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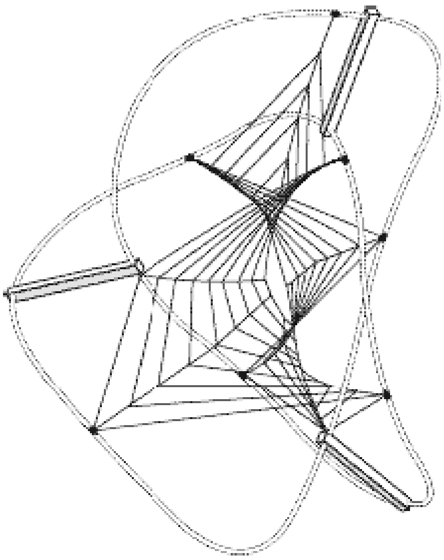

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Is structural econometrics evidence-based?[1]

Damien Fennell

Editor's Note

In this paper, I ask, in a highly simplified way, how structural econometrics can be seen as evidence-based. It considers three different ways in which one might do evidence-based causal inference from non-experimental data, a strict inductivist approach, a deductivist approach similar to the conventional approach of econometrics, and a 'middle path' similar to approaches developed in recent years, for instance, by Judea Pearl and others. The paper can be seen as exploration of some of the contingencies on which econometric methods, and causal inference methods more generally, may depend.

1. Introduction

Many see economics as the most scientific of the social sciences and econometrics as the most scientific part of economics. Econometrics has an impressive arsenal of statistical tools for making inferences in difficult situations. As a result, to the layperson econometrics appears to be a powerful scientific tool where large datasets are transformed into economic conclusions using opaque, extremely complex methods. Given these sophisticated empirical methods, it may seem odd to ask whether econometrics is evidence based. I think the question is nevertheless warranted. For any set of data there are many probability distributions and functional relationships that can account for it. These underdetermination problems are well known. In econometrics, however, this problem is compounded by the fact that a particular probability distribution and/or functional relationship can in turn be consistent with different and inconsistent structural hypotheses about the economy. These two underdetermination problems show that the use of data alone is not sufficient for selecting a preferred hypothesis. As such, it is not clear – at least without further analysis –whether econometrics is reasonably considered evidence-based. It all depends on how these problems are overcome.

It could be countered that econometric methods do not make arbitrary use of data or infer hypotheses without foundation. This is indisputable. Indeed, econometric

[1] This paper was presented at 'The Role and Evaluation of Evidence in Economic Analysis', University of Bologna, Italy, May 24 2008. I would like to thank the workshop participants for their helpful comments especially Maria Carla Galavotti, Philip Dawid and Mary Morgan.

theory relies centrally on the logic of statistical inference to set out why claims the method supports are warranted. However, the rigor of these methods tends to be that of statistics, on how to infer population probability distributions (or their characteristics) from sample data. This is not sufficient for structural econometrics which aims to identify economic structures in addition to probability distributions.

Structural econometrics aims to infer causes from probabilities, inferred from sample data generated in non-experimental settings. Arguably, it is the most ambitious part of econometrics. It aims to identify economic structures, robust parts of the economy to which interventions can be made to bring about desirable events. This part of econometrics is distinguished from forecasting econometrics in its attempt to capture something of the 'real' economy in the hope of allowing policy makers to act on and control events. This paper asks how the methods of structural econometrics can do this and to what extent their resultant claims can be considered evidence-based.

This question is general and ambitious. As a result, it is in danger of oversimplifying econometrics. In response to this, a couple of clarifications are in order. First, the aim in this paper is not to come to a simple black and white, yes-or-no judgement as to whether structural econometrics is evidence-based. Such an approach, even if it were possible (which is doubtful), would be of scant use to econometricians, philosophers or policy-makers. Instead the aim is to explore the nature of the evidential support for claims made in structural econometrics, with the aim of making clear the strengths and limits of econometric claims. Second, the methods of structural econometrics are numerous and highly complex. Given this, it is not immediately obvious how one can answer the question of evidential support in general. To deal with this, the paper makes an important abstraction. It focuses on how one can use statistical regression methods for making causal inferences from non-experimental data in an abstract way. Causal inference from regression lies at the heart of structural econometric methods and thus provides a natural way to answer the question without becoming lost in the particularities of individual methods. This simplification comes at a price, however. Any conclusions of this paper may require further qualification for application to the particular econometric methods and the specific purposes and circumstances for which these methods have been developed. I grant this, but nevertheless hope that this general approach will be useful as a first step for considering the evidence bases of diverse methods of structural econometrics.[2]

The paper begins with a discussion of a couple of necessary preliminaries. It briefly considers two very difficult questions: what is meant by *evidence* and what is meant by *causality*. Some clarity on these two concepts is necessary for any sensible attempt to answer the question of this paper. With this done, I distinguish two paths to causal conclusions using regression on non-experimental data. In the spirit of John Stuart Mill's (1836) distinction between *a posteriori* and *a priori* methods for political economy, I distinguish between an inductive and a deductive path to causal conclusions. The first proceeds from observation to the causal conclusions via regression. The second, the deductive path, starts with a general model, supported by theoretical assumptions and auxiliary hypotheses that we have good reason to accept, then, assuming observed data to be generated by a system represented by the model, uses the regression to parameterise a specific structural model, which is then used to support causal conclusions. The distinction between the inductive and deductive approaches in science is a standard distinction in philosophy of science. However, unlike Mill, I do not straightforwardly privilege one approach over the other for economics. My aim is instead to consider the evidence-bases for the two approaches and to relate these to the kinds of causal claims the two approaches can provide. Which is 'best' then depends on the context and on the aims of the analysis. Note also that these two paths do not exhaust the ways one can consider regressions as a route to causal knowledge. In fact, one can construct approaches that are in part inductive, in part deductive. One such 'middle' path is considered at the end of the paper. This approach uses a concept of a surgical intervention to make sense of structural equations and is currently popular in certain philosophical discussions of causality.[3] The paper briefly considers how such an approach can be evidence-based, before concluding on the general issue of the extent to which, and the conditions under which, regressions on non-experimental data can secure causal knowledge.

[2] A secondary benefit of this abstract approach is that the analysis here should in principle apply to other disciplines where regressions on non-experimental data are used to infer causal conclusions (in sociology for example).
[3] See, for example, Woodward (2003), Hitchcock (2001), Hausman (1998) and Pearl (2000).

2. What is evidence?

Recently there has been an influential movement in medicine and policy-making to make decisions 'evidence-based'. This has generated a lot of literature and sub-disciplines dedicated to evidence-based decision making. Perhaps surprisingly, however, it has not lead to a clear view on what evidence is. This difficulty with the concept of evidence can be seen in other disciplines. In philosophy of science, philosophers have spent decades working on the vexed issue of how and to what extent observation supports hypotheses in the sciences. Work on theories of confirmation has not lead to a consensus on how observations support hypotheses, nor to agreement on what evidence is. Likewise in statistics, where arguably the most work has been done on formalising the relationship between evidence and hypotheses, there is entrenched disagreement between various schools of statistical inference.

Depending on the view adopted, evidence can have a variety of different features. It might be characterised as a proposition that is implied by a hypothesis but which is ruled out by others, as might be adopted if one were to follow the hypothetico-deductive method or Popperian falsificationist approaches to testing hypotheses.[4] If one modelled evidence using a Bayesian approach, one might require evidence to raise the degree of belief in the hypothesis. More generally, one might require that evidence be probabilistically relevant to the hypothesis. Another feature of evidence might be that it make the hypothesis more likely than an alternative hypothesis (as might be the case if one were follow the Neyman-Pearson or Bayesian approach to statistical inference) or that it be more likely than its negation (as Sherrilyn Roush (2005) requires in her recent definition of evidence). Or, one might require that evidence provide reason to believe the hypothesis, a requirement imposed by both Peter Achinstein (2003) and Sherrilyn Roush (2005) in recent treatments of evidence.

Given this diversity of opinion, how is one to talk about evidence? The above suggests either a need to resolve disagreements by constructing a new widely-acceptable view of evidence, or to take a particular side in the debates. Both positions

[4] This is not to suggest that the H-D method and Popper's approach to testing are the same Rather the point is that evidence can be related to whether it is implied (the h-d method) or not (Popperian) by the hypothesis. Both approaches share their focus on the implications of a hypothesis for testing it. See Nancy Cartwright's (2007, chap 3) discussion of clinching and vouching methods.

strike me as unattractive. The first because it requires overcoming a whole set of recalcitrant issues which have led some of the greatest thinkers in philosophy, statistics and science to profoundly and reasonably disagree. The second because it requires being partial and antagonising those with alternative views. My response to this conundrum is to adopt a much simpler approach that should be largely uncontroversial and should suffice for the purposes of this paper. It avoids the difficulty of specifying exactly what evidence is by leaving it open or to be specified in the context by those best positioned to judge what is or is not evidence, and how strong that evidence is.

When one presents an argument or a proposal for a particular scientific hypotheses one often faces two questions, formulated in a variety of ways. With hypothetical claims in science, one is often asked what if any evidence one has for the claim. If the answer is that there is evidence for the claim, then this can take a wide-range of forms. It can be an appeal to other already accepted claims, to some form of observation, or some mixture of the two. Another response is that the claim was tested in some appropriate way. In all these cases, the aim is to show that the claim is suitably evidence-based and that the person asking the question has reason to accept the claim in some (possibly qualified) sense. This 'game' of giving justifications also takes place in relation to non evidence-based domains, such as philosophy, where claims may be justified on their metaphysical attractiveness, coherence, clarity or other grounds. But I take it that in all these intellectual domains we are naturally pushed to justify our claims when we attempt to persuade others to accept them for some purpose. What is particular about the justification by appeal to evidence is that it adopts an empiricist philosophy for the justification of belief. The aim to ground our claims (and ultimately our beliefs) on the truth or as close to the truth as we can, where the truth is best found by engaging with the world with our senses and with the measurements devices that have been constructed to augment them.

For the purposes of this paper, I assume uncontroversially that scientists are empiricist and that they desire to base their claims on evidence. As such, there is a need to justify scientific claims by appealing to evidence (or appropriate testing method) or by appealing to claims that are in turn already accepted with good reason (which may be further hypotheses supported by evidence, or metaphysical assumptions that we have good reason to accept).[5] Since I stay largely agnostic over the details of what counts as evidence, or how exactly scientific hypotheses should be tested, I set aside the difficulties faced by those who would like to define evidence, provide a logic of confirmation, or set out how theories should be tested in science. I take it for the purposes of this paper that in a practical context we can often reasonably and constructively question, discuss and explore the evidence basis of a claim without having a full definition of evidence in hand.[6]

So, I adopt the following approach. In analysing the method of structural econometrics I ask:

- What assumptions underlie inferences?
- Can these assumptions be well-supported by evidence?

A few points of clarification are in order here. In order to answer the first question, assumptions are made explicit that set out the conditions under which the method generates knowledge. More than this, the assumptions should reflect the rationale of the method. To answer the second question, one looks at what evidence we could have for the assumptions presented in the answer to the first question. This serves the purpose of showing why we should have (some) reason to believe the assumptions that underlie the method. So, ultimately this analysis is based on epistemological analysis of why we should believe what a particular method tells us, and it answers this by setting out why we have reason to believe the method is appropriate for a given situation and then why the method gives us reason to believe its results for that situation.

[5] The ultimate need to rely on some claims not supported by evidence is implied by the infinite regress that follows if one searches for justification for the claims which one has just asserted to justify some original claim. Ultimately the 'why' questions must stop however, and we make a metaphysical assertion (as to how the world is or about how we should behave) to end the regress. This does not undermine the justification appealed to. For instance, induction is by and large a good way to find out about the world and to act. This is in spite of the continuing frustration of the problem of induction, that is, the inability to justify the assumption that the future will resemble the past we make when we use induction as a guide to knowledge. In the end, we make assumptions that we may not be able to justify, but if they serve us well then continuing search for further justification seems practically, if not philosophically, a waste of time.

[6] This is not to say that a definition would not be useful in these cases.

3. Causality in Structural Econometrics

The aim of structural econometrics is to identify robust economic relationships that can be exploited for intervening into, structuring and ultimately controlling the economy. This requires relating evidence with causal claims, which in turn requires clarity as to the concept of causality assumed.

Causality has been a difficult and persistent topic in philosophy that, like the concept of evidence, has evaded neat and widely-acceptable resolution. In particular, there is a strong divide between those who take causal relations to be real and power-laden, and anti-realists who – following Hume – view it as a projection onto the phenomena we observe. This divide is reflected in different formal treatments of the concept of causality, between those who would define a causal relation 'from above', that is from regularities of phenomena and those would define a causal relation 'from below', that in terms of the relations that obtain between more fundamental entities. For example, contrast David Lewis's definition of causal relations in terms of counterfactuals (1973), which are true in virtue of regularities that hold in the world, with Nancy Cartwright's (1989) treatment of causal relations as holding in virtue of arrangements of capacities. More generally, treatments of causality in terms of counterfactuals can be contrasted with views that argue that causal relations hold in virtue of certain extant mechanisms, or entities with certain dispositions or causal powers.

This divide is a profound one in the philosophy of causality and it reflects two causal intuitions that evade straightforward reconciliation.[7] The first intuition is that of a cause producing its effect. This intuition is that which holds when we think of a causal process, a series of events where an event brings about the next and so on. It is appealed to when we explain a cause and effect relation by citing how the cause leads to the effect, how and in what steps. For example, we observe lightning striking a tree and setting it on fire, and intuitively take this to be a causal process. A natural way to describe this case is to consider the lightning as having the power to bring about the splitting of the tree, the causal process is that of the powers being exercised in series

[7] Cf. Hall (2004).

thus bringing about or producing a series of events.[8] This power-laden view also underlies more complex mechanistic descriptions that sometimes explain the connection between a cause and its effect. To borrow an example from Nancy Cartwright (2007, p.85), when we ask why a pushing a lever on a toaster causes a slice of bread to toast, we can answer by specifying the intricate machinery, its parts, their arrangement, powers and relations to explain how the act of pushing down the lever leads to toasted bread.

The second intuition is that causal relations hold in virtue of counterfactual dependencies. So in the lightning-tree case we might claim that the lightning caused the tree to split by asserting that if the tree had not been hit by lightning it would not have split in two. More generally, this view can be seen as one of explaining the causal relation in terms of difference-making, that the cause made a difference to the situation, one that would have been the case had the cause not been present. The claim of difference-making need not appeal to underlying processes or mechanisms, being supportable by appeal to the observation of regular cooccurrence of lightning and tree-splits in a contiguous, time ordered and constantly conjoined way (to follow Hume).

Both of these intuitions seem to work well in a range of situations. There are some situations where both intuitions are met (e.g. the tree-lightning case) and cases where one intuition seems to hold and the other does not.[9] My aim here is not to argue that one of these intuitions as fundamental. Instead the point here is to make these two intuitions clear and to note that there are differing views of causality that draw on these two intuitions. More important still is to note is that in economics we find causal claims that rely on one or both of the intuitions. In economic theory, for example, the mechanistic intuition seems central. In microeconomics we talk of agents, be they individuals, households, firms or institutions *acting* to *bring about* certain ends. These are constrained within certain contexts and *transact* goods and services. Likewise in macroeconomics, one hears talk of *flows* of goods, labour and

[8] To give a plausible power-laden description for this case: the lightning travelling through the water in and on the tree leads the water to evaporate (due to heat due to the resistance of the water to the current) and this sudden conversion of the water into steam with its resultant expansion violently pushes on the wood leading the tree to split in two.

[9] For example, the counterfactual intuition fails in cases of causal pre-emption, while the mechanistic view fails in cases of causation by absence. See Hall (2004).

money. Importantly, this is not to say that counterfactual dependencies do not also hold in many of these cases. However, the counterfactual dependence typically holds in virtue of some assumed mechanistic arrangement.[10]

In principle there could also be causal counterfactuals asserted in economics without relying on mechanistic information. For instance, one might observe a natural experiment, like that described by Julian Reiss (2007, chap 7), where an increase in the minimum wage occurred in one US state but not in another neighbouring, similar state. In such a case one could argue that certain causal counterfactuals hold (for example, if one were to raise the minimum wage in such a state then *ceteris paribus* the employment would not fall). So assumptions about mechanisms need not be necessary for making at least some causal claims in economics.

Another important connection between economics and the two different intuitions of causality relates to the use of mathematical models. In these, the relationships between economic factors are presented using functional relations that set out the values different factors can take as a group. In other words, the functional relationships stipulate a set of counterfactual claims as to the values the variables in the model can take. In structural models, these counterfactuals are used to support causal claims such as how changes in one variable will make a difference to another. When such a model is constructed by appeal to mechanistic claims (about agents, flows of goods, money, information etc.) however, there is a difficulty to be overcome. How do the counterfactuals (set out by the functional relations) follow from the mechanistic claims? In particularly rigorous cases of modelling one may be presented with a proof linking mechanistic assertions to the functional relations. But often this is not the case. Particularly in econometrics, there is a tendency to assert a functional form and to impose constraints on these functional forms by appeal to some (mechanistic) economic theory, which leaves unarticulated why the mechanistic assertions imply the *particular* functional (and thus counterfactual) constraint applied.

[10] For instance, one might explain the claim that an increase in price causes an increase in demand by appealing to the difference in two equilibrium demand states given different equilibrium prices. Yet, even this paradigmatic case of explaining a causal relation by asserting a counterfactual, the counterfactual in turn rests on some assumed (if not fully specified) dynamic mechanisms of two groups of agents (suppliers and consumers) with their respective goods and money bargaining to a point where a new equilibrium is reached.

This problem is compounded by the different evidence used to support the two kinds of causal claims. Evidence for mechanistic claims may follow from the observation of agents' behaviour in a market, whereas evidence for counterfactuals naturally follows from regularities (e.g. evidence for how market equilibria change from past observations of past changes in those equilibria). If one attempts to use evidence at the dynamic, mechanistic level to support emergent (e.g. equilibrium level) counterfactuals, not only is some connection from the mechanisms to the counterfactual relations required, but an evidence-base is also required to support the connection.

Given these two kinds of causality claims and the challenges in relating them to each other and supporting these with evidence, the analysis of econometric methods that follows takes particular care in making explicit the concepts of causality assumed and the evidence required to support associated causal claims.

4. Three paths to causal Nirvana

With these preliminaries in place, I return to the question of how and to what extent structural econometrics can be evidence-based. I address this by looking at how one might go about making causal inferences using regression on non-experimental data. This is done by considering the regression in two different ways, each way presenting a different path to desirable causal claims. The first path is inductive, the second deductive. The inductive approach generalises carefully from observation to causal claims trying to minimise *a priori* assumptions and where assumptions are made these should be supported or tested by further observation. The second, deductive approach, uses background theory and other accepted claims to construct a general model which is then made more specific by regressing on the observed non-experimental data. I finish with a brief discussion of a third middle path to causal claims.

Given that the goal is to infer causal claims from non-experimental data, I analyse these with a view to (i) what kinds of causal claims these two approaches can support, (ii) what is assumed to support these causal claims and (iii) what kind of evidence-bases there can be for these assumptions.

4.1. The inductive path to causal claims using regression

Imagine a situation where one has non-experimental data for a set of economic factors whose values are denoted by the random variable vector, Z.[11] The aim, following the inductive path, is to construct a regression model relating these variables that ultimately will be useful for making causal claims. This regression model will be constructed to resemble the simplest structural models found in econometrics but, in contrast, here only minimal assumptions will be made, since the aim of the inductive approach is to generalise very carefully from observation only making assumptions that can be suitably tested or supported by observation. In the process, however, some assumptions will be made that are not testable in that they are consistent with all possible observations. These assumptions will – in line with the strict inductive attitude adopted here – be arbitrary or conventional in that they rest on the modeller's choice rather than observation. This will be also taken into account in interpreting what claims are warranted by the model.

To begin, suppose as a first step that our inductivist wants to use the data to parameterise the following regression model

$$AZ + b = V$$

Where A is an invertible[12] matrix of unknown parameters, b a vector of unknown parameters, and V a vector of 'error' random variables. Suppose our inductivist also assumes that $E(V) = 0$ and $Cov(V,V) = \mathbf{1}$, that is, that the error terms have mean zero, unit variance and are uncorrelated to one another. But what motivates this? The imposition of zero mean and unit variance on error terms merely imposes scaling constraints on A and b, normalising the error terms. The assumption of uncorrelatedness is stronger, why should it be presupposed? A natural way to

[11] I assume here that a probability distribution for Z exists. Also, for simplicity, I assume that the variables of interest are all observable. I also assume that the moments of Z are time independent and that Z accurately measures the factors of interest. Though this makes the analysis too simple for many cases, it provides a natural first step toward the analysis of more difficult systems.

[12] This is equivalent to requiring that the equations on the l.h.s. be linearly independent. This can be motivated, from an inductivist view, by modelling with a minimal set of equations i.e. where no equation can be expressed as a linear combination of the others.

motivate such an assumption is to interpret the error terms in a causally substantive way and to read this constraint, along the lines of Faithfulness condition used in Causal Bayes nets models,[13] as implying a lack of hidden causal connection among the uncorrelated error terms. I do not attribute such an assumption to the inductivist because it requires adding a lot more content to the modelling process, which needs further observational support. This is antithetical to the strictly inductivist approach. Instead, I simply take it that this is a convenient way to construct a regression model because it does not leave unmodelled variance in the error terms, that is, such a regression model will model the covariance among the Z in the parameters, A.[14]

Assuming a regression model of this form implies the following relationship: $\mathrm{Cov}(Z,Z) = A^{-1}(A^{-1})^T$. Though this gives a condition on A in terms of observables (the second moment of Z), this constraint does not uniquely specify A. As a result, the inductivist cannot infer a unique value of A using a sample estimate of the covariance matrix of Z. Thus, a unique regression model of this form cannot be obtained from any sample data for Z, no matter how large. What is our inductivist to do? Without an inferred regression model, there can be no regression model on which to base causal claims.

A partial solution to this problem is to introduce a distinction between two types of variables in Z. One partitions Z into a vector of dependent variables (Y) and a vector of independent variables (X). This distinction is commonly made in statistics and econometrics and is labelled in diverse ways. In econometrics, for example, the typical terminology is between exogenous and endogenous variables. While in statistics one finds distinctions between independent and dependent variables, and between predictors and dependent variables, to name just a couple. Unfortunately, the different labels sometimes go hand-in-hand with a lack of clarity as to what just motivates the distinction between the variables. In structural econometrics, however, there is typically some causal content attributed in the distinction, with (roughly speaking) exogenous variables representing causes of the effects that are represented by the endogenous variables. In the case of our strict inductivist, however, I will

[13] See Pearl (2000, p.48) and Spirtes, Glymour and Scheines (2000, p35) for example.
[14] That said, if such a parsimonious approach turns out to be unable to be useful for basic causal inference, as one would expect, stronger assumptions will be required, as will be seen below.

assume that the distinction is motivated by a desire to construct a regression model of the Y's in terms of the X's for an as yet unspecified reason.

I also assume that there are as many linearly independent equations as dependent variables. With this, our inductivist desires to parameterise the following revised regression model

$$BY = CX + d + U$$

Where B and C are sub-matrices of A, B is invertible, d is a vector of parameters from b, and U a new vector consisting of some of the error terms of V. From previous assumptions it follows that E(U) = 0 and Cov(U,U) = **1**. I also suppose in addition the inductivist assumes that Cov(X,U) = 0. This last assumption could be intuitively motivated by the view that the independent variables should not depend on the error terms U.[15] For the inductivist, however, I treat this instead as an assumption of convenience, whose substantive justification is to be provided at a later point if necessary. Moreover, despite this additional assumption the model as specified is still not uniquely specified since one cannot uniquely solve for B and C in terms of the moments of the distribution of X and Y.[16] Therefore, even with this refined regression model, the inductivist still cannot parameterise the regression model.

This last problem is termed the 'problem of identification' in econometrics. To solve it, it is typical for econometricians to impose further constraints on the functional form. The simplest form of constraint is to assume that certain elements of B and C are zero. I assume that the inductivist does this, again without motivating the assumption, to get to a point where the regression model is such that B, C and d can be uniquely solved for in terms of the moments of the distribution of the observable variables Z. So at this stage the inductivist has constructed a regression model:

$$BY = CX + d + U$$

[15] Which in turn could be motivated by causal assumptions, that there are no causal connections between the independent variables and the error terms. For reasons already stated, I do not make such an assumption.

[16] This follows that from the fact that for any orthogonal matrix F (i.e. a matrix such that $FF^T=\mathbf{1}$) of the same order as B, if the equations are multiplied by F, then a new system is created that also satisfies the assumed properties of the system.

Where $E(U) = 0$, $Cov(U,U)=\mathbf{1}$ and $Cov(X,U) = 0$

with constraints on B and C such that these can be uniquely solved for in terms of the moments of the joint distribution of X and Y.

The assumptions made have not been motivated nor empirically supported. As such, this model is to be seen as a particular transformation of the observable variables Z to define orthogonal error terms U such that these are uncorrelated with the chosen independent variables, X.

As it stands this regression model now 'looks like' the basic structural model assumed in introductory econometric textbooks. However, there is nothing *structural* about it. What our inductivist has done is to construct a regression model to present the distribution of Z in a particular way. The regression model as constructed is best interpreted as a definition of a new random variable, U, where this is relative to a particular chosen partition of Z into dependent and independent variables with certain constraints on B and C to ensure U is well-defined. Nevertheless, at this stage the parameters of the model, B and C, can be solved for in terms of the moments of the distribution of Z. As a result, the inductivist can now estimate values for these by regression.

The lack of substance of this regression model can be observed in the fact that as constructed the model does not imply some of the most basic non-causal, observational claims typically read from similar looking regression models. To see this consider a simple univariate regression model of similar form.

$$Y_1 = \alpha_{11}X_1 + U_1 \quad (cov(X_1,U_1) = 0).$$

These equations tend to be read as implying:

$$\alpha_{11}\Delta x = E(Y_1 | X_1=x+\Delta x) - E(Y_1 | X_1=x)$$

Where this is read as 'if we were to observe a change in X_1 of Δx we would expect a change of $\alpha_{11}\Delta x$ in Y_1'. But this does <u>not</u> hold unless the following holds:

$$E(U_1\,|X_1=x+\Delta x) - E(U_1\,|X_1=x) = 0 \quad \ldots \quad (+)$$

However, this is not implied by $Cov(X_1,U_1)=0$ or the other assumptions made. This is as one would expect, however, since the regression model built by the inductivist acts essentially as a definition of the error terms in terms of some variables of interest, X and Y. Such U, which are defined by orthogonalisation, can be so defined for almost all possible distributions on Z, most of which do not satisfy (+).

Therefore, to be able to infer the basic intuitive observational claims from the regression model, the inductivist needs to supplement the regression model so that a generalised form of (+) for the variables X and Y holds for all possible values of x i.e.

$$E(U\,|X=x+\Delta x) - E(U\,|X=x) = 0 \text{ for all } x,$$

This is in turn implied by the stronger, yet intuitive assumption:

U is probabilistically independent of X … (XIndU)[17]

The introduction of this assumption adds substance to the assumed functional form in the model[18] in that it can be inconsistent with observation.[19] It can be tested by looking for dependence of the residuals on the independent variables. Given the definition of the U using the functional form assumed in the regression model, such a test is a test of the specified functional form of the regression model (i.e. a form of specification testing). Since (XindU) can be checked/tested using sample observations, the inductivist should be happy to augment the model with this assumption and test it accordingly.

Thus if the inductivist has a regression model above, has strengthened it by assuming (XIndU), and has tested this by analysing the residuals and found no dependency in

[17] This is a conventional exogeneity assumption in econometrics.
[18] This becomes clear if one replaces U with BY-CX-d (as implied by the definition of U in the regression model), since (XIndU) then becomes an assertion that the assumed functional form is independent of X.
[19] Or more precisely, given that the hypothesis is uncertain, particular observations would be very unlikely to occur if (XIndU) were true.

the residuals on the independent variables, then he can make evidence-based observational subjunctive claims like that made above, namely warranted claims about what would happen to values of the independent variables given changes to the dependent variables.

Of course the observational claims need not be causal. The model as constructed thus far may not support causal subjunctive claims of the sort 'if X were *caused* to change value by …, then Y would change value by…' nor 'changes in X of Δx would *cause* changes in Y of …'. There is no reason to assume that the regression model, even if it fits the observed data well and there is no sign that there is misspecification, supports interventions or claims that X causes Y. The model may allow us to claim that if X changes in line with population inferred from past data, then we can expect Y to change in a certain way. Changes to X that are attributed to an intervention, or to some cause, may not – and there is no reason to assume so without further information – preserve the distribution of the data.

So, what can an inductivist do to use the model to support causal claims? One assumption that can be made which would, if supported by evidence, permit some causal conditionals to be made with the model, is an *invariance* assumption. This assumption stipulates that

The model is invariant to a specific set of interventions/caused changes. (Inv)

If this assumption holds, then if a change or intervention from the specified set occurs then the model will not be disrupted. As a result, one can support the claims about how Y would change given changes in X when the change in X is one of those to which the model is invariant.[20] But what evidence can be found for this invariance assumption? One case is where one has observed that in the past changes (from the specific set) in X and these have not disrupted the model. This provides inductive evidence that future changes of the same sort will not disrupt the model. However, to provide such an inductive base faces serious data challenges. One must check that the same regression model (inferred on different data sets) has invariant parameters to the

[20] Such an assumption of invariance is made by Engle et al. (1983) in their definition of superexogeneity.

set of possible of caused changes/interventions to the factors of interest X. For even small sets of possible changes in X this poses serious data challenges as very large sets of data are required to perform the multiple regressions, to check if their parameters are invariant.

Another kind of invariance claim can also be supported by appeal to background structural information. In this case one has background knowledge about the system generating the data and about some relevant change and one has good reason to suppose the former will not be disrupted by the latter. For instance, suppose I construct a regression model to predict stock returns and use it as a small private trader to trade. Given the size of the market and the limited size of my investments, there is good reason to believe that my using the model to trade will not disrupt the overall market, the data from which was used to construct the regression model. However, note that in this case, the causal conditional claim (i.e. if I invest in certain assets I can expect a certain profit) does *not* refer to an independent variable in the model, since by assumption, my actions as a trader have no (or negligible impact) on the variables in the model. So, though this case is one where the induction model is useful for intervention, it is not making use of the model as *structural* in the sense of setting out the impacts of the independent variables on the dependent variables.

At this point, the inductivist can make certain evidence-based causal subjunctive claims using the inferred regression model. However, the types of causal claims supported by the model are limited. In cases where there is no evidence of invariance, there is no basis for strengthening observational subjunctive conditional claims to their stronger causal counterparts. Indeed, attempting to extend the regression model to make causal claims where invariance may not hold seems particularly difficult, given the inductivist approach adopted thus far. Consider a change or intervention that affects one or more dependent or independent variables, or which disrupts the system that generated the data on which the regression.[21] In these cases it seems difficult to use the model to make causal claims, since there is simply no basis for claiming that observed correlations (on which the model rests) will be invariant in a suitable way. Importantly, the regression model does not even have a structural

[21] Such cases form the basis of Robert Lucas' (1976) famous critique of econometrics.

interpretation. The model is simply a well-specified, evidence-based statistical model that is invariant to certain changes, this does not imply that it can read in a causal way, such as that the independent variables cause the dependent variables, and that the linear form denotes how causal inputs combine. Far from it, the approach adopted here has not even introduced a concept of cause or of how causal contributions combine. Without these, there is no basis from which to relate the caused change – which may not satisfy invariance – to the regression model, let alone a way to support causal subjunctive claims in relation to that change.

This seems to be the end of the road for the strictly inductivist path adopted here. To take it further would require additional work. One method would be to adopt a probabilistic theory of causality that relates probabilistic independencies and dependencies to causal relationships. This is the method of the currently popular causal Bayes nets approach. However, these rely on key metaphysical principles relating the probabilistic properties to causal claims, such as the Causal Markov and the Faithfulness conditions. Of course, it is not obvious that one should accept these principles, and there has been serious philosophical debate as to their validity and their scope.[22] Another related move is to use time order of variables to help with the causal ordering of variables. Though the association of time precedence to causes is intuitive and has a long philosophical pedigree, it is not obvious that intuitions that rest in the natural sciences are appropriate in social sciences, where the time dating of data is coarse-grained and expectations are at play. For now I leave the inductive path and consider the more conventional way to use regression to infer causal claims in econometrics, the deductive path.

4.2. The deductive path to causal claims using regression

As shown above the strict inductive approach has difficulty finding an evidence base for causally substantive interpretation of the regression model. So here I consider a radically different deductivist alternative. In this approach instead of building the regression model from observation, a causally substantive model is assumed from the outset. This causal substance attributed to the model is supported by appeal to theory

[22] For example, see the recent debate on the causal Markov condition between Nancy Cartwright (2007, chapters 8 and 9) and James Woodward and Dan Hausman (1999) and (2004).

and other claims that are well-supported (for example background knowledge about the system represented by the model). In economics, the theory appealed to can involve agents (individuals, firms, households, institutions) their preferences and expectations. It may also involve claims about the transfers of goods, money, labour and so on. Crucially, these claims are mechanistic in nature, they are about the processes at play, the *actions* of agents, the *transfers* of goods. This is the case even with equilibrium models in economics, since these are supported by appeal to the dynamic system underlying the equilibrium. In this way, the mechanistic-orientation of the deductivist approach diverges from the inductivist approach, which attempted to base causal claims on observed regularities that are robust to certain changes.

We can consider the earlier regression model from a deductivist point of view. The deductivist uses theory and auxiliary hypotheses to construct an abstract causal model which gives the dependent variables as a function of the independent variables i.e.

$$BY = CX + d + U$$

Where $E(U) = 0$, $Cov\ (U,U)=\mathbf{1}$ and $Cov(X,U) = 0$

with constraints on B and C such that these can be uniquely solved for in terms of the moments of the joint distribution of X and Y. Despite sharing the same syntax as the regression model in the inductive approach, here the model is substantively causal, and the assumptions of orthogonality are associated with stronger constraints on the variables and the error terms. Some of the key assumptions of the deductivist approach can expressed as follows:

- Functional relations represent a causal structure supported by background (theory-based + auxiliary) assumptions.
- Error terms represent the net impact of omitted causal factors on particular endogenous variable(s).
- Exogenous variables are not caused by other variables.
- Exogenous variables do not have causes that cause an endogenous variable via an unmodelled path.

In setting these out, the term ‘cause’ occurs again and again. Econometricians may take issue with this presentation, since there is a tendency to avoid using the term, with terms such as ‘exogenous’, ‘endogenous’ or ‘structure’ used instead. However, regardless of the terminology used, some theory relating functional relations, probability distributions and causal subjunctive claims is required in order to secure the deductivist aim of using regression to infer ‘structural’ relations. Indeed, even if the term ‘cause’ is used, some philosophical story is ideally required to fill in its content, to tell us what it means to say that A causes B, and what such a claim licenses one to do (for example, what can we say about interventions to change B using A?).

Following the deductivist approach, the next step is to use the data to parameterise the model, to estimate B and C. This can be done because, as in the inductive case, B and C can be uniquely solved for in terms of the moments of Z, which can be inferred from sample data. However, unlike the inductivist, the deductivist takes the estimates of B and C to measure structural parameters, or strengths of causal impacts of one variable on another. To support this (much stronger) claim, the deductivist assumes that the sample data adequately measure the theoretical variables in the model and that the data have been generated by a structural system of the form assumed in the model.

Thus, unlike the inductivist, the deductivist achieves a very rich result. If the assumptions are met, then the inferred regression model measures the actual structural system relating the different economic factors together. The inferred regression parameters measure the strength of impact of different factors on each other, and the causal relations support a whole host of causal subjunctive claims. So it seems we have arrived in Nirvana, where we can make strong and powerful causal assertions from non-experimental data.

But, of course, this is much too hasty. The arrival in Nirvana is *conditional* on the many assumptions made being well-supported. But what evidence can there be for these? What evidence is there for the crucial theoretical and background assumptions supporting the choice of model, for the assumption that the functional form chosen actually represents the causal structure generating the data, that the error terms do accurately measure the influence of omitted causal factors, that exogenous factors are not caused by any endogenous factors, and finally that exogenous variables do not have causes that cause an endogenous variable via an unmodelled path. Moreover, what reason is there to accept the concept of causality presupposed in the model and the relationships it presupposes between probabilistic relationships and causal claims?

Thus, though the deductivist approach appears more powerful, it does this by using much more powerful inputs. Moreover, given the strength of its assumptions it seems unlikely that one will ever have good evidence for all of the background claims. Even in areas of economics that are well-established, where one may have good reason to assume certain causal relations – these will rarely specify an associated general functional form which sets out just how causes may combine to the point where regression will pick out a unique model as correct. This difficulty is compounded by the gap between mechanistic and counterfactual causal claims. The functional form of the model specifies a set of possible counterfactual relations that the factors in the model must satisfy. And yet, these are typically constructed by appeal to mechanistic assertions. What is required is a detailed story as to how the mechanistic assertions lead one to the counterfactual relations asserted in the assumed functional form of the model and an evidence-basis for this story. However, this is extremely difficult, and typically (and conveniently) overlooked as a problem. Instead, functional forms are often chosen for reasons of convenience or tractability and intuitive causal claims are used to assert certain probabilistic independencies or ‘zeros’ in the functional form. But moves such as these do not respect the epistemic aim of inferring causal claims from non-experimental data. The result is often a statistical model that is assigned a structural interpretation, where it is unclear the extent to which the interpretation is supported by the data used in the regression.

So, if the inductive path is a road that ends too soon the deductive path is a bridge to the final destination without a route onto the bridge itself. The inductive path can in certain cases allow one to make certain subjunctive claims that are robust to certain interventions or caused changes. And while the deductive path in principle allows one to make all sorts of strong causal claims, it does this only if we know an awful lot about the system generating the non-experimental data in the first place.

4.3. A Middle Path

To finish this section, I briefly consider a third alternative, currently popular in some recent philosophical discussions of structural equations. In particular, it is an approach adopted in varying forms by Pearl (2000), Woodward (2003), Hausman (1998) and Hitchcock (2001).

As with the other methods, a distinction is made between two kinds of variables in the equations, here between causes and effects. These treatments are based on recursive structural equation models (where each equation specifies the value of an effect in terms of its direct causes). Taking an extremely simple example that of a single (univariate) structural equation

$$Y = aX + U$$

Under this view, the structural equation gives the value of the effect Y in terms of a direct cause, X and an error term denoting the joint impact of all other omitted direct causes. Here the causal relation expressed is given content through the following assumption: a *surgical intervention* on the direct cause, X, results in a change Y of $a\Delta X$, where (loosely speaking) a surgical intervention is an intervention on X that effects only X, does not directly effect Y nor effect any other direct cause of Y.[23] This is a neat characterisation of the causal relation in terms of what is essentially an experimental intervention and partially defines the causal relation (since the definition is circular). It is also a counterfactual concept of causality since it partially defines the causal relation in terms of subjunctive conditionals.

Given the characterisation of the structural equation in terms of surgical interventions, the obvious device for inferring such structural equations would be to perform surgical interventions on certain variables and to measure resultant changes in the dependent variables. The natural way to do this would be by controlled experimentation, so that one can license the claim that the interventions are indeed surgical. If this were repeated systematically, that is, if independent variables were

[23] Cf. condition I3 in Woodward (2003, p.98).

systematically surgically intervened to individually (and then in groups to see how they combine) and viewing the resultant effects, the structural equations could be inferred. However, this approach relies on experimentation and thus assumes away the problem with which we started that of using regression from *non-experimental* data to make causal inferences. So what sense can one make of the regression modelling using non-experimental data using this approach?

Viewed inductively, there is the problem of justifying the attribution of the structural content (in the surgical sense above) to the regression model inferred from sample data for variables X and Y. Making such an inference, would require a way to support a bridge principle, from the inductive exogeneity properties (e.g. Cov(X,U) = 0, XindU) to the much stronger claim that a surgical intervention into an X changes Y in accordance with the structural equation. Doing this would require evidence for the claim that the regression model is robust to surgical interventions in the dependent variables. If one could assert that the sample data was such that the changing values of X were *surgical* changes then one would have reason to read the resulting regression model in this way. But to make this claim inductively when the data is non-experimental seems extremely difficult. Of course, if one followed the deductive approach one could assume the non-experimental data are generated by a structural equation model of this form, with a surgical intervention interpretation as assumed above. But such a move simply faces the same problem as the deductive approach above, namely how to support such strong assumptions with evidence. So a brute problem remains whether viewed an inductive or deductivist perspective is adopted: how to justify the structural content of the equations (in terms of surgical interventions) when the data was generated in a non-experimental situation?

5. Conclusion

This paper has attempted to shed some light on how evidence-based causal inferences can be made using regressions on non-experimental data. Though this has been done in a largely abstract way, it is of direct relevance to econometrics, given the reliance of its methods on such regression methods. Moreover, the discussion on the assumed concepts of causality have been directly related to the economics. The paper has mainly focused on two ways regressions on non-experimental data can be rationalised as providing causal knowledge. The first, the inductive approach, generalises carefully from observation, holding fast to a careful empiricism. However, the price of this caution is that the evidence-based causal claims that can be made from the regression model are limited and do not support a structural reading of the equations. Nevertheless, it has the advantage of having a clear evidence basis. At the other extreme, the deductivist approach –the conventional logic of structural econometrics – is much more ambitious. By making many strong background assumptions, the deductivist reading of the regression model allows one – in principle – to support a structural reading of the equations and to support many rich causal claims as a result. Here, however, the difficulty is that of finding good evidence for many of the assumptions on which the approach rests. It seems difficult to believe, even in cases where we have good background economic knowledge, that the background information will be sufficiently to do the job that the deductivist asks of it. As a result, the deductivist approach may be difficult to sustain, at least in economics. Finally, the paper has briefly considered a 'middle' alternative, where structural content is explicated in terms of surgical interventions. Though this has the advantage of a particularly clear expression of what causality means, it also faces serious problems of warranting its claims with evidence, particularly for the situation faced by econometrics that of making causal inference from *non-experimental* data.

In conclusion, the prospects are perhaps best for the inductive approach, since it stays closest to the evidence and as a result its weaker claims can have a more secure evidence base. However, this comes with the loss of the quantitatively precise causal information econometricians and policy-makers would like. But the difficulties in providing an evidence base for the deductive approach show just how difficult it is to warrant such strong causal claims. In short, as might be expected there is a trade-off between the strength of causal claims we would like to make from non-experimental data and the possibility of grounding these in evidence. If this conclusion is correct – and an appropriate elaboration were done to take into account the greater sophistication of actual structural econometric methods – then it suggests that if we want to do evidence-based structural econometrics, then we may need to be more modest in the causal knowledge we aim for. Or failing this, we should not act as if our causal claims – those that result from structural econometrics – are fully warranted by the evidence and we should acknowledge that they rest on contingent, conditional assumptions about the economy and the nature of causality.

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