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1 Method

It is important before discussing various econometrics approaches to have a bit of historical perspective and talk a little about some important philosophies for empirical work. One could write books about this so this is just an unsystematic introduction (some of it maybe a little dated, but I want to give you a flavor of developments over time also).

1.1 Methodology proper

Books about methodology proper is not of too much help for the applied economist. Two widely read books on methodology are Blaug (1980) and Caldwell (1982). One thing that strikes an econometrician when reading those books is the fact that a major theme for both authors is whether testing of economic theories is possible; but they have absolutely no discussion on how this testing should or might take place. Nevertheless, it is important to read books on methodology, if one has interest in that area, because methodologists develop a precise language by which one can discuss methods.

Blaug seems to be quite positive towards the idea that economic models should be testable, although he realizes that it is not as simple as that, whereas Caldwell has a much more sceptical view towards testability as you might guess from the title ("Beyond Positivism..."). One interesting line of thought is developed by Lakatos. We won't go into details of his theories; but just note that he claims that all theories come with a "protective belt" of supporting hypothesis, so that an empirical rejection can always be attributed to flaws in the protective belt rather than in the theory.

An example from economics. Say you want to test the hypothesis of Rational Expectations. Then you will build a model, usually assuming a form of a utility function etc., and estimate it. If you reject the implications of the model, does that mean that RE is wrong. No! You probably just chose the wrong utility function. Or if you didn't do that, there is an infinity of reasons why the data might not be just right. And so on. You can think of many examples yourself. I find the protective belt analogy very convincing; but I don't see it as devastating for testing. If models generated from a certain theory consistently fail to perform, this theory will sooner of later fall out of fashion. Isn't this a reasonable description of what has happened over the years in economics?

1.2 Problems with older macroeconometric work

The debate on econometrics, among econometricians, intensified around 1970 and after. There was a widespread dissatisfaction with the general state of empirical work, presumably provoked by the hard times that the big econometrics models had with predicting the developments in the economy in the early 70ies.

A caricature of empirical work in the 60ies could be like this. An article would contain a first half of theory, which could be more or less formal. In the middle of the article the theory would drop out and the researcher would start estimating linear regressions. Presumably the theory had motivated which variables should enter the equations. Now the researcher would run 100, say, regressions and only 5 per cent of those would give significant coefficients. So the other 95 per cent would be discarded and the 5 per cent "good" results presented in the paper. Variants of this method will be termed "Data Mining". If results have been obtained by data mining the test statistics are totally unreliable. (Even if researchers do not data mine, one may be skeptical about published results, since editors on very rarely publish negative findings (i.e. if you have a nice model but your data do not deliver significant t-values, you will have a hard time getting it published). So again you may imagine that there is no relationship between two variables, but if enough researchers try to estimate the relation (independently) some of them will randomly get significant t-values and those (and only those) will get published.)
Econometricians have tried to device methods that correct for data mining, in the sense that economists will have used the data to "try out" different models. This literature can be found under the heading "pre-test bias" or something similar. However, as one might imagine, such methods only work if search for models is done according to a specific initially determined plan; rather than the higgledy-piggledy in which empirical papers progress more often than not.

The standards of empirical modeling was attacked by several econometricians and economists. Leamer had a famous article called "Let's Take the Con Out of Econometrics" (see Granger (1990)) and Hendry suggested an approach based on much more explicit recognition of time series structure combined with extensive testing. Sims criticized structural equation modeling and suggested the use of vector time series (VAR) methods (see the collection by Granger (1990) for articles by Hendry and Sims). Hendry was particularly influential in England whereas Sims had a quite large following in the US. (In fact, it seems that VAR methods are still very popular indeed.)

1.3 Rational expectations and the Lucas critique

(If you feel sure you grasp this issue from Macro I, using the PIH as the prime example, you can skip this subsection)

In the US, the most influential critique of econometric practice was voiced by Lucas, who claimed that typical econometric equations could not be assumed to be stable to policy interventions and therefore useless for what they were primarily intended for (see Lucas and Sargent (1981)).

Consider the model

$$y_t = b'x_t \ ,$$

where x_t is a vector of regressors and b a vector of parameters. The early way to estimate this model would consist in adding an innovation term and estimate the model by least squares. One would then look at diagnostics like DW and maybe test for heteroskedasticity and if these diagnostics were satisfactory, so were the model. (This is also a bit of caricature, I know that many econometricians did quite a good job, especially making use of residual plots to check for problems. The problem of this approach is that it is quite subjective as compared to formal tests).

The Hendry critique was primarily centered on the fact that such an approach did not take the actual stochastic structure of the model into account. Hendry suggested that one instead assumed a general *statistical* model for the data, which he described by an unknown density $D(x_t, y_t \ ; \beta)$, where β is a vector of parameters. This unknown density is called the Data Generating Process (DGP) and it is the job of the empirical econometrician to try and uncover this DGP or at least the part of it that is relevant for modeling y_t. In order to do this in practice, one ought to start with the most general model possible and then *test* down to the preferred model. Hendry claims that the three golden rules of econometrics is "test, test and test". Hendry has developed a software package PC-GIVE that makes his method easy to implement.

It is of course impossible to evaluate broad methods in general; but it is important to notice Hendry's statistical (as opposed to economics-based) approach. Hendry's method may be superior if you want to obtain the best possible statistical model for the selected series. Notice that this is totally different from what the methodologists were considering, namely whether economic theory is testable. Note: I first wrote this in the mid-90s. Today, few people in the U.S. know about Hendry, but Hendry's method was a precursor for the modern (single-equation) co-integration methods—even if today's (U.S. based) co-integrators do not think of themselves as part of any particular school. (In Europe, there are still people who sees Hendry's approach as the final solution.)

One development that is less susceptible to data mining is the estimation of non-linear economic models with functional forms taken from economic theory. Such models became popular in the late 1980s (especially, in energy economics, which was quite mainstream after the 1970s oil price shocks—the goal was to estimate the ability to substitute away from expensive energy). Still the time series structure was not taken into account to any major degree. In their article in the Handbook of Econometrics Granger and Watson (1984) claims that "For many years economists and particularly econometricians behaved as though they did not realize that much of their data was in the form of time series or they did not view this fact as being important". However, the situation has changed a lot since then.

The Rational Expectations (RE) school of econometrics which was pionered by Sargent, evolved as a reaction to the Lucas critique. Let us look at a simple example, as is typical for the RE school this assumes an "agent" maximizing "utility". Here look at a firm maximizing profits over an infinite time horizon. The criterion function is

$$Max_{N \to \infty} E_t \sum_{j=0}^{N} \beta^j [(a_{t+j} - w_{t+j}) n_{t+j} - (\gamma/2) n_{t+j}^2 - (\delta/2)(n_{t+j} - n_{t+j-1})^2 \ ]$$

here n_{t-1} is fixed, where n_{t+j} is input of a production factor to time t+j, w_{t+j} is price of the factor at t+j, and a_{t+j} is a stochastic variation in the production process that the producer observes; but that is unobserved to the modeler. γ , β, and δ are positive constants. It is assumed that w_t and a_t follows stochastic processes that are known to the producer.
This is a very typical example of an RE model of early vintage. (The linear quadratic models that can be solved explicitly is treated in some advanced examples by Hansen and Sargent (in Lucas and Sargent (1981)). In the present course we will concentrate on the later non-linear models; primarily because they need more specialized econometric methods (the subject of the course!) but they have also been more popular in recent years).
Hansen and Sargent (see p. 94 ff. in Lucas and Sargent (1991), and also Sargent (1987) App. A.5 for mathematical details) show that the above model has the solution

$$n_t \ = \ \rho n_{t-1} - (\rho/\delta) \sum_{j=0}^{\infty} (\beta\rho)^j E_t [w_{t+j} - a_{t+j}] \ ,$$

where $1/\rho$ is the smallest root of the equation $1 + \frac{\gamma/\delta + 1 + \beta}{\beta} z + \frac{1}{\beta} z^2 = 0$. Now assume for simplicity that $E_t a_{t+j} \ = \ 0; j > 0$ and that $w_{t+j} \ = \ \alpha^j w_t + u_{t+j}$ for all $j > 0$ where $E_t u_{t+j} = 0$ and positive parameter $\alpha < 1$. Then the solution is

$$(1) \quad n_t \ = \ \rho n_{t-1} - b w_t + a_t \ .$$

for

$$b \;=\; \frac{\rho}{\delta(1-\beta\rho\alpha)}\ .$$

This model demonstrates all the outstanding features of the RE school.
I. Heavy reliance on economic theory (but often with a representative agent),
II. All the stochastics is *part of the model*. (Notice that I made explicit assumptions about the process followed by w_t in order to solve the model.)
III. Cross-equation restrictions: To estimate the model you would use the equations

$$(2)\quad n_t \;=\; \rho(\gamma,\delta,\beta)n_{t-1} - \frac{\rho(\gamma,\delta,\beta)}{\delta[1-\beta\rho(\gamma,\delta,\beta)\alpha]}w_t + a_t\ .$$

and

$$(3)\quad w_t \;=\; \alpha w_{t-1} + u_t\ .$$

This is a simple example of cross-equations restriction, where α occurs in both equations. A note on identification. We can estimate ρ and b and α from equations (1) and (3). We would like to estimate the 'deep structural parameters' γ, δ, β, and α. ρ, b, α are functions of these parameters, and these parameters will be identified (can be estimated from data) if the multivariate function $\rho(\gamma,\delta,\beta), b, \alpha$ can be inverted. In this example this is not the case (there are too many 'deep parameters'). In non-linear equation systems there are no simple conditions for identification, so one has to examine on an ad hoc basis whether there is a unique solution in terms of the structural parameters. Here, the second equation identifies α, but it is not so easy to see if the rest of the parameters are identified - it is pretty obvious that we can not identify both β and δ, and my guess is that you would choose to fix β and the system would then be identified. We probably won't discuss identification any more in this course (and there are not really any general results), but be aware of the problem. I could also mention a 4th typical feature of linear RE models: they are often complicated. Some of the linear RE models that Hansen and Sargent developed in the early 80ies are very complicated (the above were very much simplified), and they may well be too complicated to estimate on most datasets, which may well be the reason why this type of models are not more popular. The point is that need more data to get 'reliable' estimates of 'complicated' non-linear models than you need for 'simple' linear models. What does 'complicated' and 'reliable' mean? You often need to determine this on a case-by-case basis using Monte Carlo simulations.

The example is also useful in order to illustrate the Lucas critique. The main point of the Lucas critique is that if one estimates the model for n_t, one will get an estimate of the parameter b, but if the model is not derived from a basic optimization problem the model will break down if an intervention (in most examples a policy intervention) changes one of the basic parameters, e.g. α. See Lucas (1976) (printed in Lucas and Sargent (1976)). The Lucas critique was seen as a scathing critique against the large Keynesian macroeconometric models;

but even though it was widely influential it is not obvious how important it is in practice (Sims (1982) argues that it may turn out to be more a cautionary footnote than a devastating critique.) In my view, the Lucas critique certainly is correct that model are useless to model major policy shifts, but practioners have always know that. In everyday practice at, say, the Fed, econometric models are used more to keep track of whether the informal forecasts made by economists are consistent, rather than for actual forecasting. They are also, to some extent, used to calculate marginal effects of changing fiscal and, in particular, monetary policy. The main point I want to stress, is that the bit macroeconometric models *never* generate forecasts *per se*.

For our purpose here it is most important to notice the model building strategy that explicitly takes into account the stochastic nature of the variables *at the model building stage* and typically imposes numerous non-linear restrictions on the parameters of the model.

Can one say that an economic theory approach is better than a statistical approach. I doubt it. There seems to have been quite a bit of discussion in the literature of various models' advantages (see the articles in Granger (1990)); but in general my point of view is that such a discussion is totally meaningless unless one discusses which method is best for a specific purpose. For short term forecasting a more statistically based model may be preferable; but if one want to test economic theory, the model has to be based tightly on theory. An appreciation of that point would, I think, make for a more enlightened debate.

1.4 My take on current work 5 year ago

Presently the dominating approach in main stream macro/general journals like the QJE is the "Cambridge" or "NBER" style, which stresses simplicity and transparency. The JPE (Chicago) and JME (Minnesota/Rochester) are a lot more sympathetic to 'deep structural' modeling (although the difference between the journal may be strongest in micro econometrics.) For a provocative methodological paper from an influential Cambridge macro economist see the article by Summers (1991). Summers claims that "fancy" econometric studies has had very little influence on macroeconomic theory and only very simple estimations or even tabulations have been convincing. [Summers doesn't define "fancy" (of just about anything else) but he is thinking about Hansen-Sargent style R.E. models.] Summers challenges the reader to find one simple instance where an econometric test has changed the way that macroeconomists think about the world. I cannot think of any example of that, but I think that is besides the point. As I argued previously, this is exactly what the "protective belt" analogy would make us expect, but this does not imply that a series of empirical papers (maybe not all performing

Tests with capital T) will not change the way that macroeconomists think about the world if they consistently reject (or confirm) a certain economic hypothesis.

Finally, the RBC-tradition decided that models should be explicitly General Equilibrium which, typically, results in models that are very complicated and impossible to estimate using standard methods. (The main proponent of RBC-modeling, Edward Prescott, expresses an almost irrational hatred of econometric work [his WEB-page used to state "PROGRESS DON'T REGRESS].") Instead RBC-economists calculate some simple correlations from the data and use the model to generate the same (if the model is true) correlations and the data-generated and the model-generated correlations are then compared. The problem with this (considered as empirical work) is the models typically have many variables that can be changed to match a few correlations (i.e., few degrees of freedom) and that the model generated correlations do not have any quantified measures of how precise they might be. (What I mean is that you don't have standard errors and t-statistics to help you gauge when the model is "close" to the data.) Many important insights have been obtained from such modeling—the point I stress here is that there are many problems in using the approach for empirical inference. Another issue is that many parameters in RBC-models are chosen from reading microeconometric studies. This has particularly many problems, for example, in the macro economy most substitution between labor and leisure comes from people (esp. older people) leaving the labor force, while microeconometric studies focus on prime aged males (sometimes females, but almost always in the prime working age). How can such parameters be assumed to fit into a macro model with a representative agent? For much more discussion of this issue, read Browning, Hansen, and Heckman's article in the *Handbook of Macroeconomics* 1999.

1.5 My take on current work

To get a feeling for current work you should skim the top journals. Macro still comes in many variations. Stuff that is basically theory, which I won't comment on here. Empirical work may still use "old fashioned" methods, *if* the are clever in some other dimension but otherwise empirical work is typically using "natural experiments" as instruments or a structural approach. Most things in macroeconomics are determined jointly so how do we estimate, say, an effect of income on consumption. Well, look for some (maybe unusual) situation where a government policy/oil find/natural disaster/...give shock to income and see what happens (but if you use government policy you need to make sure that this was a random change and not a response to economic conditions...wars are often good for this). It is hard to summarize this work, because it is quite scattered and I can think of a good reference for you. But I would like you to read Mankiw's little paper about the economist as an "engineer" or a "scientist." A URL is

`http://www.economics.harvard.edu/faculty/mankiw/files/Macroeconomist_as_Scientist.pdf.`

Also read the paper by Summers that I have posted. I is a “journalistic” piece...Summers uses a lot of terms that are not defined properly (that is what we need the methodologists for) but you will do well to remember his insistence that papers have to be convincing. (Not well defined, but you recognize it when you see it.)

“Minnesota type” economists no longer insists that shocks are of the RBC type but they insist on Dynamic Structural models and very often that it has to be General Equilibrium; i.e., they write DSGE models. Historically, by finding some correlations and writing a model to match but it has become more sophisticated with many people getting the facts to model from regressions or more elaborate descriptions, such a graphs. The most ambitious approach is to estimate the full DSGY using maximum likelihood or Bayesian methods. To my taste this is too ambitious (large parts of big models are stylized) but that is a matter of taste, I guess you can say that the more ambitious models give up being very useful for the economic engineer but may be pushing towards methods that will be the core of future economic research. (What I am trying to say is that you may have to choose between writing models close to reality or models that a methodologically advanced; the top journal reward the latter, but I always feel there is a tension where methodological sophistication threatens to focus on some dream world.) Instead of trying to explain more of this in a very short introduction, I suggest you read the paper http://www.econ.upenn.edu/ jesusfv/econometricsDSGE.pdf by Villaverde. He does seem to exaggerate how much of the profession subscribes to his point of view, but it is certainly a very active research area which he describes.