Ethnogenesis, especially the ethnogenesis of the Slavs, is still a popular research topic, and not just in Russia. While presenting new material and arguments, the essays above are a good illustration of that popularity, and they offer much that will require careful consideration by future students of early Slavic history. Instead of responding to each and every one of the points raised by contributors to this volume, I shall confine my comments to highlighting some lines of reasoning which are either particularly promising or problematic. In the meantime, it bears emphasizing that more nuanced approaches to ethnogenesis and unexpected evidence keep emerging from studies from several countries and in several languages. Ethnicity is now viewed by both archaeologists and historians as fundamentally performative, which explains the emphasis placed on identity as a category of historical analysis (for the absence of an archaeology of identity in Russia, see my paper in this volume). The critique of the ethnogenesis model embraced by many German and

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Austrian scholars inspired by Reinhard Wenskus’s work has now drawn attention to the need to treat written sources as texts, using traditional means of textual analysis, as well as current theoretical approaches to literary analysis (e.g., narratology) in order to establish the cultural context and to define authorial purpose. Much has recently been written in that vein about Jordanes, but there are already signs of change in scholarly approaches to the works of other authors mentioning the Slavs, especially Procopius. The implications of such a “literary turn” for the analysis of written sources remain to be seen, but it has already become clear that in order to make any progress the research on (Slavic) ethnogenesis needs to distance itself from the practice of perpetuating the stereotypes embedded in the late antique ethnography. Meanwhile, new approaches to the construction of ethnicity through material culture and ethnicity have also transformed our understanding of the relations between the late antique Empire and the barbarians. Ongoing research should clarify how the militarization of the sixth-century Balkans affected the rise of new ethnic groups on the northern frontier of the Empire. A hitherto neglected approach to language contact promises dramatic changes in the study of early medieval languages and their relation to ethnic identities. And, as some now maintain, throughout the early Middle Ages (Common) Slavic may have been used as a lingua franca, that too would have obvious implications for the notion of a Slavic ethnogenesis and migration.
For migration is the real bone of contention. That is indeed the case needs no better illustration than Paul Barford’s paper. As he points out, a polemical debate is currently taking place in Poland between advocates of a Polish Urheimat of the Slavs (primarily linguists or linguistically trained historians) and those students of Kazimierz Godłowski who embraced the idea that the Slavs had come to Poland from an Urheimat located in Subcarpathian Ukraine. Both positions are therefore predicated upon the idea that the Slavs must have come from somewhere, a more or less large area in Eastern Europe, which may be viewed as their primordial homeland. Barford in any case thinks that the migrationists are wrong, but he still believes that until some time before the mid-sixth century, when they appear before the eyes of the East Romans on the Danubian plain... the Slavic speaking groups were separated from the Roman world by three to five hundred kilometers of steppe, forest steppe and mountains, although, of course, no evidence exists for such a statement. Historians are now moving away from the idea that there ever was such a thing as the Great Migration. Barford still believes that since this was after all the Volkerwanderungszeit, «some Slavic speaking groups... moved south and southwest to the areas faced by Justinian’s frontiers». Again, no evidence of migration from the north exists for the areas faced by Justinian’s frontiers, a point that Barford himself acknowledges. Nevertheless, the problem with Barford’s idea is even deeper. It has long been noted that for an ethnic group to exist, a name must be attached to it, which represents two concomitant processes taking place in any ethnogenesis: self-identification and recognition by others. For the Slavs to exist anywhere, one would expect a certain group of people to call themselves by that name or be called by such as others. We do not know anything about how the inhabitants of the areas faced by Justinian’s frontiers called themselves in their own language. All we have is the testimony of sixth-century authors, such as Procopius, who claim that those were Scalvines. Whether or not the Scalvines spoke a Slavic language, we at least know who those people were, even though our only source of knowledge about that is what is reported by outsiders. What about the regions of Europe, for which there is no such report? The problem, as Barford rightly notes, «is that “the Slavs” is as much a linguistic concept as an ethnic or archaeological one». No source written in the sixth century or earlier mentions the Slavs in what is now Poland. For the sixth century, there are in fact no ethnic names to be associated to the territory of present-day Poland. We also have no way of knowing how inhabitants of the settlement sites of Barford’s «Central Polish groups» called themselves. Nor is it possible to put any ethnic names on the «Mogila group», which appeared at some point in southeastern Poland, allegedly from outside the territory of present-day Poland. What then is the basis for calling any or all of those peoples <Slavs>? If I correctly understand Dmitrii Polyviannyy’s argument, the very use of that name is historically associated with the classification methods and


patterns of Byzantine authors. When referring in a ninth-century inscription (otherwise written in Greek) to the «Slavs under imperial rule» and the «Slavs who live along the sea coast» and are not ruled by the emperors, the Bulgar ruler Omurtag (or the stone carver he employed for the job) had in mind an audience of Byzantines, not of Bulgars. The same appears to be true about Constantine of Preslav writing in Old Church Slavonic in the 900s about the «Slavic people soaring high, having all turned toward baptism». Denis Alimov’s exceptional analysis of the historiographic topics of «migration» and «Christianization» in reference to the Croats in Dalmatia makes a similar argument from the other, Latin-speaking side of the evidence. In three out of five inscriptions mentioning his name, Branimir, a ruler otherwise known from five letters of Pope John VIII dated between 879 and 882, is described as ruling over some group of people. Two of them (the inscriptions found in Nin and Zdrapanj near Skradin) call Branimir dux Slcavorum and dux Cruatorum. Why was Branimir a ruler of the Slavs in some inscriptions and of the Croats in others? Given that most papal documents and contemporary Frankish sources have no knowledge of the Croats, it is possible that Branimir’s title in the Nin and Zdrapanj inscriptions is simply the result of a Roman or Frankish usage. Alimov believes however that «Slavs» and «Croats» were two names for one and the same ethnopolitical community. But as John Fine has long observed, the variation in Branimir’s title may not be accidental. Few among those who studied the inscriptions have paid sufficient attention to their archaeological context and the possible audience for their messages. All five inscriptions with Branimir’s name originate from churches and were carved onto architraves and gables of altar screens. None of them was dedicated to the Slavs (Slavi), but not much ink was spilled over the explanation of those misspellings. A third inscription found in Sopot near Bokovac has Branimir’s name associated with such titles of ruler that are simply the result of a Roman or Frankish usage. Alimov believes however that «Slavs» and «Croats» were two names for one and the same ethnopolitical community. But as John Fine has long observed, the variation in Branimir’s title may not be accidental. Few among those who studied the inscriptions have paid sufficient attention to their archaeological context and the possible audience for their messages. All five inscriptions with Branimir’s name originate from churches and were carved onto architraves and gables of altar screens. None of them was dedicated to Branimir himself, whose name appears only as a means to authenticate (and date) the dedication.
on behalf of some other donor\textsuperscript{17}. In other words, Branimir’s title matters to the donor as a means to confirm publicly the act of the donation. The name of the donors is preserved relatively well in the inscription from the Church of St. Ambrose in Nin: \textit{Dem partibus domino B[ra]nnimero dux Sclavorum} See: Delonga V. Latinski epigraficki spomenici... S. 207. \textsep\textsuperscript{16} Delonga V. Latinski epigraficki spomenici... S. 217-218.\textsuperscript{19} For the title of \textit{zhupan} in ninth-century Croatia, see: Curta F. Southeastern Europe in the Middle Ages, 500-1250. Cambridge; New York, 2006. P. 139-141.\textsuperscript{20} Matijevic-Sokol M., Sokol V. Hrvatska i Nin... S. 70.\textsuperscript{21} Let me employ here two modern analogies in order to clarify the point. No one among those called in the United States \textit{Hispanics} actually uses that label to define himself or herself, for he or she is always of Cuban, Colombian, Venezuelan, or Peruvian origin. Much like \textit{Slavs}, \textit{Hispanic} is a label created by outsiders for purposes that were initially foreign to all or any of the interests of those to whom that label referred. Similarly, \textit{Indian} was initially not a name willingly used or even accepted by those called so by the European settlers to North America, people who would otherwise employ for themselves such names as Potawatomi, Kwakiutl, or Hopi. However, when, in more recent times, at stake were political or economic interests depending upon the classification produced for that purpose by the United States government, both Hispanics and (American-) Indians quickly assumed that identity forced upon them in order to further their group interests. Indeed, to this day, both \textit{ethnic} groups — otherwise completely artificial creations based on \textit{umbrella} terms — operate effectively in the political field manipulating that identity, which has by now been recognized by others, from politicians to media pundits.

\textsuperscript{17} The standard phrase including Branimir's name is preserved relatively well in the inscription from the Church of St. Ambrose in Nin: \textit{Dem partibus domino B[ra]nnimero dux Sclavorum}. See: Delonga V. Latinski epigraficki spomenici... S. 207.

\textsuperscript{16} Delonga V. Latinski epigraficki spomenici... S. 217-218.

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\textsuperscript{21} Let me employ here two modern analogies in order to clarify the point. No one among those called in the United States \textit{Hispanics} actually uses that label to define himself or herself, for he or she is always of Cuban, Colombian, Venezuelan, or Peruvian origin. Much like \textit{Slavs}, \textit{Hispanic} is a label created by outsiders for purposes that were initially foreign to all or any of the interests of those to whom that label referred. Similarly, \textit{Indian} was initially not a name willingly used or even accepted by those called so by the European settlers to North America, people who would otherwise employ for themselves such names as Potawatomi, Kwakiutl, or Hopi. However, when, in more recent times, at stake were political or economic interests depending upon the classification produced for that purpose by the United States government, both Hispanics and (American-) Indians quickly assumed that identity forced upon them in order to further their group interests. Indeed, to this day, both \textit{ethnic} groups — otherwise completely artificial creations based on \textit{umbrella} terms — operate effectively in the political field manipulating that identity, which has by now been recognized by others, from politicians to media pundits.
as to claim that even the «Slavic excursus» in the Russian Primary Chronicle may be viewed as an attempt to obtain recognition by the same cultural means that have consecrated the Slavs in the political discourse of early Byzantium. He may be right as far as the use of the Byzantine chronicle of George the Monk is concerned, but the Rus' chronicler's approach is different from both Procopius of Caesarea (see below) and Constantine of Preslav or others writing in the Cyrillic-Methodian tradition. Unlike all of them, the chronicler assigned to the Slavs a homeland on the Danube, no doubt modeling the history of the Slavs after that of the Hebrews in the Old Testament (twelve tribes spreading out over the face of the earth from an original homeland). Moreover, he used «Slavs» as a name for a territory, along with Epirus, Illyricum, and Lychnitis. His goal was clearly not just to link the Slavic Rus' to the lands where the Slavs had received the Word in their own language, but also to advance the idea of a new Chosen People. It is that that I had in mind when writing that with the Russian Primary Chronicle, another story begins, namely that of the «national» use of the Slavs «for claims to ancestry».

But let us return to Barford's paper about the Polish lands. According to him, «linguistic and other types of evidence seem to show that Slavic languages were being spoken over a wide area of east-central Europe by the ninth century at the latest». What about the sixth century? Do we know anything for sure about the language(s) spoken in what is now Poland? Honest scholars have long given a negative answer to such questions. It would be a mistake, as Barford is right to point out, to associate river names of archaic Slavic type with any specific archaeological culture. If so, then it would be equally mistaken to associate such names with the Slavs as known from the written sources. Barford writes: «We are therefore faced with the paradoxical situation that the area may have been occupied by Slavs who were not in the slightest interested in wearing so-called “Slavic fibulae”». In the light of my remarks above, it is curious that Barford did not see a much simpler explanation for his paradoxical situation: those rejecting «Slavic fibulae» were not Slavs, at least not like those who not only wore «Slavic fibulae», but were also called Slavs (Sclavenes) by contemporary, early Byzantine authors. This remains true even if, as Barford invites us to do, one shares the widespread belief that Slavic was spoken in what is now Poland during the sixth century (a belief otherwise based on no evidence whatsoever). «Slavs did not become Slavs because they spoke Slavic, but because they were called so by others».

Similar problems emerge from Petr Shuvalov's contribution. Like Barford, Shuvalov believes in the existence of a «(Baltic-)proto-Slavic population» somewhere in the forest belt of Eastern Europe, even though the very existence of any such population remains to be demonstrated, for it is not attested by any source. Much like Barford, Shuvalov's Slavic-speaking groups «moving south and...»
southwest to the areas faced by Justinian’s frontiers. Shuvalov’s Balto-Proto-Slavs «spilled» over the fertile lands of the forest-steppe belt in the aftermath of the Hunnic invasion of the late fourth century. But unlike Barford, Shuvalov is upset by my supposedly flummocky formulations, particularly the INVENTION OF THE SLAVS. He writes: «Curta denies the proto-Slavs the possibility of migrating from the north». Shuvalov sees no problem with the old model of Slavic ethnogenesis advanced by Russian scholars such as Mark Shchukin and Dmitrii A. Machinskii. That the Slavs were aware of being Slavs without having to be invented by Byzantine authors results (so Shuvalov) from an episode in Theophylact Simocatta’s History. Given the weight of this piece of evidence in Shuvalov’s line of arguments, the episode is worth citing in full:

On the following day three men, Sclavenes by race (αυτόν τον τρείς Σκλαβηνός το γένος), who were not wearing any iron or military equipment, were captured by the emperor’s bodyguards. Lyres were their baggage, and they were not carrying anything else at all. And so the emperor enquired what was their nation (βασιλεῖας διήρησε τι το ἐθνός σωτοῦ), where was their allotted abode, and the cause of their presence in the Roman lands. They replied that they were Slavones by nation and that they lived at the boundary of the western Ocean (οἱ δὲ τὸ μὲν ἐθνὸς ἡφάσαν περισκέψαν Σκλαβηνοί πρὸ τοῦ τέρματι τοῦ δυτικοῦ ὑπερήφανον Ἑλέαντον); the Chagan had dispatched ambassadors to their parts to levy a military force and had lavished many gifts but refused him the alliance, assenting that the length of the journey daunted them, while they sent back to the Chagan for the purpose of making a defense these same men who had been captured; they had completed the journey in fifteen months; but the Chagan had forgotten the law of ambassadors and had decreed a ban on their return; since they had heard that the Roman nation was much the most famous, as far as can be told, for wealth and clemency, they had exploited the opportunity and retired to Thrace; they carried lyres since it was not their practice to gird weapons on their bodies, because their country was ignorant of iron and thereby provided them with a peaceful and trouble-free life; they made music on lyres because they did not know how to sound forth on trumpets. For they would quite reasonably say that for those who had no knowledge of warfare, musical pursuits were uncultivated, as it were.

This Shuvalov takes as a proof that the Slavs were calling themselves by that name and were fully aware of their own ethnic identity. He astutely, yet rhetorically asks «how could those northerners, who had never before visited the Danube region, have been able to tell Emperor Maurice that they «were Slavones by nation» if not fully aware of that very fact?». Shuvalov clearly took the text at face value, without even questioning its authenticity. That the Slavones claimed that they had heard «that the Roman nation was much the most famous, as far as can be told, for wealth and clemency» raised no red flags for him. He does not seem to have been troubled at all by the contradiction between the fact that the country of the Slavones is said to have been «ignorant of iron» and the intention of the qagan of the Avars «to levy a military force» from among those same peace-loving Slavones. Nor does he seem to have noted the striking similarity

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between this story and Tacitus’ description of the Fenni, who like Theophylact’s Slavs, had no iron\(^{31}\). More importantly, he let himself duped by Theophylact’s otherwise worn-out narrative strategy: For the Selavence to say that they lived at the boundary of the western Ocean, they must have known that there was also an eastern and southern ocean. In other words, the lyre-players, who (and on this point Shuvalov understood Theophylact correctly) had never before visited the Roman Empire, must have had some solid knowledge of Greek and Roman geography, from Herodotus to Ptolemy. Perhaps more importantly, when asked about it, they must have been aware not only of the fact that they were «Selavence by nation», a point Shuvalov was much too quick to pick, but also of such abstract categories of ethnographic classification as \(\text{\varepsilon\varphi\omega\varphi}\). For all their peaceful and trouble-free life on the beaches of the western Ocean, Theophylact’s Slavic musicians seem to have kept themselves busy studying all those works of Greek and Roman ethnography in preparation for their interview with Emperor Maurice.

As The Making of the Slavs states (P. 18), the transactional nature of ethnicity resides in that, «in the practical accomplishment of identity, two mutually interdependent social processes» are at work, «that of internal and that of external definition (categorization)». The argument is that the Slavs cannot be recognized by others as such, without knowing themselves that they are Slavs. Conversely, there is no point for any group of humans to affirm being Slavs, if by doing so, they are not going to be distinguished from, and recognized by others who are not Slavs. In this typically social interactionist perspective, objective cultural difference is always a by-product of something else, largely to be explained with reference to social interaction. It is important to understand, however, just how the «others» come to perceive «us» as Slavs when «we» declare ourselves to be Slavs. In other words, for ethnic identity to be visible (literally), the very process of ethnic formation must involve the manipulation of material culture, be that dress, food, house architecture, or pottery decoration. The self-conscious use of specific cultural features as diacritical markers distinguished an ethnic group from others. Ethnic boundaries are therefore created in specific social and political configurations by means of material culture styles.

Andrej Pleterski’s impassioned plea would have us shift the emphasis from ethnicity to religion. To him, since the internal categorization in the process of ethnic formation is fundamentally subjective, there is no necessary material culture correlate. Instead, he suggests that the we regard the archaeological evidence as pointing to religious, and not ethnic phenomena. Pleterski proposes that the room structure of a cluster of Slavic settlements (\(\text{\z\!h\!p\!a}\), «nest») be viewed as based on fundamental religious principles, primarily three distinct sanctuaries dedicated to three different deities. Ethnic identity would thus be just another name for membership in a religious community, an idea which echoes similar claims made in recent studies on early Slavic ethnicity\(^{32}\). Moreover, Pleterski suggests that religious structures may have been responsible for the rise of the first leaders in Slavic society, a process he believes to date back to the fourth century, given that the word \(\text{knez}\) in Slavic is a Gothic loan. This of course is a very tempting hypothesis. Unfortunately, there is no evidence to substantiate it. There are so far no early Slavic sanctuaries or any special-purpose buildings known from any part of Eastern Europe\(^{33}\). The description which Procopius gives of the...


\(^{33}\) Not a single one of the structures discussed by Irina P. Rusanova (Русанова И. П. Истоки славянского язычества. Культовые сооружения Центральной и Восточной Европы в I тыс. до н. э. – I тыс. н. э. Чернин, 2002) can be securely dated to the sixth or seventh century.
religion of the Scavinenes and the Antes is in fact a watered-down version of Greek paganism, with little, if any relation to the actual beliefs of the sixth-century barbarians living north of the Lower Danube. Whatever archaeological evidence exists of ritual or magical practices, they seem to be strongly associated with domestic activities taking place in individual households, and not with special-function structures or buildings, such as sanctuaries. Finally, the trocan of the nineteenth- or twentieth-century Slovenian folklore can hardly be regarded as evidence of the religion of the early Slavs in the sixth, and much less in the fourth century (if one can even speak of Slavs at that time).

Almost in opposition to Petr Shuvalov, Boris Todorov rightly insists upon origines gentium accounts in Byzantine sources to be analyzed not as «objective reports of real events», but as later constructions of a rather intellectual, if not altogether bookish, nature. Barrimg new insights into the way in which Byzantine authors, such as Constantine VII Porphyrogenitus and Theophanes Confessor, obtained their information about «native» versions of local history, what their accounts of origines gentium can tell about ethnic history is naturally limited. But they do seem to illuminate Byzantine attitudes towards the ethnic groups whose history they purport to tell.

I find myself in general agreement with Todorov’s propositions. Our opinions differ, however, on the sources of information used for origines gentium. Todorov contests the very evidence of such texts for the «collective memory of the empire’s neighbors», as well as its interpretation. Thus he suggests that, while the purpose of such texts was to justify imperial claims, serious doubts exist about the authenticity of much of the information they provide. The reasoning is unclear. Though he probably made up the association between the Serbs and Emperor Heraclius or the etymology of the ethnic name «Dukljans» («Diocletians», allegedly derived from the name of Emperor Diocletian), Emperor Constantine did not make up the word «servula» (apparently referring to the pig-skin footwear of Balkan peasants) from which the name of the «Serbs» is supposedly derived. Similarly, he could not have concocted out of thin air the names of the five brothers (Kloukas, Lobelos, Koszentis, Mouchlo, and Chrobatos) and of two sisters (Touga and Bouga) who are said to have split from the Croats «beyond Bavaria, where the Belcroats are now» and have come with their folk to Dalmatia. Nor is it possible to treat as Emperor Constantine’s invention the information on the Pechenegs contained in chapter 37 or that on the Milingoi and Ezeritai of Peloponnesus to be found in chapter 50 of the De administrando imperio. As Danijel Dzino rightly shows, the general goal of that work was didactic. Emperor Constantine’s purpose was to educate his son, as clearly spelled out in the Proem:

"Lo, I set a doctrine (διδακτη) before thee, so that being sharpened thereby in experience and knowledge, thou shalt not stumble concerning the best counsels and the common..."

Footnotes:

36 The fallacy of anachronistically linking sixth-century accounts of migrations from across the Danube into the Balkans to nineteenth- or early twentieth-century linguistic or folkloric phenomena in Balkan countries is also evident in Ivan Matić’s insistence upon the different map distribution of the pair of words vatra / oganj or the historical significance of the krsna slava. The problem with both examples is of course that there is no way to date them with any precision. Without chronological precision, one should instead be cautious about using any ethnographic or linguistic parallels for historical reconstruction.
Dzino points out that because of its didactic character, *De administrando imperio* relies heavily on a didactically useful form of ethnographic classification. Although generally drawing inspiration from the model of the late antique ethnography, Emperor Constantine pays little if any attention to such details as the «ancient» history of the «nations» described. His is a much more practical goal, namely to classify and explain, not to display his knowledge of the ancient authors:

And if in setting out my subject I have followed the plain and beaten track of speech and, so to say, idly running and simple prose, do not wonder at that, my son. For I have not been studious to make a display of fine writing or of an Atticizing style, swollen with the sublime and lofty, but rather have been eager by means of every-day and conversational narrative to teach you (SiSa^ai) those things of which I think you should not be ignorant, and which may without difficulty provide that intelligence and prudence which are the fruit of long experience.

To classify is to give names to political categories. As Dzino persuasively argues, «Croats» was a name for those, who in the aftermath of the Carolingian encroachment into Southeastern Europe, regarded themselves (and were regarded by others) as neither «Romans» («Dalmatians»), nor «Slavs». In that respect, Boris Todorov is quite right: in Emperor Constantine’s narrative, *origines gentium* function as a justification for such political categorization. But unlike Todorov, Dzino claims — quite rightly, in my opinion — that the story of the five brothers and two sisters moving from the land of the «Belocroats» into Dalmatia is not a fabrication. Instead, it must have been a local version of Croat history, one that served the interests of local elites and justified the tribal or clan distinctions within the new Croatian polity. Emperor Constantine simply put a new spin on it, thus giving the myth a new meaning within the context of his scholarly effort towards ethnographic explanation and teaching of good government. But where did Emperor Constantine learn about the story of Kloukas, Lobelos, Kosentzis, Mouchlo, Chrobatos, Touga, and Bouga? It has long been noted that the detailed information about the Magyars («Turks») in chapters 38 and 40 of the *De administrando imperio* may well derive from conversations with the Magyar envoys accompanying *harka* Bulcsu to Constantinople, in 948, where he received baptism with no other than Emperor Constantine Porphyrogenitus himself as sponsor at the baptismal font.

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* Constantius Porphyrogenitus, *De administrando imperio*. P. 45, 47.
* DAI 1: Constantius Porphyrogenitus, *De administrando imperio*. P. 49.
* DAI 30: Ibid. P. 143.
I think it is not too far-fetched to imagine Emperor Constantine relying on informants from Dalmatia, perhaps one of those «Croats who wish to engage in commerce, traveling round from city to city, in Pagania and the gulf of Dalmatia and as far as Venice».

This by no means implies that the accounts of Croatian history to be found in De administrando imperio are a transcription of the testimony of Emperor Constantine’s informant. But it would be equally misleading to ignore that certain categories of information in De administrando imperio could not have been found either in the works of ancient ethnographers or, for that matter, in the imperial archives.

Whatever his sources, Emperor Constantine’s general attitude towards authentic information appears to be somewhat different from that of Procopius of Caesarea. As Anthony Kaldellis has recently showed, the latter’s excursus on the Hephtalites, who are compared in civilization to the Romans and the Persians «is undeniably peculiar, the Roman reader would probably find it utterly preposterous. I believe that it is entirely an invention of Procopius. None of the information available to him would have justified it».

Sergei Ivanov disagrees with that. He simply dismisses the testimony of Procopius’ own work without engaging with the analysis that substantiated its significance in the Making of the Slavs. To him, it is without any question that the «Slavic excursus» in the Wars is entirely the result of the personal interviews Procopius had with Scævene and Antian mercenaries in Italy. Ivanov even claims to know the precise date of those interviews, namely April 537. According to him, «the barbarians communicated to Procopius their own self-designations, Slavs and Antes, as well as the name which they themselves initially used for both tribes, Sporoi. Whether or not one shares the appraisal of my work as «deliberate manipulation of facts», Ivanov’s treatment of Procopius at this point is a far cry from his older views on the Wars. In his 1991 comment on the passage (Wars VII.14.29) in which Procopius mentioned Sporoi as the old common name of both Scævenes and Antes, Ivanov astutely noticed Procopius’ playful intentions, for he derived Sporoi from the «sporadic» settlements of the Scæveni and the Antes».


43 Kaldellis A. Procopius of Caesarea... P. 72. — Turning the Hephtalites into civilized humans amounts to diminishing the civilized character of those with whom Procopius compares them, namely the Romans. In other words, the goal of Procopius’ narrative strategy is not to justify imperial claims, but to criticize the very basis for such claims, at least under the rule of Justinian.

44 Иванов С. А., Гиндин Л. А., Цымбурский В. Л. Провинциальный Киевский // Свод древнейших письменных известий о славянских, т. I. (I—VI вв.) / Сост. Л. А. Гиндин, С. А. Иванов, Г. Г. Литаврин. М., 1991. С. 227–228. — True, Ivanov claims that Sporoi is a hapax, deriving from some Slavic self-designation. Needless to say, there is absolutely no evidence for that, either in the text of the Wars or in any other source (pace George Vernadsky). There is no connection between Procopius’ Sporoi and Jordane’s Spalh. It seems to me to be much easier to explain Procopius’ playful etymology in terms of his own narrative goals. His remark about the Sporoi comes at the end of the «Slavic excursus» in which he mentions, among other things, that the Scævenes and the Antes «live in pitiful hovels which they set up far apart from one another, but as a general thing, every man is constantly changing his place of abode» (Wars VII 14.24; English translation: Procopius of Caesarea. Wars / Ed. by J. Haury; Engl. transl. by H. B. Dewing. Vol. 5. Cambridge, Mass.; London, 1924. P. 271). Sporoi is a quite adequate name for those whom Procopius viewed as living sporadically, each man at a large distance from his neighbor and constantly on the move. This is further substantiated by the examination of Procopius’ usage of the word he employs for the Scæveni and Antian «hovels»: Sporoi. In two other contexts in which he employs the same word (Wars II 19.32 and IV 6.10), reference is made either to temporary shelters for soldiers on campaign, or to houses of nomads.
He now sees the story of the Sporoi as nothing less than a myth about origins. In his eyes, Procopius’ playful etymology has turned into one of those origines gentium discussed by Boris Todorov, for Ivanov strongly believes Procopius’ to be a «native», authentic story.

The shift in emphasis from Ivanov’s earlier endorsement of a (moderate) instrumentalism to his current primordialist views is quite clear in his discussion of language. He contests that «language in general did not play any consolidation role». Against this, Ivanov argues that «people were not indifferent as to what language they spoke». However, at the beginning of his paper, he also argued that «one does not know that one speaks, say, proto-Slavic, unless of course one is a linguist». This seems to contradict all the sociolinguistic evidence available: in a bilingual situation, when one switches from one language to another, one always conceptualizes that transition in some way, even if the name attributed to one or the other of the two languages is simply «our language». But one could not speak any language without calling it by some name, and thus applying to the linguistic map the same categories of classification as those used for the ethnic map. Be it as it may, the question again is how to take Procopius’ statement about Sclavenes and Antes having «the same language, an utterly barbarous tongue (ἐστὶ δὲ καὶ μίας φωνῆς ἐξετήσας βαρβαρος)». This Ivanov interprets to mean that «the Antes were not initially speakers of Slavic, but ultimately adopted the Slavic language». There is, however, no mention in Procopius of what was the language that both Sclavenes and Antes spoke: the only adjective modifying the noun φωνή is ἐξετήσας. This is unusual for Procopius, who always uses the noun φωνή together with some ethnic attribute, i.e., always mentions a language of some kind: Latin, Gothic, Armenian, Phoenician, Persian, or Greek. That Procopius had knowledge of at least some of those languages is beyond any doubt. He described a horse whose body was dark gray, except for this head, which was white: «Such a horse the Greeks call “phalius” and the barbarians “balan”». The barbarians in question are the Goths, for Procopius explains that the Goths understood that they needed to shoot at that particular kind of horse, since it was Belisarius’ horse. By contrast, nothing suggests that he knew the linguistic value of «barbarous», when applied to the language spoken by Sclavenes and Antes. To claim that the language referred to by Procopius was what we now call (Common) Slavic is an over-interpretation, at the very least, and a gross mistake, at most. All that Procopius tells us is that, to his ears, the language that both Sclavenes and Antes spoke was «utterly barbarous». This is to be read as an ethnic stereotype («barbarians cannot speak but barbarous languages»), not as a bit of information resulting from Procopius’ «long and detailed conversations» with Sclavene and Antian mercenaries in Italy. This is further confirmed by what he has to say about further similarities between Antes and Sclavenes:

Nay further, they do not differ at all from one another in appearance. For they are all exceptionally tall and stalwart men, while their bodies and hair are neither very fair or blonde, nor indeed do they incline entirely to the dark type, but they are all slightly ruddy in colour. And they live a hard life, giving no heed to bodily comforts, just as the Massagetae do, and,
-like them, they are continually and at all times covered with filth; however, they are in no respect base or evil-doers (νομοπολοι καιρονομενοι), but they preserve the Hunnic character in all its simplicity (ἀλλὰ καὶ τῷ ἀρχαῖο διασώεται τῷ Οὐσιον ήπος)\(^9\).

Simplicity was a typically barbarian feature to Procopius\(^9\). That he mentioned the Sclavenes and the Antes as neither base, nor evil-doers raises a red flag as to his intentions. Πολλοίπισε is an attribute he typically associated with such characters as John the Cappadocian, Emperor Justinian, or tyrants, in general\(^1\). His remarks are therefore to be read as an attempt to turn the Sclavenes and the Antes into boni sauvages, the mirror into which the wicked Romans need to look in order to understand their moral degradation. What about the physical features? The ruddy complexion of the Antes into

Antes «also Slavs», as claimed by Ivanov? And what exactly is the relation between Procopius' Sclavenes and Ivanov's Slavs?

Ivanov takes issue with my conclusion that «the Slavs did not become Slavs because they spoke Slavic, but because they were called so by others». He states that «the Slavs became Slavs, because they called themselves Slavs». This is to turn again to the realm of «linguistic beliefs», rather than facts, for no evidence exists that any Slav-speaking people in the early Middle Ages called themselves «Slavs». Nor do we know what was the name which Procopius' Sclavenes used for themselves, although most historians presume that Procopius employed that very name, with which the Sclavenes called themselves. As I conceded in the Making of the Slavs, «it might be that "Sclavene" was initially the self-designation of a particular ethnic group» (P. 119). It is nonetheless significant that in Romanian and Albanian, two languages for which we may safely presume an early contact with the idiom in use among the Sclavenes, șchiu and Shqëi derive not from Σκλαβίνος/Sclavus, but the shorter form Σκλαβός/Sclavus, which is undoubtedly of Byzantine origin. Be it as it may, naming and classifying a group of people as Sclavenes was a Byzantine, not Sclavene practice. In that respect, I believe that Ivan Mužić’s approach to the confusion between Goths and Croats (or Slavs) in medieval sources is inadequate, while I find Aleksei Kibin’s paper on the Yatvingians most illustrative of the process at work in the case of the sixth- and seventh-century Slavs. Both authors deal with late sources, the Chronicle of the Priest of Dioclea and the Historia Sabaudiana of Archbishop Thomas of Spalato, in Mužić’s case; and the Russian Primary Chronicle (or the Hypatian Chronicle), in Kibin’s case. However, Mužić’s taking the sources at face value is not very convincing.

The confusion between Goths and Slavs (or Croats) is not a direct mirror of what had happened in the early Middle Ages, but the result of the several bookish influences, which have been painstakingly delineated by Neven Budak and which would form the basis for the kind of historiography that A. I. Filiushkin rightly called «Illyrianist». Most believe that the Chronicle of the Priest of Dioclea is a reliable source for more recent periods, such as, for example, the late eleventh- and early twelfth-century history of southern Dalmatia. However, that same source is completely unreliable when it comes to earlier periods, specifically to those which concern Ivan Mužić. Inconsistencies, obscurities and downright fictional characters have permanently damaged the reputation of the Chronicle, which is now believed to be the work of Gregory, Archbishop of Bar. Like the unknown author of the Bulgarian Apocryphal Chronicle, Archbishop Gregory was not interested in separating fact from fiction, but in writing a report of Dalmatian history that could support the claims to superiority of his see over that of Split, no doubt in the context of the re-elevation...
of Bar to the status of archbishopric and of the Dukljan-papal contacts of the late 1100s\textsuperscript{62}. To accept at face value the evidence of the Chronicle is at best naive and at worst suspect.

Tibor Zivković has shown how in at least two cases — the account of the Council on the Duvanjsko Polje and the legend of Pavlimir Belo — Archbishop Gregory made up stories designed to serve a political purpose, namely the demonstration of the long-established supremacy of Dioclea over Rascia and the subordination of its ecclesiastical organization to the Archbishopric of Bar\textsuperscript{63}. Archbishop Gregory claims that a Croatian king named Tomislav defeated in battle the Hungarian king Attila\textsuperscript{64}. Should we believe him? And if we dismiss that as being unreliable information, why should we accept at face value what Archbishop Gregory has to say about Silimir and Bladina and their Slavic-speaking Goths? What is ultimately the difference between reading the twelfth-century Chronicle of Archbishop Gregory as a genuine and veridic account of what had happened in Dalmatia in the sixth century and taking at face value the account of King Arthur and his father, Uther the Conqueror in the equally twelfth-century History of the Kings of Britain by Geoffrey of Monmouth? Muzić does not seem to be troubled by such questions and comparisons, and his uncritical approach to sources written six centuries after the events narrated makes his otherwise interesting idea of native Croats (or at least Croatian Slavs) look very dubious. The thesis has meanwhile received a theoretically much more sound treatment by Danijel Dzino and has a good chance of stirring much debate, especially since it does not contradict what others have written from a rather different point of view\textsuperscript{65}.

This is certainly not the time to evaluate the idea, which historians and archaeologists alike will have to consider in great detail. However, no one could seriously raise any doubts about the survival of the local, «late antique» population of the northwestern Balkans after ca. 600. Were these Goths, especially those Goths that Archbishop Gregory had in mind? While Muzić seems ready to jump to that conclusion, few would follow him. Equally suspect in my mind are attempts to read ethnicity (-ies) in haplotypes and old names. Names such as Mutimir or Branimir may well be just as «Germanic» (or Gothic) as Mezamer (or Mezamir), the name of the Antian envoy killed by the Avars in the early 560s\textsuperscript{66}. No connection can however be established by such means between the Goths and the Croats\textsuperscript{67}. As for genetics, the main problem is the high degree of uncertainty involved in the identification of group affiliation on the basis of biological data. First, as modern studies have shown, there is no complete overlap between haplotypes and ethnicity\textsuperscript{68}. While haplotypes may be able to show a degree of similarity between any given population and its recent neighbors, they do not in fact map the ethnic diversity within that same population. More
importantly, the study of haplotypes of any modern population cannot inform about any other populations in the past, especially since no data have so far been collected from the skeletal remains of medieval populations. Finally, Muzić's positivist stance undermines his otherwise instrumentalist approach to ethnicity. If ethnicity is in the genes, then why did the name (and «culture») of the population in the northwestern Balkans have to change from Goths to Slavs (or Croats)?

Kibin' has a very different approach to ethnic names. As he points out, despite many claims to the contrary, Yatvingians (ятвяги) was one of four names (the other three being Sudovians, Dainovians, and Pelleziane) given to one and the same group of people inhabiting the region of Suwalki, on the present-day northeastern border of Poland. Much like варяги and колбяги, the origin of the word Yatvingian is Scandinavian, not Baltic or Slavic. In other words, this was not a self-designation and certainly not a name of Baltic «native» origin. That during the second half of the thirteenth century, the word designated a political and ethnic entity perceived by Rus' princes, Mazovian dukes, and crusading orders as the enemy is an indication that the original meaning had meanwhile changed. But the Rus' chronicler who first mentioned the Yatvingians did not fabricate their initial identity. The late tenth- and early eleventh-century archaeological record of the region around Vawkavysk in northeastern Belarus may be interpreted as indicating the presence of retinues of warriors, the same warriors against whom Prince Vladimir directed his expedition of 983. Archaeology clearly supports Kibin's suggestion that Yatvingians was an all-encompassing label, which the chronicler applied while painting with a broad brush the image of the enemy. In fact, the reality on the ground was clearly much more complex.

Similarly, my argument in the Making of the Slavs was not that the name «Sclavene» was a Byzantine invention, but that the «Sclavenes» (as an ethno-political category) were invented by the Byzantines. There is much misunderstanding in Sergei Ivanov's critique of my approach, which is ultimately based on an error of translation. To both Ivanov and Shuvalov (but neither to Polyviannyi, nor to Kibin'), the English word «invention» means in Russian изобретение, a word which in English may be translated as «contrivance» or «fabrication» (Ivanov even writes of «propagandists devices», using such words as уловка, which means «subterfuge» or «flim-flam»). He does not seem to be aware of the other, etymologically older meaning of the word «invention», namely «discovery» (as in the «Invention of the Cross» by St. Helena). Invention-as-discovery is what I had in mind when writing: «The making of the Slavs was less a matter of ethnogenesis and more one of invention, imagining and labeling by Byzantine authors».

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Adding insult to injury, Petr Shuvalov even insists that in English «invention» means only «fabrications». Such insistence turns him into ridicule, for Shuvalov is utterly wrong. «Inventions» mean first and foremost the action of coming upon and finding, the action of finding out, discovery (whether accidental or the result of research and effort). This is in fact no surprise, for the word ultimately derives from the Latin verb inuenio, -ire, which means «to come upon», «to find out», or «to discover, as in Cicero's famous adage, inuenio coniurationem. This is not contradicted by what we know about the earliest attestation of the word in English, a gloss in a mid-fourteenth-century manuscript of the Life of St. Stephen: «Saint Stanislaus inuenit»; that is, he finding of his body (Altenglische Legenden / Hrsg. von C. Horstmann. Heilbronn, 1881. S. 30). The most frequent use of «inventions» in modern English is in such phrases as «scientific inventions», which thoroughly preserves the initial meaning of the word. Invention-as-discovery is also widely used in the media jargon, both in Britain and in the United States. Catchy book titles such as The Invention of the Native American Literature (Ithaca, 2003) or Invariance and the Invention of Seeing (Chicago, 2001) must certainly refer to something else than «contrivance» or «fabrication». One currently speaks of the «invention of religion» in American politics, while the horse in Southeast Asia and Southern Africa is regarded as a British colonial invention.
As Table 4 (pp. 115–116) in the Making of the Slavs shows, the first independent raid of the Sclavenes known from the sources is that of 545. According to Procopius, in 530 Chilbudius was «ordered to keep watch so that the barbarians of that region (the Danube frontier) could no longer cross the river, since the Huns and Antae and Sclaveni had already made the crossing many times and done irreparable harm to the Romans» 70. From this, Ivanov draws the conclusion that the Sclavenes must have been a threat well before Chilbudius’ campaigns against them in the early 530s 71. But Chilbudius did not move against the Sclavenes alone, and Procopius himself lists the Sclavenes after the Huns and the Antes, who were apparently perceived as more dangerous. Moreover, after the death of Chilbudius in 533, those crossing the river Danube «just as they wished» were not the Sclavenes, but «barbarians» in general 72. Be that as it may, Ivanov’s arguments are set against a straw man, for my only claim was that the first recorded raid of the Sclavenes was that of 545 73. More importantly, Procopius did not write his remarks about Chilbudius in either 530 or 545. If Chilbudius campaigned against Huns, Antes, and Sclavenes, then the conclusion is that in 530 the Sclavenes were not the only, or even the main problem. Although there certainly were Sclavenes north of the Danube before 545, they seem to have become a major military and political problem only after that. AD 545 is therefore important for the demonstration in the Making of the Slavs because the Sclavenes appear to have react to a political and military situation, namely the implementation of Justinian’s fortification program and his alliance with the Antes, not because the Sclavenes first appeared on the «historical stage» at that date 74.

There is no contesting of the possibility that the Sclavenes may have in fact participated in barbarian raids across the Danube before 545. But the Sclavenes were not on the mind of those who built the forts in the Balkans. The forts themselves are a response to a particular threat, namely that of rapid raids by horsemen. Here, too, precise chronology is critical for the broader history of the Danube frontier. When the Sclavenes began to raid the Balkan provinces by themselves, Justinian’s fortifications were already in place. In the absence of any indication that between 545 and 558, the Sclavenes joined marauding expeditions organized by others (Huns, Bulgars, or Cutrigurs), the Sclavene reaction to the changing circumstances in the Balkans (the implementation of the fortification program) would mean something very different from their reaction to military campaigns organized against them by Chilbudius in the early 530s.

71 A similarly uncritical treatment of Procopius appears in: Liebeschuetz J. W. G. The Lower Danube Region under Pressure: From Valens to Heraclius // The Transition to Late Antiquity on the Danube and Beyond / Ed. by A. G. Poulter. Oxford, 2007. P. 111 with n. 77. — He postulates «extensive raiding by Sclavenes in the 520s and later 530s. This may well have been so, but there is absolutely no evidence of that in any of our existing sources.
73 Similarly, there is no use of the phrase «Slavic colonization» in the Making of the Slavs, and no conclusion «that the sixth-century Slavic society was at a very low level of development». I have not denied the veracity of the Miracles of St. Demetrius, either on page 54 of the Making of the Slavs, or anywhere else.
74 Sergei Ivanov uses a quote from page 339 in order to show that I «amazingly» ignored the Sclavene threat to which Justinian’s program of fortification responded. The quote, however, is truncated: «These measures were not taken in response to any major threat, for Roman troops were still in control of the left bank of the Danube, possibly through bridge-heads such as those of Turnu Severin (Drobeta) and Celci. This is shown by the edict 13, issued in 538, which clearly stated that troops were still sent (if only as a form of punishment) north of the Danube river, “in order to watch at the frontier of that place”». By truncating the quote, Ivanov has altered its meaning in order to adapt it to his own line of arguments. He thus attributed to me statements that I have never made.

2008. № 2 (4). Июль—Декабрь
These remarkably diverse and stimulating contributions make one thing crystal clear. Historians of the early Middle Ages in Eastern Europe are entering a new era in which new concepts, new data, and new approaches will foster new insights and correct old ones, so long as we are attentive to when and where the evidence is coming from, to the complexity, chronology and context of the data. The hope indirectly expressed in the *Making of the Slavs* that the model of analysis proposed there could be brought to bear on the great question of early medieval ethnicity, to test and improve our understanding of later periods, is becoming reality far more swiftly and broadly than I would have ever dared to imagine.