

Există paradigme
în științele sociale?

ARE THERE PARADIGMS IN THE SOCIAL SCIENCES?

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In sociology, political science, economics or social anthropology, scientific progress is achieved by cumulative knowledge, by the adding of successive layers of sediments, and not by abrupt jumps. There are no paradigmatic upheavals in the social sciences.

How to explain the diffusion of the concept of "paradigm", a word of Greek origin, used by some philosophers during the XVIIIth century, and which, in the writings of some contemporary scholars, has a pedantic tone? One of the reasons may be the fact that it raises, in the history of science, a fundamental interrogation: is scientific progress achieved mostly by steady accretion or mostly by abrupt jumps? This concept has been concocted by Thomas Kuhn in a particular context, and the circumstances of its formulation poses directly the question to be treated here: Are there paradigms in the social sciences?

A polysemic concept

Thomas Kuhn explains in the preface to *The Structure of Scientific Revolutions* (1962/1970) that it was during a stay at Palo Alto Center for Advanced Studies, in the company of social scientists, that he was led to formulate the concept of paradigm with the aim of making clear the essential difference between natural sciences and the social sciences. The reason given by Kuhn was the absence of a theoretical consensus in any discipline of social sciences: "I was struck by the number and extent of overt disagreement between social scientists about the nature of scientific problems and methods... Controversies over fundamentals seem endemic among, say psychologists or sociologists... Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called paradigms". (Kuhn, VIII). For him, the use of the word paradigm in the social sciences is not justified.



As a preliminary, it should be noted that many scholars have formulated doubts about the reality of paradigms even in the natural sciences — an issue that cannot be discussed here. Kuhn himself has, in the postscript to the second edition of his book, reformulated the concept of paradigm, by disentangling this concept from the notion of scientific community, and by admitting that he has used the term in two different senses. On one hand, paradigm stands for a constellation of beliefs, values, techniques and so on shared by the members of a given community; on the other, it denotes a concrete puzzle-solution. He labelled the first meaning "disciplinary matrix": "disciplinary because it refers to the common possession of the practitioners of a particular discipline; matrix, because it is composed of ordered elements of various sorts" (p. 182). Kuhn makes a distinction between paradigms in the large sense, the disciplinary matrix, and small paradigms called "exemplars". This distinction is considered by several sociologists of science as unclear.

In the revised version of Kuhn's thesis "any period of scientific development is marked by a large number of overlapping and interpenetrating paradigms, which do not replace each other immediately and do not spring up full-borne, but instead emerge in a long process of intellectual competition... These concessions dilute Kuhn's original message, and in its final version the argument is difficult to distinguish from the average historian's account of the history of science" (Blaug, 1976, 154). Among the sociologists of science, very few have asked if the concept of paradigm is, or not, applicable to social sciences. Harriet Zuckerman in her substantial *Sociology of Science* (1988) does not dedicate a single line to this issue, nor does so Merton, Price, Collins, Lakatos or Mullins.

Are there in the social sciences instances of paradigmatic upheavals comparable to those created by Copernicus, Newton, Darwin or Einstein? Can the theories of Keynes, Freud, Chomsky or

Parsons be described as paradigmatic? In the social sciences, does progress occur through paradigmatic revolutions or through cumulative processes?

For there to be a paradigm, one condition must be met: theories must refer to essential aspects of social reality. However, the more ambitious a theory is, the less it can be directly tested by the data available. In the social sciences there are no "fundamental discoveries", as there sometimes are in the natural sciences. Instead, unverifiable theories are constructed, partly because social reality itself changes. Kuhn held that a paradigm shift occurs when two "incommensurable" paradigms are in competition. Then, each school rejects the premise of the others, and contests their work. Communication breakdown is the sign of an incompatibility. Such "incommensurability" takes in the social sciences the pattern of mutual ignorance.

Mutual ignorance

There are two kinds of mutual ignorance: involuntary and deliberate. The formal disagreement may be based on personal rivalries, on contest about priority, on methodological quarrelling or on ideological hostility.

Within a formal discipline, several major theories may cohabit, but there is a paradigm only when one testable theory alone dominates all other theories and is accepted by the entire scientific community. When Pasteur discovered the microbe, the theory of spontaneous generation collapsed: contagion became the new paradigm. In the social sciences, however, we see at best a confrontation between several non-testable theories. Most of the time there is not even a confrontation but careful mutual avoidance, superb disregard on all sides; this is relatively easy owing to the size of scientific disciplines, and their division into schools. This is true for all countries, big or small.

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This mutual disregard is an old practice in the social sciences. At the turn of the century, the great scholars did not communicate, or very little. In the writings of Weber there is no reference to his contemporary Durkheim. Yet Weber was acquainted with Durkheim's journal, *l'Année Sociologique*. For his part, Durkheim, who could read German, makes only one, fleeting reference to Weber. Yet they worked on a number of the same subjects, such as religion. "Weber certainly knew of the work of Durkheim but there can be little doubt that he was barely influenced at all... The same applies in a reciprocal vein; Durkheim knew of Weber's work, and there is a minor reference to aspects of it in one passage in Durkheim's writings, but he claimed no particular kinship with those of Weber's ideas with which he was familiar" (Giddens, 1987, 182). Durkheim does no more than mention in passing Simmel and Tonnies.

Harshly criticized by Pareto, Durkheim never alluded to Pareto's work. Pareto's judgment of Durkheim's book on suicide was unfavourable. "Unfortunately, he wrote, its arguments lack rigour". Weber seems to have been unaware of Pareto's theory on the circulation of elites, and Pareto in his turn says nothing about the Weberian theory of political leadership. There was no exchange between Weber and Freud. Ernst Bloch and Georg Lukacs met regularly with Weber in Heidelberg, but their work shows no sign of his influence. The only one of Weber's contemporaries who referred to him was Karl Jaspers, but he was a philosopher (cf. Mommsen and Osterhammel, 1987).

Weber and Spengler had not exerted the slightest influence on the ideas of the other. Croce said about Weber: « I had met Weber in 1909. I had no further contact with him, nor did I follow his scholarly work, from which I had only read his early book *Roman Agrarian History* ». As was noted by Raymond Aron, each of the three great founders of sociology followed a "solitary path". There is no room for two theorists like Parsons and Sorokin in the same habitat; the clash

between them became inevitable. The Parsonian grand theory had a short life. Robert Dahl and Arnold Rose refute the theory of C. Wright Mills; they do not adopt the stratagem of deliberate ignorance; they contest it openly. On the contrary, C. Wright Mills chooses to ignore the books published by Berle and Means and by James Burnham, embarrassing for his own theory.

Mosca had accused Pareto of plagiarism. The imprecision of the language of both elitist theorists, who adopted synonyms with confusing meanings, has generated in Italy an enormous literature with the participation of dozens of Italian scholars debating the priority of the authors of the "political class" and of "political elites" (Albertoni, 1983). Pareto and Mosca are not mentioned by José Ortega Y Gasset (1883-1955). Schumpeter does not refer to Mosca, Pareto, Michels or Burnham. Angus Campbell and Paul Lazarsfeld have spent a large part of their parallel life in the study of the same political behavior; they never cite each other. Johan Galtung born in 1930 and Stein Rokkan born in 1921, both citizens of the same small country (Norway), have each contributed a theory of "center versus periphery". They have never collaborated, avoiding each other carefully; they coexisted by ignoring each-other.

Three lists of major contributions to economics were compiled in 1982 at a symposium in Berlin. The first, prepared by W. Krelle contained 30 names, the second, by J. Timbergen, 36 names, and the third, by B. Fritsch and G. Kirchgässner, 44 names. In the first two lists, there were only two names that were the same (including Klein, Nobel prize winner, and Krelle himself); in the first and the third, there were only nine that were the same. The last two lists did not have any name in common (J. Platt, 1986, 350). Such widely differing views about leading figures do not say much for the coherence of economics. This is also the opinion of the economist Kenneth Boulding, who speaks of economics as being "disorientated", comparing it with the story of the



blind man and the elephant. The Nobel prizewinner Wasily Leontieff was not more indulgent: "Year after year, economic theorists produce mathematical models without being able to get any further towards understanding the structure and functioning of the real economic system" (quoted in J. Platt, 1986, 350). The monetarist George Stigler objected to six econometricians (Tinbergen, Samuelson, Malinvaud, Moore, Frisch and Fisher) being described as the authors of "exceptional contributions" because, so he wrote, "econometrics has no unified core or methodology" and "has not yet had a major impact on economics" (idem, 342). But three of these six economists have later won the Nobel prize, as has Stigler himself. "The existence and unity of a discipline called economics reside in the eye and mind of the beholder. Samuelson, Solow and Tobin and others reserved their harshest words for neoclassical economists like Friedmann and members of the Chicago School, who ridicule Keynes's consumption function" (Amariglio *et al.*, 1993, 150, 175). The same remarks can be made with reference to other disciplines, except linguistics.

If scholars belonging to the same generation and working on the same subjects ignore each other, involuntarily or deliberately, how could one perceive the existence of a scientific community able to adhere to a common paradigmatic *Weltanschauung*? Paradoxically, the nearest the proximity, the more vivid is the rivalry and the more frequent the mutual ignorance. To the isolation of scholars motivated by competition should be added the weak communication between specialties within the same discipline.

The absence of consensus among contemporaries is compensated by the transmission of knowledge from one generation to the next, as revealed by citation patterns, particularly in handbooks and compendia. Such a generational transmission testifies that in social sciences scientific advancement occur mostly by cumulative knowledge.

Cumulative knowledge

Science consists largely in the adding of successive layers of sediments, which form a patrimony for each discipline and field. In all sciences "the extent of the innovation that any individual can produce is necessarily limited, for each individual must employ in his research the tools that he acquires from a traditional education, and he cannot in his own lifetime replace them all" (Kuhn, 183). Progress does not arise in a vacuum, but develops out of the scientific patrimony. It is difficult to find in the social sciences a virgin domain. Every decade that passes adds layer upon layer to the patrimony (with one exception in recent times, the 1930's in Europe). New explanations supersede older interpretations. Many examples of cumulative progress can be given. Even giants rely on the patrimony. Karl Marx refers to Adam Smith on 296 of the 1721 pages of his *Theory of Surplus Value*. In this book he draws on the work of his predecessor in more than one page out of every six. Ralf Dahrendorf cites Marx 160 times (Dahrendorf, 1957). Max Weber does not cite Karl Marx, but many of his writings were a reply to the thesis of his predecessor. He once noted that he could not have accomplished crucial parts of his work without the contributions by Marx and Nietzsche.

Darwin had recognized his debt to Malthus. Keynes would not have been able to write one of the most famous books of the twentieth century without the incremental advance achieved by several generations of economists. The "theory of development" consists of a long chain of accumulated contributions in several disciplines.

The literature on elites is a good example, among others, of cumulative knowledge even in the case of mutual ignorance. Many contributions emphasize the sedimentation of layers of knowledge. The old layer Mosca-Pareto, impregnated with plagiarism, has become consolidated ground-work. Two dozens important books

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published in the last five decades have built an impressive patrimony. Obsolete theories have served as fertilizers for new complementary theories. An example of cumulative knowledge is the influence of electoral techniques on party systems. A bibliography, even a very selective one, on this theme could easily comprise two or three hundred titles in English, not to mention the many varied observations derived from the direct experience of politicians in numerous countries. From Condorcet, Bachofen, John Stuart Mill, Hare and Hondt to Hermens, Downs, Duverger, Sartori, Lijphart, the theory is based on the contributions and successive improvements made by a very large number of scholars. (The consequences of proportional representations were described already in 1850 by Bachofen).

For each of the dozens of important domains in the social sciences there is a substantial patrimony, the accumulated knowledge of a long chain of scholars. Imagine the following imposture-exercise. Take from the literature a classic article or chapter, for instance Max Weber's seminal chapter on "Domination and Legitimacy", disguise the name of the author, make a few cosmetic changes, and propose it to a major journal. Would such a manuscript be accepted today? Would not a paper that ignores the literature of the last decades preclude publication? The patrimony is common property. Although every concept or theory or method has its creators and developers, they do not need to be cited every time the term is used. Even the identity of the innovator may disappear into anonymity. We do not, and cannot, remember who used for the first time concepts like "role", "revolution" or "social mobility". Having such a patrimony each scholar can start his or her research at a much higher level than did their predecessors (Dogan and Pahre, 21). A graduate student knows today more than the founders of his or her field, even if they do not have the reasoning capacity of their forebears. That knowledge is largely acquired by cumulation is an obvious fact for all sciences; otherwise, simultaneous

discoveries in the natural sciences would not occur, theoretical debates in the social sciences would not be engaged. They are generated when the issue "is in the air", when "time is ripe". The dispute between Mosca and Pareto about the paternity of ideas in elite studies is similar to the dispute between Newton and Leibnitz about priorities on calculus. In elite studies, for a variety of reasons "the time was ripe" at the end of the XIXth century. "Discoveries are like apples on a tree", wrote Price (1963/1986, 60). In the history of social sciences some books have played a crucial role but they are only summits in a chain of mountains, not paradigmatic contributions in the sense given to this word by Thomas Kuhn.

Overarching theories, that is, paradigmatic frameworks, can be built on more solid ground in the natural sciences than in the social sciences, because in the former truth is universal, in the later, contextual.

Contextual diversity versus universal truth

Chemical substances keep indefinitely the same composition and are identical in all latitudes. Not social phenomena! In contrast with the natural sciences, contextual diversity and social change are two important parameters in all social sciences. Both parameters resist ambitious paradigmatic generalizations. Extrapolating Kuhn's perception of the history of astronomy, physics or chemistry to social sciences is like looking into a distorting mirror. Many concepts and theories do not have equal pertinence when applied to Western Europe and to Tropical Africa. Dependency theory may be a pertinent theory for yesterday's Latin America and for Eastern Europe today, but not for other areas in the world. The comparative method reduces the significance of paradigmatic frameworks. Dozens of theories with universal pretensions neglecting national



configurations have been invalidated.

In the natural sciences an experience can be repeated many times in identical conditions anywhere in the world (except when magnetism has an impact). On the contrary, social realities change. For instance, at the time of Malthus, overpopulation in Europe was a realistic hypothesis. Today birth rates in this continent are not sufficient to replace the outgoing generation. Traditional values concerning religion, nation, family, eroticism or authority have changed at an accelerated rhythm in the last decades. Theories reflecting the social reality one generation ago have to be replaced in order to explain contemporary trends. History never repeats itself because factors do not recombine in identical ways at all epochs. The scope of social sciences is rarely the discovery of laws with universal validity, but rather the explanation of social diversity. "What is true in one side of the Pyrénées is not in the other side", wrote Montaigne four centuries ago. In the social sciences truth is not universal, it is contextual. Social scientists do not make inventions, and rarely make discoveries; what they do best is to observe regularities and exceptions (anomalies). It is for this reason, that the most imaginative strategy in the social sciences is the comparative method, the best surrogate to the experimental method. Social phenomena being contextual, paradigmatic statements in political science, sociology, anthropology, social psychology or social history often appear as ethnocentric and misleading.

Most social scientists who adopt the concept of paradigm apply it to particular domains, not to their entire discipline. For some scholars, elitism is the most powerful paradigm against the Marxist paradigm. In the domain of political economy, rational choice is elevated to the rank of paradigm. Many psychiatrists are mobilized around the Freudian paradigm.

Competing theories contribute to the advancement of knowledge. To explain the fall of the Roman empire, fourteen competing theories have been formulated

by great scholars, from Montesquieu to Max Weber, to which can be added a theory proposed by chemistry (saturnism). None is fully comprehensive, but each one enlightens part of the phenomenon. The history of social sciences is not a history of paradigmatic upheavals, but of competing theories, with many being invalidated, but many others constituting the foundations of the contemporary social sciences. Without competing theories social sciences would not advance. The clash of theories leaves no room for paradigms.

A symptomatic analogy can be drawn between what Kuhn calls in the revised version of his theory, "scientific community" (adepts of a paradigm) and what some social scientists call "school" (Almond, Merton, Smelser, Zuckerman). In the recent history of social sciences there are dozens of schools: monetarist, rational-choice, bio-social, individualist, post-modernist and so on, and that means that attempts to formulate overarching disciplinary paradigms may appear as "imperialist views".

"Since all virtues can readily become vices merely by being carried to excess, the sociological paradigm can be abused almost as easily as it can be used. It is a temptation to mental indolence. Equipped with his paradigm, the sociologist may shut his eyes to strategic data not expressly called for in the paradigm. He may turn the paradigm from a sociological field-glass into a sociological blinker" (Merton, 1957, 16). In some cases, a paradigm may take the features of a dogmatic orientation. No wonder if so many scholars do not adopt it: "The notion of paradigms and paradigmatic revolutions often seem to be taken up only in order to be rejected. The beneficiary of this exercise in several cases is Lakatos, mostly because he appears as a type of moderate Kuhn" (Weingart, 1986, 267). In the social sciences theoretical disagreements are beneficial to the advance of knowledge. Nevertheless, the word paradigm has taken roots, particularly in sociology, political science, psychology and normative philosophy. But most philosophers of

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sciences reject it. Most historians are reluctant to make such generalizations. Most economists continue to think in terms of assumptions. It may be too late now to try to exclude this word from the lexicon, in spite of the fact that many other expressions are available (conceptual

framework, assumption, dominant theory, theoretical breakthrough, grand theory, general model, axiom, and so on). It has become necessary to specify it, or to limit its use to particular domains, like cognitive science, international relations, or hybrid demography.

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