Epistemology in a Nutshell:

Theory, Model, Simulation and Experiment

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In the Western tradition, at least since the 14th century, the philosophy of knowledge has been built around the idea of knowledge as a representation [BOU 99]. The question of the evaluation of knowledge refers at the same time (1) to the object represented (which one does one represent?), (2) to the process of knowledge formation, in particular with the role of the knowing subject (which one does one represent and how does one represent it?), and finally (3) to the relationship between the representation and the represented object. Criteria of evaluation such as “validity”, “adequacy” or “truth”, as mentioned in [AMB 07], make sense only with respect to these three dimensions. An evaluation can thus (1) depend on the ontological nature of the object of knowledge, (2) relate to the relationship between subject and object—including the structures (cognitive, social) which organize this relationship, or (3) relate to the relation of similarity between the object and its representation as well. The relevant criteria of evaluation thus depend on the points of view adopted on these questions. As there are indeed a plurality of points of view in this field, the goal of this appendix is to summarize, as briefly as possible, the various positions adopted by the philosophers and to refer to the relevant texts of reference for more information (for a first outline [CHA 76, SCH 01]).

The first section[1] introduces useful discussions about the philosophy of theoretical knowledge and general epistemology, from a quasi-historical perspective. Section two[2] discusses the intermediary but central notion of models. Section three[3], more exploratory, introduces an approach to simulation as “concrete experiment”. It suggests that such a frequent claim in the literature, when precisely evaluated, can, to some extent, renew both the representational and the linguistic views on simulation.

1. Theorizing: the representation and Beyond

For common sense, the “objects” of the (perceptible) world such as they are given to our senses have a “real existence”. But to know the world, the human being uses entities, classifications and theories that are far beyond what is directly perceptible to our senses. Hence, to say that we” know” the world is only to say that we represent it from such a person's point of view. Beyond what is representable is what is conceivable or

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[1] Based on an initial contribution by D. Phan, F. Varenne

[2] Based on an initial contribution by A.F. Schmid

[3] Based on an initial contribution by F. Varenne

conceptualisable. But the “reality” of these un-observables is problematic. Do such abstract entities exist effectively independently of us, or are they simple constructions of the spirit? Moreover, as Putnam [PUT 81] argues, it is problematic to consider as real entities the objects both from the scientific and from the common sense points of view. According to the current state of physical theory, tables and chairs as we ordinarily perceive them would not exist as basic “real” entities. Rather, they are a collection of tiny particles in motion. Thus, would the individual belief that tables and chairs are real solid objects be false? By extension, the nature of the reality itself seems to be problematic. Up to what point are the objects of the perceptible world themselves not a construction of the mind? Finally, is the “real” world reducible to the way each person or culture believes it is? Are there some “objective” facts rather than matters of opinion concerning the empirical phenomenon? In philosophy, these ontological problems are tackled by the question of realism.

The Middle Ages opposed realism, for which the abstract entities (universals) that we use in our operations of classification exist effectively in nature on the one hand, and nominalism, for which these entities are only the product of thought. In logic, realism is the thesis according to which a property is true or false independently of the means we have in order to check it. In mathematics, realism holds that mathematical entities exist independently of the human mind [HER 97]. There are several forms of mathematical realism, such as (roughly speaking) mathematical platonism for which mathematical objects exist as abstract (non-spatio-temporal, non-physical and non-mental) entities or mathematical empiricism for which a priori mathematical categories do not exist, but mathematical facts are to be discovered by means of empirical research.

Currently, the so-called metaphysical realism [ZWI 00] is opposed to idealism and indicates a thesis (Z1) according to which an external reality exists (or reality “in itself”) independently of the existence of observers and of the knowledge that the observer could have of it. To define a realistic position in philosophy of knowledge, Zwirn introduces an intermediate thesis on the intelligibility of reality (Z2): reality by itself consists in understandable entities regulated by mechanisms, which are (completely or partially) accessible for us. Epistemic realism (7.3) is then the thesis according to which scientific theories are accepted as “true” if their objects refer to real entities and that their mechanisms hold effectively in reality. For epistemic realism, the confirmation of a theory is thus its adequacy with reality. If one does not adopt the Z3 point of view, knowledge considered as a representation raises the question of “styles of representation” [HAC 83]. Representations can thus differ by the perception that a particular subject has (the significant experiment), by the means of which his mind proceeds (rationality, cognition), or by the technical know-how he uses (practice of representation). Historical studies suggest that these three dimensions are interdependent and are changing over time. But surprisingly, the desire to produce objective “and timeless criteria” of the scientific activity have hidden these interdependencies for a long time.

1.1. The Origin of Knowledge: Experimenting or Reasoning?

The “folk” notion according to which our knowledge results both from our sense experiment and our reasoning seems reasonable. However, the focus on one or the other of these poles is the basis of two initially antagonistic philosophical traditions: rationalism and empiricism. The epistemological empiricist position (according to which “any knowledge derives from our sense experiment”) indeed developed against the rationalist epistemology of Descartes (for which “knowledge is initially founded on reason”). The combination of realism and empiricism (as for Locke) is problematic. In particular, it raises the question of the “truth” of knowledge (in the Z3 sense) when those are derived from a sense experiment by an inductive approach. This "problem of induction" as highlighted by Hume, one of the founders of the empiricist tradition.

The inductive approach raises the problem of the switch from a singular statement of observation to a general statement of a mono-logic type ("empirical law"). This switch is carried out by an inductive inference, which proceeds by generalization starting from a certain amount of observations. From a logical point of view, this inference is incorrect, because whatever the number of observations to which one proceeds, a later observation could always come to contradict this generalization. However, from a practical point of view, this generalization corresponds to an effective cognitive mechanism as well. The repetition of observed facts generates a wait for a repetition. This cognitive mechanism is described as induction, according to common sense by Popper [POP 72]. For Hume, it is the habit that is the cognitive framework in which the law of causality comes to be built as an acquired reason (far from being a necessity in the things). Starting from an empirical criterion of verifiability Hume aimed to distinguish "meaningful" empirical statements from the others (empirically meaningless or metaphysics). Thus, starting from practical considerations, Hume sought to found an empiricist epistemology, which uses a principle of verifiability founded on inductive inference. However, he had shown himself the problematic nature of the induction principle from a logical point of view.

The logical bases of induction are still a problematic question for generic conceptions today, even in probabilistic versions [BOU 72, HAC 01]. Under these conditions, to combine an epistemic realism (Z3) with a naive version of the epistemological empiricist principle according to which "there is nothing in our understanding that doesn't come from the senses" led to a paradox. An "empirical law" obtained by an inductive method could be supposed to be "true" only if it "reflects" reality (it would be the "mirror of reality"), whereas there is no logically valid criterion to check this statement. Given the impossibility of knowing the truth of such "laws", the "Copernican revolution" of Kant is a first answer to the questions of induction and epistemic realism. For Kant, the question is no longer how our representation may conform to an object as such, but rather how objects may necessarily conform to our representation. The Kantian synthesis combines a possible metaphysical realism with the principles of an idealism said to be transcendental. If there is a reality apart from the human mind, it is unknowable. One cannot reach the things "in themselves" (*noumena*), independently of our cognitive capacities (the relationship with representations for Kant), but only the phenomena, as objects of our sense experience, and related to our faculties of representation. Since science cannot be properly explained as coming from observations alone, it is necessary to claim that scientific knowledge largely stems from our capacity to produce synthetic (non-analytic) a priori judgements. Those judgements are based on pure forms of intuition (such as space and time) and on the spontaneous concepts of our understanding as well, rather than on direct empirical data. According to Zwirn, the postulate according to which we do not have access only to phenomena and to reality "in itself" is now accepted by the majority of realists and non-realists as well. A weak realism then supposes the reduction of a tension: how can one logically support the existence of a reality which one admits at the same time not to be properly knowable?

1.2. From Peirce's Pragmatism to the Conventionalism of Duhem and Poincaré

According to a post-Kantian point of view, the truth cannot result any more from an impossible correspondence with an unknowable reality. Peirce would try to substitute the criterion of truth-correspondence for anew methodological and social criterion. He describes knowledge as an investigation, which proceeds by successive revision of the beliefs with the aim of reaching some—temporary— "stable" belief from an inter-subjective point of view.

Then, the truth is “what the investigation tends to” [TIE 93]. The value of entities or relations used in academic knowledge lies in the practical effects which result from their setting in science and becomes socially auto-corrective and rational. Its objectivity results from the collective stabilization of the rational beliefs of the researchers. According to Peirce himself, such a collective process acts as an “objective idealism” (rather than transcendental). But it is also a fallibilism because these beliefs are always imperfect, revisable.

For Peirce the justified doubt is the reason for the investigation. The first arises more as a subjective state of dissatisfaction than from some objective empirical criteria. The later process can be decomposed into three steps: abduction, deduction and induction. Abduction consists in proposing an explanatory assumption (provisional conjecture) to explain a surprising fact, which activates the doubt. From this preliminary assumption, one draws the consequences by deduction. Finally the inductive phase is an empirical evaluation of the deduced consequences of the abducted assumptions, rather than a process of construction of assumptions (or laws) by inference and generalization. Negative results lead to reformulating a new assumption (by abduction) whereas positive results decrease the doubt. This whole process (with these three interrelated steps) is sometimes said to be “abductive” in a broad sense [WAL 04], while abduction in a narrow sense is just a moment of this process. This approach is anti-inductivist in the sense that the starting point is not a generalization of observations, but an art to produce relevant conjectures starting from the observation. This process is similar to those followed by a doctor from symptoms, or by a detective from clues. Peirce justifies this “art” in a naturalist way through an evolutionary argument: if human beings could not formulate such relevant assumptions (or conjectures), the mankind would have disappeared a long time ago.

No more than induction, abduction (limited to the narrow sense of an “inference to the best explanation”) is not a logically valid reasoning. An explanatory assumption can mobilize several interrelated conditions; some of them are not necessary when some others can separately have the observed phenomenon as a consequence. Moreover, the fact that one assumption seems convincing does not exclude the possibility of competing assumptions to be explicative too. In the case of physics, these questions were reformulated at the beginning of the 20th century, by Duhem [DUH 06] and Poincaré [POI 03].

For Duhem, a mathematically formalized physical theory is a system of interrelated symbols, which involves an indetermination because of the holistic nature of the theories. The induction facilitates the constitution of “experimental laws” starting from observations and stimulates the emergence of new enigmas. However, the interpretation of such laws presupposes a related symbolic and theoretical corpus in order to make sense. For Duhem, as for Poincaré, theories are provisional and their adequacy with phenomena is only approximate. Thus, an experimental phenomenon can be reinterpreted in a more suitable symbolic system. If a theory is consistent with a set of data, one could always build a more sophisticated theory using non-observables, the predictions of which will be more consistent with the same set of data. But two incompatible theories could also be in competition to explain the same empirical observations without it being possible to choose between them. The physicist will choose among the possible theories according to the criterion which will appear to be the most relevant with respect to some specific goal and to the rules of his (academic) community as well: simplicity, degree of accuracy of the results, coherence with other theories. The theories are then based on justified conventions because of their convenience to achieve specified goals. According to this conventionalist point of view, a theory is nothing but a useful formal system.

Accordingly, rather than depending on the correspondence between theoretical statements and reality “in itself “ (Z3), the validity of a theory refers (1) to the internal consistency of the corresponding symbolic system (2) to the inter-subjective agreement of the scientific community and (3) to the capacity of this theory “to save the phenomena’. However, for Poincaré, this agreement is not arbitrary: the efficiency of science results from the adequacy between the structure of the theory and the structure of reality.

1.3. From The Verificationism of Logical Positivism to Popperian Falsificationism and Lakatosian Research Programmes

Within the empiricist tradition, positivists of the “circle of Vienna” combined new developments from the formal logic and from the analysis of language (starting from Frege). Refusing synthetic a priori judgements, they consider that induction makes it possible to conceive laws by generalization and to organize knowledge according to the “deductive nomologic” model based on the rules of logic and mathematics. For them, the validity of a scientific law results from three principles: sense experience, logical coherence and verification

(1) A statement is “meaningful” if it is verifiable by a sense experiment. (2) Logical and mathematical deductions are “valid” if they check the principle of non-contradiction (in the propositional logic sense), but are meaningless without (1). The last principle (3), empirical validation, is logically problematic. Induction cannot be checked at the moment of the formulation of law (because of the logical problems of the inductive inference and of the rejection of the syntheses a priori): thus it can be only a posteriori by the evaluation of predictive capacity. The goal of logical positivism was to found empirical sciences on (1) the principle of verifiability (2) the unification of scientific language. This project presupposes in particular a distinction between the theoretical field and the empirical one. All these points were seriously questioned, in particular by Popper and Quine.

The epistemology of Popper considers that a theory remains a conjecture [POP 63]. A conjecture is not an inference and cannot be said to be true, but its consequences will be corroborated or refuted by means of critical experiment. Thus, it is a fallibilism because knowledge is imperfect and revisable by construction. Un-refuted theories are classified according to their degree of corroboration. In spite of constitutive uncertainty (due to their obligation to remain falsifiable) of the conjectured theories, the (weak) scientific realism of Popper tends to consider that certain theories are less “false” (or uncertain) than others. The function of the criteria of empirical adequacy of Popper is the inverse of the verificationism criterion of the logical positivists. For the latter, one formulates initially a theoretical conjecture, which one deduces consequences which will be subject to empirical tests. For the latter, one builds statements by generalization of empirical observations, draws some predictions by means of a deductive system and uses additional observations to confirm or check these previously established statements. One can see the forming of conjectures as a cognitive step close to abduction; but Popper imposes the testability of the conjectures just like the concept of empirical contents (derivable and empirically testable statements), as the criteria of scientificity. One can address two types of criticisms to this approach. (1) One cannot test a statement or an assumption separately (or individually) (Duhem-Quine). Popper however includes this difficulty in a sophisticated—but weakened —falsificationism. (2) Like the logical positivists, who consider the protocolar statements resulting from empirical experiment beyond doubt, Popper tries to base scientific knowledge on “objective facts” [POP 72]. Certain experiments are thus based on a measure for which one admits objectivity. But to regard a measurement as objective means to make an induction by supposing that the measuring instruments are functioning “as usual”. The question of the validity of a refutation is thus posed. If there is no certainty on the procedure of refutation, but only one conjecture, it is logically contradictory to hold the refutations for some: the Popperian criterion of demarcation between science and non-science is thus not refutable itself!

Imre Lakatos proposed a sophisticated methodological version of Popper's falsificationism. This version combines several traditions: empiricism with its determination to learn from experiment, Kantianism with its activist approach to the theory of knowledge, conventionalism with its stress on the importance of academic customs and consensus in methodology.

For Lakatos, scientific activity must be apprehended from cognitive frameworks called “research programs”. These programs represent the structural unit from which it is possible to give an account of the various forms of scientific development. Each research program can be analysed according to two dimensions and their interrelations. On the one hand the hard core, i.e. a set of axioms, which defines at the same time the ontology and the epistemology of the research activity. In principle, this hard core cannot be subjected to refutation. It thus constitutes an exception to Popperian falsificationism, which is more compatible with many empirical descriptions produced by both historians and sociologists: confronted with direct falsification, scientists seldom give up their theoretical constructions but develop alternative strategies[4].

On the other hand, there is a set of auxiliary assumptions directly or indirectly subjected to falsification whose function is to protect the hard core. Lakatos associates to the hard core the auxiliary assumptions—the so-called “positive heuristics” —of the research program (i.e. directions of research which are encouraged within the framework of the program). To the hard core corresponds the "negative heuristics”, i.e. all the elements and propositions that the scientists cannot touch without dissolving their own program. For Lakatos, this idea of negative heuristics makes it possible indeed to rationalize traditional conventionalism: “We can rationally decide not to allow the refutations to transmit falseness to the hard core as long as the corroborated empirical contents of the protective glacis of auxiliary assumptions increase” [LAK 77]. But, closer to Duhem than to Poincaré on that point, Lakatos considers that for logical and empirical reasons, when a program durably stops predicting new facts — when it becomes degenerescent and no longer develops an ability to comprehend or formalize such new facts—its hard core can be abandoned by scientists.

1.4. The Role of “The Conceptual Background” and the Rejection of the Myth of Unified Science

Nowadays, a great number of authors admit that induction and empirical testing as well presuppose some “background theories”. Thus, our perceptions as well as our experiments must be related to conceptual frameworks which work like a “filter” at the level of interpretation: the “facts” are thus partly cognitive, theoretical or social constructions. It is the thesis of the over-determination of the facts by the theories. After Duhem [DUH 06] and Bachelard [BAC 34], Hanson [HAN 58] refers to Wittgenstein to discuss the weight of interpretations in the observation, supposed to be objective both for logical positivists and for Popper as well. For Hanson, the facts are always constructed and “theory laden”. Being based on the distinction (due to Reichenbach), between the context of discovery and the context of justification, it shows that “shaping” the facts is one necessary moment of the discovery.

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[4] Popper conceded this fact but answered that falsification becomes conclusive when an alternative and better theory is available. But “better”: in what sense? Lakatos proposed that a theory could retrospectively be seen to be “better” when it can be said that it has lead to new predictions. Feyerabend objected that this was only retrospective epistemology and thus of no use. Nonetheless, this strategy (to overlook a falsification) is also coherent with the “maxim of minimal mutilation” of Quine [QUI 64] and with Livet’s putting into suspension hierarchy, as well [LIV 07]. See below an abductivist interpretation of such strategy as an attempt to maintain overall coherence by preserving the most deeply anchored beliefs [WAL 04].

Shaping the facts is necessary to their intelligibility: the explanatory systems "are built into our observations and our appreciation of facts and data" [HAN 58, p.3]. It is the same non-neutrality constitutive from the scientific point of view on its object which causes fruitfulness: "the paradigmatic observer is not the man who sees and recounts all that the normal observers see and recount, but the man who sees in the familiar objects what nobody else saw before" [HAN 58, p. 38][5].

At the beginning of the Sixties, the works of Quine and Kuhn destroyed the dominant idea in epistemology of a possible (objective) unicity of significances and of an access to the real world "in itself" (72) through experiment. Quine [QUI 60] extended the thesis of the under-determination of the theories with the thesis of the indetermination of the translation. It refers to a confrontation between different points of view on the world (or ways of referring) where "conceptual schema" introduce an anthropological and/or historical dimension in epistemology, not far from the "paradigms" of Kuhn [KUH 62]. Thus, there is a certain incommensurability between rhetoric and theories. In Kuhn [KUH 77] such incommensurability can result from the object of theories, the historicity of conceptual designs or the unicity of meaning. In Quine, the translation is possible, but it is not univocal. This destruction of the "myth of meaning" is as devastating for logical positivism as the problems of empirical realism are for the principle of truth-correspondence (where truth is defined as an adequacy between things and their representation). Finally, by different ways, one finds the problem of interpretative hermeneutics, which was developed later in the social sciences. The existence of different conceptual schema results in relativity of ontologies: to speak about objects and properties makes sense only with respect to a particular point of view. Even if relativism itself is severely contested [DAV 74, BOU 05], the contributions of Hanson, Quine and Kuhn question the unicity of the criteria of evaluation for the theories and then reinforce anti-realism. In the presence of the same phenomenon, observers who have different theories will have different perceptions and interpretations. In such a context, epistemology risks being dissolved by relativism (i.e. Feyerabend [FEY 75], cf. [CHA 76, SCH 98, SCH 01]).

The rejection of monist criteria is obvious and the authors look for new frameworks of reference, more flexible and compatible with a certain historicity. For the constructive empiricism (anti-realist) of van Fraassen [FRA 80], science builds models which "save the phenomena" and the acceptance of a theory implies only the belief in its empirical adequacy and not in its truth. Van Fraassen showed that, unlike truth, empirical adequacy has to be decided at the level of models. A theory is empirically adequate when substructures of a model of this theory are isomorphic to some systems of empirical relations that phenomenologically appear to—or through—our instruments. This property cannot be syntactically isolated. In this context, van Fraassen stressed the fact that such semantic facts as models ("relation of the expression to the world") can be seen as abstractions of pragmatics ("relation of the language to its users") from usage. Therefore, for this semantic-modellistic conception of theories, theory acceptance must have a pragmatic dimension. From similar considerations on models (empirical adequacy, mutual inconsistency, resistance to theory shift), and others concerning the paradoxical role of approximations of theories, Nancy Cartwright [CAR 83] concludes that theories are not realistic representations and that only

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[5] In social science, this dependence of the facts on cognitive structures as well as on background theories is now recognized by many authors [BER 01]. In economics, for example, the instrumentalization of the collection of statistical data [FOU 80], the inadequacy of the concepts both theoretical and operational, the discussions on the stylized facts or the definition of the economic magnitudes [BLA 80] come to be added to other problems of accuracy of economic observations [MOR 50]. In history, Revel in [BER 01] considers that beyond the historical facts, this "production" also appears in the choice of the temporal and space scales of observation. The reader will find in [HAC 99] a critical discussion of the concept of "social construction" and many examples.

models can have some realistic role: they serve to point to some local truth in some phenomenological laws. At the beginning of the 1980s, a quite general return to pragmatism occurred. Putnam [PUT 81] thus adopted a neo-Peircian point of view, in order to counter the "strong" version of relativism while he admitted the plurality and the historicity of the points of view. This allows him to preserve the realism of common sense, while rejecting epistemic realism (Z3—described as "metaphysical" or "external"). According to Putnam, to support (73) leads to a paradox: to explain the world of the common sense, epistemic realistic scientists consider only as "real" the objects of theories (like atoms), which amounts to denying the reality of the objects of common sense, like tables and chairs, which would exist only in our mind. His "internal" realism consists in relating the meaning of a concept to a contextual interpretation. According to the externalist (or epistemic realistic) point of view, there is only one true description of the world, in accordance with (Z3). According to the "internalist" point of view, an ontological question makes sense only within a particular system of belief.

One finds again the ideas introduced by Quine (following Carnap): the conceptual schema as well as the relativity of ontology; but they are subordinated here to a principle of internal consistency. The "truth" is relative and depends thus on the internal coherence of beliefs as well as experiments—such as they are represented in a conceptual system of reference (like tables and chairs which "exist" in the framework of reference of common sense). From an internal point of view (within a conceptual framework) the property to exist thus does not refer to a reality "in itself", but to a conceptual framework in which it makes sense in a particular way. The "facts" become dependent on the particular conceptual framework of reference, which leads to reconsider the dichotomy facts/values [PUT 02] as well as the myth of the impartial spectator's epistemological point of view (the "point of view of God").

1.5. A Theory as an Algorithmic Construction in a PostKantian Standpoint

The program of logical positivism regarded as well established the objectivity of the facts drawn from experiment as well as the consistency and the completeness of logical formalisms. We saw that we cannot be fully ensured of the objectivity of facts. The second point also appeared to be problematic. At the beginning of the 20th Century, the program of Hilbert aimed at founding mathematics on logic, within the framework of a formal meta-mathematics system. He expected to prove the non-contradiction of this system and thus provided an indubitable framework of reasoning with its related proofs. Later, Godel showed that in any formal system containing arithmetic, there are always true but un-decidable proposals (neither provable nor refutable within the system), which introduces an intrinsic limit for any formalism[6]. One cannot prove the consistency of such a formal system in a purely syntactic way within this system. It is thus an un-decidable property itself of this system. Then, it is necessary to resort to external methods: the truth cannot be reduced to the provability, nor the semantics to the syntax. For Zwirn [ZWI 00], these limits show that "the ideal of an absolute certainty and a total isomorphism between the theories and the reality in itself must be abandoned" (pp. 260). Nevertheless, that does not prevent from considering that one can have a "high degree of belief" in the methods and the results of the scientific approach within a framework that we now have to specify.

The tripartite design of Zwirn falls under a post-Kantian view and benefits from the results established from a consideration of quantum physics, on calculability and on cognitive sciences, while adopting a structural realism inherited from Poincaré. Zwirn distinguishes what is representable, what is only conceptualisable and what human beings human beings

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[6] See [LAD 57, GIR 89, DEL 94, ZWI 00]

cannot know. Recognizable empirical reality (phenomena) is thus broken up into two layers. On the first layer, phenomenal reality is what is perceptible and representable in empirical reality, thanks to our cognitive capacities. It is what one usually calls "empirical reality". But Zwirn holds this term for a naturalistic (potential) capacity to know, rather than what is actually perceived and represented. As in Quine and Putnam, cognitive and social conceptual frameworks filter phenomena: language, culture and education, in relation to the physical limits of our senses. One can have a monist or pluralist vision of this phenomenal reality. In this latter case, as with the thesis on the inscrutability of the reference in Quine, it is impossible to be fully aware of this diversity: in this case, intersubjectivity is an illusion which we have no means of dissipating. At the intermediate layer, the empirical reality of Zwirn is specific and corresponds for him to the "knowable unknown". Its perimeter is a priori given in a naturalistic manner by what is recognizable objectively according to the perceptive and cognitive human capacities. "In this sense it does not exist independently of the human being". But if the perimeter of this layer is well determined by objective anthropological characteristics of mankind, it is to some extent without being known by man himself. This level is not recognizable as such but determines for Zwirn all that is conceptualisable, while not being necessarily representable. It thus makes it possible to integrate the unobservable levels. Zwirn "refuses to consider that the conceptualisable exhausts all the possible" This led to define a layer corresponding to what is not conceptualisable by the human being, "which one cannot speak about" and is thus unknowable for mankind. To restrict the perimeter of empirical reality to what is conceptualisable and to define what is not in a negative way avoids having to refer to Kant with a reality which cannot be reached. Can the knowable be reduced to the conceptualisable? The extension of empirical reality beyond the conceptualisable would be relevant from the experimental point of view in simulation which will be developed further.

Zwirn proposes a constructivist definition of the theories, considered as formal systems. If one can translate the phenomena into statements in relation with the considered formalism, a theory can be seen as an algorithm, which can generate these statements. That is nothing but a compression algorithm in the sense of information theory. One can thus distinguish in the academic activity some formalizable sciences "which are able to describe such algorithms". With this computational definition of the theories (also proposed by [PAR 01]), the questions of the under-determination of the theories by the experiment as well as the interpretation of the theoretical entities find an elegant explanation: "the algorithms are not simple formulas but invoke entities which are used as intermediary of calculation". "Two algorithms generating the same results do not invoke necessarily the same intermediaries of calculation" [ZWI 00, p. 352]. The mathematicians' known as "quasi-empirists" [PEC 98] based part of their arguments on these considerations related to the problem of calculability (algorithmic theory of information and algorithmic theory of the proof). This idea is widely shared today. Parrochia [PER 01] affirms thus that "faced with an unspecified reality, one must initially try to compress it, because it is the only scientific attitude, which can be adopted" (p. 172).

The construction of formal systems does not imply that the latter can be entirely checked by means of demonstration, as Zwirn underlines: "any sufficiently complex system generates consequences which cannot be proved within the available capacities". This approach does not explicitly envisage a necessary sequence of tasks from phenomenal reality towards theory (as for the positivists) or, on the contrary, from a theoretical conjecture towards an empirical test (as for the falsificationists). According to [WAL 04] knowledge can be seen as a system of belief that can be revised according to the check of empirical phenomena, while seeking to maintain overall coherence and to preserve the beliefs, which are the most deeply anchored. One will find an alternative to this "maxim of minimal mutilation" [QUT 64] with Livet's putting “*into suspension*” hierarchy [LIV 07]. The predictive success of the theories is explained by the argument of structural realism. But instead of applying this latter to the metaphysical reality, as in Poincaré, it is applied to empirical reality (intermediate layer). The failures are explained because “our theories (formally built) only apply to the suitable part of the phenomenal reality”. Finally, the particular “algorithmic” point of view of Zwirn is anti-realistic in the traditional sense, because it does not postulate the existence of a reality completely independent of the human being (Z1). It is at the same time partially realistic and partially idealistic insofar as the world is not a pure creation of the spirit, but an “empirical reality” depending on our perceptive and cognitive capacities, which can be conceived independently of each individual spirit, but cannot be by definition independent of the external world: as Putnam said [PUT 81], pp. 9, quoted by Zwirn [ZWI 00]: “the spirit and the world jointly build the spirit and the world”.

2. The Question of Modelling

Models were first introduced as dependent upon theory. It was a question of finding conditions of application for the theory, and of contriving conditions under which equations would allow for a concrete characterization of the observed phenomena, such that these could be described as the exact solutions for those equations. Thus, for example, in the case of Newton's system, one allowed the model of central forces, reducing masses to their central point, such that forces were calculated along the right angles linking up those points. In this context, it sufficed to be in possession of a model, and if the latter was well adapted, it seemed to provide an entirely natural link in the relationship between the theory and the empirical phenomenon, or even reality, depending on the theorist’s philosophical position. The model provided the condition for the relationship between calculation and measure; it was like an integral part of the consequences of the theory and did not seem to require a special epistemological treatment. The model was part of the theoretical structure. It contributed indirectly to the idea that theory could be an "image of nature”, rather than just a scientific construction.

2.1. History of Models in Epistemology

Toward the end of the 19th Century, the situation changes with the multiplicity of physical and mathematical theories. It becomes apparent that, in order to ensure the coherence of science, we need to establish whether a theory can be a model for another theory. It is at once a question of the compatibility between theories, and of the solution to the question of the consistency of the new theory. Thus, for instance, Poincaré showed that Euclidean geometry provided a model for nonEuclidean geometries. This allowed one to construct a “dictionary” for translating the terms and relations from one geometry into another. In another register, that of physics, Maxwell devoted much time to constructing concrete models in order to know whether there were mechanical models for electromagnetic phenomena. During their lecture courses on thermodynamics, Planck and Poincaré considered whether there could be mechanical models for thermodynamic phenomena. All these problems differed in nature from Newton's problem, for it was no longer a matter of obtaining a model of application. In the same lecture course, Poincaré showed that if one possessed a mechanical model for a physical theory, then one possessed infinity of others. Thus, the question is not that of knowing which particular model is relevant, but simply of knowing whether there is a model. This alone allows one to say whether there can be possible indirect translations from one theory into another, which is what guarantees a common structure, if one adopts the viewpoint of the theory, or, if one is engaged in interdisciplinary conception or modelling, an indirect form of compatibility. But to know whether it is this or that model which is ‘true’ in reality is a metaphysical question. This means that the scientist works with sets of models, and that, where the latter type of problem is concerned, the idea of an isolated model is meaningless. This formulation, which I am proposing here, provides a key for understanding the multiple meanings of the term ‘modelling’. The latter can be understood as the action of constructing a model, which is not banal, because each decision often opens up new choices. Alternatively, modelling can be understood entirely differently. Consider a model. There is nothing to prevent us considering each element of the model as itself a model, just as one may consider each term in a series as another series, as Bertrand Russell [RUS 01] once did. Modelling is then understood as an activity of connection, or as the unification of a set of models, which takes on its full meaning with conception and pluri-formalizations.”[7]

A new stage in the question of models is linked to the development of fluid mechanics. Bouasse had already pointed out that the relations between theory and observation in the latter were not the same as in other disciplines. He exhibited some suspicion towards those solutions, which presented themselves as purely mathematical—it is important for him that a place be kept for observation. This is because fluid mechanics presents a particular problem relative to other ‘mechanical’ theories. When a flow is ‘slow’ it can be described as the solution to equations, but when it is ‘fast’ there are variations of speed in the flow, known as turbulences, which render any such description ‘intractable’. This example bears some relevance for our argument here, because MAS’ allow one to simulate the flow of water in a siphon, whereas differential equations do not. This requires theoretical additions, specifically multidimensional analysis, in order to ensure that both the reduced models and those of scale 1 possess the same properties, as well as the construction of models which allow one to test these reduced models under certain constraints, something which is carried out specifically in wind tunnels. In a case like this, one constructs a model because the theory is not ‘complete’ relative to the set of empirical phenomena. One does not have a model because one has a theory, as in the previous cases, but because the theory does not suffice to deal with what is presented under certain specific conditions. This is an important stage because it shows that one can construct models even where theory is absent.

In conformity with such examples, many models have been constructed in the absence of theory, or at least with parts of theory. The greater the distance from the epistemological model of mechanics and physics, the more one encounters this schema. Models are manufactured on the basis of empirical observation, which is then related to concepts. This type of model has been understood as an ‘abstraction’, from reality or from the empirical.

A similar argument has been proposed for economics models. For instance, Solow [SOL 97] distinguishes in economics between pure “formalist” activity and “model building” activity, that have for objective to isolate and analyse specific dependencies taking from empirical phenomenon (or empirical reality in the sense of Zwirn [ZWI 00]): “the idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact” (p. 43). In that sense, model building can be viewed as a quasi-experimental activity or “economists laboratories” for Maki ([MAK 92, MAK 02], see [DUB 2007] for more development). 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 explain power of some causal mechanism taking in isolation, by postulating some “constraints on the

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[7] To use both terms of “modelling” and “model” makes it possible to identify level distinctions and to subsume under the same term the different (and developing) practices of models. We therefore take liberties with the historical meanings of the term “model”. Modelling, as understood in such a way, is not yet simulation, which will be clarified in the following paragraph.

operations” that can be applied in this domain (following the terminology of Livet [LIV 07]). According to Livet, this does not presuppose that we have previously an adequate theory (or more radically a theory) of the corresponding domain. In this case, the model is not an interpretation of a pre-existing theory, but a way to experiment in the model's virtual world the explaining power of some empirically selected assumptions. More plausible the explanation will be, on the basis of sufficiently parsimonious assumptions, more the quasi experiment modelling process will be fruitful. “A good model makes the right strategic simplifications, In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows” [SOL 97, pp. 46.]. Starting from the concrete, such an abstract simplifying model is however “false by construction”. The key question is then about the relevance of this abstract and fictitious economic model world for the explanation of related empirical phenomena in the “real world” (see [DUB 2007] § 2.4.1).

From the 1950s onward, there has been a huge proliferation of these models, whether in the experimental sciences or the human sciences. Ever since this era, there has been a tremendous variety in the type and the nature of models. But epistemology implicitly classified them according to two basic schemas: on the one hand, models which, close to logic and mathematics, appear to provide ‘true interpretations’ of theories, or a ‘concretization’ of the theory so to speak; on the other hand, models which, closer to the experimental and human sciences, seem to represent an abstraction from the concrete, a way of simplifying reality by abstracting its salient traits. Both types of models seemed to be opposed, the former were a concretization; the latter an abstraction.

In Britain and the United States, this opposition was simply a consequence of the opposition between theory and experience. In works by Carl G. Hempel [HEM 66], or Ernest Nagel [NAG 61], we find these oppositions exemplified with regard to a set of general hypotheses derived from the Vienna Circle. Hempel attenuates the latter by acknowledging non-immediately observable or ‘dispositional’ terms possessing an implicative form, alongside the ‘observational’ terms, which yield ‘protocol statements’. Although it represents a more ‘liberalized’ version of a thesis of empiricism, it continues to operate within the bounds of the opposition between theory and experience. As for Nagel, he seeks a realism which would allow one to limit the role played by conventions, and to that end he insists upon the difference between experimental law and theoretical law. Yet in spite of this, this opposition has not become rigid in Britain and the United States, no doubt thanks to attentiveness toward the empirical. In the United States, think-tanks produced models whose relationship to theory was already slightly different because they set themselves objectives that were independent of theories, and because these objectives required the help of several disciplines. This has been described in works by: Peter Galison [GAL 97], Amy Dahan-Dalmedico, and Dominique Pestre [DAH 04]. The practice of science differed from that on the European continent. In France, this opposition has played a paradigmatic role because for a certain period, where the question of models was concerned, science as understood by scientists and science as understood by philosophers partially coincided, insofar as the starting point for this understanding was much more theoretical than empirical. This opposition functioned as an epistemological obstacle, which impacted upon both the experimental and the human sciences. It was systematized to such an extent that certain philosophers would admit ‘logical’ but not empirical models. At the beginning of the 1970s, Marxist philosophers, such as Alain Badiou [BAD 69] and Louis Althusser [ALT 74], underlined their opposition to the use of non-mathematical and non-logical models in the case of the former, and to inter-disciplinarity in the case of the latter. What was paramount was theory and structures. The mathematical school known as ‘Nicolas Bourbaki’, which at that time exerted a widespread influence in academic institutions, provided an indirect sanction for this interpretation of the primacy of structures over models. So much so that non-mathematical models were sometimes treated as subterfuges designed to lend an aura of scientificity to what was characterized as merely ideological. Badiou [BAD 69], for example, was very harsh towards the anthropologist Claude Lévi-Strauss, whereas the latter showed epistemological subtlety in affirming that kinship structures did not apply directly to empirical reality, but rather to the models constructed on its basis. This could be understood in the context of an epistemology principally influenced by the notion of theory.

But even if the concept of theory remains a fundamental one, it is possible to propose an alternative conception of the relationship between model and theory; something that was difficult in the early 1970s. It is one thing to criticize models, given that the latter are plural and must be adapted, and given that it is necessary to ensure the relevance of their parameters, etc.; but it is another thing to let this critique lead to a structure which opposes 'good' models to 'bad' models. All models can be co-opted for ideological purposes, but the fault does not lie with them but rather with the political uses to which they are put. It is this that has made it difficult to develop models in biology, where there is no unified theory. The French solution to this problem consists in treating a model as something distinct from a representation. It was first formulated by the biologist Jean-Marie Legay [LEG 71, LEG 97, LEG 04]. It has also been defended in geophysics by Jean Goguel [GOG 68], in an entirely different context, in one of the articles on 'Model' in the Encyclopedia Universalis. This thesis allowed one to destroy the opposition between these two types of model. It is necessary to recognize the existence of varieties and multiplicities of model, and the category of representation played a part in the opposition which functioned as an 'epistemological obstacle'. Legay saw models as tools rather than as representations. This conception of model is one of the first attempts to alter the relationship between the concepts of theory and those of model.

It is now necessary to acknowledge that models come in very different types and have very different functions. But this cannot be acknowledged in principle if one begins by seeking to classify models according to categories that are not only scientific but also metaphysical, such as the notions of 'theory' and of 'reality' can be. In order to overcome this situation, models have been presented as intermediaries, neither theory nor reality but in-between [MOR 99]. As conditions of application, models are intermediaries between theory and empirical phenomena; and as intermediaries, they can have heuristic and metaphorical functions, to the extent that these functions are seen as continuous with theory. In this perspective, models can be seen as very useful extensions, but ones which it is nevertheless necessary to confine to the context of the validity of the theory, without which they can become 'dangerous' [BLA 62]. Accordingly, many epistemologists have posited limits according to their conception of theory, whether the latter is construed as an image of nature, or as a free construction allowing for a scientific account of nature, or some intermediary between these two extremes. The situation becomes more complicated when one considers that some epistemologists already see theory as a metaphor, such as Poincaré. Finally, models have been understood as mediators [MOR 99], which allow them to be determined through a function, orienting them toward a relative autonomy. Nevertheless, they continue to be trapped in this in-between. They have not yet been ascribed a status as relatively autonomous [SCH 98].

Economists have also used models as a means of dialogue and of common culture for the sharing of concepts. According to Bernard Walliser, models then become ideal and abstract. According to an explicitly or implicitly conventionalist perspective, such as Mäki's, this also pertains to their intermediary or 'mediating' function. A 'pragmatist' or 'dialogical' interpretation of models becomes possible within this particular framework.

Thus, to take models seriously means to be able to account for the sciences without reiterating oppositions such as that between 'theory' and 'experience'. This is difficult, for the latter keep re-emerging, since they come from philosophy's projections onto science, and hence from a spontaneous feature of thought. Science can be described by taking its components into consideration without organizing them in terms of this opposition. There is theory, there is experiment, there is measurement, there is observation, there are models, there is simulation, etc. To claim that experiment is the continuation of theory by other means is often correct [FRA 89], but to claim it as a generality is to reiterate, albeit in an attenuated form it is true, the opposition between theory and experience. To claim that models are mediators is an affirmation of the same kind. Such a formulation is interesting and is often true. But it continues the same philosophical procedure, which consists in extending as far as possible the distinctions of epistemology in conformity with the conception of the latter that has been elaborated around theories. These distinctions proper to theory-centric epistemology lose none of their value through modelling, but it is necessary to re-adapt them to the relatively new situation concomitant with the explosion in the number and variety of models. Carnap, Popper, Kuhn, and Feyerabend, all of whom were principally concerned with theory, despite many nuances, cannot be directly adapted to the contemporary situations of modelling. Each of the elements of science or of scientific practice can be conceived of as the equivalent of a "'parameter', such that their connection gives rise to combinations which are specific in each particular case depending on the contexts. This idea may seem forced, but it is beginning to prove useful in several domains. In contemporary musical composition, the elements that enter into the composition are each treated as parameters. In philosophy, where modelling is beginning to become a preoccupation in the attempt to attain a theoretical understanding of philosophy and of philosophical multiplicities, the idea of parameter can also play a role in philosophical fiction or philo-fiction [LAR 06].

Modelling itself teaches us to treat the relations between the components of science in a new way. This meta-theoretical idea is important in the very construction of models. One may connect the results of an observation or an experiment using fragments of theory, definition, mathematical formulations, conventions, general laws, measures, etc. This is a way of seeing scientific practice under the aegis of modelling, rather than under the aegis of a structure wherein all those notions that are taken to be 'secondary' are subordinated to the hierarchical dyad 'theory/experience'. In order to understand contemporary science, we have to formulate minimalist hypotheses, but ones, which allow us to account for the complexity of models and of modelling. These considerations are essential in order to identify the place of modelling within the human sciences, which we have again seen recently contested in the context of geography. If we can find a manner of treating models freely, and not as directly related to theory, or to experiments, then it will become possible to construct rich indirect relations, which will allow us to invent models in complex situations. Thus, for example, theory can assume the role of guarantor for the compatibility of models when these have different origins. It becomes fuller as a result of the idea that it can assume various functions with regard to models, depending on the case.

Models have did not play an important role in epistemology prior to the 1950s. Some of the research from that era is cited in the bibliography. It was thought that the model should disappear once the theory had been enriched by it, unless the theory itself was considered as a set of models. From the latter point of view, the notion of model has been allotted a role similar to that of hypothesis, which was also supposed to disappear once the process of proof or validation was complete. All these notions regain their importance once one allows the elements of science to be treated otherwise than as subordinated to the dyad 'theory/experience'.

The difficulty in interpreting models during the 20th Century has been complicated by the fact that the concepts of the description pertained not only to the realm of logic, but also to that of the philosophy of language. The relevance of this cultural fact for the philosophies of the sciences in general has already been emphasized by Gilbert Hottois [HOT 04]. Those models directly connected to the axiomatization of theory were described as 'syntactic'; those presenting themselves as object domains for a formal system and allowing one to characterize logical truth [TAR 41] were described as 'semantic'; lastly, the plethora of models allowing one to describe or produce concrete phenomena were described as 'pragmatic'. This classification has the merits and demerits of distinctions that have been elaborated in another domain. The term 'semantic' is probably the most apposite since it describes an initial systematic distinction between a formal system and a model, or between language and reality. But it also invokes linguistics. The term 'pragmatic' accords with a movement that was initially philosophical, and which has been interpreted in debates in the philosophy of science since Peirce and Nagel, but also more recently in the debates between scientific realism and anti-realism, as well as in the contemporary interpretation of models [ARM 04]. Lastly, the term 'syntactic' was probably applied retroactively. This classification encourages a view of models as linguistic constructions, which was already a classical point of view. Max Black [BLA 62] approaches the question of the model from the more general problem of the question as to how language is able to inform us about the structure of reality. It is likely that computer simulations and polyformalizations shall encourage further refinements or criticisms of this thesis. The inventiveness of modelling requires that models, just like the other components of science, be considered as relatively autonomous. This autonomy supposes that the relations between the components of science or scientific practice be indirect. Models are not directly dependent upon theory, or observation, yet they may enter into many kinds of indirect relations with them without having to concede any hierarchization between pairs of notions. Theory can be the guarantor of the compatibility between models, while observation can enrich or modify the choice of parameters, and select models. But this is a process that does not proceed in linear fashion from one stage to the next, because each new stage requires new arrangements. This allows for the re-mobilization of classical distinctions and for inventiveness in the use of models such that these can be deployed in complex phenomena and the human sciences.

3. Simulations and Experiments

We decided to focus first on theories and not on models, because contemporary epistemologies of models themselves, until recently, laid stress on models seen from the point of view of theories. Nonetheless, according to the most recent conception of models, the pragmatic one (such as proposed by Morgan and Morrison, [MOR 99]), models are seen as "autonomous mediators" between theories, data and practices. But this autonomy still tends to be conceived in a rather limited manner, especially because the main domains of enquiries of pragmatic epistemologies still are physics, applied physics or mathematical economics, for instance, but not mixed engineering practices of modelling (which developed either for pragmatic or theoretical purposes) at the frontiers of the more traditional domains. Hence, seen from this more generous pragmatic point of view, models are no more conceived from the standpoint of theories. Nevertheless, they still are seen as a kind of linguistic practice. According to [VAR 06, p. 32], that is the reason why this so-called autonomy of models remains quite relative, as is relative the pragmatic dimension of speech compared to the syntactic and semantic ones. In fact, the recent expansion of new programming techniques and languages in many interdisciplinary domains (through the concepts of objects, agents or components, etc.) coupled with more fundamental considerations on the limits of computer science could offer new opportunities and new epistemological visions on models. On the one hand, there is a strong tradition in data processing, that logic can offer a general and sufficient theoretical framework to data processing [WAG 98]. Consequently, computer sciences applied to modelling, and especially computer simulation of models, have not been seen as radically new practices. They were seen to differ in degrees but not in nature. On the other hand, some authors now consider that current data processing is by nature interactive, and non-reducible to the calculation of recursively denumerable functions [GOL 05]. They thus oppose a new "interactive" vision to a more traditional "mathematical" one of the data-processing theory. This opposition is not without pointing out (without covering it) that introduced here by Axtell [AXT 07] between simulation as "complement" and simulation as "substitute" of the traditional formalisms. This section thus presents an alternative thesis which makes it possible to think of these forms of simulation as "substitutes" of the traditional formalisms. It opposes what we call mono-formalized simulations— i.e. the ones which are based on the formal homogeneity of a unique underlying model—to pluri-formalized simulations based on many heterogeneous (from a formal and an axiomatic point of view) interacting models [RAM 07]. [VAR 06] saw the more traditional practice of mono-formalized simulations as being epistemologically founded on a "rational computationalism", i.e. a type of use of computer simulations in which they serve to compute or to explore a simple formal model. On the contrary, the use of computer simulations which is founded on the integration and step-by-step aggregation of interdisciplinary heterogeneous models or objects, and the purpose of which often is to enable *silico* experiments, relies on what could be called an "applied computationalism". [VAR 03] argued that this recent and second kind of epistemic use of computer simulation obliged epistemology to go much further than did the first proponents of the "semantic turn" in the 1970s [FRA 80] in the rejection of a linguistic reductionism first introduced due to the epistemology of logical empiricism. Even when models are no longer seen as propositions capable of being true or false, verifiable, refutable and so on, even when models only serve to denote in ordinary language nothing more than an "empirical adequacy" [FRA 80, p. 47], this semantic conception still relies on the assumption that knowledge embedded in models and simulations is always propositional in nature. Our claim here is that pluri-formalized simulations ask again for much less language centred epistemologies. Today, there is evidence that the nature of knowledge embedded in these pluri-formalizations is uniformally reducible neither to a representation (picturesque, iconic, symbolic, etc.) nor to a model, nor, more generally, to any anthropomorphic propositional object nor speech construct or "language play". This is probably the crucial reason why this kind of simulation can sometimes be used and seen as extended experiments of a novel kind but no longer only as delegated "thought experiments". Therefore, the terms of the question of "validation" tend to change, as what is compared here to representations (collective, individual, etc.), discourses, theories is not necessarily itself of a direct representational nor of a direct propositional or linguistic nature. It seems to us fundamental to be aware of such a discontinuity, not to favour it, but to give the opportunity to correctly evaluate the now difficult, various and not only pragmatic dimensions of the huge problem of validation of complex simulation systems.

3.1. Ulam and the Numerical Simulation

Many authors have drawn attention to the quasi-empirical epistemic role of computer simulations [WAG 85, HUM 90, LAS 96, WIN 03] (see [VAR 01] for a partial review). What is astonishing here is that this ability of computer simulations to go beyond traditional formalism and to enable some of the first kinds of pluri-formization, and then of virtual 'experiment', was already consciously attributed by one of the pioneers in the domain. In fact, according to one of the founders of the simulation techniques on digital computers, Stanislaw Ulam, the Monte-Carlo method enables the "physical 'production' of models of combinatorial situation" [ULA 51]. This technique could be also defined as the approximate resolution of a continuous model thanks to a sampling process based on a generator of pseudo-random numbers through a digital computer, i.e. as a mathematical practice. But according to Ulam, it nevertheless could be described as a real kind of "experiment" because it tends to replicate a part of the modellized system from a rather non-abstractive point of view. Although von Neumann was impressed by the ability of the Monte to rapidly solve huge mathematical problems, Ulam was more impressed by its ability to treat all mathematics as an empirical stuff. Contrary to what is often said about his vision, Ulam did not attribute this ability of simulation to the stochastic treatment. Having worked on ergodicity for years, he knew the common distinction between stochastic and determinist processes to be very relative [MET 49, ULA 51, p. 266]. Ulam emphasized this empirical ability because he saw computer simulations as fundamentally based on discretization and, hence, more generally on what he sees as a "physicalization" of mathematics. Computer simulation tends to discretize and then sub-symbolize the symbols—where a symbol is defined here (partly following Peirce) as a sign whose behaviour is controlled through a conventional rule—used in the traditional mathematical analysis so as to make these sub-symbolizations interact in a more step-by-step and then intricate manner than ever possible in the rigid analytical models. These sub-symbolization processes explain why computer simulation tends to be less abstractive and more physicalized, although this physicalization does not necessarily give birth to a "replication" in the naive realistic sense. The important point here is that, as a counterpart of their step-by-step computing, some computer treatments of symbols tend to be much more based on their instantiation i.e. on their "materiality" in this sense (e.g. a neutron is re-"physicalized" through a memory address) than on the conventional rules which otherwise let these symbols automatically work on a formal level, as is usual in mathematics since the algebraic revolution [SER 05]. Ulam was one of the first to understand that, in the forthcoming computer simulations, sub-models, entities, laws or processes would increasingly give birth to sub-symbolized formalisms and that they would be no longer restricted to any algebraic or mono-symbolistic treatment. In those times, Ulam tried to explain that this movement of desymbolization of mathematics was not to be understood as a naive come back to the old positions of scientific realism, as many already misinterpreted it. Few of his colleagues followed him to this point, on this new, hence narrow epistemological path.

But many years later, it finally seems rather commonsensical to see computer simulation not only as a numerical technique of computation but, more fundamentally, as an emancipation from standard formalization techniques traditionally used in empirical sciences (and of course not only in mathematics themselves as Ulam saw it first in the 1940s, before extending this view to empirical sciences other than physics in the 1960s, especially through the modelling of morphogenesis).

So let us come back first to this extensionalist, discretized and sub-symbolized method and, second, let us try to explain why it would make no sense to reduce prima facie computer simulation—at a general level —to a calculus or to any "formal compression" procedure whether algorithmic or not. Here, the very source of the empirical status of pluri-formalized (and now often subsequently interdisciplinary) simulations is at stake.

3.2 Sub-symbolization and Thought Experiments

In its early period, the Monte Carlo method essentially served to solve analytic and deterministic problems in nuclear physics. Because it was founded on the use of pseudo-random numbers, it first could be presented as a heuristic method of resolution of mathematical models. But rapidly, as Peter Galison showed, Ulam generalized this heuristic point of view to any mathematical procedures. Ulam proved this point through transition via the usual logical formulations of meta-mathematics (predicate calculus). In fact, he showed that any logical formula, when not yet otherwise decided or showed decidable, could nevertheless be "empirically" explored through a technique of random computerized tests, which he directly applied, to the extensions of their classes. Through this direct treatment of some individuals of classes instead of the classes themselves, and the subsequent measure on these singular results, we now can recognize a transposition in meta-mathematics of what we propose to call a sub-symbolization process. It is important to note that this vision of mathematics and heuristics went, and still goes, against the venerable tradition of the ars combinatoria, i.e. against the huge movement of algebrization and symbolization which took place in modern mathematics and empirical sciences [SER 05].

Moreover, due to its large desymbolizing way of proceeding, computer simulation cannot always be seen as a kind of "thought experiment" which you simply delegate to the computer. Unless you re-elaborate the notion of "thought experiment" and give it a radically other meaning than the one imposed by the very influent modellistic epistemologies of Mach, Boltzmann and Wittgenstein, you'll hardly find the way to emancipate yourself from this current conception: "thought experiment" as an experiment on a working inner image or a linguistic game or, at least, a symbolistic game (with rules on signs and which rules you 'internalised' or more or less consciously 'embedded') which in fact plays the real role of a model. But this could be at most an instantiation of a model not an experiment with a simulation. The difference stems here from the fact that simulation not only discretizes the rules but it discretizes the following of various heterogeneous rules too, and it intricates step-by-step this following, not only the rules. Therefore the process of sub-symbolization, seen at its most general level, is not in principle (and not only in fact) embeddable in a symbolic form as is a model. Simulation cannot always can be reduced to the parsimoniously regulated computation of a model (the one possible in thought if we had enough time or memory, etc.) as it is not entirely working on symbols always taken as such. Moreover, during the simulation and depending on the step, the same symbols are either treated as genuine symbols or partly as replica. The same symbol can alternatively be a factor of a simulation (treated through a desymbolized point of view) or a factor of calculation (operation). Hence simulation is generally not a kind of thought experiment, even based on variants of language plays, even if we take into account the inner heterogeneity of the elements of a language game. According to Wittgenstein, for instance, the heterogeneity of elements in a "language game" does not entail the deconstruction of rules nor any destructured following of rules. Otherwise, no game would be possible. Heterogeneity of elements (as recognized by Wittgenstein) is not the same as heterogeneity of rules and applications of the rules during the game. The latter leads to local constructivity (the simulation) but could lead to global incompatibility (i.e. if we don't take into account the step-by-step changes of standpoints i.e. of rules applied on symbols-as-type or on symbols-as-token during the simulation). This fact tends to be dramatic as it makes rather problematic the universality of the metaphors of "game", "rule" or even "algorithm" as far as an epistemology of simulation is concerned.

It is noteworthy that Ulam explicitly describes this treatment of formalisms—or, which is better, this formalized distributed treatment—as a fundamental alternative to the established way of idealization and symbolizing in mathematics and empirical sciences. After having showed the interest of an approach via a generalized stochastic method of any dynamic system with an infinity of points interacting with each other, he claims that "actually infinite systems of this kind may be thought of, however, as a new kind of idealization of systems already considered in present theories", [ULA 50] (pp. 272). According to him, it is always possible to replace a physical phenomenon by this kind of distributed formalism based on a maximum number of elements and a maximum number of degrees of freedom. On the contrary, the traditional way of idealizing relies on a parsimony, for instance on selections of elements and/or parameters which are supposed to be average. Apart from metaphysical or aesthetic reasons, the need for parsimony, i.e. the need for algebraic mathematics can be explained by the need to find some kind of automatic or “blind” (as Leibniz said) operation on these symbols. Facing this, the acceptability of the kind of extensional idealization proposed by Ulam is due to the fact that the Monte Carlo method, when applied to any system, can be showed equivalent to the iteration of finite rank matrices on an infinite space of discrete elements of this system. As we know, this standpoint led Ulam and von Neumann directly to the technique of formalization lately called “cellular automata” by Arthur Burks. Thanks to computers and to their power of recursivity, the responsibility for the automaticity of operations has been delegated again and moved back from the symbolic aspects of models to their more iconic ones.

3.3 A Presentation Without Concept

As a consequence, such an extensional idealization cannot always be reduced to a numerical filtering nor to an algorithmic compression, as these latter procedures still are some kind of mathematical modelling. For instance, today, it is sometimes conceded that computer simulations are not models but remain instantiations of models [SIM 06]. Yet a computer simulation, as Ulam conceived it at first glance, was directly seen from a much broader point of view. On the contrary, every simulation, which is so much, constrained that it first assumes some model (to instantiate) implicitly assumes, at the same time, some kind of classical idealization with the help of a symbolic language. Whereas the radically distributed approach of Ulam devalues this stage of classical idealization.

Nonetheless, it would be erroneous to think that this kind of weak and distributed idealization immediately entails some empirical use of the simulation [VAR 01]. If it exists, the empirical import of such a simulation is chiefly complex and of the second order. In particular, apart from some exceptions, the result of such a simulation generally cannot be naively compared to a perceived real object without implying some solid (but relative) theory of knowledge. For instance, a neo-kantian would admit that every percept is linked to a concept. He would therefore say that the result of a simulation gives birth to an “empty concept” and not to an intuition without concept. But, as the result of a simulation cannot be conceived in principle and not in fact (because no single scheme can serve to construct it in our intuition [VAR 03]), it makes no sense to see it as an empty concept. As the concept is here supposed to be constructed through an algorithmic but uniform procedure, no particular concept (empty or even misconceived) can be directly put in front of the complex result of a simulation. More generally, any mono-algorithmic vision of perception and/or construction of percept (this construction being infinitesimal as for Leibniz, or differentialist as for Kant, or compressist) would fail to fully conceive the richness and complexity of empirical use of computer simulations [VAR 07]. This is the reason why [VAR 03] proposed to describe some simulations based on MAS as “ecosystems of objectified formalisms”.

Compressist epistemologies presuppose the use of a single “formal system” (FS) which is founded on some kind of formal language. This FS could be given axioms and other transformation rules on such a language. It is in principle mono-axiomatized as it is based on non-contradictory rules [TAY 97, WAG 98]. The elements and the products of such a system are homogenous whereas the elements of a pluri-formalization can be diversely axiomatized. An argument of [FRA 80] (that we apply here to formal sub-models themselves and not to syntactic theories) enables us to say that a co-calculation is possible as the sub-symbolized calculus of one of the sub-models of a pluri-formalized simulation can be embedded in a substructure of another sub-model of the same simulation, although some inconsistency can exist between their two axiomatizations. Hence, the semantic point of view can be used from another level (at the level of the model and not of the theory) and deepened so as to explain precisely why "reification" in objects or agents (such as in OOM, IBM, ABM, MAS), i.e. iconic sub-symbolization of models, is necessary for some complex co-calculations of diverse modelling formalisms.

On the contrary, a compressist point of view remains at the most a first order semantic view of a model. From this standpoint, any numerical filtering or algorithmic compression entails an artificial imposition of a particular language. This practice reveals that simulation is always seen as a superficial symbolic and linguistic operation on models. Continuing in the sense of van Fraassen's anti-linguistic analysis in epistemology (although, as a first order semanticist himself, he did not apply it to simulation), it is now possible to criticize the persistence of a linguistic presupposition influencing modelling as it earlier did with theorizing (Vienna Circle).

Of, course, as we see with Peirce and Quine, the semiotic and linguistic approach to modelling is very attractive to the social scientist as his objects appear as immediately homogeneous to some major objects of these disciplines. But we would like to suggest that the facility of semiotizing (and correlative agentifying), which is appreciated in MAS, could lead us to neglect the fact that such a pluri-formalization technique can also be powerful (as already seen in MAS for biology and geography) in that they do not always impose a linguistic vision on things, representations, individuals. Hence, besides their current theoretic (heuristic...) and pragmatic roles, the diversity of the epistemic roles of MAS could be detailed and enriched.

1.4. References

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