

Stefan Breuer: *Der charismatische Staat. Ursprünge und Frühformen staatlicher Herrschaft*. Darmstadt: Wissenschaftliche Buchgesellschaft, 2014, 319 pages

This is an important and immensely learned book. To the best of my knowledge, there has been no comparably detailed attempt to trace the emergence of early states in different regions of the Old and the New World. Drawing on a vast array of anthropological, archaeological and historical sources, Breuer analyzes the transition to statehood in Oceania (where the process was, as he argues, not completed), the Andean region, Mesoamerica, China, Mesopotamia, Egypt and the Aegean. The general stance of his analysis might be described as consistently sceptical about claims made on behalf of archaic political forms; he thus regards widely shared views on early evidence of state power in the Andes, the ancient Near East and the Bronze Age Aegean as misguided. Such doubts also explain the absence of the Indus civilization from his research programme. Its political framework was, as he suggests, probably a cluster of chiefdoms, characterized by a “nonstate type of complexity” (p. 36). In fact, there would be valid reasons to omit this case, even if the criteria of early statehood were to be relaxed: the evidence is, as many scholars in the field have agreed, more enigmatic than in the other relevant instances, and it does not translate into the kind of narrative that Breuer constructs for other regions and civilizations.

But if the merits of the empirical content are massive and obvious, the conceptual framework seems more directly questionable. Breuer takes his clue from Max Weber’s distinction between charismatic, traditional and legal-rational domination, and draws on recent clarifications grounded in more careful readings of the sources (to which he has been a major contributor). But apart from a comment to be noted below, late in the book and immaterial to his discussion of the early state, he does not suggest any revisions at the basic conceptual level. At this point, we may register a *prima facie* objection to the proposal set out in Breuer’s long introduction (pp. 9–37). The typology of domination is one of the most markedly unfinished parts of Weber’s work; it

went through many versions, and the last ones belong to the final phase of Weber’s career; many questions concerning the relationship between the three types and the contextual meaning of each one in particular remained unanswered. In a book dealing with early states, this unfinished conceptual scheme is applied to a field which Weber hardly touched (the main exception is Egypt, but as Breuer shows, his over-modernized views on this subject must now be corrected). It seems unlikely that this major extension can leave the frame of reference unaffected, all the more so when the latter is still in the making.

Not that Breuer regards the Weberian scheme as sufficient for his purposes. He wants to combine it with categories and models developed in recent decades by anthropologists and archaeologists. There is everything to be said for that kind of interdisciplinary contact, and not doubt that those who deal with prehistoric, stateless or archaic societies can still learn some lessons from classical sociology. But a mutually induced critical reflection might take us further than a mere combination of resources. To get a tentative idea of that option, we must first take a brief look at Breuer’s choices.

On the positive side, it is worth noting that Breuer is highly critical of the more faddish notions that still enjoy some popularity among archaeologists, especially the attempts to identify “world systems” in every historical stage and every geographical region; as he argues, they rely on systemic models that are either defined too rigorously to be applicable outside their original modern context, or too loosely to carry any specific meaning (the latter applies to André Gunder Frank’s macro-historical escapades). A holistic prejudice is also evident in neo-Marxist interpretations of the “mode of production” as an overarching structure; but here Breuer finds an opening to the kind of neo-Weberian analysis that he wants to develop. It is the “epigenetic civilizational theory” formulated by Jonathan Friedman and Michael Rowlands, and originally conceived as a way of bringing divergent paths and multilineal evolution into a structuralist-Marxist vision of history. One of its key themes is the control over “imaginary conditions of production”, seen as a possible

and in fact frequent but not necessary road to statehood. These “imaginary conditions” consist of beliefs, symbols and rituals; they should obviously be thematized in their own right, not just in relation to production, and such a turn would seem to link up with Weber’s comments on the ideas that chart the paths of human action. From there is a direct line of thought to the cultural worlds that crystallize around constellations of ideas. But this is not the road Breuer wants to take. His extensive and meticulous work on Weber has never confronted the question whether a comparative analysis of civilizations was emerging as the unifying goal of Weber’s project.

The approach chosen in the book under review is starkly opposed to civilizational analysis. Breuer collapses the worlds of beliefs, symbols and rituals into the Weberian category of charisma. Recent scholarship has undeniably shown that this notion is more ubiquitous and more significant in Weber’s work than mainstream interpretations tended to admit; but it has also highlighted Weber’s failure to define it in a clear and consistent way. If it is to be used in the systematic fashion envisaged by Breuer, a minimum of stabilizing content is required. Breuer’s solution to the problem is to describe charisma as a “trans-epochal phenomenon, linked to anthropological constants that are relatively resistant to the social and the natural environment” (pp. 18–19). This claim is not substantiated by anything more than a general reference to “biocultural” and cognitive-psychological foundations. But if that is where we are supposed to look, the first step would be to face the unending and multi-faceted controversy about the relative weight of natural and cultural factors in the making of human destinies. The dispute is at least as vigorous in anthropology as in any other discipline, and at least for those of us who tend to think that the defence of culture as human creation is a more convincing stance than any naturalist reductionism, the identification of charisma with an infra-cultural core is implausible. And Breuer does not move in that direction; instead, he returns to Weber and defines charisma in terms of non-everyday (*ausseralltäglich*) dimensions of social life.

We can, in that sense, speak of charismatic objects, symbols, experiences and activities. As often noted, Weber never clarified the relationship of this fundamentally transpersonal and primarily religious meaning to the emphatically personal and primarily political one that figures in his sociology of his domination; for Breuer’s argument, it is essential to shift the balance towards the former side, and he therefore criticizes Weber for conflating the routinization (*Veralltäglichung*) and objectivation (*Versachlichung*) of charisma with its de-personalization, understood as a step towards disappearance and displacement by traditional or legal-rational domination. For Breuer, the most decisive transformation of charisma is its institutionalization, and it includes – especially in the early stages discussed in the book – a personal component. “Kingship and the state” are thus to be explained as results of “an institutional turn of charisma” (p. 41).

This is an attempt to integrate the different aspects of charisma, more efficiently than Weber did, and make the concept more suitable for explanatory purposes. The problem is that it starts with the very vague notion of a “non-everyday” (perhaps more precisely “trans-everyday”) dimension. For phenomenologically inclined readers (including the present reviewer), the most obvious response is to take this term as a shorthand reference to distinctions within the lifeworld, and if the historicity of the latter is taken seriously, we must consider cultural variations in the meaning, extent and importance of phenomena or perspectives defined as transcending the framework of everyday life. That line of thought leads to a comparison of cultural world-articulations. Breuer hints at such possibilities with reference to Philippe Descola’s efforts to re-centre anthropology around a comparative analysis of basic world-views, but then neutralizes that idea by positing a rough correlation between Descola’s models of world-views and stages of social development (pp. 19–20). The issues raised by Descola’s work are too complex to be discussed here, but a familiar classical source may help to take our point further. In Durkheim’s *Elementary Forms of Religious Life*, the distinction

between everyday and non-everyday spheres seems to converge with the one between sacred and profane; this is one among several manifestations of the original omnipresence of religion. Durkheim's argument is empirical, based on what he thought was an exemplary case, as close to human origins as sociological or anthropological inquiry could get, and should not be mistaken for an unconditional equation of the sacred with a realm beyond everyday life (an inverted version of that claim seems to be at work when it is proposed to replace the notion of sacred rulership with that of the charismatic state). Durkheim did not live to develop the projects foreshadowed at the end of *Elementary Forms*. But it seems clear that if he had gone on to deal with the later history of religions, he would have taken note of their varying impact on the distinction between everyday reality and dimensions beyond it. Moreover, he outlined a model of socio-cultural differentiation that would allow us to trace the emergence of key institutions from the original all-encompassing religious framework, and although this programme was not carried out, the overall thrust of his work suggests that he did not regard this differentiating process as exclusively modern. To mention only the most prominent cases, it seems clear that the spheres of politics, philosophical and/or scientific inquiry, and aesthetic creation have their specific ways of transcending everyday reality. It is true, and reflected in Durkheim's comments on certain trends of the modern democratic imagination, that projections and appropriation of the sacred appear in these other contexts; but this secondary sacralization is a separate problem, and should not be obscured by conceptual levelling. The notion of charisma seems either too loaded with connotations derived from its most familiar pre-Weberian use (the informal authority of religious virtuosi), or – if separated from that context – so vague that it threatens to bring on the night in which all cows are black.

To sum up, this conceptual analysis does not support the proposal to redefine archaic state power as charismatic rather than sacral. The widely accepted notion of sacral rulership allows for varying forms. Kingship was clearly

the most common type; divine kingship in the strict sense was a specific and relatively rare version; the relationship to the sacred differed from one civilization to another and from one historical phase to another. For example, the contrast between Egyptian and Mesopotamian kingship was not as clear-cut as earlier scholars assumed, but some basic divergences are still acknowledged; on the other hand, the Egyptian imaginary of divine rulership obviously underwent significant changes (both these points seem to be confirmed by Breuer's analyses). Further distinctions raise the question of divided or secondary power centres. The division of power between king and temple is a recurrent and controversial theme in discussions about the most archaic civilizations. Similarly, the balance of power between the sacral ruler and the economic, military and administrative elites of the society in question what subject to change and often difficult to assess. Here we need only underline the point that these differentiations – all taken into account in Breuer's analyses of particular cases – are perfectly compatible with the general notion of an archaic state anchored in the sacred.

Any definition of archaic statehood must prove its worth by helping to grasp the emergence of the state as a historical process; and in that context, it should also do justice to the pre-comprehension that Breuer shares with virtually all authors working in the field: the view that this innovation represents a major turning-point in human history. A minimalist conception of the state as a regulating centre with a territorial domain does not meet these criteria. For a more adequate model, Breuer draws on Max Weber's general theory of the state (in contrast to the specific one, focused on the rational bureaucratic apparatus invented in the West, and favoured by some later readers of Weber's work). When it came to characterizing state power in cross-cultural and trans-epochal terms, Weber stressed the monopoly of legitimate force. It has rightly been objected that many early political formations, intuitively and more or less unanimously classified as states, were far from achieving such a monopoly. Breuer therefore suggests a more historical version of

Weber's claim: political associations (*Verbände*) that show a tendency to monopolize legitimate force should be characterized as states (p. 15). Obviously, the cases to be compared – especially the very numerous ones where the evidence is exclusively archaeological – will often be difficult to assess on that basis. But that does not invalidate the implicit basic point: a historically grounded theory of the state must be conceived in processual terms. Unfolding dynamics of state formation and transformation, rather than stable types or permanent structures, are the main theme to be clarified. This was the general message of Norbert Elias's work, to some extent blurred by his one-sided focus on the infrastructures of statehood (the aspects that Breuer wants to subsume under charisma were largely neglected), but a more multi-dimensional approach can nevertheless build on his insights and extend them into new fields. In particular, the emergence of the state is to be analyzed as a process, rather than an invention or a once-and-for-all historical watershed. To quote the concluding statement of Breuer's introduction, "the state is certainly domination in space", but a closer examination of statehood "cannot do without the *longue durée* and thus the dimension of time" (p. 37).

As Breuer explicitly notes, this emphasis on temporality applies to the early state no less than to later formations. We might ask whether that view is easily compatible with his attempts (in the empirical chapters) to draw a clear line between states and pre-state societies (the question becomes particularly acute when the state is contrasted with the chieftom, supposedly a category with clearly defined content and boundaries. If we treat pristine state formation as a long-drawn-out and emergent process, we may be able to identify turning-points and convergences of multiple trends, as well as blockages and reversals; but it becomes more difficult to pinpoint a take-off that would mark the beginning of a new form of political life. The problem is compounded by the incomplete and elusive character of the record. It is now widely accepted that political organization is part and parcel of tribal societies, and it is no less clear that the trends culminating in the archaic states

and civilizations, studied by archaeologists and historians, were conducive to major transformations. We will most likely have to accept permanently blurred borderlines and transitions between these two states of affairs.

With that in mind, another look at the sacral connection may be useful. It should help to gain a better view of the shift to statehood, but it will also have to be adapted to the conceptual and evidential limits indicated above. A convenient starting-point is Marcel Gauchet's theory of the early state, not mentioned in Breuer's discussion (an understandable omission, since Gauchet does not engage in the concrete historical analysis that is all-important for Breuer). Gauchet's interpretation of the emerging state as a "sacral transformer" is the cornerstone of a "political history of religion" that has aroused controversy, especially about later historical stages, but it has yet to be assessed in the context of archaeological and anthropological debates. As it stands, it is no doubt too dependent on notions of an abrupt break; a more processual version could still retain the idea of a reorientation, turning away from patterns of order ascribed to mythical ancestors and towards an empowering of rulers with some kind of sacral (not necessarily outright divine) status.

This view is not incompatible with a multi-linear conception of primary state formation. Max Weber had noted the varying power balance between priests and warriors in early phases of social development, and the long-term effects of such constellations. Breuer links both sides of this agonistic relationship to charisma, more systematically than Weber did, but tones down the role of military charisma, as against the magical and religious types. He stops short of ascribing primacy to the latter, but a revised version of Gauchet's thesis can take us further in that direction. A complex conception of the sacred, drawing on Durkheim but expanding his definitions, would combine three aspects. The sacred, in contrast to the profane, is – as Durkheim duly emphasized – the dominant side of a fundamental division; it is also, as he less clearly saw, central to the constitution of the world as a unifying horizon and a field of meaning; and it is, as he implicitly recognized, an

enduring but mutable frame of reference for the structuring of social power. In short, it would seem to possess an integrating capacity lacking in other factors involved in the rise of the state. Breuer is no doubt right to insist on the multiple lines of development contributing to this process, and his argument is backed up by the “dual processual theory” of American archaeologists (in fact, the duality in question seems to have multiple meanings for different authors: it refers to monocratic and oligarchic power structures as well as to priestly and military leadership, and sometimes to patrilineal and matrilineal succession). But I do not think that his empirical analyses include a clear case of military state-building bypassing the sacral connection.

At this juncture, a brief comparison with another foray into the same field may be in order. Norman Yoffee's book on the early state and its interpreters [*Yoffee 2004*] is mentioned in a footnote to Breuer's introduction, but does not enter into the subsequent discussion. There are some basic affinities between the two books. Both authors set out to demolish theories that exaggerate the strength and the dimensions of early states; Yoffee links these retrospective illusions to neo-evolutionist views, whereas Breuer is less concerned about that background and – as some of his formulations suggest – more receptive to certain evolutionist ideas. But more importantly, there are three distinctive aspects of Yoffee's argument that seem relevant to the issues raised by Breuer. In the first place, Yoffee develops a more explicit critique of the tendency to equate the institutions of surviving tribal societies (our supposed “contemporary ancestors”, as he calls them) with those of prehistoric ones; and on that basis he questions the notion of the chiefdom, which turns out to be very difficult to define in precise terms and very dependent on selective projections of anthropological evidence into the past. Secondly, he proposes to study the rise of the state in connection with processes of differentiation and integration, both types being defined in ways that go beyond functionalist assumptions while emphasizing the distribution and concentration of power and wealth. Finally, neither wealth nor power develop independently of ideas about their proper

uses and possibilities, and the role of ideologies in the rise of early states thus becomes an important theme, however difficult it may often be to grasp his aspect of the picture.

Within the limits of this review, it is not possible to discuss Breuer's regional case studies in detail. An adequate response would, at any rate, require specialist knowledge of each field. But some strengths of the argument should be underlined. Breuer notes the importance of interstate relations, and the very different forms they could take in various parts of the world. There is, for example, a very marked contrast between interstate dynamics in Mesoamerica and China. He signals the importance of great empires in the Old World (p. 15), and is well aware of the pioneering turn towards empire in the Near East (beginning with the third-millennium expansion of Akkad). He is no doubt right to reject over-enthusiastic attempts to depict late fourth-millennium Uruk as an empire and his account of the very gradual Egyptian shift to empire-building, culminating in the New Kingdom, sounds convincing. The refusal to recognize the Inca state as an empire seems more problematic. Here the unquestioning application of the Weberian concept of patrimonialism obscures the originality of a state that achieved extraordinary power despite limited technological resources (Breuer is, however, on the right track when he criticizes traditional narratives, still accepted in some recent literature on the Incas, that describe them as coming from nowhere; in fact, they built on a long history of state formation). Another instance of misplaced scepticism might be the conclusion that Oceania did not make it to statehood. It is not clear, at least not to the present writer, that Breuer has effectively countered the claims of other authors – notably Patrick V. Kirch – who have found evidence of archaic state structures in Polynesia. And to add a last comment on empirical shortcomings: The trajectory of the Hittite state in Anatolia, whether we define it as an empire or not, would have merited inclusion alongside Egypt and Mesopotamia, all the more so since Breuer mentions the interesting hypothesis that the collapse of this great power, very likely brought about by internal fissures

and conflicts, may have been a decisive factor in the regional crisis of the Late Bronze Age.

As I mentioned at the beginning, Breuer does foreshadow one fundamental criticism of the kind that strikes at the very core of Weber's sociology of domination. This happens in the course of a chapter devoted to ancient Egypt, a civilization with an exceptionally long and continuous history. But there were also significant shifts and innovations within its framework, and it is logical to raise the question whether traditional domination replaced the original charismatic pattern. If I am not mistaken, Breuer tends towards a positive answer, but realizes that Weber's typology does not provide a sufficient reason to defend it. As he writes, Weber envisaged the transformation of everyday routines into custom, tradition and ethos; Breuer objects that "an ethos never emerges from repetition and mimesis, only from reflection, distance and explication" (p. 257).¹ This is a far-reaching concession from an author otherwise very inclined to stay the Weberian course, and we should at least note the most obvious implications. Reflection, distance and explication were at work in all the great historical traditions, and they produced very different conceptions of legitimate power; it may even be questionable whether the notion of legitimacy is uniformly applicable. It is not at all clear or plausible that a general conception of traditional legitimacy would make sense. As Breuer notes, the Weberian concept won't do, and neither he nor anybody else has produced an acceptable alternative. The relationship to the sacred is certainly a recurrent theme, but its various articulations are worlds apart (it is enough to think of the Chinese mandate of heaven, the Islamic caliphate, and the medieval Western Christian notion of the king's two bodies). Moreover, a general model of sacral legitimacy would lump these traditions together with archaic civilizations. And there is a further (for our purposes final) comment to add. If reflection, distance and explication were active in premodern traditions, they were doubly so in the modern era. Taking

that as a cue, it quickly becomes clear that the notion of legal-rational domination is far too narrow and covers only one aspect of the problematic that has figured in modern traditions of reflection and debate on the legitimacy of power. We need a broader framework, but here I can only suggest that Shmuel Eisenstadt's bipolar conception of democracy, constitutional and participative, and his analysis of the paradoxes resulting from this combination might prove more useful than the standard Weberian approach. It should be added that both the constitutional and the participative pole can appear in extreme and mutually estranged forms that amount to a negation of democracy. All this is beyond the scope of a review. But we seem to have reached a point where a radical reconstruction of Weber's sociology of domination becomes urgent.

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Marcin Kula: *Kartki z socjologii historycznej*. Warsaw: Scholar, 2014, 253 pages

In 2014 Marcin Kula, a rigorous Polish historian and historical sociologist well known to readers of "Historical Sociology" had already published three books with the term "historical sociology" in their titles. The first one was *Kartki z socjologii historycznej* ("Pages from Historical Sociology"), published by Scholar, a reputable Warsaw publishing house. The second and the third are published versions of his lectures in historical sociology, entitled *Trzeba pracować i produkować. Wykłady z socjologii historycznej* ("It is Necessary to Work and Produce. Lectures in Historical Sociology") and *Trzeba mieć*

¹ I take the liberty to note that I argued along similar lines in an essay on Max Weber [Arnason 2012].

pieniądze (“It is Necessary to Have Money”), co-published by the Muzeum Historii Polskiego Ruchu Ludowego and Instytut Studiów Iberyjskich i Iberoamerykańskich UW. What makes the books interesting is the way in which the author uses the very notion of “historical sociology”, a term that does not often appear in the titles of Polish scholarly books. When it does, the respective publications deal at most with certain concepts of historical sociology, and rarely offer studies in it.

Here I would like to deal only with the former book, which – as the title suggests – is a collection of papers that have been previously published elsewhere. It opens with a brief introduction which explains the author’s concept of historical sociology. For him historical sociology is a meeting place of history and sociology and an alternative (or rather an indispensable supplement) to the historiography that concentrates on sources in order to reconstruct “what really happened”. Hence, it deals with broader research issues, attempting to acquire knowledge that reaches beyond the phenomena being studied (p. 8). Although such a disciplinary program may seem obvious, it is not at all clear whether any social science is able to do accomplish this. In my personal opinion the essential service that history may offer to sociology is a clear demarcation of spatial and temporal limits of analysis – ergo, the necessity to narrow the range of possible generalizations. Thus, Kula’s idea of historical sociology is probably more challenging than it looks, and one may wonder to what extent the author himself is really ready to follow it ...

Apart from the introduction the book contains seventeen, mostly short, papers on a wide range of topics, from individual social and historical phenomena to more general ideas. They do not seem to be arranged in any particular order, neither by the subject material, nor by chronology of writing. Two texts concern migration – the opening one, entitled “Nations and migrations”, and the fifth one, dealing with various rulers’ attempts to limit their populations’ international contacts. The paper on factors that influence spatial organization of cities is supplemented by the essay on moving of capital

cities, while the more general piece on students as rebels – by the paper on the Polish “March” events of 1968. Other texts deal with such subjects as Polish rock music, sport (especially great international sport events), violence in history, and the feeling of fear (including public fears). The second paper in the collection is an essay on work and national stereotypes related to it, while the fifth one deals with the idea of modernization and modernization programs in Polish history. A thirty page essay deals with the twentieth century as the supposed age of thinking people. For a student of nationalism the paper entitled “Is national culture national?” may appear interesting. Two of the texts seem more personal, one containing a list of issues for a possible book on Marshal Piłsudski (that the author does not intend to write himself), and a three page reflection on the Polish cult of Pope John Paul II. Interestingly the collection contains one text already familiar to “Historical Sociology” readers – a variation of the paper on the Communist sociotechnics published in the 2/2011 issue.

To assess such a wide-ranging collection of papers is by no means an easy task, especially when they are – as in case of the Kula’s book – impressionistic essays rather than systematic studies. Of course, certain criteria apply even to essays, while the brief yet programmatic introduction enables us to pose the question whether the author managed to follow the approach he proposed.

The general impression after reading the essay is mostly positive. One must appreciate the author’s erudition and his broad yet detailed knowledge on such distant subject as Polish political life during the interwar period or urban development, both in Europe and overseas. The most interesting texts seem to be those dealing with topics best suited for the essayistic form, such as the reflection on the idea of modernization, or suggestions for a possible book on Piłsudski, which exemplify Kula’s qualities as a remarkable figure of Polish intellectual life – his ability to see things ignored by the dominant (i.e. right-wing and nationalist) perspective and his disregard for nationalist mythology. On the other hand, sometimes – as in the case of the text on rock music – Kula seems to be reaching

the very limits of his professional expertise. All in all, as a collection of historical (I would not dare to say: historical-sociological) essays the book is indeed attractive and worth reading.

The question of the actual relation of the essays to historical sociology and in particular to the ambitious program outlined in the introduction is quite another matter. In my opinion the answer to it is positive in the case of those papers that concentrate on concepts and ideas, such as the already mentioned pieces on the idea of modernization. Those dealing with “harder” phenomena, such as migration and urban development still offer interesting, sociologically relevant observations, and often illuminating impressions. But do they add to more general knowledge, as the author’s concept of historical sociology suggests? In my opinion in order to offer sociologically relevant knowledge on those “harder” phenomena one should use precise conceptual instruments and employ careful, disciplined analysis. An example of a text in which the author’s approach turns out to be counterproductive is the essay on national culture. The very idea of “national culture” purposefully conflates a few different concepts: the word “culture” as a symbol of usually abstract and rarely well-defined spiritual values, culture as production and consumption of art (mostly perceived as a supposed transmitter of the former), as well as culture as a medium of communication. In the case of academic writings they all blend with the all-embracing, anthropological concept of culture typical for mid-twentieth century cultural anthropology. As a result, any serious attempt to deal with the national culture issue must start (and may probably end) with disassembling the concept. Otherwise – as in case of Kula’s essay – it turns into an idle presentation of examples that prove the obvious fact that the term “national culture” is a mere political symbol. Using examples from various epochs and region, disregarding temporal and geographical diversity and without taking into account their specific social contexts, which appears in some of the papers, including those on capital cities movements or migrations, was by no means problematical in their original publication or conference context. I have the

impression that some conceptual refinement would turn the reflection on the twentieth century as an age of thinking people into an entirely different text too. Still, what seemed appropriate in individual texts, published individually, among more conventional studies in scholarly journals or conference proceedings, looks much more problematic in a collection of essays entitled “Pages from Historical Sociology” – even when the reader employs a less ambitious idea of historical sociology as a social science discipline that respects particular historical contexts of the studied subjects and realizes the spatial and temporal limits of its own findings.

All in all, Kula’s book forms a fine collection of well written and insightful historical essays, full of novel facts and observations, often offering the readers interesting and sometimes not at all obvious thoughts and insights. On the other hand its title seems to be to some extent misleading, and the readers do not get what they are expecting. This is not because “Pages from Historical Sociology” are not sociological enough, but rather because some of the topics would better serve a more intellectual discipline and more analytical approach – at least when they are dealt with not in dispersed papers, but in one, more or less coherent book.

Jaroslav Kiliás

Jan Čermák: *Kalevala Eliase Lönnrota a Josefa Holečka v moderní kritické perspektivě*. Prague: Academia, 2014, 1116 pages

During the last two decades science has entered into a wide interdisciplinary – one could almost say post-disciplinary – phase. Many topics of study form part of more than one scientific discipline, leading to a differentiation in the original sciences. The recently reviewed edition of *The Kalevala* can be placed not only at the intersection between literary science and folklore, but also the sociology of literature, or possibly historical sociology of text. Other areas that could be considered are general narratology or the sociology of knowledge (in this case

traditional). Undoubtedly there are other fields of science which could address the issue of cultural artefacts of this type. These could include cultural anthropology with an emphasis on the relationship between orality and literacy, or written, as well as the ethnography of reading, focusing on cultural specificity.

The new edition of *The Kalevala*, by the Czech linguist Jan Čermák, comprises several approaches. The edition itself is presented in a traditional format and this is for several reasons. At first glance it is surprisingly hefty so this is not a matter of an easy read and a “fat” book cannot easily be placed on a bedside table. I stress that I am not being ironic here. Standard practice dictates that the typeface used for the book is Preissig Antiqua, created by the painter, graphic artist and typographer Vojtěch Preissig (1873–1944). Furthermore it is decorated using illustrations by the famous Finnish painter Aksel Gallen-Kallela (1865–1931), whose art focused on Finnish mythology in a style that moves between realism and art nouveau. Still on the topic of the formal page, the edition is hardback with a sleeve. This is certainly not a paperback. *Summa summarum* – *The Kalevala* is actually a bibliophile edition. We should also mention the author of the introduction in the new Czech *Kalevala*, Markéta Hejkalová. She is a writer, Finnish translator and member of the PEN International club.

The original translation by the classic Czech writer Josef Holeček (1853–1929) complements this antique appearance. This exponent of realism and ruralism in literature learned Finnish and in 1894 published *The Kalevala* in Czech. Holeček’s translation remained unchanged in further Czech editions of this Finnish cultural jewel [1953, 1980, 1999]. However the Anglicist and Finno-Ugric specialist Jan Čermák, currently the latest editor, provides the “foreign” translation with a rich critical commentary, notes to the text and a wide ranging study on the origins and structure of the epic based on modern research. The result is an unusually voluminous publication which can, without a doubt hold its own in the international field of the study of heroic epics. The editor Jan Čermák chose to keep the original translation by the

writer Josef Holeček due to its excellence, rich vocabulary and accuracy. The editor of the new edition has provided detailed notes to the text showing possible deviations from the original. This demonstrates that nearly the majority of translators cannot adhere strictly to the original text. In this context it could almost be said that *The Kalevala* could also act as a text book for the theory of translation.

It may also be worth adding that Jan Čermák to some extent takes on the role of commentator, mediator and performer. He has already published a translation and critical presentation of the Anglo-Saxon epic *Beowulf*, the only manuscript dating back to the year 1000. It was while comparing the epic *Beowulf* and *The Kalevala* that he realised that both works represent the result of a long creative oral process culminating at the end with an imaginary “last singer”: the anonymous creator of *Beowulf* and the Finnish revivalist Elias Lönnrot. Čermák maintains that although the two texts are very different in many ways and far apart in terms of age, nevertheless it is possible to use comparisons between the two in order to gain a better understanding of the circumstances surrounding the creation and structure of this genre. According to Čermák, *Beowulf*, which is a sixth of the size of the *Kalevala*, is unique: the manuscript does not exist in any other form. The Anglo-Saxon tradition did not last long. *Beowulf* was also heavily influenced by monastic culture as well as trying to accommodate a pre-Christian, mystical period. In the case of *The Kalevala* we do not find such a strong Christian influence. Moreover Catholic hagiography did not take root in Finland for long, the growth of the Lutheran reformation dissolved the Catholic cult of saints. In Karelia orthodoxy prevailed.

In the analysis of the heroic epic the concept of bricolage peeps somewhat impishly from behind the scenes. This concept is mainly connected with the social anthropologist and mythologist, Claude Lévi-Strauss. Bricolage means do-it-yourself in terms of structural improvisation, shifting terms of reference, fixing and mending. It even includes veering from the original plot, using scraps, assembling etc. Nevertheless in general the result tends to be

professional, virtuosic, unique. At the end of the day these creative steps are applicable to all human activity. Even Elias Lönnrot, creator of the literary version of *The Kalevala* could not avoid some do-it-yourself when reconstructing this complicated multi-layered work. He created a single narrative structure of the epic by combining several variations and omitting irrelevant verses. Basically he codified oral literature. To a certain extent he used Homer's epics as a template for his work.

I will only make brief remarks on the work itself and will certainly not narrate the contents of which there are many other variations apart from the fixed literary form. Firstly I propose that *The Kalevala* shows a lesser representation of the heroic element. If we were to summarise then *The Kalevala* is the birthplace of the main heroes of the work which are Väinämöinen the fortune-teller, Ilmarinen the skilful blacksmith and Lemminkäinen, the womaniser. These heroes embark on a search for adventures which mainly take place in the northern kingdom of Pohjola. *The Kalevala* is set in a period of time stretching from the "beginning of the world" to the birth of Christ.

In Finnish-Karelian runes heroic battles play a much lesser role than so called "worldly occupation". For example the aim of an epic fight takes place in order to gain and take control of the Sampo mill which gives abundance. In short heroism is replaced by magic. The hero is more likely to wield spells than a sword and even then we are not talking about some young gun but rather a wise old man. There is further evidence of a certain idealisation of the shaman figure, which may lead to a consideration of shaman legends and the "role" of the shaman. However one of the main protagonists, Väinämöinen does not appear in the role of shaman – as a hunter of souls, he is only accompanied by magic.

The Kalevala should also be studied in terms of the myth-folkloric continuum. I have found it contains motifs which are characteristic of mythical cultural heroes. In the majority of the different versions about the sea voyages of the wounded Väinämöinen we find the cosmogonical myth about the creation of the world from the eggs carried by a duck, placed on the knee

of this hero. It is told that a duck or a goose lays golden eggs into a copper nest on his knee. The eggs fall into the water and break into pieces. Väinämöinen magically turns the lower part of the eggshell into the earth and the top part becomes the sky, the yolk becomes the sun and the white the moon. The rest of the eggshells turns into stars and clouds. Clearly here we can identify the universal creation myth of the cosmic egg. Elsewhere a mythical prehistoric bird carries eggs on to a ship, to an island, to an elevated hillock etc.

Within the plot of this epic there are also allusions to the cosmic hunt of the elk, considered the guardian of the forest animals. Sometimes the hunt for the elk is carried out on skis made of sacred wood. Incidentally this plot also appears among smaller ethnic groups in Siberia: The Evenks, Khakas, Yakuts and Altays. Victory over a mythical or demonic creature is considered to be the first task of a young hero. As we can see the elk also functions on a cosmic scale.

In the Finnish-Karelian epic cosmogonical topics and motifs about the creation and population of the earth feature heavily. There are runes about the origin of things, the mythical origin of animals (for example the elk and the bear), about the discovery of fire and metal, creation of tools etc. Runes of an etiological nature do not deal with tribal leaders, warrior castes, there is no talk of ethnic identity or early states. *The Kalevala* creates literary strands where narrative is mixed with love poems, magical songs, spells and enchantments. *The Kalevala* is not an easy read, it is necessary to contend with so called cultural ambivalence where something appears thus and thus at the same time. At the same time one must not exclude the issues of monstrosity, hyperbole and gigantism.

The Kalevala contributed to the development of Finnish folklore which then significantly influenced the study of folklore. Researchers of world literature and folklore include the aforementioned Elias Lönnrot, also Julius Krohn (1835–1888) and his son Kaarle Krohn (1863–1933) and last but not least Antti Aarne (1867–1923) and Lauri Honko (1932–2002). During their research these researchers also studied the migration of plots and motifs using

a historical-geographical method. Kaarle Krohn's publication on *The Kalevala* (five-part *Kalevalastudien*, 1926–1928) should be considered as a reference book not just for Lönnrot's original. It was Krohn who accurately captured the etiological and magical character of Finnish runes (*Magische Unsprungrunen der Finnen*, 1923). It would not be possible to carry out research on *The Kalevala* without these publications.

The presumed orality merits more attention since *The Kalevala* was performed as “loud” singing, and not “quiet” reading. Single chapters in *The Kalevala* are considered as runes which means a “song” relating to a single thematic plot. The Finnish term “runo” means “song” or “poem”. Obviously this is a case of hypothetical assumptions on the performance of runes by singers based on relatively scarce knowledge or comparison with other ethnic groups. Furthermore it is not possible for us today to precisely imagine a performance of *The Kalevala* or other epics, presumably the whole could not have been presented in one single performance due to the limitations of human memory.

In order to assist memory the so called *Kalevala* verse was used. Only professional or semi-professional singers would have been able to manage this rhythmic speech. I would like to point out that a very thought-provoking study was carried out by Anna-Leena Siikala into the singing, customs and physical practices of the singers (*Body, Performance and Agency in Kalevala Rune-Singing*, in: *Oral Tradition*, 15/2, 2000: 255–278).

It is also important to note that Elias Lönnrot brings up the serious scientific problem of the textualisation of oral tradition. This is also connected to contextualisation based on the impact of nationhood and nationalism. In short it is a question of transforming oral poetry and a heroic epic into a textual discourse on nationalism and representing orality in the written form.

On the whole the new edition of *The Kalevala* graphically illustrates its influence on Finnish culture in creating a Finnish-Ugric ethnic identity. Last but not least, the heroic epic *Kalevala*, undoubtedly fulfils the essential desiderata necessary to be considered, according to

Goethe's interpretation, as a supreme work of world literature.

Bohuslav Šalanda

Dennis Smith v Ljubljani: s prispevki Avgusta Lešnika, Marka Kržana in Polone Fijavž / Dennis Smith in Ljubljana: with contributions by Avgust Lešnik, Marko Kržan and Polona Fijavž. Ljubljana: Znanstvena založba Filozofske fakultete Univerze v Ljubljani, 2014, 154 pages

Dennis Smith develops a brilliant panoramic of the current financial crisis in the European Union which is far from over, arguing what he calls “humiliation” of all the countries – without exception – that form part of the EU. With the collaboration of Avgust Lešnik, Marko Kržan and Polona Fijavž, Smith also clarifies what is the role of historical sociologists in this important fact.

In the lecture given by him in Ljubljana in 2014; past, present and future of the EU are treated carefully. In Smith's words, the future of this crisis is being decided on the margins and the only recipe to the European Intellectuals who wants to take part in this process of decision-making is to face the European truth that is “lived” on the peripheries (mainly Greece and Ireland). He reminds us that, the Humiliation does not stem from our cultural incompatibility, it is spreading across the EU, in its core and on the borders, attached to the only true motor of the current progressive demise; the global dictate of the capital.

To understand better the situation of Europe and its financial crisis, Smith arises two main metaphors based in children's stories. The first one is the well known “Hansel and Gretel”. It is a story of a wicked witch who deceives and betrays two hapless infants. By “witch” he means bankers and financiers and instead of “hapless infants” he sees employers, workers, consumers and small investors. Hansel and Gretel pushed the witch into her over and made their escape. In this point, Smith argues that in reality, the bankers and the financiers have largely survived, with

a few bruises, mostly temporary. The second story is “The three little pigs”, in this case the pigs are countries such as Portugal, Spain, Greece and Ireland. Ravenous wolves in the global market destroyed their badly built dwellings. Smith is right saying; “these predators huffed and puffed and blew the pigs”.

The author talks about the struggles in Europe comparing the writers Jürgen Habermas and Ulrich Beck, who have both put their minds in this topic. In Beck’s thought, we baldly assert that what is good for Germany’s economy is right for the European economy as a whole and beyond. The austerity programmes have only intensified the economic crisis in Europe, leading to the opposite of what was intended. In short, Beck talks about the plague of German “euro-nationalism”. In this case, Habermas is not agreeing and prefer to talk about “executive federalism”. He thinks that political austerity in Europe is gradually becoming less brutal and hierarchical. He places great hopes on the learning being done by Europe’s political elites, as their constitutional lawyers educate them to be more cosmopolitan-minded. Nevertheless, both authors agree in that we need to factor two crucial agents of change: governments, who are able to deploy the massive military, judicial and tax-gathering power to the state and big business with enormous financial, technological and persuasive characteristics.

Smith highlights two keys to understand the development of the European Union. The first one is the triad that links together the state, big business and ordinary citizens, but the question is; which kind of citizenship will have priority in the programmes of government, will it be what might be called “market citizenship” or will be “social citizenship”? The second key is the relationship between European Union and United States. The present crisis and the future of the European Union are a mirror of the American Civil War and the development of the United States. In both cases there is a framework of governance struggling to contain two opposite forms of political economy, there is a clash between property rights and human rights, there is hypocrisy, corruption, and some fanaticism as well.

Dennis Smith narrates the “European story” as two sequences; the first one is established between 1939–89 and is defined by catharsis, genesis and sclerosis. “Catharsis” refers to the period between 1939 and 1945 where three different interests and ideologies (German Nazism, Russo-Chinese communism and American capitalist democracy) killed at least 60 million people, probably half of them from Europe. The war and its aftermath make a deep impact in the European population and after 1940 people were ready to build peace rather than violence. Coming up next, the author sees the “Genesis”, United States planted its tanks on west European land and turned Europe’s bloody warriors chiefs into servile courtiers, as Norbert Elias’s description, state formation in early modern Europe began with the establishment of strong centralizing royal courts. In 1951 The Coal and Steel Community led the European Community and therefore becomes a West European club giving its members a field of action that excluded ruthless economic protectionism. During these years the club’s membership doubled. Going back to the story, in 1945 the highpoint of America pride and European submission arrived with a US-led victory subsequently reinforced by French and British humiliation in Dien Bien Phu, Algeria and Suez between 1954 and 1962. The tables changed when the US was unable to enforce its will in Vietnam, and had to accept the delight of many European intellectuals. Ultimately the period of “Sclerosis”, during 1970s Europeans desired the peace at all costs and preferred to buy their way out of trouble rather than change their ways. As a result, they lost their flexibility and capacity to adapt. After that time, Brussels was knocked by two massive events; the “big bang” (1986), which opened up the City of London to American finance houses, developing the creation of a vast reservoir of public and private borrowing capacity, fuelling and funding the ambitious of politicians and consumers and second of all, the collapse of the Soviet Union, which meant the end of the Cold war and the re-unification of Germany.

The second sequence would be since 1989 and in this case the author splits it in hubris, nemesis and crisis. After 1989, business lobbyists

were promoting packages for providing health-care education, management services and other functions in order to help the vacuum left. In the other hand, the EU set itself a very ambitious target: to be a disciplined and dynamic business-friendly economy; to be a post-humiliation polity for citizens, not just for governments; to build appropriate structures and systems to achieve these objectives; and to do all this while expanding its memberships, bringing in as many as possible of the countries “released” by the crumbling of the “socialist bloc”, in Smith’s eyes we are talking about “Hubris”. The author follows his historical sequence with “Nemesis” in which the aftermath of the American-led wars plus Obama’s lack of track record and the collapse of Lehman brothers were major background factors that contributed to the loss of financial confidence in September 2008, triggering the precipitate collapse of the vast international mountain of debt and the Eurozone crisis. “Crisis” is the last step of this route; banks on both sides of the Atlantic stopped lending to each other, taking massive amounts of liquidity out of the system. In consequence, many mortgages were foreclosed and national governments stepped in where they could to recapitalize the banks, increasing the own national debts. The cuts in public sector were imminent. The creation of a large amount of unemployment, especially in young people was the main cause of many protests. They have experienced being victims of humiliation.

In the wake of the crisis, a sharp distinction in EU between two types of political economy was clear, one of them operating in the “market” (Germany and UK) and another one oriented to serve “the people” (Greece and Italy). To understand better this period Smith designs one more classification in which as a chessboard he describes the struggles and collaborative relationships in the EU (after 2008) through 4 types of elites; “High priests” which represent the European Commission; “Puritans” or in other words, ordo-liberals mainly in Frankfurt and Berlin; “Cavaliers” formed by political clientelism in countries such as Italy, Romania and Hungary; and “Buccaneers” with neo-liberals in London. His point of view is clear, four political

struggles are currently under way within the EU, in this point Smith raises the following question; so where that leave us? In his opinion, the EU is stuck in a rut due to the visceral conflict and pragmatic cooperation between elites. He argues that, the continuing low level of trust between member states inhibits serious movement towards internal reforms that would sharply increase economic growth, reduce high unemployment, improve wage levels and restore lost ground in the realm of social rights. The most effective way forward would be to overcome the structural incongruities between the EU’s two political economies, one focussed on the rights of property in the market place, the other promising to protect human rights within a democratic polity.

Prof. Smith continues his speech now talking about America. Two bank panics (1857 and 2008) were instigated by the banks themselves, trying to protect their capital in the wake of a speculative boom fed by easy credit. This situation led to a sharpening of socio-political decisions, a spasm of uncertainty, as implications for the existing balance of power were considered. The United States added twenty-one new states in the seventy years after 1791, increasing the number of “voices” in the council. As in the Europe’s case, the author uses the same characters but now applied in antebellum America; now “High priests” are southern planter elites; the “Puritans” would be northern abolitionists; the “Cavaliers” represented by fire-eaters or in other words, lawyers with military background; and “Buccaneers” defined by northern big business.

In conclusion, Dennis Smith proposes three possible future scenarios. The first option in which the EU will become an arena of resentment and revenge that could lead to a process of secession and fragmentation. Smith point out that in the hypothetical case that SYRIZA may enter government a very sizeable minority would be ready to consider leaving the Eurozone. We can add the fact that in January 2015, the head of SYRIZA, Alexis Tsipras, reached the prime minister position being the most voted party. Meanwhile the UK, between a third and half of MPs in the Conservative party would support Britain

leaving the EU as well. This is not that far from reality if popular hostility to immigrants became so great that throughout Europe voters demand a return to strong national border controls. The second scenario argues that the wake of austerity business lobbyists in national capitalists and in Brussels will press hard to ease the way for corporate capital to invest heavily in services traditionally provided by governments in the public sector. At the same time, they are likely to lobby for a lowering of standards (less bureaucracy), which still mean lower costs and higher profits but worse services and a dilution of social rights. The last forecast says that citizens may be brought to recognize that big business is acquiring increasing influence and control over their lives while their own influence through the workings of national parliamentary democracy is being gradually reduced. The author sees this idea as a serious challenge but a positive move in the direction of reducing the structural contradictions between big government and big business that are a major cause of sclerosis within the European economy.

In the second chapter of the book, the main concept switches from European Crisis to Historical Sociology. Avgust Lešnik performs a short view of Dennis Smith and his role in this field of study. Smith is considered one of the most renowned names of historical sociology and one of its founders. His work is an indispensable reference for scientists and researchers but, what is historical sociology for him? Smith believes that, this discipline tries to make sense of the past (and present) by investigating how societies work and change. He defines the interest of historical sociologists as exploration and investigation of the mechanisms that could be subject to change in certain societies or their reproduction.

Smith develops a classification with two waves in historical sociology. The first wave began in the mid-eighteenth century in Britain and France. It was driven by the need to make sense of contemporary political events. This wave finally crashed against the wall of totalitarianism in the late 1920s (Montesquieu, Hume, Tocqueville, Marx, Durkheim and Weber). Smith divides the second wave in three phases;

the first one, before the mid-1960s, was built by the battle with totalitarianism (Talcott Parson and T. H. Marshall); the second one, from 1960 to 1980, is a period that contributed to protest movements for student rights, Black power, the end to the Vietnam War, inequality and resistance movements and women's rights (Marc Bloch, Norbert Elias, Barrington Moore, E. P. Thompson, Tilly and Skocpol); and the last phase that began in the mid-1970s and is developed under the impact of the fragmentation of the stable bi-polar world of the Cold War (Anderson and Wallerstein).

After this classification Smith emphasize that historical sociologists have the chance to give their fellow citizens knowledge and skills that may help them to assess competing views about what is "possible" or "impossible". In brief, historical sociology can be a positive force for democratic citizenship.

The book ends with two interviews done by Marko Kržan and Polona Fijavž in which Dennis Smith accentuates, one more time, the concept of humiliation in the European Union. In his opinion, the politics of the EU over the next decade are almost going to be influenced by the humiliating experiences that have been endured by all populations. He points out two main factors regarding this "humiliating experiences". The first one is about the widespread political effects of powerful emotions such as anger, fear and sorrow, these emotions can conclude in aggressive measures by different groups. The second factor says that the population that have become cynical about Brussels may be vulnerable to ethno-nationalist programmes proclaimed by demagogic politicians.

In his speech, Smith remarks that humiliation is a shared emotion and we are all experiencing it, we need collectively analyze what is happening to us to begin to talk about the problem openly. Smith argues that, this is not a problem that Brussels can solve for us; this is a problem that we have to solve for ourselves. In the last question of Polona Fijavž, Smith underscores the obligations of all populations saying that people have to be careful if they do not take themselves strong, dynamic, civilized, democratic and with a sense of purpose again. If

they do not remember that they are about something more than economics, more than individual profit. He ends up by reminding that we are about creating communities that are committed to making life worth living for all their members.

To conclude this review, I consider this short book as a brilliant and concentrates description of the current situation of the European Union explaining the past and present and even giving future scenarios of what can be the EU in a few years. The author plays all over the text with metaphors that make easier and understandable for the reader to follow his arguments. His clear view shows us a problematic situation (humiliation) where in his opinion all countries have been affected and therefore they play an essential role in order to solve it. We can perceive how Smith invites the lector to make a personal reflexion in order to understand the gravity of the situation. We are being humiliated and this is the time to do something in respect, something to revive the initial essence of the European Union.

Esther Martos

Jacques Le Goff: *Must We Divide History Into Periods?* Columbia University Press, 2015, 184 pages

Many basic aspects of human culture are closely related to the fact that people have to live their lives in time. In fact, the very act of colonizing time is amongst the foundations of all modern civilizations and societies. We are struggling to make sense of the endless time-flow, that we have no choice but to inhabit, in order to interpret the changes and continuities, and to attach meanings and interpretations to events in our shared and private pasts. Dividing time and history into different periods is amongst the most crucial activities in this sense-making effort.

Eminent French historian Jacques Le Goff (1924–2014) dedicated his 2013 essay precisely to the topic of periodization of history. This text had to become the very last work that he was able to prepare for publication himself. It

is not very long, but highly inspirational, neat and sharp, filled with expertise, and not far from being even provocative. The essay is composed of seven chapters and aims to answer a simple but important question: “Is history really divided into parts?”

In order to provide his answer, Le Goff starts with ancient periodizations of the Old Testament and early Christianity. In his approach to periodization of history, Saint Augustine uses six ages of human individual development, from infancy to the old age. According to Le Goff, the world of the Middle Ages is therefore filled with pessimism, stemming from the phrase *mundus senescit* – world is getting old. In this worldview, there was no place for any explicit notion of progress, until the middle of 18th century. However, Le Goff dedicates much of his effort to show that there were some signs of the “progressivist” interpretation of historical development already present in the Middle Ages.

In the second chapter, Le Goff discusses the birth of the concept of “Middle Ages” in the 14th century. It was used to delimit certain distance from the previous age, which was seen as somehow a “middle” epoch between the idealized antiquity and a new era, which had yet to come. Any historical periodization, the author reminds us, is very often ideological, as it provides an interpretation and evaluation of the historical development. Periodization is inherently artificial and provisional, for it also changes itself in time.

The need for historical periodization, in Le Goff’s perspective, results from the establishment of historical education at schools and universities, and he provides a review of these processes in the third chapter. Surprisingly, teaching history is quite a late achievement, and the subject of history was not widely taught until the end of 18th century. Then, during the 19th century, Jules Michelet’s work gave birth to the contemporary conception of the Middle Ages as a dark age, defined in contrast with the later period of “Renaissance”, being (supposedly) the time of growing enlightenment, reason and humanism.

From the fourth chapter onwards, Le Goff proceeds to one specific aim of the essay,

showing that such an approach to the Middle Ages and so-called Renaissance is not correct. This is the provocative aspect of the reviewed essay, which I have mentioned earlier. Le Goff argues that in fact, the “Renaissance” was not a specific historical period itself. Rather, we should speak of a “long Middle Age”, which is delimited by the late antiquity (3rd to 7th century) and mid-18th century (publication of *Encyclopaedia* in France). Le Goff discusses many aspects of the so-called Renaissance, sometimes to show that they were neither groundbreaking nor historically new, including the orientation towards reason and the centrality of human individual. The Renaissance is, in his eyes, certainly an important era, which is to be seen as specific and important, but it was not in any case marked with profound social or economic transformations. In other words, there are more continuities between the Middle Ages and the “Renaissance”, than there are differences. The Western “long Middle Ages” should be seen as a continual period following the fall of Rome, which includes several different renaissances, some of them longer and some of them shorter, sometimes more and sometimes less profound or influential (and the period we are used to call “the Renaissance” is just the last one in a row, and perhaps the most prominent). Rather than being a separate period itself, the Renaissance is an era when certain traits of the new modern period started to manifest themselves, including phenomena like fashion, colonization, national languages, or dietary customs.

In the brief conclusion, titled “Periodization and Globalization”, Le Goff dedicates several paragraphs to the contemporary discussions about “world history”. He does not advocate the elimination of historical periods from historical thought, but he proposes to combine them with Braudel’s concept of *la longue durée*. Historical periodization can only be conceived in relation to certain civilizational areas, and studies in world history should then aim to uncover similarities between periods in different cultural contexts.

I stumbled upon Le Goff’s essay just exactly at the time when I was trying to wrap my thoughts about something that I have

provisionally called “ethno-historiography”, in relation to analysis of oral history interviews, which is part of my doctoral thesis. Periodization seems to be a profound part of the “ethno-historiography” in oral histories. For instance, interview participants naturally and simply refer to general “pre-war”, “war” and “post-war” periods. They seem to know what they are talking about, the knowledge is self-evident, and the basic structure does not only function for time periodization, but inseparably also as the basis for plot development and life story dynamics. Ruptures between the periods are moving the narrative forwards. Outbreak of the war and the liberation several years later mark the borders of the three periods, even though these events often took time on more or less different dates than the political historiography is teaching us. In other words, it is probably very natural and routine approach to past time, at least in Western society, to divide history into periods, and ordinary people themselves tend to do it when they are asked to speak about the(ir) past. There is a certain parallelism of the “great history” and “personal history”; people narrate their pasts on the background of political events, and historians sometime narrate history personified in the story of one person. Individual and collective dimension of human lives merge, as the very distinction is transcended through imagination and metaphors. Le Goff’s essay does not really discuss any of these issues, but it provides basis for such discussions. As I have mentioned earlier, the text starts with the ancient approaches to periodization of history, and amongst the very influential periodizations is Saint Augustine’s conception of history according to the human individual development. Le Goff also acknowledges (albeit marginally) that periodization had become a rule not only for Western historians, but also for anyone else who is providing an account of the past.

Jacques Le Goff’s last work is indeed a thought-provoking and inspirational text, rooted in deep knowledge of secondary literature not only from historiography, but also philosophy (Kristeller, Ricoeur) and historical sociology (Elias). It is a respectable finale of the long and fruitful career of the great scholar,

perhaps one of the most important historians of the 20th century. Even though the essay is meant as a contribution to an expert historiographical debate, it is a pleasure to read even for a non-historian, and – in my opinion – deserves to be read by sociologists, anthropologists,

philosophers and everyone else, who share some kind of interest in different human ways of conquering and grasping the times that people have lived through.

Jakub Mlynář