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SOME PECULIARITIES OF THE CONCEPT OF CAUSALITY IN MACROECONOMETRICS

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This paper analyzes the philosophical underpinnings of the most relevant approaches to causal inference developed in macroeconometrics, from the Cowles Commission to the var program. The development of macroeconometrics in the last sixty years reflects a change of position regarding the concept of causality. Initially, by the Cowles Commission, this concept was conceived as something derived from economic theory and providing structure to models. Later, it was viewed as something that can be inferred from minimal background assumptions and statistically tested. Furthermore, this paper examines the connections between causality, stability and the problem of identification. There emerge some methodological tensions and lessons.

1. Introduction

One of the main tasks of applied macroeconomics is to isolate, of the empirically measured connections between variables, those that seem to have some special features and that are often called causal relationships. The usage of causal concepts has a long tradition in the history of philosophy and science, and is also present in our daily life, since causal language appears to structure the way we think and how we make decisions. Instead of a unique concept, it is more correct to talk of a family of concepts, whose meanings are interrelated, but whose common factor is not always clear.[1] This is also demonstrated by the difficulty which philosophers have faced as they seek to agree upon a unique definition of causality, free of counterexamples. Economists also use the word 'cause' with different underlying meanings. Causal relationships are sometimes referred to as those relationships in which it is possible to control one variable in order to influence another. This definition is very important for macroeconomics because one of the professed goals is to provide know-

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[1] A similar idea is expressed by Cartwright 1999, 118-121.

ledge for policy makers. Other economists, however, often use causal language to refer to relationships in which the value of a variable helps to predict the value of another variable.[1]

In the past, numerous methodological and theoretical discussions in economics have involved causal concepts. There was a consensus built around the Cowles Commission approach, between the 1940s to the 1970s, on the role of macroeconometric modelling. In this framework, causes were derived from economic theory and the role of statistical theory was to measure them.[2] There was little agreement about the structure of such causal relationships, as the debate between 'interdependent' and 'recursive' models suggests. Economists of the Cowles Commission, however, concurred with the view of causation as a relationship based on economic theory, which eventually could permit policy intervention. In subsequent developments of econometrics, this concept of causality was partially replaced by a concept of causality that was tied to statistical properties and could be tested. The history of these developments is connected to how the *problem of identification* has been faced. The problem of identification amounts to the problem of isolating the unobserved relationships – among the variables of interest – that have generated the data, choosing among the possible relationships that are consistent with the measurable statistical properties (e.g., correlations) of the data.

In this paper, the philosophical roots of such developments are analyzed in a manner that is consistent with the spirit of the few works that combine an explicit philosophy of science viewpoint with an analysis of econometric practices. For example, see Cartwright 1989, Galavotti and Gambetta 1983, Hoover 2001, Keuzenkamp 2000, Lawson 1989, Lawson 1997.[3] The idea that a philosophy of science viewpoint permits a better understanding of econometric analysis has sometimes been discredited by the argument that philosophers are even farther behind than economists from reaching an agreement on causal concepts (Granger 1980, 331). The aim of a philosophy of science viewpoint, however, is not necessarily to produce new concepts. Such viewpoint may indeed be very important, as the works

[1] Although in such case economists often qualify what they say by using the term 'Granger cause' rather than simply 'cause', as is shown below.

[2] Nonetheless, there were also competing conceptions of the role of econometrics in those years. One of them, inspired by Tinbergen's work on business cycles, viewed the possibility of inferring structural relations from data and bringing them to confront the theory *ex post facto*. The reader is referred to Morgan 1990, in part. ch. 4 and 251-258, and Hoover 2004.

[3] There is, however, an increasing number of philosophy of science studies which, in order to analyze causal models and causal inference, refer to econometrics as a special discipline. See, for example, Woodward 2003, and references therein.

quoted above demonstrate, to clarify and criticize notions which are commonly used by practicing scientists and which have a long and turbulent history in the philosophy of science.

This paper tries to attempts what are the philosophical roots of the causal concepts used in three paradigmatic econometric practices: the Cowles Commission approach, Rational Expectations econometrics and the Vector Autoregression (VAR) program. The goal of this paper is to shed light on some conceptual and methodological tensions that emerge from these econometric developments. The first tension exists between the conceptualization of causality as a theoretical entity which provides structure to a model, and a concept of causality as reducible to the statistical properties of the data. A second tension involves the interpretation of causality as control and the interpretation of causality as predictability (Granger-causality). A third tension concerns the relationship between causes among macroeconomic aggregates and microeconomic behaviours.

The following section of this paper will outline the philosophical framework of the investigation and present operational definitions for terms such as reductionism, realism and instrumentalism in the context of macroeconomics. In econometric studies, the philosophical roots underlying causal concepts have been disclosed by the manner that econometricians have faced the problem of identification. The third section will examine the problem of identification faced by the Cowles Commission approach, Rational Expectations econometrics, and VAR program. In the fourth section the three tensions mentioned above are discussed in greater detail. The fifth and final section draws some conclusions and directions for further research.

2. Philosophical Setting

In the philosophical analysis of causality, it is often useful to distinguish, as Hoover (2001, 12) does in his reconstruction of Hume's causal account, among queries about the meaning of the term 'cause' (*conceptual* analysis), queries about the reality which causal relations refer to (*ontological* analysis) and queries about the way in which beliefs about causal relations are inferred (*epistemological* analysis). In the context of macroeconomics, epistemological questions are: how are causal relationships discovered in econometric practices? How are the methods justified? What does it make a procedure for causal inference in macroeconometrics correct?

Ideally, we would like to address only epistemological problems, to provide some reliable grounds to the operations of econometrics. However, the difficulty of reaching an agreement on epistemological issues is in part due to the fact that they involve conceptual and ontological issues that are problematic as well. As far as conceptual difficulties are concerned, it is almost impossible to give a definition of causality free from counterexamples, even for the specific context of macroeconomics. A potential way to overcome conceptual problems is to endorse some of the ideas of philosophy of language expressed by Wittgenstein (1953). In the *Philosophical Investigations*, Wittgenstein claims that the meaning of a word is not rigidly tied to a limited concept; the meaning stems from its use (Wittgenstein 1953, see in particular remarks 30 and 68). If we follow this approach, we should not be concerned with finding a stable definition of the term 'cause', because the meaning of any term stems from its use in the linguistic community, and we should just investigate the use that members of the scientific community and, possibly, agents operating in the economy make of this term. Indeed, it turns out that the manner in which not only economists, but also policy-makers, consumers, and firms use this term is overlapping but not always the same. This also explains why it is so difficult to reach agreement on a unique definition of causal relations.[1] Yet, even if we admit that causal concepts are variegated and not reducible to a fixed definition, it is important to ask whether there is a definition of causality that is most appropriate for the specific context of macroeconomics.

There are additional problems related to ontological issues. Major ontological questions in the philosophy of causation include the following: are causal relations theoretical entities which exist independently of the observer? How are causal facts related to non-causal facts? Are the former reducible to the latter or logically independent? Depending on the answer given to these questions, *reductionist* and *realist* approaches to causation are distinguished.[2] According to a reductionist approach (to causation), there are no such things as causal relations distinct from regularities (e.g., statistical correlations), or, at least, even if they exist we are not able to identify them. In a realist approach (to causation), there is something in the reality that diffe-

[1] We do not need, of course, to capture ordinary language when the discussion about causality is a scientific one. However, also within the scientific economic community, there seem to be a variegated usage of causal terms.

[2] In the philosophy of science there are many different forms of realism, depending on the different positions about theories, entities, existence and truth held. This paper will focus on realism (and instrumentalism, which is usually formulated in opposition to realism) only as far as causal relationships are concerned. A realist philosophical position about causal relationships holds that such relationships exist or are real in some specified ways. Of course, the notion of reality is not reducible to a single interpretation. Thus, realism about causality also admits several possible forms.

rentiates causal relations from non-causal connections.[1] Of course, the claim that such relations exist independently of the observer *tout court* is hardly justifiable in economics, where the observers are also the object of the theory. Independence from a particular observer or from a particular representation, however, is more easily justifiable in economics.

An influential stream of the philosophy of science of the twentieth century considered scientific laws and entities to be more conventional and functional models for measuring and predicting, than realistic mirroring of natural phenomena. This philosophical stream, which has sometimes been referred to as *instrumentalist*, was inspired by some scientists between the second half of the nineteenth century and the beginning of the twentieth century. Ernst Mach, for example, proposed to eliminate the concept of causality from the scientific language and substitute for it the concept of function, which he believed to be less tied to metaphysical issues. The concept of causality received more consideration in the second half of the twentieth century. The history of such revaluation is connected not only with the use of new probabilistic, statistical and logical tools, but also with a need for an *applicable* concept coming from special fields. Social sciences, epidemiology, and engineering, indeed, have been using causal notions regardless of whether or not such a concept was considered legitimate in the philosophy of science and in physics. The upshot is a variety of concepts, without an agreement on a definition and on a method of inference. A common factor among these concepts is that they invoke domains that differ in many important respects from the one originally considered in traditional analyses of causation, which have physics as the science of reference. Indeed, the disciplines which have fostered causal concepts in the last decades belong to what Simon (1969) called the «sciences of artificial». These sciences, among which Simon placed economics, deal with a human built reality. Since it is a reality which has been generated by *mechanisms* or *powers*, it contains, before and independently of any scientific investigation, something similar to a causal structure. However, another characteristic of the social system is its *complexity*, in the sense that economic behaviour seems to incorporate a large element of chance and dynamics. Thus, we may hypothesize that there is a relatively simple set of causal mechanisms that generate the observable economic behaviours, but the action of such set of causal mechanisms can be offset by the

[1] Notice that a reductionist approach to causation may be realist with respect to other entities such as non-causal facts.

countervailing actions of juxtaposed and hidden mechanisms.[1] This means that, even if it is believed that there are some causal powers in the social system, it may be difficult to isolate them from the study of empirical associations. As Lawson (1998, 493) stated, «[t]he social system, including the economy, is *open* in the sense that regularities of the form 'whenever event (type) *x* then event (type) *y*' are not pervasive» (italics added).

On the one hand, a certain degree of complexity and juxtaposition of causal mechanisms permits one to conceive behavioural economic relationships as non-deterministic or as tendencies that holds on average or for the most part. This is reflected in time-series econometric models in the division between 'signal' (which captures the tendencies that are object of interest) and 'noise' (accounted by the error terms). This would permit the application of probabilistic accounts of causality, which claim, roughly speaking, that an event (type) *A* causes an event (type) *B*, if the realization of *A* renders more likely the realization of *B* (in the same way that taking an aspirin renders it more likely that one's headache will subside). On the other hand, if we admit that the social system is highly complex, the possibility of capturing, starting from the probabilistic measure of regularities, the causal relationships that we are interested in is jeopardized. Indeed, it may be the case that noises are stronger than signals and that the mechanisms involved undergo several or even continuous structural changes.

Thus, philosophical controversies such as realism versus reductionism should be contextualized to the specificities of macroeconomics. It should also be noted that the terms reductionism and realism are often used in the economic literature with meanings that appear different from those given above. Reductionism, indeed, is usually associated with the program of giving microfoundations to macroeconomics and with 'realism' one often denotes macroeconomic models which reproduce relations easily recognizable in the 'commonsense' or 'folk' view of the economy.[2]

[1] The notion of complexity due to the presence of many 'disturbing causes', which interfere with the major causes that are object of investigation, has a long tradition going back to John Stuart Mill (see Hausman 1992, ch. 8).

[2] Mäki (1996) denotes this notion with the term *realisticness*, to be distinguished by the philosophical notion of *realism*. As far as reductionism is concerned, we should, generally speaking, distinguish among *at least* three different notions of reductionism. The first is the *eliminative* reductionism: a theory T1 is reduced to a theory T2 by getting rid of the ontology of T1 (e.g., the rejection of Phlogiston theory with the development of modern chemistry or the proposal of Russell of abandoning the term 'cause', because it is a relic of old ages). The second one is the *identifying* reductionism: T1 is reduced to T2 and the explanations proceed via an identification of the ontology of T1 with part of the ontology of T2 (e.g., reduction

3. Causality and Reality in the Development of Econometrics[1]

3. 1. *The Problem of Identification*

The problem of identification involves the difficulty of isolating, among the possible models consistent with the statistical properties of the data, the structural model which has actually generated the data. This challenging problem has a history which is linked with the development of causal concepts in econometrics.[2] Econometricians usually use the term 'structural', referred to models or equations, in the sense of «invariant with respect to a specified class of intervention» or, in a perhaps weaker sense, «motivated by an explicit economic theory» (Bernanke 1986, 51). These structural relations are usually considered to be causal relations that can be used for policy interventions. Thus, structural models, invariance and causality form intertwined concepts, which have evolved in the history of econometrics. The philosophical roots of such developments are revealed by the different ways in which the problem of identification has been faced. Lucas and Sargent (1979, 298-299) stated that «[t]he problem of identifying a structural model from a collection of economic time series is one that must be solved by anyone who claims the ability to give quantitative economic advice». The various approaches to econometrics can be seen as different ways of facing the problem of identification. The Lucas (1976) critique can also be interpreted as an attack against the method, according to which the problem of identification was addressed until that time, and as a proposal of an alternative method to address this problem.

Let us now give a formalization of the problem of identification, which is useful to compare the various approaches. Suppose X_t is a $k \times 1$ vector of macroeconomic time series, which is assumed to be non-stationary.[3] Suppose also that the Wold representation of X_t is:

of macroeconomics to microeconomics in the representative agent models). The third one is the *conceptual* reductionism: T1 is reduced to T2, in the sense that the notions used in T1 are defined in terms of the notions used in T2 (e.g., we define causes in terms of probability and we reduce causality to statistical regularities).

[1] This section is a revised version of Moneta 2004a, sect. 1.3; and Moneta 2005, sect. 5.

[2] As shown below, this does not mean that any causal concept or causal method developed in econometrics aimed to identify the structural model of the economy. Granger-causality, for example, is a method dealing only with reduced form equations. But the success of atheoretical methods such as Granger-causality was linked with a particular view of the identification problem (claiming the failure of the Cowles Commission approach).

[3] Indeed, most of macroeconomic time series, like income, consumption, investment, interest rate and inflation, usually have to be differenced before they are stationary. The formalization of the identification problem in this terms and notation follows King *et alii* 1991, 822.

$$\Delta X_t = \mu + C(L)\,\varepsilon_t, \qquad (1)$$

where ε_t is a white noise vector, that is serially uncorrelated with a mean of zero and variance-covariance matrix Σ_ε, C(L) is a lag polynomial (so that $C(L)\,\varepsilon_t = \varepsilon_t + C_1\,\varepsilon_{t-1} + C_2\,\varepsilon_{t-2} + \ldots$), and μ is a constant term.

Of course, there are infinite representations of this form, since it is sufficient to define a lag polynomial $A(L) = C(L)\,A_0$ for an appropriate matrix A_0 such that the vector $u_t = (A_0)^{-1}\varepsilon_t$ is still a white noise. What economists are looking for, however, is a model that represents stable relations and in which white noise terms are interpretable as economic shocks. Such a model is called a *structural* model and has the form:

$$\Delta X_t = \mu + \Gamma(L)\,\eta_t, \qquad (2)$$

where η_t is a $k \times 1$ vector of serially uncorrelated structural disturbances with a mean of zero and a covariance matrix Σ_η (again, $\Gamma(L)$ is a lag polynomial so that $\Gamma(L)\,\eta_t = \eta_t + C_1\,\eta_{t-1} + C_2\,\eta_{t-2} + \ldots$). The corresponding *reduced* form model is of the form (1) with $\varepsilon_t = \Gamma_0\,\eta_t$ and $C(L) = \Gamma(L)\,(\Gamma_0)^{-1}$. The solution to the problem of identification consists in finding a way to deduce η_t and $\Gamma(L)$ in (2) from ε_t and C (L) in (1).

Let us analyze how such problem has been faced in three influential macroeconometric approaches: the Cowles Commission, the Lucas critique and Sims's VAR program. For each of these approaches the questions of interest are the following:

i. Which relations are considered regular or stable in the economic analysis?

ii. Is there any characteristic of such relations that distinguishes them from empirical regularities? When can they be called causes? When can they be used for economic policy control?

iii. Which methods are considered more appropriate to infer and measure causes?

3. 2. *Cowles Commission Approach*

In «Keynesian Macroeconometrics»,[1] which is the approach to econometrics criticized both by Lucas (1976), and, as we will show later, by Sims (1980), structural parameters are identified by the imposition of several types of *a priori* restrictions, for example by classifying variables as exogenous and endogenous. In the form of equation (2) this

[1] The expression «Keynesian Macroeconometrics» has been explicitly used, in a critical way, by Lucas and Sargent 1979.

would correspond to assuming that certain blocks of Γ_0 are zero, as illustrated in King *et alii* 1991.

Hoover (1988, 270) notes that «there is a certain irony in criticizing any econometrics as *Keynesian*, given Keynes's own scepticism of econometrics. …What is of course true is that most builders of large-scale macroeconometric models classified themselves as *Keynesian*». Keynes's scepticism about the first developments of econometrics (see Keynes 1939) can be interpreted as a consequence of his own view of economic causal structures. Keynes points out that to measure economic relationships with statistical techniques alone we would need beforehand a correct and complete list of the significant causes, which seems to be an exceedingly arduous request for economic theory (Keynes 1939, 560). Lawson (1989, 247) claims that we can reconstruct from Keynes's writings a view of economic processes as constituting a non-atomistic and open system.[1] What is certainly true is that Keynes is very sceptical about the capacity of statistical techniques to be able to measure a cause-effect relationship because the mechanism which produces such relationship is intertwined with other mechanisms (see Keynes 1939, 560). However, Keynes believes that causal mechanisms do exist in the economy: there are *tendencies* in the economy, which «are likely to persist» (Keynes 1973a [1936], 254). It is possible to grasp and examine such tendencies, but not to measure them using empirical regularities. In a letter to Harrod of 1938, Keynes wrote: «[i]n economics … to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought» (Keynes 1973b, 299). In philosophical terms, Keynes's view of causality is strongly non-reductionist and realist: causal facts are primary with respect to non-causal facts, such as empirical regularities.

The Cowles Commission program in econometrics aimed to give a formal and quantitative treatment of the macroeconomic theories that were developed in the 1930s. The main focus was Keynesian theory, but interpreted as a general equilibrium system, and without endorsing Keynes's approach to probability and statistics. The basis of what will be called the Cowles Commission methodology is found in Haavelmo's seminal paper «The Probability Approach in Econometrics» (1944). In this paper, Haavelmo proposed a coherent framework for applying statistics to economics. The general idea was that

[1] An open system, according to Lawson, is a system which does not satisfy the condition of *closure*, whose necessary conditions are, in turn: «each causes has the same effect» and «each effect has the same cause or set of causes». A system is non-atomistic when the «behaviour at the level of complexes» cannot «be determined from behaviour at the more autonomous or atomic level» (Lawson 1989, 241-242 and 247).

observed data can be seen as a realization of an independent causal structure, which econometricians now refer to as the data generating process. According to Haavelmo, the causal relationships constituting the data generating process have three characteristics. First, they are *stochastic*, in the sense the causal relationships constituting the structure underlying the data are disturbed by many irregular and less influential latent causes (Haavelmo 1944, 40 and 52-53). This structure can thus be represented by a system of equations, in which the systematic causal factors are the dependent variables (regressors), while the disturbing elements are accounted by the error terms. Such terms follow a joint probability distribution, which is well-defined if the system is properly specified. Second, they are *simultaneous*, in the sense that the economy as a whole can be characterized as a Walrasian general equilibrium system in which «everything depends upon everything else» (Haavelmo 1944, 22). This does not contradict the presence of *one-way* causal relationships within the various sectors of the economy, because simultaneity is built upon by aggregation (Haavelmo 1944, 22). Third, they are *autonomous*, wherein the systematic relations do not break down when some external conditions change (Haavelmo 1944, 29). To explain what an autonomous relation is, Haavelmo uses a mechanical analogy:

> If we should make a series of speed tests with an automobile, driving on a flat, dry road, we might be able to establish a very accurate functional relationship between the pressure on the gas throttle (or the distance of the gas pedal from the bottom of the car) and the corresponding maximum speed of the car. And the knowledge of this relationship might be sufficient to operate the car at a prescribed speed. But if a man did not know anything about automobiles, and he wanted to understand how they work we should not advise him to spend time and effort in measuring a relationship like that. Why? Because (1) such a relation leaves the whole inner mechanism of a car in a complete mystery, and (2) such a relation might break down at any time, as soon as there is some disorder or change in any working part of the car. … We say that such a relation has very little *autonomy*, because its existence depends upon the simultaneous fulfilment of a great many other relations, some of which are of transitory nature.
>
> (Haavelmo 1944, 27-28)

Thus, what distinguishes autonomous relations from empirical regularities is, on the one hand, the explanatory power, and on the other hand, the invariance under new conditions. Haavelmo does not use the word *causal* in the quoted passage (he uses more causal notions to refer to microbehaviours), but other exponents of the Cowles Commission (see, for example, Simon 1953 and Hurwicz 1962) use the related concept of «invariance under intervention» to define those special causal relationships that constitute the structural equations in an econometric model.

According to Haavelmo – to answer the questions posed at the end of section 3. 1. –, «there are more fundamental relations than those that appear before us when we merely stand and look» (Haavelmo 1944, 38). These relations are among aggregates, but they are underpinned by «fundamental behaviouristic relations» that describe «individuals' decisions to produce and to consume» (Haavelmo 1944, 28). The fundamental relations are *causal*, according to Haavelmo, in the sense that individuals make the *best decisions* given the conditions in which they are involved. But Haavelmo claims that we are able to discover autonomous relations among aggregates, not reducible to constant relations. What distinguishes autonomy from constancy or persistence of a relation is that autonomy «refers to a class of *hypothetical* variations in the structure, for which the relation *would be* invariant, while its actual persistence depends upon what variations *actually occur*» (Haavelmo 1944, 29).

The definition of causality as structural relations which are invariant to intervention given by Simon is analogous to Haavelmo's notion of autonomy. The focus of Simon (1953) is on the syntactic properties of the model. The main issue is to specify the algebraic properties that a model should satisfy, in order to determine its causal ordering. However, Simon's (1953) discussion of observational equivalent systems of equation, as well as his definition of structural equations as those equations representing causal relations invariant with respect to interventions, suggests that the ultimate interest is mapping to causal relations that exist somewhat autonomously in the world.

How can these relations be tested and measured? The tools are given by the well-known Simultaneous Equation Models (SEM). In this approach it is assumed that there exists an underlying mechanism that generated the data. However, it is emphasized that without a priori restrictions imposed by economic theory it would be almost impossible to identify (causal) economic relationships.

In its declared objectives, the Cowles Commission methodology is non-reductionist and realist. There is something in the reality at the macro-level that distinguishes autonomous (Haavelmo) or causal (Simon) relationships from empirical regularities. Haavelmo explains the causal relationships as underpinned by causal microbehaviours, while Simon is more focused on causality as a property of a model, but both authors believe in the possibility of measuring causal properties of objective economic processes.

In the practice of econometrics, however, empirical studies inspired by the Cowles Commission methodology have often failed to endorse a realist approach. Especially in the 1970s, when doubts were increasing about the reliability of Keynesian macroeconomic theory, econometricians were using theoretical restrictions, without much belief in the real existence of causal relations associated with such restrictions (for a criticism of this practice, see Sims 1980). Lawson (1989) claims that Haavelmo's approach is ultimately instrumentalist. He states that Haavelmo's concern about autonomous relations denotes a realist orientation, but which is counterpoised by a view of econometric models as «serving merely as an instrument or tool for deriving practical statements» Lawson (1989, 252). This is not enough, however, for an instrumentalist position, as defined in philosophy of science: a realist position does not exclude the claim that models are instruments for deriving practical statements. The instrumentalist position, on the other hand, typically excludes the claim that we find the goal of true representation of the reality among the goals that a theory or a model is pursuing (Mäki 1998). Since the goal of representing the «real structure» of the economy (Haavelmo 1944, 28) is certainly present in Haavelmo's approach, the label of instrumentalist is hardly applicable.

When we consider the applications of the Cowles Commission methodology, however, it becomes problematic to prove, in a non-experimental setting such as economics, that some relations are invariant under interventions. In order to achieve identification of large scale econometric models typically used until the end of 1970s, a large class of *a priori* restrictions were used. These restrictions were dictated not only by economic theory, but also by common practices not grounded on representations of the reality. Thus, in the econometrics inspired by the Cowles Commission, we find a continuum of positions between realism and instrumentalism. The most realist positions are found in the works of theorizers of the Cowles Commission approach, whereas the most instrumentalist positions are found in the works of practitioners of the 1970s. These latter scholars were inspired by the Cowles Commission approach, but treated regular relations *as if* they were generated by causal structures, and models as useful instruments of predictions, with little belief in their power to represent true causal relations of the world.

3. 3. *Lucas' Critique*

Lucas pointed out that the restrictions imposed by the *Keynesian* approach to macroeconometrics were not sound, because the parameters estimated were not invariant to a change of policy regime.[1]

[1] Lucas 1976, 21 remarks that the criticisms he raises «have, for the most part, been anti-

He proposed that stable relations should be estimated from the underlying choices of individual agents. On the other hand, Keynesian macroeconomics neglected two 'classic' postulates: that agents act in their own interest and markets clear.

The seminal paper of Lucas (1976) can be interpreted as a criticism of the solution to the identification problem put forth by the Cowles approach, and of the use of large scale macroeconometric models based on this approach. Lucas considered an analogous problem of autonomy or invariance.

> His own important contribution [to the problem of invariance] is to observe that one of the relations frequently omitted from putative causal representations is that of the formation of expectations. He notes, further, that the formation of expectations may depend upon people's understanding of the causal structure in general and of the process of policy formation in particular.
>
> (Hoover 1988, 191-192)

Thus, the properties of an economic aggregate are to be explained, according to Lucas, by reference to the behaviour of rational economic agents such as those postulated by neoclassical microeconomics, with the supplement of the rational expectations hypothesis. Moreover, Lucas and his fellows recommend the use of representative agent models, in which the behaviour of diverse agents is in turn reduced to choices of one 'representative' standard utility maximizing.

To further address the questions posed at the end of section 3. 1., the stability of economic relations is confined within the microbehaviours, in Lucas's approach. All the economic relations are to be founded on individual decision-making processes, which consider that individual agents do their best given the available information. The equations simply describing macroeconomic aggregates cannot systematically be used to guide a change in policy. If the statistical descriptions of macroeconomic time series cannot be used for economic policy control, one could argue, as Granger (1980) does, that controllability is something different and perhaps stronger than causality, by considering examples of causal relations that vanish once the cause is used for control. Indeed, the notion of Granger-causality plays a certain role in the models founded on the rational expectation hypothesis. The concept of Granger-causality is much weaker than controllability. A time series $\{x_t\}$ is said to Granger-causes the time series $\{y_t\}$ if the knowledge of the past and present values of $\{x_t\}$ contributes to forecasting $\{y_t\}$, as it reduces the variance of the prediction errors. Thus, Hansen and Sargent (1980, 92) claim that Granger-

cipated by the major original contributors» to econometric theory, and cites Marschak and Tinbergen.

causality plays a «natural role» in the models based on the rational expectations hypothesis, because

> agents' decision rules typically involve predictions of future values of the stochastic processes, say w_t, that they care about but can't control, e.g., in competitive models output prices and / or input prices. …All processes agents see and that Granger cause w_t belong in agents' decision rules.
>
> (Hansen and Sargent 1980, 92)

However, models founded on rational expectations models following the Lucas critique involve a notion of causal structure which is completely consistent with the Cowles Commission, and which ultimately requires an interpretation of cause as control. Once the formation of expectations[1] are included, the resulting model of the economy is a model whose parameters are invariant under interventions. Indeed, Hansen and Sargent (1980) propose a method to solve the problem of identification, which is immune to Lucas critique. In the form of equation (2), it would correspond to impose cross-equation restrictions on the various elements of $\Gamma(L)$. The peculiarity of these restrictions is that they are grounded in the bedrock of given tastes and technology. The goal is to isolate stable parameters and enable the evaluation of the effects of policy interventions.

In philosophical terms, Lucas's approach is reductionist, in the sense that macroeconomic causal relations are reduced to micro-behaviours in which rational expectations play a primary role. But Lucas claims that we are able to distinguish between objective microeconomic structures and empirical regularities. Thus, at the micro-level Lucas maintains a realist position.

3. 4. VAR *Approach*

Sims (1980) pursued the criticism of traditional macroeconomic models further. He claimed that «econometricians imposed large numbers of restrictions that were *incredible* in the sense that they did not arise from sound economic theory or institutional or factual knowledge, but simply from the need of the econometrician to have enough restrictions to secure identification» (Hoover 1995, 6). Sims's reaction to the failure of the Cowles approach, however, is alternative to the rational-expectations econometrics programme. While one stream of econometric research after the Lucas critique continued to pursue identification of structural models, by using restrictions grounded in

[1] In the formation of expectations, Granger causality plays indeed a role in determining which variables enter in the decision rule, but the crucial role is played by the structural model of the economy, which is known by the individual agents, according to the rational expectations hypothesis.

individual decision-making, Sims argued that economic relations are in principle not identifiable. «Sims proposed that macroeconometrics give up the impossible task of seeking identification of structural models and instead ask only what could be learned from macroeconomic data without imposing restrictions» (Hoover 1995, 6).

The approach proposed by Sims involves unrestricted reduced form equations, namely Vector Autoregressive (VAR) models. Each variable is considered to be endogenous and is regressed on lagged values of itself and of all the other variables. Sims (1982) argues that his program is immune to the Lucas critique, because policy action consists in an «implementation of a fixed or slowly changing rule» and so the agents form probability distributions over the range of possible policy stances. The shocks embody all the surprises and innovations related to the information set of economic agents.

More generally, in VAR models, the shocks represent exogenous actions on the economy. These actions are usually given a causal interpretation, although they cannot be systematically exploited by the policymakers. A large part of the literature about VAR models has been dedicated to transforming the random terms, in order to get uncorrelated exogenous shocks, which can be given an economic interpretation. In terms of equation (2), Sims (1980) assumes that the covariance matrix Σ_η of the structural shocks η_t is diagonal, and Γ_0 is triangular. The form of Γ_0 reflects the causal structure among the contemporaneous variables: if Γ_0 is triangular we have a Wold causal chain, in which each variable causes all the variable below in the vector and each variable does not cause any variable above in the vector.[1] Since the orthogonalizing transformations form an observationally equivalent class, the so-called Structural VAR approach proposes to use, in order to identify the model, restrictions derived from background knowledge (for example institutional knowledge) or economic theory (see Bernanke 1986). Thus, Structural VAR models recover some issues of the Cowles-Haavelmo approach. In fact, VAR models appear to be very widely used among very diverse economic schools.

Let us consider the questions raised at the end of section 3. 1. In the Sims's approach, macroeconomic relations are not stable under intervention, since they cannot be exploited by policymakers (as in Lucas). Is there something in the relations between shocks and variables, which has a causal attribute or at least distinguishes such relations from empirical regularities? Theoretically yes, but the 'true'

[1] In this case Γ_0 can be easily obtained via the Choleski factorization of the covariance matrix of the estimated residuals Σ_ε.

economic shocks, according to Sims and many VAR practitioners, are not identifiable from data alone. Yet, when many *a priori* assumptions are used, the flaws that have been made in previous large scale macroeconomic models would be repeated.

Therefore, VAR models have often been deemed as more useful tools more for forecasting, than for explaining causal relations (see Stock and Watson 2001). Structural analysis is limited to the measurement of the effects of the shocks calculating impulse response functions, which are the partial derivatives of each variable with respect to each shock. However, since the impulse response function depends on the way in which the structural shocks have been derived from the reduced form, the problem of identification reappears.

In the context of VAR models it is rather straightforward to set tests of Granger-causality, which has been mentioned for the rational expectations models. But Granger-causality is more similar to a measure of incremental predictability than to a measure of causality, as it has been pointed out by several works (see Leamer 1985 and Hoover 2001).

In philosophical terms, the original VAR program proposed by Sims (1980) is more oriented towards an instrumentalist and reductionist position. It tends to reflect instrumentalism because the identification of real causal structures is not pursued. The main focus is prediction or the collection of stylized facts to be confronted with theories, but without believing that econometric tools capture a structure which is not reducible to statistical regularities.[1] It tends to reflect reductionism, in the sense that causal relations are reduced to non-causal facts (statistical properties).

Nonetheless, the developments of Structural VAR (Bernanke 1986, Blanchard and Watson 1986, Sims 1986), to which Sims himself contributes, are an attempt to give a more realist flavour to the study of the structural shocks and the impulse response functions: the goal is to identify the actual contemporaneous causal structure among the variables entering the VAR model, using a priori restrictions derived from economic theory. This would permit, according to these authors, a meaningful interpretation of the structural shocks, which are intended to mirror real economic phenomena. Unfortunately, such interpretation depends upon the reliability of the theoretical restrictions, which is often questionable. Recent works on Structural

[1] Moreover, there is a somewhat circular reasoning in some Structural VAR econometric practices, as argued by Uhlig 1999: to obtain such stylized facts several alternative theoretical restrictions are used and the criterion of choice among them often turns out to be the conforming of the empirical results to the accepted background theoretical knowledge.

VAR (Awokuse and Bessler 2003, Demiralp and Hoover 2003, Moneta 2003 and 2004b, Moneta and Spirtes 2005, Swanson and Granger 1997), maintain a realist orientation, while attempting to identify the actual causal structure among the VAR contemporaneous variables. These authors, however, apply methods which do not use restrictions derived from economic theory. Indeed, they apply graphical methods for causal inference developed by Spirtes *et alii* 2000 and Pearl 2000, which permit the identification of several features of the contemporaneous causal structure moving from the analysis of the partial correlations between estimated VAR residuals. *A priori* assumptions, however, are also crucial here. They concern the nature of the relationship between causation and probability. Indeed it is assumed, roughly speaking, that all the partial correlations in a set of variables are generated by a stable causal structure (for more details see Pearl 2000, 63-64, and Spirtes *et alii* 2000, 32-42). This implies the *a priori* assumption that a contemporaneous causal structure in a VAR model is invariant in a certain time window.

4. Tensions and Partial Solutions

In the three approaches considered in the previous section, causality is strictly related to autonomy or stability, in the sense that the causal notion that economists have in mind depends on what they believe remains stable in the economy. From the developments of econometrics considered throughout this paper, it seems that the entities which remain stable over time and under interventions are not the same for Keynes as for Haavelmo, Lucas and Sims. Keynes claims that there are «tendencies» in the economy, but he is very sceptical about the possibility of isolating causal relations using statistics, as pointed out in section 3. 2. This scepticism is opposed by Haavelmo, who sees the possibility of isolating autonomous relations among aggregate macroeconomic variables using simultaneous equations models (see section 3. 2.). In the Cowles Commission approach the structural model represents invariant (under hypothetical modifications) relationships. According to Lucas, however, the relationships among aggregate macroeconomic variables estimated by the Cowles Commission method are not stable. What remains stable are, instead, relationships based on the rational and intentional behaviour of individual agents in the economy (see section 3. 3.). Sims accepts the issues raised by Lucas, but he proposes to give up the task of isolating stable relationships among macroeconomic variables (even if micro-founded). Instead, he seeks to isolate the exogenous shocks to the economy and their dynamic effect on the variables of interest (see section 3. 4.). Thus, what remains stable for Sims are merely the probability distributions that the agents (and the econometrician) form over the innovations affecting the economy.

Consistent with this shift of which relationship remains stable in an econometric model, the development of macroeconometrics has brought about a change in the scope of the concept of causality. In the Cowles Commission approach, causality is derived in a large extent from theory and provides the structure of a model. In the use of Granger-causality tests in the framework of VAR models, causality is derived from the statistical properties of the time series and is reducible to the concept of predictability.

From such developments of econometrics there emerge three methodological tensions, which explain the problem of this concept, but also its importance in macroeconometrics. The first tension is reflected by the uneasy relation between model and reality. An extreme form of realism about causality, such as the claims that causal relationships are independent of any human mind, and they are completely prior to regularities, is hardly defensible in macroeconomics. This is because, first, causal relationships in economics depend on the agents' decisions and representations. Second, causes relevant to economics are strictly connected to regularities and drawing the boundary between causes and regularities (or deciding what is primary) is not always an easy task. Yet, the existence of causal powers, social influences and mechanisms (e.g., production) is something which is not reducible to our experience of regularities. To put it loosely, sometimes we know that some causal mechanisms exist in the economy prior to regularities, because we have built them. Moreover, the goal of macroeconomics seems to be not completely reducible to the task of forecasting. Economists are also interested in providing explanation of the causal powers governing the economy and having the capacity to manipulate these powers. Thus, a strong form of antirealism is also hardly defensible in macroeconomics.

Even if we believe that causes do exist in the economy, however, the task of identifying and measuring them provokes a tension between causes in the model and causes in the reality. Indeed, the inference of causal relationships from statistical properties alone is very controversial. The development of the problem of identification in econometrics appears to show (see in particular the literature on Structural VAR, mentioned in section 3. 4.) that, in order to infer causes from data, one needs a commitment about what remains constant under change. In other words, one needs a «background of causal knowledge to begin with» (Cartwright 1989, 39). Even using a datadriven method, the econometrician is compelled to begin based on some background knowledge about the possible causal structure. Such background knowledge, which dictates restrictions to be imposed on the model, is often conceived as a useful tool for obtaining identification, rather than as a set of true and reliable assumptions. In other words, in an identified econometric model, it is often difficult to distinguish between causal relationships that are just properties of the model and causal relationships that are also properties of the reality. A possible and partial solution to this tension would be to make explicit the background knowledge which constitutes the basic assumptions on which the model is built.

The second tension that emerges from the development of econometrics is between the interpretation of cause as control and the interpretation of cause as measure of incremental predictability. The definition of cause given by Granger (1969) (see section 3. 3.), which economists usually refer to with the term 'Granger-causality' rather than simply 'causality', interprets '*X* causes *Y*' as 'the value of *X* helps predict the value of *Y*'. But, as Hoover points out, also the following «definition of 'cause' is widely acknowledged»:

> *A* causes *B* if control of *A* renders *B* controllable. A causal relation, then, is one that is invariant to interventions in *A* in the sense that if someone or something can alter the value of *A* the change in *B* follows in a predictable fashion.
>
> (Hoover 1988, 173)

On the contrary, Granger claims that «controllability is a much deeper property than causality»:

> If Y causes X; it does not necessarily mean that Y can be used to control X. An example is if one observes that the editorial recommendations of the New York Times about which candidates to support causes some voters to change their votes. However, if one started controlling these editorials, and this became known, the previously observed causes may well disappear.
>
> (Granger 1980, 350)

To put it loosely, both interpretations of causality – predictability and control – contain elements which, at first glance, seem at odds with some tenets of philosophical realism. Granger's interpretation contains a reductionist element: causes are hardly distinguishable from regularities and thus are in principle reducible to them. Indeed, Leamer (1985), in criticizing, among others, Granger's interpretation of causality as incremental predictability, brings as counter-example the forecasts that regularly help to predict the weather, but do not cause it. The interpretation of cause as control, on the other hand, in his attempt to reduce causation to manipulability, takes as primitive a notion – agency, manipulation or control – that seems to contain anthropocentric and subjectivist features. Moreover, the strategy of reducing causation to manipulation or to 'bringing about an event as a result of a free action' has often been criticised in the philosophical literature as unilluminatingly circular, since the notion of 'bringing about' already presupposes a causal notion. At a first glance, an appropriate concept of causality for macroeconometrics, seems to be located somewhere between the extreme points of the interpretations of causes as predictability and causes as control.

However, Woodward (2003) provides an account of causation as manipulability, which qualifies the interpretation of cause as control, and which seems to be very appropriate for macroeconomics. He does not endorse a reductionist approach, in the sense that his strategy does not involve the typical reductionist procedure, which in this context would be, first, to take the notion of manipulation as primitive, then to elaborate such notion, and finally to reduce causation to it. On the contrary, Woodward attempts to show how causality, manipulability and invariance form a «circle of interrelated concepts», which are not reducible among each other (Woodward 2003, 27). For example, the definition of causal relation as control, quoted above from Hoover (1988), should be qualified by the class of interventions, to which the relation is invariant, according to Woodward. The notion of 'invariance', indeed, should be relativized by recognizing that «a relation can be invariant under some changes and interventions but not under others» (Woodward 2003, 243). The relationships we are interested in are those «which continue to hold under an intervention that changes its independent variables sufficiently (or in such a way) that the value of its dependent variable is predicted» by the relationships «to change under intervention» (Woodward 2003, 250).

Granger's example quoted above about editorial recommendations of the *New York Times* can be re-interpreted, using Woodward's framework, as two different classes of intervention. The first class of intervention is constituted by the editorial recommendations which influence voters. Writing an editorial with recommendations is, indeed, an act of intervention. Such intervention preserves the causal relation between 'editorials' and 'votes' intact, and 'votes' changes when 'editorials' change. The second class of intervention, of different sort, is publicly controlling the editorials (e.g., by paying the editorialists if they support a certain candidate). In this case, the causal relation between 'editorials' and 'votes' may break down, as Granger maintained.

Thus, if we want to deal with a causal concept stronger than mere predictability, and if we want to avoid a reductionist account of causality as control, it seems crucial that we define, in each concrete case, a class of interventions and a condition of invariance under such interventions. This implies that our concept of causality will be dependent on a choice about interventions (a class of interventions we are interested in) and on a background (empirical or theoretical) knowledge which tells us if such interventions will keep invariant the causal relation.

Finally, the third tension to be pointed out is the tension between causality among aggregates and microbehaviour. The prevailing idea after the Lucas critique is that the sources of causal relations among macroeconomic variables are in the decisions of individual agents. In fact it is very reasonable to conceive macroeconomic variables as the outcome of the interaction of large numbers of decisions and behaviours made by agents and institutions. The problem is that in the widespread representative agent models all the activities pursued by individuals are modeled «as the choice of one *representative* standard utility maximizing individual whose choices coincide with the aggregate choices of the heterogenous individuals» (Kirman 1992, 117). As might be expected, this approach has been strongly criticized, with a persuasive argument by Kirman (1992). In this context, the following question arises: is it possible to conceive causal relations among macroeconomic aggregates that are under-determined by microeconomic decisions? In other words, could there be relations among aggregates with some degrees of invariance with respect to microeconomic decisions?

This conception is possible only if we abandon the *methodological individualism*, according to which «explanations of social, political, or economic phenomena can only be regarded as adequate if they run in terms of the beliefs, attitudes, and decisions of individuals» (Blaug 1992, 44). Arguments against methodological individualism and the strictly connected *program of microfoundations* are pursued by Hoover (2001), who attempts to show that macroeconomics is a suitable subject for a realist causal account. Thus, he claims that the macro-level is characterized differently from the micro-level, and that the former is not reducible to the latter. In his words, «macroeconomic aggregates *supervene* upon microeconomic reality»:

> What this means is that even though macroeconomics cannot be reduced to microeconomics, if two parallel worlds possessed exactly the same configurations of microeconomic or individual economic elements, they would also possess exactly the same configuration of macroeconomic elements. It is not the case, however, that the same configuration of macroeconomic elements implies the same configuration of microeconomic elements.
>
> (Hoover 2001, 120)

It is also possible to claim that not only the macro-level is supervenient upon the micro-level, but also that the former is *emergent* upon the latter. 'Supervenience' describes a situation in which identity at the micro-level guarantees identity at the macro-level. The concept of 'emergence', on the contrary, suggests that identical or near-identical (identical except for tiny and seemingly insignificant differences) configurations and interactions of micro-level components can yield different outcomes at the macro-level. An emergent property may be defined as a feature of a complex system in the manner that:

i. it can in principle be described in terms of entities at the macro-level, without references to the attributes of the micro-level,

ii. it is governed by a dynamics, whose description requires a time scale which is not appropriate for describing the underlying micro-interactions,

iii. it is not explicable entirely in terms of properties of elements at the micro-level (adapted from Lane 1993, 91, and Hodgson 1998, 157).

It appears that the argument of the emergence of structures among macroeconomic aggregates deserves further investigation. Indeed some criticisms to the representative agent models claim that «it is methodologically acceptable to theorize about and model aggregate behaviour directly, without requiring *all* behavioural specifications to be derived from atomistic individual behaviours» (Martel 1996).

If we allow that macroeconomic relations may be invariant to changes in microeconomic structure (the differences between supervenience and emergence notwithstanding), this idea should influence the way according to which we infer causes in the practice of macroeconometrics. It has been argued above that, in order to infer causes, some prior knowledge of a structure, that describes the invariants and limits the set of the possible causes, seems to be necessary. In order to build up such a structure, two kinds of questions should be considered.

i. How do structures emerge from the complex mechanism of aggregation?

ii. What are the statistical properties of stable structures?

These open questions may be considered as avenues of further research. Let us note that these questions and the issues that emerged from the methodological tensions that have been underlined will be faced in a more effective way, if logical tools are available, jointly with statistical techniques.[1]

[1] For this direction of research see Moneta 2004a.

5. Conclusions

Causality is a complex philosophical concept, because it involves not only epistemological, but also conceptual and ontological problems. Indeed, between the second half of the nineteenth century and the beginning of the past century, scientists and philosophers like Mach and Russell have proposed its elimination from the scientific discourse. However, no matter how much the word 'cause' has been used by econometricians, the problem of identification is subject to a causal interpretation. The problem of identification concerns one of the main tasks of the economist, that is understanding the mechanisms which produce the values of the observed macroeconomic variables. Since these variables show some regularities, it seems a natural assumption that there is an underlying structure which generated the observational data. The problem of identification involves determining a set of equations that describe such structure. In philosophical theories of causation of the last decades, especially those based on probabilistic accounts, much of the effort has been dedicated to the introduction of some asymmetric attributes (derived from time order, theoretical structures, interventions, counterfactuals, etc.) in the definition of causation to distinguish it from mere correlation. In macroeconometrics we have an analogous problem: the mechanisms or structures of the economy generate regularities. These mechanisms have some asymmetric attributes that distinguish them from empirical regularities. Keynes, Haavelmo, Lucas and Sims express different views among each other about the reality of these stable structures. The change of method for causal inference from the Cowles Commission to Sims and Granger has been interpreted as a consequence of the views about what remains stable in the economy.

Three methodological tensions emerge from this development in econometrics. The first is the uneasy relation between model and reality. Are identification and causality properties of the model or properties of the world? A prevalent view underlying much of the econometric practice, especially after Sims (1980), is that we should think about an approximate structure which has generated the data as a useful tool to interpret the data and to make predictions. It has been argued that, in the context of macroeconometrics, extreme forms of realism and reductionism are hardly defensible. A partial solution of the tension between properties of the model and properties of the reality would be to make clearly explicit the background assumptions from which the model is built.

The second tension exists regarding the relation between the interpretation of cause as something we can control (more prevalent in the Cowles Commission approach) and the interpretation of cause as something that improves our predictions (Granger causality). The former interpretation invokes a concept that can be used in economic evaluation. The latter interpretation invokes a concept that can be tested. We have defended, appealing to Woodward 2003, an interpretation of cause as manipulation, qualified by making explicit the class of interventions to which the causal relation is invariant.

The third tension that exists is related to the differentiation of causality among aggregates versus microbehaviours. In the developments of macroeconometrics the view that prevails is the one that claims that we should stick with microeconomic behaviours if we want to model stable relations. This paper explored the possibility of alternative methodologies by discussing views which consider macroeconomic aggregates as structures which are not reducible to microeconomic behaviours. These views may also influence the method according to which we seek causes at the macro-level. An interesting stream of future research in macroeconometrics could be to conceive methods for causal inference which consider how macroeconomic structures emerge from the complex mechanism of aggregation.

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