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Economists, Incentives, Judgment, and Empirical Work[1]

David Colander

In this paper I ask a simple question: Why has the "general-to-specific" cointegrated VAR approach as developed in Europe had only limited success in the US as a tool for doing empirical macroeconomics, where what might be called a "theory comes first" approach dominates? The reason this paper highlights is the incompatibility of the European approach with the US focus on the journal publication metric for advancement. Specifically, the European "general-to specific" cointegrated VAR approach requires researcher judgment to be part of the analysis, and the US focus on a journal publication metric discourages such research methods. The US "theory comes first" approach fits much better with the journal publication metric.

What I am arguing is that the research approach chosen is likely to reflect the incentives in the system. There will not necessarily be an invisible hand of truth that leads researchers to choose the research method that is most likely to advance understanding. Thus, even though the US "theory comes first" approach to macro econometrics may be a far worse way of understanding the macro economy, it may beat out the European "general-to-specific" approach because the proxy metric used to measure economist's output within the US economics profession is biased against the European approach. The approach could develop in Europe because, until recently Europe had a different incentive system that gave less focus on journal publication metrics, and thus was not biased against research methods requiring researcher judgment as part of the analysis.

The paper is structured as follows. First, I consider the incentive systems in the US and in Europe as they have evolved historically. Second I consider the European specific-to-general approach to empirical macro more carefully, and contrast it with the US theory-comes-first approach. Third, I consider the implications of differential incentive systems for macroeconomics, and relate them to the different paths that Europe and the US have followed in macro economic and macro econometric research. I conclude by relating the discussion to the current "reforms" of the European incentive system.

European and US Incentive Structures

Historically, European academic researchers' incentive systems for advancement tended to be more informal, political, and social than incentives facing US researchers. As Frey and Eichenberger (1993) point out, the differential assessment system in Europe and the US is explainable; in the US, there is an integrated labor market; in Europe, there was not, so in Europe there was less need to develop a generalized ranking system to compare researchers across borders. An important reason for this difference was that, until recently, European economics was fragmented by different languages and national borders, and a different promotion system that was based less on publications and more on subjective judgments that would develop over time. The smaller markets meant that

[1] I would like to thank Peter Kennedy and Katarina Juselius for suggestions on earlier versions of this paper.

all top economists in a country would meet. They could know one another personally, discuss with them, and were able to come to independent judgments about each other's work. Assessments based on quantitative output measures, such as publications, would matter, but they would be only one element in the subjective judgment. With the development of a common educational policy and a common language, the structure of European economics is now in the process of change, although the change is occurring slowly.

The informal European assessments were often quite vague and idiosyncratic by school and country; the assessments have relied on multiple subjective elements, which have often been difficult to determine precisely what they were. For example, in England, through the 1970s, judgments about who was good rather than publications guided placements. This meant, that until recently, it was considered unnecessary to get a PhD in order to get a job, and in fact the getting of a PhD in England meant that the researcher was not necessarily considered good enough to be hired in the market directly. Doing good work, impressing higher-level professors, measures of publications and citations, and positively interacting with other professors were all equally important (Frey and Eichenberger, 1993). As Britain has integrated its profession with the US profession, that is no longer the case.

While this informal assessment system has many problems, it has advantages as well. Specifically, judgment is given much more weight, and there is not a strong push to publish unless one has something to add to knowledge. The European system is now in a state of flux as schools adapt to the European common educational policy, but the changes are not as yet substantial enough to change my above characterization. (Some individual European schools which elsewhere I have characterized as "global" European economic programs, (Colander, 2008) have essentially adopted the US system, and there is strong pressure for others to do so as well.)

While all the above elements are important in the US, the US incentive system gives greatest emphasis on quantitative measures of quality weighted journal publications and citations. It is this difference in emphasis that I see as having lead to the differences in US and European economics. Because measures of journal publication output were key elements of judging a researcher's contribution, US economic researchers have focused their research more on "journal publishable research" than have European economic researchers. The measures of publication are continually evolving, as researchers learn how to game the existing measures. The need for publications has lead to an ever increasing number of "peer-reviewed" print journals (publication which count for tenure and promotion), even though, with the development of the web, print journals are a highly inefficient method of communicating research among researchers.[2]) Publishability of one's research affects all aspect of economic research in the U.S, including choice of research topic and research methods. It leads economists away from asking big questions, and instead leads them to focus on areas of research where data and results readily exists. Also, because papers longer than 25 journal pages are difficult to publish in a journal, research topics that take longer than that to explore are discouraged.

[2] I have explored this issue in Colander and Plum, (2005).

Addressing fundamental research questions, preparing new ground for innovative research, changing present paradigms requires painstaking, time-consuming research that requires continual interplay between empirical analysis and theory that are based on researcher judgment. Using journal article publications as the measure of an economist's output discourages work requiring interplay between data and theory, as well as work that requires judgment. Alternatively put, it discourages the creation of knowledge, and replaces it with the creation of journal publications.

The focus on journal publishability starts early on in US graduate school. In my interviews with US graduate economics students, (Colander 2006a) I found an enormous concern about publishability of research. Consistent with this focus, US graduate students have almost abandoned full-length dissertations, and replaced them with three-essay dissertations, so that the essays can be adapted into journal articles quickly. This was different in Europe, but is changing at global European schools to be identical to the US. I also found that students differentiate various types of papers that they write by the paper's usefulness for advancement. For example, some of my interviewees distinguished a job market paper from a journal article paper from a dissertation essay. A major professor at a top university tells students to spend all their effort on developing the job market paper, which demonstrates one's cleverness, and one's technical prowess, and can be easily presented in the 45 minutes one has for a job market talk. He further advises them to "blow off" the other two essays in the dissertation. Graduate education in the US has adapted to the US assessment system by focusing on preparing graduate students to become article writers, rather than preparing them to become general researchers; US graduate schools produce highly efficient journal article writers.[3]

There are a number of interesting elements of the US publication metrics; books count for little in most school's ranking system, and only peer reviewed journal articles count. The value of a book can actually be negative. One telling story is told by Paul Volker, former head of the Fed, who asked a young assistant professor to collaborate with him on a book. The assistant professor went to his chair an asked whether the chair thought it was a good idea. The chair said that one book might not hurt him; but he definitely should not do two.

My purpose here is not to assess the advantages and disadvantages of the incentive systems in Europe or the US. Both have advantages and disadvantages, and any assessment system of something as esoteric as "economic research" is necessarily imperfect and problematic. Problems are inherent in measurement; any measure used in an assessment will be gamed by the participants so that it does not achieve what it was meant to achieve. It is an inherent problem of output measurement and assessment. As long as a measure is not used for assessment, it provides useful information. The more it is used as an assessment tool, the more it distorts choices and does not measure what it is supposed to measure.

[3] Given journal publication's importance, it is not surprising that monetary values of publications and citations have been calculated. Sauer (1988) considered the value of a published paper to an academic economist, and found that a publication in a top journal was worth an annual increase in salary of over $1600; Diamond (1986) found that the marginal value of a citation was between $50 and $1300.

Talking about distinctive European and US approaches is difficult since there is much overlap in both training and employment, with many European economists being trained in the US, and then coming back to Europe. Moreover, the US dominance in economics over the past 50 years has meant that the US approach to economics has generally been seen as the global economic approach. Despite these integrating forces, there are discernable differences between European and US approaches. I argue that an important reason for these differences is the difference in incentives in the US and Europe. These different incentives have had a feedback affect on the way in which US and European economists approach problems and on the models they use. Two distinctions I see are (1) Europeans tend to be more eclectic, and to have a lower ranking of theory models than do US economists, and (2) European economists tend to be open to different approaches that may not have definite answers than are US economists. These distinctions are consistent with those which Bob Coats (2000) and Frey and Eichenberger (1993) listed in their comparison of US and European economics, in his summary of a set of studies of economics in various European countries.

The European vs. the US Approach to Macro

The difference between the dominant US and European approach to macroeconomics are striking and has been noted by numerous researchers such as Frey and Eichenberger (1993) and Coats (2000). The US approach to macro places theory first; the European approach is much more eclectic. This difference can be seen in recent theoretical developments in macro—rational expectations models, new classical economics, and the DSGE model; all have been primarily US phenomena.[4] Until recently, few European economists took an active interest in developing or exploring these highly theoretical models, and many considered much of this US work a fad. (Frey and Eichenberger, 1993) With the change in incentives in Europe, that is now changing. Consistent with that theory-first approach, in their empirical work US economists have focused recently on calibration, and have seen the goal of macro economists as developing a theoretical model that generates data that resembles certain moments of the actual data. European economists' approach has been more eclectic; the Hendry/Johansen general-to-specific" cointegrated VAR approach is part of that eclecticism.

The US approach to macro placing theory first might be called the "simplify and analyze" approach. In it, researchers create an analytically solvable model that captures some of the key elements of the economy, and then use that model for thinking about macro policy. An important element of this approach is that it separates theory and empirical work. Campos, Eriksson and Hendry (2005) summarize this approach as being one that "insists on a complete theoretical model of the phenomena of interest prior to data analyses, leaving the empirical evidence as little more than quantitative clothing" (pg 1). The institutional reason this approach is favored in the US is that it takes much longer to develop theory that is supported by empirical evidence than it is to develop theory alone. By separating pure theory from empirical work, the "simplify and analyze" approach leads to many more publications. US economists who follow it advance; those who do not, fall behind or move from macro to another area. Despite the dominance of

[4] I explore these developments in Colander. (2006b)

this work, most European observers and many US economists, including most non-macro specialists, see this theoretical work in macro as intuitively unsatisfying. The US approach to macro is not highly considered by many non-macro specialists even in the US, as is evidenced by interviews with graduate students (Colander 2006a). Even some older top US macro economists have been highly critical of it. For example, Robert Solow (2006) commenting on the state of macro in the US stated "the macro community has perpetrated a rhetorical swindle on itself, and on its students."

I describe the European approach to macro as the "complexity approach" (Colander, 2006) because it sees the underlying macro model as being far more complicated than any model that can be captured in an analytically solvable model. This approach has its foundation in Alfred Marshall's view of the complexity of the economy and his proposition that all aspects of the economy are interconnected. Campos, Eriksson and Hendry capture this view nicely when they state that "the economy is a complicated, dynamic, nonlinear, simultaneous, high-dimensional, and evolving entity [in which] "social systems alter over time; laws change and technological innovations occur." (pg. 1) This European approach strongly questions any simple macro model because it excludes aspects of the macro economy that intuitively seem highly relevant. A researcher taking this approach might work on theoretical models, but would believe that, given the state of our understanding, carrying that theoretical work as far as US economists have carried it is unlikely to lead to important insights. These differences are matters of degree. For example, most US macro economists accept that the macro economy is complex, but argue that light can be shed upon the macro economy by exploring much simpler theoretical models that are analytically solvable. This argument that exploring much simpler theoretical models can shed light on our understanding of the macroeconomy is itself an empirical claim; future historians of thought will consider whether or not it is correct.

The movement in the US to the "simplify and analyze" approach has evolved over the last fifty years, with the theory becoming more and more complicated and technically sophisticated. Back in the 1960s, before the journal publication ranking system became institutionalized, there was far less difference between US and European economists. But as those US economists who held the complexity view were weeded out through natural selection; (they either retired or were pushed out of macroeconomics for lack of quality-weighted publications), the differences became greater.

Let me now consider how these two different approaches to macro lead to different ways of doing macro econometrics. The European "general to specific" approach to macro sees the macro economy as extraordinarily complex and hence not susceptible to simple theories; it places empirical observation before theory. The US "theory comes first" approach to macro sees the economy as sufficiently simple that one can usefully theorize about it from first principles, and then use the results of that theorizing to guide empirical work. The difference between the 'general-to-specific' and the 'theory-comes-first' approach to empirical macro can be illustrated with the model analysis in Ireland (2004) and the discussion of that paper in Juselius and Franchi (2007).[5]

In his study, Ireland starts with the assumption that a simple RCB model can explain the US experience in the post second war period. He makes his theoretical model more ‘flexible’ by imbedding it in a DSGE model framework in which total factor productivity is assumed to be a stochastic near unit root trend driving the other variables. The paper is impressive, and is high-level cutting edge work to almost all economists who do not specialize in time series econometrics, which is the large majority of them. It was published in a good journal. By US ‘theory comes first" macro econometric standards, it is a good paper; it takes the model to the data and finds the model acceptable.

Now, the model is derived using the following assumptions: (1) All structural parameters are constant over time, (2) total factor productivity is driving the system, (4) log output, consumption, and capital are trend-stationary, (5) labor is stationary, (6) labor augmented technological progress follows a linear trend which influences the other variables identically, (7) the observable variables follow a VAR(1) process, (8) the errors are normally, independently and identically distributed. Each of the above assumptions is easily testable, but Ireland did not test them, or, rather, he did not report the test results. Nor did the reviewers for the journal that published the paper feel any need for the assumptions to be tested, because that is not the norm in the US “theory comes first approach”.

By European “general to specific” standards, it is not a good paper because in this approach testing assumptions is absolutely required. In this vein, Juselius and Franchi replicated the results in Ireland and tested the above assumptions. Essentially all of them were rejected. Even more seriously, when the model was reformulated based on the general-to-specific approach, the conclusions were reversed. For example, the results from the properly specified model showed that it is shocks to consumption that have generated the long business cycles, which is exactly the opposite of what Ireland finds.

There is nothing technically “wrong” with Ireland’s conclusions. It correctly states that, if all the assumptions it makes are true, then the empirical data match the DSGE model being tested. That qualified statement is correct. But, since none of the assumptions are in fact true, what does the qualified statement add to our knowledge of the macro economy? It is hard for an outside observer to see what it adds.

Ireland did not test whether the basic underlying assumptions were true because the US incentive system and commitment to “theory comes first” macro did not guide him to add to knowledge, but instead guided him to get a published paper. He was successful; the paper was published and widely cited because it used high-level econometric techniques, and because it brought a DSGE model to the data.

The European general-to-specific approach to macro econometrics does not see Ireland’s paper as successful because this would require that all empirical assumptions

[5] While Ireland’s work is chosen as an example, it should be seen as representative, and numerous other papers could have been chosen to represent the US theory comes first approach.

(whether explicitly or implicitly made) have been tested and confirmed by the data. In the general-to-specific approach theory and data must match, and if they don't match, then theory is either rejected as empirically relevant, or it is modified in the light of the information obtained from the empirical analysis. Thus, in the European approach the modeling process of the data provides both a critical framework and constructive insight.

For someone worried about publications the disadvantage of the European general to specific approach to macro econometrics is that it is a time consuming craft that has no precise ling of demarcation telling us whether data match the assumptions and model. But a specialist knows, just as wine critic knows whether a wine is a $10 bottle of wine or a $100 bottle. The existing blind peer review publishing system is not set up to handle papers that involve craftsmanship where, without extensively redoing the entire analysis, one cannot tell what the contribution of the paper is, and thus one must make the judgment based on how much trust one puts in the researcher doing the tests. Blind peer review journal publication articles specifically rules out assessment of the author's judgment to be a component of the review process, and thus, to be done correctly would require far more work than is almost even done in the real-world peer review; the reviewer would have to essentially rework all the analysis to determine whether the necessary judgments were reasonable judgments to an expert. For this reason the approach does not easily lead to publications. Thus, the European general to specific approach does not lead to as many journal article publications as does the US theory comes first approach.

It is not only in DSGE macro econometrics where the push for publication has guided empirical research in the US more than has the search for knowledge. It is in almost all areas of econometrics, as has been pointed out by a number of researchers, starting with Ed Leamer's "Let's take the Con out of Econometrics", (Leamer, 1983) and continuing through Larry Summers' (1991) attack on empirical macro, and through Deirdre McCloskey's attacks on statistical research placing more weight on t-statistics than is warranted and in mistaking statistical significance for substantive significance. (McCloskey and Ziliak, 1996). Much of the macro econometric work published through the 1980s could not even be duplicated (Dewalt et al. 1986) and the assessment that was held by many was that the informational content of many aspects of empirical research in macro was close to zero. (Cooley and Leroy 1981). Despite the concerns expressed about the informational content of the econometric studies, thousands of such studies were published in the US.

Distinguishing the Johansen methodology from pressing the J button

My argument that the European approach to macro econometrics is not conducive to success in journal and citation-ranking systems may seem somewhat strange given that both David Hendry and Soren Johansen, two of the leaders of the European approach do rather well in the journal rankings. For example, in one study, Johansen was found to be the most cited economist in the world. (Coupe, 2000) However, the large number of citations to Johansen's work on cointegration does not reflect the adoption of his approach to macro econometrics, which involves placing empirical observation—the statistical model—first, and then using theory to guide the macro econometrician's judgment when interpreting what that model is telling him. Instead, it simply reflects what happens when Johansen's cointegration techniques are used by individuals facing set of incentives driving them toward journal publications.

Considering the way in which Johansen's work has been integrated into the economics literature provides an example of what occurs when publications become almost an end in themselves. The problem is that the push for publication leads people to use the *Johansen procedure,* but not to use the *Johansen methodology,* which requires judgment and enormous amount of work pulling information out of the data. My claim is that while the Johansen procedure is widely used, the Johansen methodology, which is an embodiment of the European approach to macro, is not. The distinction between the method and procedure comes from a recent interview I did with Soren Johansen after talking about some journal articles in the US that used his procedure. In this interview he stated the following:

> So there is now something called the Johansen Procedure, which essentially involves pressing the J-button. That is not the approach I advocate. *What I advocate is an approach that involves a continual interaction between theory and data.* (My emphasis) You do it by continually checking to make sure that the model you are using, and the assumptions that you are making, fit the data. Once you know the model is reasonably OK, you can go ahead and apply the Johansen Procedure, but the first stage—the checking of assumptions—is absolutely necessary. If you analyze the data using a VAR model that does not take account of, say, non-constant parameters or residual correlations and then you apply the Johansen Procedure, what you are doing is completely inappropriate. It may look like you are doing sophisticated econometric work, but what you are doing can be almost worthless.
>
> When we developed this procedure we simply analyzed a statistical model by the general method called maximum likelihood. But before you use maximum likelihood, you have to be sure that you have the right statistical model. A lot of econometrics today is taught as cookbook techniques--you have technique 1, technique 2, and technique 3; econometrics is seen as applying techniques. That's not the way Katarina and I approach the data. We want to choose the method that fits the circumstances. Our method requires a lot more careful thinking about the problem than usually goes into the writing of an applied paper in econometrics. But this has nothing to do with cointegration, of course. But it has everything to do with applying statistical methods to data. It is something that has to be done very carefully. With modern computers, it is getting easier to do, but it is also getting easier to do wrong.

The distinction Johansen makes in these comments goes to the heart of the reason why the European approach to macro econometrics will have such a hard time integrating itself into the US. There is enormous pressure on US economists, whose future depends upon publications, (and, more and more on European economists who are at schools that have adopted the US ranking system) to press the "J button" or, more generally, to use whatever econometric technique that will come to a specific "publishable" conclusion. In a publish or perish world, researchers face strong pressure to use techniques that they may have some acquaintance with, but that they do not fully understand. The reason why this misuse happens is unfortunate, but understandable. Since new econometric techniques are continually developing, it takes enormous effort for applied macro economists to keep up with the theoretical developments. When there is strong pressure to publish, as there is in the US, researchers have an incentive to use econometric techniques that they have not fully mastered, and to focus on the technique, as long at the resulting research can get through the reviewing process.

The point that Johansen is making is that actually applying the Johansen method, or any advanced statistical method, requires high levels of judgment that have to be based on the researcher's knowledge of theory, institutions, theoretical econometrics and applied econometrics techniques. All are intricately related in an interactive process of data discovery interacting with theory. Econometrics does not provide a definitive answer; instead it provides a tool for researchers to arrive at a reasoned judgment. Econometrics is a science that necessarily involves an element of craft and art. Who does it matters because without extensive reworking of the entire data analysis, it is hard to make a determination whether or not the work is solid. The articles must stand on their own, which leads researchers to look for definitive cut-off points and decision variables. Peer-reviewed journal publications require econometric tests that give definitive results independent of the researcher doing the research. Using the Johansen Procedure does that; using the Johansen Method does not. Because the Johansen method requires that the researcher do far more work that is generally necessary to get a paper published, it is ill suited to an incentive structure that uses a journal publication assessment metric. That is the reason why I believe it has experienced a better reception in Europe, and why I believe it will have such a hard time being adopted in the US.

The problem is that journal publication is a poor metric for good data analysis. If the goal of researchers were to understand what the data are telling you, then the using the Johansen methodology makes the most sense, but if the goal is to publish the results in a peer-reviewed journal, then using the Johansen procedure makes sense.[6]

Good applied macro econometric research more often than not does not fit a journal article format, and there is little incentive to do good applied econometric research if the publication is an end in itself. Thus, while theorists such as Johansen and Hendry can do well in the journal publishing metric, those applied economists who want to follow their method, cannot.[7] True followers of the Johansen method will find it difficult to prosper in an environment such as the US, which is focused heavily on journal publication as the metric for advancement. The reason is quite simple; it takes a much longer time to do a paper using the Johansen Method than it does using the Johansen Procedure, and it is very difficult for anyone other than a specialized expert in time-series

[6] Since there are few reviewers who fully understand the Johansen methodology, and there are many journals, a researcher can almost always find a journal that will accept the paper.

[7] The easiest type article to publish using the Johansen method is one showing problems with existing research. Unfortunately, editors discourage such papers (it does not make them look good), and such papers are not advancing a true understanding of the macro economy; they are simply pointing out what is wrong with the existing research.

econometrics who has extensively worked with the procedure to distinguish between the two. When publications become the metric for advancement, a publication version of Gresham's Law applies: "bad" technique-oriented empirical research replaces "good" method oriented research, as those who follow the Johansen methodology do not get the publications needed to stay in the profession.

One could argue that having a publication that demonstrates a poor understanding of an issue will hurt a person's career, but that is seldom the case. Generally, few bother to actually correct a paper, since doing so will seldom lead to a publication. Each economist tends to focus on his or her own work. That is why Dewalt et al (1986) found that much of the research they considered could not even be duplicated, much less replicated. The reality is that having one's results challenged generally does not hurt the researcher. In fact, if someone later points out that the approach they use is wrong, and the journal editor publishes the criticism, they get credit for a citation. Moreover, they can respond to the criticism, and that response can count as another peer reviewed article.

One's immediate answer to the problem I am posing is that it is simply a statement that we need better reviewers.[8] After all, if we had expert reviewers who insured that all published work met the highest criteria of both theory and applied work, there would be no problem. Unfortunately, expert reviewers are in short supply, especially when new techniques are involved. One can only ask true experts to review papers so many times; the experts have better things to do with their time.

Concluding discussion

Let me conclude with a summary of my argument. From my nonspecialist's perspective, the European general-to-specific approach to macro econometrics in areas where replication of results and controlled experiments are generally impossible, and in which the system being studied is so complex that theory does not provide much guidance, seems to be a better way of doing macro econometrics than the US theory-comes-first approach. The approach is not used in the US because it requires the macro econometrician to be a craftsman, not a technician, and thus is not consistent with the peer review journal publication process. It requires high levels of author judgment in pulling information from the data and an ability to use theory to guide that search. The US incentive system which focuses on blind peer review journal publication involves a bias against approaches requiring judgment, and thus is not a good environment for such work. Until recently the more eclectic European incentive system gave more weight to judgment and provided an environment within which this approach could develop.

This presents a concern for me about the nature of the changes that are occurring in the European academic incentive system. While I agree that Europe's more eclectic incentive system has been characterized by much waste and enormous inefficiency,

[8] Alternatively, one could argue that we need a system of assessment in which doing less-than-stellar research has serious negative consequences. I know of no examples in economics where that has been the case, and I know of a number of examples where finding problems with one's empirical research has not hurt the person's career at all, (or has even helped it since they get additional citations and publications responding to the criticisms.)

(Kirman and Dahl, 1993), it is important to remember that that eclectic incentive system also allowed pockets of excellence to develop that did not have develop in the US. The European approach to macro econometrics is one of those pockets of excellence. Europe provides an alternative environment that is friendlier to research requiring judgment. As Europe moves to a common educational policy and changes its incentive system to further excellence in research, one can only hope that it does not eliminate the institutions that allows those pockets of excellence to develop in the first place.

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