconciling the archaeological and the historical record, the pitfalls of invoking migrations for explaining changes and other phenomena in the archaeological record, and there was a blissful unawareness of the fact that the interpretation of early medieval archaeological assemblages could be – and were – strongly coloured by national sentiments. For long decades, Gyula László remained the authoritative voice in early medieval archaeology he was impervious to both the criticism levelled at him during the academic debate preceding the publication of his book,¹⁵ which according to its title covered aspects of Avar society,¹⁶ and the critique of his dual conquest theory,

⁷Marosi and Fettich (1936) 86, 88–89.

⁸László (1955) 285.

⁹Csallány (1939) 174–176; Csallány (1946–1948) 360–361.

¹⁰László (1965) 73–75.

¹¹Lecture given at the conference of the Association of Hungarian Archaeology and Art History in Nyíregyháza.

¹²László (1970).

¹³I have attended the debate, whose presentations was not published in print.

¹⁴László (1978).

¹⁵Harmatta (1955).

¹⁶Instead of a more traditional title, the book’s title reflected the period’s general political attitude (Gyula László’s personal communication).

³László (1944) 95–96.

⁴László (1955) 179–180.

⁵Deér (1965) 721–722.

⁶Nagy (1895) CCCLII; Nagy (1907) 268.



which brought him widespread acclaim across the Hungarian-speaking world. The period's other specialists were forced onto the sidelines: stripped of his membership in the Hungarian Academy of Sciences for political reasons in 1949, Nándor Fettich¹⁷ struggled to make ends meet for a decade,¹⁸ while Dezső Csallány, who had taken his degree at Vienna university, had been similarly dismissed for political reasons, and was in 1962 eventually given a job in the Nyíregyháza museum, whose library lacked even a complete series of *Archaeologiai Értesítő*,¹⁹ while Ilona Kovrig, a curator in the Hungarian National Museum, was kept busy by her museum duties. Although István Bóna's MA thesis focused on the assessment of an Avar cemetery and he subsequently published a series of smaller studies on the Avar period that have lost none of their relevance,²⁰ his main field of interest in the 1960s was the Bronze Age and the early Migration period. István Bóna became an authoritative voice in Avar studies through his seminal assessment of the Ivánca burial,²¹ published exactly at the same time as Gyula László's work on the dual conquest, and his general critical analysis of early medieval archaeology in Hungary. At first, István Bóna restrained his critique of the theory, voicing merely his doubts regarding the Avars' survival and the claim that the Avar- and the Conquest-period settlement territories complemented each other;²² he penned his first, more scathing critique in relation to the entry "Awaren" written by Gyula László published in the *Hoops Realexikon*.²³ Gyula László's former students regarded their professor as an untouchable²⁴ and were unwilling to challenge his theory, simply keeping their silence,²⁵ which "in most cases was more of a covert rejection of it all"; for them, "István Bóna's outspoken rebuttal was by far the most formidable", but they shied away from coming out with their own detailed critique of Gyula László views.²⁶ It is therefore not mere chance that the first critical comments in this general cautious climate came from István Fodor, who had studied in Moscow, and László Madaras, who had graduated in Szeged as Fodor's student.²⁷ Among Gyula László's students, the first to point out the archaeological flaws of the theory was Kornél Bakay,²⁸ while I reviewed the theory's

main elements and its key problems in an archaeology handbook.²⁹ Today's younger generation of archaeologists tends to view it as a "national-romantic theory"³⁰ and they have no qualms about pointing out that Gyula László barely kept abreast of the archaeological literature of the post-war decades.³¹

His theory did gain some acceptance among historians. Some of these are wholly unacceptable in terms of archaeological chronology: for example, György Györffy argued at length that the eighth-century griffin- and tendril-ornamented find material actually represented the legacy of the tenth-century Magyar commoners; Antal Bartha assigned the same finds to the ninth century.³² Imre Boba, who is known for his singular theories, posited a "political continuity" between the "Onogurs of the 680s" and the population group led by Álmos (!) in 895 from his reading of Hungarian medieval sources and Constantine Porphyrogenitus's *De administrando imperio (DAI)*.³³ However, when constructing his theory, he simply chose to neglect the detailed research on the historical reliability of the medieval chronicles of Hungary, ranging from the twelfth-century Anonymous to Simon de Keza and Bonfini as well as the innumerable studies on the relevant chapters of *DAI*.³⁴ Historian Pál Engel's comments on the dual conquest theory came as a surprise because he had never before shown any particular interest in archaeology or in this period. He began one of his articles with an admission of sorts: "to historians engaged in the study of the late Middle Ages [...] the problems of the early history of the Magyars are like a plague that we do our best to avoid", but, given Gyula László's authority, he nevertheless set down his ideas.³⁵ In his view, the Magyar tribes had reached the eastern slopes of the Carpathians around 700 and therefore the migration and settlement of a larger Magyar population group was not wholly inconceivable.³⁶ True enough, we know next to nothing about possible migrations on the steppe around 700 – and even more importantly, nobody had previously even suggested that the Magyars had advanced to the Carpathian foreland by that time. Archaeologist Gábor Vékony, who often came up with rather startling views on various archaeological and historical issues, one of these being his take on Gyula László's theory, suggested that certain western South Slavic loanwords in the Hungarian language could have or had been adopted before the ninth century.³⁷ This claim had never been voiced

¹⁷Werner (1972) 149–152, Erdélyi (1973).

¹⁸Nándor Fettich was posthumously rehabilitated by the Hungarian Academy of Sciences in 1991.

¹⁹Németh (1977).

²⁰E.g. Bóna (1957, 1963, 1965, 1968).

²¹Bóna (1970).

²²Bóna (1971) 326; Bóna (1973) 81.

²³Bóna (1976).

²⁴Bóna (1997) 353.

²⁵E.g. Dienes (1972); Bálint (1975) two works covering the Conquest Period.

²⁶Takács (2006) 80.

²⁷Fodor (1973b) 121; Madaras (1975) 45–46.

²⁸Bakay (1978) 166–167, 187, note 278. He later changed his views: Bakay (2004) 213–220.

²⁹Bálint (1989) 233–235.

³⁰Szente (2020) 35.

³¹Fodor (2011) 22, 25; Szente (2020) 35.

³²Györffy (1959) 119–126; Bartha (1979) 12.

³³Boba (1984).

³⁴E.g. Györffy (1948); Kristó (1972); Kulcsár (1973).

³⁵Engel (1997) 54.

³⁶Engel (1990) 105; Engel (2001) 11.

³⁷Vékony (2002) 213. For the current state of linguistic research, cf. Zoltán (2015).



previously and can be wholly rejected according to specialists of Slavic studies.³⁸

When constructing his theory, Gyula László's springboard was the classification of the early medieval archaeological finds of the Carpathian Basin elaborated in the late nineteenth century. This classification represented a significant milestone at the time. József Hampel assigned Germanic-type finds to his Group I, finds that are currently dated to the late Avar period to his Group II, early Avar finds to his Group III and Conquest-period finds to his Group IV.³⁹ The dating and ethnic attribution of the latter were widely accepted since the publication of the first Conquest-period burial in 1835.⁴⁰ In 1907, József Hampel introduced an additional group that was roughly contemporaneous, but in his view represented an ethnically different population: Group A comprised the Magyars of the Conquest period, while Group B the finds of the Slavic population of the Carpathian Basin.⁴¹ Taking this research situation as his starting point in 1970, Gyula László's thought was ultimately coloured by two considerations: the first, the undeniably steppean nature of Group A – and the knowledge that the Finno-Ugrian peoples quite certainly lived farther to the north! – the second, the proportion of the burials of Group A and B.⁴² Citing the case of the Onogur Bulgars who were assimilated by the Balkanic Slavs, most Hungarian archaeologists tend to subscribe to the mistaken notion that the survival of a language can only be ensured through a numerical superiority,⁴³ a view which I, too, accepted in 1976.⁴⁴ However, socio-historic linguistic studies have shown that this is not necessarily the case,⁴⁵ one good example being the still living language of another Onogur Bulgar group that had similarly departed from Magna Bulgaria just like the one led by Asparuch, namely the Chuvash, who have preserved their ancestral language to this very day in an essentially foreign-language environment.

These are the main reasons that Gyula László pursued an entirely different train of thought both historically and geographically for demonstrating the presence of the assumed Hungarian-speaking population. He simply took no notice of the first major finding in the post-war study of the tenth-century archaeology of Hungary, namely that in 1962, Béla Szőke conclusively demonstrated that Group

B, previously regarded as Slavic following the lead of József Hampel, actually represented the archaeological heritage of the tenth- and eleventh-century ethnically mixed commoners of the Carpathian Basin.⁴⁶

Gyula László's main arguments were as follows:

- I. The spatial distribution of late Avar and Conquest-period sites complement each other
1. The research coverage of Avar and Conquest-period sites

The maps published by Gyula László in 1970 were based on various publications of Avar- and Conquest-period sites that were since long outdated by that time. In any case, it is rarely practical to base one's work on a much earlier state of research,⁴⁷ in this case, on a gazetteer of Avar-period sites assembled 17 years earlier⁴⁸ and a Conquest-period one that was compiled 11 years before.⁴⁹ The 1960s brought an upswing in archaeological research: many museums across Hungary now employed archaeologists, as a result of which blank spots gradually disappeared and, additionally, a catalogue of the Conquest-period finds from Slovakia was published shortly before the final elaboration of the dual conquest theory.⁵⁰ Although Gyula László insisted that "more recent sites did not essentially bring novel findings",⁵¹ István Fodor and László Madaras challenged this claim, as did Kornél Bakay somewhat later (see below). A closer look at the new distribution maps of the late Avar-period and the tenth–eleventh-century sites clearly revealed that the distribution of the sites of the two periods did not complement each other in any way.⁵²

2. The chronological relevance of the distribution maps
 - a) The sites treated as late Avar-period sites by Gyula László ultimately comprised all the cemeteries used during the late Avar period, whose use-life lasted up to the early ninth century. This was misleading insofar as the purported complementing distribution with the Conquest-period sites would imply that only the cemeteries dating from the terminal phase of the late Avar period (corresponding to the SPA IV period in the current periodisation) should have been considered and *not all* of the eighth-century burial grounds. Only after the identification of the former can the issue of the possible survival of the Avars into the ninth century be addressed⁵³ – which should have been Gyula László's starting point. This would have

³⁸Zoltán (2013); Zoltán (2020).

³⁹Hampel (1894, 1897); an enlarged version was published later: Hampel (1905).

⁴⁰Jankovich (1835).

⁴¹Hampel (1907).

⁴²Roughly 1:25 according to István Bóna's estimate: Bóna (1997) 350–352.

⁴³E.g. Kniezsa (1938) [365–472] 374.

⁴⁴Bálint (1975)/1991, 186–188.

⁴⁵Crystal (2000); Guérin and Yourupi (2016). I am grateful to Marianne Bakró-Nagy for calling my attention to these studies.

⁴⁶Szőke (1959); Szőke (1962) 100–102. For a typo-chronological assessment, cf. Giesler (1981); for a monographic assessment, cf. Tomićić (2019). For discussion of Béla Szőke's work, cf. Langó (2007) 124–127.

⁴⁷Bakay (1978) 187. note 278.

⁴⁸Csallány (1956) For the current research situation, cf. Szentpéteri (2002).

⁴⁹Fehér et al. (1962).

⁵⁰Točík (1968).

⁵¹László (1970a) 174.

⁵²Fodor (1996) last unnumbered pages; Szentpéteri (2002) Karte 4.

⁵³A critical discussion of the problem had already been published by that time: Tomka (1971).



called for a detailed typo-chronological assessment and comprehensive study of the late Avar material, which had not been undertaken at the time and is still a task ahead of us today. István Fodor and István Bóna were correct in pointing out that there was nothing to confirm that a substantial portion of the “griffin-tendril” population had lived to see the arrival of the Magyars (and the same still holds true today). Responding to their critique, Gyula László acknowledged that “they were both right”, adding that “at the same time, the presence of no other population can be demonstrated either, and it seems more plausible that the previous population had continued its life”,⁵⁴ which is hardly a compelling argument.

- b) As its title clearly indicates, one of the gazetteers used by Gyula László also listed the eleventh- and twelfth-century sites in addition to the tenth-century ones. In other words, when studying a possible complementing distribution, only the sites of the first few decades after the Hungarian Conquest should have been considered, and among those, solely the ones that could demonstrably be linked to the Magyars, while disregarding the sites used by the local population – however, the separation of the two either in terms of relative chronology or in terms of ethnic attribution is virtually impossible even today. Moreover, there were several regional migrations and re-settlements after the Hungarian conquest proper,⁵⁵ and while we know that the occupation of Transdanubia occurred some ten years after the conquest, after the Battle of Pozsony in 907,⁵⁶ this cannot be demonstrated archaeologically. Neither can we identify the settlement territory of the Avars possibly surviving into the ninth century,⁵⁷ and even less so of the assumed “tenth-century Avars” (!) for demonstrating a possible complementing distribution.
- c) The complementing distribution could be chronological in nature (see above); however, as Géza Fehér pointed out shortly after the debate on Gyula László’s book published in 1955, it could just as well be explained by the different subsistence strategies of the respective population groups,⁵⁸ in which case a complementing distribution is simply irrelevant.
- d) Some methodological reservations can also be raised: “I have neglected those adjacent areas where the two settlements not only overlap, but the cemetery itself was used jointly by the ‘late Avars’ and ‘Árpád’s Magyars’.”⁵⁹ We know that the incidence of finds from different periods on the same site does not

necessarily imply a continuity in occupation,⁶⁰ which has to be conclusively demonstrated in each case.

In sum, a comparison of the distribution of the sites of the two periods, the eighth century and the tenth–eleventh centuries, is ultimately a pointless exercise.

II. István Kniezsa’s map and the ethnic conditions in eleventh–twelfth-century Hungary

Following a brief overview of previous archaeological and historical research, Gyula László’s springboard was that “*virtually* no linguistic relics of the Avars have survived” [my italics].⁶¹ However, this cannot be regarded as a realistic starting point: true enough, the linguistic affiliation of the Avars is one of the thorniest and currently irresolvable problems of Turkic studies owing to the scanty linguistic record and its nature (solely personal names and dignitary titles are known) and the absolute lack of words of Avar origin. Aside from the assumption that the settlement territories of the two peoples complement each other, Gyula László’s other main argument was based on István Kniezsa’s map of the assumed ethnic conditions in eleventh–twelfth-century Hungary based on toponyms that can be derived from the names of the Conquest-period tribes and various other toponyms, personal names and hydronyms of the Árpadian Age.⁶² The textual evidence was principally drawn from twelfth-century charters, which is certainly permissible for the reconstruction of eleventh-century conditions, but hardly so for earlier periods lacking documents of this type. Gyula László simply transcribed the distribution of the eighth-century “griffin-tendril” sites in the Avar site gazetteer onto this map. Although he was aware of the fact that “few of these sites survived up to the tenth century”, he asserted that “in terms of place-naming, it is practically indifferent whether a cemetery dates from the eighth or the tenth century”,⁶³ a wholly untenable stance in terms of toponymy. To objections that the 200 years between the tenth century and the regular issue of charters poses an unbridgeable gap,⁶⁴ he retorted that “these place-names are still alive today, after 800 hundred years!”⁶⁵ which can hardly be construed as a historical argument. Linguist László Benkő made this clear in relation to ethnonyms, population groups and tribal names: “these do not provide sufficient information for establishing the origins, the language and the ethnic affiliation of their bearers.”⁶⁶ Regarding the onomastic conclusions drawn by István Kniezsa, Turkologist András Róna-Tas raised serious objections, pointing out that the map only seems to show homogeneous settlement areas at a very low resolution and that in fact there is a much

⁵⁴László (1978) 109.

⁵⁵Kiss (1968); Révész (1996) 204–206; Mesterházy (2002) 333–334.

⁵⁶Zsoldos (1996) 190.

⁵⁷This issue has since been addressed at greater length: Lőrinczy (1993); Takács (2017).

⁵⁸Fehér (1956) 27.

⁵⁹László (1978) 165.

⁶⁰Bóna (1996) 40.

⁶¹László (1978) 164.

⁶²Kniezsa (1938).

⁶³László (1970b) 49.

⁶⁴Róna-Tas (1980) 226; Bóna (1984) 328; Tomka (1981); Kristó (1983) 27.

⁶⁵László (1983) 27.

⁶⁶Benkő (2002) 257.



higher degree of mixing in the toponymic record.⁶⁷ The issues with István Kniezsa's map were also addressed by historians Gyula Kristó, Ferenc Makk and László Szegfű,⁶⁸ to which Gyula László responded somewhat evasively: "a most interesting attitude seems to be emerging in the departments of historic and Altaic studies of Szeged University. [...] Their objections are certainly worthy of consideration."⁶⁹ The linguistic objections to the map were clearly worded: the eleventh-century date of the toponyms that can be derived from the names of the Conquest-period tribes cannot be conclusively proven⁷⁰ and their use as evidence in resolving problems of ethnicity is controversial.⁷¹ Personal names⁷² and hydronyms⁷³ are in themselves unsuitable for determining the ethnic composition of the Carpathian Basin, while the early eleventh-century written sources can only be used for reconstructing demographic condition with certain limitations⁷⁴ and virtually nothing is known about the language of the Slavs who had been assimilated by the Magyars.⁷⁵ Although these studies were published later, Gyula László was probably familiar with their main points because he had amicable personal relations with all Hungarian linguists and Turkologists, whom he regularly met in the university building. The bottom line of the critique levelled at his map can be summed up as follows: "István Kniezsa's [...] methodology and the conclusions he drew thereof are largely untenable according to our current knowledge."⁷⁶

In sum, István Kniezsa's map of eleventh–twelfth-century Hungary is wholly unsuitable for a comparison with the micro-regions of the eighth-century "griffin-tendril" sites and for proving that the latter had been settled by a Hungarian-speaking population.

III. Settlement around 670/680

Gyula László's starting point was as follows: "József Hampel and András Alföldi were quite explicit when speaking of two different peoples, leading me to draw the obvious conclusion from the late appearance of this group: we must reckon with the settlement of a new people in the second half of the Avar period."⁷⁷ (It must here be noted that neither József Hampel, nor András Alföldi spoke of "two different peoples": they simply identified different types of find assemblages.) Gyula László linked the arrival and settlement of this population to the story of Kuber as

narrated in two Byzantine sources and since the designation of the Magyars in other languages (*hungarus*, *venger*, etc.) can be derived from the ethnonym of the Onogurs,⁷⁸ Kuber's migration became the second pillar of Gyula László's argumentation, even though he knew full well that not all "Onogurs" could be regarded as Magyars.⁷⁹

1. The migration in the written sources

Following the disintegration of Magna Bulgaria ruled by Khagan Kuvrat (r. ca. 630–ca. 650), the separation of his sons and the settlement of one of his sons among the Avars is recounted by Theophanes Confessor (758/760–817/818) and Patriarch Nicephorus (758–828), both of whom drew from a chronicle written around the turn of the eighth and ninth centuries:

"The fourth and fifth went over the river Istros, that is the Danube, the former became subject of the Chagan of the Avars in Avar Pannonia" (AM 6171)⁸⁰

"the fourth went over the river Istros and settled in Pannonia, which is now under the Avars, becoming an ally of the local nation." (Brev. 35)⁸¹

The narratives⁸² clearly follow well-established *topoi* and are chronologically somewhat confusing:⁸³ Alciocus (*Alti-og*, "Six Arrows"), one of Kuvrat's sons, appears in the Avar Khaganate at the time of the Bulgar revolt in 631, well before the generally accepted date of ca. 650 for the separation,⁸⁴ while Kuber's settlement is generally dated later, to between 668–669, 664–670 or 674–678.⁸⁵ The sources leave no doubt as to the important role of Kuber in seventh-century Avar history:

- a) Instead of the more general *λαός* ("people"), which included also women, children and the elderly, Theophanes Confessor expressly uses the word *δύναμις* in relation to Kuber. The latter is invariably rendered as "army" in the English, "Heeresmacht" in the German and "had" in the Hungarian translation.⁸⁶
- b) Two other sources, both from the early eighth century and independent of Theophanes and Nicephorus, agree that Kuber moved to the Balkans from the Avar Khaganate:

⁷⁸Németh (1925) 148–150; Melich (1934); Szádeczky-Kardoss (1970) 905. This fact was quite certainly known to Gyula László owing to their personal connection.

⁷⁹László (1978) 173–174: "When asked what makes me think that the Onogurs were in fact Magyars, I usually reply that it calls for a goodly stretch of the imagination."

⁸⁰Mango et al. (1997) 498.

⁸¹Mango (1990) 89.

⁸²Moravcsik (1983) 27–28.

⁸³Pohl (2018) 319–321.

⁸⁴Who then fled to the Wends: Pohl (2018) 217, 319–321.

⁸⁵Beševliev (1970) 298; Beševliev (1981) 161; Popović (1990) 118; Pohl (2018) 217.

⁸⁶Cf. notes 80 and 81, Szádeczky-Kardoss (1998) 219; Lauterbach (1967) 555.

⁶⁷Róna-Tas (1980) 226.

⁶⁸Kristó et al. (1973, 1974); Kristó (1976, 1983, 2000).

⁶⁹László (1978) 112, 113.

⁷⁰Hoffmann and Tóth (2016) 278–280.

⁷¹Rácz (2006) 13.

⁷²Benkő (2003) 19; Hoffmann and Tóth (2016) 301.

⁷³Hoffmann and Tóth (2016) 288.

⁷⁴Póczos (2019) 18, 20.

⁷⁵Hoffmann and Tóth (2016) 290. For a more detailed discussion, cf. Zoltán (2004).

⁷⁶Hoffmann and Tóth (2016) 315.

⁷⁷László (1955) 179–180; László (1965).



After the victory, Kouber, together with all his people, crossed the river Danube, came to our regions and occupied the Keramesion plain (Miracles of Saint Demetrius, Book V).⁸⁷

This is indirectly also confirmed by the Madara inscription:

“My uncles at Thessaloniki did not trust the emperor with the cut-off nose” (Inscriptio Protobulgarica 1c)⁸⁸

There can be no doubt that Kuber had been accompanied by his warriors to the Carpathian Basin, where according to both the sources and modern scholarship, he lived for about a decade and then made his way to the Balkans.⁸⁹ Similar armed forces appeared regularly on the Asian steppe: these are designated as retinues, *druzina* or *comitatus* made up of men who had severed all ties with their tribe or were foreign mercenaries in historical studies.⁹⁰

Although the cited passage of the *Miracula* was known to Gyula László, he maintained that “the fact that a few years later he [Kuber] was already living in Thessaloniki does by no means imply that he had moved there with his people.”⁹¹ György Szabados argued that Kuber had not departed with the same people he had arrived with,⁹² even though there is nothing to substantiate this claim, not even indirect evidence.⁹³ As far as the origins of Kuber and his armed retinue are concerned, there can be no doubt as to their Onogur background or that they had come from Magna Bolgaria, located on the northern littoral of the Black Sea (perhaps in the Sea of Azov region, the Kuban region or the Middle Dnieper region⁹⁴). The archaeological legacy of Asparuch, another of Kuvrat’s sons, and his people who settled in Bulgaria can provide useful insights when searching for traces of Kuber in the archaeological record.⁹⁵ (At the time, Gyula László could only know of a single cemetery whose finds were linked to the immigrant Proto-Bolgar population.⁹⁶)

The possibility that groups from the Kama region had perhaps joined Kuber and his retinue was never seriously considered in Hungarian scholarship (see below).

Teréz Olajos’s expectations turned out to be far too optimistic:⁹⁷ the discovery of new Avar runic inscriptions will hardly contribute to resolving the controversies of the dual conquest theory. Attempts at deciphering the few short Avar runic inscriptions are fraught with controversy⁹⁸ and the discovery of a text that would provide an answer to historical questions cannot be expected: none of the longer Eastern European runic texts narrate historical events.⁹⁹

2. The immigration and the internal chronology of the find material

Relative chronology played an important role in research on the Avar period, which began in 1874.¹⁰⁰ The find material was divided into two main groups by András Alföldi and Paul Reinecke,¹⁰¹ which was by and large accepted by Gyula László:¹⁰² in his 1955 book he wrote about the people of the early pressed sheet metal belt sets and the late griffin-tendrill belt sets.¹⁰³ It must here be emphasised that well before his formulation of the dual conquest theory in the 1970s, the appearance of assemblages with griffin- and tendrill ornamented belt sets was dated to the late seventh or early eighth century in Avar studies,¹⁰⁴ to a period after Kuber’s arrival and settlement. Ilona Kovrig’s chronological framework was well known even before the publication of her monographs in 1955 and 1963.¹⁰⁵ As a matter of fact, Dezső Csallány had already pointed out during the debate over Gyula László’s study, which can be regarded as the first draft of his theory, that the griffin- and tendrill-ornamented belt sets could hardly have been brought by the immigrant population arriving around 670 because they simply did not exist at the time.¹⁰⁶ To which Gyula László’s answer was that “Csallány’s ideas regarding the emergence of the griffin-tendrill belt sets as the result of an internal development is unacceptable because this would leave the high number of griffin-tendrill-type belt fittings of the Volga-Kama region without an explanation.”¹⁰⁷ However, there are no griffin-ornamented fittings in that region, only pieces adorned with tendrill designs!¹⁰⁸ Given the further variations on the dual

⁸⁷“Kouber victorieux passe la Danube avec tout son peuple, arrive dans nos régions, occupe la plaine Kéramésienne”: Lemerle (1979) 223.

⁸⁸“Dem Kaiser mit der abgeschnittenen Nase vertrauten nicht meine Onkel in Thessalonike”: Beševliev (1963) 97.

⁸⁹Beševliev (1970) 298; Szádeczky-Kardoss (1998) 218–219; Takács (2006) 83; Pohl et al. (2021) 326–330.

⁹⁰Golden (2006) 21, note 15, Biran (2015) 4; B. Szabó and Bollók (2018) 515–516.

⁹¹László (1978) 170.

⁹²Szabados (2016) 677.

⁹³Samu Szádeczky-Kardoss suggested that Pannonian Slavs had also departed together with Kuber, an assumption based on the fact that Maurus, one of Kuber’s commanders, also spoke Slavic in addition to the Greek and Bulgar Szádeczky-Kardoss (1998) 220, Pohl et al. (2021) 272.

⁹⁴Ziemann (2007) 142–160, esp. 142, 146, with the earlier literature. For a convincing localisation to the Middle Dnieper region, cf. Róna-Tas (2000); Atanasov (2017) 20–27.

⁹⁵Komatarova-Balinova (2016) 216. For the Sivašovka group, whose identification with *Magna Bolgaria* remains a matter of controversy, cf. Gulyás (2021).

⁹⁶Stančev (1957).

⁹⁷Olajos (2012) 208.

⁹⁸Cf. Fehér (2020).

⁹⁹Cf. Bajčorov (1985); Kyzlasov (1994).

¹⁰⁰For a recent overview of research history, cf. Szenthe (2020) 22–39.

¹⁰¹Alföldi (1924–1926); Reinecke (1928) 87–98.

¹⁰²László (1935) 27.

¹⁰³László (1955) 179–180; László (1970a) 185–186.

¹⁰⁴Csallány (1948); Vinski (1958) 58; Stein (1968a) 204; Stein (1968b) 233–244; Bialeková (1968) 30; Szabó (1968b) 34. For recent discussions of chronology, cf. Breuer (2007); Szenthe (2020) 55.

¹⁰⁵Radnóti (1956) 343, Kovrig (196), 123–148, 198–223, 229–230.

¹⁰⁶For Dezső Csallány’s remarks, cf. Harmatta (1955) 102.

¹⁰⁷Harmatta (1955) 105.

¹⁰⁸Goldina (1992).



conquest theory, István Bóna felt the need to clearly set down: his middle Avar group, linked to the population arriving around 670/680,¹⁰⁹ cannot be equated with the griffin-tendrill population of the late Avar period,¹¹⁰ which post-dated Kuber's settlement by a decade.¹¹¹ Had Gyula László heeded the critique levelled at him at an early date, he might not have formulated his theory at all.

3. The immigration and historical demography

Although none of the countless physical anthropological studies covering the period include a comparison of the populations of the early, middle and late Avar period, one point that nevertheless emerges clearly is that there are no substantial differences between the early and the late Avar period.¹¹² Harboursing national romantic sentiments, most physical anthropologists followed Lajos Bartucz's lead and had a predilection for Mongolids,¹¹³ who were tacitly regarded as the "real" Avars (with some modern projects involving archaeogenetic sampling too reflecting this bias). In contrast to the 16% noted by Pál Lipták, Tibor Tóth found 7.7% among the samples from 23 sites.¹¹⁴ Among the period's specialist, László Szathmáry expressed his surprise over the "relatively low number of immigrants".¹¹⁵ In a personal conversation in the early 1970s, Antónia Marcsik plainly stated that there is nothing in the physical anthropological record to support an immigration (see also below).

IV. Migration from the Kama region?

It must be borne in mind that when Gyula László first came out with his theory, he was only aware of the tendrill-ornamented belt fittings of the Kama region, but did not consider the similar ornaments from the Caucasus and the Crimea known at the time.¹¹⁶ (Since then, metalwork adorned with the distinctive round-lobed scrolls [*Kreislap-penranken*] has been published from several sites,¹¹⁷ belying the notion that this motif was typically "Avar".) His answer to the question of "Whence did they come and who were they?"¹¹⁸ was that the commingling of the "griffin" and the "tendrill" people occurred along the Kama, whence they

migrated to the Carpathian Basin¹¹⁹ – however, he side-stepped the issue of the possible background to this population blend as well as its exact date. (Later, he suggested that this commingling took place "along the Volga",¹²⁰ again bypassing the question of location, whether it lay closer to the Kama or the Kuban region). This idea can be traced to a laconic remark made by Arnold Marosi and Nándor Fettich on one of the burials of the Nevolino cemetery: "*Cette description correspond à celle de nos tombes avares de Hongrie.*"¹²¹ Their claim, "*Sans aucune doute, ce sont les traces d'une grande colonie avare qu'on a retrouvés au pays de Kama*",¹²² was adopted by Gyula László, according to whom the Avars had probably passed through the Kama region.¹²³ As Arnold Marosi and Nándor Fettich noted, "*La chronologie des antiquités du pays de Kama correspond à ce que nous avons écrit sur la crise [what crisis? Cs. B.] du règne des Avars vers 680.*"¹²⁴ It must be noted that they believed that the metalwork known from Hungary and the Kama region drew its inspiration from the same source – however, Falko Daim has convincingly demonstrated that scrolling tendrills were a widespread decorative motif in the eighth century.¹²⁵ There is no direct connection between the tendrill decoration of the Kama region and the ornamentation of Avar metalwork.

The possibility that a population group had migrated to the Kama region in the sixth century and that one of its group had thence moved south-westward has never even been suggested in studies on the Eastern European steppe.¹²⁶ The written sources on the steppe are quite clear on this point: the Avar embassy travelled to Constantinople through the northern Caucasus; the Avars resided on the eastern European steppe between 558 and 568; they negotiated with the Langobard envoys in the Lower Danube region;¹²⁷ they could hardly have been joined by Finno-Ugrian-speaking groups because the latter resided much farther to the north.

On the testimony of the archaeological record, there was a continuity in the cultures of the Kama-Ural region up to the period when Finno-Ugrian speaking peoples can be conclusively identified,¹²⁸ it was quite obvious to Gyula László that the Magyars who spoke a Finno-Ugrian tongue had to be sought in that region and not on the steppe, a point on which there is a general consensus in modern scholarship.¹²⁹ Gyula László asserted that the tendrill-ornamented belt fittings of the Kama region would suggest that

¹⁰⁹Bóna (1970). – Bálint (2008a) dissociated the onset of the middle Avar period from Kuber's migration to the Carpathian Basin, although this has been challenged on numismatic grounds: Somogyi (2004); Bálint (2008b).

¹¹⁰Bóna (1971) 323–324; Bóna (1985) 13.

¹¹¹Bóna (1984) 328.

¹¹²Éry (1970) 25; Éry and Marcsik (2015) 47. Fig. 1, 48. Table 8, 49; Fóthi (1995) 170; Fóthi (2004) 153–154.

¹¹³Lipták (1959).

¹¹⁴Tot and Firštejn (1970) 29–30. It is noteworthy both from a historical and anthropological perspective that significant similarities were found between certain Avar-period cemeteries and modern Khanty, Buryat and Tunguz skulls regarding the facial flatness index.

¹¹⁵Barabás et al. (1996) 83; Szathmáry et al. (2008) 18.

¹¹⁶For an overview, cf. Daim (2000) 107–109.

¹¹⁷Cf. Szenthe (2020) 381, Fig. 140.

¹¹⁸László (1978) 39.

¹¹⁹László (1955) 285.

¹²⁰László (1978) 53.

¹²¹Marosi and Fettich (1936) 87.

¹²²Marosi and Fettich (1936) 88.

¹²³László (1955) 179–182.

¹²⁴Marosi and Fettich (1936) 88.

¹²⁵Cf. Daim (2000).

¹²⁶Cf. Šmuratko (2010) 100–107, for the relevant literature.

¹²⁷Pohl et al. (2021) 60–61.

¹²⁸Sedov (1987) 6.

¹²⁹Bakró-Nagy (2012).



“following the great catastrophe of the 670s” – what catastrophe, we may well ask – compelled large population groups to leave the region, who then migrated directly to the Carpathian Basin.¹³⁰ (This idea perhaps led Pál Engel to surmise that the Magyars had skirted the eastern side of the Carpathians around 700.¹³¹) Gyula László believed that the Nevolino culture of the Kama region, whose main decorative motif was the tendril scroll, disappeared at roughly the same time from region along the Volga as the “griffin-tendril” people arrived in the Carpathian Basin;¹³² this assertion was made despite the fact that the monograph of the Nevolino cemetery had been published in Budapest at the time he was elaborating his theory,¹³³ a volume that would have contributed much to a more lucid view. We know that the Nevolino culture appeared sometime in the late fourth or early fifth century, that it developed on a local substrate, and that its decline can be dated to the earlier ninth century,¹³⁴ not to the time when griffin- and tendril-decorated belt sets began to be widely used in the Carpathian Basin. Gyula László’s other contention was that the “griffin-tendril culture evolved somewhere *near* the Nevolino population” [my italics]¹³⁵ – however, there is nothing to substantiate this assertion. Irrespective of any theoretical objections that could be raised,¹³⁶ his belief that the tendril was the main symbol of the Nevolino culture cannot be confirmed. It must also be borne in mind that some large strap-ends of the Avar period often have a griffin on the obverse and scrollwork on the reverse, meaning that an ethnic distinction between the two is pointless.

This element of the dual conquest theory is solely based on the occurrence of tendril-decorated cast bronze belt fittings in the broader Kama region. However, the number of such belt fittings is not particularly high and they rarely form a complete belt set because fittings decorated in a different style used in conjunction with small strap-ends in a different arrangement were far more popular. Antal Bartha and István Erdélyi had demonstrated already in 1961 that “there is not one single belt fitting that can be cited as a good parallel to the Avar-period griffin-decorated mounts, while the tendril-ornamented mounts differ even more strongly if one may say so.”¹³⁷

The early medieval period of the Kama-Ural region has an exceptionally intense archaeological coverage: the journal *Voprosy Archeologii Urala*, published since 1961 and also available in Hungary, focuses on the region’s early medieval cultures and their many aspects. The belt fittings in question

are known from the distribution of three archaeological cultures: the Lomatovo culture in the Upper Kama region, the Nevolino culture in the Sylva basin and the Polom culture along the Cheptsas.¹³⁸ Aside from the tendril-decorated belt fittings, the material record of the Kama region has little in common with the Avar material. It was already known at the time the dual conquest theory was formulated that these cultures in part had local roots and in part roots beyond the Urals,¹³⁹ there were no discernible traces of any “great catastrophe”, they did not disappear around 670/680, and neither are there any indications that their population migrated elsewhere.¹⁴⁰ Responding to this critique, Gyula László noted that according to István Fodor and István Bóna “there is no evidence that the ‘late Avars’ arrived to the Carpathian Basin from the Kama region or some other Finno-Ugrian territory and we may therefore harbour strong doubts that they had spoken Hungarian.”¹⁴¹ Moreover, István Fodor underlined that the ethnogenesis of the “Avars of the 670s” did not occur in the region where the Finno-Ugrian peoples lived¹⁴² – and Gyula László did concede that this point was the perhaps “most serious argument”, “a wholly warranted doubt”, adding that “all suppositions are uncertain”.¹⁴³ In his response to this critique, Gyula László cited Károly Mesterházy, who in his interpretation¹⁴⁴ contended that the Turkic nobles and the middle layer of the Conquest-period Magyars came from the Kama region. However, Mesterházy was referring to the Volga and Onogur Bulgar groups who had joined the Magyar tribes and not to any “griffin-tendril” groups migrating thence in the seventh century.¹⁴⁵ In the 1970s, the Tankeyevka and Bolshie Tigany burial grounds, both lying much farther away, stirred quite a sensation among scholars studying the early history of the Magyars, but these sites had little relevance for Avar studies since they lay far from the distribution of the Nevolino culture, they post-dated the seventh century by some two hundred years and they represented an entirely different archaeological culture.

Gyula László’s association of the belt fittings of the Kama region with the late Avar period is a textbook example of equating artefacts with peoples, a typical approach in Hungarian and Eastern European archaeology during the mid- and later twentieth century. The same approach is reflected in the assertion that “the belts with pseudo-buckle fittings appearing among the early Avar belt sets can also be found in the Kama region.”¹⁴⁶ In this particular instance,

¹³⁰László (1955) 179.

¹³¹Cf. Engel (2001) 11.

¹³²László (1970a) 163.

¹³³Erdélyi et al. (1969).

¹³⁴Šmuratko (2010); Goldina (2012).

¹³⁵László (1970a) 168; László (1978) 53.

¹³⁶Obviously, the ancestry of an ornamental motif is wholly independent of the origins of a particular assemblage type and neither can the latter be conclusively correlated with a “people”.

¹³⁷Bartha and Erdélyi (1961) 75.

¹³⁸Fodor (1973a); Bálint (1976) 87–91. For modern overviews of these cultures, cf. Rozenfel’dt (1987); Goldina (2004) 386–387, ris. 192; Goldina et al. (2012) 34–44.

¹³⁹Bartha and Erdélyi (1961) 75.

¹⁴⁰Fodor (1973a) 66–69; Bóna (1984) 329.

¹⁴¹László (1978) 109–110.

¹⁴²Fodor (1996) 239.

¹⁴³László (1978) 109–110.

¹⁴⁴László (1978) 110.

¹⁴⁵Mesterházy (1980) 45, 64, note 38.

¹⁴⁶László (1978) 109–110.



Gyula László accepted Dezső Simonyi's far-fetched reasoning that "the fifth-century Bulgars had perhaps been joined by Magyar population groups".¹⁴⁷ However, the pseudo-buckles of the Kama region were in use more than fifty years after the Avars passing through the region could have been joined by any Magyar groups from there, at least according to Gyula László's hypothesis. The pseudo-buckles of the early Avar period date from the earlier seventh century,¹⁴⁸ meaning that they are in part contemporaneous with the pieces in the Kama region; in any case, comparable belt fittings are also known from Eastern Europe and western Siberia, and therefore they can hardly be invoked as signalling ethnic affiliation.¹⁴⁹

Regarding the origins of the "griffin" population, Gyula László merely noted that the Ozora, Igar and Dunapentele assemblages,¹⁵⁰ which he associated with his assumed immigrant population, represented the legacy of the elite of a population that had arrived from the Caucasus [specifically from its northern part – Cs. B.].¹⁵¹ However, since these assemblages are known to predate the late Avar material by several decades, a Caucasian origin has never been posited for them, and neither has the migration of a population from the Caucasus to the Carpathian Basin ever been suggested. Gyula László later argued that the "griffin" people were of Inner Asian stock among the Avars, remarking that "the cultural affiliations of these belt fitting can be traced as far as Korea",¹⁵² which seems rather irrelevant given the long ancestry of the griffin in Western Asia.¹⁵³ Finally, a rather trivial point: none of the belt fittings from Ozora, Igar and Dunapentele bear a griffin depiction.

In sum, there is nothing to support the supposition that Kuber's warriors arriving to the Carpathian Basin around 670/680 had included groups of Finno-Ugrian speaking Magyars, or that "the *mass* [...] of Prince Kuber's people [...] had been Magyars" [my italics]. There is no evidence that the warriors had been Onogurs and the mass of commoners had been Magyars, and therefore "the homeland of the 'griffin-tendrill' population is largely irrelevant because" – and here comes a baffling claim by Gyula László – "it was exactly this group that was absorbed by the commoners."¹⁵⁴ Neither is there the slightest proof that "the Avars arriving in 568 and newer population groups in the sixth century had brought with them Magyar groups"¹⁵⁵ – who and where were the Magyars in the sixth century?

V. "Black and white Magyars"

Scholarship on the early history of the Magyars and the Hungarian Conquest period has quite understandably lavished much attention on the "white Ugrians" (*ugri bjelii*) and the "black Ugrians" (*ugri černii*) mentioned in the *Russian Primary Chronicle (Povest' Vremennych Let)* compiled in the 1110s. The chronicle draws a clear chronological distinction between them (Heraclius's reign on the one hand, and the time of the Pechenegs on the other). Nevertheless, the passages mentioning these Ugrians were accorded a prominent role in the dual conquest theory. On the one hand, *ugri* were invariably interpreted as designating the Magyars, on the other, the reference to the Avars of Heraclius's age more or less coincided with the date of Kuber's migration. However, the chronicle's Laurentian text translates *belye ugry* as Khazars and *černye ugry* as Magyars.¹⁵⁶ Gyula László had consulted neither the Russian, nor the English translation, nor Antal Hodinka's translation (which has "white" and "black" Ugrians¹⁵⁷), but instead used István Kniezsa's translation, which has "white" and "black" *Magyars*.¹⁵⁸ (Later, Slavist Péter Király similarly cited this translation, likewise neglecting the available Russian and American critical editions.¹⁵⁹) As far as István Kniezsa's translation is concerned, the Russian translation, Josef Marquart and Gyula Moravcsik's studies (which would have been available to him),¹⁶⁰ as well as János Harmatta's personal words of caution¹⁶¹ should have been important caveats to Gyula László before finalising his theory. The identification of *ugrii* with the Magyars is not as straightforward as it might seem from a Turkological point of view because *ugrii* is the singular of the *ugur* tribe of the *On ugur/ogur* tribal alliance,¹⁶² meaning that it has nothing to do with Ugrians! More recent Hungarian scholarship unanimously agrees on this point.¹⁶³

The Kievan chronicler in part drew from Georgius Monachus's tenth–eleventh-century Slavic translation¹⁶⁴ and in part from Nicephorus's *Breviary*, the latter being the source of the distinction drawn between the two *ugrii* and of the affair between the Avars and Heraclius.¹⁶⁵ There was never any doubt about the exact identity of the "black Ugrians" mentioned in the Kievan chronicle; one intriguing possibility suggested more recently is that this piece of

¹⁴⁷Simonyi (1968); Simonyi (1964).

¹⁴⁸Samu and Daim (2018).

¹⁴⁹Gavrituchin and Oblomskij (1996) 227, ris. 48.

¹⁵⁰Garam (1993) 62–64, 96–103.

¹⁵¹László (1955) 284.

¹⁵²László (1978) 25, 163.

¹⁵³Kazhdan et al. (1991).

¹⁵⁴László (1978) 171, 177.

¹⁵⁵László (1970b) 61. – For the sixth-century history of the Eastern European steppe, cf. Golden (1990) 256–284; Pohl et al. (2021) 26–33.

¹⁵⁶PLV (1950) I, 14, II, 224; Cross and Sherbowitz-Wetzor (1953).

¹⁵⁷Hodinka (1916) 33–35. The same holds true for more recent translation, such as the one by István Ferincz, which has "black Ugrians" (=Magyars): Balogh (2015) 24.

¹⁵⁸István Kniezsa's translation in Györffy (1958) 126.

¹⁵⁹Király (1997) 117.

¹⁶⁰Marquart (1903) 39; Moravcsik (1930) 86–88.

¹⁶¹Cf. László (1962) 21, note 64.

¹⁶²Róna-Tas (1996) 269.

¹⁶³Tóth (2013) 43; for a detailed discussion, cf. Balogh (2005) 31–45; Balogh (2015).

¹⁶⁴Toru (1974) 52–53.

¹⁶⁵Balogh (2005) 31–37, 45.



information had perhaps been imparted by Bruno of Querfurt, who had visited the Grand Duke Vladimir in Kiev in 1008.¹⁶⁶

Other important sources on the period which saw the emergence of the medieval Hungarian state are the early eleventh-century *Chronicle* of Adémar of Chabannes, a Benedictine monk living in central France, and the *Five Martyred Brothers* written by Bruno of Querfurt, who had been active in Magdeburg and Rome, and had a good personal knowledge of Eastern Europe. The problems surrounding White and Black Hungary, *Alba Ungria* and *Ungria Nigra*,¹⁶⁷ mentioned in Adémar's chronicle remains a debated issue in Hungarian scholarship. Several inaccuracies in his work have already been pointed out and it has been repeatedly noted that, unlike Bruno of Querfurt, Adémar had *never actually visited Hungary*.¹⁶⁸ According to recent monographs on his life and work, the tone of the passage in question recalls Tacitus and it seems likely that Adémar drew his descriptions of Central and Eastern Europe from a Carolingian geographical treatise.¹⁶⁹ The passage in which he compares the skin colour of the inhabitants of *Ungria Nigra* to that of the Ethiopians (*populus est colore fusco velut Etiopes*, "its inhabitants are of the same dark colour as the Ethiopians"), has been often discussed. It is more than telling that Bruno of Querfurt, who had visited Hungary twice and had been engaged in missionary activity for five years, only knew about the *Nigri Ungri* from hearsay (*audivi*)¹⁷⁰ and makes no mention of what would have been a rather striking physical trait.¹⁷¹ Adémar could only have learnt about the "black Hungarians"¹⁷² from Bruno of Querfurt's work.¹⁷³ Similarly, there has been much speculation on the exact location of "Black Hungary" in Hungarian scholarship.¹⁷⁴

Most scholars tend to link these two sources, despite the geographic and cultural differences between them, and they invariably cite the black/white symbolism of the steppe peoples in their discussions.¹⁷⁵ However, it seems a futile exercise to link the latter to the problems of Christian missionary activity; in this case, "black" simply denotes "pagan".¹⁷⁶ In any case, the "white" *ugrii* of the *Primary Chronicle* can hardly be identified with the Magyars and thus whoever the "black" and "white" Magyars were at the turn of

the millennium is basically irrelevant for the dual conquest theory.

VI. Possible dialects?

Gyula László posited that "at least two, strikingly different dialects can be distinguished in the early Hungarian linguistic relics", which in his view again supported his dual conquest theory.¹⁷⁷ However, Hungarian linguistic studies have not detected a duality of this type in the earliest Hungarian language relic: there is no indication either of two languages¹⁷⁸ or of two dialects¹⁷⁹ in the foundation charter of Tihany Abbey from 1055, a fact that had been established at the time Gyula László proposed his theory,¹⁸⁰ in which he claimed that "what we have are two kindred peoples, two languages, of which one was assimilated by the other".¹⁸¹ (What kinship, we may well ask: linguistic, anthropological or cultural?) Dialect and language are not identical, and neither can one or the other be equated with a particular "people". In his review of one of Gyula László's other books, Finno-Ugrian linguist László Honti noted that his linguistic assertions are unacceptable in linguistic scholarship.¹⁸²

VII. Physical anthropology¹⁸³

Pál Lipták, the doyen of physical anthropology in Hungary in the 1960s and 1970s, was only familiar with Hampel's Groups A and B as far as Conquest period archaeology was concerned. He subscribed to the general research direction still followed today in the countries of the former Soviet Union, which focuses on anthropological types and taxons¹⁸⁴ that are then directly interpreted in terms of ethnic history by several archaeologists. Gyula László adopted Pál Lipták's assertions based on the physical anthropological record available at the time,¹⁸⁵ as well as his arguments, which reflected an alarming unfamiliarity with steppe history. Gyula László compared the "Turanid" (i.e. Europo-Mongolid) and the "Pamirid" (corresponding to *sredneaziatskij*, "Central Asian", in the Russian literature) with the personal names regarded as dating from the Conquest period (assigned to western Old Turkic in Turkic studies¹⁸⁶). He was aware of the glaring contradiction between the Finno-Ugrian language and the Turkic anthropological type, but nevertheless drew – methodologically

¹⁶⁶Nazarenko (2002) 161.

¹⁶⁷Ademarus (1841) 31,2: 129–130, Gombos (1937) 15–16, 430.

¹⁶⁸MGH (1934) 129, 86, note 89, Györfy (1977) 70; Veszprémy (2003); Font (2012) 72; Halmágyi (2012a) 16.

¹⁶⁹Cf. Grier (2012).

¹⁷⁰Szabó (2012) 139.

¹⁷¹Halmágyi (2012b) 101, note 89.

¹⁷²Veszprémy (2003) 463.

¹⁷³Bruno (1884).

¹⁷⁴For recent studies with the earlier literature, cf. Kristó (1985); Sames (2010) 89; Koszta (2013) 26, note 27.

¹⁷⁵Ludat (1953); Golden (1980) I, 103, 142.

¹⁷⁶Csákó (2011) 26, with further literature.

¹⁷⁷László (1978) 38, 56, 107. These claims are erroneous and the citations are inaccurate, cf. Nyíri et al. (1976) 51–52.

¹⁷⁸Szabados (2016) 676, cites Chapter 39, *De administrando imperio*, which, however, refers to the two tongues spoken by the Turks and the Kabars.

¹⁷⁹Maticsák (2020) 460.

¹⁸⁰Bárczi (1947) 3, 6, 8–9; Benkó (1957) 60–84; for a recent discussion, cf. Kiss (2017) 208.

¹⁸¹László (1978) 107.

¹⁸²Honti (1981).

¹⁸³For an overview of current Hungarian research in this field, cf. „Zegernyei“ (2012).

¹⁸⁴Cf. Debec (1948).

¹⁸⁵Lipták (1957).

¹⁸⁶Róna-Tas and Berta (2011).



fully unacceptable – a spate of historical conclusions.¹⁸⁷ This was the lens through which he looked at Pál Lipták studies and the physical anthropological record of the Avar and the Conquest period, and claimed to have identified the mass of a Hungarian-speaking population. Pál Lipták identified the late Avar and Árpáadian Age population as being predominantly Europids, while he assigned the majority of the Conquest-period skulls to the Europo-Mongolid and Pamirid types,¹⁸⁸ and assumed the mass survival of the Avars.¹⁸⁹ In this respect, he followed Gyula László's lead, who only considered Hampel's Group A – the elite and middle layer of the tenth century – as representing the legacy of the Conquest period and had little interest in the period's commoners (Hampel's Group B). He believed that the lack of the Cromagnoid A type among the latter and its presence in the Avar- and Conquest-period material confirmed this. (Later, doubtless influenced by the dual conquest theory, Pál Lipták spoke of "Ugrian Magyars" and "Turkic Magyars".¹⁹⁰) It must be borne in mind that the mass presence of the Europid anthropological type in the Carpathian Basin hardly comes as a surprise since the Bronze Age and Pál Lipták knew full well that the Europid type comprised several sub-types, with the Cromagnoid A type representing the Proto-European population.¹⁹¹ The immensely simplistic "correlation" between the Europid-type Avar and the Árpáadian Age physical anthropological record is historically irrelevant.

When István Fodor challenged the equation of the late Avars with the Magyars citing Kinga Éry's research findings,¹⁹² Gyula László responded by noting that Lajos Bartucz had identified a significant Avar-period component in the medieval and post-medieval Hungarian population,¹⁹³ adding that "denying the continuity of 'large masses of the late Avar population' would be tantamount to denying biological laws".¹⁹⁴ In contrast to this curious line of reasoning, the craniological traits of the first-to sixth-century population of the Hungarian Plain can be discerned more often in the tenth century than those of the seventh-to ninth centuries,¹⁹⁵ the general physical anthropology of the late Avar period differs from that of the tenth century in Transdanubia,¹⁹⁶ and we also know that the Magyars of the Conquest period found a predominantly Europid-type

population in the Carpathian Basin upon their arrival.¹⁹⁷ One compelling piece of evidence is that a genuine similarity between the Avar- and Conquest-period skeletal material could only be demonstrated in 4.5% of the theoretically potential cases.¹⁹⁸ As in those years and decades, so today, Hungarian physical anthropological studies focus on the assessment of the human remains from particular cemeteries and regions, as well as on the comparison of the anthropological remains and clusters from various periods.¹⁹⁹ They still heed Lajos Bartucz's recommendation from 1957: "caution should be exercised in the immediate assignation to anthropological types and in drawing ethnogenetic conclusions thereof".²⁰⁰ Modern physical anthropology is not concerned with taxonomic determination or with linking these to peoples or archaeological cultures²⁰¹ because biological traits are not ethnospecific and are therefore unsuitable for drawing direct historical conclusions.

A breakthrough in biological studies can be expected from the currently ongoing archaeogenetic analyses on large series of samples collected from the key cemeteries of the Carpathian Basin.²⁰² The assessment of genetic profiles can shed light on the kinship relations in various cemeteries and the dynamics of a particular cemetery's use-life.

VIII. *Uungariorum marcha*

Samu Szádeczky-Kardoss and his wife Teréz Olajos, both renowned specialists of Byzantine studies, had supported the dual conquest theory from the very beginning, although somewhat cautiously. Taking his cue from Gyula László's book published in 1955, Samu Szádeczky-Kardoss wrote a rather inaccurate assessment of the late Avar archaeological record and the Vrap Treasure (which he believed could be associated with Kuber).²⁰³ A few months after the debate over the dual conquest theory in the Kossuth Club, he held a lecture together with Pál Lipták at Szeged University, in which he essentially voiced his endorsement of the theory.²⁰⁴ Soon afterwards, Teréz Olajos published a paper in which she argued that the phrase *Uungariorum marcha* ("the boundary of the Wangars") in one of the charters of Louis the German (r. 843–876) was a Slavic form of the ethnonym of the Onogurs; she associated it with *hungarus*, i.e. the Magyars.²⁰⁵ Slightly later, Samu Szádeczky-Kardoss

¹⁸⁷László (1962) 15, 17.

¹⁸⁸Lipták (1957) 252–253;

¹⁸⁹Lipták (1983) 19–31, 49–52.

¹⁹⁰Lipták (1977) 237.

¹⁹¹Lipták (1971) 218–220.

¹⁹²Fodor (1973b) 132.

¹⁹³László (1978) 111 – however, the cited study claims nothing of the kind, cf. Bartucz (1958).

¹⁹⁴László (1978) 112. – As a matter of fact, not all archaeologists, historians and geneticists believe that the populations of previous periods had survived, even though more recent studies seem to point in this direction: Fóthi (2015).

¹⁹⁵Szathmáry et al. (2013) 712.

¹⁹⁶Barabás et al. (1996) 82–83.

¹⁹⁷Éry (1994) 217–224.

¹⁹⁸Éry (1982).

¹⁹⁹Éry (1982) 65; Szathmáry (1978); Szathmáry and Guba (2004).

²⁰⁰Bartucz (1958).

²⁰¹E.g. Katzenberg and Saunders (2000).

²⁰²Pohl et al. (2021).

²⁰³Szádeczky-Kardoss (1968).

²⁰⁴On April 23 and 29, 1970: Szádeczky-Kardoss (1971). – He expressed his support, although very carefully worded, later, too: Szádeczky-Kardoss (1990) 224–225. Golden (1990b) 243 devotes no more than a single sentence to Gyula László's theory.

²⁰⁵Olajos (1969).



presented her supposition as “an *argument* supporting the late Avar migration assumed by Gyula László” [my italics].²⁰⁶ Gábor Vékony and Péter Király both commented on this toponym, the former suggesting that it was the name of an Avar ethnic group²⁰⁷ (although there is no evidence for ethnic groups of this type), the latter set this toponym and several other similar-sounding ones known from the broader area in a similar context, although with certain reservations, suggesting that similar toponyms and personal names perhaps preserved the memory of Avar monks who had converted to Christianity, while conceding that further studies would be needed to clarify this issue.²⁰⁸ Irrespective of the above interpretations, it stands to reason that there could well have been Onogurs other than the ones in Kuber’s retinue in the Avar Khaganate (and we specifically know about the presence of Bulgars).²⁰⁹ Additionally, there is evidence that steppean ethnonyms were often “inherited” by one people from another, as there is for the indiscriminate use of some of these ethnonyms in the written sources, as well as for their inconsistent use for denoting ethnic groups and constantly shifting nature.

The charter in question is dated May 9, 860, and records that the king donated twenty plots of land to the Mattsee monastery in Zobern and in the area between Spratzbach and Zobernbach. The passage in question reads as follows: *[u]sq[ue] in summitatem ill[ius] montis qui dicitur Uuangulariorum m[archa]*.²¹⁰ Hungarian research has not really kept abreast of the historical research on the Carolingian period, which established that the *mons* in question is a solitary mountain in whose broader area there were twenty *mansiones* in the interior of the duchy of Odalrich, making it unlikely that it had been a marchland (*Mark*);²¹¹ the toponym in question can be derived from Bavarian *wang* meaning *Wiesenheng* (“meadow slope”),²¹² and thus it has no relevance for the possible survival of the Avars.

EPILOGUE

In the foregoing, I surveyed the main points of Gyula László’s dual conquest theory and their reception. Not one single element is tenable. Owing to the fateful turn of history, it became impossible to publish a translation of one of Gyula László’s main works on the Magyars of the Conquest period written in 1944, even though its overall

approach and perspective would doubtless have been a guiding light in the archaeological assessment of the tenth-century Magyars in Central and Eastern European scholarship. His analyses²¹³ and reconstructions of various artefacts,²¹⁴ his meticulous examination of the period’s gold and silver metalwork,²¹⁵ and his ethnographic approach to the past²¹⁶ are still relevant and must-read studies today. His perspective and thought were immensely influenced by the personal contacts during his early years with the best minds of the period’s Hungarian ethnographic research.²¹⁷ Around 1940, he had ties to the covertly anti-German intellectuals,²¹⁸ which he continued to maintain even as late as the 1970s. As he was fond of saying, he aspired to be “the poor man’s archaeologist”, one of the reasons that he sought to identify the Magyar-speaking commoners, whom he believed to have found in the Avar period.

A critical assessment of the current evidence available to scholarship on the origins and early history of the Magyars is indeed an immense task:

- on the testimony of the linguistic record, the early Magyars separated from the Finno-Ugrian language family in the ninth–eighth century BC and from their closest linguistic cognates in the fifth century BC;
- in the Bronze Age, they lived in a steppean cultural milieu;
- they are designated as *tourkoi* in the ninth-to twelfth-century Byzantine sources and on seals;
- around the time of the Conquest, the Hungarian language was enriched with a high number of culturally significant Chuvash-type Turkic loanwords;
- solely Turkic-type male personal names have survived of the elite of the ninth-to tenth centuries;
- the ninth- and tenth-century dignitary names are of Turkic origin;
- the earliest tunes and dances are rooted in Finno-Ugrian and steppean traditions;
- the current physical anthropological and genetic record of the ninth-to eleventh centuries indicates the dominance of the Europid type, although this might also be a reflection of sampling bias.

A fresh look at the above points will doubtless contribute to a better understanding of the social, linguistic and cultural conditions of Hungarian society during the Conquest period.

²⁰⁶Szádeczky-Kardoss (1971) 13.

²⁰⁷Vékony (1981) 215, 225.

²⁰⁸Király (2006).

²⁰⁹Cf. Szádeczky-Kardoss (1970).

²¹⁰MGH (1934) 145–146, No 101.

²¹¹Kramarík (1969). I am grateful to Walter Pohl, who wrote a lengthy letter explaining the background to this passage.

²¹²Pohl (1987) 50.

²¹³E.g. (László) 1950.

²¹⁴E.g. László (1940a,b, 1943, 1955).

²¹⁵E.g. László (1940).

²¹⁶E.g. László (1941, 1960).

²¹⁷For Zsigmond Bátky, István Györfy and Károly Viski, cf. Paládi-Kovács (2018) 15–84.

²¹⁸Borbándi (1976).



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