

**Social Evolution in the Mirror
of a Journal: To the Twentieth Anniver-
sary of the ‘Social Evolution & History’**

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ABSTRACT

The paper presents a review of articles published in the ‘Social Evolution & History’ journal since the 2002 until present. Over the past twenty years, 375 articles have been published in the Journal. The subject matter of publications is fairly comprehensive. However, the Journal obviously focuses on the key problems of social evolution in modern science. A considerable part of the Journal's articles are devoted to the origin of the state and the early state (24 per cent of all published articles). The central point is the discussion of Robert Carneiro's concept of the so-called ‘circumscription theory’. Considering this discussion, the author comes to the conclusion that Carneiro's theory has completely ‘passed the test’. In fact, the opponents' critical remarks may well be integrated into Carneiro's theory and this may only contribute to its improvement and elaboration.

Keywords: *social evolution, history, early state, Robert Carneiro's theory, discussion.*

The year 2022 marked the twentieth anniversary of the ‘Social Evolution & History’ journal. So there is a nice reason to sum up some results and evaluate the path of the journal over the past two decades.

One may argue that the achievements are very impressive. The journal has obviously become a significant phenomenon in the world science. Over the past twenty years, 375 articles have been published

in the Journal while among the authors, one may find many world-renowned experts in the field of the social evolution theory. Undoubtedly, the main merit here belongs to the inspirers and founders of the journal (among whom I would especially single out Leonid Grinin and Andrey Korotayev), who managed to gather all these great scholars into a single and very productive scientific community.

On the one hand, the subject matter of publications is very wide. On the other hand, the Journal obviously focuses on the key problems of social evolution in modern science. I think, even just the titles of the articles may be rather illustrative here. Let me list only some of them: 'Alternative Pathways of Social Evolution', 'Was the Chiefdom a Congelation of Ideas?', 'Was the State Inevitable?', 'Democracy and Early State', 'Bands, Fertility and the Social Organization of Early Humans', 'Population Dynamics and Internal Warfare: A Reconsideration', 'Beyond States and Empires: Chiefdoms and Informal Politics', 'Religion and the Revised Circumscription Theory', 'The Origin of Polities: A Preliminary Inquiry into the Evolution of Command and Control in Human Collectivities', 'Model of Demographic Cycles in a Traditional Society: the Case of Ancient China', 'Ritual and Rationality: Religious Roots of the Bureaucratic State in Ancient China', 'Modeling Malthusian Dynamics in Pre-industrial Societies' 'Manipulations of the Corpus in the Context of Life Cycle Rites among the Datoga Cattle Breeders of Northern Tanzania', 'The Cybernetic Revolution and Historical Process', 'The Average Word Length Dynamics as an Indicator of Cultural Changes in Society', 'Macroeconomic Evolution: The Multipolarity of the Process and Quantitative Estimation Models', 'Reconsidering the Issue of Eastern Migrations in Connection with the Artificial Cranial Deformation Practices among the Late Sarmatians', 'Evolution of Eurasian and African Family Systems, Cross-Cultural Research, Comparative Linguistics, and Deep History', 'Mathematical Model of Interaction between Civilization Center and Tribal Periphery: A Description', 'Aromorphoses in Biological and Social Evolution: Some General Rules for Biological and Social Forms of Macroevolution', 'Woman in Ancient Egypt: Evolution of Personal and Social Positions (by Didactic Texts)', 'Vavilov Centers or Vavilov Cultures? Evidence for the Law of Homologous Series in World System Evolution', and '2d:4d, Big Fives and Aggression in Young Men of Caucasian, Ural and Asian Origin'.

Within the framework of the present review article, it is impossible to consider all the published articles or even just mention each of

them. However, I do not see the point in doing this since I do not set myself such a task here. Moreover, in the age of the Internet, an interested reader can visit the journal homepage (www.sociostudies.org/journal/seh/) and use the available index of articles to find any article needed.

Therefore, not to turn into a simple telegraphic retelling of just a part of the works published in the journal, we define the objective of the present review as a reflection and commentary on some of the most important and interesting issues raised by the authors of publications.

And first of all, of course, it is worth performing a certain generalization of the considered set of articles.

So, if one decides to group all the articles published in *Social Evolution & History* according to the topic, it becomes clear that they mainly focus on several issues which will be presented below. In particular, a number of articles are devoted to social evolution in general, parallels between biological and social evolution, to industrial revolutions; discussion of the creative heritage of Ernest Gellner (in 2003, an issue of the journal was devoted to this topic); the problem of periodization of history; big history, mathematical modeling of population dynamics and socio-demographic cycles; evolution of globalization.

I would also point out the subject of the formation of World Systems is covered in a significant number of interesting articles. And here the ideas of Andrey Korotayev and Leonid Grinin, who proposed the emergence of the first World System during the Neolithic revolution in the Middle East, are of particular interest (Zinkina, Ilyin, and Korotayev 2017: 70; Korotayev, Grinin L., and Grinin A. 2021, 2022). According to Korotayev and Grinin, the formation of the first World System (in the Middle East and also in a significant part of Eurasia) should be attributed to the end of the Upper Paleolithic-Mesolithic. The physical substratum of this first World-System was the migration of speakers of Dene-Caucasian languages from Eastern Eurasia to the west (Romanchuk 2009a; 2009b; 2012, 2013a; 2015; 2019a, 2019c; 2020b), and native speakers of the Altaic languages from Western Asia to the eastern part of Eurasia.¹

Articles devoted to the study of particular societies (from antiquity and the Middle Ages up to the present) occupy a prominent place in the Journal. Considerable attention is paid to the societies of Africa. There is also a noticeable number of articles examining the societies of ancient Mesoamerica and China, Polynesia, ancient and especially medieval nomads of Central Asia (here I would particularly point out the articles by Nikolay N. Kradin [*e.g.*, Kradin 2019] and Anatoly M. Khazanov

[Khazanov 2010]), ancient societies of the Middle East, Egypt and India, and also medieval Europe (including the Old Russian state).²

However, a considerable part of the Journal's articles are devoted to the origin of the state and the early state: one may count at least eighty articles (*i.e.*, 24 per cent of all published articles). Yet, we should note that one way or another, the problem of the emergence of the state is also covered by a number of other articles.

However, on the other hand, some of the articles addressing the early state issue *de facto* consider it on a much broader scale, that is as a phenomenon of politogenesis, albeit extremely significant. As Dmitri Bondarenko, Leonid Grinin and Andrey Korotayev emphasized,

... among the students of politogenesis one can observe a tendency to narrow the analysis to the study of the state formation process only. ... In order to find solutions for a certain range of political anthropology problems it is necessary to consider the genesis of early state in the general context of socioevolutionary processes coeval with it (Bondarenko, Grinin, and Korotayev 2002: 67–68; see also Grinin 2003: 132).

Or, moreover, as a part of social evolution in general; starting from the emergence of the early state, they eventually answer questions about the general laws of social evolution. Therefore, in some cases it would be difficult to unambiguously attribute this or that article within the framework of the proposed classification.

Anyway, if we talk about eighty articles belonging to this group, most of them present theoretical studies with a comprehensive coverage of considered data and with broad generalizations. And in fact, their central point is the discussion of Robert Carneiro's concept of the so-called 'circumscription theory' ('This theory came to be known as the *circumscription theory* since it pointed to the key role played by tight environmental constriction in giving rise to population pressure, which in turn had brought about recurring warfare, culminating, in certain areas, in the rise of the state' [Carneiro 2012a: 10]) in which both supporters and opponents of his proposed model of the emergence of the first states participated (I think, Henri J. M. Claessen should be particularly distinguished here).

However, few of these eighty articles (as well as of many others – which I have assigned to other categories of my classification) represent narrow studies of individual societies; yet, they are eventually also involved in the discussion on the relevant reasons for the emergence of the first states proposed by Robert Carneiro.

Actually, this is both an indicator and a direct consequence of the

scientific importance that Carneiro's concept represents ('The theory of origin of the state by Robert Carneiro (1970), no doubt, is one of brightest achievements in the twentieth century macrosociology' [Rozov 2012: 86]). And, although, as some of the participants of the discussion rightly point out, '... Carneiro has many predecessors holding different political views, beginning with Gesiod' (Ganzha and Shinakov 2012: 49), it was Carneiro who managed to give this idea, which had been in the air for a long time, the form and meaning that made it the central point in the discussion of any issue in connection with the problems of emergence of the first states for more than half a century.

Nevertheless, we should note that almost simultaneously and throughout the past half-century some alternative positions to Carneiro's concept (sometimes sharply disputing to him as in 'Early State Formation: A Complete Rejection of the Circumscription Theory' [Stocker and Xiao 2019]) were expressed and argued. The authors of these alternatives are very authoritative scholars. However, Carneiro himself presented many of his opponents in great detail in an extensive, sixty-page article published in the journal in 'Answers to Critiques' (Carneiro 2012b; see also Carneiro 2012a).

Therefore, it seems the best choice to arrange this article as a comment on the margins of the discussion about Robert Carneiro's theory.

So, first of all, it seems to me that the assessment that Carneiro gave to the idea of 'multicausality' (as he designated it) is completely justified. The latter in its ultimate expression actually leads to the denial in general of the possibility of creating a general theory of the emergence of the state (*i.e.*, of *de facto* theory of social evolution):

The first interpretation of multicausality, let me repeat, holds that in *every case* of state formation a *different* set of factors was at work. Each state that arose – in Egypt, in China, in the Andes, or anywhere – had its own unique set of determinants. No single theory could hope to account for more than one or two instances of it (Carneiro 2012a: 6–7).

Carneiro argues that it is possible to single out a certain limited set of factors (four or five in number) that was common to all cases of the emergence of the first states:

... all states, regardless of how disparate they may have been in detail, had at least certain basic elements in common. They each arose through the combined action of the same small set of factors. The role of each factor need not

have been exactly the same in every case, but their joint operation was sufficient to give rise to the state, wherever it arose (Carneiro 2012a: 6).

And as far as the degree of my awareness in the matter allows me to judge, I believe that in this case it is Robert Carneiro who is closer to the truth.

Moreover, it seems that we should definitely take the next step in this direction. And, using the ideas and conclusions from synergetics, we should raise the question of the so-called ‘control parameter’ which acts as a Kapellmeister in the orchestra in the group of four or five factors that Carneiro speaks about.

However, Carneiro himself (like many of his followers) actually comes to defining such a ‘control parameter’. Summing up the results of the discussion on ‘the circumscription theory’ on the pages of the Journal (and emphasizing that the main points of this theory seems unchanged to him), Carneiro formulates the following definition:

The core of the theory, though, remains the same. It can be encapsulated in the following proposition: *A heightened incidence of conquest warfare, due largely to an increase in population pressure, gave rise to the formation of successively larger political units, with autonomous villages being followed by chiefdoms, the process culminating in certain areas with the emergence of the state* (Carneiro 2012a: 27).

Without going into a discussion here about the pivotal role of war³ in the emergence of the first states (while Carneiro's critics somehow challenge this thesis in a significant number of articles published in the Journal; yet, on the other hand, this idea has also found very convincing supporters [Turchin and Korotayev 2006; Grinin and Korotayev 2009]⁴), it seems appropriate here to focus precisely on the key factor defined by Carneiro. And this is just the ‘control parameter’ for the emergence of the first states (and actually of social evolution in general). Moreover, this is the ‘control parameter’, which retains its effectiveness throughout the whole existence of humankind. And it is the ‘population pressure’.

In his works, Robert Carneiro also spoke about ‘population pressure’. Elsewhere (starting with Romanchuk 2006) I introduced the parameter of ‘complication of the anthroposphere’. However, one should take into account that one of the key (if not the most important) component of the ‘complication of the anthroposphere’ is just the population growth (which, as I have also emphasized, is considered

primarily on a planetary scale (along with individual societies as well)). Thus, using the terms close to those employed by Carneiro, we should rather define this parameter as ‘complication pressure’.

Of course, in each particular case, the factor of ‘complication of the anthroposphere’ interacted with a different set of variables in one way or another; and obviously, this is the reason for the possible ‘Alternative Pathways of Social Evolution’ (Bondarenko, Grinin, and Korotayev 2002; Grinin 2003; Grinin, and Korotayev 2011). However, in my opinion, Carneiro is absolutely right that this whole range of possible options for ‘Answering the Call’ (Romanchuk 2006: 435) should not prevent us from ‘seeing the forest for the trees’.

However, the critics of Carneiro's concept strongly disagree with its key idea about the role of ‘population pressure’. Thus, Henri Claessen writes, ‘*From the comparisons it appeared that all cases developed in a situation of relative wealth. They were not a consequence of hunger or population pressure* (emphasis added – A. R.)’ (Claessen 2016: 3). However, it seems that in this respect the positions of Carneiro and of many his opponents are quite prone to convergence. Below I will try to prove this.

First of all, basing on the understanding of the ‘complexity of the anthroposphere’ as the desired ‘control parameter’, I should point out that some of Carneiro's ideas formulated in the framework of ‘the circumscription theory’ probably need clarification. In particular, his idea about ‘physical barriers’ as a crucial prerequisite for the emergence of the first states, ‘When population is growing in a region tightly hemmed in by physical barriers such as mountains, deserts, and oceans, the pressure exerted by this growing population is prevented from dissipating by escaping into surrounding regions’ (Carneiro 2012a: 13).

Note that Carneiro's opponents doubt this thesis as well:

Carneiro postulates that in a circumscribed area a shortage of food causes tensions between the inhabiting tribes (peoples, villages *etc.*) that inevitably lead to a war of survival after which the defeated groups have to leave the region, or accept a subordinated position. The organization to suppress the defeated is the (pristine) state. The findings in this article are hardly supportive for Carneiro's views. With the possible exception of Tongatapu **there was not found a region with circumscription** (emphasis added. – A. R.). Neither the river valleys, nor Oaxaca, nor China, nor the Grasslands of Ghana show traces of it. It is very well possible that after the emergence of the pristine state in one or

more of the regions, the population pressure increased, so that a struggle for survival might have occurred. This then, however, was not a cause of state formation, but a consequence (Claessen 2016: 43).

However, contrary to the skeptics ('Carneiro's Social Circumscription Theory: Necessary but not Sufficient' [Gibson 2012]), it seems to me that having proposed just two new types of 'circumscription' ('resource concentration' and 'Social Circumscription'), Carneiro brilliantly elaborated his theory. So in its revised form, the theory explains the examples cited by Claessen to the full extent.

Further, it seems important to take the next step here and to generalize the idea of 'physical barriers' to the level when 'mountains, deserts, and oceans' (as well as 'resource concentration') are considered only as particular cases of the basic key parameter of 'Social Circumscription'. And, respectively, one should generally assume that every human society faced the most important and significant 'physical barrier' (and its significance would only increase with time) – that is, the neighboring societies. The impact of these 'barriers' increased over time, as human societies grew in number and complexity, eventually turning into states.

'Mountains, deserts, and oceans' were a very significant barrier at the dawn of human history. However, one should not overestimate to what extent they are 'impenetrable' for potential migrants (about the so-called 'East Side story' see Romanchuk 2018a); and the stronger was the stimulus (*i.e.*, the scale of crisis) for a particular society, the more permeable were these barriers. But from a certain moment their relevance as 'physical barriers' became increasingly inferior to the significance of the primary factor of 'Social Circumscription' (including, figuratively speaking, the density of the 'geopolitical and domestic political environment').

As for the 'resource concentration', I repeat, it is actually only a particular (and most common) case among a number of 'physical barriers'. So one may point to the fact that the constraining factor here is the area within which a successful (or most effective) functioning of a certain ECT (Economic and Cultural Type) is possible.

At the same time, I think such an area should not be necessarily anything extraordinary in terms of abundance of resources. There may be no less (or even more) resource-rich areas near but they may be different and requiring different management skills and approaches. I am inclined to consider the history of reclaiming the prairies by

North American Indians as an example of this kind – the richest region in terms of resources, including inexhaustible food reserves (i.e., bison herds) was mastered by the prairie Indians rather late, and only as they were forced to leave their former habitat under the pressure of strong enemies.

Thus, it was often the habit of a certain economic mode (and, accordingly, an area suitable for it) that kept people in certain territory and in a relatively overpopulated region, and its influence was often much stronger than of some external constraints. And the more specialized this ECT was, the more expensive adaptation was required when moving to a new area and the stronger was the influence of the limiting factor. Moreover, one should keep in mind that farming in different areas may vary: thus, farming, for example, in the forest zone of Eastern Europe requires different skills, habits and knowledge than farming in the steppe zone of Eastern Europe.

There is also no doubt that the emphasis should be probably shifted to the term ‘habit’. ‘Habits’ were the most important reason why the adaptation to a certain ECT turned into a kind of ‘a physical barrier’ that limited migration. It was, first, of all the inertia of thinking and habits.

This conclusion can be exemplified by the attempts to relocate Russian peasants to the Far East, which were undertaken during the so-called ‘Stolypin reforms’. They came across just the most powerful inertia of the Russian peasant consciousness and habits, which prompted them either to refuse resettlement, or even to return back.

Finally, basing on the ethological observations over the behavior of species forming colonies, we should, apparently, consider, the ethological mechanisms that encourage individuals to concentrate in space as another significant component of the ‘Social Circumscription’ which provides them with advantages in interacting with other individuals of their species (for some considerations on this point see Romanchuk and Medvedeva 2009).

Having discussed these issues, we can now return to the above-quoted Claessen's remark concerning the origin of ‘pristine state’: ‘*From the comparisons it appeared that all cases developed in a situation of relative wealth. They were not a consequence of hunger or population pressure*’ (Claessen 2016: 3). Elsewhere, I presented my ideas on this issue (in particular, in Romanchuk 2006; 2008; Romanchuk, Medvedeva 2009; Romanchuk 2019b).

While the idea about ‘relative wealth’ is beyond doubt (and we will talk about it later), along with the fact that ‘They were not a consequence of hunger’, then the conclusion about the absence of ‘population

pressure' makes us pose the question: does this conclusion relate to societies which are known exclusively due to archaeological data?⁵

In any case, it is evident that even the assessment of the population size in such cases will be only approximate. And even more doubtful is our ability to establish the presence or absence of 'population pressure' in this case.

Moreover, we should remember that traditional societies had a number of traditional and, apparently, very effective mechanisms to control the population size (Romanchuk 2019b: 132) which were applied when the resource limit was approached (or rather, much before reaching this limit). And I think, these mechanisms normally kept traditional societies from slipping into the so-called 'Malthusian trap' (for general criticism of this idea see Romanchuk 2019b).

Thus, taking into account these restraining mechanisms, one may wonder to what extent the supposed 'overpopulation crises' represented sharp (and, accordingly, easily perceptible by an observer) 'population jumps' (similar to those characteristic of animal species using the so-called r-strategy of reproduction) (more about the meaning of this analogy with respect to human societies see in Romanchuk and Medvedeva 2009)? And to what extent did they present what I would rather define as more or less small but constant 'smooth demographic pressure' which would be even more problematic to define for the societies known only from archaeological data.

However, Claessen (and researchers who support him), while denying 'population pressure' for primary states, apparently proceeds exactly from the fact that 'They were not a consequence of hunger', but, on the contrary, were in a state of 'relative wealth'.

Well, for me it is also explicit (Romanchuk 2006: 424) that a vast majority (if not all) of archaic societies were able to obtain a surplus product, yet, they often did not use it. That is why I offer to use the concept of a *potential* surplus product (*Ibid.*) which, as far as I can tell, was characteristic of virtually any traditional society of the past.

Even the Kalahari Bushmen, the inhabitants of a very resource-poor region, periodically found themselves in a situation when they could not even get close to mastering the available food resources which literally rotted under their feet (Sahlins 1972: 19–52).

However, the fact that all archaic societies (or almost all) were doing quite well with food security, and they apparently did not experience *permanent* hunger at all (and I argue again that if needed they were quite capable of obtaining an additional product) does not mean that these societies did not perceive the pressure of population growth

(more precisely, of the ‘smooth demographic pressure’), and ‘pressure under the complication of the anthroposphere.’ At that, the population growth should have obviously influenced (and actually, did influence) not only food but also many other resources, including intangible ones (such as prestige and social capital in general) that people need.

In particular, population growth should have affected the provision of traditional societies with such a resource as space. And one may suppose (Romanchuk 2006: 427), that one of the factors of the transition to settlements (which, as anthropologists supposed back in the 1970s, is in turn a powerful factor accelerating population growth) was caused just by reduced opportunities for free movement of human collectives over long distances.

In other words, one should not reduce the concept of ‘Malthusian crises’ to excessive demographic pressure only on food (and material, in general) resources. Since it is obvious that a person feels the need not only for material resources, but also for non-material ones (like, once again, prestige and social capital in general). And, moreover, a reasonable question arises here: what kind of resources (material or non-material?) turn out to be the vulnerable aspect which comes first under ‘demographic pressure’?

So we can make an assumption (which I expressed earlier) that intangible resources may become just that vulnerable aspect.

Therefore, taking into account the above said, one may argue that the conclusion of the most radical critics (that ‘obviously, large population aggregates (characterizing a state by archaeologists) were only possible because they were in areas of unique abundant wild/renewable food resources ...’ [Stocker and Xiao 2019: 166]), regardless of its validity, is by no means a reason for rejecting Carneiro's theory.

On the contrary, this conclusion fully agrees with Carneiro's theory. And it should be interpreted in such a way that at a certain stage of humankind historical development, the factor of ‘population pressure’ (and, more broadly, of ‘complicating anthroposphere’) became especially sensitive on a planetary scale for societies that lived in regions with abundant (primarily food) resources and at the same time, possibly (but not necessarily) limited by some physical barriers (in full accordance with Carneiro's theory) which led to the fact that these societies perceived the pressure of ‘Social Circumscription’ earlier than others. And this, in turn, led them to the need to develop social mechanisms to compensate for the pressure of ‘Social Circumscription’ which inevitably leads to the emergence of the state.

Since the creation and maintenance of the state requires serious economic costs and expenditures, the societies that lived in less favor-

able regions would come to the emergence of the state when, under the influence of the ‘complicated anthroposphere’ (on a planetary scale), the costs of the absence of the state were outweighed by the efficiency of its creation and maintenance of functioning.

I believe that such an economic formula is the verbal model that expresses the essence of Carneiro's theory in the briefest form. And which can also be used to create an adequate mathematical model of the process of the emergence of states (including the early ones) and social evolution in general.

Thus, summing up, we can say that in my opinion, Carneiro's theory has completely passed the test of discussion. In fact, the opponents' critical remarks may well be integrated into it and this may only contribute to its improvement and refinement. And, concluding this comment on the discussion on Carneiro's theory and the article in general, I want to note that it was thanks to the journal ‘Social Evolution & History’ that such a significant clarification of the ideas about the causes, mechanisms and processes of the emergence of primary states (and social evolution in general) became possible thanks to this discussion.

NOTES

¹ Andrey V. Korotayev also investigates the early history of some of the main language macrofamilies of Eurasia, including in his extremely interesting articles published in ‘Social Evolution & History’ (Korotayev *et al.* 2019). This is, I think, an example of a broad interdisciplinary synthesis based on data from linguistics, genetics, archaeology and anthropology. I am really impressed by the approach used by the authors of the article. I believe that it can be applied to other cases. For example, to establish the actual role of wars in the societies speaking Afrasian, Nostratic and Dene-Caucasian languages in the early period of their history.

Also, I quite agree with many of the conclusions made in that article. In particular, the authors supported the idea of Alexander Y. Militarev about the localization of the ancestral homeland of the Afrasians in the area of the Natuthian culture (Korotayev *et al.* 2019: 287). One may also agree with the idea that ‘the recent genetic data do not contradict the “soft version” of the Anatolian Hypothesis’ (Korotayev *et al.* 2019: 300). However, my approach proceeds from different reasons and is based on a different model and scenario (as well as the locus) of the formation of the Indo-European linguistic community (Romanchuk 2020b: 254, note 22).

However, the ‘Steppe Hypothesis of the Origins of the Indo-European Languages’ (Korotayev *et al.* 2019: 300) seems unconvincing even if one proceeds from purely genetic data (Romanchuk 2020b). Even more so if one takes into account the respective archeological data.

I think also that attempts to connect the Proto-North Caucasian linguistic community with ‘various branches of NRY haplogroups G or J’ (Korotayev *et al.* 2019: 300) will fail. As I tried to demonstrate (Romanchuk and Semenov 2014; Romanchuk 2015, 2019a, 2019c, 2020b), the speakers of the Dene-Caucasian languages (including, respectively, the Proto-North Caucasian one), at the time of

their formation and up to the collapse of this community, were carriers of haplogroups R and Q of Y chromosome (and of the autosomal component ANE).

² The question of the origin of the Old Russian state is of particular interest to me, since it has long been the focus of my academic activities (in particular, see Romanchuk 2013b; 2017; 2018b; 2020a; 2022). The scope of this article, however, does not allow me to pay any extensive attention to this issue here.

Nevertheless, I would like to draw attention to one fundamentally important point. Namely, to the idea that '*Ancient Rus was created as a result of treaty between the local elite and part of the Varangians ...*' (Shinakov 2007: 121). And, accordingly, the emergence of the Old Russian state is associated with the control of the Scandinavians over trade routes from the Baltic Sea to the Caspian and the Black Sea: 'the international trade, which had been monopolized by the middle of the 9th century by the strange 'Varangians' ...' (*Ibid.*: 128).

In my opinion, this is obviously not the case. As follows from the analysis of the entire set of sources (including the information of the Old Russian chronicle itself), Ancient Russia arose thanks to the consensus among the *local* elites who *would invite* only *one* Varangian – Rurik with 'brothers' and the squad, of course. However, it does not mean 'a part of Varangians', moreover, that Rurik represented any center of power from Scandinavia. The agreement was not between the local (future Old Russian) elites and the Varangians – the agreement was between the local elites and Rurik as a compromise that satisfied everyone in local elites.

Besides, following the sources, we should not automatically equalize 'Varangians' with Scandinavians; and each case should be studied since the answer may be different.

Well, similar aberrations occur in many researchers of the problem. They begin by adopting the model of the *invitation* of the Varangians and the *agreement* with them. Then, their reasoning and interpretations incomprehensibly start to follow the idea of the Scandinavian *conquest* (and even colonization) of the Slavs. And this by no means corresponds to the data from the written sources, or to the data from archaeology, linguistics and physical anthropology (as well as from genetics).

³ Also in the discussion if chiefdom represented a stage in the emergence of states: '*Norman Yoffee* rejects the traditional (neo) evolutionist views and holds that pristine states did not develop from chiefdoms. Our findings confirm his views. With the exception of Tonga there are no clear cases in which chiefdoms were found before state formation took place. The case of Oaxaca is not clear' (Claessen 2016: 45).

⁴ However, some of these defenders seem to have changed their minds somewhat later: 'The general impression is that conquest warfare should be regarded not as a cause of chiefdom and state formation, but rather as one of its results' (Zinkina, Korotayev, and Andreev 2016: 192). Without going into the discussion again, we think that their earlier point of view still seems to be closer to the truth.

In addition, we must take into account such an important aspect of the problem as the socio-cultural relativity of the phenomenon that we call 'war'. What representatives of one society consider as a nice morning squabble between loving spouses, the representatives of the other society may, figuratively speaking, consider equivalent to a nuclear conflict of medium intensity (in this regard see data

on Kalahari Bushmen in Kazankov 2002). The fact that at least some of the reasons for such a difference in the behavior of representatives of different societies are quite clear today (*Ibid.*: 17, 86) and should not be regarded as a reason to doubt the existence of a causal relation between ‘population pressure’ and the growth of conflicts within (an important point! This is also a ‘war’) and between human societies. On the contrary, the idea that in extreme environment (with scarce and unpredictable food resources) the hunter-gatherers' behavior became flexibly territorial and mildly aggressive because it was absolutely necessary for survival (*Ibid.*: 17, 86) allows explaining why the correlation between ‘population pressure’ and ‘warfare’ is so difficult to grasp for human societies. At the same time, the conclusion (to which many researchers come) seems reasonable that the emergence of states became a new powerful factor for the growth of ‘warfare’ (yet, I believe, it was not the root cause).

⁵ The societies considered by Henri Claessen belong precisely to such type. I would like also to note that in the case of the first states in Egypt, the real course of events seems to me somewhat differs from the one from which Claessen proceeds from (see Romanchuk 2005, 2014).

Also, it seems doubtful that Ghana and Tonga may be attributed to the primary states as well (Claessen 2016: 25–33). It seems that Claessen (and other scholars) does not take into account such a parameter here, which I would define as ‘planetary historical time’ (Romanchuk 2006: 430). So one must take into account that societies that emerged relatively late in the regions separated (and distanced) from the primary centers of the emergence of statehood (and Ghana and Tonga belong here), in fact, were not radically cut off from planetary information flows. Thus, in their emergence, they could well rely on historical experience (which would push and stimulate them) of other societies that have already become states.

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