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Writing Sociology at the Beginning of the Twenty-first Century

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Abstract

Paul Veyne has suggested in 1971 that Sociology lacked a study object. Three quarters of a century after Durkheim's *Rules*, it had yet to discover social types and orders of preponderant facts. At any rate, Veyne claimed, since Sociology or at least sociologists exist, we must conclude that, under that label, they do something else. Briefly, besides studying the logical conditions of Sociology, we should also sociologically consider it, as well as other neighbour and potentially rival disciplines.

In this paper it is argued that, contrary to other scientific fields, Sociology lives in an environment of permanently renewed crisis. Different authors and traditions have indeed asserted exactly that, while based on entirely diverse assumptions. In order to justify the characteristic traits of today's crisis, we try to list some of the little demons that have contributed to the current situation: 1) The hagiographic syndrome; 2) The isomorphism defence; 3) The acceptance urge.

Keywords

Sociological theory, economics, history, crisis, hagiography, isomorphism, recognition

JEL: A14, Sociology of Economics

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Como se Escreve a Sociologia no Começo do Século XXI

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Abstract

Paul Veyne sugeriu em 1971 que a Sociologia não tinha um verdadeiro objecto de estudo. Três quartos de século depois das *Regras*, observou, aquela ainda não teria descoberto nem tipos sociais nem factos preponderantes. Em todo o caso, sustentava Veyne, dado que a Sociologia existe, ou pelo menos os sociólogos existem, temos de concluir que debaixo dessa etiqueta se faz realmente algo de diverso. Em suma, para além de investigarmos as condições lógicas da Sociologia, deveríamos também considerá-la sociologicamente, procedendo aliás de forma análoga para as disciplinas suas vizinhas e potencialmente suas rivais.

Neste artigo defende-se que, ao contrário do que acontece noutros domínios científicos, a Sociologia vive num ambiente de crise permanentemente renovada. Diversos autores e correntes, com base em diferentes assunções, defenderam precisamente essa ideia. De modo a explicitar os traços característicos da presente crise, procurámos neste artigo elaborar uma lista de pequenos demónios que contribuíram para a presente situação: 1) A síndrome hagiográfica; 2) A compulsão do isomorfismo; 3) A urgência da aceitação.

Palavras-chave

Teoria sociológica, ciência económica, história, crise, hagiografia, isomorfismo, reconhecimento

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Writing Sociology at the Beginning of the Twenty-first Century

Writing Sociology

In his work *Writing History* (published in 1971, but written in 1969-70), the French historian Paul Veyne formulated the problem of what he considered to be the “lack of object” of Sociology by mentioning Durkheim’s *Rules of Sociological Method*: in order for Sociology to be possible, he wrote, “the present must not be only what the past has made of it; it cannot be, no matter what, at the will of antecedents, but it must always have its own structure. It must resemble an organism rather than a kaleidoscope” (1984, p. 269). Happily, and according to Durkheim’s reassuring opinions, that condition is respected: we have the “social milieu”, defined by volume and density, exercising a preponderant influence over other concomitant facts, allowing us to think in terms of anatomy and types, authorizing the establishment of true relations of causality. Sociology could therefore consistently aim at being “a sort of biology of societies” (1984, p. 270).

“Three-quarters of a century have passed since those beautifully lucid pages were written”, Veyne added sardonically. Indeed, in face of the unavoidable facts that Sociology has never since discovered either social types or orders of preponderant facts, it would be necessary to acknowledge that the “historians’ nominalism” is well founded and conclude in favour of a fundamental lack of object for the aforementioned field. Still, Veyne finalized: “since it exists, however, or at least

sociologists exist, it is because the latter do under that name something other than sociology” (Veyne, 1984, p. 270).

Four decades have faded since these highly spirited pages were written and it seems about time to try to make a balance as to what there might be of true or false in them. According to Veyne, to the basically non scientific nature of the historian’s procedures, we ought to oppose a set of “praxeologies” — defined more by their own internal coherence and their appeal to a hypothetic-deductive method rather than by any exact capacity to predict any order of facts — which could be properly set in order under the generic designation of “sciences”.

Of course, in concrete terms and according to the same Veyne, since the circumstances and their logical components multiply, potentially up to the infinite, what really happens may depart significantly from that indicated by models. Still, even so, that would be no reason enough to dispute their scientific status. A certain number of academic practices satisfy Veyne’s criteria of science, which, as can easily be inferred, have much more to do with “formal elegance” than with any kind of (proclaimed or genuine) adherence to “empirical” reality. If agents do not behave as *hominis economici* nothing decisive is to be found there. What really matters is that, inasmuch as they stray from what the model prescribes, they can do so only with an unavoidable cost: according to Veyne, sooner or later the event then “avenges” the theoretical model disobeyed as an expression of free choice in human action — or, with the same result, of the endless multiplicity of its effective determinants.

Under the category of human sciences, or “praxeologies”, Paul Veyne puts aside a relatively small group of academic disciplines, obviously including “pure” economics: ordering scarce resources, naturally considering a multiplicity of goals and

under conditions of decreasing productivity and utility of the various resources and goals, always admitting a certain amount of possibility to substitute them (the “opportunity cost”). As to agent preferences they can be no matter what, since Economics never intended to investigate their origin or nature, of course only postulating the transitivity of choices. From this relatively small branch of conditions Veyne excludes, and conveniently so for him, the celebrated independence of utility-functions that had already provoked so much discussion prior to his time and ever since. However, he does include the principle of time discount of values associated with interest, discovered and theorized by Böhm-Bawerk and to which all concrete realities, regardless of the nature of property, must conform if the aforementioned “revenge” of events is to be avoided — a fact, we are informed, that even Soviet economists were forced to recognize and incorporate into their calculations, if belatedly and against their will (Veyne, 1984, p. 248).

Not only “pure economics” is deemed worthy of scientific status. Chomskyan linguistics — revolving in its typical quarrels between semantics and pragmatics, and to which the real problem is not the possible practical relevance or irrelevance of any given linguistic system, but the very fact of the existence at all of systems of language gifted with logical coherence — seems a good candidate to satisfy the conditions of “formal elegance” that enable it to obtain approval by Veyne’s criteria. As indeed it does.¹

¹ “Great efforts have been made to try to answer the objection «How can you construct a grammar without appealing to sense?» And yet the question is in itself badly put, since the postulate that one evidently can construct a grammar by appealing to sense is not justified by any effective realization... The true question that it should have been raised was this: «How can you construct a grammar?»” (Chomsky, apud. Veyne, p. 323, footnote).

The same goes for the philosophical speculations that Kant called “practical reason”: these are all about obtaining, so to speak, the logical quintessence of any moral action regardless of the intentions of agents, not about investigating the greater or lesser adherence of such a scheme to factuality or attempting to discuss whether the real motivations stemmed more from this or more from that dimension. This may be very interesting precisely from the perspective of a “History of morals”, but brings anew nothing relevant concerning knowledge as to the deep logos of morality. Indeed, that constitutes basically a sideshow to a scientific activity considered to relate closer to Plato than to Aristotle, according to Veyne’s own words (1984, p. 252).

Within the group of the so-called “praxeologies”, he very much insisted on including that which we usually designate as operations research and game theory — and indeed the then fresh “prisoner’s dilemma” scheme seemed to fascinate him with particular intensity. Veyne does not discuss the extensions and ramifications of that dilemma: “battle of the sexes”, ultimatum and dictator games, etc., of course much less considering the so-called “liberal paradox” of Amartya Sen, which was developed only latter. Although he refers appraisingly to Kenneth Arrow, he also fails to elaborate on his “impossibility theorem”. Furthermore, he clearly does not consider the group of cogitations associated with what is known as “network theory”, particularly the notion of small-world networks, which have since become so famous and with such vast fields of application, indeed probably transversal to the generality of what are usually recognized as the human sciences.

At a totally different level from this putative core of a “science of man”, besides being fundamentally at the project state, Veyne thinks of the historian’s practice as concerned with “empirical” reality and allowed to generalise or individualise to a

greater or lesser extent, but always escaping the abyss of *individuum est ineffabile* inasmuch it considers each concrete case, each “event”, as the product of a series of determinations — an endless series of determinations, to be sure, but at any rate susceptible of being referred to an analytical scheme endowed with an approximately general validity. That is to say, the subject-matter of the historian’s activity would be *the specific*, not so much the particular or the individual. But this specific, this “event”, may obviously be built in order to conglobate degrees of validity very different from each other: from the “History of the Battle of Marathon”, if you will, unto the “History of War”. All that may be considered as the matter of History. All that comes out of a “sublunar” causality that identifies with the weakness of determinations concerning each logical order of facts, when it is about intermingling and crossing all of them, and therefore effective causality is precarious or appears as such, without still dissolving itself completely. All that is susceptible of being the fulcrum of analysis, the subject of interest, more or less *événementiel* as it may be, inasmuch as the degree of generality grows and statistical regularities end up by imposing their weight.

And yet, even in the apparently less *événementiel* of cases, we do not leave the closed sea of historiography to enter into one of some (any) science of man, dubbed Sociology or anything else. Specifically under this name, says Veyne, one has fundamentally produced either philosophy (mostly political philosophy); or contemporary History: non-*événementiel* contemporary History, of course, but no less History for that motive; or finally a genre of literature basically akin of what was in the 17th/18th centuries associated with the “moralists”, that is, one drawing its value mainly from its inherently aesthetic qualities.

These activities, true, do not engage in mutual recognition as such, but according to Veyne it would be fully advantageous from the point of view of the clarification of ideas that they did. History, for instance, is generally defined on too narrow a basis: hence the “History of France” thinks of itself as History while the “city across the times” tends by opposition to represent itself as... Sociology, precisely. And yet it is an error to make the creation of “niches” or “vital spaces” for two allegedly different academic disciplines depend on some criterion of setting of “facts”: it is substantially the same kind of activity. In the same token, it is also false that a study on Emperor Friedrich Wilhelm be properly History, or that Friedrich Wilhelm be worthy of entering History immediately, as such, whereas his custom-tailor would enter History only indirectly, via his connection with the star, or included in the general category of custom-tailors, which would make his study less “individuating” than “generalizing”... or, in other terms, more “sociological” than “historical”. In spite of recognizing that tradition, for easily understandable reasons, has incorporated a “relation with values” that induced it to tend to make its object of interest more easily Friedrich Wilhelm than his custom-tailor, the truth is that nothing prevents — except maybe, suggests Veyne, a prejudice of Nietzschean genealogy — that the situation henceforth undergoes radical change, the “Custom-tailor X” starting to be, under certain sets of circumstances, the main star in historiography’s script.

But, some will reply, is that not precisely Sociology and its academic triumph? According to Veyne, in this case we are still facing History, indeed probably good or even excellent History, yet still in whatever case not some allegedly “scientific” Sociology. But is the latter not capable of identifying regularities, patterns within the infinitude of historical narratives? Does it not know, or perceive to know, the

“community” and the “society”, the “status” and the “role”, the “values” and the “attitudes”, even the “manifest function” and the latent function”, not to mention the “ascription” and the “achievement”, the “universalism” and the “particularism”, the “pattern maintenance” and the “integration”, the “goal attainment” and the “adaptation”? Does Veyne intend to throw all that away, to deny its heuristic value?

Exactly here we touch a crucial point. According to the French historian, as already seen, under the label of Sociology one has indeed often made History. However, the reality is that, whether or not aware of it, all of the historiographer’s activity has absolute need of a topic, which decisively operates as a propitiator in setting down materials and as an aid to their memorization. In fact, it is precisely as to this aspect, the construction of a topic, that Sociology has mostly been concerned with. Topic, however, any topic, is still worthy mainly as an auxiliary: the *major et melior pars* of historiography’s work is not herein found, rather in the density and wealth of the capture of specific realities (both in what they have of the predominantly singular and that more susceptible of generalization) with which it has become associated. Correspondingly, sociologists understandably tend to resemble researchers who, obsessed with the intensity of perception of the novelty they believe to have identified, trained in the conceptual finesses and subtleties thereby created, often intend to hammer out reality by referring it to those categories — discovering or inventing “community” and “society” in all concrete situations, or reducing them to combinations in different degrees of those proclaimed “ideal types”: X per cent of “community”, so to speak, (100 – X) per cent of “society”, no more and no less. In this way, the genuine value and merit of the greatest proportion of works presented under the label of “Sociology” often really lies mostly in what the respective authors tend to

consider secondary, sometimes even in the more openly “artistic” traits of those works, with the facets generally deemed more important nothing but a group of schemes serving to simplify and “synthesize”. Indeed, they fail to explain and all too often fall into logomachies while sliding into amputating obsessions: sociologists sometimes think they find the “community” and its “values” everywhere in much the same way as Ionian physicists thought they found the “fire”, the “earth”, etc. everywhere (Veyne, 1984, pp. 239, 279).

Life without Sociology

It seems hard not to recognize an element of truth in the comments via which Veyne, on his own terms, set out to dispute “the flag and not the goods” (1984, p. 271) in Sociology. Some aspects of his reasoning, however, do at this distance seem rather more questionable. First of all, is Economics really a science worthy of that name, at least accepting the French historian’s thin sieve? If it is really so, and beyond those aspects of “formal elegance” and “praxeology” that it tries to assume, must it not try to make real predictions? Yet still, in good truth, the arguments concerning Economics seem mainly to configure narratives in which one constantly leaps from straightforward “either-or” situations — either one obeys the rule or this one is “avenged” by the famous event and therefore one pays the cost of non compliance — to others in which human history once again consists of stepping from a rigorous dualism to an endless “either-or-or...”; that is to say, to situations where it is permissible not to obey the rule and still not suffer the “revenge” of the event, because it is really viable to indefinitely procrastinate, avoid or transfer the costs of

non compliance. And if that “game” of transfers is truly susceptible to being held off and the payment of the aforementioned cost protracted *sine die* — what then remains of the Veynean notion of a true “science of man”, conceptually distinct from the level at which History is supposed to operate? Is it not a fact that in that case all causality is indeed rendered fundamentally “sublunar”?

Let us take another example: according to Economics, the productivities of factors and the utilities of goods are both supposed to be marginally decreasing... except, of course, in cases where we can confirm that they are not, in which it is the economies of agglomeration to explain the very economic growth (out of which the gulf between rich and poor societies “naturally” tends to grow, unlike that suggested by the logic of decreasing marginal productivities), in which international trade mainly occurs between countries with similar productive structures — but not just for weekly “institutional” or “sublunar” reasons, rather (so we are enlightened by the newer and more sophisticated explanatory models) for motives or logical, strictly “scientific” coherence concerning the fundamental fact that the famous marginal utilities are decreasing... only after a certain order that remains essentially undetermined. Indeed, in these cases, if we proceed to baptize the apparently uncomfortable facts with the name of an academic celebrity and explicitly set them as “paradoxes”, the margin of malaise really seems to tend to diminish and wither away, the whole situation returning to an apparent normality: hence “Lucas paradox”, of course, but also, and analogously, “Leontief paradox”, “Kaldor paradox” and so on...

We could, of course, follow an analogous path of disputation in case we decided to argue, for instance, about the famous independence of utility-functions. And indeed even the notion of interest that Veyne picks up from the “Austrian school”

is not itself as rigorously insurmountable as he suggests. But these questions should not be considered our main issue. The central point here is: Veyne's fascination with the "formal elegance" of mathematical models (economic and others), also conspicuously revealed in his encomiastic references to the works of Nicolas Bourbaki (1984, p. 313, footnote), has a clear risk of sliding into a game of subtleties in which — as much as in the exposed logomachies of functionalist sociologists — the purpose is mainly about "finding" in the concrete facts... what one had previously decided to "find" in them and nothing beyond, and certainly not the confutation of that. Once the "adjustments" are indefinitely possible (the marginal productivities are really decreasing but only after a certain order; the utility-functions are indeed independent inasmuch and only inasmuch as they are defined as such, etc.), what remains of the "falsifiable" character of theories and facts in order to be able to keep up such "scientific" arrogance?

The effectual History of Economics during the last century or so exhibits, as is known, an unstoppable tendency to appeal to mathematics (not just quantification, as with the tradition of Economic History, but truly *mathematization*) on the grounds, so Veyne proposes, that it is advantageous as it supposedly allows deductions to be carried out and conclusions reached otherwise unattainable via common language ("synthetic judgments a priori" à la Kant, that is) — but regarding which others have also stressed their concerns as to the merely supplementary character those procedures should assume given the unavoidable risks of "autistic" deviation imported through recourse to mathematics. Still others, and likely with some reason, more deeply wonder about the effects of going-cryptic with all its apparent sophistication, sometimes associated with fundamentally flawed argumentative schemes indeed

addicted to excessive simplicity and repetitiousness: is the Nietzschean saying really true that those who navigate in shallow waters tend to muddy them so as to cause a false impression of depth?

But we must make a pause here. If one really can argue that Economics tends to appeal to mathematics somehow the way others professional groups (lawyers, physicians...) nurture their jargons as a means of establishing entrance barriers and guaranteeing the production and preservation of the *aura* associated with their professions, that still invalidates nothing of the substance of the criticism Veyne directs at the effective practice of sociologists. In sum, and now according to us, we should instead follow the opposite path, that is, generalize to other academic disciplines the suggestion of reduction to the “sublunar” (and therefore to History and “Aristotelism”) that he points at Sociology.

Let us take, for instance, Parsons’ attempts to find a distinct academic “niche” for Sociology. From the formulations of the 1930s, seeking to differentiate it particularly from Economics — the study of “ultimate ends” versus a rational use of scarce resources by peaceful means — through to the meta-theorizing purposes characteristic of the 1960s (where in any case such concerns with academic partitions refer mostly to Anthropology), it seems indeed reasonable to acknowledge the fundamental wisdom of Veyne’s critics concerning the tendency to logomachies and obsession with classification. And yet, what about the campaigns (as to many aspects diametrically opposed) for the application of the mental schemes imported from Economics, that is, “rational choice theory”, to Sociology? Is it not true that, although *mutatis mutandis*, one can also easily find in this scenario materials to build a case

against argumentative circularities and/or hammering out facts in order to make them fit the explanatory schemes?

Where one finds that Sociology has been (and probably cannot but continue to be) History under another label, it still seems to make sense to apply to the case of its effective existence as an academically recognized discipline a set of mental dispositions which, incidentally, seem to fundamentally correspond to what economists call “opportunity cost”, historians try to capture as “counterfactual” and sociologists believe to recognize via expressions such as “functional balance” and “latent function”. Briefly: in case there was no Sociology... how would the academic panorama appear, what would there be substituting it or occupying its place?

And, if there is gain to be made out of confronting what really is with what there would be without, how to make the balance shift for Sociology? In its absence, this “niche” would likely be invaded — but by whom or what? Maybe a less *événementiel* History, and also more elastic in matters involving “reference to values”. Or would it rather be some Economics leaning more to “institutionalism”? Arguably some more socially inclined Psychology? An Anthropology more openly concerned with modernity? Probably a Geography with a more developed “human” component? Or rather a more eclectic and less specialized Demography?

One factor seems reasonably sure: the “post-modern” tendency to revert the process of specialization-differentiation induced, so they say, by modernity, the undeniable de-differentiation and in-disciplinarity trend, typical of recent decades, would certainly be felt all the same and correspondingly the discussions over disciplinary divides would thus carry on as much alive and uncertain as to their outcomes just as they are now. But the effects of this “life without Sociology” on its

neighbour disciplines appear rather more discussable: would Economics, for example, in its absence tend to become more or less prone to “institutionalism”? To put this another way: would there be an occupation of the same conceptual “niche” under another label, or would the very “niche” tend to disappear, being simultaneously suppressed with the “contagion” of problems and devices which Sociology, rightly or wrongly, can still tend to inspire in its neighbours? In such a scenario, would History tend to be less *événementiel* and hagiographic, or would these traditional traits be prone to amplification with Sociology stripped from the landscape? Analogously, would Psychology really be more social... or rather on the contrary?

One aspect at any rate remains sure: it would still be possible to refer to all of these groups of studies with the derisory comments that Veyne elaborates for Sociology. To study its History, he claims, 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, pp. 277-8).

Hagiography, isomorphism, refereeing

Indeed, and if we move from a philosophical discussion of Sociology into a more sociological one, we must acknowledge that since its inception it has been anchored to a particular set of social, economic and political transformations that took

place in Europe from the late 1700s to the middle 1800s. That said, it is clearly possible to assume that the sociological endeavour was basically a careful evaluation of a broad spectrum of transformations occurring in a space-time framework linked to the three major revolutions that shaped the continent during the period (Industrial, French and 1848). Nevertheless, despite this limitative experience, the lexicon and the conceptual tools developed by the most prominent authors of that era aspired to be universal descriptions of societies distant both in time and space from the European practice of modernity.

Still, if the classic project, associated with people as diverse as Comte or Marx, Weber or Durkheim, was full of creative approaches and theoretical insights making use of what Mills (2000 [1959]) would call sociological imagination, to study the strenuous transformations of modernity, post-1930s Sociology shows some signs of being unable (or unwilling) to move in the same direction. Indeed, and ever since Parsons decided to synthesize the contributions of “his” classics, establishing a theory that could justifiably be considered a pastiche of contributions by others, Sociology seems to have taken the path of reinventing the classics — sometimes establishing a cut between youth texts and works of maturity while on other occasions portraying a schizophrenic personality split, leading authors into contradictory statements. In a way, a while in Economics we have the widely known “Das Adam Smith problem”, in Sociology it would probably be safer to say there is a problem in each and all of the classics. Every new study produces a novel image of the author under observation. We can even encounter tribal opposition between defenders of the same sociologist, each trying to prove the righteousness of their approach. In this particular sense, Sociology has partially become a sort of hagiography with disputes over its saintly relics being

paramount: in the place of bones we instead find books and texts used to establish the primacy of one specific group over another.

More than trying to build up new theoretical approaches or analyse the nature of social settings, most contemporary sociologists tend to opt either for coining new words applied to already fully known realities and concepts or to head off on a sacred quest to find a brand new sociologist from the past. The rules of the game are relatively simple: go to the libraries and find an author that no one seems to remember. Secondly, discover if he/she is a part of a segregated, marginalized or disregarded identity group. Thirdly, find an obscure or cryptic text (written by him/her), supporting many and contradictory views. Fourthly, establish this text as a cunning anticipation of some of today's problems, emphasizing the prowess shown by the author. Having proven that, you are able to support your claim that he/she was an undeservedly forgotten sociologist, destined to historical oblivion by generations of biased sociologists. This is a trend that has already produced literally dozens of new classics to incorporate into the sociological cannon and led to hundreds of pet concepts constructed more to advance the self image of their authors than any contribution towards enriching sociological theory.

Theoretical vagueness, conceptual void, ideological and political parochialism, theoretical coining obsession, inability to offer middle range theories or explanations based on social mechanisms, incapacity to present a set of cumulative paths to existent theories, lack of historical content, hagiographic leaning, grand-theory paranoia, absence of consideration of the biological underpinnings of human nature, deficiency in the use of mathematical instruments, failure to recognize the importance of developing concepts adjusted to a reality no longer industrial, loss of a nuanced and

paradoxical view of social reality are only a sample of arguments found in the meta researches conducted by numerous sociologists (from distinct theoretical traditions) during the last four decades. Hence, it becomes possible to identify an important shift in the discipline – Sociology is no longer the study of crisis; indeed, it became the crisis itself. Sociology tends to study itself more than it studies social reality. Sadly, many of those who do not follow such a path are left with research lines ending up in crushingly sterile data, proving the obvious in a quest for a legitimacy that is seldom granted to them by the pundits of “hard” sciences.

What’s more, especially since the widely publicized Sokal hoax, sociologists, alongside cultural theorists, seem to have been thrown onto the defensive. Irritated by the provocation and unwilling to recognize certain wrongdoings and flaws in their fields, they had to make their stand. Whenever a scientific field, or a research area claiming to be scientific, or even a social practice threatened by powerful enemies, is under severe attack, it is only normal that the search for legitimacy takes on paramount among members. The defensive strategies are predictable: establishing a set of formalized rules in order to typify behaviours (a role formerly performed by tacit socialization); creating deontological codes setting a strong sense of responsibility and defining moral frameworks accepted by the class, unifying scholarly programmes preparing the new generations; defining a set of core areas and methodologies receiving a legitimating stamp; clarifying models of publications. The combined effects of these legitimating strategies reinforce the community construction but at the cost of losing part of the creativity shown in the past, when rules were far more diffuse.

The need for formalization is more important when an aristocratic or elitist practice is democratized either by the will of its members or by the sheer pressure

from newcomers. As we know, in professional groups where codification is stronger, the creation of important entry barriers limits new member access. But when these barriers are nonexistent or feeble, the flow of neophytes might cause deep problems. These are the moments to formalize or to regroup the troops under norms and values learned by all members. Under these conditions, defence and regrouping can easily become mechanisms driving homogeneity.

Contemporary Sociology seems to suffer from a combination of isomorphic effects, resulting mainly from normative mechanisms imputable to the class of sociologists itself. It is well known that the proliferation of alternatives may not lead directly to more real options – an effect that we can call the supermarket shelf paradox – the number of brand names available is not directly correlated with the variety of products consumers can choose from. The same goes for television – the cable makes it possible to zap between more than 100 channels at the pace of your finger, but that does not mean the alternatives available are more widespread than in the past when we had access to a fraction of the channels available today. This isomorphic effect has to be understood in a time in which many sociologists are well aware that they can fall victim to clever agent provocateurs, cunning hoaxes and practical jokes. Insecure and doubtful as to their own merits, sociologists adopt rituals of recognition, signs granting protection.

Even if Sociology, compared with Management, is less prone to what Rosabeth Moss Kanter defined some years ago as homosexual managerial reproduction (a tendency towards homogeneity of practices and uniformity of speech), it is more than obvious that Sociology presents a clear cut demonstration of what normative isomorphism is all about. Facing risk and threatened by enemies at the gates, the

professional groups tend to impose more discipline and concentrate themselves around a specific set of rules easily maintained, controlled, and evaluated. In terms of a scientific discipline, control over diffusion is a decisive issue. The sociological journals have become a sign of a sense of malaise, particularly salient in the field.

Refereeing is considered, in general terms, to be a condition necessary for advancing science. A democratic system of evaluation in which a peer evaluates another peer, using the tools of the trade either to prove or disprove the validity of research, refereeing emerges as the best way to secure both deontology and quality. But the journal system paves the way for hyper specialization. As in any arms race — each move by one party has to be perfectly matched by the author. The researcher wanting to make a career specializes in increasingly narrow areas, thus ensuring a survival niche granting an ability to publish articles on fragments of something only vaguely or remotely connected to reality. By way of this specialization, each researcher becomes the sole master of their turf, leaving journal editors with the tremendous task of finding a suitable referee to evaluate the submitted paper. Referees, in fear of being put to ridicule by clever hoaxes, rely on easy defence mechanisms, accepting only more of the same, thus contributing to the sterility of the sociological field.

Another dimension of this story is presumably the fact that social sciences, with the probable exception of Economics, are not particularly adapted to the structure of paper journals. The journal paper is indeed the right way to go when we are faced with incremental advances and minor and painstakingly researched limitations. On the contrary, the great theoretical shifts operated by sociologists were produced by books, not by articles. The 8,000 word paper is a good way of clarifying issues and solidifying piecemeal improvements, but remains too limitative when attempting to make a series

of statements as to the nature of society. It is indeed doubtful whether most of the theoretical ground breaking texts of the past would have seen the light were they produced today. Defying conventions, ignoring rules or presenting counter intuitive analyses is unfashionable among today's referees. A powerful mixture of fear and search for legitimacy leads so-called experts to search for a safe haven where they calmly accept more of the same and refuse anything that looks remote, strange, or unusual.

If we consider the major sociological publications of the last decades, especially the top ranked journals of the Anglo-Saxon world, we probably have to conclude that the basic output reveals more of sociological community manias, idiosyncrasies, political leanings and fads than anything else. Reading AJS or ASR from the 1970s onwards will likely give a historian of 2100 a clear understanding of the rules of the Sociology tribes of the period, but not a true illustration as to the social problems of the epoch. Sociologists are no longer essentially discussing society: they are more and more quarrelling over sociological rivalries. Social capital and clever networking become solid resources in the market for academic prestige. Being able to introduce a "new" concept — or one presentable as such — is as important to the advancement of one's career as is the launch of a company brand name into a very competitive market.

The fact that it represents itself as a cumulative process growingly induces the sociological enterprise to try to mimic the cumulative efforts of the natural sciences. Mathematics and statistics became widely used by sociologists. But results have remained poor and unimpressive. Spurious correlations transformed into major evidence of statistical association, or inferences of causality where no causality is present, are common and widespread. Mathematization is no longer a tool to push

Sociology forward but merely a way of legitimizing research practices fearful of not being recognized by their natural science counterparts. The quantitative shift, however, is likely not a cause but a consequence of the very trivialization of Sociology. The search for legitimacy leads to ritualized uses of techniques that do not answer any sort of sociological imagination. The tools become paramount and they seem to precede the choice of topic. The Weberian value relationship, the subjective moment that was supposed to enable all objectivity, is now substituted by a technical imperative, the quest for legitimacy that destroys all imagination.

And so, somehow paradoxically, all articles tend to look more or less the same — the structure identical, the methodologies standardized, the techniques predictable and the results trivial. The sheer amount of articles produced has been rising steadily, a fact mainly due to the concomitant increment in the number of publications and researchers. If a young researcher wants to be published (a sine qua non condition for survival in an environment that abides by the rule of “publish or perish”), he/she has to follow the *via sacra* and its standardized steps fostering still more repetition rather than any difference.

Crisis? What Crisis?

Claiming sociology is in the midst of a crisis is, however, an idea at least as old as sociology itself. This is only to be expected from a discipline that elected crisis and social change as the main issues to be addressed. Yet, crisis is a catchword that when applied to the current state of sociology takes on literally dozens of different meanings. A unitary view of crisis does not exist and the diffuse nature of the

descriptions hints towards the main problems listed. Fragmentation of sub disciplines, demoralization, excessive academic specialization, instability of boundaries, bipolar or schizophrenic tendencies in disciplinary moods, declining quality and significance, irrelevant production, trivial conclusions, lack of imagination, ideological partisanship, biased research, lack of public interest, are only a few of the examples that easily come to our attention while browsing a random selection of sociologist websites. When a scientific field starts (or keeps) discussing its status in conferences, seminars, meetings and journal articles this is probably a good indicator of its internal state and awareness of the rampant ongoing crisis.

Other interpretations associate crisis with the conceptual core of sociology. The capital sin of sociology would be to define itself as the science of society and social facts. The fluidity of today's social phenomena would push sociologists out of society, moving them in the direction of new institutions, organizations and associations. Overcoming crisis will force a conceptual revolution and the birth of a new type of science, no longer anchored to the old statements of the "saints".

Even considering that the most famous statements on sociology's crisis date to the early 1970s, when Gouldner (1970) presented his demolition of Parsonian sociology and Boudon (1980 [1971]) started to build up his methodological individualism, it is undeniable that many different authors (cf. Lemert, 2004; Himmelstrand 1887a,b; Schroter, 1992) have claimed that something is wrong either with sociological theory, sociological methodologies or both. The question however remains — are we experiencing a recurrent crisis always shaped by the same causes or are we witnessing something radically new that goes along with new types of change?

Simultaneously, how much is crisis itself shaped by the perceptions of sociologists and their particular ideological leanings and political biases?

The current crisis is partially the continuation of an old crisis underlying the definition of the scientific area of sociology, thus dealing with the conceptual core and methodological apparatus, but also linked to an inability of Sociology to describe, explain and predict some of the emergent social phenomena characterizing today's societies. At a third level the crisis of sociology is also a crisis of institutions at the centre stage of scientific production — universities, research centres and publication systems. At this particular level, we are not only talking about the merits and demerits of the field but also of the quality of research, its ability to present clever solutions to social problems, and contributions towards the advancement of modern societies. But when we talk about quality we also have to refer a special type of crisis — that dealing directly with acceptable evaluation criteria establishing what is a “good quality paper” or commendable research. Let us now present three different examples of how sociologists have defined and experienced the crisis of sociology.

In *The Coming Crisis of Western Sociology*, Alvin Gouldner defined sociology's crisis in strong words, underlining not only the internal flaws of the discipline, but also paying attention to the necessary relationship between sociology and government and the impact of knowledge produced on society. Gouldner's statements were both a devastating critique of the professional sociology of the 1950s and 1960s, especially functionalism and an attempt to establish a sociological project based on reflexivity and critique. Gouldner posited that isolationism, absence of reflexion on most new social problems emerging in post war society and an absence of self criticism as to methods and theories were really the main causes for the crisis. The Sociology of the

1960s was somehow dull, repetitive, chained by strict professional boundaries, without any ability to open itself up to society.

This state of affairs leads Gouldner to consider that sociology was living a situation characterized by divorce from reality — forgetting (or willingly ignoring) the political and conflicting dimensions existing in society and paying no attention to the forces commanding important social changes. Dominant and canonical sociology was increasingly technocratic, bureaucratic and deferential to state institutions in terms of its funding, methodological principles, research and practices. Sociology was not only blind to political problems but also morally flawed and unable to take a stand when facing questionable social situations.

This particular emphasis on politics and morality leads Gouldner to consider that Sociology needs self reflexion and self criticism, but also an awareness of the fact that theory and methodology are never fully neutral but closely linked to the choices, preferences, sentiments, and ideologies of sociologists themselves. These particular leanings are not something to eliminate, but something to take into account. The particular flavour of sociology results from the control that ethical principles should impose on these social and personal influences. Sociology should be committed not to a hypothetical neutrality but to a credible, critical and transformative science. Human emancipation should be the product of critical theoretical sociology. If the theory is never neutral, the same can be said about methodology. Method without theory becomes no more than technocracy or instrumentalism.

More recently, Lopreato and Crippen (2001) follow a different approach. For them, the crisis of sociology is widespread and results mainly from a sort of constitutive flaw — over-ambition. Contrary to many others, the solution cannot

involve going back to the classics and to the roots of the discipline because this is right where the original problem lies. The glorification of the turn of the century generation conducted sociology down the wrong path. At the same time, sociologists are unable to focus their attention on tasks that would eventually lead to building up a truthful social science. How can sociology claim to be a science if it has proven unable to define a single law or principle? Without these foundations it is obviously easy to explain the failures at the level of cumulative know-how. Empirical work is misguided, classifications are absent and scientific advances through falsification remain impossible. Hence, the problem is established at two levels: firstly a question of mismatch between the (huge) ambitions and the (poverty) of theories, secondly, the theories are unable to offer guidance for cumulative empirical research. The problem of sociology could well be defined as a science that bites off more than it can chew. Defined in this way, the crisis of sociology is the outcome of the lack of scientific strategies — the corpus of the discipline remains dominated by a lack of formalization, wild guesses, disconnections at all levels, fragmentation, confusion, and biased discourses. Bearing in mind this scenario, no one could be surprised by the fact that the original space of Sociology is now being occupied by other social scientists and applied fields. At another level, sociology fails to keep track on the recent evolutions in life sciences, namely biology. If sociology wants to be a behavioural science, it cannot fail to notice the important new contributions coming out of evolutionary biology. Lopreato and Crippen remain convinced that Sociology will not survive as an independent science unless it assumes a new perspective and embraces the evolutionary research underway. The lack of intellectual rigor in Sociology can be overcome by relying on methodologies from the natural sciences and factual

observation. The crisis of Sociology is also a crisis of confidence, resulting from declining methodological and ethical standards.

For Savage and Burrows (2007), the crisis lies elsewhere. These authors claim that some of the methods used by sociologists, namely sample surveys and in-depth interviews are clearly dated and cannot constitute the basis for Sociology into the future. In this particular dimension, more than the theoretical flaws that would describe the field, Sociology is in danger because it has become too attached to methods that do not grant any type of scientific authority: living in a new type of society demands new methods. Furthermore, sociologists suffer from the increasing competition provided by experts working for public and private companies who have access to invaluable data. The public availability of data and its dissemination have somehow compromised the role of sociologists. In a certain way, these authors seem more worried about the future of sociologists than the profession itself. They claim that sociologists are losing their position at the apex of social science research, becoming more and more marginalized. However, this does not at all demonstrate the crisis of the field, but merely indicates that Sociology is being practiced by a plethora of other researchers using number crunching software and taking advantage of the data available. Once again, the problem lies in the fact that most of these researchers lack the theoretical underpinnings that would make it possible to establish social trends and to produce fine interpretations of what is happening in the new, globalized world. The variable that has been absent from most of the sociological programme in the past few years has been imagination, not methods. The authors do address this question of a commercial type of sociology emerging to compromise the academic strand, but offer a nuanced view of the subject:

“There is plenty of research taking place in the cultural sector, but it does not depend very much on academic intervention. Cultural institutions have impressive databases, mailing lists, research projects and interventions. They have a range of ‘rules of thumb’, models and practices, which are informed by extensive research coordinated by consultants and partners as well as ‘in-house’. For the most part, the kind of academic research carried out in the name of culture is largely irrelevant. The ideas of Bourdieu and Foucault, indeed all the glorious flourishes of the cultural turn, do not — with a few exceptions — speak to the workaday needs and interests of such institutions. Once again, in such a situation it would also be possible to be precious and condescending to those who work in the sector, and bemoan their limited awareness, their instrumentalism, and so forth. However, our main point is that from their perspective, the research they do generally meets their needs: it is productive and is ‘effective’ in its own terms” (Savage and Burrows, 2007, pp. 887-888).

Most of the methods and techniques that constituted path breaking avenues of research are today widely incorporated into the activities of state agencies and private companies. The academic public sociologist offering her innovative methods and clever research is gone and is being substituted by a new stream of investigators. But the authors do not want to cynically stress the limited knowledge of market researchers, nor to claim that academics should mimic these private companies, they merely stress that most leaders of contemporary organizations do not recognize the need for the expertise of social scientists, contrary to what happened in the past. Currently, these companies have both the data (sometimes the by-product of their own commercial transactions) and the people to analyze them. There is a sense of nostalgia in this type

of analysis. The good old days in which sociologists were recognized by public authorities and private company CEOs are gone for good.

To conclude: within a framework of some contemporary sociological theories advocating the idea of an end to the break between professional sociology and some lay thinking, it is only natural to observe the emergence and flourishing of many different sociologies that may lack the theoretical sophistication of classic sociology but use, sometimes with considerable success, the methods and techniques granting scientific status to the research. It really seems as much legitimate to think of a “death” or “terminal crisis” of Sociology, as it is to argue for the notion of its perpetual self-transcendence and indeed omnipresence.

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