

THE LAST CENTURY OF PANNONIA

The history of the last century of Pannonia (376—476) is an organic part of the process of decline and dissolution of the Roman Empire, and at the same time it is closely connected also with the problem of Roman continuity. Thus it is not a mere chance that this complex of questions has long since raised a lively interest both in Hungarian and foreign historical research. Regarding the decline and fall of the Roman Empire, and the fate and survival of Pannonian romanization within it, two conceptions are opposed to each other. One of them regards the 4th century as a period of decline and it counts with the destruction of romanization in Pannonia already at the end of this century. The other conception, on the other hand, still considers the 4th century in many respects the golden age of the Roman Empire and would like to extend the survival of romanization in Pannonia as long as possible, eventually up to the Hungarian conquest.¹ Naturally, both views have several variants and shades, and endeavours for intermediate solutions are not missing either. In recent times a certain approach can be observed between the two conceptions. In fact, the view is more and more spreading that the 4th century in its whole cannot yet be regarded as the period of decline, while, on the other hand, the conviction according to which we cannot speak about the continuity of Pannonian romanization up to the Hungarian conquest, also becomes stronger.²

In Hungarian scientific investigation this question was elaborated for the last time with full thoroughness and with the utilization of the available literary, numismatic and archaeological sources by A. Alföldi in his work entitled «Untergang der Römerherrschaft in Pannonien». I - II. (Berlin

¹ For the problem of the decline and fall of the Roman Empire see S. MAZZARINO: *La fine del mondo antico*. Milano 1962 and J. VOGT: *The Decline of Rome*. London 1967 with the earlier literature.

² Cf. for example A. RADNÓTI: *MTA II OK* 5 (1954) 489—508, with the remarks of T. NAGY, L. BARKÓCZI, K. SÁGI and J. SZILÁGYI, *ibidem* 510—521, 523—527; K. SÁGI: *Acta Ant. Hung.* 9 (1961) 397—459; A. KISS: *A Janus Pannonius Múzeum Évkönyve 1965* (Yearbook of the Janus Pannonius Museum 1965). 81—123; K. SÁGI—F. M. FÜZES: *Agrártörténeti Szemle* 9 (1967) 79—97; A. KISS: *Arch. Ért.* 95 (1968) 93—101; K. SÁGI: *Acta Ant. Hung.* 18 (1970) 147—196.

Leipzig 1923 - 1926). In contrast to the earlier conception Alföldi pointed out that the circulation of Roman coins discontinued in Pannonia only about the year 395, and he endeavoured to show that the West Roman Empire surrendered Valeria only in 406, and Pannonia I in 433 to the Huns.

The archaeological material discovered later on, however, pointed to the fact that the regime of the Huns did not cause a sharp break either in Pannonia or in the Great Hungarian Plain. Thus during the last few centuries I pointed out also several times that in the territory of Pannonia certain elements of Roman life survived also during the existence of the Hunnish Empire and in the disturbed times following Attila's death.³

The further development of this optimistic judgement of the fate of Pannonian romanization was attempted by L. Várady in his recently published book entitled «Das letzte Jahrhundert Pannoniens 376—476» (Budapest 1969). The essence of Várady's historical conception is that part of the Huns lived in Europe already in the 2nd century (the *Xoivoi* to be found in Ptolemy), and these later on, together with an East Gothic and Alan group, settled down in 379 as *foederati* in the territory of Pannonia II, between Servitium and Saldae along the Save. The above mentioned three ethnic groups stayed in this area up to 399, when the Goths were transferred to Picenum and the Alans to Valeria. The Hun military settlements in Pannonia II were dissolved only in 427 upon the demand of the Huns living outside the Roman Empire, who in 433 gained right to settle down similarly as *foederati* in the place of the earlier Hun military settlements in Pannonia II. According to Várady's conception thus the historical fate of Late Roman Pannonia was determined essentially by the Gothic-Alan-Hun group settled in the territory of Pannonia II in 379, which kept far from the province the external Huns and the other barbaric peoples. Attila's Huns had never occupied the territory of Valeria and Pannonia I, and also in Pannonia II they received certain places of settlement only as *foederati*. Thus in contrast to the earlier conceptions, according to Várady the regime of the Huns did not affect at all the life of the Pannonian Romans, the cities continued to flourish and the population also continued to increase under the protection of the barbaric *foederati*. And even beyond this, Várady maintains that Roman administration prevailed in Pannonia up

³ See J. HARMATTA: Introductory study to the book of N. FETTICH: A szeged-nagyszéksósi hun fejedelmi sírlelet (La trouvaille de tombe princière hunnique à Szeged-Nagyszéksós). Budapest 1953. AH XXXII. 6, 104, as well as the lecture «Göten und Hunnen in Pannonien» at the congress of the DAW «Probleme der Spätantike» in Berlin on the 30th November 1965, and the lecture «Les Huns et le changement et conflit à la frontière danubienne au IV^e s. ap. J. C.» at the conference of the FIEC «Changement et conflit au IV^e s. ap. J. C.» in Bordeaux on the 8th September 1970. However, on the basis of convincing argumentation of T. NAGY: Buda régészeti emlékei. Budapest műemlékei (Archaeological monuments of Buda. Ancient monuments of Budapest) II. Budapest 1962. 64, 109 (note 20), the finds of the Csúseshegy Roman villa, valued earlier as a sign of Roman-Barbarian coexistence, have to be stricken out from the rank of the relevant archaeological observations.

to 488 and the fortified Roman cities up to the settling in of the Langobards, up to 546 remained the bases and centres of romanization.⁴

This historical conception is undoubtedly interesting. It fits in the line of the historical investigation of Pannonia initiated by Alföldi, and thus it is worth of discussion. Just therefore I supported the publication of Várady's work especially as a suitable material for debate, on the basis of the positive expert opinions of I. Hahn and L. Barkóczy, because debate about certain bold opinions very often can considerably promote the solution of a question. I saw, however, that the showing of my name as publisher's reader of the book causes a certain confusion both in the circle of local and foreign scholars, inasmuch as in certain investigators it creates the impression as if I would agree with Várady's historical conception. In a certain degree this is supported also by the fact that in my earlier works I also shared the optimistic historical valuation of the decline of the Roman Empire. It seems, therefore, to be necessary to delimit my own standpoint from the historical conception of Várady's book. Of course, it is impossible here to expound my opinion in all questions discussed or touched by Várady. This would mean the writing of a monograph. Thus I confine myself to pointing out my standpoint differing from that of Várady in the most essential questions of history, methodology and aspect of history, without entering into debate.

1. I hold first of all fundamentally incorrect V.'s method according to which he wants to draw up the history of the last century of Pannonia only on the basis of the written sources. This means in comparison with Alföldi a serious methodological step backwards, which can by no means be justified. In certain questions of detail the numismatic or archaeological sources can eventually be disregarded, but by no means in the formation of the general picture. If V. had taken into consideration the archaeological sources,⁵ he could have seen that we cannot speak about the flourishing of the Pannonian Roman cities in the 5th century.

2. I do not hold satisfactory and reassuring the linguistic and objective interpretation of the texts of Greek and Latin sources in V.'s work. There are even such cases when V. enriches the Greek grammar with new, non-existing forms, viz. from the gen. pl. *συμβαλλόντων* he concludes the nom. pl. *συμβαλλόντοι* (p. 306), on which even the accent is incorrect. At another place (p. 169) in the text of an inscription he shows a verbal form as two separate words, viz.: *quem c u p e r et plebs aliena s u u m*. These linguistic uncertainties can by no means be approved. We can even less agree with the arbitrary method followed by V. in the interpretation of the classical texts. Very often

⁴ L. VÁRADY: *Das letzte Jahrhundert Pannoniens 376-476*. Budapest 1969. (Cf. especially the chapter «Summa operis», 375-402.)

⁵ See e.g. T. NAGY: *Buda régészeti emlékei* (Archaeological monuments of Buda), chapters 63-64 on the discontinuation of the Roman life of Aquinicum.

he almost creates the impression in the reader that in his opinion the Greek and Latin words can be given any deliberate meaning, if his theories demand it. Here are one or two examples: p. 59 according to V. *maximum flumen* means «flooded river», and *ripa* «steep bank». These interpretations make possible for V. to draw a grandiose picture of the battle of Siscia, viz.: the cavalry of Theodosius threw itself into the flooded river, swam it and jumped on the steep river bank. Apparently he has no idea that it is impossible to swim a flooded river if one is in armament, or to jump up to the steep river bank from the water. V. is carried away by his own grandiose conception, viz.: only the Hun–Alan cavalry could be capable of such a performance, therefore it really was the Hun–Alan cavalry (p. 60)! At the same time he does not even notice that according to the text of Pacatus in question the horsemen drove their horses into the river by their spurs, and he does not know either that neither the Huns nor the Alans used the spur.

The same source tells that the barbaric peoples voluntarily offered their services to Theodosius, who also accepted it, *ut et limiti manus suspecta decederet et militi auxiliator accederet* «that, on the one hand, the troop suspicious for the *limes* should depart, and on the other hand that a helper should join the (Roman) soldiers». In contrast to Alföldi, V. would like to refer this to the Gothic–Alan–Hun *foederati*, who according to his assumption were settled between Servitium and Saldae, along the Save, i. e. not on the Roman *limes*. However, between the *populi barbarorum* and *Gothus ille et Hunus et Italanus* the text also mentions *omnes Scythicae nationes* and nothing proves it that under all the three designations the same should be understood, and the latter phrase rather clearly excludes this. However, V. would like to refer the *populi barbarorum* voluntarily offering their services by all means to the Gothic–Alan–Hun *foederati*, but he is disturbed in this by the phrase *limiti manus suspecta decederet*, which points to the circumstance that the barbaric peoples under discussion lived outside the *limes*. Therefore he asserts that the verb *decedere* does not mean here «depart, march away», but «avoid». Naturally, this meaning of the verb occurs only in connection with persons with dative government. Therefore, V. is compelled to the further assumption that the word *limes* «border» appears here as a personified concept. He does not take into consideration that the dative *limiti* does not depend from the verb *decedere* but from the word *suspecta* «suspicious», and he does not know either that he wants to prove such two assumptions with each other, which are unprovable themselves.

We cannot agree with these and similar arbitrary linguistic interpretations. Unfortunately, there is hardly any source where V. would not act so arbitrarily. These forced interpretations of sources are closely connected with the basic conception of V.'s work. Since he wants to prove by all means the exceptional significance of the historical role of the Gothic–Alan–Hun

foederati settled in Pannonia in 379, he is compelled to deny the use by the Romans of auxiliary troops from the circle of Huns and Alans and also other barbaric peoples living outside the borders of the Roman Empire. For this reason he wants to interpret forcibly otherwise all such passages, where exterior Barbarians are mentioned as allies of the Romans. In the course of this he asserts that the Greek prepositional phrases with *πέραν*, *ὑπέρ* have lost their concrete place denoting function. Thus for example the phrase *οἱ πέραν τοῦ Ἰστροῦ βάρβαροι* does not mean «the Barbarians living beyond the Danube», but only «Barbarians living in the Danube region» (p. 86).⁶ This is an unacceptable, baseless assertion. But it cannot be accepted either that the phrase *οἱ παρὰ τὸν Ἰστροῦ βάρβαροι* of Sozomenos determines the geographical position of the Pannonian Gothic–Alan–Hun *foederati*, because according to V. these were exactly settled not along the Danube but along the Save.

It is similarly the erroneous interpretation of the preposition that makes for me unacceptable the use of one of the passages of Sozomenos recommended by V. (p. 520) for the definition of the place of settlement of the Alan–Hun *foederati*. Here a «barbaric land» is mentioned, from where Alarichos sets out to Epeiros, and the location of which is defined by the phrase *πρὸς τῇ Δαλματία καὶ Παννονίᾳ* «adjacent to (or in the vicinity of) Dalmatia and Pannonia». From this it is clear that whatever the origin of this denomination «barbaric land» was, territorially it was situated outside of both Dalmatia and Pannonia. Consequently it could not be identical with the place of settlement of the Gothic–Hun–Alan *foederati*, placed between Saldae and Servitium, which however was located by V. to the territory mentioned above exactly with reference to the Sozomenos passage discussed.

I do not hold correct that superficial and forced way of use of the sources either, which can frequently be observed in V.'s work. V. wants to support the placing of the settlement of the Gothic–Alan–Hun *foederati* between Saldae and Servitium also with an inscription, according to which a certain Amantius was for 20 years bishop of two leaders and two tribes in Iovia. On the basis of Egger, V. sees in the two leaders Alatheus and Saphrax, and in the two peoples the Goths and the Alans, and finally in Iovia he sees the town of the same name situated along the Drave about 30 kilometres southeast of Ptuj. But this concretely excludes the possibility of finding the place of settlement of the Gothic–Alan–Hun *foederati* on the banks of the Save, between Saldae and Servitium, because in this case the episcopal seat would have been separated from its parish by 150 air kilometres.

⁶ V.'s defective knowledge in the field of Greek accent is clearly shown by the fact that he does not know that the nom. pl. of the word *βάρβαρος* is not second-, but third-acute (p. 86, see also p. 306).

Similar desultoriness is shown also in the appraisal of the sources used for the definition of the age of Vegetius. V. refers to his earlier work,⁷ in which, referring the sentence «. . . *quantum profecerit murorum elaborata constructio, Roma documentum est . . .*» of the Prologue of Book IV to the restoration of the fortification walls of Rome ordered by Valentinian III in 440, he regards it as an «indisputable evidence» to the effect that Book IV was written a few years after 440. However, he does not quote the continuation of the sentence which goes as follows: «*quae salutem civium Capitolinae arcis defensione servavit*». Thus we have to do not with the restoration of the walls of Rome, but the construction of the *Capitolina arx* is the example, which proves the usefulness of the construction of the walls.⁸ Now, as a further evidence for the late dating of Vegetius, that part of the *Epilogus* of book III, which praises the eminence of the Emperor in *sagittandi peritia, equitandi scientia, currendi velocitas* and *armaturae exercitatio*, is referred by V. to Valentinian III on the basis that according to Ioannes Antioch., Valentinian III after riding in the Campus Martius dismounting his horse, was just going on archery, when he was attacked by his murderers (p. 496). However, this argumentation is unacceptable, because on the one hand the phrase of Vegetius . . . *ad sagittandi peritiam, quam in serenitate tua Persa miratur . . .* shows that he kept in view shooting with the bow on horseback, while Valentinian III went on shooting afoot, and on the other hand this praising could be said about almost every ruler of the age, because they were well versed in the handling of weapons, and it was also said because it was almost a commonplace. Let us see a few examples: Ammianus Marc. XXI. 16,7 (on Constantius) *equitandi et iaculandi, maximeque perite dirigendi sagittas, artiumque armaturae pedestris perquam scientissimus* or Claudianus Fesc. de nupt. Hon. Aug. I. 1–3 *princeps corusco sidere pulchrior, Parthis sagittas tendere docior, eques Gelonis imperiosior, . . .* etc.

It would be difficult to agree also with that type of forcible interpretation of sources which we find in V.'s work in connection with one of the reports of Orosius. According to Orosius on the occasion of the invasion of Radagaisus the leaders of the Hun and Gothic auxiliary troops were Uldin and Sarus. Uldin's appearance is very uncomfortable for V., because it shows that Stilicho in 405/406 used already Hun auxiliary troops from outside the Empire, and not Pannonian Hun *foederati*. V. thinks to avoid this difficulty with the following series of assumptions (p. 201 ff.). Orosius did not know who was the commander of the Hun auxiliary troops (N.B. Orosius wrote 10 years after the events!), and since the commander of the Gothic auxiliary troops was named, under the stylistic compulsion of parallelism he sought a name also for the

⁷ L. VÁRADY: *Későrómai hadügyek és társadalmi alapjaik* (Late Roman military affairs and their social bases). Budapest 1961. 281 foll.

⁸ As, otherwise, this had been pointed out already long ago by S. MAZZARINO: *L'impero romano*. Roma 1956. 542.

commander of the Hun auxiliary troops, and since he knew that in that time the Huns living outside the Empire were under the rule of a certain Uldin, he mentioned this as the commander of the Hun auxiliary troops. Not a single element of the whole series of assumptions can be proved. It is not rendered likely by anything that in Italy 10 years after the events Uldin's name would have been forgotten. Besides this V. himself stresses that Orosius did not know about Uldin's role in connection with the East Roman Empire in the years 400 and 408. But in fact, he could have got to know Uldin's name only in connection with these events, if he had not known even otherwise that he was the commander of the Hun auxiliary troops fighting against Radagaisus. Otherwise we cannot agree with the assertion of V. either that in that time in connection with the East Roman Empire Uldin would have been the only leader of the Huns known by name, because through the mission of Olympiodoros in the year 412, the names of the Hun kings Donatos and Charaton were already known in 417, at the time when the work of Orosius was written.

We cannot approve that strange way of the use of the sources in V.'s book either that he very often attributes such meanings to the texts which they do not contain. Thus for example in connection with the well-known passage of Claudianus *De cons. Stil.* (II. 184 ff.), which tells that from the provinces delegates, the personifiers of the provinces are coming: *undique legati properant . . . Gallus . . . Poeni . . . Pannonius* etc., V. maintains that everywhere *colonus* has to be supplemented (p. 136), although the text says *legati*. But V. goes still much farther than this, when he forms a whole historical story in connection with the good crop of the Pannonian provinces in the year 383 (530 ff.). According to Ambrosius at this time there was a grain shortage in Rome, while in the Pannonian provinces (*Pannoniae*) there was a surplus what they sold. V. asserts that the Pannonian grain was transported to Rome and that as from this time the province of Valeria was placed under the jurisdiction of the *vicarius* of Rome in order to ensure this way the grain supply of the city. The text of the source, however, does not give any foothold for the assumption of the transport of Pannonian grain to Rome, or for the assumption that this good crop was exactly in Valeria (Ambrosius says *Pannoniae*), or that the province of Valeria was placed under the jurisdiction of the *vicarius* of Rome on account of this.

3. Since in my opinion the historical valuation, and in many cases even the linguistic interpretation, given by V. to the most important sources is not correct, it is self-evident that his historical conception is not acceptable either. Already the assumption is unacceptable that part of the Huns would have lived in Europe already in the 2nd century.⁹ I do not see it proved that

⁹ Ptolemy's datum *Χοῦνοι* came to Europe as a result of a cartographic error, see J. HARMATTA: *Les Huns et le changement et conflit à la frontière danubienne au IVe s. ap. J. C.*

the Gothic–Alan–Hun group settled in 379 in Pannonia would have played such a significant role, as it is presumed by V., neither that at this time the Romans would not have used other barbaric (Hun, Alan, etc.) auxiliary troops originating from territories outside the border. In my opinion the placing of the area of settlement of the Gothic–Alan–Hun group between Saldae and Servitium is also unfounded. Even less convincing are those series of assumptions, with the help of which V. wanted to reconstruct the later history of the Gothic–Alan–Hun *foederati* at the end of the 4th century and the beginning of the 5th century. In my judgement V.'s assumption regarding the status of Valeria is unprovable and at the same time unlikely, and in the absence of a detailed argumentation and material of evidence V.'s assertion regarding the flourishing of the Pannonian cities up to 546 also appears to be unreliable.

4. In my opinion in V.'s work even the use of scientific literature is not quite correct either. He likes to argue with the earlier scientific literature and presents the earlier conceptions, especially Alföldi's conception in an adverse light. On the other hand he very often keeps silent about the fact that his assumptions shown to be new had been raised already long ago in scientific literature. Moreover, he also attributes to himself views expounded in works quoted by him, and gives the matter an appearance as if the investigators concerned would have represented other opinions. Thus for example in connection with the Amantius inscription he writes (168 foll.) that on his part he can supplement Egger's explanation with the statement that the two leaders were the Goth Alatheus and the Alan Saphrax, and the two peoples the East Gothic and the Alan–Hun group. But exactly this is Egger's opinion expounded and motivated in detail.¹⁰ It comes under a more serious judgement that in connection with the basic conception of his book, *viz.* his theory on the Pannonian Gothic–Alan–Hun *foederati*, V. keeps completely silent about the fact that it originates from P. Váczy, who in contrast to Alföldi clearly stressed the significance of the Gothic–Alan–Hun group settled in Pannonia, referred to the data of the panegyric of Pacatus already on this, and followed their historical role from Gratianus and Valentinian II up to 401.¹¹ V. also keeps silent about the circumstance that further essential elements of his conception can be found already in the work of B. Szász, who tried to further develop Váczy's theory regarding the Pannonian Gothic–Alan–Hun group in several respects, and regarding the *Xoῖνοι* mentioned by Ptolemy elaborated the historical conception, which is followed also by V. in his work up to the

¹⁰ R. EGGER: JÖAI BB 21/22 (1922–1924) 335 foll., 339 foll. Be it said by the way that in connection with Iovia, the seat of Amantius, we can also think about Iovia-Felsőheténypuszta.

¹¹ P. VÁCZY: A hunok Európában. Attila és hunjai (The Huns in Europe. Attila and his Huns). Budapest 1940. 72 ff., 80, 292. I remark that VÁCZY misunderstands the sentence *notari infrequens verebatur* of Pacatus Paneg. 32, 4, when he translates it as follows: «They were afraid that their number will be held small» (*op. cit.* 73).

presumed role of the Hun cavalry in the battle of Siscia.¹² I do not agree with this act of V. not only from the viewpoint of science ethics, but I do not approve of it also because by this he can rightly be accused with plagiarism in international scientific literature.

5. Last but not least, I cannot agree with V.'s aspect of history manifested in his Introduction (14) and in the Epilogue (372). It frames his work, essentially consisting of mosaics of source-criticism and history of events, with the concepts of the «cultural circle» and «high culture» theory, instead of trying to elucidate the process of dissolution of the Roman Empire with the analysis of the forces of production and relations of production.

6. Probably it becomes clear from the aforesaid that I have held the publication of V.'s work correct only as a material for debate. Besides the freedom of expression of opinions, I was governed in this also by the experience recently formulated by A. Debrunner, the great scholar in classical conciseness: «...der Fortschritt der Wissenschaft sehr oft aus dem Wettspiel zwischen kühner Phantasie und schärfster Kritik hervorgeht.»¹³ As a matter of fact V.'s work is full of the boldest products of phantasy, now only sharp criticism must follow.

Budapest.

¹² B. SZÁSZ: *A hunok története* (History of the Huns). Budapest 1943. 40, 106, 117, 121.

¹³ A. DEBRUNNER: *Kratylos* 3 (1958) 29.