

## Algorithms of Inductive Causation, DAGs, and Econometrics

As a graduate student at California in the mid 1970's my greatest disappointment in my study was with the way we tried to force the data to reveal what we "knew" to be a "true" *a priori* theory. In almost all cases of empirical study we used observational data to estimate the basic demand and or supply parameters as specified via this *a priori* theory. Of course the theory was that generated from the assumption of maximizing behavior as laid out in several places, two most prominent being, Paul Samuelson's *Foundations of Economic Analysis*, (Cambridge: Harvard 1948) and John Hicks' *Value and Capital* (Oxford, Clarendon, 1946). While that theory is essentially an exercise in *ceteris paribus* reasoning, it was more or less adapted and made to fit with observational (non-experimental) data. Haavelmo's 1944 supplement to *Econometrica* discusses difficulties in merging *ceteris paribus* theory with observational data (he labels these data passive observations). He writes (pages 14 and 15):

*A design of experiments (a prescription of what the physicist call a "crucial experiment") is an essential appendix to any quantitative theory. And we usually have some such experiments in mind when we construct the theories, although – unfortunately- most economists do not describe their designs of experiments explicitly. If they did, they would see that the experiments they have in mind may be grouped into two different classes, namely, (1) experiments that we should like to make to see if certain real economic phenomena – when artificially isolated from "other influences" – would verify certain hypotheses, and (2) the stream of experiments that Nature is steadily turning out from her enormous laboratory, and which we merely watch as passive observers. In both cases the aim of theory is the same, namely, to become master of the happenings of real life. But our approach is a little different in the two cases.*

*In the first case we can make the agreement or disagreement between theory and facts depend upon two things: the facts we choose to consider, as well as our theory about them. As Bertrand Russell has said: "The actual procedure of science consists of an alternation of observation, hypothesis, experiment, and theory."*

*In the second case we can only try to adjust our theories to reality as it appears before us. And what is the meaning of a design of experiments in this case? It is this: we try to choose a theory and a design of experiments to go with it, in such a way that the resulting data would be those which we get by passive observation of reality. And to the extent that we succeed in doing so, we become master of reality – by passive agreement.<sup>1</sup>*

Our econometrics in the mid-1970's basically tried to split the difference between Haavelmo's two classes of experiments: we modeled observational data (his passive observations) on the variables suggested by economic theory, but added (conditioned on)

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<sup>1</sup> Actually Haavelmo's discussion is not unrelated to earlier treatment of inference by John Stewart Mill (1884): *A System of Logic, Ratiocinative ad Inductive*, eighth edition, Harper & Brothers Publishers, New York, NY. Here Mill argues that wherever possible both experimental and observational studies can (if possible) be used together. Two modern works that appear to agree with Mill's recommendation are Rubin (*J. Educational Psychology* 1974) and Bessler and Covey (*AJAE* 1993).

small sets of “confounding” variables, variables that changed over our sample period and obviously negated the ceteris paribus assumption made in the initial theory. Of course the set of confounding variables was not known with certainty and, in fact, that prior theory which we were trying to model suggested that the set of potential confounding variables was indeed very large, as general equilibrium was the theory we were trying to model in the first place.

Haavelmo considers the relation between  $y$ , a dependent variable, and a large number of potential independent variables  $x_1, x_2, \dots, x_n, x_{n+1}, \dots$ . He warns us that it would be impossible to obtain ceteris paribus using observational data if one of two conditions holds: *The first relates to a situation where the independent variables are such that each independent variable may possibly have a great influence on  $y$ . The second condition is when each  $x$  tends to be constant or have a small variance, but on occasion, any of the independent variables can vary greatly. To pick out a small number of factors  $x$ , assuming the rest to be constant, would then be of very little help in ‘explaining’ the actual variations observed for  $y$  ... simply because the ceteris paribus conditions ... would be no approximation to reality.... From the point of view of verifying certain simplified relations of theory we might say that ... it would be impossible to find data for such a purpose by the method of passive observations (Haavelmo, 1944: 25).*

The 1970’s showed us the consequences of ignoring Haavelmo’s advice. Rausser (*New Directions in Econometric Modeling and Forecasting in U.S. Agriculture*, New York: North Holland Pub. Co., 1982) describes this period in agriculture econometrics:

*To the U.S. government officials who were struggling to control inflation... the tremendous increase in food prices was indeed a bitter disappointment. At this juncture, it became crystal clear that the constructed models of the USDA were no longer viable. The forecasts generated by these models appear to be outliers in comparison to the actual behavior of the system. (page 2)*

This period was one of “extra-ordinary science” in Kuhn’s vernacular (Kuhn, *The Structure of Scientific Revolutions*, Chicago, 1962). We knew existing “structural” methods guided by strong doses of prior theory did not work (a similar disappointment as Rausser describes above was evident in economics in general in the mid-1970’s, see Granger and Newbold, *Forecasting Economic Time Series*, New York: Academic Press, 1975). Econometricians looked around for other research methods proposed by earlier generation of scientists. Here methods in experimental economics of Vernon Smith (*JPE* 1962, *QJE* 1964) were explored. This literature took the suggestions of Haavelmo (described above) seriously by working with data generated by his first class of experiments. Today this line of inquiry is well-accepted for studying micro-economic behavior. A second line of activity motivated by the “structural” failings of the 1970’s was the time series approach, which looked back at the earlier work of Burns and Mitchell (*Measuring Business Cycles*, New York: NBER, 1946) and Lui (*Econometrica* 1960) and the statistical work of Box and Jenkins (*Time Series Analysis :Forecasting and Control*, Oakland: Holden-Day, 1976). Here economists focused on the second class of

Haavelmo experiments – essentially looking for explanations which were consistent with the regularities in the observational data.

The DAG and algorithms of inductive causation belong to the second category of experimental inquiry suggested by Haavelmo. This inquiry is with passive observations, where our goal is to make our theory agree with the observational data. The inquiry is not unlike that of Kepler (*Astronomia Nova*) or many nineteenth century economists (see Nerlove, Grether and Carvalho, *Analysis of Economic Time Series*, New York: Academic Press, 1979). The basic idea of “screening off” and its implications for partial correlation, which Box and Jenkins recognized with time series data, can be extended to cross section data or combined with multiple time series methods with time series data to offer evidence on contemporaneous time relationships.

### *DAGs and Algorithms of Inductive Causation in Economics*

A directed graph is a picture communicating the causal flow among a set of vertices (variables). Lines with arrowheads are used to represent such flows; the graph  $A \rightarrow B$  indicates that variable A causes variable B. A line connecting two variables, say  $C - D$  indicates that C and D are connected by information flow but we cannot tell if C causes D or vice versa. Here we consider directed acyclic graphs (DAGs), which means that we do not consider inference on systems such that information created in one variable (say variable A) passes on to other variables (B and C), but ultimately returns to its source (A); we do not study systems such as  $A \rightarrow B \rightarrow C \rightarrow A^2$

The fundamental notion which allows us to assign direction of causal flow to a set of variables is that of screening-off phenomena and its more formal representation in terms of d-separation (Pearl, *Causality*, Cambridge, UK, 2000). For three variables E, F and G, if variable F is a common cause of E and G so that  $E \leftarrow F \rightarrow G$ , then the unconditional association between E and G will be non-zero, as both E and G have a common cause in F. Pearl refers to such a structure as a “causal fork.” If we measure association (linear association) by correlation, E and G will have a non-zero correlation. However, if we condition on F, the partial correlation between E and G will be zero. Common causes “screen-off” associations between their effects.

A similar “screening off” condition exists for three variables exhibiting a “causal chain” relationship”  $H \rightarrow I \rightarrow J$ . The unconditional association (correlation in a linear world) between H and J will be non-zero; however, if one conditions on the middle variable I, the conditional association (partial correlation) between H and J will be zero. Knowledge of the middle variable in a causal chain will “screen-off” association between the two end variables (the root cause, here variable H, and the sink, here represented as J).<sup>3</sup>

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<sup>2</sup> Of course we could model feedback in dynamic systems as a DAG by dating variables, so that  $A_t \rightarrow B_t \rightarrow C_t \rightarrow A_{t+1}$ , etc. is a perfectly valid DAG representation (see Akelman, Bessler and Burton in Glymour and Cooper *Computation, Causation, and Discovery* Cambridge, MA: AAAI/MIT Press, 1999).

<sup>3</sup> Readers familiar with Box and Jenkins’ (1976) univariate time series work will recognize the screening-off conditions on a causal chain as part of the “identification” conditions of an autoregressive process of order p in terms of the “tail-off” and cut-off” behaviors of the autocorrelation and partial autocorrelation functions.

On the other hand, if we have variables K, L and M such that  $K \rightarrow L \leftarrow M$ , labelled by Pearl as a “causal inverted fork” (Pearl 2000, page 17), the association conditions differ from those discussed above. Here we have L as a common effect of K and M. K and M will have no association (zero correlation if we constrain ourselves to linear association); however, if we condition on L, the association between K and M is non-zero (the partial correlation between K and M, given L is non-zero). Here we say knowledge of common effects do not “screen-off” associations between their common causes.

The independence structure of “causal forks” and “causal chains” is the same, as the middle variable in each given above (F and I) “screens off” association between the respective end variables (E and G for forks and H and J for chains). On the other hand, the independence structure on “causal inverted forks” is different, as the middle variable on an inverted fork does not screen off association between its two end points (L is said to open up the communication between K and M in the above inverted fork). It is the existence of causal inverted forks (arrows coming together at a variable) which will allow us to infer causal structure using independence and conditional independence relationships among a set of theoretically related variables whose measurements are observational (non-experimental).

These screening-off phenomena associated with common effects, causal chains and common causes have been recognized in the literature for some fifty years now; for example, Orcutt (*REStat.* 1952), Simon (*Studies in Econometric Method*, New York, 1953) and Reichenbach, (*Direction of Time* University of California Press, 1956). However, it is only recently that they have been formally introduced into the literature for assigning causal flows among three or more variables. Key to this modern re-birth is the technical work of Pearl and his associates (Pearl, *Causality*, Cambridge UK, 2000) and Spirtes, Glymour and Scheines (*Causation, Prediction and Search* Cambridge, MIT Press, 2000). Applications include Bessler and Akleman (*AJAE* 1998), Bessler and Lee (*Empirical Economics* 2003), Hoover (*Ecmt Theory* 2005) and Moneta (*Empirical Economics* 2008).

Our recent work with these algorithms and DAGs has focused on inference methods for testing a hypothesis that one random variable (A) causes another random variable (B). Contingent on a sufficiently strong correspondence between the hypothesized cause and effect, an appropriately related third variable (C which we may call an instrument) can be employed for such a test. The basic logic of the procedure naturally suggests strong and weak grounds for rejecting the hypothesized causal relationship. Monte Carlo results suggest that weakly-grounded rejections are unreliable for small samples, but reasonably reliable for large samples. Strongly-grounded rejections are highly reliable, even for small samples (Bryant, Bessler and Haigh *Oxford Bulletin of Economics and Statistics*, forthcoming).