"AN HESITATING JOURNEY THROUGH FOREIGN KNOWLEDGE": NICULESCU, THE OSTRICH, AND CULTURE HISTORY

BY

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Abstract

In a recent article, Gh. A. Niculescu raises the question of the relation between culture-historical archaeology and the so-called "production of knowledge" on ethnic phenomena. He targets the works of Volker Bierbrauer, Sebastian Brather, and Florin Curta. At a closer examination, however, Niculescu's paper is based on a distorted understanding of what culture history actually is, and on wrong assumptions about such fundamental concepts as ethnicity or (material culture) style. Besides flaws in this line of thinking, his paper reveals Niculescu's dishonest citation practices, his efforts to create a straw man, and his weak credentials for assuming any critical position in terms of the "production" of archaeological literature in the culture-historical mode. Beyond rhetorical tricks and smearing tactics, Niculescu does not in fact advance any solution to the problem, and remains ambiguous, if not altogether confused about the role of "social sciences" in the archaeology of (medieval) ethnicity.

Keywords: *culture history, ethnicity, archaeological theory, nationalism, style.*

Words matter. They matter because without them we could neither formulate our thoughts nor communicate in a coherent manner, and the choice of particular words has a great impact upon the way we write and evaluate the works of others. A recent article in *Dacia* aims at unraveling "what happens when new knowledge about ethnic phenomena, taken from other disciplines and/or from other ways of thinking and doing archaeology, is used in culture-historical archaeology."[1] The author chooses Volker Bierbrauer, Sebastian Brather, and myself as examples of practicians of culture-historical archaeology, the latter two singled out "for the impact of their training in a research tradition which was built around the task of identifying peoples from the past on how knowledge from the social sciences is used."[2] Although Gh. A. Niculescu's crusade against culture-history is certainly a laudable cause, things are neither as easy, nor as clear-cut as they might appear from his article. Niculescu's argument for the conclusion contrasts my views on ethnicity in (medieval) archaeology with those of Sebastian Brather. Despite the harsh criticism with which Niculescu treats him, Brather eventually gets the accolade "for his effort to maintain active what is unsettling for the traditional 'ethnische Deutung' in the knowledge on ethnic phenomena taken from the social sciences."[3] By contrast, Curta is someone who "continues to use the same methods he used before his claimed conversion to non-nationalist views on ethnicity."[4]

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[1] Gh. A. Niculescu, *Culture-historical archaeology and the production of knowledge on ethnic phenomena*, in *Dacia*, N.S., 55, 2011, p. 6. Elsewhere in the text, Niculescu seems to equate "tradition of research" with "knowledge," while at the same time maintaining that the latter is accumulated in the former (*Ibidem,* p. 7 with n. 8). Without any operational definition in the text, "knowledge" appears as truly "foreign" to the author. Moreover, the title of Niculescu's paper promises that the emphasis will be on the *production*, not the accumulation of "knowledge." That promise is never fulfilled.

[2] *Ibidem*, p. 5.

[3] *Ibidem*, p. 22. Apparently, Brather's is a "serious attempt to transform the research tradition into which he was trained following the guidelines suggested by it: better definitions and methods." One is left with the impression that Niculescu has found a soul mate.

[4] *Ibidem*.

Some might wonder whether Niculescu's lumping together Bierbrauer, Brather and Curta's works as "culture‑historical archaeology" violates his own warning against taking any "local methodology, arguably related to a groupist social ontology" as a "recognition of the structure of past worlds."[5] He might respond by distinguishing between a philosophical discussion of theoretical positions or assumptions, and archaeological typologies, and argue that while his objection is to the latter, his own discussion of my views is of the former sort. Perhaps this response is implicit in his mention of "undiscussed assumptions, exemplary practices, and particular ways of conceiving interdisciplinarity."[6] This response is not without its difficulties. Niculescu acknowledges the tensions. In a sudden confession, he admits being "also educated as a culture‑historical archaeologist" and says that he knows "how difficult it is to engage in describing what" to him has been "a tortuous and hesitating journey through foreign knowledge." In the course of that journey, Niculescu was apparently "blinded" by what he believed he knew, but only partially. Like Saul on the road to Damascus, he eventually recovered his sight, only to be puzzled by "lasting misunderstandings" surrounding him, while benefiting "from being recognized as an archaeologist" by his colleagues. He appears to have arrived at conclusions "of such importance that you try to convince them to abandon what they were accustomed to believe."[7] Leaving aside for the moment the paramount concern with obtaining recognition (and, one presumes, validation) from his peers, the confession in the middle of his article shows Niculescu to be a man with a mission. I shall leave open whether his position can avoid contradiction. I will also not discuss his views of Bierbrauer or Brather[8]. Instead, I shall focus on getting the record straight and pointing to key problems in his deeply flawed argument[9].

Gh. A. Niculescu seems to recognize what he is up against. He notes that "any attempt to assign finds to peoples from the past does not start with the archaeological record, but with the belief that those peoples existed."[10] Archaeologists, according to Niculescu, "do not see all that there is out there to see, but only what their archaeological training prepares them to see." It seems to me that Niculescu's intention is to take the owl's‑eye view. Just as the owl sees even in darkness, so he aims to evaluate views even when they are obscured in the dim light of convention and orthodoxy. In other words, Niculescu knows that there is more out there, which archaeologists trained, for example, in the culture‑historical tradition cannot see. It comes as a great surprise, therefore, that he takes an alternative avian model. Like the proverbial ostrich burying its head in the sand, he refuses to tell his reader how things could be better than currently available, or what the alternative is to what he regards as a "research tradition built on the ethnic paradigm."[11] One would simply love to see where a man with a mission like Niculescu would lead all those roaming through the uncharted

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[5] *Ibidem*, p. 12.

[6] *Ibidem*, p. 21. As if in need to support his point of view, Niculescu cites M. Johnson, *On the nature of theoretical archaeology and archaeological theory*, in *Archaeological Dialogues*, 13, 2006, p. 117: "there is a lack of correspondence between theoretical backgrounds and affiliations that are overtly cited by archaeologists, on the one hand, and, on the other, the deeper underlying assumptions and traditions that structure their work and condition its acceptance." The irony of the situation is obvious: Niculescu does not even think of the possibility that Matthew Johnson's words apply better to his (Niculescu's) own position than to anyone else.

[7] *Ibidem*, p. 10. To illustrate his efforts, Niculescu mentions "an attempt at a conference" in Nyíregyháza, and sends the curious reader to the online version of his paper presented at the conference. From that paper, one can learn, among other things, that Niculescu was "trained to believe that peoples are the movers and shakers of history and that the history of a nation is the most natural thing to do and the main task of a historian" (http://jam.nyirbone.hu/muzeum/nka/Niculescu.pdf, visit of April 6, 2014).

[8] My own reactions to Sebastian Brather's views may be found in F. Curta, *Review of S. Brather, Ethnische Interpretationen, in der frühgeschichtlichen Archäologie. Geschichte, Grundlagen und Alternativen (Berlin, 2004)*, in *Archaeologia Bulgarica*, 10, 2006, pp. 91–94; F. Curta, *Some remarks on ethnicity in medieval archaeology*, in *Early Medieval Europe*, 15, 2007, no. 2, pp. 159–185; F. Curta, *Medieval archaeology and ethnicity: where are we?* in *History Compass*, 9, 2011, no. 7, pp. 537–548; F. Curta, *The elephant in the room. A reply to Sebastian Brather*, in *Ephemeris Napocensis*, 23, 2013, pp. 163–174.

[9] This is meant to help also those who have taken Niculescu at face value. A. Dobos, *Gepidák vagy avarok? Az erdélyi kora avar kori soros temetık kutatásának kérdéseirı l*, in *Dolgozatokaz Az Erdélyi Nemzeti Múzeum Érem – És Régiségtárából*, 6–7, 2011–2012, p. 107 with n. 140 sends his readers to Niculescu for a critique of my notion of "emblemic style." As I will show below, Niculescu's is far from being a true critique. Meanwhile, to I. Stanciu, *The problem of the earliest Slavs in intra-Carpathian Romania (Transylvania and the north-west vicinity)*, in *Slovenská Archeológia (SlovArch)*, 61, 2013, no. 2, p. 324 with n. 4 Niculescu's text appears as a "very interesting recent study." I agree with Stanciu that this text is interesting, but for other reasons than those he may have in mind.

[10] Gh. A. Niculescu, op. cit. (n. 1), p. 6. To Niculescu, the existence of "peoples from the past" is a matter of belief, not "knowledge."

[11] *Ibidem*, p. 6. Instead of a definition of what the "ethnic paradigm" is, at least to him, Niculescu illustrates his point with Dan Gh. Teodor's recent plea for archaeology as national history. To be sure, Gh. A. Niculescu, *Archaeology, nationalism and the "History of the Romanians" (2001)*, in *Dacia*, N.S., 48–49, 2004–2005, pp. 99–124, is a thorough critique of nationalism in Romanian archaeology, including Teodor's scholarly output. But besides describing the water (the all‑powerful influence of culture history), Niculescu does nothing to save the man (the Romanian archaeology) from drowning.

territories of culture history, and what is his plan for a brighter future of the discipline. He has after all concluded his little confession with the idea that "the description of such journeys [as his] might help us put into perspective what we know and what *we* have to do in order to achieve our scientific goals."[12] The message is clear: Niculescu has gone alone on this difficult journey of recuperating good archaeology from the claws of culture history, but he is willing to share with all of us the boon of his conversion. However, when one seeks some guidance from the man with a mission, one is quickly disappointed. All that Niculescu has to offer is some vague and emotional advice: "If we wish to avoid this obvious reproduction of the contemporary past in our representations of the distant past, we have to leave open what we think about it, to let our finds surprise us [!], not to make them behave [!!] according to our plans."[13] "Good" archaeology, according to Niculescu, is one in which finds behave and in doing so, they take archaeologists by surprise.

Gh. A. Niculescu's argument is a rambling, repetitive one, but it can be distilled. His complaint about my work on medieval ethnicity is that I have been supposedly "educated within the research tradition of culture-historical archaeology."[14] Having earned his Ph.D. from the University of Bucharest at the age of 46, with a dissertation entitled "The Romanic population on the territory between the Alps and the Middle Danube in the 4th–7th centuries AD," Niculescu chooses to ignore the fact that my archaeological education is from the United States, not Romania. In fact, much of what I wrote on ethnicity in archaeology is the result of, and postdates my doctoral studies at Western Michigan University in the United States. The greatest influence on my mode of thinking has not been that of Niculescu's professors at the University of Bucharest, but that of Allen Zagarell, whose course on archaeological theory has been both an eye-opener and an introduction to the work of Bruce Trigger[15]. Contrary to Niculescu's assertions, there is actually no basis for his claim that I was "educated as a culture-historical archaeologist who explores the social sciences."[16] If one must attach a label to my education and training, that is "post-processualist," not "culture-historical."[17] Leaving aside for now Niculescu's bizarre insistence to present my academic curriculum as "culture-historical," it is worth asking what exactly is culture-historical archaeology to him. At a close examination of his paper, he appears to reduce it all to "a research tradition built on the ethnic paradigm," in which the main conceptual tool is the notion of "archaeological culture."[18] So, if one follows Niculescu's line of reasoning, if anyone works within the "ethnic paradigm" and employs the notion of "archaeological culture," that person is automatically a culture-historical archaeologist[19].

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[12] Gh. A. Niculescu, op. cit. (n. 1), p. 10 (emphasis added). The emphatic plural is in sharp contrast to the second person singular of the actual confession.

[13] *Ibidem*, p. 15. In a footnote to this passage, Niculescu appears surprised that in a recent article (F. Curta, op. cit. [n. 8], in *History Compass*, 9, 2011, no. 7), I did not mention anything that would change my previously expressed views. Leaving aside the absurd assumption that one is expected to adopt in one's work points of view contrary to one's own opinions on the matter, it would have been interesting to see who (or what) could make me change my mind, according to Niculescu.

[14] *Ibidem*, p. 10.

[15] B. G. Trigger, *A History of Archaeological Thought*, Cambridge University Press, Cambridge, 1989. For Zagarell, see his homepage at http://homepages.wmich.edu/~zagarell/ (visit of April 6, 2014). My name is mentioned in the acknowledgments at the end of A. Zagarell, *Gender and social organization in the reliefs of the Nilgiri Hills*, in *Forager-Traders in South and Southeast Asia. Long-Term Histories* (eds. K. D. Morrisson, L. L. Junker), Cambridge University Press, Cambridge, 2002, pp. 77–104.

[16] Gh. A. Niculescu, op. cit. (n. 1), p. 10. My education at the University of Bucharest is irrelevant in this respect, since I was trained as a Classicist, not as an archaeologist. My senior thesis is a translation and commentary of the second panegyric that Julian wrote for Emperor Constantius II (see F. Curta, *Atticism, Homer, Neoplatonism and Fürstenspiegel: Julian's Second Panegyric on Constantius*, in *Greek, Roman, and Byzantine Studies*, 36, 1995, no. 2, pp. 177–211; and F. Curta, *Kaiserliche Lobrede und politisches Programm: die dritte Rede des Julianus Caesars*, in *Eranos*, 95, 1997, pp. 39–56). As such, my thesis has nothing to do either with archaeology, or with culture history. Following my B.A. from the University of Bucharest (1988), I have not received any other form of post-graduate education in Romania.

[17] Even Gh. A. Niculescu, op. cit. (n. 1), p. 15 is forced to admit that much, when writing that "Curta's epistemic optimism is based on the highly questionable idea that dominated the beginnings of *post-processual archaeology*, before practice theory kicked in, that 'material culture is a 'text' to be read' by archaeologists..." (emphasis added). Niculescu may have mixed up his priorities at this point: he wants to paint the "enemy" (me) as a culture-historical archaeologist, but he also hates post-processualism with a passion, so much so that he cannot let the opportunity go without warning his readers that the idea that material culture is a text is "highly questionable."

[18] He also seems to believe that culture-historical archaeology is recognizable by its "mapping methodology" (*Ibidem*, p. 17). The culture-history archaeological literature – which Niculescu appears to know very well – is apparently full of types, which one could pick up and use in one's work (*Ibidem*, p. 17 with n. 69)

[19] While it is true that I have been for a long while concerned with ethnic identities in the Middle Ages (whether or not that puts me in the "ethnic paradigm"), I never defended the notion of "archaeological culture." On the contrary, I have in fact criticized the idea of a Prague-type culture (F. Curta, *The Prague-type: a critical approach to pottery classification*, in *Archaeologia Bulgarica*, 5, 2001, no. 1, pp. 73–106; F. Curta, *Utváření Slovanů [se zvláštním zřetelem k Čechám a Moravě)*, in *Archeologické rozhled]*, 60, 2008, pp. 1–54;

Niculescu leaves out a number of key features of culture history, which are otherwise important for his discussion. One of them refers to the idea that cultures (including archaeological cultures) are normative, i.e., people "behave" in certain culturally characteristic ways without necessarily being aware of what they are doing. The culture-historical understanding of culture thus typically excludes agency from the equation[20]. Second, typology is of great significance for culture history not because of "groupist social ontology," as Niculescu would have it, but because of a stringent need of chronological control, in the absence of which no history could be written. In other words, nobody could write history on the basis of the archaeological sources without a good chronology of the material, which, in the absence of independent methods of dating (such as radiocarbon or dendrochronology) is necessarily based on typology and seriation. Niculescu would have us believe that typology is just as bad as culture history, but in reality typology is merely a tool, i.e., only a method of classification based on some pre-established criteria. One can definitely toss out culture history, and keep typology. As a method, typology is not "contaminated" by culture history. Only its use (which is dictated by theoretical choices) could be incriminated as "culture-historical." In and for itself, typology is therefore neutral. Whether or not types existed in the minds of ancient producers and users is, of course, an entirely different question. Third, Niculescu appears to believe that culture history is intrinsically nationalist. Eerily similar to a mantra, the phrase "nationalist representation of society" appears several times in his paper. He goes as far as to say that "if we apply this representation to the early medieval *gentes* this means that everybody behaved more or less like the royal or princely families, and that the behavior of those families had more in common with what the rest of the ethnic group did than with the behavior of other royal or princely families, which brings to mind the nationalist claim that having people 'of your kind' as rulers is natural."[21] The use of the singular "representation" implies that Niculescu thinks of a kind of template, but he never explains what exactly is a nationalist representation of any society. Even more incomprehensible is his link of my supposedly culture-historical inclinations to a desire "to make the past relevant for the ethnic present." One problem with his argument is that it is not based on evidence. In fact, quite the contrary is true. I wrote a book about the early Slavs, at the beginning of which I asked, "Who were those enigmatic Slavs?" Niculescu thinks that that is a "traditional question about the Slavs." Be as it may, one is left wondering how is that relevant for the ethnic present? Who in present-day Romania (the southern and eastern regions of which are at the center of my study) is served or assisted by my book? If anything, its conclusions are against the ethnic concerns of the present (or recent past). Niculescu does not understand that the Slavs are not a desirable ingredient of the Romanian ethnogenesis, and definitely not an element of the past that could be effectively used by Romanian nationalists. Perhaps he thinks that I serve the interests of non-Romanian nationalists. If so, it would be good to know, for the early Slavs are claimed as ancestors by a dozen of modern nation(alism)s, none of which would be happy with my idea that the Slavic ethnogenesis happened on the territory of present-day Romania. Be as it may, Niculescu is definitely barking at the wrong tree. That I do not make the past relevant for the ethnic present results clearly from my critique of such concerns regarding the early Slavs, a critique that was published twenty (yes, 20!) years ago[22]. At that time, I dealt in much greater detail with the

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F. Curta, *The early Slavs in Bohemia and Moravia: a response to my critics*, in *Archeologické rozhledy*, 61, 2009, pp. 725–754). In the absence of any positive evidence of culture-historical sins either in my curriculum or in my work, it remains unclear why would Niculescu think of me as a "culture-historical archaeologist." He is either completely confused in matters of archaeological theory, or an overzealous man with a mission.

[20] To be fair, Niculescu hints at precisely this matter when writing that Bierbrauer's view "makes of culture rules everybody follows, hence the assumed internal homogeneity and non-conflictual nature of the cultural units and the imagination of past human beings as *cultural robots*" (Gh. A. Niculescu, op. cit. [n. 1], p. 8, emphasis added).

[21] *Ibidem*, p. 19. Instead of a reference to any nationalist claim(s) that a native ruler is natural, Niculescu cites Fredrik Barth on a completely different matter. Moreover, Niculescu does not seem to know that, to employ Anthony Smith's terminology, not all nationalisms are lateral (aristocratic). Romanian nationalism, for example, is vertical (demotic). That is why the "nationalist representation of society" in Romania does not conjure any image of royal or princely family. The fascination with native dynasties is a characteristic of lateral nationalisms, as in the case of the Hungarian obsession with the Arpadian dynasty (and the Holy Crown), in sharp contrast to the lack of similar concerns with the Piasts or the Přemyslids in Poland or the Czech lands, respectively.

[22] F. Curta, *The changing image of the early Slavs in the Romanian historiography*, in *Revue des etudes sud-est-européennes*, 32, 1994, nos. 1–2, pp. 132–137; F. Curta, *The changing image of the early Slavs in the Rumanian historiography and archaeological literature. A critical survey*, in *Südost-Forschungen*, 53, 1994, pp. 229–233, reprinted in F. Curta, *Text, Context, History, and Archaeology. Studies in Late Antiquity and the Middle Ages*, Ed. Academiei Române/Istros, Bucureşti/Brăila, 2009, pp. 136–142. It is interesting to note in this context that five years after expressing doubts about the Sarmatian character of certain finds from Târgşor (Gh. A. Niculescu, *The cremation graves from the cemetery of Tîrgşor [third-fourth centuries A.D.]*, in *Dacia*, N.S., 37, 1993, p. 207 with n. 25), Niculescu wrote a paper about the "Sarmatian culture." At the beginning of that paper, which includes a map and a list of

changing attitude towards the Slavs in the post-war works of the Romanian historians and archaeologists writing under the Communist regime. I have demonstrated that before 1958, the Slavs were the main preoccupation of the Romanian school of historiography and archaeology studying the early Middle Ages (and the Romanian ethnogenesis). After that date, and particularly under Ceauşescu's regime, archaeologists and historians alike made every possible effort to write the Slavs out of the Romanian history. Therefore, Niculescu's mention of my "claimed conversion to non-nationalist views on ethnicity" is utterly ridiculous[23].

Not only does Niculescu ignore my 1994 article on the image of the Slavs in the Romanian historiography and archaeology, but he also passes over in silence my later study published in Romanian about ethnicity in medieval archaeology[24]. Neither could fit into this (pre)fabricated portrait of F. Curta as a "culture-historical archaeologist." His selection of my works cited in support of his arguments is very interesting, for it reveals his strategy, and ultimately his goal. Besides the fact that he deliberately ignores things that I wrote and would have directly contradicted his claims, Niculescu manipulates the quotes and often misrepresents my thoughts and arguments to score points. For example, according to him, "after mentioning the opinion that ethnicity is a relatively recent phenomenon, F. Curta states that 'ethnicity is just as likely to have been embedded in socio-political relations in the past as in the present'."[25] The quote in question is from my 2001 book on the making of the early Slavs. However, if one goes to the page indicated[26], one immediately notes that there is no mention of ethnicity being a recent phenomenon. Those words are not mine; they have only been attributed to me by Niculescu. Similarly, Niculescu claims that "F. Curta qualifies it [my supposed characterization of Fredrik Barth's work] as a 'subjective approach to ethnicity'."[27] He then proceeds to criticize my calling Barth's approach subjective. But what I wrote at the point to which Niculescu refers is something completely different: "To be sure, the subjective approach to ethnicity, which is so often and almost exclusively attributed to Barth, long precedes him."[28] Furthermore, Niculescu pontificates:

"Sarmatian finds" in Walachia, Niculescu warns his readers that in his paper "soll weder die ethnische Deutung archäologischer Funde noch die Frage behandelt werden, inwieweit der Begriff 'archäologischer Kultur' für die Beschreibung eines archäologischen Sachverhaltes angemessen ist." He then proceeds to do the exact contrary, namely to describe the features of the Chilia-Militari, Sântana de Mureş-Chernyakhov, and Sarmatian cultures. See Gh. A. Niculescu, *Die sarmatische Kultur im Zusammenhang der kaiserzeitlichen archäologischen Funde aus Muntenien – unter besonderer Berücksichtigung der Funde von Tîrgşor*, in *Kontakt-Kooperation-Konflikt. Germanen und Sarmaten zwischen dem 1. und dem 4. Jahrhundert nach Christus. Internationales Kolloquium des vorgeschichtlichen Seminars der Philipps-Universität Marburg, 12.–16. Februar 1998* (ed. C. von Carnap-Bornheim), Wachholtz, Neumünster, 2003, p. 177 with n. 1; p. 183 fig. 3; pp. 200–203. One wonders whether this text was not written specifically for an audience of German practicians of culture-historical archaeology, the favors (and letters of recommendation) of which Niculescu was trying to win. At any rate, just a couple of years later, he earned his doctoral degree with a dissertation that clearly made the past (of the so-called "Romanic population") relevant to the ethnic present (and the then current political concerns) of Romania.

[23] Gh. A. Niculescu, op. cit. (n. 1), pp. 15, 16, and 22. "Conversion" in this context implies that at one point in the past I had nationalist views of ethnicity. There is nothing in my publication record to prove the point, so Niculescu's attempt to throw mud at the wall looks pathetic, for nothing is going to stick.

[24] F. Curta, *Consideraţii privind conceptul de character etnic (etnicitate) în arheologia contemporană*, in *Arheologia medievală (ArhMed)*, 4, 2002, pp. 5–25. Equally ignored are F. Curta, *Pots, Slavs, and "imagined communities": Slavic archaeology and the history of the early Slavs*, in *European Journal of Archaeology*, 4, 2001, no. 3, pp. 367–384; F. Curta, *From Kossinna to Bromley: ethnogenesis in Slavic archaeology*, in *On Barbarian Identity. Critical Approaches to Ethnicity in the Early Middle Ages* (ed. A. Gillett), Brepols, Turnhout, 2002, pp. 201–218; and F. Curta, *The archaeology of identities in Old Russia (ca. 500 to ca. 650)*, in *Russian History*, 34, 2007, pp. 31–62.

[25] Gh. A. Niculescu, op. cit. (n. 1), p. 14.

[26] F. Curta, *The Making of the Slavs. History and Archaeology of the Lower Danube Region, c. 500–700*, Cambridge University Press, Cambridge/New York, 2001, p. 15.

[27] Gh. A. Niculescu, op. cit. (n. 1), p. 14.

[28] F. Curta, op. cit. (n. 26), p. 18. See also F. Curta, op. cit. (n. 24), in *ArhMed*, 4, 2002, p. 9 ("Dar abordarea subiectivă a problematicii caracterului etnic, deşi adesea atribuită pe nedrept lui Barth, are tradiţii mai vechi, atât în lucrările lui Weber, cât şi în opera lui Leach") and F. Curta, *Apariţia slavilor. Istorie şi arheologie la Dunărea de Jos în secolele VI–VII*, Ed. Cetatea de Scaun, Târgovişte, 2006, p. 16 („Abordarea subiectivistă a etnicităţii, atât de frecvent atribuită exclusiv lui Barth, îl precede însă cu mult."). An even more egregious example may be found in Gh. A. Niculescu, op. cit. (n. 1), p. 11 with n. 55: "F. Curta misrepresents F. Barth's ideas: 'the social interaction model rests on the assumption that stylistic characteristics will diffuse or be shared among social entities to an extant directly proportional to the frequency of interactions between these entities'." However, F. Curta, op. cit. (n. 26), p. 29 (the page from which this quote has been taken) has no mention of Barth. Instead the social interaction model is illustrated in note 65 with the so-called "Deetz-Longacre hypothesis," which has nothing to do with Barth's theories. Gh. A. Niculescu, op. cit. (n. 1), p. 16 with n. 65 uses a quote from F. Curta, op. cit. (n. 26), p. 294 to illustrate that I supposedly believe that the distribution of vessels with stamped decoration vs. that of vessels decorated with finger impressions and notches maps a form of emblemic style. The quote, however, refers only to the decoration with finger impressions and notches on the vessel's rim.

"F. Barth did not favor the 'we vs. them' perspective'."[29] He then claims that in my book I have misled the reader by sending him or her to Barth's work "for the 'we vs. them' perspective." At this point, it is worth citing in full the passage in my own book on which this confusion is based: "Barth's followers thus built on concepts of self and social role behavior typified by a dyadic transactional (the 'we vs. them' perspective) or social exchange theory."[30] Not only is the "we vs. them" perspective not attributed to Barth in this passage, but this is all about Barth's followers, not Barth.

Niculescu's goal is to create a straw man, which could serve him as a target for his attack against culture history. Tilting at windmills like Don Quixote, he draws a caricature of what F. Curta said by means of putting words in his mouth or of claiming that he had not provided proper citations[31]. According to Niculescu, in my "presentation habitus becomes ethnic style, a particular way of doing things, an idea as old as the nation state," despite the fact that he cannot cite a single passage in my book to support his claims: I have actually not equated habitus with ethnic style. After having attributed to me things that I have not written, he then depicts me as a follower of G. Carter Bentley, whose theories I supposedly preferred to Barth's[32]. Similarly, he cites a passage from a recent article, in which I mention scholars who are now beginning to realize that, as in the modern world, women in the medieval past symbolized ethnic collectives, and were attributed the role of being ideological reproducers of their ethnic group. Niculescu interprets this as my attempt to "equate ancient ethnic phenomena with nations" and as an illustration of the fact that, to me, women "were unable to choose what they wanted to wear, just passive vehicles of identity, reduced to their presumed 'ideological role'."[33] That there is no mention of nations in my text needs no emphasis, while Niculescu's distorted representation of my views on gender and ethnicity ignores everything I wrote on the subject, especially in relation to "Slavic" bow fibulae[34]. Much more serious, in my opinion, is his inability to distinguish between how women were viewed in the medieval past (which is obviously something different from what they truly were) and my own notion of the role of women in medieval society.

I wrote recently that "very few would now disagree with Max Weber that ethnic groups are 'human groups that entertain a subjective belief in their common descent because of similarities of physical type or of customs or both, or because of memories of colonization and migration; conversely, it does not matter whether or not an objective blood relationship exists'."[35] Niculescu thinks that "this is not true."[36] "This" apparently refers to the first part of the quote, namely to my claim that very few would now disagree with Weber. To prove the point, Niculescu quotes from an article by Michael Banton, according to which Weber's review of "ethnic communities" is antiquated. In reality, Banton's article is specifically about an untitled draft found among Weber's posthumous papers, which was later published under the title "Ethnic Communities." At the beginning of his article, Banton explicitly writes that his critique of that manuscript of Weber "does not extend to any passages in Weber's other works which bear on questions of ethnic community."[37] That Niculescu ignored this particular caveat is not an accident: Banton's quote, so hastily brought as ammunition against my statement, does not have any bearing on the passage I have taken from the English translation of Weber's *Wirtschaft und Gesellschaft*. Even if we assume for a moment that Banton could have referred to that work, I did not write that no people would disagree with Weber, only that *very few* would find themselves in that position. In his zeal for the cause, Niculescu writes with reckless disregard for details. He declares that he does not know "any social scientist who would agree with Weber that the belief in common descent is based on 'similarities of physical type or of customs or both'." In fact, that is not what Weber wrote: to him, what

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[29] Gh. A. Niculescu, op. cit. (n. 1), p. 14 with no. 49.

[30] F. Curta, op. cit. (n. 26), p. 18.

[31] E.g., Gh. A. Niculescu, op. cit. (n. 1), p. 15 with n. 52, quotes from F. Curta, op. cit. (n. 26), p. 295, claiming that "no reference to literature" is given that might support my claims. In reality, F. Curta, op. cit. (n. 26), p. 295 with n. 73 (F. Curta, op. cit. [n. 28], 2006, p. 255 with n. 73) has three references to such authors as Ina-Maria Greverus, Matti Räsänen, and Diane Tebbetts. There is a hint at lack of proper citation (allegedly no quotation marks for something that was, in fact, a paraphrase, not a direct quote) in Gh. A. Niculescu, op. cit. (n. 1), p. 15, with n. 56.

[32] But at the same time Niculescu asks himself (or the readers) rhetorically why I continue "to mention F. Barth as a source" of my thinking about ethnic phenomena (Gh. A. Niculescu, op. cit. [n. 1], p. 16). What is one to do with Niculescu's inconsistencies?

[33] Gh. A. Niculescu, op. cit. (n. 1), p. 21. The only reference to "nations" is in *Ibidem*, p. 20 with n. 89.

[34] E.g., F. Curta, *Female dress and "Slavic" bow fibulae in Greece*, in *Hesperia*, 74, 2005, no. 1, pp. 101–146.

[35] F. Curta, op. cit. (n. 8), in *History Compass*, 9, 2011, no. 7, p. 537. The quote is from M. Weber, *Economy and Society. An Outline of Interpretive Sociology* (transl. E. Fischoll *et alii*), University of California Press, Berkeley, 1968, p. 389.

[36] Gh. A. Niculescu, op. cit. (n. 1), p. 11 with n. 30.

[37] M. Banton, *Max Weber on "ethnic communities": a critique*, in *Nations and Nationalism*, 13, 2007, p. 20.

mattered was the "subjective belief" in common descent, a belief that was based on such things as similarities of physical type or customs, or on memories of colonization and migration. Niculescu's distorted understanding of Weber reveals superficial reading and an impatient desire to be right at any cost. In this case, at least, the cost is Niculescu's reputation as a serious scholar. His disregard for details occasionally produces hilarious effects. Niculescu confesses that he has never "encountered a discussion among archaeologists or social scientists about this 'prototypical manner'" in which ethnic group may define themselves[38]. But he cites an entire passage from my book, which he otherwise regards as "an important statement." The passage in question mentions the "primarily prototypic manner" in which people may identify their own ethnic group and cites the work of Cynthia Mahmood and Sharon Armstrong[39]. Apparently, Gh. A. Niculescu needs to see the word "prototypic(al)" twice before reacting.

There are more serious problems with Niculescu's idea of ethnicity. He strongly believes, for reasons known only to him, that "ethnicity as 'a phenomenon of everyday life' is what we live in our world of massive transmission of ways of thinking and of acting through the mass media and state supported educational systems, not of Late Antiquity."[40] The idea that there was no ethnicity in Late Antiquity will definitely raise the eyebrows of such scholars as Michael Maas, Patrick Geary, or Walter Pohl. At the very least, that idea needs to be demonstrated, not simply uttered (or, as it were, regurgitated): the burden of proof is on Niculescu's shoulders[41]. Niculescu says that he is aware "of the existence of some stereotypes" that could apply to him in Western Europe, but knows that such stereotypes only rarely translate into behavior. He then proceeds to tell the curious reader that, when in the United States, his English was said to have a German accent, while a Spanish-speaking inhabitant of Tucson, after hearing him speaking Romanian, asked him what kind of rotten Spanish that was. This, supposedly, is meant to illustrate that Gh. A. Niculescu was never identified as a Romanian by people who did not know where he was coming prior to entering a conversation with him[42]. There is absolute confusion at this point. Niculescu does not even see that his examples are of a Romanian speaking English, not of a Romanian behaving in any culturally specific (i.e., "Romanian") way. Nor does he consider the fact that, unlike Western Europe, there are no stereotypes about Romanians in the United States, as Americans had only limited contact with Romanians over the years. Even more embarrassing is his equation of style with culture history: "Style, despite the construction around it of a 'better' story by using the information from the written sources about the historical context, is just a new name for what culture-historical archaeologists usually do."[43] For all his journey through the land of foreign knowledge, and his defense of women whom I supposedly prevent from choosing for themselves what to wear, Niculescu still

[38] Gh. A. Niculescu, op. cit. (n. 1), p. 21.

[39] *Ibidem,* p. 14; F. Curta, op. cit. (n. 26), p. 21. See C. K. Mahmood and S. L. Armstrong, *Do ethnic groups exist? A cognitive perspective on the concept of cultures*, in *Ethnology*, 31, 1992, pp. 1–14. Similarly, but without using the term "prototypic(al)," Walter Pohl has argued that, when guessing at a stranger's origin, early medieval people scanned physiognomy, language, dress, and behavior, and were inclined to believe what the stranger told them. "Such pragmatic strategies of identification often are not too far off the mark, although they may fail in many cases. But they hardly offer a sound basis to define ethnicity." See W. Pohl, *Introduction – strategies of identification: a methodological profile*, in *Strategies of Identification. Ethnicity and Religion in Early Medieval Europe* (eds. W. Pohl, G. Heydemann), Brepols, Turnhout, 2013, p. 7.

[40] Gh. A. Niculescu, op. cit. (n. 1), p. 15.

[41] Fourteen years ago, Niculescu was definitely not that radical. Back then, his research was on "when and how the Goths came to Walachia, an attempt to do traditional history with the awareness of the influence the representation of ethnic entities might have" (Gh. A. Niculescu, *Ethnic phenomena in Late Antiquity*, in *Wissenschaftskolleg Jahrbuch*, 2000–2001, p. 136).

[42] Gh. A. Niculescu, op. cit. (n. 1), p. 13. Those examples are brought as a supposed argument against Brather's claim that people behave "wiees die eigene Gruppe und 'die Anderen' von eine mer warten" (S. Brather, *Ethnische Interpretationen in der frügeschichtlichen Archäologie. Geschichte, Grundlagen und Alternativen*, Walter de Gruyter, Berlin/New York, 2004, p. 108). Niculescu gives two examples from the United States, and another from Norway, none of which is in Western Europe. This points to equally stereotypical views of the West populating his own mind.

[43] Gh. A. Niculescu, op. cit. (n. 1), p. 18. According to Gh. A. Niculescu, op. cit. (n. 1), p. 16, my "way of identifying what is stylistic in artefacts takes us back" to the discussions about distinctions between style and function. See also Gh. A. Niculescu, op. cit. (n. 1), p. 17. Apparently, in his rush to score points, Niculescu skipped the following passage in F. Curta, op. cit. (n. 28), 2006, p. 32: "But style and function are not distinct, self-contained, mutually exclusive realms of form in themselves, but instead complementary dimensions or aspects of variation that coexist within the same form" (F. Curta, op. cit. [n. 28], 2006, p. 28: "Stilul Ŭ funcŃa nu sunt însă termeni autosuficienŃŬ reciproc exclusivi, ci dimensiuni complementare ce coexistă în aceeaŬ formă"). Gh. A. Niculescu, op. cit. (n. 1), p. 18 with n. 76 sends the reader to A. Gillett, *Ethnogenesis: a contested model of early medieval Europe*, in *History Compass*, 4, 2006, no. 2, p. 253 with n. 3, "for an understanding of what F. Curta does with his concept of style as a return to culture-historical archaeology." The only reference to F. Curta, op. cit. (n. 28), 2006 in Andrew Gillett's article is to a "reassertion of the association of style and ethnicity." There is no mention of culture-historical archaeology, and Gillett's remark is positive, not critical.

does not understand that, in order for style to work, it needs to be deliberate. Unlike culture history, which regards culture as working "behind the backs" of people, style has been defined as an intentional form of communication. Because it is a matter of choice, style cannot work "behind the backs" of producers and consumers, a point Polly Wiessner raised in her debate with James Sackett[44].

Given the rhetorical tricks Gh. A. Niculescu employs, not to mention his smearing tactics, it is ironic that he alleges that I have introduced "heteronomous knowledge to fellow archaeologists, knowing that they have little or no means of judging its quality."[45] It is not quite clear to whom Niculescu refers, or whether or not he includes himself in the group of fellow archaeologists. By contrast, it should have been crystal-clear from both the cover and the content of my book that the *Making of the Slavs* was not written for "fellow archaeologists" alone. The subtitle of the book, "History and Archaeology of the Lower Danube Region" (emphasis added) is there for a reason, and the order of words is not accidental. One can easily notice, however, the difference between my approach to ethnicity and Niculescu's. He believes that "structured meanings are present in the archaeological record, in the associations of artifacts and features, and we can try to grasp them using any available additional information on the possible referents, such as that provided by physical anthropology and the written sources" (emphasis added)[46]. Mine is not a text-aided approach. Nor do I think of historical sources as additional or some kind of crutches helping the archaeologist make sense of the archaeological record.

What Niculescu suggests as a warning against culture-history may in fact be a self-contradicting proposition[47]. He asks the readers to follow the example of the ostrich rather than the owl. There is an enormous quantity of material – both archaeological and historical – pertaining to ethnic difference and salience in Late Antiquity and the early Middle Ages. Given that, his suggestion that archaeologists adopt the point of view of the social scientists, who presumably see the whole humanity with different eyes, seems like an invitation to disregard the questions he himself raises about "the relevance of the social sciences for the past" or "the problems concepts designed for the understanding of contemporary phenomena and certainly influenced by them, like ethnicity, might create when used for Late Antiquity."[48] One cannot eat the cake and have it too.

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[44] Gh. A. Niculescu, *The material dimension of ethnicity*, in *New Europe College Yearbook*, 1997–1998, pp. 235–236 comes very close to that realization. In the 1990s, emblemic style was OK to Niculescu, just not for ethnicity (*Ibidem*, p. 238). Two decades later, the fog has set again on Niculescu's understanding of what style is and how it works.

[45] Idem, op. cit. (n. 1), p. 10.

[46] Idem, op. cit. (n. 44), p. 240.

[47] *Ibidem*, p. 241: "The recovery of ethnic meanings, as difficult as it is for the archaeologists, should not be abandoned." Apparently, fourteen years had to pass before Niculescu would abandon the "ethnic paradigm."

[48] Idem, op. cit. (n. 1), p. 10.