Probability in the Sciences

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CONTENTS

Mediating between causes and probabilities: the use of graphical models in econometrics

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ABSTRACT. The development of macro-econometrics has been persistently fraught with a tension between "deductivist" and "inductivist" approaches to causal inference. The former conceives causes as something that economic theory must provide and that statistical methods must measure. The latter opens the possibility of inferring causes from statistical properties of the data alone. I argue that these conceptions can be interpreted as two opposite responses to the problem of under-determination of theoretical causal relations by statistical properties (the problem of identification). Econometrics offers a clear example as to how the general problem of causal inference can be solved only by delicately mediating between background knowledge and the statistical properties of the data. I show how graphical causal models, appropriately interpreted, can serve this purpose.

1 Introduction

Econometrics represents a privileged locus for studying both the problem of causal inference from observational data and for verifying to what extent philosophical theories about causation apply to special sciences. Indeed, many well-studied problems in the philosophy of science—such as that the problem of underdetermination of causal theoretical models by data, of identifying causal relationships that are invariant under intervention, of differentiating between causation and correlation—have all been rigorously addressed by econometricians. The aim of this paper is to show that the debate about causal inference in econometrics contains some useful lessons for the philosophy of science.[1]

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[1] The focus of this paper is on macro-econometrics, which originally coincided with econometrics itself, but which should now be distinguished by the parallel discipline of micro-econometrics, that is econometrics applied to microeconomic data. This is also a discipline in which the problem of causal inference is crucial, but the nature of data is quite different between the two sub-disciplines. While micro-econometrics deals with cross-

The development of methods for causal inference in macro-econometrics has been fraught with a tension between what I call a "deductivist" approach and an "inductivist" approach. The first conceives of causes as something that economic theory must provide and that statistical methods must measure. The second considers economic theory a not very reliable source of causal knowledge and opens the possibility of inferring causes from statistical properties of the data. "Without pretending to have too much a priori theory" (Sargent and Sims, 1977). The first conception was advocated by some exponents of the Cowles Commission during 1950s and is fashionable among the calibration approach to econometrics. The second conception was formalised by Granger's (1969) test of causality and by Sims (1980) vector autoregressive models, methods which are still very popular in econometrics.

These conceptions can be interpreted as two opposite solutions to the same problem of underdetermination of theoretical causal relations. In econometrics this is called the problem of identification. This first approach risks the commitment to an apriorist strategy, while the second approach is impeded by the well-known difficulties of the probabilistic theories of causality. I argue that econometrics offers a clear example of how only a delicate mediation between background knowledge and the statistical properties of data can solve the general problem of causal inference. The methods for this careful handling are much dependent upon the discipline considered.

With respect to macro-econometrics, graphical models, that is the methods for causal inference developed by Pearl (2000) and Spirtes et al. (2000), can be very useful in mediating between probabilistic and causal knowledge. Indeed, graphical models permit us to take into account the maximum amount of probabilistic information (partial correlations of all possible orders), which can be used to exclude false causal relations. Partial correlations, however, are never sufficient to isolate the unique true causal relations, except in very exceptional circumstances. Indeed, background knowledge always has to be incorporated and this approach permits the use of background causal knowledge in a very efficient way.

In the next section I consider the tension associated with the problem of causal inference in macro-econometrics; in the third section I discuss how the use of graphical models can mediate such tension; in the fourth section I present an empirical example that shows how graphical models can perform that task; the fifth section concludes.

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section or panel data, macro-econometrics prevalently deals with time series variables in which experiments (or quasi-experiments) are not feasible.

2 Causal inference in macro-econometrics

The *Econometric Society* was founded in 1933 with the aim of unifying two approaches to economic problems that divided (and perhaps still divide) economists into those devoted to economic theory without measurement and those devoted to develop formalised theory without measurement and those devoted to develop measurement without theory. In fact, although Frisch (1933, p. 2) advocated a "mutual penetration of quantitative economic theory and statistical observation," it is possible to identify a similar tension inside econometrics itself. This is the tension between econometrics as an instrument of empirical application of theory and econometrics as an instrument of discovery of theoretical economic relationships. It is also reflected on the debate on causal inference in macro-econometrics.

The basic ingredients of any econometric study are data and models. The role of an econometric model, which is usually an algebraic model, is to abstract particular features of the world by means of a system of equations (Intriligator, 1983). An actual process or phenomenon is represented by the model for the sake of forecasting, explanation (understanding), and intervention. For each of these purposes econometricians have implicitly considered and sometimes made explicit a notion of causation. For example, if macroeconomists use the model to advise policymakers, they are looking for causal relations invariant under intervention. If they are just seeking macroeconomic forecasts, perhaps a weaker notion of causation is sufficient. There are, in other words, different ontological conceptions of causation involved in macro-econometrics, but I am not facing this issue here (the reader is referred to Moneta 2005). The focus here is on the different epistemological strategies for causal inference.

The typical macroeconometric model consists of a system of equations involving a number of *endogenous* variables (whose values depend upon the values of the other variables in the model), *exogenous* variables (whose values are determined outside the system but which influence it by affecting the values of the endogenous variables), and random shocks (which account for the omission of relevant variables, specification and measurement errors, etc.). The idea is to use the data to estimate (or *fit*) the model. The typical linear macroeconometric model takes the following form:[2]

(1) $A_0 Y_t + A_1 Y_{t-1} + \ldots + A_m Y_{t-m} + B_0 X_t + B_1 X_{t-1} + \ldots + B_n X_{t-n} = \epsilon_t,$

where Y_t is a $(l \times 1)$ vector of endogenous variables, X_t is a $(k \times 1)$ vector of exogenous variables, and ϵ_t is a vector of stochastic disturbances. The matrices A_i's are each $(l \times l)$; the B_j's are $(l \times k)$. The vector ϵ_t is a white

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[2]See, for example, Intriligator (1983, pp. 187–195). A more complicated model would be one with shocks entering in the equation with lags, or a non-linear model. But this would not change the substance of the present discussion.

noise, which means that is serially uncorrelated with a mean of zero and variance-covariance matrix Σ_ϵ. Moreover, by definition of exogeneity, X_t is uncorrelated with ϵ_s for every t and s.

The model (1) is a system of l equations (equal to the number of endogenous variables) in which the relationships are interpreted as causal and invariant under intervention for the sake of policy evaluation. It can easily be normalised so that one is able to write each equation which specifies one endogenous variable as a function of other endogenous variables, exogenous variables, and a stochastic disturbance term, with a unique such endogenous variable for each equation:

$$y_{1t} = f_1(y_{2t}, \ldots, y_{lt}, y_{1(t-1)}, \ldots, y_{l(t-m)}, x_{1t}, \ldots, x_{l(t-m)}, \epsilon_{1t})$$
$$y_{2t} = f_2(y_{1t}, y_{3t}, \ldots, y_{lt}, y_{1(t-1)}, \ldots, y_{l(t-m)}, x_{1t}, \ldots, x_{l(t-m)}, \epsilon_{2t})$$
$$\ldots$$
$$y_{lt} = f_l(y_{1t}, \ldots, y_{lt}, y_{1(t-1)}, \ldots, y_{l(t-m)}, x_{1t}, \ldots, x_{l(t-m)}, \epsilon_{lt})$$

This normalised system of equations, however, cannot be estimated via ordinary least squares regression, because the ϵ's are in general correlated with some of the endogenous variables entering in the equation, since some right hand side variables can be caused by some left hand side variables.

In general, the structural model (1) can be solved for Y_t in terms of lagged Y's, X's, and current ϵ's. Multiplying (1) by A_0^{-1} and solving for Y_t yields

(2) $Y_t = -A_0^{-1}A_1Y_{t-1} - \ldots - A_0^{-1}A_mY_{t-m} - A_0^{-1}B_0X_t - \ldots - A_0^{-1}B_nX_{t-n} + A_0^{-1}\epsilon_t.$

Introducing the matrices $P_i = -A_0^{-1}A_i$ and $Q_i = -A_0^{-1}B_i$, and the vector of disturbances $u_t = A_0^{-1}\epsilon_t$, equation (2) can be re-written

(3) $Y_t = P_1Y_{t-1} + \ldots + P_mY_{t-m} + Q_0X_t + \ldots + Q_nX_t + u_t.$

Equation (3) is called the *reduced form* and can be estimated consistently using least squares regression, because it is clear that the left hand side variables cannot cause anyone of the right side variables, which are either exogenous or lagged variables (and it is assumed that the future cannot cause the past).

In general, it is not possible to deduce the estimates of the structural parameters A's and B's from the estimates of P's and Q's, because there are infinitely many matrices like A's and B's which are compatible with a single set of P's and Q's. This is what econometricians call the *problem of identification*. It corresponds to what philosophers of science call "the problem of under-determination of theory by data": any theory that makes reference to unobservable features of the world will always encounter rival theories

incompatible with the original theory but equally compatible with the currently available data. This problem is particularly relevant in econometrics for two reasons. First, theoretical relations in economics are always approximate and "the error in approximation constitutes an auxiliary hypothesis of typically unknown dimension" (Sawyer et al., 1997, p. 21). Second, and crucially connected with the topic of this chapter, econometricians try to confirm causal relations using statistical properties (like correlations). This raises the problem of differentiating between an asymmetric relation like causation and a symmetric relation like correlation. Econometricians have reflected on this problem for a long time and indeed "[a]n important contribution of econometric thought was the formalization of the notion developed in philosophy that many different causal interpretations may be consistent with the same data" (Heckman, 2000, p. 47).[3]

2.1 Deductivist Approaches

Haavelmo (1944) presents some algebraic conditions that a system of equations like (1) must satisfy to be identifiable. These conditions refer to the number of endogenous variables relative to the number of exogenous variables ("order condition for identification") and to the rank of the reduced form matrix P's and Q's ("rank condition for identification"). I am not going to go into details of these conditions: it is important here to highlight the fact that structural parameters (coefficients of equation 1) are identified by the imposition of several types of a priori restrictions on the A's and B's. In the so-called Cowles Commission approach to econometrics, of which Haavelmo was one of the founders, these restrictions consist in a priori setting of many of the elements of A's and B's (in equation 1) to zero, and in a priori classifying of variables as exogenous and endogenous (considering the fact that a relatively high number of exogenous variables aids identification.). It is important to notice that these restrictions correspond to causal auxiliary hypotheses. Indeed, setting a priori some structural coefficients to zero in equation (1) corresponds to a priori assuming that a particular variable is not causally influencing a particular endogenous variable. Moreover, a priori assuming that a particular variable is exogenous corresponds to a priori assuming that that variable is not causally influenced by any other variable in the system.

The solution pursued by the Haavelmo-Cowles program was that these causal restrictions had to be derived from economic theory. The theory in consideration was Keynes's macroeconomics, but filtered by the neoclassi-

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[3] However, the econometric literature on structural analysis and the problem of identification was in part anticipated by the work of the geneticist Sewall Wright on path analysis in the 1920s.

cal synthesis, which introduced the Walrasian notion of general equilibrium. However, the object of the Haavelmo-Cowles program was more general: it was not explicitly specified which theory one had to use in order to get restrictions. The crucial issue was that restrictions had to be derived from economic theory. Once the model was identified, it could be estimated using sound statistical methods and tested against the empirical evidence. But both the problem of confirming theoretical causal models and of choosing between competing models are not central issues in the Cowles Commission methodology. Statistical techniques such as regression analysis are mainly designed to estimate the importance of each causal factor that is dictated by economic theory, and only to a lesser degree to perform empirical validation. However, as Hoover (1994 and 2006) points out, the Cowles Commission methodology is subject to alternative interpretations. Koopmans in his debate with Vining about the possibility (denied by Koopmans) of "measurement without theory" demonstrated what Hoover (1994) calls a strong apriorist view. This corresponds to considering theory prior to data and to denying the possibility of interpreting data without theoretical presuppositions. According to this view, econometric models have to be built imposing restrictions derived from a well-articulated theory accepted a priori. Thus, the object of econometrics would be one of measurement of causal relationships and not of validation or discovery of causal hypotheses. This view would correspond to a very strong interpretation of the under-determination thesis denying the possibility of any induction from correlations to causation. But the problem with Koopmans' position is, as argued by Hoover (2006, p. 74), that it "places the empiricist in a vicious circle: how do we obtain empirically justified theory if empirical observation can only take place on the supposition of a true background theory?"

The position of Haavelmo, however, was quite different from Koopmans's one. Although also Haavelmo maintained that empirical investigations were to be founded on a priori theoretical restrictions, he favored statistical testing of causal hypotheses. Thus he endorsed a view of econometrics, called by Hoover (1994) weak apriorism, which recognizes the need for an interplay of theoretical models with empirical results. This permits one to partially avoid the danger, implicit in the strong apriorist view, of being committed to a set of a priori causal assumptions without having the possibility of empirically confirm them.

Lucas's (1976) article, "Econometric Policy Evaluation: A Critique," is a crucial step in the development of causal inference in econometrics. In fact, the Lucas critique was an attack more directed to the economic theory commonly used to derive the a priory restrictions necessary for the identification of the model, than the general Haavelmo-Cowles methodology for causal in-

ference. However, the research program pursued by Lucas with his critique, which shaped the basis of the "New Classical Macroeconomics," yielded new and alternative econometric methodologies for causal inference. The point raised by the Lucas critique was, in few words, the following: large-scale econometric models based on the Cowles Commission methodology and using restrictions derived by Keynesian macroeconomic theory (filtered by the neoclassical synthesis) could not be used for policy evaluation. This is because the estimated coefficients of such models were unlikely to remain invariant to the policy interventions that are object of evaluation. In other words, the causal relationships identified by the Haavelmo-Cowles methodology were not invariant under intervention, according to Lucas. They were not invariant, or stable, because the standard macroeconometric models inspired by the Haavelmo-Cowles approach did not take into account the fact that people have forward-looking behaviour (rational expectations) which prompts them to change behaviour as soon as the intervention takes place, in order to take advantage of the new policy regime associated with the intervention.

Thus, the object of Lucas's attack was not the general deductivist approach in which causal relations are identified in the Cowles Commission methodology, but the lack of foundation of the a priori theoretical restrictions used to identify the models. Moving from this criticism, Lucas focused on micro-founded theoretical assumptions that were able, in his view, to dictate structural (causal) relations invariant to changes in policy. The first assumption was the rational expectations hypothesis mentioned above: individual agents have forward-looking and perfectly rational behaviour, which permits them to take the maximum advantage of the available information, without making any systematic error. The second principle was that, in line with the Walrasian tradition, markets continuously clear, so that all observed output are the results of a continuous state of (short and long-run) equilibrium. Moreover, theoretical models do not need to formalize the behaviour of every agent, but, thanks to the homogeneity of individual rationality, just the behaviour of typically one representative agent, which stands in for the behaviour of all agents. In other words, the problem of aggregation of the causal relations among microeconomic agents into causal relations among macroeconomic aggregates is simply bypassed (Moneta, 2005).

A first response to the Lucas critique was completely consistent with the Haavelmo-Cowles methodology. The idea was to supplement economic theory with the rational expectation hypothesis, from which it could be possible to derive cross-equations restrictions on the matrices A's and B's in equation (1), in order to identify the structural model (Hansen and Sargent, 1980).

Thus causal relations are inferred, once again, in a general methodological approach in which theory is prior to data. Although testing of a theoretical causal hypothesis is still pursued, the theoretical assumptions used to restrict the estimatable equations are not questioned. Therefore, this approach shares with all the forms of apriorism the problem of obstructing an empirically disciplined knowledge of causal relations.

Even more apriorist and deductivist approaches to causal inference, however, have been developed in the wake of the Lucas critique. I am referring to the calibration approach that has been developed as the method of empirical assessment of equilibrium real business cycle models (see Kydland and Prescott, 1982). But its roots are in the method proposed by Lucas (1980): "[o]ne of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost. ... Any model that is well articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently 'unreal'" (Lucas, 1980, p. 271).

A theoretical model, which can be thought as representing a set of causal relations invariant under interventions, need not fit the data according to criteria dictated by statistical theory, according to the calibration approach. Indeed, it would be easily rejected, since it is built upon very idealised assumptions that do not take into account all the contingencies, which are not related with the deep structure, whose knowledge is essential to answer a limited set of policy questions. Then, such disturbing factors, unaccounted in the model, but present in the reality, would deform parameter estimates. Thus, the model has to be calibrated, instead. A model is calibrated when its parameters are not estimated in the context of their own model, but are picked in micro-economic unrelated empirical investigations, or are chosen to guarantee that the simulated model matches some particular and unrelated features of the historical data, drawn from considerations of national accounting, etc. Once calibrated, the model is validated via simulation. The model is validated if it matches moments of the data or reproduces some stylised facts obtained by independent empirical analysis of the data. In fact, this approach seems to appeal to the sound principle that a theory is better supported when validated on information not used in the formulation (Hoover, 1995a). But the acceptance of this principle is not clear, at least in two respects. First, the collection of stylised facts through statistical analysis of data is only partially an independent exercise. Indeed the so-called stylised facts express more or less implicitly causal relations (saying, for example, that a monetary shock is neutral, which means that it does not

have any causal impact on income, in the long-run), which also need some a priori assumptions to be identified. Second, equilibrium business cycle models, for which the calibration approach has been developed, are based on the simplification of the representative agent. Thus, when the models are calibrated using parameters derived from microeconomic investigations, it is tacitly assumed that aggregation does not fundamentally alter the structure of the aggregate model. Such assumption is hardly defensible, as Forni and Lippi (1997) and Kirman (1992), among others, have shown.

Thus, the main characteristic of the calibration methodology is a strong commitment to economic theory (with the typical new classical features: general equilibrium, rational expectations, perfect aggregation), taken for granted a priori. This is a form of apriorism even stronger than Koopman's one, because it rules out likelihood-based statistical estimates of model parameters, which are standard in any version of the Cowles Commission methodology. This raises the question as to how to judge between competing calibrated models. And, more important: is there any possibility of growth of knowledge at all, if the hard core of the new classical theory, thanks to the protection of the calibration methodology, is immune from revision? (Hoover, 1995a).

To conclude, all the approaches just presented share several features in common with the hypotetico-deductive method for causal confirmation. Theoretical statements about causal relations together with the data (which can be thought as initial conditions) imply the event to be explained. Haavelmo's methodology is quite well in tune with Popper's falsificationism: a theoretical causal statement is hypothesised, from which consequences are deduced, and if these consequences do not fit the data, the theoretical causal statement is re-formulated. In fact, the Haavelmo's apparent falsificationism is beset with the problem of under-determination of theory by data, but Haavelmo, as mentioned above, recognizes the fundamental importance of the empirical testing of causal hypotheses. The other approaches, Koopmans's one and calibration in particular, represent a deductivist approach without falsificationism. The possibility of rejecting theoretical causal statements are reduced to the minimum.[4]

In general, all these approaches share the difficulties of the hypothetico-deductive approaches to causal discovery (Williamson, 2005), which amount to failing to explain how causal relationships are to be hypothesised (to what extent is economic theory a reliable source of causal hypotheses?), and to failing to explain how predictions can be reliably deduced from the causal

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[4] The strong apriorist approach corresponds very closely to a scientific research program, as defined by Lakatos, in which a large set of assumptions, constituting the *hard core*, is never confronted with the data.

statements in spite of the under-determination (identification) problem.

2.2 Inductivist Approaches

Sims' (1980) article, "Macroeconomics and Reality," pursued the criticism of traditional macroeconometric models in another direction, with respect to Lucas (1976). Sims claimed that econometricians inspired by the Cowles Commission methodology "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, 1995b, p. 6). But the reaction is different to the rational-expectations econometrics approach. While Hansen and Sargent (1980), as mentioned in the last section, continued to pursue identification of structural models, by using restrictions grounded in 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, 1995b, p. 6). The approach proposed by Sims deals with unrestricted reduced form equations, namely vector autoregressive models (VARs). Each variable is considered as endogenous and it is regressed on lagged values of itself and of all the other variables. This corresponds to the reduced form considered in equation (3), devoid of the exogenous variables X's:

(4) $\quad Y_t = P_1 Y_{t-1} + \ldots + P_m Y_{t-m} + u_t$

Once the model (4) is estimated, it is possible to study the dynamic causal effect of a single shock on each variable of Y_t. However, it is not possible to isolate the effect of a single shock u_{jt}, since u_{jt} is in general correlated with the other components of u_t. Sims (1980) proposed to orthogonalize the residuals u_t by multiplying both sides of equation (4) by a particular matrix Γ, obtained by the Choleski factorization of the covariance matrix of the residuals u_t. Indeed this is one of the most simple ways to transform equation (4) into another equation in which the shocks are orthogonal, like the following:

(5) $\quad A_0 Y_t = A_1 Y_{t-1} + \ldots + A_n Y_{t-m} + \epsilon_t$

But there are many ways of obtaining equation (5), and the one with A_0 is just a particular case. In other words, the problem of identification reappears. Indeed, the transformation of equation (4) in another equation in which residuals are orthogonal—residuals orthogonalization, in short—is equivalent to imposing a contemporaneous causal structure on the variables (Stock and Watson, 2001). The method of orthogonalization proposed

by Sims (1980), mentioned above, corresponds to impose on the system a strictly recursive causal structure among the contemporaneous variables.[5]

Sims' method is atheoretical and inductivist: the idea is to impose the most common and most simple causal structure in order to obtain identification, and to learn causal relationships directly from data. The causal relationships that are objects of interest in this method are the relationships between exogenous shocks and the components of Y_t at any lead and lag, and not the relationships among the components of Y_t as in the Haavelmo-Cowles framework. But Sims (1980) solution to the VAR identification problem is highly arbitrary because he picks up a very special causal structure (the recursive causal ordering) among a very big number ($l!$) of possible causal structures.

The so-called structural VAR literature recognizes this arbitrariness and focuses its efforts on the imposition of restrictions on the contemporaneous causal structure, derived, entirely consistently with the Cowles Commission methodology, from economic theory or institutional knowledge. However, it is not clear to what extent the restrictions suggested by economic theory are reliable. Thus, the structural VAR approach recovers some issues of the deductivist methodology, included some of its problems. In the next section I will show how such problems can be faced using graphical causal models. In general, the VAR approach is atheoretical, in the sense of letting the data speak as much as possible, and so at odds with an apriorist methodology. But the problems of the other approach are replicated here in a new form: since measurement without theory (because of the under-determination problem mentioned above) is very a difficult task, strong a priori assumptions turn out to be hidden behind implicit (but often arbitrary) assumptions.

In this general framework Granger's conception of causality has flourished. Granger (1969, 1980) defined causal relationships in the following way: a time series variable x_t causes *prima facie* another time series variable y_t if the probability of y_t conditioned on its own past values and the past values of x_t (besides the set Ω of the relevant information) does not equal the probability of y_t conditional on its own past history alone (and Ω). More formally, x_t Granger-causes y_t if and only if:

(6) $\quad P(y_t|y_{t-1}, y_{t-2}, \ldots, x_{t-1}, x_{t-2}, \ldots, \Omega) \neq P(y_t|y_{t-1}, y_{t-2}, \ldots, \Omega)$.

The intuition behind this definition is that x_t renders y_t more likely, or, in a more epistemological sense, x_t contains some special information which

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[5]With strictly recursive causal structure, I mean a causal chain among the components of Y, according to which the only causal connections are: y_{1t} causes y_{2t}, y_{2t} causes y_{3t}, ... y_{n-1} causes y_t.

helps predict y_t. Indeed, another way of reading (6) is that x_t Granger-causes y_t if the knowledge of the past and present values of x_t contributes to forecasting y_t. Based on this idea and definition, Granger was able to devise very simple tests of this conception of causality. Indeed, the "incremental predictability" of a variable is easily measured as a reduction of the variance of the prediction error. In the VAR framework it is straightforward to test the absence of Granger causality. In order to test the Granger *non*-causality from y_{jt} to y_{it}, it is sufficient to test that the (j, i) entries of the matrices from $P_1, \ldots, P_m$ in equation (4) are significantly close to zero.

In fact, Granger causality was devised before the formulation of the VAR approach. Moreover, Hansen and Sargent (1980) claimed that Granger causality played a "natural role" in rational expectations models. Nevertheless, the methodological approach behind Granger causality is extremely inductivist and is well in tune with the VAR framework.

The closeness of Granger causality with probabilistic theories of causality developed in the philosophy of science is evident. In particular, Spohn (1984) highlights the closeness with Suppes (1970) account. Indeed, Granger causality shares with any other probabilistic account of causality all its difficulties, well studied in the philosophy of science. First, merely probabilistic accounts are not able to identify causation as an asymmetric relation. This is because if A renders B more likely ($P(B|A) > P(B)$), the probability calculus implies that also B renders A more likely ($P(A|B) > P(A)$). Granger causes must temporally precede the effect. But this is not sufficient to solve the second difficulty: mere probabilistic accounts are not able to distinguish between statistical association and direct causation. The typical example is that the barometer helps predict the weather, but is not causing it. This problem can be solved assuming a common cause (e.g. pressure) which is causing both the barometer index and the weather, but how does one know that all possible common causes are included in the set Ω? Thus, unless one can appeal to some background knowledge of the causal structure, the dependence on the set Ω of all relevant information makes the concept of Granger causality non operational.

In sum, the VAR approach and Granger causality share all the difficulties of the inductivist approaches to causal learning: either they are not able to identify a causal structure under-determined by the statistical properties of the data (that is, there may be other causal structures observationally equivalent), or they are able to do that with implicit background assumptions, which are typically not validated.

3 Graphical models

The deductivist and inductivist approaches can be thought as two opposite responses to the problem of under-determination or problem of identification. The risk of the first approach is the commitment to an apriorist strategy, while the second approach is impeded by the typical difficulties of the probabilistic theories of causality. I suggest that econometrics offers a clear example as to how the general problem of causal inference can be solved only by delicately mediating between background knowledge and statistical properties of the data. I want to argue that graphical causal models can be helpful for this purpose.[6]

Graphical causal models developed by Pearl (2000) and Spirtes et al. (2000) are a suitable tool for the task of mediating between causes and probabilities. These techniques (I refer in particular to Spirtes et al. (2000)) have been shown to be very useful to infer partial information about causal structures from observational data. A graphical causal model consists of a graph[7] whose vertices are random variables with a joint probability distribution subject to some restrictions. The graph is given a causal interpretation (a directed link from A to B means that A causes B) and in many cases it is assumed that the graph is a directed acyclic graph (DAG), excluding feedbacks and loops. In the DAG case, the restriction on the probability distribution is the *causal Markov condition*, which limits the pairing of DAGs and probabilities: each variable is independent of its graphical non-descendants given its graphical parents. A second assumption that is made for the sake of causal discovery is the *faithfulness condition*: all of the conditional independence relations in the probability distribution follow from the causal Markov condition. Based upon these two conditions, Spirtes et al. (2000) provide some algorithms (operationalised in a computer program called TETRAD) that identify the causal graph which has generated the data from tests on conditional independence relationships. Often this graph is not a unique DAG, but a set of Markov equivalent DAGs, i.e., a set of graphs which share the same conditional independence relations among the variables. Variants of these algorithms are given for environments where the possibility of latent variables is allowed (Spirtes et al., 2000, chap. 6). Richardson and Spirtes (1999) extend the procedure to situations involving

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[6] I am not denying, however, that there are other econometric approaches that also perform very well in the task of mediating between deductivist and inductivist approaches. I am referring in particular to the London School of Economics approach to econometrics (Hendry, 1995) and to the "extreme bound analysis" of Leamer (1983). But in these two approaches the issue of causal inference is not as central as in graphical models.

[7] A graph can be thought as a pair (V, E), where V is a nonempty set of *vertices*, and E is a subset of the set $V \times V$ of ordered pair of vertices, called *edges*. For a more detailed graphical model terminology see Spirtes et al. (2000, p. 5-17).

cycles and feedbacks.

This opens up the possibility of a logic of scientific discovery, which was explicitly denied in the philosophy of science for many years, from Hempel onwards. However, I want to argue here that graphical models need not to be interpreted or used as instruments of pure inductive learning. To begin with, both the causal Markov and the faithfulness condition should be taken with caution, because, although in general statistical models for social sciences with a causal significance satisfy these conditions (Spirtes et al., 2000, p. 29), there are still several environments in which these conditions are usually violated.

In general, the causal Markov condition does not hold if variables relevant to the causal structure are not included in the set V of the vertices (although it is possible to test for latent variables), if probabilistic dependencies are drawn from non-homogenous populations, if variables are not properly distinct from one another, or if causality cannot be assumed to be local in time and space (for example in quantum mechanical experiments). In macroeconomics the problem is compounded by the problem of aggregation: causal structures may be effective at a low level of aggregation (at the micro level), but variables are measured at a high level of aggregation (at the macro level).

The faithfulness condition can also be thought as claiming that the probability distribution on V embodies only independence relations that can be represented in a causal graph (through the Markov condition), excluding independence relations that are sensitive to particular values of the parameters and vanish when such parameters are slightly modified. Pearl (2000, p. 48) calls this assumption *stability*, because it corresponds to assuming that all the independence relations remain invariant when the parameter values change. This means that external influence (exogenous shocks) will tend to change parameter values and not the causal structures (from which all the independence relations derive). In economics this concept recalls Simon's (1953) characterization of causal relations as invariant under interventions, and Frisch and Haavelmo's concept of "autonomy" or "structural invariance" (Aldrich, 1989).

Thus, it is important to stress the fact that causal Markov and faithfulness are *a priori* assumptions. In a macroeconometric framework, causal Markov and faithfulness condition should be taken as working assumptions. Indeed, it is important to be aware that the results may depend on the choice of variables, level of aggregation and presence of structural changes. Econometric tests are available for many of these specification issues (AIC criterion, Chow test, *etc.*) and should be taken into account before applying the algorithm. In other words, graphical causal models should be based on

background knowledge based on independent statistical techniques (besides theoretical knowledge). Graphical models permit one to take into account the maximum amount of probabilistic information (partial correlations of all possible orders), which can be used to exclude false causal relations. Partial correlations, however, are never sufficient to isolate the unique true causal relations, except in very exceptional circumstances. Background knowledge has to be incorporated and this approach permits the use of background causal knowledge in a very efficient way.

The view I am proposing here is much in the spirit of the synthetic approach proposed by Williamson (2002). As Williamson (2002, p. 10) argues, "while the causal Markov condition may fail it remains a good default assumption, in the sense that if one knows of the causal relationships amongst a set of variables, and one knows of no counterexample to the causal Markov condition amongst those variables, then one's subjective probabilities ought to satisfy the condition."

4 Graphical models and structural VARs

In this section, I show how the task of mediating between a deductivist and an inductivist approach can be put forward, through graphical models, in the special context of structural VAR. In section (2.2), I have shown how the tension between a deductivist and inductivist approach emerges again in the identification of a structural VAR. I want to show here how graphical models can be useful in mediating between an inductivist and deductivist approach to impose the restrictions to identify a structural VAR. Recall that the problem of identification, in the VAR framework, consists in recovering the structural equation

(7) $A_0 Y_t = A_1 Y_{t-1} + \ldots + A_m Y_{t-m} + \epsilon_t$

from the estimate of the reduced form equation

(8) $Y_t = P_1 Y_{t-1} + \ldots + P_m Y_{t-m} + u_t,$

where $A_0 P_j = A_j$ (for $j = 1, \ldots, m$) and $A_0 u_t = \epsilon_t$. These systems of equations can be solved only by imposing restrictions on the matrix A_0. The elements of A_0, appropriately normalised, can be thought as the coefficient of l regression equations:

$u_{1t} = \alpha_{11} u_{2t} + \ldots + \alpha_{1(l-1)} u_{lt} + \epsilon_{1t}$

$u_{2t} = \alpha_{21} u_{1t} + \ldots + \alpha_{2(l-1)} u_{lt} + \epsilon_{2t}$

...

$u_{lt} = \alpha_{l1} u_{1t} + \ldots + \alpha_{l(l-1)} u_{(l-1)t} + \epsilon_{lt},$

where some of the α's may be zero, but we do not know *a priori* which ones. But looking at the equation (7), it is straightforward to see that A_0 incorporates the structural relations, that is *caused* relations, among the contemporaneous elements of Y. Thus, there is an isomorphism between the causal relation among the residual variables $u_{1t},\cdots,u_{t}$ and the contemporaneous variable $y_{1t},\cdots,y_{t}$.

The idea of Swanson and Granger (1997), Reale and Wilson (2001), Blesser and Lee (2002), Demiralp and Hoover (2003), Moneta (2003) is to use graphical causal models to infer the causal relationships among the elements of u_t (equivalent to the causal relationships among the element of Y_t) from the estimate of vanishing partial correlations among u_t.[8] This allows the imposition of enough zero-restrictions on the elements of A_0 (i.e., on the α's) in order to get the model identified. A zero on A_0 corresponds to a lack of causality among two elements of u_t.

I will clarify this approach through an empirical example.[9] An important question in macroeconomics is which shocks are the main causes of income fluctuations. This is not only an important question per se, but it is crucial to assess a theoretical hypothesis, like, for example, the Real Business Cycle hypothesis, which claims that shocks to real variables (consumption, investment, income) are the dominant sources of income fluctuations and that shocks to nominal variables (money, interest rates) play an insignificant role in determining the long-run behaviour of real variables. To address this question, I estimate a VAR very similar to the one used by King et al. (1991). Let $Y_t = (C,I,M,Y,R,\Delta P)$, where C denotes per capita consumption, I per capita investment, M the real balances, that is the ratio between money and price level, Y per capita gross national product, R nominal interest rate, and ΔP inflation. The data are six quarterly U.S. macro variables for the period 1947:2 to 1994:1 (188 observations).

A series of specification tests (cointegration, number of lags, structural change, etc.) confirmed the possibility of a stable causal structure for these years. Thus, assuming causal Markov and faithfulness condition, a modified version of the PC algorithm incorporated in TETRAD could be applied using as input the tests on vanishing partial correlations among the elements of u_t. The resulting graph is displayed in Figure 1.

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[8]Swanson and Granger (1997) apply a technique which assumes the Markov condition, but not the Faithfulness condition; Reale and Wilson (2001) apply conditional independence graphs; Blesser and Lee (2002) and Demiralp and Hoover (2003) apply the PC algorithm incorporated in TETRAD; Moneta (2003) applies a modified version of the PC algorithm which is more severe in orienting edges.

[9]This empirical example is drawn from Moneta (2003). The reader is referred to this paper for more details.
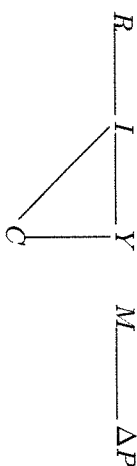
Figure 1. Output of the search algorithm.

Notice that the algorithm does not direct any causal relationship because the modification that I made to the PC algorithm rendered directing edges more severe. The set of DAGs for this pattern consists of 24 elements. Each of these 24 causal structures corresponds to overidentifying restrictions on the matrix A_0, i.e., restrictions such that the model has a number of known parameters (estimated coefficients and estimated covariance matrix) greater than the number of unknown parameters (parameters of the structural model). This constitutes an advantage with respect to the standard recursive VARs identified using the Choleski factorization of residuals covariance matrix (Sims, 1980), which are just-identified, because overidentified models can be tested using a χ^2 test statistic (see Doan, 2000). It turns out that some DAGs do not pass this test, in particular the DAGs which contain one or both of the following configurations: $R \rightarrow I \rightarrow Y$ and $R \rightarrow I \leftarrow C$. The number of DAGs ruled out is 8. Thus there are 16 DAGs left. The number of DAGs left is narrow enough to check if there are results about the effects of shocks on output (Y) fluctuations which are robust across the different specifications of the models. I will show the results imposing another *a priori* specification. Among the 16 models considered, four are consistent with the conjecture that interest rate and investment are leading indicator for output. Although this is an hypothesis which is well in tune with much economic theory and empirical stylised facts, it has not to be taken for granted: it is always possible to check whether the results change dropping this hypothesis. In Figure 2 two of the causal graphs for these four models are displayed. I call model 1 and model 3 the models corresponding to the causal graphs displayed in Figure 2. Model 2 and model 4 have causal graphs equal to model 1 and model 3, respectively, except that the causal relationship between M and ΔP runs in the opposite direction.

Figure 3 shows the calculations of the dynamic responses of output (impulse response functions) to the shocks to consumption, investment, money and interest rates for the 4 different model specifications. The results point out that not only shocks associated to real macroeconomic variables (out-
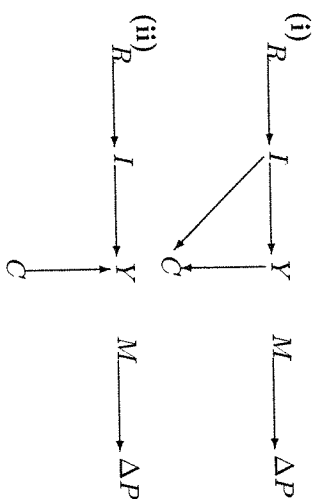
put, consumption and investment) but also shocks associated to nominal variables (money, inflation and interest rates) have a considerable effect on macroeconomic fluctuations (at all frequencies). This result shows how US data are not consistent with the Real Business Cycle hypothesis, which claims that a single productivity shock is driving output fluctuations. These general results are robust across different specifications of the other 12 models.[10]

Figure 2. (i) Causal graph for model 1. (ii) Causal graph for model 3.

5 Concluding remarks

The aim of this paper was to show how graphical models can help to approach the problem of causal inference mediating between deductive and inductive learning. Indeed, these techniques are very powerful in generating causal models starting from a probability distribution, but the general assumptions which permit them to work are not innocuous. I propose to use the causal Markov and faithfulness conditions as working assumptions to be used in a certain temporal window, when empirical evidence and theoretical

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[10] These results are not reported here for limits of space. There are also several other tests that could be run to know how results are robust to change of number of lags (a main problem also in Granger-causality tests), significance level, and across sub-samples. However, a careful analysis of the epistemic virtues of robustness for each of this case has yet to be done for the methods presented in this paper. Another important issue is the exclusion of feedbacks and loops in the DAGs. This is a very useful simplification, but it is not always reliable in aggregated data. In another paper (Moneta, 2004) I have relaxed this restriction for a similar macroeconomic data set, but it remains an open question how two interpret similarities and differences between the results with and the results without the acyclicity condition.
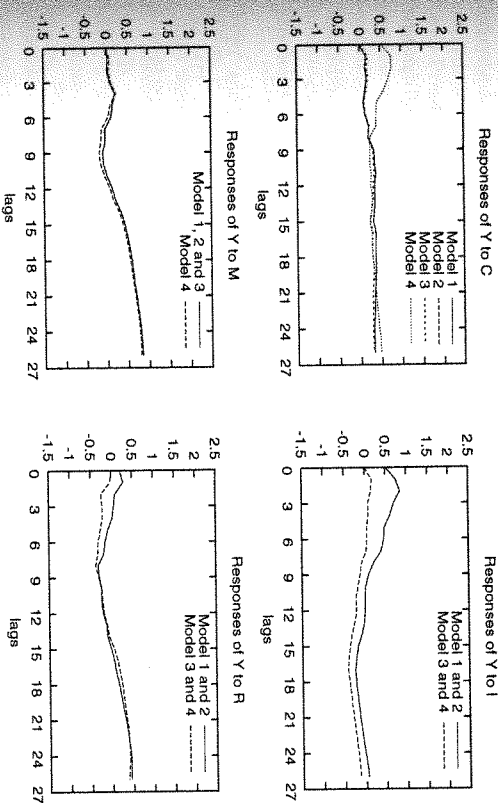
background knowledge are not at odds with the hypothesis of a stable causal structure generating the data. Thus, both the inductive stage carried over by the graphical algorithms, and the deductive stage are equally important. Thus, one should view the inferred causal structures as a set of hypotheses that has to be tested independently. Moreover, since the set of inferred causal relationships is only in exceptional cases a unique causal structure, additional information about the causal structure has to be derived from background or theoretical knowledge. The advantage of using graphical models is that they express such a priori assumptions in an explicit causal language, which helps in testing their validity.

Figure 3. Impulse response functions of income to consumption, investment, money and interest rates shocks.

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BIBLIOGRAPHY

Aldrich, J. (1989). Autonomy. *Oxford Economic Papers*, 41:15–34.

Blesser, D. and Lee, S. (2002). Money and prices: US data 1869-1914 (a study with directed graphs). *Empirical Economics*, 27:427–446.

Demiralp, S. and Hoover, K. (2003). Searching for the causal structure of a vector autoregression. *Oxford Bulletin of Economics and Statistics*, 65:745–767.

Doan, T. A. (2000). *RATS Version 5, User's Guide*. Estima, Evanston, IL.

Forni, M. and Lippi, M. (1997). *Aggregation and the microfoundations of dynamic macroeconomics*. Clarendon Press, Oxford and New York.

Frisch, R. (1933). Editorial. *Econometrica*, 1:1–4.

Granger, C. (1969). Investigating causal relations by econometric models and cross-spectral methods. *Econometrica*, 37:424–438.

Granger, C. (1980). Testing for causality: a personal viewpoint. *Journal of Economic Dynamic and Control*, 2:329–352.

Haavelmo, T. (1944). The probability approach in econometrics. *Econometrica*, 12:1–115.

Hansen, L. and Sargent, T. (1980). Formulating and estimating dynamic linear rational expectations models. In Lucas, R. and Sargent, T., editors, *Rational expectations and econometric practice*, pages 295–320. Allen & Unwin, London.

Heckman, J. (2000). Causal parameters and policy analysis in economics: a twentieth century retrospective. *Quarterly Journal of Economics*, 115:45–97.

Hendry, D. (1995). *Dynamic Econometrics*. Oxford University Press, Oxford.

Hoover, K. (1994). Econometrics as observation: the Lucas critique and the nature of econometric inference. *Journal of Economic Methodology*, 1:65–80.

Hoover, K. (1995a). Facts and artifacts: calibration and the empirical assessment of real-business-cycle models. *Oxford Economic Papers*, 47:24–44.

Hoover, K. (1995b). *Macroeconometrics, developments, tensions, and prospects*. Kluwer, Boston, Dordrecht and London.

Hoover, K. (2006). The methodology of econometrics. In Mills, T. C. and Patterson, K., editors, *Palgrave Handbook of Econometrics, vol. 1, Econometric Theory*. Palgrave Macmillan.

Intriligator, M. (1983). Economic and econometric models. In Griliches, Z. and Intriligator, M. D., editors, *Handbook of econometrics*. Elsevier Science Publishers.

King, R., Plosser, C., Stock, J., and Watson, M. (1991). Stochastic trends and economic fluctuations. *American Economic Review*, 81:819–840.

Kirman, A. (1992). Whom or what does the representative agent represent? *Journal of Economic Perspectives*, 6:117–136.

Kydland, F. and Prescott, E. (1982). Time to build and aggregate fluctuations. *Econometrica*, 50:1345–1369.

Leamer, E. (1983). Let's take the con out of econometrics. *American Economic Review*, 73:31–43.

Lucas, R. (1976). Econometric policy evaluation: a critique. In *The Phillips curve and labor markets. Carnegie Rochester Conference Series on Public Policy*. North Holland.

Lucas, R. (1980). Methods and problems in business cycle theory. In Lucas, R., editor, *Studies in business-cycle theory*. Blackwell.

Moneta, A. (2003). Graphical models for structural vector autoregressions. *LEM Working Paper, Sant'Anna School of Advanced Studies, Pisa*, 03/07.

Moneta, A. (2004). Identification of monetary policy shocks: a graphical causal approach. *Notas Económicas*, 20:39–62.

Moneta, A. (2005). Causality in macroeconometrics: some considerations about reductionism and realism. *Journal of Economic Methodology*, 12:433–453.

Pearl, J. (2000). *Causality. Models, reasoning, and inference*. Cambridge University Press, Cambridge.

Reale, M. and Wilson, G. T. (2001). Identification of vector AR models with recursive structural errors using conditional independence graphs. *Statistical Methods and Applications*, 10:49–65.

Richardson, T. and Spirtes, P. (1999). Automated discovery of linear feedback models. In Glymour, C. and Cooper, G. F., editors, *Computation, causation, and discovery*. AAAI Press and The MIT Press.

Sargent, T. and Sims, C. (1977). Business cycle modeling without pretending to have too much a priori economic theory. In *New methods in business cycle research: proceeding from a conference*, Federal Reserve Bank of Minneapolis, Minneapolis, MN.

Sawyer, K., Beed, C., and Sankey, H. (1997). Underdetermination in economics. The Duhem-Quine thesis. *Economics and Philosophy*, 13:1–23.

Simon, H. (1953). Causal ordering and identifiability. In Hood, W. C. and Koopmans, T. C., editors, *Studies in econometric methods*. Wiley.

Sims, C. (1980). Macroeconomics and reality. *Econometrica*, 48:1–47.

Spirtes, P., Glymour, C., and Scheines, R. (2000). *Causation, prediction, and search*. The MIT Press, Cambridge, MA.

Spohn, W. (1984). Probabilistic causality: from Hume to Suppes via Granger. In Galavotti, M. C. and Gambetta, G., editors, *Causalità e modelli probabilistici*. CLUEB.

Stock, J. and Watson, M. (2001). Macroeconomics and reality. *Journal of Economic Perspectives*, 15:101–115.

Suppes, P. (1970). *A probabilistic theory of causality*. North Holland, Amsterdam.

Swanson, N. and Granger, C. (1997). Impulse response functions based on a causal approach to residual orthogonalization in vector autoregressions. *Journal of the American Statistical Association*, 92:357–367.

Williamson, J. (2002). Learning causal relationships. *Technical Report, Centre for Philosophy of Natural and Social Science, London School of Economics*, 02/02.

Williamson, J. (2005). *Bayesian nets and causality. Philosophical and computational foundations*. Oxford University Press, Oxford.