

## DISKUSE

## The early Slavs in Bohemia and Moravia: a response to my critics

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*In pondering how the existing evidence pertaining to the early Slavs in Bohemia and Moravia may be assessed, the paper rejects the arguments presented by Nada Profantová, Felix Biermann, and Andrej Pleterski, in particular the idea of a migration of the early Slavs from western Ukraine and Bukovina into Bohemia and Moravia. The chronology of assemblages of the so-called “culture with Prague-type pottery” proposed by Profantová is demonstrably wrong, while Pleterski’s idea of a specifically Slavic “kitchen culture” rests on insufficient knowledge of the changes in eating practices accompanying economic transformations in the Mediterranean region during the sixth and seventh centuries. Against Biermann’s uncritical treatment of the written sources, particularly of Fredegar, the paper argues that the text of the Chronicle of Fredegar is not to be taken at face value, but understood as a means to please Fredegar’s Austrasian audience.*

migration – ethnicity – early Slavs – handmade pottery – Fredegar – Procopius of Caesarea – “Slavic” bow fibulae

**Počátky Slovanů v Čechách a na Moravě: odpověď mým kritikům.** V úvaze o hodnocení stávajícího dokladového materiálu vztahujícího se k raným Slovanům v Čechách a na Moravě odmítá autor tohoto textu argumenty přednesené Nadou Profantovou, Felixem Biermannem a Andrejem Pleterskim, obzvláště myšlenku příchodu prvotních Slovanů do Čech a na Moravu ze západní Ukrajiny a Bukoviny. Chronologie souborů takzvané „kultury s keramikou pražského typu“, předložená Profantovou, je prokazatelně chybná, kdežto Pleterského myšlenka specificky slovanské „kuchyňské kultury“ spočívá na nedostatečně obeznámenosti se změnami v jídelních zvycích doprovázejících ekonomické změny ve Středomoří během 6. a 7. století. Proti Biermannovu nekritickému zacházení s písemnými prameny, zvláště s Fredegarem, namítá autor; že text Fredegarovy kroniky nelze brát doslovně, nýbrž je jej třeba chápat jako snahu vyhovět Fredegarovu austrasijskému čtenářstvu.

migrace – etnicita – počátky Slovanů – ručně robená keramika – Fredegar – Prokopios Caesarejský – „slovanské“ paprscité spony

*The one who is at a loss to understand the other is I.  
But at least the immediate point should be plain,  
unless you are determined to have it otherwise.*

Nikolai V. Gogol, *The Nose*<sup>1</sup>

I wrote *The Making of the Slavs* more than ten years ago in a rush of urgency and optimism. The urgency came from the strident discrepancy between interpretations of various categories of sources and from the then current state of research. In light of the momentous transformations taking place in the discipline of archaeology in the 1980s and 1990s, I found myself thinking a lot about the changing bases of scholarly dialogue between historians and archaeologists. I was contemplating in particular the dismal gap between the new and exciting perspectives opened by anthropologists studying ethnicity and the conceptual and methodological difficulties encountered by historians and archaeologists inspired by a “linguistic” approach to the history of the early Slavs. For any reasonable

<sup>1</sup> English translation from *Gogol 1952*, 164.

scholar whose understanding of the Slavic ethnogenesis required an equal consideration of written and archaeological sources, the state of research in the late 1990s must have indeed seemed very dispiriting. Because of a fundamental belief in a primordial definition of ethnicity (often placed squarely in the realm of language), which obscured both the interpretation of the written sources (especially of Jordanes) and an adequate treatment of the archaeological evidence, there was apparently no way to reconcile the diverging conclusions drawn from the analysis of different sources. The earliest mentions of Slavic raids across the Lower Danube River may be dated to the first half of the sixth century, yet no archaeological evidence of a Slavic settlement in the Balkans could be securely dated before *ca.* 600 (Koleva 1993; Angelova – Koleva 2007). Aping the way in which, according to linguistic paleontology, Common Slavic spread across Eastern Europe, historians imagined a migration from the Pripyet marshes on the present-day border between Belarus and Ukraine to the Lower Danube, across which the Slavs are known from written sources to have crossed into the Balkans. However, archaeologists could not provide sufficient (or even positive) evidence of any such migration from north to south. Nor is there any evidence of such a migration in any of the sixth-century sources pertaining to the early Slavs. Procopius of Caesarea and the author of the *Strategikon* clearly placed them in the Lower Danube region and did not think of them as coming from anywhere else. Nonetheless, archaeologists in Poland, former Czechoslovakia, Ukraine, Belarus, and Russia interpreted as Slavic archaeological assemblages dated to the sixth or seventh century and found in their respective countries, despite the lack of any firm evidence from the written sources that any Slavs lived in those territories around AD 600.

The optimism, on the other hand, came from a sense of buoyancy and opportunity in the fields of both history and archaeology. The critique of the ethnogenesis model embraced by many German and Austrian scholars inspired by Reinhard Wenskus's work seemed to be drawing the 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 (to cite only the studies published before the *Making of the Slavs*: Cesa 1982; O'Donnell 1982; Ivanov 1984; Cameron 1985; Goldstein 1986–1987; Goffart 1988; 1989; Pizzaro 1989; Adshead 1990; Lošek 1990; Ivanov – Gindin – Cymburskii 1991; Curta 1997).<sup>2</sup> The full implications of this “literary turn” for the analysis of the written sources remain to be seen, but it has by now become clear that in order to make any progress the research on the early Slavs needs to distance itself from the practice of repeating the stereotypes embedded in late antique ethnography. Meanwhile, new approaches to the archaeology of ethnicity have also transformed our understanding of the relation between material culture and group identity (most prominent before the publication of the *Making of the Slavs* are Daim 1982; Hodder 1982; McGuire 1982; Bentley 1987; Buchignani 1987; David – Sterner – Gavua 1988; Anthony 1990; Olsen – Kobylinski 1991; Shennan 1991; Hides 1996).<sup>3</sup> More constructive conversations appeared to be possible both between and within disciplines, not least because a younger generation of scholars with less of the old baggage were coming to the fore (Klein 1988; Champion 1990; Beaudry – Cook – Mrozowski 1991; Gojda 1992; Moreland 1997; see also Shanks – Tilley 1989).<sup>4</sup>

One of my book's purposes was certainly to be an exercise in blending the historical into the archaeological analysis. I wanted to fashion a plausible synthesis out of quite heterogeneous materials.

<sup>2</sup> Many more studies have been published on this matter after the *Making of the Slavs*. See, for example, Gantar 1998; Roques 1998; 2000; Curta 1999; 2008b; Rousseau 1998; Flusin 2000; Howard-Johnston 2000; Whitby 2000; Brodka 2004; Kaldellis 2004; Loma 2004; Goffart 2005; Revanoglou 2005.

<sup>3</sup> Among the great number of studies published on this topic after the *Making of the Slavs*, worth mentioning in this context are Pohl 1998; Tabaczyński 1998; Mamzer 1999; Brather 2000; 2002; Urbańczyk 2000; 2003; Curta 2002; 2007; Mirmik Prezelj 2002; Gąssowski 2003; Barford 2004; Milinković 2004; Bertašius 2005; Lucy 2005; Stepanov 2006; Hakenbeck 2007; Härke 2007; Heather 2008.

<sup>4</sup> For newer studies relevant for this paper, see Härke 1998; Neustupný 1998; 2002; Lang 2000; Mirmik Prezelj 2000; Moreland 2001; Třeštík 2001; Macháček 2003.

The conclusion I have reached is in sharp contradiction with most other works published on this topic both before and after the *Making of the Slavs*. Instead of the traditional approach, that of opposing the barbarian Slavs to the civilization of the Late Roman Empire, I preferred to look at the Danube *limes* as a complex interface. Instead of a great flood of Slavs coming out of the Pripet marshes, I envisaged a form of group identity, which could arguably be called ethnicity and emerged in response to Emperor Justinian's implementation of a fortification project on the Danube frontier and in the Balkans. The Slavs, in other words, did not come from the north, but became Slavs only in contact with the Late Roman frontier. Contemporary sources mentioning Sclavenes and Antes, probably in an attempt to make sense of the process of group identification taking place north of the Danube frontier of the Late Roman Empire, stressed the role of "kings" and chiefs, as war leaders who may have played an important role in this process. This kind of exercise was only possible for a territory—broadly defined as the "Lower Danube region" – for which we have both archaeological evidence and information from the written sources, which can undoubtedly be established as referring to that territory. The last thing I wanted to propose was any straightforward interpretation of the evidence from all territories believed to have been inhabited by the Slavs in the early Middle Ages, from Bohemia to the Dnieper River and from the Baltic Sea to Greece. To claim, as Naďa Profantová does, that I accept, *cum grano salis*, the concept of Slavdom (*slovanství*) for the eighth and ninth centuries, is therefore a regrettable misunderstanding of my main arguments and a serious conceptual confusion.<sup>5</sup> Rather than impose my reading of the written sources on the interpretation of archaeological assemblages from territories outside the Lower Danube region, I wanted to show that the conclusions drawn from the analysis of the materials known from that region may well force us to see the evidence from Poland, Slovakia, Czech Republic, or Belarus in a different light. I wanted to give due emphasis to the subtle and complicated ways through which a group identity which could arguably be called ethnicity may be established by means of material culture, in order to invite archaeologists working in those countries to reassess the fundamental assumptions of their interpretation of the archaeological evidence. Several reviewers noted that the conclusions I have reached in the *Making of the Slavs* have only limited applicability to territories farther away from the Danube frontier of the early Byzantine Empire (Barford 2002; Brather 2003). They also wondered what, in the light of the *Making of the Slavs*, would be my approach to the question of the early Slavs in Central and East Central Europe.

It was for this purpose that I decided to make modest use of my own story and to take a fresh look at the "making of the Slavs" with a special emphasis on Bohemia and Moravia. First, I have been struck over the years by the obstinate efforts of several Czech historians and archaeologists to make the Slavs appear in those territories as early as the sixth, if not the late fifth century, despite the obvious absence of any solid evidence in support of such views (Ondrouch 1964; Zeman 1968; Váňa 1971; Klanica 1987; Třeštík 1996; Galuška 2000). Advocating an early presence of the Slavs may well have initially been an attempt to show that there were Slavs in Bohemia and Moravia before the migration of the Avars into Pannonia, a point confirmed by Profantová's decision to endorse the idea of a Slavic settlement of Bohemia before 568, even though she also admits that written sources cannot support that position (Profantová 2009, 312). Second, I was equally puzzled by the many inconsistencies in the archaeological discourse about the so-called "Prague type" of pottery, which many continue to regard as typically Slavic. In Profantová's case, this creates contradictions that can be very disconcerting. No one likes to be made aware of inconsistencies, least of all as they bear on one's fundamental principles or most firmly entrenched beliefs. More commonly, even in the face of clear evidence to the contrary, one tries to concede a few points without abandoning the fundamental thrust of one's arguments. Yet one can seldom sustain such a position for too long, while at the same time avoiding ridicule. In practice, problems creep up on one's very attempt to find justification. They register their

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<sup>5</sup> I have in fact never used the term *Slavdom* or any other such term, which in English could serve as an equivalent to *slovanství*. Profantová's basic premises are therefore wrong. By attributing to me statements that I have never made, she creates a straw man, which she then proceeds to attack, while conveniently ignoring the real implications of my arguments.

presence only partially, taking up residence in hidden corners, doing their work behind one's back. Often they ambush one's understanding, making a mockery of any standards of consistency and coherence. For example, Profantová argues that neither she, nor Gabriel Fusek (or Michał Parczewski) ever assumed that the handmade pottery of the so-called Prague type was recognized as "Slavic" by early medieval users in Slovakia, Poland, or Moravia (Profantová 2009, 306). She immediately insists, however, that those who produced and used that pottery in those areas must have been the Slavs, who are otherwise mentioned in sources considerably later than the date commonly accepted for the culture "with the Prague-type pottery" (Profantová 2009, 306–307).<sup>6</sup> If no contemporary sources inform us about how the "Slavs" viewed the so-called Prague-type pottery and whether or not they regarded it as a symbol of their ethnic identity, why then call this pottery Slavic in the first place? What else if not "ethnic badge" (or index-fossil) was on the mind of Gabriel Fusek, for example, according to whom the Prague-type pottery is typical for the early Slavic assemblages in Slovakia (Fusek 1993, 33)?<sup>7</sup>

In pondering how the existing evidence pertaining to the early Slavs in Bohemia and Moravia may be assessed, my intention was to point to possible ways to overcome such inconsistencies and contradictions. I also wanted to come to grips with the problems raised by the historical interpretation of archaeological assemblages in the absence of any contextual information derived from written sources. However, instead of retelling the story of the "Slavic ethnogenesis", I thought it would be more illuminating to suggest some alternative. To that end, I looked for a series of key assemblages attributed to the early Slavs, the chronology of which had a symptomatic relationship to the current ideas about Slavs in the Czech and Moravian lands, and which serve to illustrate the cultural changes allegedly accompanying the spread of (Common) Slavic to those lands. Through a critical examination of those assemblages, I tried to see what the most conservative estimate of their dates could imply for the historical reconstruction of what had happened in Bohemia and Moravia before and after AD 600. While confined to the nuts and bolts of the historical narrative – the story of who lived in Bohemia and Moravia during the sixth and seventh centuries – the analysis I have offered may seem banal. Yet by providing a detailed archaeological discussion of the parallels and comparisons involved in the interpretation of the key assemblages selected, we can get a much clearer idea of what is at stake. Is the material culture attributed to the earliest Slavs in Bohemia and Moravia an entirely new one? If so, is this cultural change to be interpreted in ethnic or, perhaps, social and economic terms? The answers to those questions can in turn deliver a more complicated and unexpected set of premises on the basis of which we can approach the question of the migration of the Slavs. To take only one example, Felix Biermann defends the migrationist model and the idea that the Slavic ethnogenesis took place somewhere in northwestern Ukraine and southern Belarus, within the territory of the Kiev culture (Biermann 2009, 339)<sup>8</sup>. He insists that both archaeology and linguistics support that idea. Leaving aside the dubious assumption associating the archaeological assemblages in the region in question to the Slavic ethnogenesis, Biermann seems to be completely oblivious to the fact that we

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<sup>6</sup> According to Profantová's contorted logic, I supposedly am the only one regarding the Prague-type pottery as Slavic. This is not only contrary to the central argument in *The Making of the Slavs*, but is also directly contradicted by what I wrote in *Curta 2008d*, as well as elsewhere (most relevantly in *Curta 2001b*).

<sup>7</sup> Profantová claims that no living scholar of some notoriety truly believes in the concept of Slavdom (Profantová 2009, 304). This is indeed bizarre, to say the least, given Profantová's passionate defense of her fellow travelers through the land of culture-history, particularly Michał Parczewski and Gabriel Fusek, whose work she claims to know very well. A few examples should illustrate the point. The caption for the illustration in *Parczewski 2005*, 73 fig. 4, reads "Zasięg Słowiańszczyzny w VIII wieku". Fusek (2001, 81–82) writes of a single Slavdom in permanent expansion. Similarly, *Pleterski – Knific (1999, 369)* imagine an entire Slavdom in expansion (see also *Pleterski 1990*). Finally, *Gavritukhin (2000)* writes of the great Slavic migration. *Quod erat demonstrandum*.

<sup>8</sup> It is rather amusing that while placing the Slavic ethnogenesis in southern Belarus, Biermann regards my criticism of the idea of a Slavic *Urheimat* in the Pripet marshes as misplaced, given that that location has long been abandoned (Biermann 2009, 343). Apparently, Biermann does not know that the Pripet marshes *are* in southern Belarus.

actually have no evidence whatsoever, either archaeological or linguistic, to support the idea that the Slavs in Bohemia and Moravia were immigrants from northwestern Ukraine and southern Belarus. In other words, before any attempt at ethnic attribution of the archaeological assemblages invoked in support of the migrationist model, it is first necessary to validate that model as a plausible alternative. We cannot simply assume a migration in order to explain cultural change, without some factual basis for that assumption. Unfortunately, what we know about the archaeology of migrations otherwise documented in the historical record does not square at all with the archaeological evidence from sixth- to seventh-century assemblages in either Ukraine (at the departing point) or Bohemia (at arrival); for the archaeology of early medieval migrations, see the pertinent remarks of *Bierbrauer 1993*; *Klein 1999*; *Barbiera 2005*; *Härke 2007*). It would be wrong, I think, to disregard such problems as unimportant as opposed to the supposedly essential problem of what was Slavic and what not. Part of my intention in offering my own views was to tempt others into doing the same, a wish answered by each of my three critics in their respective ways.

More directly in response to those three comments, there are several points that need to be emphasized. With respect to the particularities of my standpoint (and some of the accompanying errors<sup>9</sup>), I certainly accept *Nada Profantová's* skepticism regarding the possibility of using single- or double-sided combs for dating ceramic assemblages containing handmade pottery believed to be of the so-called Prague type. While I broadly share her reticence, I am not sure what, after all, was in dispute about our understanding of how single- or double-sided combs can or cannot be dated. At the end of almost two pages covering this topic, the reader of her comments cannot escape the impression that *Profantová* got carried away by her critical zeal and her desire to prove me wrong at any price. The price, in this case, is honesty. She writes: “According to his far-going analogies, the cemetery in *Přítluky* should be dated to *ca.* 600, only because of the fragments of two combs found there” (“*Curta ... podle dosti odvážných, prostorově odlehklých analogií dospívá k závěru, že dva zlomky oboustranných hřebenu z Přítluk datují začátek tohoto pohřebiště spolehlivě před r. 600*”; *Profantová 2009*, 320).<sup>10</sup> This is completely false. To be sure, I assigned an Early Avar date to the assemblage from house 2 in *Biharea* (Romania), in which a comb was found, which is similar to the fragments from *Přítluky*. But I also wrote that “whether or not such distant analogies are allowed for the *Přítluky* combs, they suggest the possibility of an earlier, sixth-century dating for at least some of the cremation burials in that cemetery (“*At je již užiti takto vzdálených analogií pro přítlucké hřebeny přiměřené, či nikoli, naznačují datování alespoň některých tamních pohřbů do staršího období, totiž do 6 století*”; *Curta 2008d*, 675). I also wrote that only a proper publication of all finds from *Přítluky* could clarify the problem. I specifically mentioned that the dating of the *Přítluky* combs is problematic (“*stejně problematické*”; *Curta 2008b*, 674). Against her own line of reasoning, *Profantová* then leaves the door open for a dating of the *Přítluky* graves before *ca.* 600 and adds that such a dating is not based on the double-sided combs alone (*Profantová 2009*, 321). What then is the evidence on the basis of which we can establish some rudimentary chronology of the *Přítluky* cemetery? *Profantová* assures the reader that she would never employ double-sided combs to date the *Roztoky* settlement, but then confidently places the upper chronological boundary of that settlement site between 630 and

<sup>9</sup> *Profantová* is certainly right to point out three egregious errors in my paper. The potsherd illustrated in the upper right corner of fig. 12 in *Curta 2008d*, 668, is indeed not from feature 911, but from feature 912. Similarly, the follis struck for Emperor *Heraclius* in 613/4 was found not within, but next to (and at a certain distance from) one of the features excavated in *Grodzisko Dolne* (*Curta 2008d*, 681). Finally, *Ivan Borkovský* never assigned to his Prague-type the pottery found by *Dinu V. Rosetti* in *Bucharest-Dămăroaia* (*Curta 2008d*, 659).

<sup>10</sup> *Profantová* concludes that, following my rejection of a similar dating by analogy of single-sided combs from *Roztoky* and elsewhere (*jednostranné hřebeny z Roztok či odjinud*), one would then have to accept that feature 842 in *Roztoky* is the second oldest on that site. But there is no single-sided comb in feature 842 in *Roztoky*, and *Nada Profantová* should know that better than anyone else, for she excavated that feature. The only comb found in feature 842 in *Roztoky* is double-, not single-sided. See *Kuna – Profantová 2005*, 35 (“*oboustranný... hreben*”) and 484 fig. 234.7.

650 on the basis of single-sided combs with triangular handle (*Profantová 2009*, 321).<sup>11</sup> At times, the impression one gets from reading Profantová's comments is that she is eager to score points, with little, if any regard for either logic or the minimal rules of scholarly discourse. This is apparent in her critique of my re-dating of the pottery from Libice on the basis of an iron bracelet. My own argument was that "such bracelets are one of the most important chronological markers for Avar-age cemeteries in Austria, where they have been found only in Middle and early Late Avar assemblages of the second half of the seventh and first third of the eighth century" ("takové náramky představují jeden z nejdůležitějších chronologických ukazatelů pohřebišť z doby avarské v Rakousku, kde se našly pouze ve středoavarských a raných pozdně avarských souborech 2. pol. 7. a 1. třetiny 8. stol.") (*Curta 2008b*, 679). In support of my argument, I cited Anton Distelberger's work on Avar-age female burials in Austria. In reply, Profantová cited the evidence from Alattyán, in Hungary, although the argument was clearly about Avar-age assemblages in Austria (the situation in Hungary, the core of the Avar qaganate, being of course rather different from that in Austria, on the western periphery of the qaganate). But Profantová also takes me to task for not respecting the limitation of the material, as Distelberger has done. Profantová, in other words, wants to prove that iron bracelets were quite common in Austria in the Early Avar period as well. She even thinks that the evidence from the Zillingtal cemetery could confirm her argument. She clearly did not read the book of Anton Distelberger, which she claims to know better than anyone else. Had she read that book, she would not have missed the crucial information about the lack of any solid evidence of any Avar-age material in Austria before the very end of the Early Avar period, i.e., before ca. 630 (*Distelberger 2004*, 24: "Das Material der großen Gräberfelder von Mödling und Zillingtal zeigt, daß sich der Beginn der awarischen Begräbnisstätten in Österreich nicht vor der Ende der Frühawarenzeit /ca. 630–650/ ansetzen läßt").<sup>12</sup>

With very little, if any evidence to support her arguments, Profantová pontificates: the culture with Prague-type pottery appears in Bohemia, Moravia, Slovakia, and Little Poland in the late sixth century and disappears in the late seventh century (*Profantová 2009*, 322). But the chronological diagram cited in support of her interpretation (*Profantová 2009*, 320 fig. 9) is completely wrong. Profantová lists Kozly as the earliest site with Prague-type pottery in Bohemia, dated to the second half of the sixth century. This is perhaps on the basis of the single-sided comb with triangular handle, for which no firm chronological brackets have so far been established. If so, then it is curious that within that same diagram, Profantová dates house 911 in Roztoky to the seventh century, despite the presence in that house of a fragment of a similar comb. She also lists three assemblages from Roztoky – 1412, 1708 and 1717 – which have not been published. There is no way to know on what basis she decides that the former needs to be dated to the first thirds of the seventh, while the other two are of the second half of the sixth and the first half of the seventh century. She dates features 18, 26, and 28 in Prague-Běchovice to the second half of the sixth and the early seventh century. None of those features produced anything which could be viewed as chronologically sensitive in order to obtain such a narrow dating. Profantová claims that "the stamped pottery is chronologically not so sensitive" (*Profantová 2009*, 318), but then apparently uses that to date feature 26 to the second half of the sixth century. There is in fact no evidence for a dating within the sixth century for any of the features excavated in Prague-Běchovice. Profantová also dates house 5 excavated in Březno to the late sixth and early seventh

<sup>11</sup> It is important to note at this point that Nada Profantová can very well support at the same time completely opposite points of view. While defending the dating potential of the single-combs with triangular handles in her comments to my paper, she also describes those combs as "bezüglich der Chronologie nicht aussagekräftig" (*Profantová 2008*, 625).

<sup>12</sup> That the cemetery in Mödling cannot be dated earlier than the end of the Early Avar or the beginning of the Middle Avar period is confirmed by *Daim (2003, 494)*. The reason for Distelberger to classify the earliest iron bracelets found in Austria as "FA/MA I" is that this type of artifact actually appears first in the Early Avar period, for example at Alattyán, as Profantová correctly points out. However, there are no burial assemblages in Austria, which could be dated to the Early Avar period, and the iron bracelet must therefore have appeared in that region only during the Middle or Late Avar period.

century. That house produced handmade pottery with no decoration, but also a fragment of a decorated pot thrown on a tournette; there is also a fragment of a bone comb and a spindle whorl, but the comb is not illustrated, and nothing is known about any other artifacts which could be dated with any degree of precision. Judging from the vertically combed decoration of the shard from a pot thrown on a tournette, this is probably a late seventh-, rather than a late sixth-century assemblage (*Pleinerová 2000*, 35, 55 fig. 19 [plan], 99 fig. 63: 4 [for the tournette-made potshard]; 100 fig. 64; 101 fig. 65: 2; 269 pl. 1). Similarly, pit 95 in Březno, which Profantová dates to the late seventh century, produced handmade pottery and a fragment of an iron disc, most likely a so-called “Silesian pan,” which cannot be dated before ca. 800 (*Pleinerová 2000*, 12; 76 fig. 40: 1–5; for “Silesian pans” see *Bubeník 1980*). House 1070.3 in Roztoky is dated to the second half of the sixth and the early seventh century, but the only non-ceramic artifacts from that assemblage are a fragment of a bone needle and a fragment of a glass bead (*Kuna – Profantová 2005*, 55, 533 fig. 286a: 2, 3). I have advanced the idea of dating assemblages with bone needles to the late sixth and early seventh century (*Curta 2008d*, 676). However, Profantová thinks that my attempts to use artifacts, such as bone needles, which change too slowly to be chronologically sensitive, speak more about my courage as an author than about the chronology of the analyzed assemblages (*Profantová 2009*, 322). It may well be so, but then what is the basis for her dating of house 1070.3 in Roztoky? Similarly, Profantová thinks that, like needles, barbed arrow heads cannot be dated with any precision, but then dates house 669 in Roztoky to the seventh century. This apparently is based on the date of the associated barbed arrow head, for, as I have shown in my article (*Curta 2008d*, 678), the iron strap end found in that same house could also be dated to the late sixth century.<sup>13</sup>

Much seems to depend on how Profantová’s understanding of the chronology of assemblages attributed to the “culture with Prague-type pottery” can be shown to have translated into means of understanding the evolution of pottery manufacturing techniques. In order to prove that such a culture truly existed, Profantová needs to demonstrate that similar manufacturing techniques co-existed on sites as far from each other as, say, Roztoky and Rashkiv (Ukraine). That pots in Rashkiv were manufactured by means of the coiling technique has been long recognized (*Baran 1988*, 52). According to Profantová, the same is true not only for the pottery found in Roztoky, but also for all vessels of the Prague type, whether decorated or not (*Profantová 2009*, 316).<sup>14</sup> She also claims to have examined *de visu* the pottery from Rashkiv as well as from other Ukrainian sites. Her conclusion is that “the Czech material is closer to that from Rashkiv than to that from other Ukrainian sites,” such as Obukhiv near Kiev (*Profantová 2009*, 314). This comes as no surprise, given that Obukhiv is a site most likely later than Rashkiv. First, it produced evidence of pottery with finger impressions on the lip, such as

<sup>13</sup> Profantová also dates house 1039 in Roztoky to the seventh century. This is based on the dating of the bronze strap end found in that house with a good analogy in a warrior grave (grave 446) from the cemetery excavated in Altenerding (Germany), which has been dated to ca. 600. However, that house produced only 9 potsherds of “early medieval pottery”, none of which is illustrated or described in any detail. One wonders therefore why was this assemblage included in a diagram supposedly showing the chronological ordering of “selected assemblages with Prague-type pottery” found in Bohemia.

<sup>14</sup> According to Profantová, I was wrong when writing that technological classes of pottery cannot be recognized by means of the definition of the culture with Prague-type pottery (*Profantová 2009*, 314). While referring to the coiling technique in relation to the pottery found in Rashkiv, I supposedly mentioned Roztoky to point out that on that site the technology was not recognized, because in the Czech Republic “no studies exist which examine basic techniques employed for constructing pots”. That is in fact a dishonest misrepresentation of what I wrote (see *Curta 2008d*, 665). My reference to the lack of studies examining the techniques employed for the manufacturing of pots comes before the mention of Rashkiv. Moreover, I have nowhere written that the coiling technique, as identified in Rashkiv, has not been recognized in Roztoky. How could I have possibly done so, when in fact the site monograph (*Kuna – Profantová 2005*) contains absolutely no discussion of manufacturing techniques? Only Profantová now wants the reader to believe that, like in Rashkiv, all pots in Roztoky were manufactured by the coiling technique. Whatever the reasons for her impatient claims, one can hardly be blamed for not taking her word at face value.

found in house 7 (*Abashina 1986*, 82; 81 fig. 7: 24). Second, an earring with croissant-shaped pendant – a stray find, most likely from the settlement area – is an artifact of a clearly seventh-century date (*Korzukhina 1996*, 353; 681 pl. 91: 4). Be that as it may, one wonders though how could Profantová figure out whether or not pots in Obukhiv were made in the coiling technique, since, unlike Roztoky or Rashkiv, no assemblage from that site produced any whole vessel.

Similarly dubious are other claims that Profantová makes in her comments. Take for example her firm belief that no late fifth- to early sixth-century cemeteries exist in Bohemia and Moravia to inform us about the “Germanic presence in the region“ (*Profantová 2009*, 312). Profantová’s intention may have been to show that upon the arrival of the Slavs, Bohemia or Moravia had just been abandoned by the Germanic population, in other words that the Slavs entered a *terra deserta*. Irrespective of her intention, though, Profantová does not seem to know anything about recent studies demonstrating the continuity, well into the sixth century, of a number of cemeteries beginning in the fifth century, such as Záluží near Čelákovice, Lochenice IX or Jiřice (*Droberjar 2008*, 243).<sup>15</sup> Even more bizarre is Profantová’s claim that the Prague-type pottery comes only (or at least, typically) in a sand-tempered fabric. According to her, since in the case of the pottery found in Roztoky, the addition of crushed potsherds or mica was recognized only under the microscope, it remains unclear whether such components were intentionally used as temper (*Profantová 2009*, 314). In reality, both mica and crushed potsherds may be easily recognized without the use of the microscope.<sup>16</sup> Moreover, while mica may indeed constitute a natural component of the clay, how could the crushed potsherds possibly get there without the potter’s intention to mix them in the fabric?

However, the most dubious of all arguments advanced in Profantová’s comments is that of the chronological relation between undecorated and decorated pottery. Endorsing the traditional views of such scholars as Michał Parczewski and Gabriel Fusek, Profantová believes that following a period of about 50 years<sup>17</sup> in which only undecorated, handmade pottery was produced and used, the practice was gradually established to straighten pots on the potter’s wheel and to decorate them with the combed ornament. Thus, over a period of 70 to 80 years, both decorated and undecorated pottery was produced and used. Then, during the final phase of the “culture with Prague-type pottery” the decorated pottery gradually eliminated the undecorated pottery. The fact that the decorated pottery of the last phase is allegedly absent from Ukraine and Little Poland is to be explained in terms of the expansion of the Prague-type culture: the areas from which the migration started experienced only the first phase of the development (*Profantová 2009*, 318). There are serious problems with this model. First of all, it is not at all clear why the technological innovation (the transition from handmade, undecorated pottery to decorated pottery thrown on the wheel) had to take place only outside Ukraine and Little Poland. Given that those two regions were not depopulated at the time of the last phase of the Prague-type culture in Bohemia, one is led to believe that those who remained behind must have been less innovative or smart than the Slavs migrating to Bohemia and Moravia.<sup>18</sup> Second, while arguing that “nobody defines mutually exclusive horizons with only decorated and only undecorated pottery” (*Profantová 2009*, 318),<sup>19</sup> Profantová does precisely that when claiming that the oldest

<sup>15</sup> There are also cemeteries beginning only in the sixth century, such as Mochov, Lochenice I, Zvoleněves and Prague-Libeň.

<sup>16</sup> In fact, during the 1950s and 1960s, many archaeologists in Romania and (Soviet) Moldova signaled the presence of that temper in ceramic assemblages with fifth- to seventh-century pottery. See, for example, *Diaconu 1958*; *Rafalovich 1972*; *Vilceanu – Barnea 1975*.

<sup>17</sup> No evidence is presented for Profantová’s estimate of the duration of the oldest phase of the “culture with Prague-type pottery.” Indeed, the reader of her comment is left wondering how can one know how long was the “oldest horizon” and what are the non-ceramic artifacts with which one could date that horizon with any degree of precision.

<sup>18</sup> For some bizarre reason, Profantová believes the decorated pottery to be “luxury” pottery, or in any case pottery of a better quality than the undecorated one, despite the fact that decorated and undecorated pottery remains have been found together within the same, relatively humble archaeological assemblages in Bohemia, Moravia, and elsewhere.



horizon of her “culture with Prague-type pottery” is that with only undecorated pottery. She asks rhetorically: “But isn’t the attempt to distinguish between an older, undecorated from a later, decorated pottery only some kind of evolutionist prejudice (evolucionistický předpoklad)?” (*Profantová 2009*, 317). She then proceeds to demonstrate that the stratigraphy in Roztoky and Prague-Liboc supposedly shows that an older horizon with only undecorated pottery of Prague type is followed by a younger one with decorated and undecorated pottery or, in the case of Prague-Liboc, with pottery straightened on the potter’s wheel. It is precisely at this point that Profantová’s entire argument crumbles under the burden of evidence. To be sure, neither feature 1094, nor feature 1104 in Roztoky has been completely excavated. To say that feature 1094 contained only the handmade pottery showed in *Kuna – Profantová 2005*, 143 fig. 14 is imprudent, at the best, and a sign of wishful thinking at the worst. Moreover, it is not at all clear that the two fragments with combed ornament shown in *Kuna – Profantová 2005*, 145 fig. 21 came from the occupation layer of feature 1104. All one learns from p. 130 of the site monograph is that the pit 1104 produced the pottery in question. Since the pit was dug into the filling of house 1094, it is at least theoretically possible that the pottery in question was in that filling in the first place. In Roztoky, there is also one case of superposition, in which the superposed feature (1038) produced only handmade pottery, just like the superposing feature (pit 1037; *Kuna – Profantová 2005*, 50 and 518–519 figs. 271 and 272). A similar situation is attested in Nitra – Mikov dvor (Slovakia), with features 263 and 269 (*Fusek 1991*, 296, 322 pl. IV). In Kodin I (Ukraine), there are also several cases of superposition of features, each producing both hand- and wheel-made pottery: for example, houses 11 and 13 (*Rusanova – Timoshchuk 1984*, 29 and 48–49); or houses 14 and 16 (*Rusanova – Timoshchuk 1984*, 49–50). There are also cases of superposition, in which the superposed feature contained both hand- and wheel-made pottery, while the superposing feature produced only handmade pottery: houses 17 and 18 (*Rusanova – Timoshchuk 1984*, 50–51); and houses 25 and 26 (*Rusanova – Timoshchuk 1984*, 30 and 52).<sup>20</sup> The same phenomenon is documented in Botoșana (houses 8 and 9; *Teodor 1984*, 28–29) and Davideni (houses 25 and 26; *Mitrea 2001*, 61–64), both sites located in northeastern Romania, near the border with Ukraine.

The evidence thus seems to reveal a situation that is the complete opposite of what Profantová (as well as Parczewski and Fusek) believed to have happened. But Profantová simply ignores the evidence, especially when it goes against her arguments. According to her there is no decorated pottery in Ukraine, which could be dated to the fifth or sixth century (*Profantová 2009*, 318). In reality, decorated, wheel-made pottery is known from several fifth- to sixth-century assemblages in Ukraine: Horecha, house 7a (*Rusanova – Timoshchuk 1984*, 85 fig. 38: 8); Kavetchina, house 7 (*Vakulenko – Prykhodniuk 1984*, 64 fig. 37.9); Zelenyi Gai, house 4 (*Baran 1972*, 176 fig. 53: 4); and Teremtsi (*Baran 1972*, 73 fig. 33: 3). Similarly, Profantová claims that “in Late Antiquity and the early Middle Ages, the stone oven appears in Central Europe only on sites with Prague-type pottery and, later, of the ‘hillfort’ period” (*Profantová 2009*, 324).<sup>21</sup> Again, this is simply not true. Stone ovens appear on

<sup>19</sup> Profantová’s claims must be treated with great suspicion, as they are certainly not as apodictic as she believes them to be. She declares that nobody would insist without hesitation that the Prague-type pottery must be earlier than the seventh century (*Profantová 2009*, 318). But she obviously ignores, perhaps deliberately, many examples to the contrary. Take, for instance, Jiří Zeman’s insistence that the Slavic, Prague-type pottery found in Březno must be dated *at the latest* to the first half of the sixth century (*Zeman 1968*, 5).

<sup>20</sup> Similar examples are known from Kodin II: houses 2 and 3 (*Rusanova – Timoshchuk 1984*, 22 and 54); houses 14 and 16 (*o. c.*, 57); houses 26 and 28 (*o. c.*, 59–60); houses 29 and 30 (*o. c.*, 60); and houses 39–40 (*o. c.*, 62). A similar situation appears in Kavetchina, houses 19–20 (*Vakulenko – Prykhodniuk 1984*, 80).

<sup>21</sup> There really is no way to tell how many of the squares, which Profantová added to my map (*Curta 2008d*, 657 fig. 9; see *Profantová 2009*, 316 fig. 6), indicate finds from settlements dated to the sixth or to the seventh century. Moreover, her additions do not change the point of my remarks. In the text accompanying the map, I wrote that “the distribution of heating facilities accompanying sunken-featured buildings excavated on sites in the Lower Danube region show a remarkable cluster of clay ovens in Wallachia, in sharp contrast with the cluster of brick ovens on many sixth-century forts on the other side of the river, in the northern Balkan provinces of the Roman

sixth-century sites without any Prague-type pottery, such as Dipşa in Transylvania (*Gaiu 1993*, 92–93; see also *Cosma 1996*).<sup>22</sup> For someone who authoritatively demands “fewer conclusions reached in a haste, as well as less factual inaccuracies” (“o trochu více prosty zkratkovitých závěrů a věcných nepřesností”), such blatant ignorance of the situation in the field and of the relevant archaeological assemblages in Central and East Central Europe is disturbing. The conclusion one can draw from all this is either that Profantová passes over in silence those facts which directly contradict her arguments, in which case she is simply dishonest; or that she has not read the specialized literature pertaining to the issue at stake and does not in fact know all the facts, in which case one begins to question the very basis of her authoritative criticism.

The latter seems to be the solution closest to reality, especially when turning to her reading of such written sources as Jordanes, Procopius of Caesarea, and Fredegar. Profantová does not read or does not understand Latin; she could not have otherwise translated as “jméno nyní mění podle rodu a míst” what in Latin reads as follows: “quorum nomina licet nunc per varias familias et loca mutantur.” The subject of that sentence is a plural noun (“names,” not “name”), and its meaning is therefore that the names of the Sclavenes are now (i.e., at the time of Jordanes’s writing) dispersed amid various clans and places.<sup>23</sup> Profantová is also wrong when claiming that when Fredegar writes of *Avares coinomento Chunis* (a parallel to *Sclavos coinomento Winedos* in IV 48 and *Sclauī coinomento Winidi* in IV 68), this was not a way to distinguish Avars from Huns, but a manner of referring to nomads in general (*Profantová 2009*, 308). In fact, quite the contrary is true: Fredegar regarded Avars as a “species” (or sub-category) of the Huns in the same way as he regarded the Wends as a “species” of the Slavs. Moreover, the phrase *Sclauī coinomento Winidi* is not Fredegar’s own creation; instead, the phrase was borrowed from *Vita Columbani*, in which the pair is to be explained as purely bookish, a good parallel to *Boias, qui nunc Baioarī vocantur* (for a detailed discussion, see *Krahwinkler 2000*, 406). Even more embarrassing is Profantová’s attempt to defend Dušan Třeštík’s (wrong) interpretation of a passage in Procopius’s *Wars* regarding the return of Hildigis from Italy (*Wars* VII 35.21–22). The passage in question explains that after brief skirmishes with Roman troops, Hildigis recrossed the Danube and “withdrew once more to the Sclaveni.” The order of events is quite clear: first the crossing of the Danube, then the withdrawal to the Sclaveni. Against the evidence, Profantová suggests that Hildigis first returned to the Slavs, and then crossed the Danube. Her argument in support of such a violation of the text evidence is that, had Hildigis’s troops crossed the Danube first, they would have done so only after crossing the imperial territory in the north(western) Balkans, something that Procopius does not mention, even though he normally provides details in descriptions of barbarians (*Profantová 2009*, 311). In fact, quite the contrary is true. Procopius’s account of the Sclavene raid of 549 is a good example in that respect. After the 3000 Sclavene warriors crossed the Danube and advanced to the Hebrus/Marica, which they crossed without problems, they split into two groups, one attacking the cities in Thrace, the other invading Illyricum. Procopius’s narrative focuses only on the

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Empire” (*Curta 2008d*, 657). Since all symbols, which Profantová added to my map represent stone ovens, her additions have no consequences for the argument in the text. Note also that the first map in Profantová’s comments (*Profantová 2009*, 305 fig. 1) is clearly marked as having been in fact produced by Gabriel Fusek. Judging from the copyright note, Profantová asked for his permission to re-publish the map. She never asked for permission to publish any of my maps, which were used in her comment.

<sup>22</sup> According to *Profantová (2009*, 324 with n. 19), the stone oven occurs on settlement sites of the Sântana de Mureş-Chernyakhov culture, “which in this context is considered genetically related to the later culture with Prague-type pottery.” But if so, why were stone ovens not in use in Wallachia, where the latest phase of the Sântana de Mureş-Chernyakhov culture is clearly attested in both settlements and cemeteries, and where early Byzantine authors such as Procopius of Caesarea, Pseudo-Caesarius, and the unknown author of the *Strategikon* locate the Sclavenes who are believed to be the bearers of the “culture with Prague-type pottery”? Instead, the dominant type of heating facility in sixth- to seventh-century settlements in Wallachia is the clay, not the stone oven (*Dolinescu-Ferche 1995*).

<sup>23</sup> Moreover, Profantová wrongly cites this passage from chapter 34 of Jordanes’s *Getica*, when in reality the passage is from chapter 35.

group operating in Thrace, with no details about that going into Illyricum (*Wars* VII 38.7). Moreover, Profantová's argument becomes a moot point when one realizes that, irrespective of the location of the Slavs to whom he eventually withdrew, Hildigis's troops had to go through imperial territory anyway. But in her valiant defense of Třeštík, Profantová stubbornly persists in error, verging on the ridicule. To be sure, Třeštík advanced the idea that Hildigis returned from Venetum via Sirmium (*Třeštík* 1997, 48). But Sirmium is on the Sava, not on the Danube. Apparently that is not a problem for Profantová (who otherwise knows that Sirmium is present-day Sremska Mitrovica on the Sava); her only problem is that Sirmium was in the hands of the Gepids at that time. But Procopius's text is crystal-clear: Hildigis crossed the Danube, not the Sava. Unless Profantová can demonstrate that Procopius misidentified the river, which Hildigis and his troops crossed in order to withdraw to the Sclavenes, I remain convinced that Dušan Třeštík was wrong. In order to cross the Danube, Hildigis and his men needed to approach Singidunum (present-day Belgrade), which was not in the hands of the Gepids at that time. Moreover, none of the Slavic raids into the Balkans, which Profantová cites in support of her interpretation, returned north of the Danube via Singidunum. All accounts in Procopius concerning such raids suggest that the crossing took place across the course of the Danube to the east from the Iron Gates.

I do not quite understand why Profantová wants to set up "Slavdom" and ethnicity in opposition to one another, or why it should be necessary to dissociate my notion of ethnicity from that of Reinhard Wenskus, Herwig Wolfram and Walter Pohl. I wrote: "ethnicity is now currently employed to refer to a decision people make to depict themselves or others symbolically as bearers of a certain cultural identity" (*Curta* 2008d, 647). Profantová disagrees, but in doing so she seems to neglect the fact that Walter Pohl (whom she mistakenly believes to be a German, instead of an Austrian historian) embraced a very similar concept of ethnicity: "a negotiated system of social classification," the objective features of which may be seen as "symbols, explained by myths or traditions." Pohl explains that "to make ethnicity happen, it is not enough just to be different. Strategies of distinction have to convince both insiders and outsiders that it is significant to be different, that it is the key to an identity that should be cherished and defended" (*Pohl* 1998, 21–22).<sup>24</sup> Again, the impression the informed reader gets from comparing such statements to Profantová's claims that the "Vienna school" (to which, of course, she annexes Dušan Třeštík) has a better, or even different theoretical grip on what ethnicity is and how it works, is that she has not truly read the works she cites in support of her arguments and that, as a consequence, her understanding of the issue at stake is rather limited. This is why her example of American suburban men preparing the barbecue cannot serve as an argument in favor of the idea that the Prague-type pottery was not a symbolic manifestation of ethnic identity (*Profantová* 2009, 326). It remains unclear what, if anything, does Naďa Profantová know about American men or suburbs, but barbecue is definitely not the daily food of the average American. Preparing the barbecue is a highly "public" performance – in front of the extended family, of friends, of work colleagues or fellow employees – and as such it reinforces a particular stereotype of American culture, "man-as-provider." The Prague-type pottery, with the probable exception of clay pans, most likely participated in no such "public" displays of skill and resources since, as Profantová correctly points out, that pottery was produced and used within the household. Having missed this important point, Profantová is therefore incapable of understanding that if it was not used to "show off," the Prague-type pottery could not have been ethnically specific, i.e., used to mark ethnic boundaries.<sup>25</sup> Profantová is equally confused

<sup>24</sup> For a definition of ethnicity such as used in my study, Profantová could have also consulted the work of Thomas Hylland Eriksen, which has been recently translated into Czech (*Eriksen* 2007). A very similar concept of ethnicity is in use among German (*Hansen* 2001; *Rademacher* – *Wiechens* 2001) as well as Bulgarian and Hungarian scholars (*Stepanov* 2006; *Bálint* 2006).

<sup>25</sup> Curiously, while claiming that the Prague-type pottery is ultimately Slavic, Profantová declares that there are no artifacts symbolizing Avar ethnicity (*Profantová* 2009, 326). For once I agree with her position, as I myself wrote that artifacts "cannot and should not be treated as 'phenotypic' expressions of a preformed identity" (*Curta* 2001a, 31). Profantová's "new" position, however, is in sharp contrast to her earlier views (which she never retracted; see *Profantová* 2003, 27).

about the issue of language. She pontificates that Common Slavic is not an invention of the linguists, but the very language the Slavic *gentes* (sic) spoke between the sixth and the ninth century. But what do we really know about the language spoken by the Sclavenes mentioned in early Byzantine sources? Procopius tells us that the Sclavenes and the Antes had “the same language, an utterly barbarous tongue” (*Wars* VII 14.26). This Profantová interprets to mean that all Slavs had only one language, namely Common Slavic. There is, however, no mention in Procopius of what was the language that both Sclavenes and Antes spoke: the only adjective modifying the noun “tongue” is “barbarous.” This is unusual for Procopius, who always uses the noun “language” or “tongue” together with some ethnic attribute, i.e., always mentions a language of some kind: Latin, Gothic, Armenian, Phoenician, Persian, or Greek.<sup>26</sup> That Procopius had knowledge of at least some of those languages is beyond any doubt.<sup>27</sup> 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. 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.” Similarly, to think of all Slavs as having common gods and rituals, and preferring certain names such as Perun and Svarog (*Profantová* 2009, 325) is to built theories on thin air, for not a single shred of evidence exists of any such things for the sixth and seventh century.

On a broader front, Felix Biermann takes me to task for denying that language constituted a unifying element (*verbindendes Element*) for the early Slavs, in much the same way as the “culture with Prague-type pottery” represented the material expression of a Slavic model of economic and daily life (*Biermann* 2009, 340). While this seems overdrawn – language may be a “unifying element” without necessarily being a marker of ethnic boundaries, as in the case of *linguae francae* – it is certainly true that I abstained from any extensive discussion of how we can map what we know about the spread of Common Slavic onto the conclusions of archaeological studies pertaining to the so-called Prague culture and how both could relate to the Slavs (Sclavenes) known from written sources. The reason is very simple: we actually have no firm basis for more than speculations about the linguistic spread of (Common) Slavic given the lack of any relevant information in that respect. Unlike *Biermann* (2009, 341), I simply do not see how one could learn anything about the “linguistic community” from an examination of the archaeological remains of the Prague culture. Biermann’s position in this respect is doubly flawed, because he fails to recognize that such an approach is ultimately based on the assumptions that Slavic was spoken (only) by Slavs and that the bearers of the Prague culture were the Slavic-speaking Slavs. Needless to say, both assumptions are unwarranted. There is no conceptual overlap between the archaeological and linguistic notions of “Slavs” and so far no way to link the material culture and linguistic developments. Biermann believes that Slavic was spoken in the early Middle Ages in the northern regions of East Central and in Eastern Europe, but cannot point to any shred of evidence in support of his beliefs. Similarly, scholars assume that the Sclavenes mentioned by Procopius of Caesarea as living in the lands to the north from the Lower Danube spoke what we now call Common Slavic. But that is simply an assumption: we actually have no idea what was the language that they spoke, for nothing survives from that language.<sup>28</sup> Moreover, the episode of the

<sup>26</sup> Latin: *Wars* II 1.7, II 23.6, III 1.6, V 14.4, VII 14.36, and VIII 5.13. Gothic: *Wars* III 2.5 and V 10.6-13. Armenian: *Wars* VII 26.24. Phoenician: *Wars* IV 10.20. Persian: *Wars* VIII 10.8. Greek (“Hellenic”): *Wars* II 25.4.

<sup>27</sup> 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’s (Procopius of Caesarea, *Wars* V 18.6). For another case of bilingualism, see *Wars* VI 1.13-19 (Gothic and Latin spoken by the Goths).

<sup>28</sup> As a matter of fact, the only piece of linguistic evidence that we have are names of a few sixth-century Sclavene chieftains (Daurentius, Ardagastos, Peiragastos, Musokios). None of them could be shown beyond any doubt to be of Slavic origin (*Struminskýj* 1979–1980).

“phoney Chilbudios” (Procopius of Caesarea, *Wars* VII 14. 32–35) – who, although of Antian origin, spoke Latin fluently – suggests that bilingualism was not only possible, but relatively frequent in the early Middle Ages, which completely undermines Biermann’s idea of a one-to-one relationship between language and ethnicity. Much like today, a Slav could speak Latin without being Roman. Conversely, we have incontrovertible evidence of non-Slavs, primarily Avars and Lombards, speaking Slavic. The “communication area,” which Biermann assumes to be the basis for the linguistic and cultural uniformity of the early Slavs clearly comprised other ethnic groups as well. Just like Avars and Lombards could speak Slavic, the sunken-floored building with stone oven in a corner and the handmade pottery believed to be of Prague type can appear on sites and in archaeological assemblages in Hungary and Italy, which have nothing to do with the Slavs, but instead have been associated with the Lombard and Avar polities. How did language (Slavic, in this case) spread to those non-Slavic environments? Whether or not one accepts Biermann’s idea of a “Lauffeuerprinzip” (*Biermann 2009*, 341), it is important to note that he, like Profantová, simultaneously defends two opposite points of view. On one hand he believes the migrationist model to be the most plausible of all, on the other hand he thinks he can explain similarity between things Slavic in Eastern, Southeastern, and East Central Europe as “a mirror of an interrelated communication and then linguistic area” (“als Spiegel eines untereinander verwandten Kommunikations- und dann auch Sprachraums zu deuten”: *Biermann 2009*, 339, 341). If migration is the driving engine between the transformations taking place in Eastern Europe during the early Middle Ages, how was it possible for a language initially spoken within the *Urheimat* (wherever that may be) to remain basically the same in those areas to which the migrants moved, with no major dialectal differences developing between the sixth and the ninth century? If, on the contrary, we should envisage a vast communication area, how did in fact a speaker of Slavic (a Slav, if you wish) from, say, Bohemia communicate with one in present-day Ukraine or Bulgaria, in order for him or her to learn about “new things” in those remote areas and change his or her own accordingly?<sup>29</sup> Felix Biermann provides no answers to those questions and does not seem to be aware of the deeper implications of his contradictory position. As a matter of fact, early medieval authors did not define the Slavs on the basis of either language or material culture. Neither Procopius of Caesarea, nor the author of the *Strategikon* had any interest in Common Slavic or pottery, be that of the Prague type or not. To classify and recognize the Slavs, Byzantine authors used criteria other than those which Biermann, in the early twentieth-first century, would like us to consider: “Wenn die Klassifikation als Slawen weder sprachlich noch archäologisch zu begründen wäre, was haben die Byzantiner mit der Begrifflichkeit ‘Slawen’ dann genau ‘gemacht’ bzw. erzeugt?” (*Biermann 2009*, 340).

There is a more basic issue here. Biermann rightly focused upon my insistence on a correct interpretation of the written sources. That insistence had two purposes: it was strategic, in that I argued against the “text-driven archaeology” practiced by advocates of the culture-historical approach; and it was more specifically historiographical, in that I warned against taking the sources at face value, as ethnographic reports from the field. Neither Profantová nor Biermann likes this stance. The latter sees my unwillingness to reproduce the stereotypes of ancient ethnography as a selective reading of the sources (*Biermann 2009*, 344). Biermann then casts his own proposal in a manner that makes his choice pretty clear: Fredegar’s Wendish account is not an “ethnogenetic myth,” but a reliable report of what actually happened. This is of course no novel idea. Sixty years ago, *Gerard Labuda (1949, 91–92)* similarly believed Fredegar to be a completely reliable source, which one needed to take at face value. The problem, however, with adopting such an approach is that it does not in fact support the common views on Samo and the “first Slavic state” upheld since Labuda’s time by both historians and archaeo-logists (*Chaloupecký 1950; Lutovský – Profantová 1995*; for a good *mise au point*, see *Eggers 2001*; see also *Polek 2006*). According to such views, to which Biermann subscribes, Samo’s “state” must have been somewhere in Bohemia, given that after their revolt against the Avars, the Wends

<sup>29</sup> The idea of a “communication area” is even weaker than the migrationist model because, while taking out the *deus ex machina* from history, it fails to explain the fact that Common Slavic remained remarkably homogeneous throughout the ninth and tenth century.

are said to have repeatedly raided Thuringia, until Dervan, the duke of the Sorbs, decided to leave his Merovingian overlords and join Samo's rebels (Fredegar VI 68). In addition, after his victory over the army of Sigebert in 638, the rebellious Radulf, duke of the Thuringians, allied himself with the Wends. Nonetheless, Fredegar's Wendish account makes it clear that the Slavs among whom the "Huns" wintered every year and with whose wives and daughters they slept lived within the territory under Avar control, if not also within the Avar qaganate. This was certainly not a new development, for Fredegar claims that the Wends have been subjects of the Huns for a long time (*ab antiquito*).<sup>30</sup> It is within the same territories that the sons of the Huns by their Slavic concubines lived when rising in rebellion against their Avar fathers and the Khagan (Fredegar IV 48). There is no mention of any migration of those whom Fredegar now calls Wends – because they represented a political category different from those Slavs who were subjects of the Avars – and eventually decided to elect Samo as their king. In order for Samo's polity to become independent from the Avars, it must therefore have been effectively separated from the Avar qaganate. And no evidence exists that the power of the Avars reached as far as Bohemia. Moreover, when deciding to wage war on Samo, King Dagobert is said to have been helped by the Lombards who made a hostile attack on the lands of the Slavs (*osteleter in Sclavos perixerunt*; Fredegar IV 68).<sup>31</sup> Those lands must therefore have been closer to northeastern border of the Lombard Kingdom in Italy, if the troops which offered their assistance to Dagobert came from the Duchy of Friuli (Eggers 2001, 73)<sup>32</sup>. The territory of the present-day Czech Republic cannot have possibly been the original area from which Samo's state began to develop, and which must therefore be sought farther to the east and southeast, in Lower Austria or in the region of the present-day border between the Czech Republic, Slovakia, and Austria (Fritze 1979, 519; Wolfram 1985, 130; Justová 1990, 116). This is precisely the area in which burial assemblages – mostly of warrior graves, none of which is earlier than ca. 630 – produced ceramic wares decorated with prick-like comb punches, a feature recently interpreted in relation to Samo's Wends (Stadler 2005, 159; 2008, 78; see also Eggers 2001, 68).

There is no need either to take Fredegar's Wendish account at face value or to align that interpretation of the text with the archaeological evidence in such a manner. However, I have to insist upon this particular point in order to show that if adopting such an interpretation, one arrives at conclusions very different from those drawn by Nada Profantová and Felix Biermann. Similarly, if believing that the Huns truly slept with the Slavic women and that the sons born to them truly rose in rebellion against their fathers under the leadership of Samo, then Biermann would have to reconsider the role of language for defining Slavic ethnicity. For one would have to wonder in what language did Fredegar's "Huns" communicate with the Slavs with whom they wintered or, more specifically, with their wives and daughters whom they chose as sexual partners. It is hard to imagine the Slavic women learning the language of the Avars in advance in order to please their Avar masters, but even if we assume that for a moment, one would still have to abandon Biermann's "communication area" model. If serious about taking Fredegar's Wendish account at face value, Biermann will have to admit that I was right when claiming – on the basis of other categories of evidence – that Slavic operated as a *lingua franca* within the Avar qaganate (Curta 2004).

There are in fact serious reasons to prefer a different reading of Fredegar's Wendish account. The interpretation I have offered elsewhere (Curta 1997) may have brought forward the idea of "a (literary) construction, politically motivated" more than it is acceptable to Felix Biermann's limited

<sup>30</sup> The only other time Fredegar employs the phrase *ab antiquito* is in relation to the war between the Franks and the Thuringians: from time immemorial (*ab antiquito*) no battle has ever been fought by the Franks and other peoples such as that between King Theudebert and the Thuringians (Fredegar IV 38). In both episodes, the phrase implies a considerable length of time.

<sup>31</sup> According to Fredegar, the Lombards were the neighbors of the Huns, from whom they were separated by the Danube (Fredegar III 65: "cum a Chunis Danuvium transeuntes fuisse conperiti, eis bellum conarint inferred, interrogati a Chuni, que gens eorum terminos introire praesumerit").

<sup>32</sup> The Lombards in question could not have come from Francia, together with Dagobert's troops, for no Lombard mercenaries are known to have been employed by the Franks either during Dagobert's rule or later.

interest in source criticism. But at a new examination of the text, the arguments I have presented twelve years ago gain even more strength. Take, for example, the issue of the Avars impregnating the wives and daughters of the Slavs. Judging by the story of Desiderius and Eulalius's wife (Gregory of Tours, *History* VIII 27), in Frankish society, when one's wife ran away with someone else, one was subject to laughter and scorn. From a Frankish point of view, therefore, to say that the "Huns" slept with the wives of the Slavs is to place the Slavs in a position of inferiority, which is already signaled in the text by their role as *befulci* and the many burdens they had to endure (Fredegar IV 48). The Slavs are no valiant warriors, but cannon fodder for the wars waged by the Avars. Denied the valor (*utilitas*) expected from true warriors, reduced to the status of tribute-paying subjects, the Slavs are further deprived of their dignity by being forced to offer their wives and daughters for sexual entertainment to their Avar masters. Being of mixed blood, the sons born from those unions were therefore not truly Slavs, but could not suffer the Avar oppression of their mothers and sisters any longer. They, and not the Slavic weaklings, rose in rebellion and under the leadership of Samo – a Frank – managed to defeat the Avars.<sup>33</sup> To mark the difference, Fredegar decided to apply the name Wends to those who followed Samo. They were after all a *gens* made up of illegitimate children of Slavic mothers. The issue of legitimate descendancy seems to have preoccupied Fredegar, as he told the story of the wife of a certain Chlodio, the mother of Merovech (the founder of the Merovingian dynasty), who may have conceived him either by a sea monster or by her husband (Fredegar III 9). Another parallel story is that of the first night Childeric and Basina spent together, when instead of making love, she taught him, by means of a series of animals, the degeneration of the Merovingians from lions to dogs over four or five generations (Fredegar III 12).<sup>34</sup> As Ian Wood has demonstrated, the way in which Fredegar contested the Merovingian right to power was to suggest that Merovech was the fruit of an adulterous relation, in which the king was cuckold (Wood 2003, 154). Similarly, the only way Fredegar could diminish the valor the Wends had shown in defeating the Austrasians at Wogatisburg was to say that they were bastards born from the Slavic women with whom their Avar masters slept, having obtained the forced consent of their husbands.

There are several other indications in the Wendish account that Fredegar, far from being the objective reporter in the field, which Biermann imagines him to be, was in fact determined to put a spin on the events narrated in order to please his Austrasian audience (for Fredegar's audience, see *Curta* 1997, 146–147). Most interesting in this respect is the episode of Sicharius, the envoy King Dagobert sent to Samo "to request him to make proper amends for the killing and robbing of the merchants by his people" (Fredegar IV 68). It has long been noted that Fredegar's embassy narratives are never simple reports, but literary constructs (Gillett 2003, 268). The legate is often depicted as an outsider at a foreign court, whose status is defined by the respect and fear theoretically inspired in the local leader by the displeasure and hostility of the legate's principal. This allows Fredegar to use envoys in order to unravel dramatic stories, in which the legate intervenes with impunity in local power struggles and conceals or even distorts the information he was expected to deliver. The Frankish King Chlothar sends an embassy to the Lombard king Charoald to protest the imprisonment of Queen Gundeberga, who was "his kinswoman" (Fredegar IV 51). When listening to the message delivered by Ansoald, Chlothar's envoy, the Lombard king asked him to return his answer to Chlothar. But Ansoald, "as if on his own account, without any authority" (*non quasi iniunctum habuisset sed ex se*) replied on the spot to the Lombard king's answer. The outcome of this violation of the diplomatic

<sup>33</sup> Like the Avars, Samo took many Wendish wives, who bore him "twenty-two sons and fifteen daughters." The meaning of those numbers remains obscure, but it is clear that Fredegar's intention was to depict Samo as capable of sexual excesses comparable to those of the Avars. He was after all nothing more than a pagan and man of wicked pride (Fredegar IV 68).

<sup>34</sup> As Basina explains, at the nadir of the Merovingian dynasty, "men will rule like dogs and smaller beasts, their courage similar" (English translation from Woodruff 1987, 24). One is reminded of Sicharius's haughty answer to Samo: "It is impossible for Christians and servants of the Lord to live on terms of friendship with dogs" (Fredegar IV 68).

mandate was, in this case, positive. The same is not true, however, about Sicharius, who like Ansoald, is said to have thrown threatening words at Samo, “for which he had no authority” (*quas iniunctas non habuerat*). The result of Sicharius’s inability to respect the limits of his mission as an envoy was ultimately the war which led to the defeat of the Austrasians at Wogatisburg. Fredegar places the blame squarely on Sicharius’s arrogant attitude, a mirror of his principal’s over-confidence in raising the Austrasian force, which would soon be decimated by the Wends. The Wendish account contains all the marks of this successful narrative strategy. Sicharius is said to have tried in vain to meet with the Wendish king, who “would not admit him to his presence.” In the early Middle Ages, the refusal to receive an embassy expressed annoyance with the envoy and was a reaction recorded particularly for representatives of usurpers, as in the case of the Frankish king Guntram who refused to receive the envoys from the Visigothic king Reccared (Gregory of Tours, *History IX* 1.16; Gillett 2003, 255). Samo’s refusal to meet with Sicharius was therefore a way to express displeasure with Dagobert’s arrogant demand that Samo make “proper amends.” Samo made it clear that he was ready to recognize Dagobert’s overlordship, provided that the Frankish king maintained friendly relations with the Wends. But Sicharius, as if mirroring his principal’s attitude, claimed that Samo already owed fealty (*seruicium*) to Dagobert.<sup>35</sup> The point of the story is, of course, that being “a certain Frank from the district of Soignies” (Fredegar IV 48), Samo was supposed to be a subject of the Frankish king. Instead, he was now claiming to be of a status equal to that of Dagobert himself, even though, in Sicharius’s words, he was just a ruler of “dogs.” Sicharius himself had to dress up as a Slav in order to be able to see Samo (Fredegar IV 68). Felix Biermann takes this as a proof that the Slavs were recognized as a distinctive ethnic group marked as such in a sort of “national costume” (Biermann 2009, 345). This completely misses the point of Fredegar’s story, which Biermann has not fully understood. Even if we admit, for the sake of the argument, that Fredegar had knowledge of a specifically Slavic costume as an ethnic marker, there are sufficient indications in the text to conclude that he was at work depicting Samo as a ruler with overblown pretensions, a man of wicked pride (*superbia prauorum*, Fredegar IV 68). He rejected Dagobert’s claims that criminals accused of robbing and killing Frankish merchants be apprehended and sent to him. He also proposed friendly relations with Dagobert, thus implying a status equal to that of the Frankish king (for the political connotations of *amicitia*, see Fritze 1994, 102). At his court, envoys would receive an audience from the king only after changing clothes, much like Western envoys to the court in Constantinople, who had to change in dark-colored robes before entering the imperial presence (*De cerimoniis* I 87; Gillett 2003, 258). To treat Sicharius’s changing into Slavic clothes as an indication that Slavic ethnicity in Samo’s polity was communicated through clothes is purely and simply a sign of scholarly naiveté.

Finally, if I have less to say about Andrej Pleterski’s comment, it is because, though much more favorable than those of Nada Profantová and Felix Biermann, his interpretation is built upon false assumptions. To be sure, like Profantová, he also puts in my mouth (or pen, as it were) words that I have never uttered, and reads into my work ideas that I never had.<sup>36</sup> He is also unhappy with my use

<sup>35</sup> The exact nature of this *seruicium* remains a matter of historiographical dispute. Václav Chaloupecký believed that *seruicium* referred to a tribute, which according to Sicharius, Samo was supposed to pay to Dagobert (Chaloupecký 1950, 229–231). However, Samo’s reply suggests instead some form of vassalage, for the Wendish king declares that he and his subjects are Dagobert’s men (*nos sui sumus*). See Eggers 2001, 71.

<sup>36</sup> According to Pleterski, I have defined the Slavs as the name of a social class within a stratified society and tried to demonstrate that there were no Slavs, as a specific ethnic group, in the Lower Danube region (Pleterski 2009, 331–332). He must have skipped large chunks of text when reading *The Making of the Slavs*, for in fact I have never written anything of that sort. All I was saying in *The Making of the Slavs* was, first, that Slavic ethnicity became apparent to Byzantine authors – who “invented” it (in the sense of the old meaning of the English word “invention,” namely “discovery”) – when that ethnicity was expressed by means of chieftains and military or political leaders. Furthermore, the presence of the Slavs in the Lower Danube region is clearly attested in the sources, to which I dedicated two chapters in the *Making of the Slavs* (over eighty pages, i.e., almost eighteen percent of the entire book). Why and how would I deny the presence of the Slavs in the Lower Danube region so prominently documented in the sources?



of anthropological theory, to which he prefers an ethnographic approach.<sup>37</sup> He relies on an outdated model of ethnic assimilation,<sup>38</sup> but boldly engages in comparing the ancient Greek *polis* with the Slavic *župa* as supposedly “small units of spatial organization,” without any fear of anachronism (Pleterski 2009, 332). He is also convinced that “for the Slavs the Slavic language was the existing language” (“A pro Slovany byl existenčním jazykem jazyk slovanský”: Pleterski 2009, 332), despite much evidence to the contrary, beginning with the episode of the “phoney Chilbudios” discussed above and ending with numerous examples of Greek-Slavic bilingualism in early medieval Greece (Diller 1954, 38–39; Malingoudis 1983, 105). Moreover, according to Pleterski, linguistic continuity from the early Slavs to the modern day justifies using words in modern Slavic languages to reconstruct the house culture of the early Slavs, which is otherwise not documented in the written sources. This is a highly dubious assumption. Just imagine what would mean to reconstruct the house culture of the ancient Greeks on the basis of the modern Greek language! Or, closer to home, what it would mean to reconstruct the Roman house culture on the basis of the Romanian vocabulary pertaining to the house.<sup>39</sup> Otherwise, in his attempt to attribute to the Slavs in the Balkans an exclusive “cooking culture,” Pleterski places the origins of that culture in the seventh and eighth centuries and concludes that, since it had no local traditions, that culture must be attributed to the people who called themselves “Slavs” (Pleterski 2009, 335). Leaving aside the fact that no evidence exists of anyone in the seventh- or eighth-century Balkans calling him- or herself Slav, Pleterski completely ignores the fact that very similar phenomena appeared in Late Antiquity and the early Middle Ages in parts of the Mediterranean region, which were never settled by Slavs. Significant changes in eating, and therefore cooking practices in southern Italy have been detected through detailed studies of pottery morphology and faunal assemblages (Arthur 2007). Most relevant in this respect is the significant correlation between open ceramic forms (casseroles, bowls) and faunal assemblages dominated by sheep and/or goat, as opposed to the correlation between closed forms (tall and narrow pots) and cattle- or pig-dominated faunal assemblages. Casseroles and open cooking pots serve to cook food through water evaporation and braising, the end result being a relatively dry dish, to which various sauces may be added. By contrast, closed cooking pots are intended primarily for greater heat and water retention, through stewing and boiling, generally leading to the production of semi-liquid, porridge-like foods. Unlike foods produced in casseroles, those made in closed cooking pots require comparatively less control, as they may be left by the oven gate to break down the fats, as well as to tenderize the meats and vegetables and to render them more digestible and palatable. Pork and beef are ideally prepared in tall and narrow pots, in which such meats may be long cooked in water until tender and until all tapeworms are killed, a method far safer and efficient than roasting. Because of its great nutritional and organoleptic properties, the

<sup>37</sup> I actually share Pleterski’s sense of the foreshortening in the archaeologist’s seeming incapacity to read artifacts as ethnic markers *per se*. He writes: “Neboť stejně jako nějaký předmět sám o sobě nemůže být etnickým znakem, stejně nemůže být ani znakem společenského postavení” (Pleterski 2009, 332). While I agree, of course, with the idea that material culture is not, and cannot be a direct reflection of social reality, Pleterski does not seem to realize that material culture does in fact participate in the construction of social reality.

<sup>38</sup> Pleterski believes that no process of assimilation can take place without the presence of a critical group of speakers of a new language and cites Stanisław Kurnatowski’s thirty-year old study of Slavic migrations. Much has been written in the meantime on this problem, and Kurnatowski’s approach is now long dated. Pleterski should have consulted at least some of the more recent studies dedicated to the problem (e.g., Tuite 1999; Gans 2007; Burling 2007). Similarly, instead of relying on just one study of the Bantu expansion by Albert Wirz (who is a historian, with no archaeological expertise), Felix Biermann should have acknowledged the existence of very different and better informed viewpoints (e.g., McMaster 2005; Eggert 2005, 321; Bostoen 2007).

<sup>39</sup> If tab. 1 shows, as Pleterski claims, a common (Slavic) history of kitchen processing of cereals (Pleterski 2009, 333), then Romanians must also be regarded as a part of that common history, given the presence in the Romanian language (a non-Slavic language) of such words as *test* or *caș*, not to speak about the polysemantism of such verbs as *a prăji*. Either Pleterski does not know Romanian, or he is completely unaware of the implications of such linguistic correlations in languages of completely different origin, which are otherwise spoken in areas adjacent to each other. One is reminded here of the famous Balkan *Sprachbund* phenomenon.

resulting “soup” may also be consumed separately, often with large quantities of bread or polenta-like substitutes. This cooking and eating pattern is easily recognizable in the case of sixth- to seventh-century assemblages in Eastern Europe attributed to the so-called “Prague culture” and characterized, almost exclusively, by relatively large and tall pots, undoubtedly used for porridges or porridge-like foods. This is in stark contrast with the ceramic assemblages of the fourth-century Sântana de Mureş-Chernyakhov culture, which besides pots have produced a relatively large number of bowls, plates, glassware and amphorae. Similarly, late fifth- and early sixth-century burial assemblages in Bohemia have produced a relatively large number of casseroles and bowls, both hand- and wheel-made (*Zeman 1958*, figs. 19: 2; 22: 4; 23: 1 and 3; 24: 1; *Svoboda 1965*, pls. 40: 3; 44: 10; 45: 13; 47: 8–11; 49: 13; 50: 14–15; 51: 4; 54: 6 and 8; 57: 3, 4, 6, 8 and 9; 56: 5; 58: 11 and 14; 62: 3–5; 63: 1 and 4; 64: 1–3 and 6–8; 67: 2; 75: 3–7; 79: 18; 81: 1, 6 and 7; 82: 5–6; 83: 12 and 14; 86: 15–17; 87: 8 and 11; 88: 5 and 7; 89: 3; 90: 12; 91: 10; 92: 10 and 11; 95: 14; 99: 11 and 12; 100: 11; 101: 6, 16 and 17; 102: 14; 106: 13; *Zeman 1990*, 74 fig. 26: 12–13; 75 fig. 27: 3; 76 fig. 28: 1; 84 fig. 33: 12; 89 fig. 38: 3; *Droberjar 2008*, 238 and 239 fig. 7). A few hand-made bowls and casseroles have also been found in assemblages of the so-called “Prague culture” (*Kuna – Profantová 2005*, 159–160). However, their number is very small in comparison with pots of various sizes, most appropriate for cooking or reheating portions of the food. More importantly, most settlement sites with ceramic assemblages dominated by pots of the so-called Prague type have also produced faunal assemblages dominated by cattle and pig, with only a few remains of sheep or goat (*Vencl 1973*, 355; *Peške 1980*; *1985*; *Profantová – Špaček 2003*, 376; *Kuna – Profantová 2005*, 272–273; *Bureš – Profantová 2005*, 73).

Site	Cattle (%)	Pig (%)	Sheep/goat (%)	Poultry (%)
Roztoky	35.4	51.2	5.8	1.1
Prague-Liboc	41.6	43.6	7.8	–
Horní Počernice	58	28	10	4
Březno	over 50	20–30	under 15	under 5

Conspicuously absent from Sântana de Mureş-Chernyakhov sites, as well as late fifth- to early-sixth century assemblages in Bohemia, are the clay pans which appear in great numbers on sixth- to seventh-century sites, some of which have been directly linked to the “Prague culture” (*Bérezš 1985*, 46; *Kuna – Profantová 2005*, 161–162; *Viargei 2005*, 490). However, clay pans appear also in contemporary ceramic assemblages in Spain especially in the Alicante-Murcia area, for which we now have detailed studies of late antique and early medieval pottery (*Gutiérrez Lloret 1990*). Moreover, much like in Eastern Europe, clay pans appear in the region of Alicante and Murcia at the same time as the disappearance of wheel-made pottery and the shift in pottery morphology from open to closed forms. Much like in Eastern Europe, the combination of bread baking on an open fire by means of clay pans and porridge-like foods served in pots represents a radical departure from previous eating practices. This opens the possibility of treating the similar changes taking place in Eastern Europe not as a result of the Slavic migration, much less of a specifically Slavic “cooking culture,” but of radical changes in diet, themselves an indication of dramatic social and economic changes (*Wickham 2005*, 749–751). In other words, Pleterski’s “cooking culture” is not a matter of ideology (*Pleterski 2009*, 334), but of infrastructure.

All three commentators seem to have set for themselves only the questions that they believed needed to be answered. Yet I would not agree with their narrowly defined focus, precisely because of the “plausible synthesis” I called for in the introduction to the *Making of the Slavs* (*Curta 2001a*, 3). If one writes one’s own history, it might be said, then one does so with the benefit of theoretical approaches, types of methodology, and general historiographical supports that are not always of one’s own choosing. Having that in mind, I nonetheless disagree with Felix Biermann’s conclusion at the end of his comment: “Wenn freilich Curtas Hypothese zuträfe, die hier als frühslawisch betrachteten Sachzeugen des Prager Typs seien gar nicht als slawisch zu deuten, sondern ethnisch ganz unspezifisch,

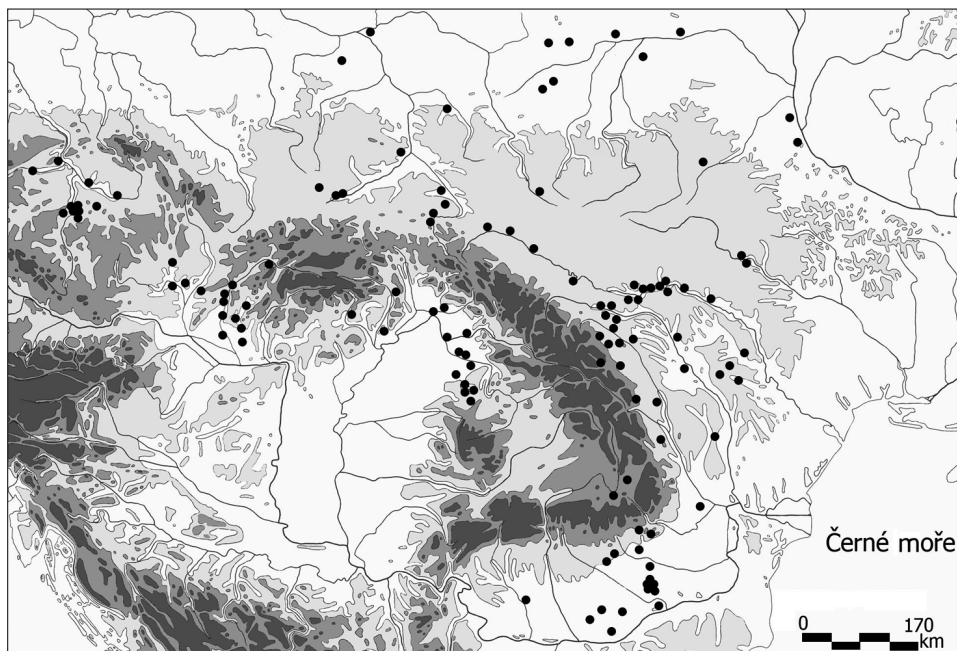


Fig. 1. Distribution of sites attributed to the first phase of the "culture with Prague-type pottery." Data after Kuna – Profantová 2005; Jelínková 1985; Parczewski 1991; 1992; Stanciu 2004; Baran 1983; 1994; Fusek – Zabožník 2005; Viargei 2005.

Obr. 1. Naleziště přířčená první fázi „kultury s keramikou pražského typu“. Data podle Kuna – Profantová 2005; Jelínková 1985; Parczewski 1991; 1992; Stanciu 2004; Baran 1983; 1994; Fusek – Zabožník 2005; Viargei 2005.

wäre ihre Datierung für die Aufhellung der Anfänge der slawischen Besiedlung letztlich unerheblich; es gäbe im Grunde gar keine Möglichkeit mehr in diesen Fragen weiterzukommen.“ In reality, the only reason for dating the beginnings of the Prague culture to the sixth century is that the Slavs are mentioned at that time in the written sources. Without written sources, the dating of the Prague culture on archaeological grounds alone would not match the traditional historical narrative pertaining to the history of the early Slavs. Independent means of dating, such as dendrochronology, have already highlighted the considerable chronological gap between the date commonly advanced for the Slavic migration and the earliest assemblages in Poland, Bohemia, Moravia, or Lower Austria, which could be attributed to the Slavs. So far, not a single dendro-date has been produced which could support the traditional model of historical development. On the contrary, everything points to a much later date for those developments and to the need of a serious re-examination of the evidence, both historical and archaeological. Dendrochronology is certainly the only solution to the problems which are now plaguing the archaeology of the early Middle Ages in East Central and Eastern Europe. But I believe we already have sufficient data to move away from the old interpretative model and to advance a new one. In order to do so, we need to be able to detach ourselves from the old habits of culture history, especially from the tendency to link archaeological cultures (which in any case are only archaeological constructs) with ethnic groups known from written sources (which are rarely, if ever taken to be anything but objective reports of what had really happened). As my book was set to argue, the turning away from culture history leaves us with little firm ground under our scholarly feet. Biermann's problem is not that, should my hypothesis prove to be right, there would be no way to re-examine the

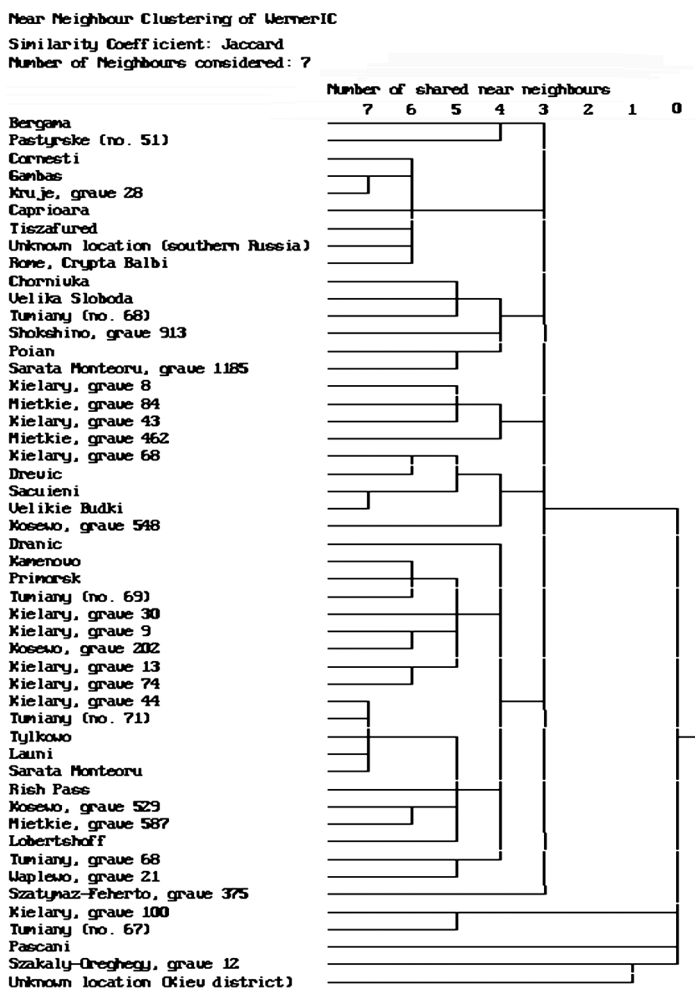


Fig. 2. Near-neighbor cluster analysis of 50 bow fibulae of Werner's class I C. Based on *Curta 2008b*, with additions.

Obr. 2. Shluková analýza 50 paprščitých spon (*bow fibulae*) Wernerovy třídy I C metodou nejbližšího souseda. Podle *Curta 2008b*, doplněno.

problems of the Prague culture. The true reason for his fears is elsewhere, namely in that, should the current chronology of the Prague culture be rejected either by a re-examination of its foundations (*Curta 2008d*) or by means of dendrochronology, there would be no support any more for the theoretical framework on which Biermann's entire set of scholarly assumptions is based. If I say that there was no migration of the Slavs – either to the Lower Danube or to Bohemia – from the Pripet marshes or from western Ukraine, I am not referring only to the absence of any indication of such a migration in the written sources (Biermann's touchstone for any historical interpretation), but also to the lack of any archaeological evidence. For migration to take place, the area from which the migration is said to have begun must have witnessed a considerable decrease of population, at least equivalent to the number of people believed to have moved out of the area and into other regions, such as Bohemia. But a mapping of all sites attributed to the "Prague culture," whether rightly or wrongly (*fig. 1*),<sup>40</sup> and

<sup>40</sup> Out of 132 sites listed in *Kuna – Profantová 2005*, 75–84, and attributed to the so-called "culture with Prague-type pottery," only 27 are dated to one of the three phases of that culture. Of those, only 12 belong to the first phase.

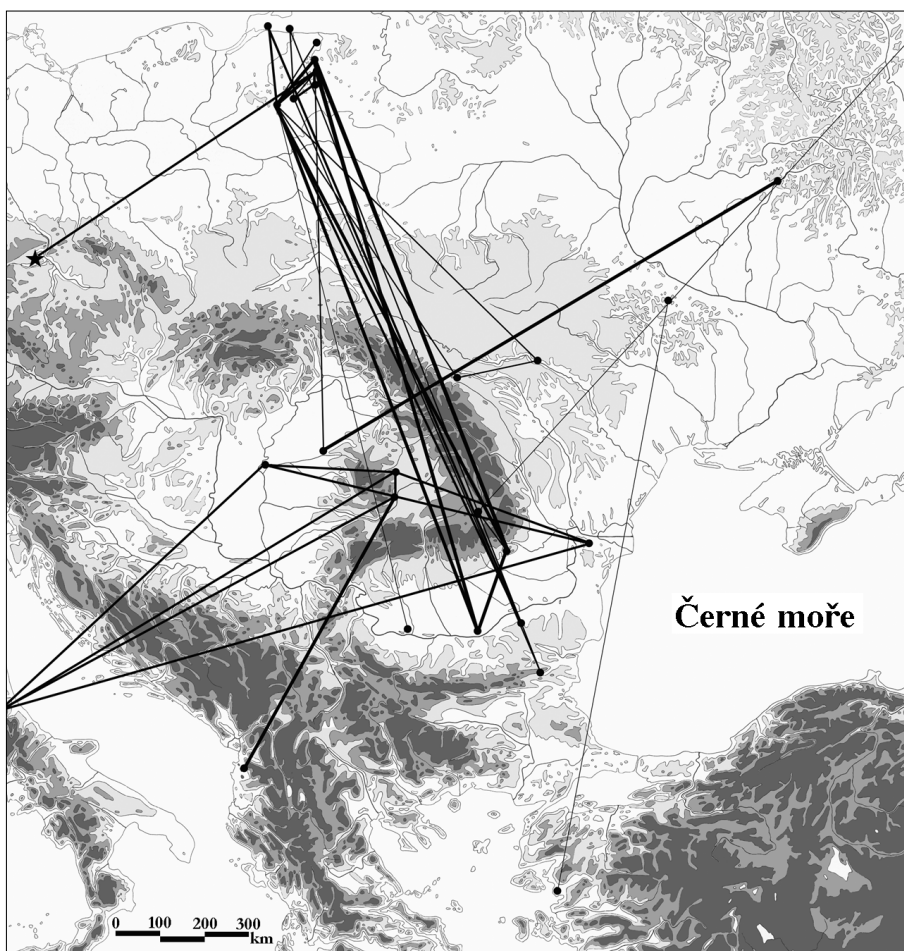


Fig. 3. Plotting of the nearest-neighbor similarity of 48 fibulae of Werner's class I C. Diminishing line thickness indicates the decreasing number of shared neighbors, from seven (thickest) to five (thinnest). Based on *Curta 2008b*, with additions. The star indicates the location of Dřevíče.

Obr. 3. Vynesení podobnosti 48 spon Wernerovy třídy I C metodou nejbližšího souseda. Ztenčování čáry označuje klesající počet sousedících charakteristik, od stupně 7 (nejsilnější) do stupně 5 (nejslabší). Podle *Curta 2008b*, doplněno. Hvězdička vyznačuje polohu Dřevíče.

dated to the sixth or early seventh century clearly shows that instead of a rarefied settlement network in that region in western Ukraine and Bukovina from which the migration is believed to have started, the number of settlements in that region is larger than that of Slovakia, Bohemia, or Poland. The supposed migration did not thin out the population in the supposed *Urheimat*. The number of settlements

Similarly, out of 63 sites of the Prague-type pottery culture in Moravia, only 5 are dated, 4 of which belong to the first phase (*Jelínková 1985*). For Slovakia, see *Fusek – Zabojník 2005*. For Poland, see *Parczewski 1991* and *1992*. For the region of the Upper Tisza, see *Stanciu 2004*. For western Ukraine and Bukovina, see *Baran 1983* and *1994*. For Belarus, see *Viargei 2005*. For sites in Romania and Moldova, see *Curta 2001a*, 235 fig. 31.

is in fact larger than that of the fifth century and that number continued to grow throughout the seventh and eighth centuries. If, as it seems likely, conditions in the supposed *Urheimat* were favorable to a population growth, why would anyone want to leave it for the distant lands of Bohemia or southern Poland? So far, no answer was offered to this question by any advocate of the migrationist model, from Kazimierz Godfowski to Felix Biermann.<sup>41</sup> Nor can any explanation be apparently found for the lack of any significant similarity between the pottery found in assemblages securely dated to the late fifth or early sixth century in Bukovina or Ukraine and the earliest assemblages with pottery of the so-called Prague type found in Bohemia. Nada Profantová believes Kozly to be the earliest datable assemblage in Bohemia (*Profantová 2009*, 320 fig. 9). However, a quick comparison between the pottery found in the house 10 excavated in Kodin together with a fifth-century crossbow fibula of the Prague type (*Rusanova – Timoshchuk 1984*, 22 and 48; 74 fig. 14: 5–12 and 14) and the pottery from Kozly (*Zeman 1976*, 125–126; 125 fig. 5) reveals substantially different types of vessels, with rim and lip variations ranging from Parczewski's classes Bc3, Ce3, and Fl3 to his class Bc2 (*Parczewski 1993*, 51–54 figs. 13–16). Nor can analogies for the Kozly pottery be found in house 20 from Botoșana, in which handmade pottery was associated with a bronze coin struck for Emperor Justinian in Constantinople between 527 and 538 (*Teodor 1984*, 36–37; 99 fig. 20: 4; 120 fig. 41: 3–4; 127 fig. 48: 1; 109 fig. 30: 2). There are no parallels for the almost vertical rims with straightly cut lips of the pots in Kozly among the rim sherds found together with a fragment of a late sixth or early seventh-century bronze bracelet with widened ends decorated with engraved ornament in house 67 in Rashkiv (*Baran 1988*, 111–112; 156 pl. 52: 1–6). Perhaps even more importantly, assemblages with handmade pottery of the so-called Prague type found in Bohemia and Moravia have so far produced no remains of the wheel-made pottery which is often associated with handmade pottery in assemblages from Bukovina and western Ukraine securely dated to the late fifth or early sixth century.<sup>42</sup>

In Nada Profantová's words, "greater relevance in this respect could only have the year 568, in which the Avars destroyed the power of the Gepids, and became sole rulers of the Carpathian Basin, after the Lombards left for Italy" (*Profantová 2009*, 312). At the time she published her path-breaking study on Avar artifacts in the lands to the north and northwest from the Avar qaganate (*Profantová 1992*), very few such artifacts were known from the territory of the Czech Republic, which could be dated with any degree of certainty to the Early Avar period (*ca.* 570 to *ca.* 630).<sup>43</sup> In the meantime, the number of Early Avar finds has increased considerably, mostly because of the use of metal detectors. Besides belt buckles of the Corinth and Balgota classes (Prague-Košíře, Tismice and unknown location in Bohemia), a belt buckle with shield-shaped terminal (Schulze-Dörrlamm's class D35) was found in Kšely together with a belt mount with open work ornament, a specimen of the so-called Martynovka class of mounts, which is typical for the late sixth and especially the early seventh century (*Profantová 2008*, 632; 636 fig. 13; 637 fig. 15: 5–7).<sup>44</sup> A bronze strap end of Zabochnik's class 7,

<sup>41</sup> This argument refers exclusively to what Biermann calls "the beginnings of the Slavic settlement" and should in no way be interpreted as an indication of my blanket rejection of the idea of migration in history. The migrationist model seems to work well with other cases (Avars, Magyars, Pechenegs), and even in the case of those Slavs who settled into such areas as northern Poland or northern Russia (*Vlasova 2001; Dulinicz 2006; Barford 2009*).

<sup>42</sup> Besides house 10, wheel-made, coarse wares have been found in several other assemblages from Kodin (houses 7, 11, 12, 14–16, 21, 26, and 36; *Rusanova – Timoshchuk 1984*). Wheel-made pottery is also known from Botoșana (houses 1, 9, 14, 17, 19, 20, 23, 25, 26, 28, 30, and 31; *Teodor 1984*) and Rashkiv (houses 12, 52, 58, 59, and 73; *Baran 1988*). Several other late fifth- to early seventh-century sites in Bukovina and western Ukraine (Hrozyntsi, Horecha, Hlyboka, Turiatka, Kavetchina, Ust'ia, Teremtsi, Zadubrivka, Khotyn, Samushin, Luka Vrublyvets'ka, and Bakota) have produced the wheel-made wares, which are conspicuously absent from assemblages in Bohemia and Moravia.

<sup>43</sup> According to *Profantová 1992*, 608, the only such artifact found in Moravia was a square belt mount from Olomouc. At that time, no Early Avar finds were known from Bohemia.

<sup>44</sup> It is perhaps relevant in this respect that two gold coins, both stray finds, are known from Kšely, one of which was also found by metal detector. One of the two coins is a solidus struck in Constantinople for Emperor Heraclius

found in Rubín, may also be dated to the Early Avar period (*Profantová 2008*, 638 fig. 18: 4). A similar dating may be advanced for the “Slavic” bow fibula from Dřevíč, a peculiar specimen of Werner’s class I C with only one pair of bird heads, a class recently dated to the late sixth and early seventh century, at the earliest (*Profantová 2008*, 638 fig. 17: 2; *Curta 2008b*). It is important to note that the closest morphological and ornamental parallel for the Dřevíč fibula within the entire class Werner I C is not a specimen from the neighboring territory of the Avar qaganate, but a fibula from grave 68 in Kielary (Poland; fig. 2, 3; *Kühn 1981*, 181 and pl. 40: 252).<sup>45</sup>

None of those artifacts was associated with handmade pottery of the so-called Prague-type. Before the archaeological attestation of such an association, it would be premature to draw any conclusion regarding the chronology and historical interpretation of contacts with the Avar qaganate during the seventh century. However, it is worth pointing out that, whatever their interpretation, those artifacts found in Bohemia signal contacts with the regions to the south and southeast (or, in the case of the Dřevíč fibula, to the northeast). No artifacts or assemblages have so far been found to support the idea of contacts with Bukovina and western Ukraine, much less a migration from those regions.

Divested of earlier prejudices, while taking on board all one can learn via the “literary turn,” new interpretations of the history of the early Slavs can now be put forward. Likewise, the broadening of the network of comparisons for the archaeological evidence from assemblages excavated in Bohemia and Moravia will undoubtedly illuminate many economic and social aspects pertaining to sixth- and seventh-century communities in Central and East Central Europe. We can now begin to think about new ways in which to open a dialogue between historians and archaeologists. In the interest of that conversation, though, it is important that we do not abandon a critical position in relation to the evidence, whether historical or archaeological. The immediate benefits of that position should be plain, unless one is determined to have it otherwise.

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between 616 and 625. The other coin is a gold-plated, silver imitation of another solidus struck for Heraclius between 625 and 629 (*Milinký 2009*, 376).

<sup>45</sup> The other two “Slavic” bow fibulae known from Bohemia (Dřevíč and Liteň; *Profantová 2008*, 634–635 [with wrong attribution]; 638 fig. 17: 1 and 3) are both fragmentary specimens of Werner’s class I F, which has been dated to the sixth century (*Curta – Dupoi 1994–1995*; *Curta 2008a*). The Dřevíč specimen has an excellent analogy in an equally fragmentary specimen from Bucharest-Băneasa (*Constantiniu 1965*, 77–78, 92 fig. 18). However, given the fragmentary state in which they were found and the lack of any information about the archaeological context in which they were found, one cannot exclude a much later date for both artifacts. Fragments of fibulae of Werner’s class I F have indeed been found in much later assemblages, such as the Kamenovo hoard (*Pisarova 1997*).

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## Počátky Slovanů v Čechách a na Moravě: odpověď mým kritikům

Kritici autorova článku otištěného v *Archeologických rozhledech* (50, 2008), Naďa Profantová, Felix Biermann a Andrej Pleterski, se především zaměřili na způsob interpretace písemných pramenů (Prokopios Caesarejský a Fredegar) a otázku migrace. Autor odmítá argumenty předložené oněmi třemi autory buď proto, že spočívají na kulturně historickém paradigmatu (N. Profantová), nebo že nepřístupují kriticky k interpretaci písemných pramenů (F. Biermann). Fredegarovu kroniku nelze brát doslovně, aniž bychom upadli do závažných interpretačních potíží, vzhledem k tomu, že neexistuje ani archeologický, ani písemný dokladový materiál o příchodu dávných Slovanů ze západní Ukrajiny a Bukoviny do Čech a na Moravu. Proti předpokladu specificky slovanské „kultury vaření“ A. Pleterského namítá autor, že většinu složek této kultury, obzvláště vysoké a úzké hrnce a užití keramických pánví k pečení plochých bochníků chleba na otevřeném ohni, lze nalézt i v jiných regionech Středomoří, kam dávní Slované nikdy nepřišli.

Czech by Petr Charvát

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*Tímto příspěvkem redakce diskusi uzavírá.*