

Writing Sociology: Writing History¹

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French historian and archaeologist Paul Veyne argued for what he saw as the fundamental lack of object in sociology in 1971. This academic field would definitely not be a science, but, at most, an auxiliary to historiography, itself devoid of any scientific condition since it refers to sublunary causalities, not allowing predictions, only “retrodictions”. Conversely, a set of “praxeologies” could be identified, the core of a future science of man, radically different from both sociology and history, including instead pure economics, operational research, and game theory. While history (and sociology) would inevitably be “Aristotelian”, that is, sublunary and imprecise, scientific disciplines could and should be predominantly “Platonic”, aiming at formal logical elegance.

Veyne was only partly right, since economics itself cannot be considered a science *stricto sensu*. Admittedly, sociology is going through a state of multilevel crisis, allowing us to confront this situation with important recent trends for the emergence of socio-historical grand narratives, sometimes officially called history, less often historical sociology, but all eminently trans-disciplinary. The aim of this research is to overcome the limitations associated with the biographical, elitist, and Eurocentric biases characteristic of traditional historiography. On the whole, the tendency of these studies is nomothetic, but the “laws” identified are at best, approximate. Therefore, they, like economics, are condemned to operate on a mere “Aristotelian” level, and thus, the great “novel of humanity” is bound to remain essentially indeterminate.

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Introduction

In his 1971 book *Writing History*, the French historian and archeologist Paul Veyne highlighted what he perceived to be the fundamental lack of object in sociology. This discipline was deemed not to constitute a science, being at best an auxiliary field for historiography, itself with a non-scientific status, given that it deals with imprecise, “sublunary” causalities. Both were to be distinguished from a set of “praxeologies”, the core of some future science of man, including that which is referred to as pure economics. It is argued here that Veyne’s thesis (discussed in the first two sections) is only partially valid: while it is true that sociology represents a variety of historiography, the very field of economics may only very obliquely be considered as *stricto sensu* scientific. The state of crisis experienced by sociology is recognized and contrasted against the recent trend towards the emergence of major socio-historical narratives, fundamentally nomothetic in approach,

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sometimes officially labelled as historiography and, on occasions, as historical sociology within which is a perceivable purpose to correct the biographic, elitist, and Eurocentric biases of traditional historiography. This emerges especially in works such as *Why the West Rules (For Now)* by Ian Morris, and *How the West Came to Rule* by Alexander Anievas and Karem Nişancıoğlu, as discussed in the third and fourth sections of the article. The conclusion proposes that, irrespective of their merits, these projects of a trans-disciplinary nature nevertheless do not overcome the prominently “sublunary” nature of sociological-historiographic efforts, correspondingly remaining themselves shorn of any specifically scientific character.

Spinoza’s dream and Parsons’ truth-and-a-half

Veyne refers explicitly to the argument in *The Rules of Sociological Method*: “for sociology to be possible, there must be social types, social species”, Durkheim reasoned. This would require the present to be more than a mere consequence of the past, having instead its own structure. “It must resemble an organism, rather than a kaleidoscope” (1984, 269). Fortunately, these conditions are met: we have the “social milieu”, defined by “volume” and “concentration”, exercising a preponderant influence on the “concomitant facts”. We can therefore think in terms of anatomy and types, assuming the existence of genuine causal relations with sociology thus legitimately aspiring to become “a sort of biology” of societies. “Three quarters of a century have passed since those beautifully lucid pages were written”, Veyne added sarcastically; and given the fact that sociology has never since discovered any social types or dominant orders of facts, we must recognize that the “nominalism of historians” is epistemologically well founded and conclude that the object of sociological analysis is still lacking. Nevertheless, he concluded in a cheerfully provocative manner, given that it does exist, “or at least sociologists exist, it is because the latter do, under that name, something other than sociology” (270).

More than a half-century after these inspired and witty pages were written, it seems time to review what they contain, both truth and falsehoods. According to Veyne, it is against the essentially unscientific nature of historians’ procedures that we can oppose a set of “praxeologies”, defined by their own internal coherence and appealing to a hypothetical-deductive methodology rather than by any actual capacity to predict facts, that can appropriately be designated as “sciences”. Obviously, in concrete terms and given that these circumstances and their logical components can multiply indefinitely, what really happens may diverge significantly from what models indicate. However, this apparent failure does not provide the grounds to contest their scientific validity. A certain number of academic practices do meet these criteria, and in doing so, they are more concerned with logical “formal elegance” than with any kind of correspondence to empirical reality. For example, while economic agents do not behave as *hominnes economici*, nothing decisive can be inferred from this. What really matters is that to the extent that they deviate from the prescriptions of the models, they will have to pay an inevitable price: according to Veyne, sooner or later “the event will avenge” (248) the theoretical model that was

disobeyed, whether this disobedience is an expression of the free will in human action or (which in the end is all the same) of the infinite multiplicity of effective human determinations.

Under the category of the human sciences, or “praxeologies”, Paul Veyne brings a small group of academic subjects together, including what is known as “pure economics”, defined as the comparative analysis of action in an environment of scarce resources and with a multiplicity of objectives, allowing for certain rates of reciprocal exchange, or the “opportunity-cost”. Regarding the preferences of agents, they should be left undefined. Economic science has never set out to research their origins or nature, postulating only the “transitivity of choices”: preferring A to B, and B to C, obviously implies preferring A to C. From this relatively small list of theoretical assumptions, Veyne conveniently excludes the famous “independence of utility functions”, which was much debated before and ever since. He does, however, include the principle of time-discount equivalent to interest, proposed by Bohm-Bawerk, to which any real economy, regardless of the nature of ownership, would have to be subject in order to subsequently avoid the aforementioned “vengeance” of events: this is a fact, he assures us, that even Soviet economists were forced to admit and incorporate into their calculations albeit belatedly and against their will (Ibid.).

It is not only “pure economics” that is considered worthy of scientific status. Chomskyan linguistics, for which the fundamental problem is not the practical relevance of any linguistics system, rather the fact that these systems do exist, endowed with logical coherence, emerges as a candidate meeting the conditions of “formal elegance” that allow for recognition according to the criteria set forth by Veyne. Hence his approving quotation from Chomsky: the true question does not reside in how we might build a grammar without appealing to meaning, but rather in how we might ever build a grammar at all (323, note). The same applies to the philosophical reflections that Kant designated as “practical reason”. Their purpose is to identify the logical quintessence of moral actions regardless of the actors’ intentions. It is not a matter of investigating the degree of adherence of a particular scheme to factual reality or of scrutinizing the motivations of the actors involved. Such matters may be highly interesting from the point of view of some History of Morals, but they have no relevance to knowledge the deep logos of morality. In fact, all this basically configures a distraction for a scientific activity that is perceived as being much closer to “Platonic formalism” than to “Aristotelian experience” (252).

In the practice of history on the contrary, we plunge into a very distinctly Aristotelian environment of “sublunary causality” (145), imprecise, allowing not prediction but only “retrodiction” (Ibid.) and correspondingly never able to find “what Wittgenstein calls the hard of the soft” (251), a precondition for any science. On the one hand, causality is not constant, since the same causes do not always produce the same effects. Moreover, “we do not succeed in passing from the quality to the essence” (Ibid.): we know how to recognize a behavior as religious, but we cannot define religion. In the group of “praxeologies”, Veyne includes operational research and game theory. In fact, the then-novel device of the “prisoner’s dilemma” seems to fascinate him with a particular intensity, even if he

understandably does not discuss the extensions and ramifications of this dilemma (ultimatum and dictator games, etc.). Although he praises Kenneth Arrow in his reference, he does not analyze his “impossibility theorem”. Furthermore, he obviously ignores the group of considerations associated with “network theory”, especially the notion of small-world networks which have become famous, and with such vast fields of application, allegedly transversal, to the generality of the “human sciences” (Mendes, 2004; Gintis, 2006).

At a completely different level from this “science of man” cluster which by then had only the status of a project, Veyne recognizes the practices of historians as dealing with an “empirical” reality, whether more prone to generalization or individualization, but always escaping the abyss of *individuum est ineffabile* to the extent that they consider each concrete case as the result of a series of determinations, referable to an analytical scheme endowed with a certain general (albeit weak) validity. In other words, the activity of the historian would focus on the “specific”, that is, the singular variation within a more generic pattern, not exactly the individual. However, this specific occurrence, this “event”, can be mentally constructed with very different levels of generality, ranging from the history of the Battle of Marathon at one extreme to the history of War at the other, while always assuming that “there is no atomic fact” (Veyne, 1987: 34). This may all be the subject of history, inasmuch it is assumed to be the result of “sublunary” causality: with a fundamental weakness in the determinations stemming from various logical orders of facts, their crossing and mixing establishing the effective causal nexuses, clearly precarious, if not absent.

All this is capable of providing the central point of interest and analysis, sometimes more événementiel, sometimes less, depending on the degree to which the generality increases, and the statistical regularities end up imposing their weight. Nevertheless, even in this second case, we should not think that we have left the closed sea of historiography to enter a pretended ocean of a science of man, whether labelled sociology or otherwise. In terms of scientific dignity, Veyne does not consider the peculiarity of a certain perspective or point of view worthy of recognition, as for example, the supposed determination of the “ultimate goals” of human action by culture in accordance with the original project of Parsons (1931; 1932; 1934; 1935a; 1935b; Graça, 2008; 2012), or according to Ralf Dahrendorf’s 1973 model of the *homo sociologicus*, the human being who performs roles and interacts with others as if the whole world were a stage and everyone merely an actor, even though within scripts with highly variable levels of compulsion. None of this provides an analytical justification for any scientific claim. Most decidedly, according to Veyne, neither sociology nor history constitute sciences:

“Parsons spoke truly, more than he perhaps thought, when he wrote that history is “an empirical, synthetic science which needs to mobilize all the theoretical knowledge necessary to explain the historical processes”. To put it more precisely, the knowledge that is necessary — laws in detail — in the measure that they complete the understanding of the plot and are inserted in sublunary causality. Spinoza’s dream of a complete determination of history is only a dream, science will never be able to explain the novel of humanity, taking it in whole chapters or only in para-

graphs. All it can do is to explain a few isolated words of it, always the same ones, that you come across on many a page of text” (1984, 253-4).

Moreover, even economic analysis, inasmuch as it deviates from its “pure” model and becomes “institutionalist” to some extent, whether in the path of Keynes, Veblen, or any other researcher with an empirical bent, is immediately removed from the scientific pedestal precisely to the extent that it tends to become “sociological”. Many studies on the consumption function, says Veyne, are completely outside of the realm of economics just as is the case with technological studies of the production function. While economic sociology informs the historian that there are consumers who purchase products precisely because they are more expensive, thereby showing off their wealth, and that this is known as “conspicuous consumption”, the historian will simply shrug his shoulders in impatience and weariness since conspicuous consumption can take many different forms. Thus, he must deal with “who consumes conspicuously, why, and whom to bluff [...]. The sociological economist being content to put names on truisms, all the work remaining to be done is for the historian” (258).

Under the banner of sociology, Veyne affirms, there has been the production of either philosophy (especially political philosophy) or contemporary history, particularly non-*événementiel* contemporary history, or, finally, a genre of literature resembling the works of the *moralistes* in the seventeenth-eighteenth centuries, that is, a genre whose value resides primarily in its inherently aesthetic qualities (271). It is true that these activities do not mutually recognize each other and do not perceive themselves as such, although it would be advantageous and clarifying if they did. History, for example, is generally defined on a very narrow basis: hence the fact that “France in the seventeenth century” is usually presented as history, whereas “The city through the ages”, on the other hand, is labeled precisely as... sociology (264). Nevertheless, the different academic niches do not mean different criteria for the definition of facts: we are essentially dealing with the same type of activity, and the real academic misfortune here is that while “history doesn’t do enough”, its vision abusively limited by units of time and space, “sociology does too much” and, not recognizing itself as history, “it believes itself obliged to do science” (Ibid.).

By the same logic, it would be wrong to claim that a study on Emperor Friedrich Wilhelm is really history or that he deserves to be included in it, while his tailor enters the scene only indirectly through his connection to the star of the show, or is included in the general category of tailors, which would then make the study less “individualizing” and more “generalizing”... or, in other words, more sociological than historiographic. Even if one recognizes that the academic tradition, for easily understandable reasons, has tended to incorporate a “value-relation” that has induced the far-easier acceptance of Friedrich Wilhelm as an object of interest than his tailor, there is nothing to prevent (except perhaps for some prejudice of Nietzschean genealogy) that the situation henceforth undergoes radical change with “Tailor X” becoming the main actor in the historiographic script (48-53). The same goes for the American Indians and the Bantu tribes, who, in

Max Weber's explicit opinion, were supposedly less worthy of study than the Athenians. But this is only due to a certain set of circumstances of the historiographic practice that Weber had abusively elevated to the status of tragic choice: "One does not prefer the Athenians to the Indians in the name of certain established values; it is the fact that one prefers them that makes them into values; a tragic gesture of unjustifiable selection would serve as a basis for every possible vision of history" (51). However, this is all abusive and rather illogical: "Weber, who was fundamentally a follower of Nietzsche [...] thus raises to the level of tragedy a state of historiography that was to reveal itself as very temporary" (Ibid.). Symmetrically, within the false continuity that is sociology, the real question is not to ask what the sociologists Durkheim and Weber have in common, "for they have nothing in common, but why the latter took the name of sociologist" (276), which is explained by his above-mentioned considerations on value-relations, and the corresponding abusive limitations of the historian's work.

Nevertheless, some will retort, does not this opposite academic trajectory (the attention paid to small "repetitive", general events) correspond precisely to sociology and its academic triumph? Not so, according to Veyne. In this other case, we are dealing with the practice of history, perhaps even excellent history, but certainly not a supposedly scientific sociology. In this regard, it might be assumed that Veyne would be able to agree in substance with Fernand Braudel and his proclamation of the fundamental identity of the work of the historian and the sociologist (1962, 88), or when he states that in the "long duration" these two subjects fields tend to merge (93), or that the defining limits of these disciplines are the same, so that the two apparently tend to fuse with each other (91). This fusion, Braudel added, would probably only be resisted if sociologists insisted on preventing historians from being historians of the present. However, this (merely corporative) reaction was something to be avoided (91). Veyne, however, is much more emphatic; and clearly far less diplomatic than Braudel. The alleged novelty in the academic panorama, sociology, is, in his view, mortally wounded from the outset by pointlessness. It may be history under a different label; it may be philosophy or literature; it may even be highly meritorious, but it is definitely not science. However, some may reply, is this field not capable of identifying the regularities and the patterns within the infinity of historical narratives? Does Veyne completely ignore "community" and "society", "status" and "role", "values" and "attitudes", "manifest function" and "latent function", "universalism" and "particularism", "functional requisites" and "needs-dispositions"? Does he really want to cast all this aside in denying these categories any heuristic value?

Here, we touch upon a crucial point. According to Veyne, as we have said, it is primarily history that has been written under the label of sociology. Indeed, whether the individual scholar is aware of it or not, the activities of a historian have an absolute need for a topic qua a group of means to define the material as well as to assist in its memorization. This has been precisely the focus of sociology's activities. However, any topic, whatever it might be, is important above all as an auxiliary device: the most important part of the historian's work is not the choice of topic, but rather the density and richness of the way in which he conceptually captures specific realities. Sociologists, in turn,

trained in the subtleties of their analytical framework, often try to hammer reality into categories, discovering or inventing “communities” and “societies” at every turn, or reducing realities to combinations of these “ideal-types”: x percent of “community”, so to speak, $(100 - x)$ percent of “society”, no more and no less. Hence, the real value of most official sociological works often lies primarily in those facets that their authors rank as secondary, sometimes in the openly artistic features, while the aspects that are officially considered to be of greatest importance turn out to be nothing more than schemes serving to simplify and “synthetize”. In fact, they are unable to truly explain and frequently slide into endless logomachies and self-crippling obsessions: sociologists often presume to find the “community” and its “values” wherever they look, just as the ancient Ionian physicists managed to identify “fire”, “earth”, etc. in everything they looked at (Veyne, 1984; 239, 279). Similarly, Parsons wants us “to consider society as Kant considered nature: as a work of art executed according to goals; he does not add, like Kant, that that finalism will never teach us anything about nature or society” (274).

Rules, avenging events, evasions (and rationalizations)

We must recognize a kernel of truth in the comments via which Veyne, in his own terms, disputes in sociology “the flag and not the goods” (271). Nevertheless, some facets of his reasoning seem questionable. First, is economics really a science worthy of the name? In that case, and beyond the mentioned aspects of its “formal elegance” and “praxeology”, should it not be predictive? In reality, though, the arguments stemming from economic science tend to configure narratives that constantly shift from situations of a simple duality (either we obey the rule or the famous avenging event is triggered, with the corresponding cost of non-compliance), to other situations where human history consists of moving on from a rigorous dualism to a constant triangulation, an endless “either-or-or...”. In other words, there are repeated cases where the rule is not obeyed while the agents nevertheless manage to avoid suffering the “vengeance of the event”, because in fact, it is possible to indefinitely postpone, escape and/or transfer the costs of non-compliance to others. And if, in fact, this game of transfers is susceptible to maintenance and the payment of these costs is postponed sine die, where does this leave the Veynean notion of a true “science of man” that is conceptually distinct from the level on which history operates? Is it not the case that all causality hereby becomes merely sublunary, and we are thus left with the pronouncement of mere retrodictions?

Let us consider some examples. According to “pure economics”, the productivity of factors and the utility of goods should decline marginally according to the quantities used... except, of course, for those cases in which they clearly do not decline. Here, in turn, we have the “economies of agglomeration” stepping in to explain economic growth, according to which the gap between rich societies and poor societies will tend to grow spontaneously, contrary to what is suggested by the logic of the decline of marginal productivities. Alternatively, we verify that international trade takes place primarily among

countries with similar production structures, but not only for vague “institutional” or “sublunary” reasons. On the contrary, the more sophisticated explanatory models clarify, by means of logical, strictly scientific coherence, referring to the demons hidden in the various possible details, such as the indeterminacy of the order, after which marginal productivity really declines. In fact, in such cases, whenever we baptize the apparently uncomfortable aspects with the name of an academic celebrity and label them as “paradoxes”, the room for ill-feeling tends to diminish, and each situation returns to an apparent normality: hence the “Lucas paradox” of course, but also the “Leontief paradox”, the “Kaldor paradox”, and so forth (Graça, 2012: 22-23).

Of course, we can follow a similar line of questioning if, for example, the famous independence of utility-functions is the focus of the discussion. Moreover, even the notion of interest that Veyne borrows from the so-called “Austrian school” of economics is not as rigorously insurmountable as he suggests. However, these issues are not our main concern. The crucial point is that Veyne’s fascination with the “formal elegance” of mathematical models (economic and others), which is visibly conveyed, for example, in his encomiastic references to the works of Nicolas Bourbaki (1984: 315, note), contains the clear risk of sliding off into a game of subtleties, in which (just like with the logomachies mentioned with regard to functionalist sociologists) the purpose is simply to find out in facts what have already been decided, and nothing else; and certainly no refutation. Given that subsequent “adjustments” are infinitely possible (marginal productivity does decline but only from a certain point on, utility-functions are indeed independent to the extent and only to the extent that they can be defined as such, etc.), what remains of the falsifiable character of the theories? What enables the maintenance of the “scientific” arrogance?

The actual history of “pure economics” over the last century demonstrates, as is widely known, an unstoppable trend towards mathematics essentially based on the justification, as suggested by Veyne and many others, that this is somehow advantageous given its supposed capacity to proceed with deductions and reach conclusions that would otherwise be unattainable through common language (in other words, to proceed with “synthetic a priori judgements”, in Kant’s terminology). However, in this regard, other commentators have also highlighted concerns about the merely complementary character that these procedures should acquire given the risks of “autistic” involutions imported through this recourse to mathematics (Marshall, 1964: XII; Mirowski, 1989). Still others, and probably with some reason, wonder about the effects of this inclination towards the cryptic and to apparent sophistication, often associated with fundamentally failed structures of argument, repetitive, and addicted to simplicity: might the Nietzschean maxim be true that those who navigate shallow waters tend to make ripples in order to convey the false impression of depth?

However, we must add a safeguard. While it may be indeed argued with some justification that economists tend to use mathematics in the same way that other professional groups cultivate their own exclusive jargon as “barriers to entry” that guarantee the production and maintenance of the aura surrounding the respective professions,

this still does not serve to offset the substance of Veyne's criticism of the effective practices of sociologists. Instead, perhaps we should take the completely opposite route, extending thereby to other academic fields this suggestion of reduction to the "sublunary" (and thus to history and the "Aristotelian" level) that he makes with respect to sociology. Consider, for example, Parsons' efforts to carve out a distinct academic niche for sociology. Beginning with his formulations in the 1930s which sought to establish a particular distinction from economics (research on the "ultimate goals" of action versus the study of the rational and peaceful uses of scarce resources), to the meta-theoretical aims characteristic of the 1950s and 1960s, when the concern about academic boundaries referred primarily to anthropology (Graça, 2008), it seems relatively reasonable to accept the fundamental validity of Veyne's criticisms of the tendency toward logomachies. Yet, what can we say about the campaigns, diametrically opposed in many respects, for the application to sociology of mental frameworks imported from "pure economics" (that is, the so-called "rational choice theory")? Is it not also true that here, too, we can easily find the materials for the construction of a libel based on argumentative circularity and/or the hammering of the facts in order to make them fit the explanatory frameworks?

Moreover, even admitting that sociology has primarily produced history under a different label, it would seem to make sense to apply to its existence as an academically recognized field a set of mental dispositions corresponding to what economists call "opportunity cost", what historians try to capture with the notion of "counterfactual", and what sociologists think they recognize with such terms as "functional equivalent" and "latent function". Briefly: should sociology not exist, what would the academic panorama look like, or what would have emerged in its place? If there are advantages in contrasting that which really exists with its absence, how can we evaluate the case of sociology? In its absence, this niche would probably be filled, but by what? Perhaps by a history less *événementiel*, which would also be more elastic regarding "value-relations"? An economics more inclined to institutionalism? A psychology more inclined to the social? An anthropology more open to modernity? A geography containing a more developed "human" component? A more eclectic form of demography?

One aspect seems reasonably clear: the "postmodern" inclination to reverse the process of specialization-differentiation, the relentless tendency toward "de-differentiation" and "undisciplinarity" typical of the last half-a-century (Anderson 1998), would have had the same effect. Correspondingly, discussions about the boundaries of disciplines would have continued, with as much uncertainty about the outcome as they do now. However, the effects of this "life without sociology" on its neighboring academic fields seem more debatable: would economics, for example, also tend to become more, or instead less open to institutionalism in its absence? Expressing this in another way, would there be an occupation of the same conceptual niche under a different label, or would the niche itself tend to disappear altogether? In such a scenario, would history tend to be less *événementiel* and biographical, or would these traditional traits simply be reinforced by sociology's departure from the academic landscape? Similarly, would psychology become

more social, or instead move closer to neurology and biochemistry? And analogously for all the other academic fields. In any case, it would certainly remain valid to apply to all these studies the derisory comments that Veyne reserves for sociology. According to the French historian,

“One sign does not deceive: to study sociology is not to study a body of doctrine, as one studies chemistry or economics; it is to study the successive doctrines of sociology, the placita of present and past sociologists. For there are reigning doctrines, national schools, styles of a period, great theories fallen into disuse, others that are sociology itself so long as the “big boss” who is its author controls access to sociological careers — but there is no cumulative process of knowledge” (1984: 277-278).

The patterns of history and the future

Actually, the history of sociology suggests in various ways that, rather than being a disciplinary field in crisis or linked in an umbilical way to the idea-motive of crisis, it actually represents a discipline that is itself the crisis, embodying at once crisis and the idea of crisis. However, the reasons for this are far more difficult to identify. They stem from a lack of awareness of the political assumptions implicit in the work of sociologists (Gouldner, 1970), passing through a consistent fixation on its own tradition, accompanied by the discrepancies between the discipline’s excessive ambition and its theoretical incapacities preventing it from being capable of identifying any law (Lopreato, Crippen, 2017), an excessive attachment to outdated methodological obsessions, that, over time, lead it to be increasingly overtaken in practice by various research studies that are productive on their own terms and dispense with any specifically supplementary sociological theorizing (Savage, Burrows, 2007) and, finally, to the very straitjacket that the imperative of expressing ideas in articles so well illustrates: “The article of 8,000 words is a good way of clarifying questions and solidifying small improvements but remains very limited when the purpose is to try and make some kind of declaration about the nature of society” (Graça, Marques, 2012: 22-23).

However, irrespective of such inquiries, it is undeniable that the sociological landscape of the last few decades has produced “empirical research” above all, and micro-theorizing as the age of the “grand theory” seems to have been left behind, possibly except for those academic celebrities able to generate momentary fashions, soon falling into oblivion upon the disappearance of the respective figures and those closest to their causes. Does this provide the reasons to concur with the pitiless diagnostic of Veyne above? Partially, perhaps. However, it should be noted from the outset that, in contrast to this author, for whom the absence of any true cumulative knowledge represents the logical crux of sociology’s poverty, from Max Weber’s perspective the opposite holds true: it is precisely cumulative progress (knowledge being perceived as tending towards the infinite) that rapidly annihilates the relevance of any academic research, rendering it impossible to die old and “fulfilled” by life, but only to be “tired” of it, as the German soci-

ologist so painfully diagnosed with regard to research activities, invoking Tolstoy and the Bible (Weber, 2004: 13). It is therefore exactly the scientific nature (not its absence) and the cumulative character of knowledge that would render sociological work worthy of a rapid fall into oblivion.

Whatever the case may be, it is equally undeniable that a set of problems and diverse theoretical stances have emerged in the field of historiography, which strongly suggest the need and the scope for the possible return of the “grand theory”. Economic history has proven to be a highly fertile terrain for this, through the work of Angus Maddison, Paul Bairoch, and the authors belonging to the so-called “California School”, especially Kenneth Pomeranz with his notion of the “Great Divergence”, as well as the series of correlate debates associated with the magna quaestio of eurocentrism and its repercussions. We can also note how ironic it is that historiography, originally biographical in approach, has, in the course of a long academic migration, come to claim this mega-theoretical and eminently nomothetic vocation,² just as sociology, on the contrary, has become increasingly oriented towards the minor registering of an ideographic inclination.

Within this other “mega-theorizing” approach, the exact opposite of Veyne’s radical skepticism is well expressed in a much more recent work that asserts the contrary, and thus the scope for setting down clear laws about the trajectories of human societies. I refer here to the boldly titled book *Why the West rules (For now)* and bearing the no less sweeping subtitle *The patterns of history and what they reveal about the future*, published about a decade ago by the British historian and archaeologist, Ian Morris. The book became a bestseller with its purpose to delineate a multi-millennial contest between a civilizational entity called the “West” (which slowly migrates from the Fertile Crescent to southeastern Europe, northwestern Europe, the British Isles, and then to North America) and another entity generically called the “East”, but in fact representing China. Among other aspects, Morris advocates for an essential equality in the capacities of all large human groups. This is due to biology decisively conditioning all societies in essentially the same way. After considering the biological questions, there is the need for the historian to allow the sociologist to appear on the scene for a moment, Morris affirms, deploying this term in a declarative way as an abbreviated designation for all the social sciences. He adds that his purpose is to refer to “the branches that generalize about how all societies function rather than those that focus on differences” (2011: 27, note). A nomothetic inclination, therefore, far more than an ideographic one, which is easily understandable in a work that openly sets out to detect patterns.

The sociologist, clarifies Morris, informs us about what causes social changes, as well as about what these changes subsequently produce. Is there an identifiable ‘catalyst’ for such changes that enables the crucial separation of the human condition from that of chim-

2. As soon as in 1971, in his review of Veyne’s book, and amidst various other relatively minor issues, Raymond Aron ends up raising what is arguably the central question. For Veyne, “history has no big lines” (“L’histoire n’a pas de grandes lignes”, Aron, 1971: 1353). But this idea, according to the reviewer, is contradictory to what Veyne himself often assumes, and besides, also to the sound common sense: obviously, history cannot be a mere kaleidoscope. Aron has arguably gone for the jugular of the problem here.

panzees, despite the unquestionable intelligence of these and their renowned tool-making capacity? Morris believes that there is, basically following the science-fiction writer Robert Heinlein's suggestion that "progress is made by lazy men looking for easier ways to do things". Morris adds/corrects that this "Heinlein Theorem" is only partially true, because, in fact, "lazy women are just as important as the lazy men, sloth is not the only mother of invention, and "progress" is often a rather upbeat word for what happens" (27-28). However, he then details and confesses, reduced to its kernel, this conception of social change may well be, when all is said and done, the very best that we are ever going to find. The simultaneously expanded and smoothed version would therefore correspond to the "Morris Theorem" that states "Change is caused by lazy, greedy, frightened people looking for easier, more profitable, and safer ways to do things. And they rarely know what they're doing". Morris adds that "History teaches us that when the pressure is on, change takes off" (28).

Consequently, here we encounter the trend, already mentioned above, for the systematic triangulation of problems by avoiding and overcoming dichotomies where both terms are unacceptable. This side-stepping, this permanent evasion, this disobedience to the norm which can at the same time avoid the vengeful event since we become capable of reshaping the previous basic formulation of problems (hence inducing "structural" changes, to use the economists' jargon), thus progress, if such a notion still makes any sense when we have learned to thoroughly distrust any teleology or any meaning/purpose of/in history and its respective changes. Its invariable origins are conflict, tension, and difficulties, but also the correlative goal to overcome them. *Polemos* is thereby ascertained, in Morris' mental framework, as the true "father of all things", but this derives from the clash between the harshness of the environment and the cunning of that simultaneously lazy and disobedient primate that is the human being. This game of skill and ingenuity between the harsh environment, on the one hand, and laziness, greed, and fear on the other hand, induces a system of displacements or successive triangulations in the effort to disobey while at the same time avoiding paying the cost of disobedience. This, in essence, what is called "progress" or, at least, "social change".

As for the distribution of the credit for such inventiveness throughout the social corpus, Morris takes an overwhelmingly egalitarian point of view. Large masses of human beings under similar circumstances systematically tend to arrive at levels of creativity and achievement of an equally similar nature, whether in comparing different societies or in considering the ambit of each one. Morris thus aligns himself with a tendency towards geographic determinism that can also be seen, for example, in the work of Jared Diamond to whom he often appeals while simultaneously emphasizing the opposite causal nexuses (human societies significantly retroacting in their geographic environments). Stripped of its traditional biographic inclination, history, through the "law of great numbers", thus veers unstoppably towards sociology; but this same movement irresistibly induces both to enter the (now apparently wider) orbit of geography. The humorist Edmund Bentley had jokingly pointed out in 1905 that, if the art of biography was "about chaps", geography would in contrast be "about maps". Morris wholeheartedly agrees, but the traditional British "chaps", in the sense of "upper-class men" would, in the meantime, have found

their group open to countless “honorary chaps”, particularly “women, lower-class men, and children” (29), thereby producing a far more interesting choral polyphony. Having said that, Morris ventures a theoretic great leap forwards: “once we recognize that chaps (in large groups and in the newer, broader sense of the word) are all much the same, I will argue, all that is left is maps” (Ibid.).

The so-called “subjective factor”, or the “role of the individual” in history, he adds and specifies, is not eliminated from the conceptual framework. Morris basically contrasts the ideal-type of the intelligent leader with what he calls the “bungling idiots” who, in truth, proliferate to a far greater extent in universal history. These bungling idiots, he notes, may just as well be societal leaders as ordinary, anonymous people. Nevertheless, even when distinguishing great men from idiots, there are profound reasons for doubting the decisive importance of whether it is ones or the other that make the most crucial decisions for society. Morris tends, resolutely, towards the weighting of the large number, the statistical and the anonymous: “great men/women and bungling idiots have never played as big a part in shaping history as they have believed they did” (616). The scant value of individual factors is complemented by the low importance attributed to cultural differences in the face of the overwhelming weight of geography: “latitudes, not attitudes”, as he also wrote (Morris cit. in Duchesne, 2011: 11). This downplaying of cultural differences, this lack of sensitivity to cultural characteristics, especially western cultural characteristics, provoked disappointment and shock in his reviewer Ricardo Duchesne, who confessed that he was deeply offended by this merely “anthropological” approach to western culture. From the anthropological point of view, Morris writes, the history of the West appears as a mere example of more general patterns, and devoid of any uniqueness or exceptionality;

“The key word here is “anthropological”. Anthropology studies the repeatable behaviours of large numbers of faceless people, and, as such, it is a discipline which has been effectively set against the elite culture of the West. From the perspective of what thousands and millions of humans do routinely to survive — the energy they consume, the tools they have, the fertility of the land — the achievements of singular individuals seem trivial. “Humans are all much the same wherever we find them; and, because of this, human societies have all followed much the same sequence of cultural development. There is nothing special about the West”” (Duchesne, 2011: 10).

Morris then proceeds to draft a list of analytical patterns of supposedly universal validity that allow for the comparison and measurement of highly different civilizations. In practice, what he adds to the tradition of geographical determinism is above all a conceptual grid for measuring progress, an “index of social development” (4), constructed by applying a group of criteria corresponding to the dimensions of energy capture, organization/urbanization, information and war making capacities (Morris, 2011: 135-71, 623-45; 2013: 1-6). Based on this “index of social development” he contrasts the millennia of development of the West and China, indicating the most important turning points in their respective trajectories. We can repeatedly verify the existence of periods of civilizational advances alongside periods of

stagnation and retreat, although there remains an extremely long-term trend for the civilizational level of each society to be higher than those of its predecessors.

Along their historical paths, according to Morris (223-226), these civilizations were repeatedly threatened by what he calls the five “Horsemen of the Apocalypse”, climate change, famine, state failure, unstoppable migration, and disease. The ability or inability to deal with these problems is a crucial factor in determining whether a society is on its way to a higher level of civilization or, on the contrary, heading towards a disastrous period of collapse and “dark ages”. Finally, we would like to add that Morris, for whom these crucial questions regularly interfere with the direction of the march of humanity, including contemporary societies, also concludes with the inevitable and imminent ending of Western global hegemony. Meanwhile, he extends his opinion much further regarding the crucial emergence of an already “post-human” reality (appealing to the notion of androids, a human-machine hybrid) as a necessary condition for the very survival of the species, both in terms of the risks of warlike conflicts and related state-failures, and in terms of unsustainable levels of resource consumption, especially energy. Only the decisive turn to the “post-human” might, according to Morris’ view, save humanity (582-622).

Europe and the privileges of backwardness

Similar efforts, with considerable attention given to non-European realities coupled with a careful consideration of the importance of geographic or environmental factors in the evolution of societies, permeate the works of various other authors. In addition to the already-mentioned Jared Diamond, on whose work Morris partly relies, James Blaut, John M. Hobson, Sugata Bose, Giovanni Arrighi, Phillip Hoffman, and, finally, Alexander Anievas and Karem Nişancıoğlu should be mentioned here. Diamond (1999, 2005) has brought into the foreground the importance of geographic, epidemiological and military factors, often neglected by socio-historical studies, as well as the real scope for the occurrence of mass civilizational refluxes, including entire societal collapses. According to Blaut (2000), the fundamental reason for European hegemony and the advantage it gained over China resided in Europe’s relative proximity to the Americas, and its partly fortuitous encounter with this vast landmass which freed Europeans from the typical Malthusian limits to growth, providing them with almost unbounded territories and resources, the use of which was later optimized by recourse to the mass enslavement of Africans. These turn out to be the fundamental leverages of European supremacy, definitely not any fantastic “superiority” or “exceptionality” whether cultural, political, or otherwise.

Hobson (2004), in turn, set out the issue of Europe’s debt (cultural, scientific, economic, etc.) to Asia, especially China, as his central research theme. Meanwhile, Bose (2006) focused his research attention on the close dependence of Britain’s success from both its military domain of India and the correlative crude economic exploitation of this subcontinent. Arrighi (2009), on the other hand, speculates on the possible emergence in contemporary China of a social model capable of blocking the “financializing” dimension that has characterized the various previous cycles of capitalist hegemony, simultane-

ously retaining in official political powers the effective control over events, and releasing (more than any capitalist society has achieved to date) the creative economic energies that correspond to market dispositions. Phillip Hoffman (2015) asks “why did Europe conquer the world” before providing an answer based on military aspects. In this case, fundamental importance is attributed to the idea of a “tournament” that encouraged various potential rivals to engage in incessant technical improvements, yet a certain amount of isolation occasionally proving advantageous. Hence, for example, Russia’s supremacy in Europe. The variety of enemies to be confronted can induce technological path-dependencies that end up being harmful, as would have been the case in traditional China, where the usual conflicts with the horse-riding archers from the steppes of Central Asia supposedly kept the Chinese from a systematic military use of gunpowder.

In this group of works, the book by Anievas and Nişancıoğlu (2015) stands out. Officially presenting it as a work of historical sociology, they openly assume that Europe was a mere “periphery” politically, militarily, economically, and culturally until very late, when a chain of events took place that allowed Europeans to benefit from the advantages frequently associated with the “privileges of backwardness”, this in a global panorama characterized by various mutually connected paces and interchanges of “unequal and combined development”. Accordingly, they explain that their work seeks to systematically establish a schematic framework incorporating what they posit to be the key theoretical concepts: “unevenness and combination — from which the ‘whip of external necessity’, ‘privilege of historical backwardness’, ‘advantages’ and ‘penalties of priority’, ‘contradictions of sociological amalgamation’, and ‘substitutionism’ necessarily follow” (44).

They draw recognizably on Trotsky for the notion of “uneven and combined development”, a concept that plays a truly central role in their argument. This argument, at least, has the clear advantage of being fairly easy to follow, as Michael Mann notes in his review of the book. “Societies with very different cultures and practices of social reproduction interact with each other culturally, economically, politically, and militarily and these different combinations produce social change. Who could argue with this? It sounds very Weberian” (2017: 4). Having said that, however, the most difficult facet remains identifying the interactions that truly matter.³ This model, it should be added, can be applied to

3. It would be very difficult to discuss how much Anievas and Nişancıoğlu are really presenting a theoretical novelty, or mostly just reprocessing what others had already advanced. For example, the idea of ‘combined and uneven development’, posited to be an important theoretical acquisition by Trotsky, might instead be presented as an idea of the Indian 19th century economist Dadabhai Naoroji. India, argued Naoroji, should not be considered a ‘backward’ country (although obviously it was poor), and Britain a ‘developed’ one. Instead, Britain’s ‘development’ and India’s ‘backwardness’ were arguably the two sides of the same ‘international’ reality. In a clear disagreement with one of the basic tenets of mainstream Marxism until the early 20th century, Britain was certainly not showing India its future (as in Marx’s famous “*de te fabula narratur*”). However, on the other hand, Naoroji, being a loyal British subject, tried to present the factual reality of his time as an “un-British” aspect of British policies; hence his book’s title, *Poverty and Un-British Rule in India* (1901). The theories of this period that oppositely took imperialism as a crucial device in international relations are to be referred mostly to John Hobson’s *Imperialism: A Study* (1902), and later, with a (strongly heterodox) Marxist wrapping, Vladimir Lenin’s *Imperialism, the Last Stage of Capitalism* (1917).

different European regions as well as to intercontinental relations. Regarding the latter, Anievas and Nişancıoğlu declare to have identified:

“‘privileges of backwardness’ in Europe against the ‘penalty of progressiveness’ of the more advanced tributary empires of Asia [...]. They have also proceeded beyond the normal Marxian focus on the economy to include as determinants all four sources of social power [ideological, economic, political and military, according to Mann’s own typology], although the main dependent variables remain various ‘assemblages constitutive of capitalism.’ I had some difficulty in understanding what they mean by capitalism” (Ibid.).

Anievas and Nişancıoğlu, like the authors of the so-called “California School” of historiography, especially Kenneth Pomeranz, underline the external component to the causes of European domination. However, simultaneously and contrary to the tendency of many others, they also emphasize the allegedly-deep historical reasons for the European rise. Indeed, while on the one hand they believe it necessary to extend the analysis of the West’s emergence to conditions and determining factors with origins outside of Europe, precisely “in order to dislodge the familiar Eurocentric claims of some innate European dynamism” (2017a: 10), on the other hand, there are also what they consider to be structural features, specific to late medieval and modern European history that deserve greater attention, as they provide important insights for understanding Europe’s advantages, whether in warfare or in the economy. Thus, they emphasize that “the decentralized and politically fragmented nature of European feudal relations” (11) would have produced a particularly competitive and aggressive interstate system.

Indeed, this high level of conflict in the European multi-state system is often cited as a crucial factor in the standard literature on what is usually termed as the “‘rise of the West’, especially among neo-Weberians who cling to a ‘geopolitical competition model’ of development” (11). However, in the opinion of Anievas and Nişancıoğlu, this geopolitical competition should be approached not in isolation, abstracted from the nature of the interacting societies, as so frequently occurs in “international relations” studies, which see “the role of geopolitical competition as a kind of Darwinian selection mechanism sorting out the weak from the strong” (Ibid.), but articulating the “international” dimensions of processes by means of a sociological analysis of the social formations in interaction, thereby enabling the detection of the truly relevant specific characteristics of each of the resulting “amalgamations” or “conglomerates” (cf. Anievas, Nişancıoğlu, 2017b; 2018).

As they make abundantly clear in their book, this alleged European specificity, which made the respective history uniquely violent, finally providing Europeans with advantages in confrontations with third parties, is posited as a distinctive feature of European feudalism, especially when confronted with the so-called “tributary mode of production” (Anievas, Nişancıoğlu, 2015: 96); or, to be more precise, when the Ottoman Empire is compared with the typical attitude of European powers. In contrast to the consistently territorial and agricultural orientation of the former, the latter were much more “explicitly focused

on bringing commercially valuable territories under direct conquest and political control for specifically [...] economic purposes” (105). The trope of European singularity, usually formulated in cultural terms, thus ends up reappearing in the discourse, but in this case, it is carefully relegated to the supposedly distinctive facets of the “feudal mode of production”, arguably more promising for the future capitalist expansion if compared with the “tributary mode of production”. This an aspect under which this work can easily be approximated to those of Perry Anderson (1974a; 1974b), whose tutelary influence is indeed easily perceivable.

Still furthermore, attempting to overcome the mere “East versus West” dichotomy, Anievas and Nişancıoğlu operate an enormous historical zoom right back to the ancient civilizational distinction/opposition between nomadic and agricultural peoples, which culminates in a lengthy consideration of what they see as the legacy of the Mongol Empire. “They draw from recent revisionist history of the Mongols which sees them as rather nice people who left major legacies for the world”, wrote Mann (2017: 4), half-jocosely, regardless of the fact that he mostly accepts the well-foundedness of this attitude, and also recognizing the truth that the Silk Road, from China to the Black Sea, along which Asian-European trade could flourish, was really protected for some time by the *Pax Mongolica*. This undoubtedly constitutes a legacy worthy of a generally positive assessment; but it was not always so, Mann adds cautiously.

Anievas and Nişancıoğlu, we would like to emphasize here, arrive at this point through an attitude of appreciation of the influence of the nomadic peoples on the agricultural civilizations of the Eurasian peripheries, in direct opposition to the basic theses of Morris. For Morris, the nomads generally brought the “Horsemen of the Apocalypse” with them, as we have seen above, and a higher level of civilization obtained by the societies on the Eurasian peripheries could only be realized inasmuch as it became possible after the decisive entente reached by the Russian and Chinese empires in Nerchinsk in 1689. It provided for “closing the steppes”, thereby definitively solving the military problem that the nomadic horsemen had repeatedly posed (Morris, 2011: 455-459) while the Europeans were resolutely turning to the domination of the seas. The same intellectual movement thus renders Anievas and Nişancıoğlu, on the one hand, prone to a positive evaluation of the role of the nomadic pastoral peoples while, on the other hand, they tend to deny the scale of the influx that the Europeans received from China. In contrast, Mann, basically in line with John Hobson’s thesis, argues for the positive civilizational influence of China, which never actually ceased to be felt; even in Europe, this backwards geographic periphery that was left relatively isolated by the invasions of the nomads from the steppes.

Concluding observations

Let me provide a balance of what has been written. Acknowledging the basic validity of Paul Veyne’s initial thesis that sociology is basically history under another name, or an auxiliary discipline to it, it seems reasonable to immediately add that the tendency towards the nomothetic (usually taken as a proxy for science itself) constitutes a prac-

tically unavoidable feature, in fact, a truly overwhelming dimension of the intellectual trajectory of contemporary societies. This inclination towards the nomothetic is additionally accompanied by a shift of attention away from not only “great events”, but also from “great men” and “more developed” societies in favor of general/repetitive facts, the crowds, the “common people”, and/or statistical aggregates and, finally, non-Western societies themselves.

Within this framework, and with due recognition of the merely “sublunary” character of the causal relations identified therein, we should nevertheless add that what Veyne takes to be a group of praxeologies, especially “pure economics”, is in truth no more than an equivalent of sociology, that is, a “way of seeing”, and also a way of mentally mapping (a topic), a perspective that supposedly allows for a certain range of analysis while simultaneously (and perhaps inevitably) inhibiting others. It is always reduced to this level and thus incapable of returning anything other than probabilistic or statistical causalities resulting from the respective mental frameworks (intended to be) endowed with logical coherence, and thus usually issuing only mere retrodictions. In other words, we can finally ask whether, and to what extent, the mental apparatus of “pure economics”, which Veyne classifies as a scientific “praxeology” based on core ideas about scarce resources, the trade-offs between different resources and different objectives, the temporal discounting associated with interest, decreasing marginal productivity and utilities, individual and independent utility-functions, but also including the transitivity of choices, generates a decisive empirical relevance more than any other intellectual framework, especially that typical of sociology based on notions such as the supposed regulation of social practices through cultural values, the scenic universal metaphor, and so on. In other words, we should recognize that “the Spinozist dream of the complete determination of history” is really just this and only this, a dream; however, such an assertion cuts in all directions: including that of sociology, certainly, but also incising into “pure economics”.

Hence, the corresponding necessity to accept the universal reduction to the “sublunary”. All these academic disciplines (economics, sociology, and every other “social science”) are merely supporting subjects for the infinitely expandable writing of the great “novel of humanity” that is history/historiography⁴ — which, in the last decades, has

4. Although Veyne disputes the very notion of social sciences, this does not mean he favors abandoning its entire idea in exchange for simple literature and comparative literary studies, in line with the project that was promoted by American pragmatists like Richard Rorty, wherein fiction, evolutionary biology, and continental philosophy should form some vague amalgamation whose aim would be to narrate (in an ironic way) the contingency of human existence. There are various important aspects that make Veyne’s endeavor rather different from (and indeed incompatible with) Rorty’s. Veyne’s book is “an essay on epistemology” (its subtitle), and it really is about discussing the theoretical assumptions of the procedures of various disciplines, all done in a rather traditional philosophical way. The notions that, for example, we should ‘abandon the idea of knowledge as an exact representation’, or that culture should not ‘be dominated by the ideal of objective cognition, but by the one of aesthetic elevation’ (as in Rorty’s *Philosophy and the Mirror of Nature*) are rather alien to him. Unlike Rorty, Veyne would certainly not praise Wittgenstein, Heidegger, or Dewey for having ‘annihilated epistemology and metaphysics as possible disciplines’. For Veyne history is indeed a novel because it is intrinsically unpredictable, unpredictable beyond appeal -, but a true novel nevertheless: definitely, not a

revealed the return of an immense appetite for “grand narratives” that in times not so distant were considered totally disposable and even avoidable. The field of historiography rather than sociology (with the possible exception of the so-called “historical sociology”) has apparently been better able to respond to this will for grand narratives, given the theoretical dead ends into which the discipline of sociology tends to drag itself, and its intimate intellectual malaise which it often avoids by taking refuge in a repetitive (or even neurotic-compulsive) inclination toward small stories, monographic sub-theoretical research, and short essays. Given this panorama, the mentioned works of Morris, and Anievas and Nişancıoğlu undoubtedly deserve applause, and are worthy of academic signposting, especially in view of their officially trans-disciplinary characteristics and their goals of a consciously non-Eurocentric orientation. However, we should emphasize that this does not enable them to overcome their intrinsic logical problems, or the difficulties of conceptual mapping described at the beginning of this article.

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fiction or ‘fake news’. A narrative, yes; but a factually true narrative. This holds entirely, notwithstanding the fact that the book’s title in the French original version, “Comment on écrit l’histoire”, is probably inspired by the comments of the Russian general Nikolay Raevsky on the Battle of Saltanovka, and particularly regarding the presence or absence of his two sons in a famous Russian attack. A rumor was by then produced that the two boys were present, but Raevsky himself wrote otherwise stating the opposite, and adding in disgust: “And that is how history is written!” (“*Et voilà comment on écrit l’histoire!*”). Historiographical criticism, however, is supposedly capable of endlessly chasing lies and misunderstandings, thus reestablishing the factual truth.

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Писать социологию — писать историю

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В 1971 году Французский историк и археолог Поль Вен выдвинул свои аргументы в пользу того, что казалось ему фундаментальным отсутствием собственного предмета у социологии. Данное академическое поле и не могло быть наукой, в лучшем случае лишь служанкой историографии, которая сама лишена какого-либо научного статуса, поскольку оперирует «подлунными» причинными связями, позволяющими делать только «ретроспективные оценки», а не предсказания. И наоборот, представлялось возможным определить набор «праксеологий», составляющих ядро будущей науки о человеке, радикально отличающейся как от социологии, так и от истории, и включающих в себя чистую экономику, исследования операций и теорию игр. Поэтому, если история и социология будут неизбежно носить «аристотелианский», т. е., неточный, «подлунный» характер, то научные дисциплины могут и должны быть «платоновскими», стремясь к формальной и логической элегантности.

Вен был прав лишь отчасти, поскольку экономику также нельзя считать наукой в строгом смысле. Многоуровневый кризис, в котором, по мнению многих, пребывает социология, позволяет нам сопоставить эту ситуацию с относительно недавними важными тенденциями появления социо-исторических «больших нарративов», иногда официально именуемых историей, и не столь часто — исторической социологией, но, так или иначе, имеющих трансдисциплинарный характер. Цель настоящей статьи — преодолеть ограничения, связанные с биографическими, элитистскими и европоцентричными искажениями, характеризующими традиционную историографию. Задачи подобных исследований можно в целом назвать номотетическими, однако устанавливаемые ими «законы» в лучшем случае весьма приблизительны. Поэтому они, как и экономика, обречены действовать на простом «аристотелианском» уровне, а значит великой «повести человечества» суждено остаться неопределенной в самой своей сути.

Ключевые слова: история, социология, экономика, подлунная причинность, праксеологии, «большие нарративы», европоцентризм.