Philosophy of Economics & Social Science in a Nutshell: From Discourse to Model and Experiment

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1. Introduction

The debates on the scientificity of social sciences in general, and sociology in particular, are recurring. From the original *methodenstreit* at the end the 19th Century to the contemporary controversy on the legitimacy of "regional epistemologies", a same set of interrogations reappears. Are social sciences really scientific? And if so, are they sciences like other sciences? How should we conceive "research programs" (Lakatos [LAK 78]) or "research traditions" for Laudan [LAU 77]) able to produce advancement of knowledge in the field of social and human phenomena? Is the progress of knowledge in social sciences similar to the one generally observed in natural sciences? Is it possible to evaluate the relative merits of each one of these research programs?

These debates are important vectors of social and intellectual polarization. The historical divide between the positivist and the hermeneutics poles precedes the structure of the contemporary debate around the epistemic space of social sciences, It is not only a question of renewing the opposition between a monist view of sciences (e.g. [MCI 96]) and a dualistic one (e.g. [GEE 73]) or even a trialist view of sciences (e.g. [LEP 85]). It is also a question of asserting dichotomies transformed into framework (including when it is a question of exceeding them): nature-culture, nomothetic-idiographic, models - narrative, structure - history, cause - reason, explanation - comprehension.

In this short introduction, we provide, in section 2, a first overview of this epistemological debate in social science. Section 3 proposes a different standpoint on the same questions, by introducing both ontological and methodological aspects in this basic epistemological debate. Namely, following [HOL 94] oppositions of the explanation/understanding, causes/meaning, etc., types discussed in the first section are comparatively examined together with oppositions of the structure/action, holism/individualism types. This allows us to discus show multi-agent design, by integrating various dimensions and standpoints in the same framework (Chapters 1, 5, 14) can help us to shift these boundaries, and to bypass these oppositions. As model building and ontology design are at the core oft his process (Chapter 12), Section4 discusses various issues of the art of modelling, starting both from economists' and sociologists' current standpoints.

2. An Overview of the epistemological debates in the Social Sciences

The social sciences show all the external signs of the ordinary disciplinary regime of sciences [SHI 02]. The teaching, communication and research structures inherent to the community of social sciences do not seem basically different from those of life sciences. In addition, the primacy of their cognitive function compared to their other functions (literary, philosophical, journalistic, etc.) is generally recognized. The institutional legitimacy of social sciences seems closely related to their capacity, under certain conditions, to generate a new and solid knowledge in connection with enigmatic phenomena. However, inside as outside this community, their capacity to have a specific mode of scientificity is regularly questioned. One can distinguish here two distinct and general registers from which the representatives of social sciences are too frequently led to doubt about the general value of their work.

2.1. How Specific are the Characteristics of the Social Sciences?

For some, if social sciences constitute an exception to the ordinary regime of sciences, it can be explained above all by the nature of their objects. This nature is generally described through three complementary aspects: complexity, historicity and consubstantiality.

Complexity - because the social and human facts are frequently presented as singularly less "elementary" than those studied by natural sciences. The "concrete" matter of the individual and collective life is inseparable from the infinity of factors in mutual interaction. It is this infinity that condemns to illusion any traditional scientific approach focused on the exhaustive study of the significant variables and their relations.

Historicity - because the social and human phenomena would be similar to hyper complex configurations never reiterated completely in the course of history. This absence of reiteration specific to the object of social sciences would invalidate in principle the possibility of an experimental approach characteristic of any scientific work.

Consubstantiality finally - because the researcher in the social sciences would have with his object a different relation from the one observed with the chemist, the physicist or the biologist: the fact that the sociologist for example is a social actor as any other would constitute an obstacle to a purely objective apprehension of the social facts.

This wills to regionalize the epistemic space of social sciences starting from a quasi-ontological characterization of their object - generally associated with more or less refined forms of realism - raises a certain number of difficulties. It is for example possible to show that the indisputable historical nature of the object of sociology by no means invalidates the possibility of identification of regular phenomena (captured, by example, through statistical methods). The existence of a relatively significant number of regularities in social sciences, sometimes similar to certain sciences of nature, justifies the use of experimental reasoning or at least of a permanent intellectual movement between reasoning by historical contextualization and experimental reasoning. The latter is conceived in its generality as an "exercise of comparison" (quantified or not) able to contain its inferences in a system of rules and being based on constant correlations of features, observed or measured "all other things being equal". And is represented, within the framework of social sciences, primarily by statistical reasoning, "which, in its formal structure, is indifferent to the structure of object on which it takes its measurements or its counting" [PAS 91]. But in a more radical way, it is possible to show that each supposed characteristic that seems to justify the epistemic specificity of social sciences can be found for the other sciences. Thus the argument of the intrinsic complexity of the object of social sciences does not just ignore the complexity of

natural phenomena. This complexity is never given but is the fruit of a process of elaboration. Social sciences like all other sciences are never sciences of "concrete reality". They produce an abstract object to answer questions put in terms of a particular theoretical language.

The argument of the historicity of social phenomena is problematic as well. The idiographic/nomothetic dichotomy implied by this argument is a philosophical artifact [BUN 98]. Moreover numerous natural sciences are historical sciences (cosmology, palaeontology, geology, etc.). Their physico-chemical object is subjected to an "arrow of time". This temporal dimension invalidates the a priori opposition between sciences of nature and historical sciences. Finally concerning the consubstantiality of the subject-object in social sciences, it is not completely useless to point out that the principle of objectivity is regarded as unproblematic within biological sciences and physical sciences even if the biologist is a biological being, the physicist a physical being. In addition history and sociology of sciences have for a long time now highlighted the importance of subjective and inter-subjective dimensions in the emergence and the development of the most objective disciplines. Despite its apparent weakness, this argument of consubstantiality deserves more attention. Interpreted differently, it allows questioning the way in which social sciences articulate their explanatory and understanding dimensions. Rare are those who dispute the fact that the object of social and human sciences differs in kind from that of other sciences on a well-delimited aspect: its meaningful and intentional dimension.

Intentionality, meaning, cognition, values within the social and human phenomena know few equivalents in the physico-chemical realm. If this ontological dualism is generally accepted, does it truly justify a more or less radical form of epistemological dualism?

The hermeneutical intemperance sometimes asserted in certain sectors of social science—ethnomethodology for example—in fine relies on confusion between ontological and epistemological fields. In the name of the significant and intentional dimensions of social phenomena, social sciences for some should be condemned to know only a purely interpretative form of rationality, conceived as a process of discovering pre-existent significances. This "interpretative turn" is characterized by three complementary aspects: a dialogical conception of empirical research; a literary conception of scientific writing; à pluralist conception of culture as a textual form likely to receive an indefinite number of readings [BER O1].

Three remarks about this representation of comprehension as a semantic-interpretation (in opposition to semiological-interpretation):

1. It is by no means a necessary consequence of an ontological dualism: there is no mechanical relation between ontology and epistemology;
2. it is only one possible interpretative form among other possible forms (causal, functional, dialectical, actional, etc.) able to be associated with the comprehensive step.
3. it is important to transform this semantic interpretation — but like any other interpretation — into rational evidence, by articulating it with a properly explanatory register.

This necessary articulation of the comprehensive and explanatory steps is particularly obvious in the case of sociology [APE 79]. When for example Weber conceives its "explanatory comprehension", his intention is not to avoid the *methodenstreit* through an intermediate way between comprehension and explanation, but above all to give to sociology the status of a "generalizing science" and to conceive comprehension as a rational interpretation in opposition to the strict epistemological dualism identified by Dilthey.

Comprehension by interpretation of cultural phenomena acquires scientific validity only

insofar as it "is controlled as much as possible, by the other ordinary methods of causal imputation" [WEB 65]. Contemporary actionist sociology renews this Weberian idea of comprehension and explanation as being two moments of a unique process [BOU 97].

For this sociological approach, to explain a phenomenon is generally to conceive it as the result of numerous actions the intention and meaning of which have to be identified. Comprehension thus represents the moment of the analysis during which the sociologist builds the reasons, at the micro-sociological level, for the macro-sociological phenomenon that he seeks to explain. This construction is generally associated with a typological approach and the production of a psychology of "convention". The logic of action is then reduced to a set of necessarily simplifying micro-sociological statements compared to the complexity of the empirical phenomena. This stylization may take various forms according to the needs of the analysis: a consequentialist and instrumental rationality [COL 90], adaptive and/or evolutionary rationality [MAC 97], imitative rationality [HED 98] and cognitive rationality [BOU 03].

This necessary articulation of comprehension and explanation makes it possible to separate the social sciences from other sciences, which are never confronted with individual actors and their intentional states. It is however not obvious that this methodological specification implies an epistemological regionalization; ie. justifies the development and the validation of specific standards of scientificity [RAY 06]. In addition the sociological stylization, inherent to the Weberian concept of "ideal-type", confers a constructionist dimension to the analysis. The latter must be clearly distinguished from certain contemporary forms of constructivism. If radical constructivism identifies the conventional character of knowledge with its contingency and local nature, on the contrary the neo-Weberian tradition in sociology conceives it as the result of a process of abstraction and generalization.

2.2. Scientific Ideal and Practices of Social Sciences - two dimensions:

It is not unusual to observe that some representatives of social science condemn their discipline to a pre- or proto-scientific status in the name of an ideal of scientificity generally ignored in the sciences of life and nature. This ideal is very often too absolute, with disproportionate requirements and without real connection to the reality of the other scientific fields. It is necessary here to underline the importance of the interdisciplinary dialogue between philosophy, history and sociology of science. This dialogue not only made it possible to illustrate the diversity of social and historical situations associated with the phenomena of scientific discoveries and more largely with the emergence and institutionalization of scientific disciplines. But it made also possible to transform in a major way the horizon of scientificity from which social sciences should be evaluated from now on. Among the many topics related to this analysis, we will consider two of them: the nomothetic inhibition of social sciences and the poly-paradigmatic character of social sciences,

The first topic frequently used to explain the supposed weakness of social sciences in comparison to other sciences, is connected to the "nomothetic inhibition" recently described by Cuin [CUI 00]. If social sciences cannot truly reach the ideal of the scientificity allegedly incarnated by the sciences of nature and life, it is simply because they would be unable to produce "true" scientific laws. This criticism of the *nomologic* dimension of social sciences is based first of all on an ignorance of the diversity of the forms of laws in sciences. It opposes in a caricatural way the weakness of the "conditional laws" of social sciences to the solidity of the "universal laws" of other sciences. However those "hard" sciences are far from producing only universal laws. They develop a wide *nomologic* variety, which combines conditional, deterministic or statistical types of laws. There is no need to oppose in principle the strict conditionality of the statements produced by social sciences to the strict universality of those

produced by other sciences. It does not mean that the rational criticism of the nomologic fallacy proposed by Boudon [BOU 84] is illegitimate. But that the over-interpretation of possible, probabilistic or conditional statements in general and universal laws is a different problem from the *nomological* diversity of the social sciences. This leads us to our second point: if one admits this *nomologic* diversity of social sciences, the study of social and human regularities does not necessarily have to be established on an opposition of principle between "law" - of general validity - and "model" - of limited validity. As Cuin reminds us, there is an "epistemological gap" between the concept of "law" and the concept of "model" only if one forgets that a model supposes an axiomatic, i.e. a set of proposals which, while making it possible to deduce certain consequences, acts as "relative laws". One perceives consequently what could be a strategy "to save" the *nomologic* principle within social sciences: on the one hand to protect the concept of "law" against any realistic interpretation in order to limit the risk of deterministic and/or naturalist drifts; on the other hand to extend its usual meaning in order to put together in the same "epistemic space" a diversity of conditional regularities (contextualist, possibilist, probabilist, axiomatic, etc)[1].

The second topic generally invoked to explain the singular scientificity of social sciences is, in Kuhnian terms, their so-called irreducible poly-paradigmatic character. Social sciences could not be sciences like the others; they could not generate a real cumulativity of knowledge, because they seem condemned to maintain a plurality of research programs. Four successive and complementary remarks must be made here.

The first consists in admitting the reality of this plurality of research programs. In sociology, programs like Bourdieu's "genetic structuralism" [BOU 93] or Boudon's "methodological individualism" [BOU 82] are indisputably two divergent and competitor research programs. Their divergence appears in the treatment of delimited objects—analysis of beliefs, social mobility, education, etc. —and the nature of the "middle range theories" generated [MER 49].

Second remark, this poly-paradigmatic dimension does not invalidate the possibility of a cumulativity of knowledge in social sciences. Within each program, it is possible to identify over a given period an accumulation of knowledge, theories, resolution of enigmas, which show their practical effectiveness. This progress is frequently associated with a thematic extension through which the research program intends to show its relevance, its heuristic character, its methodological range. For example, methodological individualism was initially associated with the resolution of social mobility problems before being associated with the study of ideologies, beliefs and values.

Third remark, this poly-paradigmatic dimension is not specific to social sciences, but can be found in other sciences. The work of philosophers, sociologists and historians of science shows that sciences like mathematics, physics, biology and chemistry are much less often than we think in a mono-paradigmatic situation. As Lakatos (1978) emphasizes, the total monopolization of a scientific discipline by a single research program is a situation at the same time extremely rare and generally brief. The study of sciences and their transformation reveals on the contrary the long persistence of rivalry between research programs. Fourth and final remark: if social sciences, through their poly-paradigmatic character, share a common characteristic with all sciences, it is not impossible that, as any science, they may have periods of reduction of this paradigmatic diversity. The increasing visibility in sociology of the various programs seeking to overcome the opposition between "genetic structuralism"

[1] The concept of law has been discussed in many recent debates. An old fashioned, classical position is [HEM 65]. Some people talk about "natural laws" while some others about "scientific laws", some change the meaning of the concept of law [FRA 89]. For more references and discussion about this question, see [SCH §1].

and “methodological individualism” seems to be an empirical manifestation of this process.

3. Ontological, Methodological, and epistemological aspects of the debate

In his enlightening introduction to philosophy of social science, Hollis [HOL 94] begins with the key question: “does structure determine action or action determine structure?” (p. 6 and p. 9). He suggests also the possibility of transcending this historic opposition by the additional: “Or is it a bit of both?” Karl Marx and John Stuart Mill are paradigmatic examples to illustrate these opposite standpoints.

For the structural standpoint, action is nothing but the constrained result of some hidden holistic principles. “In the social production of their life, men enter into definite relations that are indispensable and independent of their will [...] The sum of the relations of production constitutes the economic structure of society” (Marx, Preface to A Contribution to the Critique of Political Economy, 1959). On the contrary, for the individualist standpoint, society is nothing but the result of the individuals’ actions. “The law of the phenomena of society are, and can be, nothing but the laws of the actions and passions of human beings united together in social state [...] Human beings in society have no properties but those which are derived from, and may be resolved into, the law of the nature of individual man” (Mill, A System of Logic, 1843, book VI chapter 7, quoted by [HOL 94], pp. 10).

Both Marx and Mill have naturalistic convictions, but a similar opposition (individual/collective) could be established for the hermeneutic point of view. Moreover, the quoted texts have also an ontological, methodological and epistemological commitment embedded in these. Epistemologically, Mill follows the empiricist tradition (Appendix A1.1.1), where observation of the regularities in individual human behaviour is the basis of knowledge. This is nonsense for Marx, who said: “It seems to be correct to begin with the real and the concrete, with the real precondition, [...]. However, on closer examination this proves false” (“*The Method of Political Economy*”, in the 1857 outlines for the *Grundrisse*). Marx is looking for a theory that explains hidden mechanisms constitutive of an underlying layer of reality. This opposition in the philosophy of knowledge has its methodological counterpart. Both search for causal laws, but while Mills thinks that the right method consists in deriving social behaviour from individuals’ actions and passions, Marx wants to determine analytically the hidden structuring mechanisms that determine consciousness and relations of production. Finally, on the ontological side, individuals are the only relevant entities for Mills, while the social structure, “society”, is the relevant subject for Marx.

Neither Marx nor Mill has formal models. Hence some ambiguous proposals such as: “Men make their own history, but they do not make it just as they please, they do not make it under conditions chosen by themselves” (Marx, *The 18th Brumaire of Louis Napoleon*, 1852) have not been implemented, then, specified within different formal requirements (ontological design, model building …). Such exercises would be useful in order to explore effectively the implications of Hollis’ supplementary question: “a bit of both?”.

At first sight, multi-agent systems suggest a strong methodological individualist background, since “agent” seems to be similar to (human) “individual”. But this first hunch could be a fallacious one. To begin with, in the “computer sciences—or software—paradigm” (Vincent Ginot), the “agent” is a software technology. The agent is specific software in an object-oriented paradigm, inheriting from the generic class “object” but with more specified properties (e.g., autonomy, etc. see Chapter 1). Such software can be used for the implementation of numerous software objects, then “agentified”, like graphic interfaces, or much other software. Second, in a multi-agent framework, the agents can be the avatar of non-human active entities like ants or non-living entities as well, like fluid drops in [SER 99]

or sand in numerous models of sandpiles inspired by [BAK 96]. Third, the design of an artificial society does not imply that the relevant agents are designed to represent individuals. Accordingly, a non-individualist ontology is possible for multi-agent systems, even if the individualist ontological design prevails (ie. "individual-based" in ecology [GRI 99], "agent-based" in economy [TES 06]). It should be noted that this relation observed between ontology and methodology, described here in the case of multi-agent systems, could take at least two general forms. From a realistic point of view, ontology should determine the methodology that has to be adapted in an optimal way to the supposed nature of the entities regarded as existing in the reality. From a pragmatic point of view, methodology should determine ontology, which has to be adapted in an optimal way to the supposed nature of the procedures and experimental devices considered as effective. In other words, a pragmatically oriented methodology, corresponding to a specific scientific project (a model is used to do something) within a particular academic context presupposes, explicitly or implicitly, à related ontology with its own goal, relevance, and limits.

According to Bousquet [BOU 01] it is possible to consider social groups or institutions as agents (see also [LIV 87]); for instance firms are agents for many micro-economists, although they are organizations of several individuals. It is also possible to consider predetermined social structures, which are given in the initial design of a meta-ontology, like in the AGR (Agent-Group-Role) or extended AGRE (E stands for environment) framework ([FER 05], see also Chapter 1). Finally, in some other agents' architectures, the object of the analysis and the subject of interest as well, are not the agents (individuals) themselves. Agents act only as the support of other entities of interest, as in the models of (auto-organized) emergence of shared language structures, reviewed by [HUT 02]. Such ontology is not reducible to the orthodox view of methodological individualism, since the entities of interest are infra individuals [SPE 97] and knowledge is not a predetermined content inside the head of individuals, but the product of an autonomous process, distributed within groups [HUT 95]. This suggests interrelations—in both directions—between the social science realm (modelling paradigm) and the software design realm (computer paradigm). This implies a non-neutral relationship, and may be dialectic. On the one hand, the choice of a particular design at the software level could be non neutral for the related academic realm, embedded in some implicit or explicit commitments in all the discussed perspectives: ontological, methodological and epistemological. On the other hand, ontological, methodological and epistemological commitments from the object domain in social sciences should imply specific architecture at the software level. As both Chapters 1 and 12 are dedicated to describing tools and patterns from the software design side (including; in particular, the 4-Quadrant diagram introduced by Ferber in Figure 1.4.), this section is devoted to summarizing similar oppositions from the social sciences side.

3.1. Sum Up: A 4-Quadrant View on Social Sciences Debates

Hollis [HOL 94] introduces a useful 4-Quadrant diagram, which allows us to sum up our previous discussion on the epistemology of the social sciences together with our later methodological discussion on action and structure. The first opposition (in columns) stands for explanation/understanding while the second one (in rows) stands for action/ structure.

An example of the methodological approach corresponding to the opposition action/structure is: individualism/holism. An example of relevant notion is "cause" for explanation and "reason" for understanding. On Figure 1, Column "explanation" "systems" is taken by Hollis as an holistic approach which gives an account of individual actions in reference to an external causal power from some larger entities (or extra-individual structures). Accordingly, the explanation is "top down" from the system to the individual. On

the contrary, individualism refers to an approach that considers the structure as the result of the action of individual "agents". The corresponding explanation is then "bottom up", because the structures are nothing but the product of the action of individual agents"[2]. Let us remark that "classical" game theory [DAV 83, KRE 90] deals with strategic interacting agents, ignores collective entities and is a typical representative of South-West quarter (agent). By contrast, Hollis considers The Rules of Sociological Method of Durkheim [DUR 85] as a "classical case for thinking holistically", since social phenomena, taken "as things" are external to individuals, and generally human beings, including researchers, have no direct access to those external things[3].

	Explanation	Understanding	*Approach*
Structure	Systems	Games	*Holism*
Action	Agents	Actors	*Individualism*
Notions:	*Causes*	*Reasons*	

Figure 1: (adapted from) Martin Hollis' 4-Quadrants [HOL 94]

The second column deals with understanding, that is to say "interpretative" or "hermeneutic" social science; "the social world must be understood from within, rather than explained from without. Instead of seeking the causes of behaviour, we seek the meaning of action" [HOL 94, pp. 16-17]. According to the Weberian tradition and the methodological individualist approach in sociology (i.e. [COL 90, ELS 89a, BOU 03]) an action must make sense for an individual ideal-type. Then "actors" in the South-East (bottom-right) quadrant is the relevant case. The reasons for collective situations must be sought in their individual determinations (ideal type) and in the interaction between individuals, as Elster said: "There are no societies, only individuals who interact with another" ([ELS 89b, p. 248], quoted by [HOL 94, p. 19]). For Boudon [BOU 03]) the key psychological assumption is that action must be founded on some "good reasons" that could be apprehended and understood (*verstanden*). In other words, motives could be reconstructed rationally (in a wide cognitive sense, including axiological rationality). The building of social phenomena is then again a "bottom up" process. The North-East (top right) quadrant is devoted to the holistic side of understanding, where the key question concerns the inter-subjective dimension of the social relationship, namely the philosopher's problem of Other Mind: "how one mind can know what is in the mind of another" ([HOL 94], pp. 146). The corresponding ontology concerns inter-subjective entities, which are different both from the top left, where entities are objective structures at the whole level and from the bottom right, where the relevant basic entities—the actors have subjective motivations, but without ontological collective dimension. Hollis relates this question to the numerous debates surrounding Wittgenstein's notions of "games of languages", and "rule following" [WIT 53]. Accordingly, "Games" in the top right quadrant is related to rules, practices, usage and "ways of life", from an inter-subjective point of view. It is important to distinguish this meaning of "game" from that of "classical" game theory, which is clearly in the bottom-left quadrant. In the latter, the rules of the game are common knowledge, that is to say, everybody knows the rule, everybody knows that everybody knows the rules, and so on recursively, *ad infinitam* [AUM 76]. The problem of Other Mind does not make sense in such a world. On the contrary, in Wittgenstein's games, the rules are not the pre-condition to constitute and regulate the game, but they constitute the game itself, which makes sense only within *ad infinitam*.

[2] For recent aspects of this debate see [KIN 86, TUO 90, UDE 02].
[3] For other interpretations of Durkheim's work, see, among others: [CHER 98, BOR 00, SAW 01].

For Wittgenstein, rules are not an external description of a procedure that has to be internalized in order to produce a particular behaviour. In other words, following Wittgenstein's metaphor, rules do not operate like 'rails compelling a locomotive'. On the contrary, Wittgenstein develops a non-causal approach to rules, which far from being abstract, is practice-oriented. Rules have no meaning outside their practical application "Obeying a rule is a practice" [WIT 53, § 202]. They constitute a family, and it is not possible to provide a definitive account of what is "to follow a rule": "No course of action could be determined by a rule, because every course of action can be made out to accord to the rule" [WIT 53, $ 201], therefore a rule cannot be used to explain an action (except in a given scientific (game of) language). Following a rule is different from understanding it or talking about it. À rule can have a diversity of potential meanings or applications. Wittgenstein's *nominalistic* "family resemblance" theory and the under-determination of interpretation are promoted as revealing the determination of sense by usage in given ways of life rather than by objective reference.

More concretely, a critical feature of rules in the "real world" is the uncertainty of rule effect, linked to the incompleteness of rules. In their common work on rule dynamics, the organizational sociologists March and Zhou introduced the notion of rule implementation [MAR 97, ZOU 97], which refers to "the complication involved in translating rules into action" ([MAR 00], p 22). As their scholars suggest pre-programmed implicit or explicit rules to process in the rule implementation, their rule-following could be subject to the so-called "Kripkensteinian" criticism (Kripke's sceptical reading of Wittgenstein in [KRI 82]). Considering rule-following as an independent context process, Kripke claims that rule-following leads to an infinite regression if a rule needs another rule to be interpreted. This logical paradox[4] is generally rejected by Wittgensteinians, for whom following a rule is not an interpretation, but a social activity. À social practice does not consist of a set of rules, although it is based on rules. According to Winch [WIN 58], "rules [must] have a social setting" (p. 33): social practices exist because there are previously usages, customs and ways of life able to found the practices that will result in following the rule. Situating its rule within a specific context of practices of a community is the only way to observe the meaning of the rule that emerges from practices and interactions.

An example of this latter sense, corresponding to the inter-subjective nature of the top right quarter is Reynaud's case study of the Paris Metro Workshop [REY 05] For Reynaud, the meaning of rules appears within usage. In the context of existing usage, linked to former rules, the term of rule induces strategies that generate practices, which are not yet usage as such. Usage is then the ex post emergent result from practices, as viewed by some external observers, or some reflexive actors. In other words "actors take into account the rules in usage to induce the emergence of a next usage of this given rule" (see also [REY 02]).

For Hollis, Wittgenstein's games are inter-subjective normative structures, external to each player, but internal to the collectivity of players, situated both culturally and historically[5]. This contrasts with classical game theory where there is no collectivity of player, but only a set of individualistic entities, the agents, which internalize all information. Such an approach can also be related to the idea of distributed knowledge within groups [HUT 95]. For the project to model artificial societies—or economies—by means of multi-agent systems, this

[4] Numerous logical paradoxes do not work as soon as one considers an historical world, as underlined for example by Archer [ARC 95, ARC 98], see also [SAW 01] for the relationship with the historic dimension of social emergence. This is also the case for infinite regression such as in the case of common knowledge, where the introduction of a social inter-subjective dimension allows us to avoid the infinite regression see [ORL 04, PHA 07].

[5] For a criticism of this sociologically oriented view, see [CAV 79].

Wittgensteinian position is a real challenge. On the one hand, model building activity presupposes a sufficient degree of generality to abstract sufficiently generic relationships or at least "constraints to the operations" (following the terminology of Livet, Chapter 8). On the other hand, the Wittgensteinian position is intellectually stimulating and somehow necessary to complete the 4-Quadrant-view with a possible philosophical approach of social science, but could be problematic with respect to the model-building approaches. In particular, in the first Wittgensteinian approach to the philosophy of the social sciences, Peter Winch [WIN 58] defends a dependent-concept view of science, that is more coherent with visions of plurality of approaches (or "points of view"; see: .2.2. and the account of Putnam's internal realism or Quine' relativity of ontology in Appendix 1, S A.1.1.3) than with the traditional vision of the unity of scientific methods, or the possibility of testing theories by confrontation with factual evidences: "Our idea of what belongs to the realm of reality is given for us in the language that we use. The concept we have settles for us the form of the experience we have of the world [...] The world is for us what is presented through these concepts" ([WIN 58], pp. 15). Close to the internal realism of Putnam, this means that a particular question makes sense only from a particular point or view or system of belief, with specific ontology. This is not contradictory with the model building approach, in itself, but is far from the received view of scientific activity.

With hermeneutics and the Wittgensteinian approach (among others), understanding could lead to relativism, either in the actor or in the game perspective. Then, at least, the question of theoretical pluralism is set out. As suggested in Appendix 1 (S$ A.1.1.) and above in that appendix, these questions remain open. In particular, the rejection of monism, a stronger form of "scientific realism" (also called "metaphysical realism" by [PUT 81]) does not imply a relativist position. This is compatible with the internal realism of Putnam [PUT 81] as well as with more anti-relativist positions such as the "structural realism" of Poincaré and Worrall (see Appendix 1, $ A.1.1.4 for references). In practice, there are many ways between strong relativism and strong realism and then many ways for pluralism. This is well established among sociologists [BER 01, BER 03], and this idea is developing among economists [SAL 97]).

Finally, as emphasized by Hollis, the main challenge for social sciences is to surmount these boundaries and set up plausible combinations of approaches, first between individualistic and holistic perspectives, second—maybe less easy—between explanation and understanding. A multi-agent model is by construction a system composed of entities and relations; which includes the possibility of a multi-scale integration (through a relation of composition) and multi-perspectives. Such a framework is then a candidate to support the integration—in the explanation perspective—of both agent and system standpoints. Moreover, the cognitive functions are candidates to help the formal investigation of both subjective and inter-subjective determinants of action and structures. That is the basic meaning of the 4-Quadrant theory introduced by Jacques Ferber in Chapter 1 (section 1.3; Figure 1.4.). Initially introduced for a pure system-design purpose, this schema is extendable to the social sciences, in the way discussed here (see [PHA 07, DES 07], and more generally all the works initiated by Cristiano Castelfranchi and Rosaria Conte and co-authors since [CON 95]—in particular [CAS 98] and [CAS 00]). On the social science side, recent developments go beyond the boundaries or over the frontiers that could provide a stimulating theoretical background for an "integral" approach of the multi-agent modelling of artificial society, as seen in the next sub-section.

3.2. Beyond the Boundaries, Over the Frontiers: Towards a New Synthesis?

Among the recent candidates of paradigmatic reduction one can thus count structural

individualism [WIP 78, WIP 87, UDE 02], the morphogenetic approach [ARC 95, ARC 04], the "systemism" [BUN 98, BUN 04] or non-reductive individualism [SAW 02, SAW 03]. These approaches are generally trying to articulate ontological, methodological and epistemological constraints [6].

Ontological: because the choice of the concepts in the course of descriptions and explanations can be viewed as a representation of the ultimate nature of the social. This social ontology is focused on the interaction, synchronized or not, between structural properties and personal and/or inter-individual properties. Society is represented neither as à set of individuals, nor a supra-individual entity, but as a system of "inter-connected" individuals having global properties sometimes reducible to the individuals and their interactions, but also sometimes irreducible to the individuals and their interactions. The category of "emergence" (Chapter 14) is closely associated with this irreducibility of the various layers of the social reality [SAW 04].

Methodological also: because the tools and the methods must be adapted to the study of the relations of mutual or systemic influence between the various levels of social reality. Hence, this is the case on the one hand with the frequent use by some sociologists of the concept of "social mechanism", and on the other hand the development of the activity of modelling. According to Bunge [BUN 04], systematism and mechanism are "twin concepts". The mechanism of a system is "its peculiar functioning or activity. [..] an essential mechanism is the specific function of a system—that is, the process that only it and its kind can undergo" (p. 193). These mechanisms are generally regarded as "unobservable". The researcher needs to build stylized representations of them, and to this end he uses the various techniques of modelling [HED 98].

Epistemological finally: because it is necessary to establish the compatibility of the formal approach of the social mechanisms with the *nomologic* dimension of social sciences. For some this compatibility is at the very least problematic. Through its plea for the concept of "mechanism", Elster [ELS 98] acknowledge the generally accepted idea of an epistemological gap between "law" and "model". He does not just see scientific law as "the antonym of mechanism" but as an old conception of explanation that has to be replaced. As said previously (S .3.2.), this "gap" is by no means a necessary reality: it comes from a too narrow conception of the *nomological* dimension of the social sciences. And that is precisely what Bunge perceives when he writes that "mechanism-based explanation [..] is not at odds with law-like generalizations, and it is therefore not an alternative to law statements. Rather, hypotheses about mechanisms are components of deep scientific laws. In other words, 'mechanism' (or 'translucent-box') opposes 'phenomenological' (or "black box'), not "lawfulness" [BUN 04, pp. 200].

These questions are developed later (S .4.2.) concerning the combination of three views of modelling in sociology, after discussing the status of modelling in economics.

4. Theoretical Modelling, Virtual experiments and empirical Relevance

It is frequent to oppose in principle economics and sociology not only from the point of view of their respective delimitation of their object but also of their respective methodology and epistemology. While economics is conceived as exclusively focused on a narrow dimension of action—"under-socialized", rational in the definition of the means— and

[6] Although ignored, this idea to integrate structure, network and action has existed in sociology since at least [WIP 78]. An important distinction must be made here between the "critical realism" of Archer and Sawyer and the "analytical realism" of Hedström. For the first, ontology and methodology are strictly linked, while for the second, even the idea of "levels of reality" is contestable.

producing many "unrealistic" models based on an instrumentalist epistemology [FRI 53], sociology, by contrast, would be mainly directed towards a more general dimension of action—"socialized" even "hyper-socialized", rational and irrational in the definition of the ends and means—, and would produce an empirical knowledge based on a realistic epistemology. It is not necessary to insist on the limits of such an opposition for sociology.

The development of actionist sociology is certainly related to a general theory of action—Coleman [COL 94], for example, defines in a provocative way economics as a subfield of sociology; but actionist sociologists also take their distances with realistic epistemology and frequently use the techniques of modelling. Symmetrically, on the side of economics, one observes contemporary developments, which escape from this "stereotype" partition between the two disciplines: economics of reciprocity, identity, language, religion, social interactions for example. Beckerian works [BEC 76, BEC 00] are the paradigmatic example of such imperialism of economic methodology into the sociological domain. The theory of "informational cascades" that has multiple applications (speculative bubbles, investment decisions, etc.) shows a desire to widen the economic approach of action, integrating more interactions [BIK 98, CHA 03, ORL 95, 98, MAN 00]. From a more epistemological and methodological point of view, certain economists not only reject instrumentalism but favour the observation of the behaviours to elaborate a microeconomy and/or to try to elucidate unexplained remained anomalies. Blinder and Choi [BLI 90], Bewley [BEW 95] are studying economic actors (and in particular economic leaders) to explain the phenomenon of wage fixing in the context of economic reversals. Akerlof [AKE 84] analyses the market's dysfunctions (due in particular to information asymmetries) and/or the behaviour anomalies while taking as a starting point the theoretical and empirical contributions of sociology. This continuous transformation of the sociology-economics boundary does not mean that each discipline forgets its identity and a more or less strict principle of demarcation. Where some sociologists [CAL 98] criticize the performativity of the economic discipline and the dangers caused to social cohesion by the autonomization of economic discourse, certain economists are consider their discipline as a "universal grammar of social sciences" [HIR 85] able to take real profit from the insights and/or empirical descriptions of sociology.

In an interesting introduction to a special issue of The American Journal of Sociology on "Sociological and Economic Approaches to the Analysis of Social Structure", [WIN 88] sociologist Christopher Winship and economist Sherwin Rosen provide an example of the received views about difference and possible complementarities between sociology and economics. "Sociologists are often more empirical, and theories they develop are usually empirically motivated. As a result, many theories in sociology are quite broad and lack rigor. By contrast, most economic theories are built on the central organizing idea of voluntary action motivated by self-interest. This naturally leads to the construction of narrower and more highly constrained mathematical models and to more deductive types of hypothesis testing than the more inductive mode of investigation common in sociology. [….] Economists often view sociology as being too broad and diffuse and see the behaviour that sociologists examine as being too complex (at least initially for modelling). Similarly, it is not startling that sociologists view economics as being too narrow and based on unrealistic assumptions" (op. cit. p. S5). According to this received view, economists are specialized in the art of modelling while sociologists are specialized in inductive empirical explorations. Then, as the scientific approach deals both with deduction and induction, "sociology can supply economists with additional empirical understanding, economics hold out the promise of additional conceptual insights" (ibid, p. S7). This received view can be globally verified by the academic literature. Since Becker's works, the partition between economics and sociology has appeared not to be in the field of interest, but in the mythology used (theoretical

modelling versus empirical studies). This global assessment has some exceptions. Economists find discomfiting evidence for both instrumental and cognitive rationality in quasi-empirical studies by controlled experiments, for instance with behavioural game theory [CAM 03]. Some sociologists advocate a more formal and analytical approach for social theory [BOU 79, FAR 89, COL 90, HED 98], among others. Moreover, there is a significant commitment of sociologists to simulation, that can be viewed as equivalent to that of economists ([GIL 99, HAL 99] and Gilbert's Chapter 5 in this book). As the way of using "models" (for computing statistics, theorizing and simulating purpose) is the core of this question, this section is devoted to highlight some methodological points about the use of models in both fields, XX?

4.1. The Specific Case of Economics: From "Model Building" to Credible Worlds

As mentioned in [PHA 07] (§2.2) "model building" activity, isolates and analyses specific dependencies taken from empirical phenomena, focusing on few factors and excludes everything else, the objective being "to understand how just these aspects of reality work and interact" [SOL 97]. For Mäki, model building can be viewed as a quasi-experimental activity or as the "economist's laboratories" [MAK 92, MAK 02] (see also MOR 03]. Model building activity has then a quasi-empirical basis, and in many cases a pragmatic orientation. That class of economic models is used indeed to do something; namely to explore the explanatory power of some causal mechanism taken in isolation. According to Livet (Chapter 8), this does not presuppose a theory of the corresponding domain. The model is a way to experiment in a virtual world the explanatory power of some empirically selected assumptions.

Starting from the concrete, but selecting only some factors, such an abstract model is however "false by construction". The key question is then about the relevance of this abstract and fictitious modelling economic world for the explanation of related empirical phenomena in the "real world". This subsection is mainly structured as an account of [SUG 02]. This stimulating work claims that abstract and somewhat unrealistic models can be useful and relevant for the knowledge of the so-called "real world". This latter expression is widely used by economists, in particular theoreticians, to distinguish the abstract worlds of the models from the "real world" outside of these fictional worlds. This does not mean that modellers are all pure formalists playing a game with "imaginary" models (following the classification of Achinstein [ACH 68]). Conversely, talking about a "real world" does not imply a metaphysical realistic commitment (Appendix 1). This is just to underline the recognition of a problematic relationship between the abstract world of the models and the concrete empirical reality. As this recognition is widely accepted, there are many ways to answer the problem, and Sugden deals with some of them.

Sugden's account of the status of theoretical models in economics is based on two case studies. One of these cases is Schelling's "checkerboard" model, widely referenced in this book, as a paradigmatic example of agent-based model, generating an emergent phenomenon (clustering of segregated inhabitants). For Sugden, the strategy of Schelling is also typical of those of economists. Schelling claims that a regularity R (or stylized fact) can be found in the empirical phenomena: here persistent racial segregation in housing. He claims also that this regularity can be explained by a limited set of causal factors F (parsimony), here simple local preferences about neighbourhood. For Sugden this approach is equivalent to implicitly making three claims: R occurs (or often occurs) F operates (or often operates) and F causes R (or tends to cause it), "Neither present any these claims as a testable hypothesis, but each offers informal evidence from selected case studies which seems to support the first two claims. [...] the formal model is very simple, fully described and self contained world" [SUG 02, pp. 113-114]. The key question is now about the relationship between this abstract, sel-contained, hypothetic world and the "real world". Sugden exposes four strategies for this

relationship: (1) conceptual exploration; (2) instrumentalism; (3) model as explanatory metaphor and thought experiments, incompletely deductive, for which (4) inductive inference is requested (credible world argument).

(1) For Daniel Hausman [HAU 92], theoretical models in economics can be used for "conceptual exploration" rather than for "empirical theorizing". Conceptual exploration only concerns the internal properties of the model itself, without taking into account the question of the relationship between the "model world" and the "real world". In other words, the study of the model's properties is the ultimate aim of this approach. This can be valuable, because there are numerous examples of unsuspected inconsistency or unidentified properties in the existing theoretical models. From the division of labour standpoint, such an approach can be viewed as a momentum of a collective process of abduction (in the broad sense) that can be combined with empirical momentum both upstream and downstream. For example, a model can be built in a first step to answer a specific empirical problem (a); the properties of this model could be studied and our knowledge of this model could be enhanced in a second step, fully separated (b); and the resulting knowledge from the model conceptual exploration could be subject to appraisal with respect to the relevant empirical phenomenon is a third separate step (c). Each step could concern different specialists in the division of labour between researchers.

Conceptual exploration is also valuable by itself, because it is possible to exhibit conditions of possibilities or counterfactual investigations for a given empirical claim, as underlined by Sugden: "Schelling is presenting a critique of a commonly held view that segregation must be the product either of deliberate public policy or of strongly segregationist preferences. The chessboard model is a counterexample to the claims: it shows that segregation could arise without either of those factors being present [...] Schelling is not asserting 'R occurs, F operates, and F causes R'. All he asserts is: 'R could occur, F could operate, and F might cause R° ([SUG 02], pp. 115). Such conditions of possibilities can point out some possible lacks in the existing theories. Then, conditions of possibilities can be useful for empirical purpose, even if the related effects are not an effective explanation, but a possible - sometimes credible - explanation. From this point of view, conceptual exploration like Schelling's model must not be viewed as a complete theory of the phenomenon, but as a partial and useful exercise, that provides a candidate explanation to the related phenomenon, not the ultimate explanation: "The theorist is declaring his confidence that this approach is likely to work as an explanation even if he does not claim so to have explained anything so far" ([SUG 02], pp. 116, underlined by Sugden). Then, conceptual exploration allows us to check the internal consistency of the model and to enhance our knowledge and confidence of this model as well. But if conceptual exploration is not by itself an empirically oriented activity, it can be both empirically rooted and relevant for empirical investigation. This empirical relevance needs an additional epistemic stance about the relationship between the "world of model" and the "real world".

(2) A first discussed epistemic stance is known as instrumentalism, for which concepts and theories have no sense from a realistic point of view (they are not true or false, or correctly related to real entities) but are merely useful instruments for predicting empirical phenomena. Although the question of Friedman's instrumentalism has been discussed, the statement of his famous chapter on the Methodology of Positive Economies [FRI 53] is often commented as a paradigmatic example of instrumentalism in Economy [HAU 89, SUG 02]. The instrumentalist stance summarized previously is exactly contained in the following claim (p. 7). "The ultimate goal of positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e. not truistic) predictions about phenomena not yet

observed". As numerous economists, Friedman talks of "theories" as a formal language (e.g. model) building on some abstract simplified relationship: "it is a 'language' designed to promote systematic and organized methods of reasoning (... and …) a body of substantive hypotheses designed to abstract essential features of complex reality" (p 7, italic part is quoted from Marshall, see note 5). Accordingly, the relevance of a model depends on both logical and factual considerations. The logical statement is common to several economic methodological stances, including the previous "conceptual exploration". Taking the assumptions as given, is the model internally consistent? The corresponding checking process is known as "internal criticism" or internal validation of the model. According to the formal systems theory, Friedman adds a more problematic condition of completeness: "'The canons of formal logic alone can show whether a particular language is complete and consistent, that is whether propositions in the language are 'right' or wrong"(ibid).

On the empirical side, quoting the three following statements can summarize Friedman's stance:

(S1) "Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which is intended 'to explain'. Only factual evidence can show whether it is 'right' or wrong' or, better, tentatively 'accepted' as valid or 'rejected' [...] the only relevant test of validity of an hypothesis is comparison of its predictions with experience" (pp. 8-9). According to this instrumentalist stance, empirical assessment by prediction is always imperfect and subject to multiple concurrent solutions.

(S2) "Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it [...] The validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses [...] If there is one hypothesis that is consistent with the available evidence, there is always an infinite number that are" (p. 9).

(S3) Finally this leads to the famous and provoking statement about the unrealism of assumptions. "Truly important and significant hypothesis will be found to have 'assumptions' that are widely inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumption (in this sense)... To be important [...] a hypothesis must be descriptively false in its assumptions" (p. 14). As a result, for Friedman a hypothesis cannot be tested by the realism of its assumptions (op. cit. S III pp. 16-23). On the contrary, for Friedman, fruitful false assumption can lead to relevant predictions and we must admit a false assumption "as if" it was realistic or true. This third statement is built about a maximization principle, on the example of the density of leaves around a tree: "I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbours, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position. […] the hypothesis does not assert that leaves do these things but only that their density is the same as if they did. Despite the apparent falsity of the assumption' of the hypothesis, it has great plausibility because of the conformity of its implications with observation" (pp. 19-20).

We have dwelled on Friedman's methodological and empirical stances because of their influence on several economists. But many disagree with him. Instrumentalist goals are mainly predictive, not explanatory. On this view, a theoretical model is just a compact description of a set of predictions. Ontological assumptions do not make sense from an empirical point of view, either they do not refer to real entities or the truth or falsity of the relation to those entities so not matters for the relevance of the model. For Sugden, Schelling's model cannot be understood from an instrumental point of view, because the goal

is an explanatory one, and Schelling does not propose "any explicit and testable hypothesis about the real world" [SUG 02, p. 118]. Because Schelling tries to connect real causes (as segregationist preferences) with real effects (segregationist cluster emergence), and tries to persuade us of the credibility of the corresponding assumption, Shelling's unrealistic model is "supposed to give support to these claims about real tendencies" (p. 118). For Sugden, this method "is not instrumentalism: it is some form of realism" (ibid.).

(3) Economic models can be viewed as explanatory metaphors, or as abstractly isolated thought experiments. For McClosey [CLO 83], models are metaphors. Accordingly, model evaluation must work in the same way as metaphor evaluation: "Is it illuminating, is it satisfying, is it apt?" (Op. cit. p. 506). Following Gibbard and Varian [GIB 78], Sugden suggests that Schelling's model could be more adequately viewed as caricatures, because "the ingredient of a caricature must be taken from the corresponding reality" (p. 119) and the assumption of a model may be chosen "not to approximate reality, but to exaggerate or isolate some feature of reality" ([GIB 78], p. 676). According to Hausman [HAU 92], economic modelling "formulates credible (ceteris paribus) and pragmatically convenient generalization* by means of what he calls an "inexact deductive method". When Hausman talks about inexact propositions, Mäki [MAK 92] talks of "isolation": theoretical models describe an isolated aspect of the empirical reality: "a set of elements is theoretically removed from the influence of other elements in a given situation" (p. 318). For Sugden, this method is the opposite of instrumentalism: "a model explains reality by virtue of the truth of the assumptions that it makes about the causal factors it has isolated" ([SUG 02], p. 121). That is to say, the more relevant an assumption is with respect to empirical reality, the more explanatory would be the model. In his paper on the method of isolation in economics, Mäki [MAK 92] underlines the similarity between theoretical isolation and experimental isolation; in both cases, a limited number of elements and relations are studied, by isolating them and excluding some others.

Isolation is to the advantage of experimental control, in a "clean" but somewhat unrealistic environment, which allows us to study an aspect of empirical reality. Economists, in particular, use the notion of stylized facts, first introduced by [KAL 61] in a context of growth theory.

For Kaldor: "Any theory must necessarily be based on abstractions, but the type of the abstraction chosen cannot be decided in a vacuum: it must be appropriate to the characteristic features of the economic process as recorded by experience. Hence the theorist, in choosing a particular theoretical approach ought to start off with a summary of the facts which he regards as relevant to his problem. Since facts as recorded by statisticians are always subject to numerous snags and qualifications, and for that reason are incapable of being accurately summarized, the theorist, in my view, should be free to start off with a 'stylised' view of the facts—i.e. concentrate on broad tendencies, ignoring individual detail, and proceed on the 'as if" method, i.e. construct an hypothesis that could account for these 'stylized facts" without necessarily committing himself to the historical accuracy, or sufficiency, of the facts or tendencies thus summarized (op. cit, p. 178, see [LAW 89] for a more general discussion and a realistic account of the Kaldorian stance).

For Mäki, models are thought experiments. For Sugden, this methodology is similar to the two first steps of Hausman's inexact deductive method [HAU 92]: (i) the modeller has to formulate credible (ceteris paribus) pragmatically convenient generalizations concerning the operation of relevant causal variable (i.e. the factors that have been isolated in Mäki's method); (ii) the modeller uses deductive reasoning to identify what effects these factors will have under these specific hypotheses (i.e. this particular isolated environment). As Sugden points out, simplifications need not be isolations. The simplicity of the checkerboard model makes the analysis easier than with sophisticated assumptions. It "does not seem right to say

that the checkerboard isolates some aspects of the real cities by sealing off various other factors which operate in real cities" ([SUG 02], p. 127). Instead, most of the simplifying assumptions are convenient for the reasoning, but have nothing particular to do with the resulting phenomena; it could be changed or sometimes generalized without affecting qualitatively the properties or results of the models. For instance, multi-agent simulation experiments show that emergence of cluster with strongly segregated neighbourhood occurs from triangular or hexagonal neighbourhood instead of rectangular (Moore), from torus (periodic, two dimensional shape) instead of bounded checkerboard-like shape, etc. Moreover, it occurs also from small-world networks instead of regular lattices [FAG 07]. This paradigmatic result proves also to be "robust" to more sophisticated preferences [PAN 07]. Then, Shelling's model of segregation is said to be robust to changes in the specification of the model.

The robustness argument provides reasons to believe that the model is not specific but could be generalized, including the original model as a special case. As Sugden underlines, Schelling is satisfied in presenting a simple, suggestive and imaginative model, leaving to more technically oriented researchers the task to produce generalizations. This contrasts with most economics theorists who try to present their model in the more generalized form they can and use the simplest form only after, for pedagogical purpose. Nevertheless, checking for robustness helps to increase the modeller's confidence in the generality of the model. Experimenting the occurrence of segregated patterns with various assumptions in Schelling's model increases our feeling that such a phenomenon is likely to occur, even without any proof that it must occur. The corresponding cognitive process in an inductive inference forms the cases of already experimented models to more general model cases. But this mode of reasoning remains within the world of models, not between the world of the model and the real world, as Sugden underlines: some links between the two worlds are required: "it is not enough to be convinced that what Schelling has shown us to be true of checkerboard cities is also true of other model cities; we have to be convinced that it is true of real cities" (op. cit. p. 129).

(4) The credible world argument works as an inductive inference from the model world to the real world: the outcome is the recognition of some significant similarity between these two worlds (see Livet, Chapter 8). According to Hume [HUM 40], induction is grounded in an association of ideas that human beings think similar. The cognitive process of inference, certainly useful from both psychological and evolutionary point of view is incorrect from a logical point of view (Appendix 1, SA.1.1.1). Anyway, the result inference is an increase of credibility, that is a belief, not an objective proof of truth, "that being explained in, psychological terms, but not of being justified as rational" ([SUG 92], pp. 135). For Sugden, Schelling constructed imaginary cities, that generative mechanisms can easily make understandable; which could be viewed as possible cities, alongside real cities. We are invited to make the inductive inference that similar causal processes occur in the real cities. "Since the same effects are found in both real and imaginary cities, it is at least credible to suppose that the same causes are responsible" [SUG 02, pp. 130]. The "inductive reasoning" appears to be a part of Pierce's abducting process in a broad sense, that can be decomposed into three moments: abduction (a), deduction (d) and induction (i) (Appendix 1, S$ A.1.1.2). (a) The modeller observes that segregation occurs in the real world, and makes the abduction (in a narrow sense) or conjecture that segregation (S) is caused by individual preferences over neighbourhood structure (IPoNS). (b) The modeller experiments and deduces that in the model world, S is caused by IPONS. (c) The modeller infers that there are some good reasons to believe that IPoNS also operates in the real world, even if it is not the only possible cause of S. That is, IPONS is a credible candidate to explain S (condition of possibility) and the

model world is a "possible reality" "From this view, the model is not so much an abstraction from reality as a parallel reality. […] Although the model world is simpler than the real world; the one is not a simplification of the other. The model is realistic in the same sense as a novel can be called realistic […] the characters and locations are imaginary, but the author has to convince us that they are credible" ([SUG 02] pp. 131). Then, Schelling tries to convince us that his model world is a possible reality: as Sugden says, Schelling has constructed a model city inhabited by people who are in some way "Like real people".

In the Sugden approach, there are gaps between the model world and the real world. But economists sometimes use "rhetorical devices which tend to hide these gaps from view". Instead, it would be better to say explicitly the gaps problem is not resolving a fully / *logical* process, but contains an *argumentative momentum*, in which models can be candidates to be *credible counterfactual worlds*.

4.2. Models, Generative Mechanisms and Simulations in Sociology: New Perspectives"[7]

In sociology, models intended to produce hypotheses about "mechanisms" are called "generative models" [BOU 79, BOU 98, FAR 69]. At first approximation, they can be defined as stylized conceptual representations of mechanisms responsible for a given empirical phenomenon. À generative model can be characterized by its components (mechanisms) and by its structure. Even if the idea of mechanism is not new in sociology; it is since the 1990s that an analytical perspective in sociology has appeared [HED 98, HED 05, CHE 05]. The identification and the analysis of a mechanism make it possible to study the "mode of production of the phenomena" [CHE 98], in particular its "generativity" ([FAR 89], pp. 39-43). The generative mechanisms can be considered from a realistic [BUN 97, BUN 04, FAR 89] or pragmatic point of view [STI 91, HED 98]. In both cases, the principle is to explain the observed phenomena on a certain level (macro) through mechanisms of a lower level (micro).

À mechanism can be seen as a coherent whole of entities (structures and actors) with relations between these entities (system of influence, interaction and interdependence). À crucial question is to know if all entities can be considered, from the point of view of action, in an autonomous way. Hedström and Swedberg ([HED 98], pp. 21-23) distinguish three general classes of mechanisms: (1) the situational mechanisms (Macro—> Micro) relate to the contextual or structural elements which force the autonomy of the actors; (2) the mechanisms of action formation (Micro—>Micro) reflect the forms of rationality which the sociologist gives to the individuals; (3) the transformation mechanisms (Micro—>+Macro) correspond to the collective effects obtained by the composition of the individual actions. As Epstein [EPS 06] claims that Agent Based Simulation is a "generative social science", it could implement such transformation mechanisms.

When the individuals' actions are not modified by effects of composition due to the complex structure of their interactions, the transition from individual dimension to social dimension is only the result of an aggregation effect of individual behaviours. If on the contrary, the interactions between the individuals are strong enough, effects of composition may appear and beyond certain critical points change the collective qualitatively as well as quantitatively, or durably modify the structures. One speaks then about emergence ([SAW 01, SAW 04], and this book, chapter 14) or in certain cases of unexpected consequences [CHE 06]. Whatever the nature of the mechanisms, they have the common characteristics to be frequently unobservable. They act like "invisible codes" [CHE 05]. Because of this un-observability, the building of the mechanisms has the epistemic status of a conjecture [BUN

[7] . This section was developed freely starting from general orientations defined in [MAN 05, MAN 07].

97, BUN 04].

From a methodological point of view, Manzo [MAN 05, MAN 07] considers that the approach to sociological phenomena by generative mechanisms and models is related to a special form of methodological individualism. The various types of mechanisms mobilized in the construction of a generative model are conceived as implying a methodological form of individualism which one can describe as "complex" [DUP 92]. One of the sociological antecedents of this form of individualism is the so-called "structural individualism" proposed by Wippler and Lindenberg [WIP 78, WIP 87], see also [UDE 02]. This approach focuses "on the loop which links recursively the individual and collective levels" ([DUP 92], p. 19) in order to overcome the opposition between holism and individualism. Other approaches (evoked at § 2.) share the same goal, like the "non-reductive individualism" of Sawyer [SAW 02, SAW 03] or the "morphogenetic" perspective of Archer [ARC 95, ARC 98].Archer focuses on the phenomena of temporal shift in the morphogenesis of social phenomena. She criticizes in particular the social structure theory elaborated by Giddens [GID 84]. According to this latter theory, "structure" and "action" are in an uninterrupted coproduction process. But for Archer the problem of the relationship between the individual and the collective is not solved but "dissolved" by the amalgam of the various levels of analysis. To avoid this fusion, Manzo suggests a dynamic approach of the "generative model". This suggested approach analytically separates the macro level (structure) and the micro level (action) and produces specific conjectures about their modes of articulation. Accordingly, he proposes a dynamic version of the "Coleman boat" [COL 90] where "structure" and "action" have independent temporal scales but also a combined use of the various concepts of model (statistical, mathematical, simulation).

The process of 'combined modelling" could thus start by a descriptive empirical investigation using statistical models. This first phase would have the goal to identify systemic regularities the genesis of which remains hardly explained. The second phase, theoretical and conceptual, would then consist in producing explanatory conjectures to build a generative model, which would be formalized in a third phase. As a last step, simulation would produce quasi-experimental data the function of which could be to evaluate the explanatory relevance of the ideal model in comparison with the stylized facts of the descriptive statistics phase of investigation. The reiteration of these various phases is not far from the Piercian abductive process conceived in the broad sense (cf. above and Appendix A.1.1.2). Manzo notices however that these various steps correspond for the moment to disciplinary subsets, which do not work in a complementary way. Thus the sociology of "the variables" tends to stop with the descriptive phase while being based on statistical models of a growing refinement [ESS 96]. Analytical sociology is largely focused on the conceptual phase but rarely passes to the formalization and effective study of a model [HED 05]. Mathematical sociology favours the formalization phase to the detriment of the confrontation of the formalized model with the empirical data [FAR 97, FAR 05]. Finally, "computational social sciences would tend to do the same, by replacing mathematical models with simulation models" [MAC 02]. Manzo finished his article with a quotation from Halpin [HAL 99] to underline the great interest of such an integration: "interface between statistics, simulation, and sociological theory is critically important for the development of a sociology that is both theoretically sound and empirically founded, particularly when it comes to dealing with issues that are inherently complex"[8].

5. Conclusion

[8] 8. For a first review of the results of the empirical use of this integrative method, see [MAN 06].

The social and human sciences (SHS) need to understand not only individual motivations, but also the generative process that results in collective outcome from the individuals' interactions. Multi-agent systems (MAS) are computer science concepts and tools (Chapter 1) that allow us to design and implement agent-based artificial societies by means of agent-based modelling (ABM), e.g., building systems of interactive cognitive agents that may exhibit emergent properties and sometimes backward causation from the macro to the micro level (Chapter 14). The architecture of MAS makes explicit the nature of the social relationships between agents, as well as the individual determinants of actions, providing new perspective on the "structure-action" question. As an example, MAS provide a framework within which interesting inter-level questions can be asked, for example concerning the micro causes of emergent phenomena, or possible backward effects from structure to action. Simulations using MAS should be seen as a complement or a substitute for the classical methods of scientific modelling [AXT 00]. Their purpose can be to fit observable phenomena, or to explain stylized facts, or to couple models of varying levels (Chapter 7). Both Axelrod [AXE 97] and Gilbert [GIL 99] claim that simulation is actually "a new (or third) way of doing science". For Axelrod and Tesfatsion: "Simulation in general and ABM in particular is a third way of doing science in addition to deduction and induction. Scientists use deduction to derive theorems from assumptions, and induction to find patterns in empirical data. Simulation, like deduction, starts with a rigorously specified set of assumptions regarding an actual or proposed system of interest; but, unlike deduction, simulation does not prove theorems with generality. Rather, simulation generates data suitable for analysis by induction. In contrast to typical induction, however, the simulated data come from controlled experiments rather than from direct measurements of the real world" [AXE 05b]. À similar statement is implicit in [MAN 06]. In other words, ABM could be viewed as encompassing in some way both deduction and induction, by combining the two dimensions in a global abductive process, using specific assumptions and ontologies for the design of a computational model (Chapter 12), proceeding to controlled experiments (Chapter 11, 13), and finally doing empirical analysis on the data produced in the simulated world (Chapter 3).

Accordingly, by introducing new perspectives in the academic realms of the SHS, ABM provide opportunities for dealing in a new way with existing epistemological, methodological and ontological questions within these disciplines, in the context of existing traditions, briefly outlined in this appendix. But according to Axelrod, agent-based modelling as a "transversal" tool could also be a "bridge between disciplines" [AXE 05a], raising research questions common to many disciplines and facilitating interdisciplinary collaboration. For all these reasons, coming back within and across disciplines to the fundamental questions raised in these two appendixes is a necessity.

6. References

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