
THE IRON CURTAIN AND EURASIAN ARCHAEOLOGY

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Some notes on

**The Horse, the Wheel, and Language:
how the Bronze-Age Riders Shaped
the Modern World**

by David W. Anthony. Princeton, NJ:
Princeton University Press, 2007.

The lifting of the Iron Curtain has given scholars opportunities for closer communication with colleagues and for easier access to materials and publications. It was defined as a crucial point in the solution of the Indo-European problem offered by David Anthony in his book.

After years of traveling across Russia, Ukraine, and Kazakhstan, David Anthony, in his own words, an outsider to linguistics (p. 16), attempted to understand the existing literature on steppe archaeology “for twenty-five years with limited success” (p. 18), wrote a book on who, when and where the Proto-Indo-European language was spoken, explaining to the general reader the principles of historical linguistics and the reconstruction of the Proto-Indo-European lexicon and drawing archaeological evidence to support his conclusions.

Written in the manner of a story with close-to-poetic descriptions of cultural and historical events happened in the Eurasian steppes from the 5th millennium onward, with smooth narration mixing the linguistic and archaeological data the book was enthusiastically received by the general public and professionals outside of the field of Indo-European linguistics and Eurasian archaeology. Western archaeologists with knowledge of the subject (Kohl 2009; Kaiser 2010) in their reviews were more skeptical about the existence of the Bronze-Age riders in the Eneolithic steppes – the keystone in D. Anthony’s reconstruction, the protagonists of the book, the people to first domesticate and ride horses, to acquire wagons to “manage larger herds from mobile homes” (p. 303) and spread Indo-European languages all

over the Old World from its homeland located between the western steppes and the Ural.

But how much evidence exists for horseback riding in the 5th-4th millennia BC? None. Anthony himself tells a brief story concerning evidence disproving Eneolithic horseback riding: horse bones from Dereivka, Ukraine, initially thought to be from domesticated horse belonged to the wild species; a horse tooth with traces of bit wear, also from Dereivka, a star witness for horseback riding, happened to be from a pit of the Scythian time (pp. 214-215). Disappointed but not giving up on his idea, the story, with slight modifications, began to repeat itself: a significant amount of horse bones found at Botai and several other Eneolithic settlements were attributed by some zoologists to domesticated horses; wear facets on a few horse teeth were called traces from some “soft” bit of rope or leather (p. 218); and in the end, the conclusion that “horses were bitted and ridden in northern Kazakhstan beginning about 3700-3500 BCE” (p. 220), despite the contradictory results reported by zoologists, and uncertainty with a nature of facets on horse teeth (Kosintsev 2010, 58).

However, the origin of horseback riding in northern Kazakhstan did not fit D. Anthony’s theory because the riding has to be associated with domesticated cattle and sheep, but the finds of the bones of these animals are either scarce or completely absent in the sites of this region. And the conclusion follows: “It is likely that Botai-Tersek people acquired the idea of domesticated animal management from their western neighbors, who had been managing domesticated cattle and sheep, and probably horses, for a thousand years before 3700-3500 BCE” (p. 221). “Probably horses” quite quickly, at full gallop of the author’s creativity, were transformed in the following paragraphs into statements about the origin of horseback riding in the Pontic-Caspian steppes identified as the Proto-Indo-European homeland. To show the

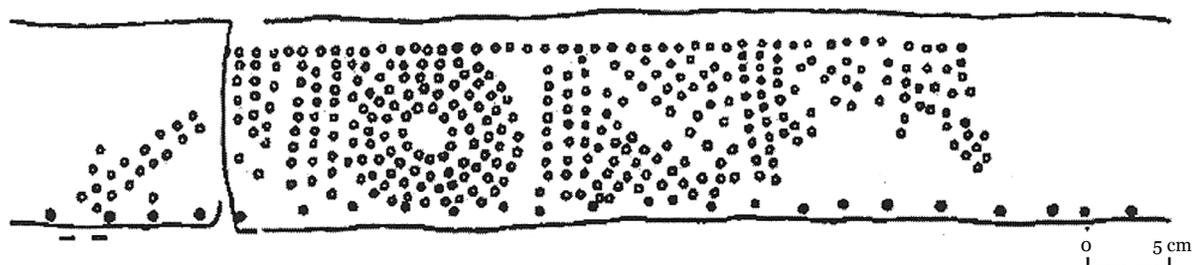


Fig. 1. Décor on a bronze vessel from grave Evdyk 8/23 (after Anthony 2007, fig. 4/3,a).

role the invented horseback riders played in the spread of Indo-European languages, with wagons appeared in the steppes from an unknown territory, D. Anthony overviews archaeological evidence.

Bringing together archaeology and linguistics for resolving the Indo-European problem is not new. What is new in this book is the scale of errors made by the author in his presentation of archaeological evidence. A detailed, critical analysis of the texts would require no less writing than the original volume. Hence, I will only touch on a few subjects.

In need of proof for the early existence of two-wheeled vehicles in the steppes (in order to connect them to the Late-Bronze chariots), for which so far there is no evidence, D. Anthony points to a bronze cauldron from a grave of the Novosvobodnaya culture, Evdyk 8/23 in Kalmykia. The elements of a partially preserved frieze decorating the vessel are the fields of concentric circles and crosses divided by three vertical lines, all made in repoussé technique – see Figure 1. However, D. Anthony suggests that the frieze “could represent, from the left, a yoke, cart, wheel, X-braced floor, and animal head” (figure 4/3a, legend). This interpretation expressed merely as an assumption on p. 68 and 69, on p. 297 turns to a statement: “The image of the cauldron suggests that the people who raised the kurgan at Evdik (the correct spelling is Evdyk – *EI*) also drove carts,” and again on p.496, “The image on the Novosvobodnaya cauldron at Evdik looks like a cart”. Of course, it is hard to argue with someone about his personal perception of an image. However the elements of décor, concentric circles and crosses made in the same repoussé technique are typical on bronze plaques found in the graves of the Yamnaya culture that chronologically follows but partially coincides with the Novosvobodnaya

culture. A pair of plaques usually comprises a set with hammer-headed pins made out of bone or horn; the two plaques can bear either identical décor, only circles or crosses, or one is decorated with circles, and another one – with cross-like figure. And the nature of this heritage would be a more interesting topic to pursue rather than looking for outlines of elusive images.

Even if we take D. Anthony’s interpretation seriously and discuss whether the images could depict a cart, we should assume that he is talking about a disassembled cart. The X-shaped impressions are often the only indicator of a wagon, when its body but no wheels preserved. However, it is not a part of the wagon’s floor. The body of a wagon was made with two parallel frames joined with wooden posts of 10-12 cm length; the X-shaped brace reinforced the upper frame in the middle, while the whole floor was covered with reed mats. Why would a prehistoric craftsman depict a constructive detail that is visible only to archaeologists during the excavations of the poorly preserved wagon? Moreover, the described construction is a characteristic for wagons of the Early Bronze Age period only in the Kuban River region, not where the bronze cauldron was found.

Resuming his arguments in favor of the existence of two-wheeled carts in the *Notes*¹ of the book D. Anthony writes: “But many graves contain just two wheels, including Bal’ki kurgan, grave 57. The image on the Novosvobodnaya cauldron at Evdik looks like a cart. Ceramic cart models associated with the Catacomb culture (2800-2200 BCE) and

¹ The way the author provides additional information in the *Notes* section does not support his arguments in the main text, but forces the reader to go through a long list of cited publications, often obscure, which the reader must verify on his own. This makes it very difficult to check the accuracy of the statements in the main text. Readers, unfamiliar with the subject, are left with no choice but to “blindly” believe the author.

in the North Caucasus at the Badaani site of the ETC or Kura-Araxes culture (3500-2500 BCE) are interpreted by Izbitser as portraying something other than vehicles. Gei, on the other hand, sees evidence for both carts and wagons, as do I. See Геї 2000, 186.” (p. 496).

The image that “looks like a cart” on the cauldron we already discussed. The drawing of grave 57 of Balki (not Bal’ki – *EI*) kurgan indeed shows two wheels at the pit’s corners. But the description of the grave mentions remains of wooden vessel with diameter of 0,25 cm near the third corner of the pit. While wooden vessels were usually placed near the deceased, and not in the area around the pit, the size of the remains, 0,25 cm, is typical for the base of a nave made in the central plank of the wheel. At the time of the excavations, in 1973, not many wagons were found, and even less were published, and the remains of the wheel were treated as a wooden vessel. Thus, undoubtedly a wagon with four wheels was deposited in grave 57 of the Balki kurgan, three of which, preserved in different degree, were unearthed, and the fourth one was not preserve at all. Similarly, only two wheels placed at two corners of the grave’s pit were discovered in Placidol, Bulgaria, but for some reason D. Anthony does not question the reconstruction of two other, non-preserved, wheels at two other corners (figure 14/6).

The statement “many graves contain just two wheels” is simply incorrect. It disregards the fact that wood is an organic material whose preservation or disappearance over time depends on various factors. The number of wheels found in graves ranges from one to eight, from one or two disassembled wagons. But all wagons placed in the graves in the assembled state had four wheels.

The last argument, in which D. Anthony points to “ceramic cart models associated with the Catacomb culture” combined with a reference to A. Gei’s opinion, and a reference to my view on them, including the one from Baadani, is a tight knot of confusion. Firstly, there are no cart models of the Catacomb culture. There are several distinctively different types of clay objects found in the graves of the Middle Bronze Age that traditionally, for different reasons, are considered to be the models of wheeled vehicles. As a two-wheeled vehicle the reconstructed was only one object of an unknown prototype found in a pit of Tri Brata kurgan 8 in

Kalmykia, the kurgan famous for the grave with remains of real wagons, the first excavated wagons in the steppes. The pit and the grave were initially thought to be contemporary and it was the only bases for the assumed reconstruction. It was suggested that six wheels preserved in the grave belonged to three two-wheeled vehicles; with addition of two wheels, axles, draught-pole, and yoke the object was turned into a cart. Since its first publication in 1948 (Синицын 1948), this artificial, out of proportion reconstruction has been treated as a cart model. But A. Gei, whose book deals with materials from the Kuban River region, does not discuss this type of model in his book, and despite D. Anthony assurance (“Aleksandr Gei counted 257 Yamnaya and Catacomb-culture wagon and cart burials in the Pontic-Caspian steppes” – p. 312), does not mention carts at all. Secondly, the models A. Gei discusses in his book on p. 186 (Геї 2000) he explains as not wagon models but as a model of a superstructure that was mounted on the wagon’s floor. This type of house-shaped model was first discovered by N.I. Veselovsky in 1909 and called “vehicle model”, using the Russian word that can be understood as either a wheeled or sledge vehicle. In time, the sledges with similarly-shaped structure disappeared from everyday life, and models of this type were considered only as wagon models. Since there is no way to create even an artificial reconstruction of any of these house-shaped models as a wheeled vehicle, archaeologists compared the rich décor of the objects with some construction details of real wagons, but still they are discussing wagons with four wheels, not two-wheeled carts. And lastly, studying the objects associated with wagon/cart models, I realized that grounds for such association are very elusive. When I first stated that none of these models is a wagon model (Избицер 1987), the idea was greatly opposed, though even earlier some archaeologists called the objects as “wagon models” only for the sake of convenience. Now, thanks to recent discoveries, models previously described as models of uncovered wagons have been identified as cradle models (Kalmykov 2007); the house-shaped type is also under re-consideration. A model from Badaani located in Transcaucasia (not in the North Caucasus) I have never discussed: it has no connection to the steppe objects, belongs to a different chronological period and different archaeological culture. The question arises: how is

it possible to agree or disagree with opinions that have not been expressed?

These kinds of mistakes in descriptions and explanations provided by D. Anthony clearly show that the author, at most, only browsed through the publications but did not understand their contents correctly. And his reference to A. Gei's book is especially revealing. The book is devoted to the Novotitarovskaya culture, for which the wagons constitute a typical feature of the burial rite. Though the culture shares a number of similar features in burial rite with the Yamnaya culture, these two are different archaeological cultures. The specific characteristics of the Novotitarovskaya culture, distinctive and independent from the Yamnaya culture, were realized from the very first discoveries of its graves in the 1970s. Although the origin of the Novotitarovskaya culture is still under debate, no one, and A. Gei in particular, ever considered the culture to be a variant of the Yamnaya culture. The main goal of his book was to demonstrate the specifics of the Novotitarovskaya culture. However, D. Anthony, referencing A. Gei, describes it as "a local Kuban-region EBA (i.e. Early Bronze Age) culture that developed from early Yamnaya" (p. 312). Perhaps, D. Anthony overestimated his understanding of the Russian language. His knowledge of the language displayed in the book is flawed – there is no single Russian title in the References without grammatical or transliteration error. Other times it is amusing, like when he follows the senseless habit of translating proper names and gives the translation of Kislovodsk as "Sweet Water" (p. 285), *kislyi* in Russian means "sour".

D. Anthony's work with secondary sources in lieu of originals is evident. For example, a model from Budakalász is confused with a model from Szigetszenmárton (figure 4/3d), obviously after erroneous reproduction in a popular publication; a reference to an article by Kondrashov and Rezepkin is given as Rezepkin and Kondrashov (p. 535), taken probably from a bibliography listed in a Russian publication whose careless author gave it from memory.

The author's interpretation of a number of artifacts, though entertaining, is rather strange, and shows that illustrations, not texts, were the grounds for his conclusions.

In section "Steppe Symbols of Power: Polished Stone Maces" (p. 234) he discusses mace-heads.

"A mace, unlike an axe, cannot really be used for anything except cracking heads." The author defines two kinds of maces – zoomorphic and eared types. However, figure 11/5 illustrated this section has only two mace-heads in it, and the rest of images are zoomorphic scepters. Here D. Anthony not only confused two different categories of objects but also made up a new type of mace-head – the eared type, probably to approximate it to zoomorphic examples, some of which are considered to portray horses (though only a few of them distantly resemble equids). Does he assume a pair of animals or, maybe a four-eared one? In another place, a mace-head has been confused with an axe of the Krivoluchie type (figure 9/7, legend) known from the 1930s. The cause of such confusion is simple – in numerous articles and monographs discussing the Eneolithic scepters, mace-heads, and axes these objects are often shown in the same tables. Without changes, the illustrations from the book were repeated in the author's recent publication (Anthony, Brown 2011).

Only looking at drawings, without the knowledge of a real situation, it is possible to say that "the best-preserved wagon graves in the steppes are in the Kuban River region" (p. 71, figure 4/5, legend). On the contrary, wood is badly preserved in the region's soils. The most detailed information on the construction of wagons came from this region, but only because the hollow spaces left by perished wood were filled with plaster for getting the cast.

Unsupported, wrong statements are scattered all over the book and mislead the reader, as, for example, "Stone anthropomorphic stelae were erected over hundreds of Yamnaya graves between the Ingul and the South Bug valleys" (p. 365), when stelae were not erected over the graves but used as construction materials. And here I would like to include comments on section "The Stone Stelae of the North Pontic Steppes" (p. 339) sent to me by A.M. Smirnov.

"Less than a page devoted to anthropomorphic sculpture of the North Pontic region. But comments to it occupy more space, since practically each sentence here surprises the specialist with the author's inaccuracy in presenting information. To begin with, Anthony says, 'Kemi-Oba grave cists were lined with flat-shaped stones, some painted in geometric designs, a custom

shared with Novosvobodnaya royal graves (e.g., the Tsar kurgan at Nalchik). But there is no kurgan under this name, and we know nothing about the institute of the royal power in the Novosvobodnaya culture. There are rich tombs of this culture but no one has ever called them royal!

Further, 'Kemi-Oba graves also contained large, stone funeral stelae, many with human heads carved at the top and arms, hands, belts, tunics, weapons, crooks, sandals...' Absolutely all these stelae were found accidentally, not in graves. Neither the connection of stelae with the Kemi-Oba culture, nor their funeral meaning is established.

The weird one is Anthony's interpretation of images that usually called "foot-prints" or "feet" as sandals. Repeated, perhaps, after J. Mallory's suggestion, Anthony makes it a fact: 'The most common clothing element carved or painted on the stelae was a belt, often with an axe or a pair of sandals attached to it.' (see p. 365). Structurally, by their location on stelae, these figures match numerous representations of the bottom parts of legs – where toes are depicted as well, on the statue-menhirs from southern France, which are often compared with the North Pontic stelae (e.g. Смирнов 2004).

Continuing, Anthony states that stelae from the Crimean peninsula spread to the Caucasus and the western Pontic steppes. This unfounded statement, perhaps should explain the next sentence, when Anthony mentions the finds of three hundred stelae in the graves of the Yamnaya and the Catacomb cultures 're-used as grave-pit covers, with more than half concentrated between the South Bug and Ingul rivers'. I can only wonder, what connection the Crimean stelae have to the series of stelae from the area of interfluvium between these two rivers? It was back in the 1970s, when D.Ya. Telegin divided the steppe stelae into the Eneolithic group – and exactly stelae from this group are decorated with complex carved images, and the Bronze Age group that is characterized with coarse triangle-shaped contours, practically without thorough finishing of the surface, and extremely rarely decoration. Only the stelae of the second group were used as pit covers in the regions between the South Bug and Ingul rivers, but these two groups are so different that nothing definite can be said about their genetic connection. Calling these stelae funereal the author contradicts himself, since he called them 're-

used' in the previous sentence. How can they be funereal?

Still without indicating a source, Anthony declares that 'the carving of funeral stelae seems to have expanded in frequency and elaboration in the Crimean and Pontic steppes after about 3300 BCE'. Where did he get this date from? Overall, only about ten carved stelae are now known, and they are still little studied, including techniques of stones' processing and imaginary; besides that, they are very poorly published which complicates their analysis (it should be noted, there is an incorrect drawing on the back side of a stele from Morel, France, (figure 13/11), which was blindly copied from the book by Telegin and Mallory).

A quite extravagant hypothesis Anthony offers on the purpose of stelae, guessing that 'Perhaps, they marked the future site of a kurgan cemetery before the first kurgan was built, or maybe they marked the first kurgan until the second one was built. In any case, they are usually found re-used as stone covers over grave pits, sealed beneath kurgans'. Here, the author at first defines these stelae as funereal by the place of finding in graves, and afterwards connects them with kurgans causally. The stelae were re-used, not made for burials, thus calling them "funereal" and functionally connecting them with burial rites and kurgan graves means misleading the reader. And again, Anthony mixes two groups of stelae: only the Bronze Age stelae were used for the graves; the Eneolithic carved stelae from Crimea were found outside of the funeral context.

The last paragraph says, 'Eerily similar stelae, with carved heads, bent arms, hands, weapons... were carved in northern Tuscany and the Italian piedmont at about the same time'. These stelae (the author probably talks about Lunigiana), however, with very specific semispherical shape of head and U-shaped face, are of different type that is absolutely unknown in the North Pontic steppes. The main attribute of the male statues of this category are daggers, while Crimean stelae do not have images of this kind of weapon. Besides the fact that stelae from both regions are anthropomorphic they have nothing in common.

Also incorrect is the statement that '...similar-looking stela was built into a stone building in Troy I'. This fragment does not have parallels among the North Pontic stelae but is close to the

series of statues-menhirs of southern France, and Blegen wrote about it in 1950 in *Troy*. But the author concludes in the next sentence, that 'It is difficult to imagine that these widely separated and strikingly similar and contemporaneous funeral stelae were unconnected'. Striking is the author's affection for the funeral purpose. These stelae were not funereal; I would like to point out that the quite large series of statues-menhirs from southern France came from remote areas located at the distance from both contemporary sites and burial grounds" (Смирнов 2011).

No less of a mix-up is in D. Anthony's representation of wheeled transport. In his opinion, the Proto-Indo-European speakers acquired wagons "between about 3500 and 3300 BCE, possibly from the west through Europe, or possibly through the late Maikop-Novosvobodnaya culture, from Mesopotamia" (p. 317), but "talked about wagons and wheels using their own words, created from Indo-European roots" (p. 74). As probable speakers of Proto-Indo-European he named herders of the Yamnaya horizon who spread Indo-European languages across the Pontic-Caspian steppes: "Since we cannot really say where the wheel-and-axle principle was invented, we do not know from which direction it first entered the steppes. But it had the greatest effect in the Don-Volga-Ural steppes, the eastern part of the early Proto-Indo-European world, and the Yamnaya horizon had its oldest roots there" (p. 317). Following this thought, we should accept, that from either direction wagons had pulled thru the steppes to get to the eastern part of the Yamnaya horizon in order to start its expansion back, to be named, and used by "herders operating out of a wagon" (p. 301). Surprisingly, the area defined as a place of initial distribution of the Yamnaya culture does not contain any find of a wagon. Several Yamnaya graves from the Ural region and one from the Lower Don region accompanied by wagons belong to the late, not to the early stage of the Yamnaya culture. The most significant number of early graves with wagons is known from the contemporary but distinctively different Yamnaya and Novotitarovskaya cultures of the Kuban River region. Perhaps, to fit his horseback riders-wagons-language scheme, D. Anthony decided to describe the Novotitarovskaya culture as the culture "developed from early Yamnaya" (p. 312).

General descriptions of burial rite with wagons and vehicles themselves are also inaccurate. The

author suggested that the wheels of disassembled wagons "were placed at the corners of the grave pit, as if the grave itself represented the wagon" (p. 312). It is true that in the graves of the Yamnaya culture the wheels of the dismantled wheels were placed at the pit's corners, but the rest of the wagon was placed on the pit's wooden covering, and often fell or sagged into the pit. It is more correct to say that the wheels were placed at the corners of the wagon's body, and such arrangement of disassembled wagons in the graves of the Novotitarovskaya culture confirms this. Unlike the bearers of the Yamnaya culture, the bearers of the Novotitarovskaya culture placed the wagon on the area around the pit, near one of its top border.

The author contradicts himself when he writes that with fixed axles and revolving wheels "most steppe wheels... were made of two or three planks cut into circular segments and then doweled together with mortice-and-tenon joints. In the center were long tapered naves (hubs)" (p. 70). How is it possible for a wheel made out of two segments to have the nave in the center and revolve on the axle? There are only two finds of two-segment wheels; the two segments were joined for their ritual deposition.

Proclaiming that "the wagon vocabulary is a key to resolving the debate about the place and time of the Proto-Indo-European homeland" (p. 75), D. Anthony gives a list of five roots from Proto-Indo-European vocabulary that linguists usually connect to the wagon (p. 35-36). The three words meaning "axle", "harness pole", and "to convey or go in a vehicle" do not have a strong association with a wheeled vehicle. Today, we cannot be sure what meaning N.I. Veselovsky applied to a word less than a hundred years ago, the word that is still in use but can describe two different types of vehicles, on sledge runners and on wheels; and we cannot know the exact meaning of words used over the millennia of history. For two roots for the wheel D. Anthony suggests the existence of two kinds of wheels (p. 63). To what extent was it necessary to distinguish one type of wheel from another in order for each to be called differently by people who spoke the same language?

It is not an easy task to write a synopsis on Eurasian archaeology from the Neolithic to the Late Bronze Age professionally. It is impossible with only a cursory knowledge of materials and the primary languages of publications, even with op-

portunities of opened barriers. With this level of knowledge the author attempted to solve the Indo-European problem. Maybe the linguistic component of the book is more reliable.

The Iron Curtain has been lifted, however, the facts are left on one side, and their unscholarly treatment presented on the other. The book is a mix of old ideas, archaeological data (often with

incorrect description of the finds), an improper methodological approach, and, therefore, highly speculative interpretations regarding the cultural history of the steppes. Written from the position “I see it in this way,” the book is useless for Western archaeologists and misleading for students. The popular audience should treat it as science fiction at best.

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„Cortina de fier” și arheologia Eurasiei

Considerații cu privire la lucrarea lui David Anthony “The Horse, the Wheel, and Language: how the Bronze-Age Riders Shaped the Modern World” Princeton, NJ: Princeton University Press, 2007.

Rezumat

În lucrarea sa D. Anthony pretinde rezolvarea problemei patriei și răspândirii indo-europenilor. Una dintre cauzele care au influențat pozitiv soluționarea acestei probleme este căderea „cortinei de fier” din secolul trecut. După acest eveniment savanții din Vest au căpătat accesul la materialele și publicațiile din țările fostei Uniuni Sovietice. Pe parcursul a câtorva decenii D. Anthony demonstra în lucrările sale că, deja în mileniile V-IV a. Chr. în stepele Eurasiei este cunoscut călăritul. Drept argument serveau urmele de uzură pe dintele unui cal, descoperit în așezarea eneolitică de la Darievka, Ucraina. După ce s-a stabilit că dintele în cauză provine dintr-o groapă mai târzie (perioada scitică), D. Anthony a recunoscut greșeala, însă, nu s-a dezis de la ideea unor reconstituiri de amploare, legând călăritul și apariția carului cu roți de origine indo-europenilor în stepele euroasiatice. În același timp nu există date despre existența călăritului în epoca eneolitică, după cum nu este clară data și locul domesticirii calului, subiect amplu discutat de specialiști. Căderea „cortinei de fier”, însă, nu poate înlocui metodele și tehnicile cercetării științifice și, de asemenea, cunoașterea materialului. Dispunând de cunoștințe superficiale în domeniul arheologiei euroasiatice, D. Anthony comite în lucrarea sa mai multe greșeli și inexactități, reflectate în articolul de față.

Lista ilustrațiilor:

Fig. 1. Ornamentul vasului din înmormântarea Evdyk 8/23 (după Anthony 2007, fig. 4/3,a).

«Железный занавес» и археология Евразии

Заметки по поводу книги Дэвида Энтони “The Horse, the Wheel, and Language: how the Bronze-Age Riders Shaped the Modern World” Princeton, NJ: Princeton University Press, 2007.

Резюме

В своей книге Д. Энтони претендует на решение проблемы прародин и расселения индоевропейцев. Устранение «железного занавеса» прошлого века он называет одной из причин, оказавшей положительное влияние на решение данной проблемы, когда у западных ученых появился доступ к новым материалам и публикациям в странах бывшего Советского Союза. На протяжении нескольких десятилетий Д. Энтони доказывал в своих работах, что уже в 5-4 тыс. до н.э. в степях Евразии существовала верховая езда. Доказательством служили следы потертости на зубе лошади, найденном на поселении энеолитического времени Дериевка, Украина. Когда оказалось, что зуб происходит из ямы более позднего, скифского времени, Энтони признал ошибку, но не отказался от идеи крупномасштабных реконструкций, связав существование верховой езды и появление колесных повозок с происхождением индоевропейцев в евразийской степи. Однако, данных о существовании верховой езды в эпоху энеолита нет, и даже вопрос о времени и месте доместикации лошади по-прежнему является предметом оживленных споров среди специалистов. Устранение «железного занавеса» не может заменить научные методы исследования и знание материала. Обладая поверхностными знаниями в области евразийской археологии, Д. Энтони в своей книге допускает много фактических ошибок и неточностей, что и показано в данной статье на конкретных примерах.

Список иллюстраций:

Рис. 1. Развертка орнамента на сосуде из погребения Эвдык 8/23 (по Anthony 2007, figure 4/3,a).

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