GRANGER AND SCIENCE AS NETWORK OF MODELS*

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A descoberta do papel dos modelos no ciéncia por Granger emparelha-se à descoberta analoga feita por Mary Hesse e Wartofsky.

O papel atribuido aos modelos realça a dimenssâo linguistica da ciència, resultando num "enfraquecimento " da epistemologia racionalista de Bachelard sem cair no relativismo, Uma teoria "linguística" da metafora, enquanto contrastada com a teoria "psicologica" de Bachelard, é bàsica para o tratamento dos modelos de Granger.

Um paragrafo final discute até que ponto a teoria dos modelos "madura" de Granger implicaria numa revisdo de sua anterior metodologia economica.

The discovery of the role of models in science by Granger parallels the analogous discovery made by Mary Hesse and Wartofsky.

The role attributed to models highlights the linguistic dimension of science, resulting in a 'softening' of Bachelard's rationalistic epistemology without lapsing into relativism. A 'linguistic ' theory o] metaphor, as contrasted with Bachelard 's 'psychological' theory, is basic to Granger's account of models.

A final paragraph discusses to what extent Granger 's 'mature' theory of models would imply a revision of his early economic methodology.

1. The rediscovery of models

Models were the object of a general rediscovery by philosophers of science during the fifties. It must be said that to the term "model" a number of different meanings and functions were attached, so that the common ground of such a rediscovery could be said to amount to the statement that "models are Good Things" (Brodbeck 1968, p. 579; for a catalogue of the different meanings see Bruschi 1971, pp. 35~56). Such widespread interest in a topic that remained difficult to define was perhaps one of the symptoms of a general malaise, the premonition of the end of a period of 'normal' philosophy of science and of the coming of a revolutionary crisis. In the after-Kuhn era, "models" - with a much more well-defined meaning - were the main element of a tentative alternative view of science offered by one of the trends of post-empiricism, represented by Mary Hesse, Marx Wartofsky and others (Cremaschi 1984).

The epistemology of Granger may be thought to parallel the contributions of the 'modelist' trend of post-empiricism. Furthermore, writings

* Silvia Breda has given invaluable help by providing me with a copy of several papers by Granger that I was not able to find in libraries.

of Granger from the mid-fifties onward progressively assign to models the role of the main tool of scientific representation, and the development of his thinking, starting from an orthodox Bachelardian epistemology of *structures,* leads to an epistemology of *models:* such an epistemology views science as a special kind of linguistic game, where representation is centred on metaphor rather than on metonymy.

In this paper I shall reconstruct the various stages through which Granger has worked out his account of models, moving further and further away from his original Bachelardian rationalism. Then I shall discuss the peculiar function that Granger gives to models in the sciences of man, and I shall try to place his contribution within the framework of the main dilemmas of the French tradition in epistemology. In conclusion it will be suggested that the development of Granger's thinking coincides with several theses that have been put forward by Pragmatism, Wittgenstein, and Hermeneutics. A final note will be dedicated to *Méthodologie économique,* the first work in which a draft of the "method of models" was presented, but also the one that should be most radically revised, in the direction of a somewhat 'softer ' image of science, in the light of Granger's later positions.

2. The definition or model

Granger's philosophy of science is an overall view of science as model building. The full implications of this view, as well as the problems that it may imply, will become apparent from a reconstruction of Granger's understanding of the concepts of model, structure, energetic versus cybernetic model, and finally qualitative versus quantitative model.

The concept of model appears for the first time in the first systematic work by Granger, which is the *Méthodologie économique.* Its meaning derives by way of explication and generalization from the use of the term by econometricians; no general definition is provided but nonetheless it is presented as the specific tool of the sciences of man.

> Dans la confection des *modèles* économiques tels que les établissent actuellement les économètres, l'idée directrice n'est plus celle de détermination exhaustive d'un ensemble de variables par un système clos de liaisons algébriques... La construction d'un modèle est donc essentiellement une tentative d'élaboration "locale" de l'ensemble des données de l'économie. Elle se réalise dans un espace épistémologique non intégré, morcelé, hétérogène. (Granger 1955a, p. 302).

Further characteristics of economic models are: to have empirical connections, to result from an assembly of ‘simple mathematical relations and to aim at the prediction of phenomena (Granger 1955a, pp. 303-10). In this vein, models are declared to be the specific tool of the sciences of man (Granger 1955b, pp. 84-85) with a *local* and *pluralistic* character, a

tool that should serve the purpose of intervention, and that accordingly implies recognition of *strategic* variables, and that is, finally, the occasion for testing mathematical instruments (Granger 1955b, pp. 85-88). Such mathematical instruments - as later writings by Granger will stress more and more - may require the production of a new mathematics for the sciences of man, a mathematics of the *qualitative* (Granger 1947, p. 299; Granger 1955b, pp. 86-87; Granger 1955a, p. 63, p. 164; Granger 1960, p. 109; Granger 1981).

A definition of model is offered in the essay on Condorcet, whose work is presented as a forerunner of the "method of models". A model is

> un montage de *relations mathématiques simples*, conservant de nettes attaches empiriques, et visant à la description explicative *locale* d'un phénomène, en vue de la *prévision* et de l'*intervention* (Granger 1956, p.99).

At this stage Granger considers the "method of models" to be the mark of the sciences of man, and he contrasts the *model* with the *system*, as a partial and local approach to phenomena with a global and exhaustive approach (Granger 1955a, pp. 301-2). A few years later the definition of model becomes more general and models are granted the role of tools for both the sciences of man and the sciences of nature: scientific knowledge is characterized by its subject-matter, by its method, and finally by its *project.* The "project " of scientific knowledge is building models of phenomena. Model building amounts to the

> recherche d'une description explicative des phénomènes par le moyen d'ensembles d'éléments dont sont précisés les rapports de codétermination dans le système (Granger 1959, p. 100).

Models should be understood in a "basically abstract" sense. Granger is careful to stress that his suggestion that scientific knowledge is essential1y modelling does not imply alignment with Campbell and other eighteenth century advocates of "mechanical models". It is better to avoid a "concrete interpretation" that would carry with it "uncontrollable intuitive aspects". It is worth noting that, at the time when he is introducing the term model as a general feature of scientific knowledge - wich represents, as will be discussed later in more detail, a substantial innovation in the terminology that Granger had inherited from Bachelard - he feels the danger of lapsing into the theoretical camp, namely the camp of Campbell and his followers, which Bachelard most vehemently opposed. To what extent such opposition between abstract and concrete model is well-founded, will be one of the central points to be discussed later.

In *Pensée formelle* Granger introduces a primary stratification of models which contrasts *energetic* with *cybernetic* models: the former are

the "homogeneous" models, while the latter are the ones in which the phenomena are organized on several levels (Granger 1960, pp. 146-160 [English transl. pp. 118-120]). Models are the product of an objectification of the human phenomena by which the subjective meaning *(significations)* with which these phenomena are endowed are reduced to objectified meaning *(sens).* Granger stresses that the task of "interpreting and explaining" sue h subjective meanings is not the job of science.

The explanation and the interpretation of these meanings does not depend on science, but constitutes, on the one hand, the practice of life, and, on the other the task of the philosopher. The scientist, who aims at constructing *models* of phenomena, thus cannot confuse this order of meaning with the abstract scheme that he claims to establish (Granger 1960: 131 [106]).

Some additions to the definition of model were made later in connection with the introduction of the term "structural model" (Granger 1965, p. 390). This term is meant to indicate the "functional" model (that is, what was previously the scientific model *tout court),* as opposed to what Granger calls at this stage "semantic model", that is, the interpretation / reduction of subjective meanings (Granger 1967, p. 773). Granger will later abandon the use of the term "semantic model" as being unsatisfactory, and he will call the preliminary interpretations/reductions of subjective meanings "hermeneutical models" or "significant systems" or "quasi-models" (Granger 1969, p. 402), including them in the wider class of "abstract models" along with those that are now called "formal models" (Granger 1969, p. 401).

To establish an abstract model of some phenomena (that is, both a hermeneutical and a formal model) implies constituting them as "objects", or a "reduction" of the phenomena (Granger 1965, p. 390) which are already the result of a first reduction of the lived experience (Granger 1965, pp. 391-392). Such reduction does not amount to reaching the *essence* of things (Granger 1959, p. 101; Granger 1968 pp. 298-9; Granger 1968, p. 41), but it is rather the production / recognition of several *structures* that need not be misunderstood as things in themselves (Granger 1957, pp. 34-41). Such "multiplication of irreducible schemes", that is, a plurality of structures, structural models and, as a consequence, of objects, in association with one given phenomenon, is a consequence of the original *polysemic* character of the given:

> C'est une propriété... dc notre rapport objectif avec le monde, que cette "polysémie " des choses, Chaque science prise séparément met entre parenthèses cette modalité de l'existence concrète; mais à l'intérieur même dc l'individuel se présente encore comme recoupement de divers codages (Granger 1965a, pp. 394-5; see also Granger 1955b, p. 86; Granger 1976,p.155, p.161).

The qualifications that have been added are intended to clarify a

sensitive point: the need not to reify structures in order not to mistake them for noumena (in the Kantian sense) implies a pluralism of models, and such pluralism highlights a peculiar relationship between the inexhaustible significations embedded in experience and the meaning objectified by the model. The suggest on can be anticipated that Granger is moving toward a linguistic revision of Bachelard's epistemology.

A further, more pronounced, step in the same direction is taken in a subsequent paper. Models are there included in the wider class of *symbolizations* of experience, along with *myths* (Granger 1967, p.771). While model is the "instrumental category" of science, myth is the instrumental category of the arts. Model is now defined as

> un ensemble d'éléments abstraits, organisés en une structure, et visant à représenter la systématicité - postulée - des phénomènes (Granger 1967 pp. 771-72).

While myth is defined thus:

> un ensemble d'éléments concrets, organisés en un récit, visant à présenter la signification des phénomènes (Granger 1967, p, 772),

The difference between the two models of symbolization results from a different method of linguistic representation. With the model, we face a split between what is represented and what represents it; and the relationship between the level of what represents and the level of what is represented constitutes some kind *metaphorical* relationship:

> la représentation du modèle est de type 'métaphorique ', celle du mythe de type 'métonymique '. Dans la première, *on superpose deux plans, dont l'organisation de l'un joue comme signe de celle de l'autre;* dans la seconde, on juxtapose deux fragments du vécu, dont les organisations s'évoquent (Granger 1967, p, 772, stress added).

A few points deserve comment. Firstly, Granger contrasts here model to myth in terms of Jakobson's definition of metaphor and metonymy. Re is worlds away now from Bachelard's definition of metaphor as something intrinsically related to the concrete, the sensuous, the qualitative, and eventually with the body (see Bachelard 1939). It is because of this more comprehensive understanding of metaphor, which may be considered to be a generally acquired knowledge of linguistics and philosophy of language in the decades between the works of Bachelard and the works of Granger (See Ortony 1979), that scientific theories may be included, along with other kinds of linguistic expressions, within the class of metaphors. Secondly, a dimension of relative continuity has been introduced between model and myth, or between science and art: both belong to the wider class of linguistic representations. Thirdly, philosophy as well as

science is characterized by the "dédoublement metaphorique", even if the nature of the split that it produces is different from that of "abstract models".

I shall review, finally, a fourth, more recent, definition of model (Granger 1969 pp. 400-1). Science is defined through its aim, that is, the aim of objectifying experience by building "abstract models" of phenomena. The objectivation is carried out through two stages: a first *reduction* of the lived experience to phenomena, and a second *reduction* of phenomena to objects (Granger 1968c, p. 5; Granger 1969, pp. 398-94). The concept of "abstract model" appears to have been so broadened as to be able to explicitly encompass both *formal models* and the *hermeneutic models* which were introduced in the earlier paper. The abstract model is now defined as follows:

> un ensemble abstrait de *structures,* mis en *correspondance* avec un phénomène *opératoirement* défini, et permettant des *prévisions* quant aux divers états de ce phénomène. Un modèle n 'objective jamais qu'une *partie* de l'expérience, et dépendant de l'état des techniques matérielles et mentales, il est toujours *provisoire* (Granger 1969 pp. 400-1)

Let us make a comparison with Granger's first attempts at defining models. To the three original characteristics - a model is: i) *structured;* ii) *partial;* iii) made for *prediction* - others have been added : iv) a model is *provisional,* depending on the stage of development of material and intellectual techniques; v) a model is made to *correspond* to a phenomenon *(dédoublement métaphorique);* v) the phenomenon is *operationally* defined *(découpage de l'objet).*

The most innovative characteristics are points v and vi, which stress the linguistic dimension of scientific knowledge. Strictly connected with this stress is the systematization of the distinction/interaction between formal models and hermeneutic models, which deserves further attention while discussing the role of models in the sciences of man: Granger seems to acknowledge that, in principle, in every kind of science hermeneutic models should be granted a role, even if in natural science "de tels modèles ne peuvent se présenter qu'aux débuts de l'exploration de l'objet" (Granger 1967, p. 77). Two years later he seems to limit the role of hermeneutic models to the "phénomènes de comportement, humain ou animal" and therefore he states that "le modèle physique ne peut ètre objectivé qu'en des modèles du premier type" (Granger 1969, p. 403; see also Granger 1955, p. 273). In any case, the hermeneutic model appears to be a first stage in the organization of the lived experience by which it is objectified. Such objectification is already independent from the mapping of experience unreflectively carried out by language (Granger 1969, p. 402, pp. 391-397). "Formal' objectification is a further stage, starting from the previous organization of experience provided by the model.

We are thus led to discuss the peculiar function of models in the sciences of man and the related topic of the transformation of the lived experience into objectified meaning. Before beginning this discussion, which will be carried out in the next paragraph, two further general features of the "method of models" deserve to be mentioned: the connection of models with *structures,* as contrasted with *essences,* and their character as tools of a science oriented towards practice and intervention, as contrasted with a contemplative attitude,

Models are primarily a means for singling out abstract structures from the phenomena. Even if this abstract element, the structure, is the goal to which scientific knowledge is directed, and so could have been named *noumenon* by Bachelard (Granger 1975, p. 71), it should not be identified with an *essence*, or with a thing in itself, in a Platonic or a Kantian sense. Science is not,

> un miroir ne reflétant que les formes pures du monde.... Connaitre scientifiquement, c'est établir un modèle structural de certains phénomènes... mais une telle réduction de l'expérience vécue ne nous conduit point aux essences des choses (Granger 1965, p. 389).
> Le projet de la science est la mise en évidence de structures... La science n'est aucunement ordonnée à la reconstitution d'une espèce d'être organique dissimulé sous les apparences, doué d'un dynamisme particulier et qui serait la "structure ". Les modèles... ne se substituent nullement aux choses; ils sont seulement le produit d'une élaboration et d'une articulation de notions abstraites (Granger 1959, pp. 100-1).

When the unreflective practice of scientists - in this case of economists - was in a sense Platonic, that is, presented as some kind of Platonic "lifting of the veil" of the phenomena, something which was actually a more or less explicit acknowledgement of the plurality of levels of the "economic phenomenology", it was in fact misrepresenting itself (Granger 1955, p. 247). Granger fights "realism", as Bachelard used to do, which he considers to be a kind of naive false consciousness of scientific practice: translating a model into a different model placed at a deeper level is not tantamount to discovering the essence; scientific theories are a kind of symbolic system, and they include, as every symbolic system does, "syntactical" concepts along with "semantical" ones.

> Seul un réalisme furieusement intempérant pourrait exiger qu'à ces concepts syntaxiques correspondissent aussi des aspects du phénomène ou... des moments absolus de la réalité (Granger 1971, p. 40; see also Granger 1968,p.l).

Finally, structures may be unduly ontologized, shifting from the role of the *product* of scientific knowledge to the role of an ultimate pre-existing ground far the intelligible character of reality. Such is the case of Levi-Strauss' "structuralism" from which Granger is careful to keep his distance

(Granger 1957, pp. 37-41; Granger 1960, pp. 65-66; Granger 1968a, pp. 298-9, Granger 1978, p. 139; Moravia 1977, pp. 11-20). Levi-Strauss' solution, besides carrying with it certain dogmatic philosophical interpretations of a 'Platonic' kind, is defective at the epistemological level - as I shall discuss later in more detail - as it reduces the sciences of man to the construction of logical structures devoid of any empirical content. Science can be regarded as an attempt at discovering / constructing the abstract structural element precise1y in so far as it is not a contemp1ative attitude, but rather a kind of theorizing related to practice. A contemplative attitude would be one that required a knowledge which was able to "extract " the essence and leave the individual as

> un déchet, une scorie accidentellement précipitée par l'opération d'extraction des essences (Granger 1965, p. 389).

or one which postulated, besides scientific knowledge, an immediate intuition of the individual (Granger 1965, p. 389). That is why the "individual" is a category of rational practice rather than a category of theory. The individual can nonetheless be grasped by science through a multiplication of irreducible schemes (Granger 1976, p. 167). The plurality of schemes applied to rea1ity makes sense only for a science that is a step in a process oriented towards intervention.

> Si la connaissance est pensée comme le moment abstrait d'une pratique qui I ‘enveloppe et la prolonge, le processus de multiplication des grilles révèle sa positivité: c'est une démarche progressive, préparant le moment proprement pratique où la connaissance se mue en intervention (Granger 1965, p. 395).

While the plurality of the schemes marks the difference between Granger and Descartes and Kant (Granger 1960, p. 181), it is the essential link between science and practice that makes his epistemology - even with all the Aristotelian traits carried along by the vindication of a Ptolemaic revolution (Granger 1960, p. 17; [English trans. pp. 9-10]), or of a multi-level "Aristotelian-Hegelian" phenomenology (Granger 1955a, p. 223) contrasted with a homogeneous "Cartesian" phenomenology - a non-Aristotelian epistemology (See Granger 1959, pp. 97-8; see also Granger 1976).

3. Models in the sciences of man

The "method of mode1s" seems to be, to a certain extent, peculiar to the sciences of man (Granger 1955b, pp. 84-85; Granger 1960, p. 64 ff.; Granger 1969, p. 403). The problem for which the method of models provides a solution - namely the problem of translating the *polysemy* of the given into objectified meaning - is common in principle to both the

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sciences of nature and the sciences of man, but in the sciences of nature it is assumed to represent a more reduced and simplified preliminary task (Granger 1961, p. 77, Granger 1976, p. 155; pp. 143-44, p. 149: see also Robert 1977. p. 681: and Granger 1955a. p. 273).

Granger, when compared with Bachelard, may be said to have introduced the concept of mode! as an intermediate element between the phenomenon and the abstract structures that are the goal of scientific knowledge. On the other hand, while the whole of Bachelard's epistemological writings was dedicated to the sciences of nature, Granger has concerned himself mainly with the sciences of man. It would be hardly surprising to discover a link between the two facts. Granger holds the sciences of man to have a number of peculiar traits which are such as to make their method identical with the "method of models". First of all, it is the significant character of human phenomena that makes an exhaustive objectification impossible (Granger 1960, p. 131; [English trans. p. 106]: Granger 1976, pp. 144-145; Granger 1978, pp. 139-140). The peculiar problem of the sciences of man is the need to translate *as far as possible* the subjective meanings into a system of abstract concepts. The second trait is the multiplication of the schemes that can be imposed on the phenomena. This multiplication derives from the *polysemy* of the given which, while being reduced to phenomenon and then to object, can be grasped every time on1y in a partial way. The phenomenon is grasped in the most exhaustive way *stereoscopically,* by bringing together the different schemes (Granger 1976, p. 167). The third characteristic is the *local and fragmentary* character of explanations and of the objectification of phenomena (Granger 1976, p. 146). The fourth characteristic is the necessity of recognizing the relative character of the different kinds of models through which one phenomenon may be approached (Granger 1976, pp. 148-149; Granger 1979, pp. 46 ff.); phenomena may be made to correspond to an energetic or to a cybernetic model - as will be explained later - according to the needs of the explanation.

Such peculiar characteristics of the sciences of man seem to derive from the solution that Granger envisages for their basic problem.
In his earlier writings he seems to adhere without qualification to the positivist thesis of a 'lag' in the sciences of man to which undoubtedly Bachelard adhered : apart from some branches of economics and linguistics, they are thought to be still at a pre-Galilean stage (Granger 1947, p. 299: Granger 195 5a, p. 125, p. 169, p. 177), and in later writings this thesis has not been abandoned, even if it is tempered with a thesis of the 'specificity' of the sciences of man (Granger 1960, p. 131: [English tram, p. 106}:
1976. p. 143, p. 149: see also Robert 1981, pp. 423·5). The project to bring them on a level with the sciences of nature should not, however, be approached in terms of a crude transfer of methods from the natural sciences to the sciences of man, and particularly of "quantitative" methods

based on measurement (Granger 1955b, pp. 76-81; Granger 1960, pp. 106-13; [English trans. pp. 85-91]; Granger 1981) as will be explained in the next paragraph. Bringing the sciences of man to a scientific level implies rather - as Granger seems to become more and more aware - finding a solution to what he thinks may be an irredeemable difficulty of the sciences of man that will make them swing forever between the Scylla of the lack of *rigor* and the Charybdis of the lack of *exactness* (Granger 1959, p. 103; Granger 1979, p. 54). The difficulty is the one implied by

> la transposition des concepts *essentiellement flous* de notre saisie des faits significatifs en concepts *exacts* que requiert la science. Et peut-être faudrait-il-dire que le critère épistémologique profond du fait non humain est que la réduction des concepts flous... s'y peut opérer sans difficultés dirimantes, et conduit à des systèmes cohérents d'abstractions. En d'autres termes, le caractère flou des notions directement tirées de l'expérience n'apparait pas ici comme essentiel (Granger 1976, p. 149; see also 1979, p. 46).

The first risk that every attempt to solve this problem runs, is the one of being caught in the pseudo-explanations that are always embedded in the given: the living experience a1ways carries along meanings and one is often the victim of the delusion of having explained the phenomena while actually having associated them with a "metonymic reliving".

Here the phenomena have an immediate *sense,* which means that they spontaneously take part in a universe of valued and directed actions, either in the consciousness of an individual, or in the organization and functioning of a collectivity which is given as a whole, even when the relations of this whole escape us (Granger 1960, p. 64; [English trans. p. 50]; see a1so p. 131; Granger 1976, pp. 157-62; Granger 1968, pp. 119-20;pp. 298-9; Granger 1967, pp. 778-80).

Much in the social sciences presents - according to Granger - such an example of bad philosophical hermeneutics smuggled in as science (Granger 1960, pp. 106-7; [English trans. p. 85]; Granger 1967, pp. 779-80; Granger 1976, pp. 145-6). The opposite risk however is that of emptiness, that is, of lack of any empirical content, in the name of "rigor". Such is the shortcoming of Levi-Strauss' structural anthropology, which falls short of being a real science of man as it radically *substitutes* for the phenomenon the abstract structures produced by scientific knowledge, by objectifying the phenomenon. So, the sciences of man have to find their way between two reefs.

> ... ou bien la "théorie" n'est qu'un décalque des phénomènes vécus, plus ou moins biaisé par des visées normatives, et le contact avec I 'expérience est alors apparemment réussi, mais à la faveur d'une sorte de cercle et d'une simulation de science. Ou bien la théorie est effectivement une construction conceptuelle, mais alors si radicalement *théorique,* si outra-

> geusement réductrice et simplificatrice que le chemin vers le phénomène est trop long, trop ramifié, trop ambigu pour être effectivement praticable (Granger 1979, p. 196; see also Granger 1959, p. 103; Granger 1978, p. 139 ff., p. 142).

The opposing dangers may be identified in the history of the social sciences, the first with the outcomes of the phenomenological and of the hermeneutical approach (which in Granger's earlier writings is somewhat rashly refuted: see Moravia 1977; Granger 1955a, pp. 173-5; Granger 1960, pp. 130-2 [English trans. pp. 50-54, pp. 104-5]), and the second with the 'empiricist ' reductionism of behaviourism and with 'rationalist' reductionism of Levi-Strauss' "structuralism" (Granger 1960, pp. 146-60, pp. 64 ff., [English trans. pp. 118-30; pp. 50 ff.]. Granger tries to find a third way by accurately balancing the reconstruction of subjective meanings with the organization of objectified meaning (Granger 1965b, pp. 266-70). In his latest writings, with a suggestion parallel to the proposals made by writers sympathetic to the hermeneutic approach, namely by Giddens and Bernstein, who suggest that comprehension should be intended as a preliminary, rather than as an alternative, stage to explanation (Giddens 1976; Bernstein 1976), he gives to the "hermeneutic model" the function of a preliminary step in the construction of a "formal model".

> [In the natural sciences] des renvois sémantiques originaires encore concrets et riches, mais peu distincts, viennent se compromettre, par symboles interposés, dans des engagements contre-nature, qui inexplicablement donnent lieu à des prévisions correctes... C'est alors que survient le coup de force conceptuel qui se traduit par une réforme syntaxique... [In the sciences of man, instead] on a toujours tendance à y supposer que les renvois sémantiques du langage sont définitivement établis, *a parte ante*... les notions sont manipulées surtout au moyen des symboles de la langue naturelle et l 'organisation de celle-ci masque les obscurités ou les incohérences des objets immédiatement désignés (Granger 1978, p.54).
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> Il faut justement insister sur la double objectivation de ces phénomènes qui devraient donner lieu, dans la maturité de la connaissance scientifique, à des modèles superposés de deux types... Non pas que chacun de ces modèles doivent correspondre à une réalité distincte; la réalité, c'est l'expérience globale pratique dont ledit phénomène social est la première réduction théorique (Granger 1969, pp. 402-3; see also Granger 1981; Granger 1960, p. 131 [English trans. p.l06]; Granger 1979, pp. 196-7).
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> ... la réduction cybernétique... doit nécessairement comporter une étape intermédiaire qui consiste en une première description conceptualisante, aboutissant à ce que nous avons nommé un modèle "sémantique" ... Il nous semble maintenant en mieux saisir la place, qui est, comme on le voit, celle d'une étape intermédiaire et d'une instance auxiliaire dans l'élaboration des *modèles de fonctionnement* du fait humain. Nous serons même amenés à conclure que le seul caractère propre au modèle sémantique est de ne pouvoir être établi que par extraction d'une *pluralité de systèmes formels* (Granger 1976, p. 155).

Such hermeneutic or semantic models should not be viewed as exhaustive models of human reality: they have rather an auxiliary function within the framework of a "cybernetic model". It should be mentioned also that human reality cannot be fully objectified because of its essential polysemy, and for this very reason the subjective meanings cannot he reduced to unique semantic models, but even at this preliminary level a plurality of different reductions is admissible (Granger 1978, p. 139).

The peculiar prob1em of explanation in the sciences of man is best solved by a stratification of models. At the lowest level we find the hermeneutic models, which produce a preliminary objectification (via comprehension?) of the original subjective meanings, which already implies some detachment from the unreflective organization of the given provided by language. After them, we meet the 'rea!' scientific models, which Granger names sometimes "functional" models and later, "formal" models; within the realm of formal models, we find in an intermediate place the energetic models which objectify the phenomena of the semantic model in a simpler way, reducing them to a system whose elements determine each other on a unique level. This kind of explanation is valuable for a number of purposes; for example, the marginalist models of price formation in a perfect market comply with the requirements of energetic models (Granger 1969, p. 401). At the highest level there are the cybernetic models which view human reality as being analogous to cybernetic systems, in which there is a double flow, of energy and of information. Cybernetic models are required by most of the contemporary sciences of man (Granger 1978, p. 139), whereas the first stages of the history of these sciences, and notably classical political economy, were based on energetic mode1s (Granger 1960, pp. 146-53 [English trans. pp. 118-24]; Granger 1967, pp. 773-4; Granger 1969, pp. 401-2; Granger 1976, pp. 146-57).

The "ideal explanation" for a science of man, as well as for the sciences of nature, is the displaying of the complex of interrelations that constitute these structures of the model (Granger 1978, pp. 137-8). But two "obstacles" are present in the process of building a science of man: the first is the feeling, created by the spontaneous organisation of the significations, of already 'knowing' what we experience; the second is the tendency - which is fully legitimate within the realm of philosophy - to interpret the lived meanings by putting them in relation to a whole.

> Mais la tentation est forte de faire alors passer cette interprétation pour une science, et la seule qui serait capable de connaitre des faits humains (Granger 1978, p. 138).

These two obstacles make it impossible for the sciences of man to reach the ideal of scientific explanation. Granger (writing in 1978) is not willing to

> substituer de gaieté de cœur une vision volontariste de l 'homme et de la société à la science.

It is necessary to acknowledge however

> bon gré, mal gré, que le caractère essentiellement signifiant des faits humains oppose une barrière naturelle à leur objectivation n complète (Granger 1978, p. 139).

To sum up: firstly, models - according to Granger - are a specific tool of the sciences of man because of the necessity of reduction of the polysemy of experience, because of the local and fragmentary character of our grasp of phenomena, because of the plurality of possible theoretical approaches, and finally because of the necessity of lifting the veil of the apparent self-explanatory character of social facts. It is worth noting that only this last reason may be said to be absolutely peculiar to the sciences of man, as the preceding ones are common, to a lesser degree, to the sciences of nature also.

Secondly, the use of models seems - at least in most recent writings - to face specific difficulties when applied to the sciences of man; the meanings cannot be completely objectified and therefore, for every scientific theory, there is a *redundancy.* Such redundancy can be ignored as being irrelevant in the sciences of nature, but it is always a constitutive element of the subject-matter of every science of man (Granger 1968a, pp. 288-99; pp. 134-43). Facing this position of Granger's, one might have the feeling of a paradox: on the one hand modelling is the peculiar method of the sciences of man, but on the other it is precisely to the extent to which they can be reduced to modelling, that the *sciences humaines* are able to reach the status of *sciences* of man, as modelling is the mark of science *qua talis.* Finally, it is precisely in the sciences of man that the "method of models" cannot be applied fully in order to reach a total objectification of "human facts".

4. Qualitative models

Granger's para-positivism has always firmly opposed a 'positivist' assimilation of the method of the sciences of man to the method of the sciences of nature. Even in his ear1y writings, he was aware of the ne ed for special mathematical instruments for the sciences of man. Such instruments should include a "mathematics of the aleatory" and of the "qualitative"

> ... la pensée économique... découvre que la mathématique qui lui convient, en grande partie reste à faire... D'une part, l'importance primordiale de l'aléatoire dans les phénomènes humains invite à développer de plus en plus les moyens d'expression du calcul des probabilités; d'autre part, l'importance des variations pour ainsi dire qualitatives, des questions d'ordre et de configuration, appelle peut-être l'élaboration de

> théories mathématiques nouvelles (Granger 1955, p. 87; see also Granger 1955, 292-3).

Such awareness will later develop - thanks also to developments within mathematics, like Thom's contributions - into an awareness of the eventually non- "quantitative" character of the mathematical instruments of science as such, and will reach the point where Granger's original contrast between his own view of models and Campbell's view of "analogical models" (Granger 1959, p. 100') may be said to have half-avowedly faded away. Granger has always been careful in contrasting his advocacy of the role of "quality" in the sciences of man, and in science in general, with a Bergsonian vindication of the "qualitative" character of human phenomena.

If, in the domain of natural entities, it seems easy today to think of quality as appearance - or more exactly, to admit another phenomenology, according to which the object is determined by abstract schemata which enable us to grasp it effectively, in the domain of man such an approach apparently meets with much difficulty. The view is eagerly embraced that the very *essence* of the phenomenon is qualitative. Bergson founded his metaphysics and his dualistic theory of knowledge on this lemma (Granger 1960, pp. 106-7 [English trans. p. 85]).

For science, and particularly for the sciences of man, it is important to clarify the ways in which quality can be conceptualized. To start with, it is necessary to overcome the confusion - which is, however, one of the secret assets of aesthetic symbolization - between the quality of the external object and the quality of the psychical object, or, in figurative speech, between *qualité-esquisse* and *qualité tonale* (Granger 1960, p. 108 [English trans. p. 86]). The proper way in which quality can be considered in the sciences of man is as *limitation,* or rather *difference,* which gives origin to structures (Granger 1960', p. 10'9 [English trans. p. 88]). The conceptualization of quality leads to a *structural typology.* Therefore, the Hegelian transition from quality is not a compulsory outcome.

> ... a scientific elaboration of qualitative notions consists in the transition from the *a-structured* to the *structural,* rather than in a quantification. At all events, the transition to the quantitative is on1y one possible result of this dialectic (Granger 1960, p. 113, [English trans. p. 91 J).

More recent1y he has added a more general statement:

> ... a-model is qualitative, not because it avoids any consideration of quantity, but because it takes such considerations as a means, and because it subordinates grasp of quantity to grasp of forms... The development of our awareness of the deep nature of scientific knowledge may be rough1y symbolized by three words, each of which reinterprets and rectifies the former. In a first time, science was deemed to be possible on1y of what is *universal;* later on, on1y of what is *measurable,* Now we

should say that there is a science on1y of what is *structurable* (Granger 1981, pp. 94-5).

The more marked awareness of the primacy of the *structurable* in relation to the *measurable* seems to go along with an awareness of the role of "qualitative" models in every science (not on1y in the sciences of man) and of the non-identity of qualitative models with the sensuous element, or the pre-scientific attitude. In the history of the sciences of nature, "qualitative models" such as the models of the structure of crystals created by Bravais, Sohnke, Schönfield and Fëderov, are an example of an important way of conceptualizing the *form,* which takes it as an *invariant of a system of transformations* (Granger 1981, p. 91). Another way of conceptualizing the form is to consider it "as being characterized by its singularities (Granger 1981, p. 91). Such is the case of Thom's theory of catastrophes. In this case, without ruling out a quantitative approach, in particular at the level of such a dynamic evolution, the model is basically qualitative as far as it exhibits the conditions for such a stability, and as far as it points at the radical changes of forms (Granger 1981, p. 92).

It seems that, from this point of view, "imaginative" or "metaphorical" representations are still not to be accorded privileged status, but on the other hand they are no longer to be banned as dangerous. What matters is *representation;* and the task of representing can be carried out by that which is more concrete as well as by that which is more abstract.

A model will be for us an abstract representation of phenomena; even if it is a material product, what matters in the model, understood thus, is its function of schematization; its aim is, rather than somehow reproducing the phenomenon, representing it, high1ighting the opposition between its form and its content. Actually, the logician's models, that are in a sense more "concrete" than that which they are models of, arrive at the same result from another viewpoint; that is, they make the operational relationship between the form and the content intelligible (Granger 1981, p. 87; see also Granger 1971, p. 32 footnote).

As a consequence, it is hardly surprising that Granger defends the idea of qualitative models in science against those who want to see in such models mere thought-sketches and those who see in their introduction a victory of irrationality (Granger 1981, p. 87).

Even if Granger's rehabilitation of quality in science starts from the contribution of Bachelard (see Bachelard 1949, p. 87), it would be fair enough to say that Bachelard, and Duhem before him, would have drawn themselves up against Granger's present stance. By his rehabilitation of quality and - as shown in the preceding paragraph - of metaphor, Granger may be suspected of being on the verge of committing parricide.

5. Metaphors, analogy, and intellectual puritanism

I have tried to make sense of the multiform literary production of

Granger by "telling a story" about his "discovery" of models. This *histoire d'une maturation* might be summarized in the three following phases: i) in the first phase Granger tries to apply to the sciences of man, and notably to economics, the rationalist epistemology of Bachelard, and finds it useful to adapt Bachelard 's scheme by the addition of models as an intermediate term between phenomena and structures; ii) in the second phase he discovers in models the tool for bringing the sciences of man to scientific status while avoiding reductionism; iii) in the third phase he is led to generalize the notion of "qualitative model" to every branch of science as being the most general theoretical tool of scientific knowledge. As in many stories, a guest hero, that is, the econometricians' model, manages to become the king. But the listener of the story might raise a number of questions. For example: are the sciences of man real1y still in a pre-Galilean stage as Granger seems to believe? Or, in other words, if Granger's net lets big fishes such as Max Weber escape, is it the fish’s or the net's fault?

And, on the other hand, is the method of the sciences of man real1y so peculiar as Granger seems to suggest elsewhere? Or, again, could not the fact that for the sciences of nature we are used to having less problems in shifting from "original still rich and concrete but vague semantic references... to exact theoretical terms and *vice versa"* depend on the fact that the technology we use is much more uniform than the social and political practices have (happily) l now? But couldn't that be a mere contingent fact that might soon change? And couldn't this contingent fact be dependent on an "ideology", or on a presumptive explanation apparently offered by our practices of interaction with nature? In other words, one could say that the ideology of the "domination of nature", which the Frankfurt School has tried to denounce, is much more universal1y shared than the ideology of Liberalism, and that this is the reason why *the facts about nature,* even if they are still endowed with vague and concrete meaning, seem to be much univocal than facts about society.

Furthermore, what is peculiar to the sciences of man, if cybernetic models are basic both for them and for biology? And are the examples of correct model building which Granger has been able to provide not so poor (like the "theory of queues") as to belong rather to something that is preliminary to science but which should not be mistaken for science itself? And finally, while Granger explicitly states that the *analogical transfer* of methods and schemes from one field to another is a typical first step of scientific rationality that needs - in a very Bachelardian spirit - to be quickly superseded, but that may be nonetheless heuristical1y productive, how can he deny that *all* his "formal models" result from such a transfer of theoretical schemes, that energetic models result from an analogical metaphorical transfer to other domains of a scheme abstracted from mechanical systems, and that the same is true for cybernetic systems?

I shall try to find my way to at least a partial answer to these questions by coming back to what there is *behind* Granger's epistemology or, in semi-Bachelardian terms, by trying a psychoanalysis of Granger's epistemology. I will not be able to give an answer to all the questions: no good psychoanalysis and, as a consequence, no good psychoanalysis of epistemology, should end too soon with too many answers. I shall recall first *the past* of Granger's epistemology, that is, its relationship with the heritage of Bachelard, and secondly what was 'repressed' by Granger's epistemology, that is, analogy, metaphor, "imagination", which reappear in science disguised as "qualitative models".

It would not be a novel suggestion to say that such a 'repression' was performed on a large scale in our century by the official history of science which tried to put aside the whole Baconian trend of modern science (Kuhn 1977, pp. 31-65). But let us consider a narrower field, that of the French tradition in epistemology. Bachelard is universally acknowledged as the founder of this tradition: thus the different contributions of Althusser, Foucault, Granger, and in some aspects even that of Deleuze, stem from the incredibly rich reservoir of ideas in Bachelard's work (See Sertoli 1974; Vinti 1977), and the distance between the goals of the theoretical paths of Bachelard's disciples may be justified by more than one ambivalence present in the work of such a *Janus Bifrons.*

Granger never tried to conceal his indebtedness to Bachelard (Granger 1974; see also Granger 1955a, pp. 132-4, pp. 180-1, p. 321, p. 331; Granger 1955b, pp. 63-4; Moravia 1977; Breda 1982, pp. 40-9)· and the general image he offers of his work is that of a kind of para-positivism (Granger 1969) which is undoubtedly - between the intellectual attitudes competing in the contemporary French philosophical arena - the most true to Bachelard 's spirit. There is, however, an evident difference between the *Méthodologie économique* and later works: in the first Granger still appears to be an orthodox Bachelardian, directly echoing the theses of the *maitre* (such as his diagnosis of a coming of age of a "new scientific spirit" in economics) and, in terms of general climates of ideas, appears to be much more confident in the "magnifiche sorti e progressive" of science (See Granger 1955a, p. 24) than the later Granger appears to be.

I shall point to two main elements of Bachelard's heritage in Granger's epistemology. The first has given rise to the most productive and original features of the latter's contributions. From the second the sore points of his thought seem to be dependent. The first element is the idea of the *constitution of scientific objects* or of the *découpage* carried out by every scientific theory of its subject-matter (renewed and modified at every theoretical revolution). In Bachelard's words, science is "phenomeno-technics" or "production of effects". So, the scientific objects are "products", located at a level different from that of the things of everyday experience and sharply distinguished from them. It is mainly this key-idea of the constitution of

scientific objects that allows Bachelard's epistemology to avoid the shoals into which the logical empiricist tradition sailed, and which makes Bachelard and French epistemology in general appealing to post-Kuhnian English readers.

This idea is central to Granger's philosophy of the sciences of man, and it is thanks to it that he is able to find a third way between reductionism and formalism, or a path out of the nightmares of naive realism and out of the mist of conventionalism (See Moravia 1977, p. 11 ff., p. 26 ff.). The notion of model is a specific tool of the process of constitution of objects that Granger has been trying harder and harder to put right. The need he feels for such a foot-stoo1 to reach the highest shelf where structures are placed is, however, a symptom of the distance existing between his rationalism and Bache1ard's. A reason as multi-level, provisional and many-sided as its too1s - the models - are, is undoubtedly less pure and absolute than Bachelard's reason is still believed to be (See Sertoli 1974, p.40 ff.).

This 'weaker' reason suffers perhaps from a lack of confidence; Granger does not acknowledge how far he has moved away from Bachelard's rationalism precisely because he fears to come too close to the *Abgrund* of Foucault 's epistemological relativism. To my mind, he is not as close to the edge as he believes and I shall try to explain why by considering the second, more embarrassing, item of which Bachelard 's heritage was composed.

At the very heart of Bachelard 's thinking lies the opposition between *concept* and *image.* Many intriguing and ambivalent aspects of Bachelard's thought derive from his partial revaluation of imagination, understood as the non-rational or pre-rational dimension of our thinking, within the framework of one of the most ambitious forms of rationalism that has ever been conceived.

The theory of the imagination Bachelard relies on is basically a psychological theory: imagination stems from affectivity and eventually from the body, and it manifests itself in rêverie, in poetry, and finally in the *soi-disant* scientific knowledge, where it is present in the form of an analogizing function (Bachelard 1939; Sertoli 1974, pp. 445-6). Metaphors and analogy are the primary forms of epistemological obstacles and the primary source of error. Bachelard's project however is not that of a radical "drainage" of the unconscious, but rather that of "enclosing" it; between science and poetry some kind of complementarity should be accepted (See Sertoli 1974).

It is important to note that Bachelard’s theory of metaphor and analogy as "obstacles" derives from this psychological approach (Bachelard 1938, pp. 13-19). It is because they are understood as "concrete" images, carrying along libidic investment and projecting undue "subjective" features on phenomena, that they are opposed to "pure" rational scientific concepts which grasp structures (Bachelard 1939). This general background theory makes Bachelard sensitive to the important role that analogy used to play

in 17th and 18th centuries science (a feature that will be stressed again by Foucault; see Piazza 1985, pp. 87-8) but he is led by his theoretical framework to downgrade the 17th and 18th centuries to an "age of prescientific spirit" (Bachelard 1938, pp. 5-10). The purity, absoluteness, and postulated unity (as an ultimate goal or as a regulative idea) of scientific reason are at an opposite pole in Bachelard's thought to the 'discovery' of discontinuity in the history of science, to the thesis of the constitution of its objects by each scientific theory, and to the stress on the experimental, operational, and technical character of scientific inquiry. Bachelard seems to feel the need for some kind of counterbalance to stop running down the path on which Foucault will slide to the bottom.

Granger develops and modifies this element of Bachelard's heritage in a direction that could possibly lead to overcoming its most aporetic premises. A comparison with a divergent development - Foucault's - may be useful. Foucault dissolved the ambiguity of Bachelard's epistemology in the direction of a radical historicization and fragmentation; he developed Bachelard's heritage into a Nietzschean epistemology of *difference* (See Saba 1985, pp. 195-205). The discontinuity that Foucault establishes between the *episteme* of the *age classique,* centred on analogy, and the modern *episteme* centred on difference, radically relativizes on the one hand the image of modern science which Bachelard postulated to be absolute (so making it an insurmountable horizon *for our age),* but, on the other, develops an opposition between analogy and difference which was present in Bachelard, giving it however those dogmatical overtones which are the price that must be paid by every kind of historicism.

The development of Granger's thinking was rather in the direction of a linguistic turn; its main points are the acceptance of the notion of model, not understood as a "copy", as in Bachelard (See Bachelard 1949, pp. 10-11), but as a simplifying, organizing, and unifying device, and the explicit theorizing of metaphor as a feature of language rather than as a feature of "consciousness".

Granger's understanding of models has already been analysed in detail. As to his statements on metaphor, it is worth pointing out that they echo a revolution in linguistics and the philosophy of language which, around the middle of our century, has transformed metaphor into a central feature of language. Even if the 'new' perspective is far from being univocal, it converges on the statement that metaphor is *not* primarily characterized by its vagueness but by its 'concrete' character (See Ortony 1979). Granger's rather sketchy 'new theory of metaphor, which appears for the first time in 1967, makes metaphor consist primarily in one of the ways of representation: it is characterized by a

> dédoublement entre représentant et représenté... on superpose deux plans, dont l'organisation de l'un joue comme signe de celle de l'autre (Granger 1967, p. 772).

This definition should be understood within the framework of the opposition metaphor-metonymy. The metonymic representation "juxtapose deux fragments du vécu, dont les organisations s'évoquent" *(Ibid.).* It may be useful to point to a consequence which Granger does not mention. As the myth, or metonymical representation, is basic in art and ideology, as contrasted with science and philosophy, *metonymy assumes the role that was given by Bachelard to metaphor.* And metaphor, obviously enough, is now left free to do another job. Ali the 'Bachelardian' statements by Granger (in writings preceding 1967, but also in later works) that oppose the "concrete metaphor" or the analogical transfer of theoretical schemes to the pure conceptual schemes of 'mature' science, should be carefully revised in the light of the 'new theory of metaphor', in order to appraise their consistency with a 'liberalized' account of this notion (See for ex. Granger 1979, pp. 27, 54,73; Granger 1969, p. 402; Granger 1960, p.63 [English trans. p. 48]).

It would not be incorrect to state that Granger's afterthoughts on his *Pensée formelle* seem to endorse the diagnosis that I have outlined of the limitations of his previous positions. In the 1982 *Postface* he takes a stance against the thesis of a radical untranslatability of scientific theories, precisely by supplementing his previous thesis of the primary role of formal thought in science by the thesis of a *relative* continuity between science on the one hand and magic, art, and myth on the other:

> they very moment of science shows that its progress is effected precisely by successive translations into increasingly flexible and powerful languages, translations which undoubtedly displace concepts and modify them by transposing them (Granger 1982, p. 182).

What makes science different from other forms of production of symbolic objects is the fact that in science "the structure of symbolic systems has been made explicit and presented as an abstract schema of the real" (Granger 1982, p. 182).

And it is within the framework of such an acknowledgment of the relative continuity between science and the other kinds of production of symbolic forms (as far as its 'representative", rather than its 'operational' aspect is concerned) that Granger makes in this *Postface* the most drastically anti-Bachelardian statement, concerning the role of metaphor in science, that he has ever made:

> The first movement of a system of formal thought in the sciences, in view of this construction of concepts, consists in rendering the natural usage of language *metaphorical.* In the sciences of nature such a change of. meaning is by now so common that it is not even really felt... In the human sciences, on the other hand, it is particularly difficult to separate a naive usage from a metaphorical usage of notions (Granger 1982, p. 183).

6. Between Iconoclasts and Idolaters

I suggest the following conclusions:

i) The discovery of models is for Granger a further step of that movement away from 'Cartesianism' of which a previous step was Bachelard's discovery of the local, operational and discontinuous character of reason, this discovery meets, at a certain point of this movement, the discovery of metaphor which represented a step in a parallel movement of twentieth-century linguistics. The full implications of the encounter are perhaps still to be drawn.

ii) Models are the tool for 'softening' the Bachelardian image of science without lapsing into the dogmatic relativism that could be implied by an 'epistemology of difference'.

iii) His work in linguistics and economics has helped Granger in finding the tools for a non-Foucauldian way out of Bachelardian rationalism: the study of linguistics has worn away the basis of the dichotomy between concept and image; the study of economics has brought to the fore such factors as the need for a plurality of models, the sub-determination of phenomena and the need for a consideration of strategic variables connected with decision and with practical intervention.

iv) The development of Granger's epistemology, leading it away from Bachelardianism, has led it into encounters - much closer than Granger himself is able to acknowledge - with different paths of contemporary thinking; with the later Wittgenstein, as far as science is understood as a language game; with Hermeneutics, as far as comprehension is accepted as a preliminary step to explanation; with Pragmatism, as far as a plurality of scientific methods is accepted; and finally with the modelist trend of post-empiricism as far as a role is acknowledged for the analogical transfer of conceptual schemes, and scientific representation is assumed to be metaphorical.

v) A development can be recognized, from the Granger of the fifties to the more recent Granger, going roughly in the same 'anti-Cartesian' direction in which the several paths of thinking mentioned above went, and this development rescues the innovating contributions of Bachelard from their dogmatic rationalist interpretation by adding the linguistic dimension that was absent from the latter image of science.

7. Addendum on models and economic methodology

The two sciences in which Granger has been most interested have been economics and linguistics; and from the two it is economics that may be the best example of one of the "sciences of man" to which Granger's general statements refer. Furthermore, the most systematic and ambitious contribution to the philosophy of one science of man provided by Granger has been his *Méthodologie économique* (see Breda 1982, pp. 190-218). It is an intriguing circumstance that such a systematic attempt was

produced at an early stage in Granger's career and that it is precisely from this book that emerges the first draft of his theory of models, which was expanded later.

The question could be asked to what extent the intellectual strategy of *Méthodologie économique* is aligned with Granger's later positions and of the degree in which a possible strategic readjustment would require a reappraisal of the status of economic science. I suggest that precisely the more comprehensive theory of models presented in the following works implies a readjustment of Granger's diagnosis on economic science which should make at least some concessions to those who, after 1955, have announced - rather than the blossoming of a "new scientific spirit" in Economics - a deep crisis of economic theory.

Granger has acknowledged in recent years that the sciences of man swing between two opposite dangers: lack of "rigor" and lack of "exactness", A detailed analysis of *Méthodologie économique* would go beyond the scope of the present artic1e. I shall limit myself to suggesting only a few questions with which a reappraisal of Granger's economic epistemology could start. The reader, at first glance, may get the feeling that *Méthodologie économique* could have been a deeply innovating contribution to the philosophy of the social sciences if any one among the mainstream philosophers of science had read it. Unfortunately, it has been almost totally ignored outside France. The peculiar contribution of *Méthodologie économique* is precisely the fact of its being an analysis of scientific "works", rather than of depurated scientific theories that have little in common with the actual content of the works of the main economists. In other words, *Méthodologie économique* presents the only post-Kuhnian philosophy of economics that has ever been written, and it was published seven years before Kuhn's work. The innovating content of the book is, however, still embedded in a scientific *Ideenkleid* (like the whole of Bachelard 's epistemology) which confers on the conclusion of the argument a too marked faith in the promises of the scientific spirit as contrasted with ideologies (Granger 1955a, p. 24), a too epistemic faith in the theoretical progress made possible by such a recent offspring of economic theory as econometrics (Granger 1956b, p. 518), and a too drastic refusal of the contribution of a social theorizing centred on comprehension (Granger 1955a, pp. 256-7). Such a frame, while appropriately stressing the practical/operational character of economic science understood as "applied rationalism", finally makes it overlook the 'dangers' (let me say so, even if, after Foucault, this is an exceedingly naive word) carried by an ineluctable association of scientific .inquiry with socio-political power.

Optimistic and scientific statements of the young Granger have been moderated by the older (See Granger 1978, p. 139). But, in more detail, some points of the content of the work should also be reassessed in the light of Granger's more recent generalized theory of models:

i) The econometrician's model is undoubtedly now a particular instance of a more general approach; the various ideas of equilibrium that have been formulated in the course of the history of economic theory should now be viewed as representing the standard case of model.
ii) The explicit acknowledgment of the problems which exist as regards the definition of the subject-matter of economics (Granger 1955a, p. 401; Granger 1956b, p. 522), should be stated in crude terms, and any hope of a solution coming from theoretical progress in economics itself should be abandoned. It is more because of the *specific character* of the sciences of man, than because of their *underdevelopment,* that the definition of the subject-matter of economic science is problematic. At a macro-level, to define what is economical implies nothing less than a recognition of shared social goals (Granger 1955a, pp. 225-6, p_ 245).
iii) It is precisely in connection with the above point that more emphasis should be given to the comprehensive approach than *Méthodologie économique* does. The economic science is, like every science, a language game, but a language game that meets and overcomes obstac1es (Granger 1979, p. 210). The hermeneutic model is the first stage of that modelling process in which economic science consists. It is this preliminary stage which grants formal models their grasp on reality, as not every system that is determined from an algebraic point of view is determined from an economic point of view (Granger 1955a, p. 301; Granger 1976, p. 149). A role should also be acknowledged for the comprehensive approach in the constitution of the subject-matter ; the substantivist definition of the subject-matter of economics presented in *Méthodologie économique* should be completed - in the light of Granger's later contributions - not with the traditional1y opposing formalist definition, but rather with a partial1y conventionalist definition: macroeconomics can be defined as the science of social material products (Granger 1955a, p. 226), provided that the socio-historical framework has been outlined which establishes what should count as social ends and what resources could possibly be means to those ends in that particular context.
iv) The enthusiasm for econometrics should be somewhat dampened provided that the operational-experimental character of econometric models, as contrasted with the aprioristic character of "systems" such as Walras' (Granger 1955a, p. 302), is acknowledged as being of authentic 'scientific' character. Further stress should now be added to a remark that is already present in *Méthodologie économique:* that measurement is not a necessary and sufficient characteristic of science (Granger 1955a, p. 400), and that we must know *what* we are trying to measure (Granger 1981, pp. 94-5; Granger 1974, p. 69).

Even if applied to *Pensée formelle* rather than to *Méthodologie économique,* this point has been explicitly made by Granger himself in the 1982 postface. He admits that he has previously insisted "perhaps too much" on

the need for new mathematical tools in the sciences of man. And he acknowledges that "decisive progress cannot be achieved in this field by merely perfecting technologies" (Granger 1982, p. 185), and that accordingly what is needed above all, before applying mathematical tools, is "a conceptual preparation of the phenomenal fie1d" (Granger 1982, p. 186).
v) The contribution of the whole classical period is rather underrated, echoing the ungenerous valuation of eighteenth-century science by Bachelard which I have already mentioned.
vi) The fina1 point is probably the most controversial: is the new scientific spirit really (or *still*, in the Eighties) blossoming in economics, or rather, is economic science undergoing a deep crisis? And couldn't the reason for this crisis be the excess of "rigor" at the cost of "exactness"? And, accordingly, couldn't the diagnosis presented in *Méthodologie économique* have overlooked, in the panorama of twentieth century economic thinking, the importance of those writers - above all Keynes - who have insisted on the need for "exactness" in the study of the ways the various economies function and of their place in societies?

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