

Putting econometrics in its place: A new direction in applied economics, Peter G.M. Swann, Edward Elgar, Cheltenham (2006). xiv + 250 pp., ISBN: 978 1 85898 305 9

Econometrics is the received technique for the applied economist, taught in just about every graduate school. Taught often to the exclusion of any other quantitative technique, econometrics is frequently presented as being applicable to any economic problem for which data exist. Swann's book is useful as an antidote to such teaching. Actually, it is more than a book; it is a manifesto intended, as its title suggests, to persuade quantitative analysts to treat econometrics as one tool of analysis rather than as the only tool. Experienced applied econometricians, including a subset of forecasters, will find almost everything in this book familiar, and will find much that they agree with. The book is well written and persuasive. Set against this, however, it contains no new ideas, nor does it claim to. It is also rather introductory in nature. The various complements to econometric analysis are not explored in any depth. Econometric forecasters, who are usually well aware of how their forecasts compare in accuracy with those from other methods, are likely to take little convincing of the limitations of econometrics.

Swann sees two problems with the present state of affairs in applied economics: isolation, and over-reliance on econometrics. Isolation shows itself in three areas. Econometric theory and practice operate in separate spheres. Academic and what Swann refers to as vernacular economics – the kind of understanding possessed by business people and consumers – are not connected. Consequently, academic researchers solve puzzles for which they have the data and techniques, rather than real economic problems faced by society. Finally, because academic reputation hinges on specialization, academics have little incentive to communicate and collaborate. This contrasts with the business world, where the goal of profitability demands collaboration among the parts of a company.

Swann also finds the status of econometrics as THE approach for applied economics (“the universal solvent”) to be a hindrance on its development, since it prevents the integration with other approaches where they are more appropriate. Here, he seems to have early economists and the founders of the Econometric Society on his side. Over one hundred years ago,

Jevons (1871/1970) argued that economic theory alone would not make economics a science. That would require the collection and analysis of data. The early vision of the Econometric Society was to combine mathematical tools, an understanding of theory and the use of statistical data to disentangle the complex causes found in economic activity. Frisch (1956, p.301) in reviewing the first 25 years of econometrics reflected on how hard it was to turn Jevons' vision into reality.

Why has econometrics achieved such a dominant position in the applied economist's toolbox? The beauty and appeal of econometrics are clear (p.78): (1) it allows us to summarize economic relationships without 'being there'; (2) it is very elegant; and (3) it is rigorous and objective. And econometrics has devised ways of extracting the most likely value of the parameter from the available data (through maximum likelihood) and of devising misspecification and specification tests that eliminate inferior models. Despite this, the attempts of econometrics to make use of limited information by restrictive assumptions (such as linearity) lead to reliable parameter estimates, albeit conditioned on the assumptions (and therefore possibly largely a consequence of the assumptions). Alternatively, using a very unrestrictive assumption (such as a flexible functional form) reveals that the available data are capable of generating only very imprecise parameter estimates. Swann argues that effort has been misdirected. Econometricians have focused on developing theory, which has advanced considerably in recent years, and neglected data gathering (which is harder, and has a higher cost, especially in advancing academic careers). They have made excessive use of secondary data, and reuse the same data sets too much.

What do the critics, many of them economists, say is wrong (chapter 5)? The list is long and will be largely familiar. Many of the problems stem from data limitations. (1) Economics is not a natural science. Therefore it cannot establish the physical constants that help considerably in modeling and forecasting the weather, for example. Parameters are local and temporary approximations. (2) Econometrics is a flawed tool: the need for measurable factors and the lack of variation hinder the discovery of causation. Omitted variables, spurious correlations, assumptions of linearity, and problems in specifying dynamics cause further difficulties. (3) Econometrics only works on a restricted domain.

“Economic truth is not always written in the form of equations” (Mayer, 1980) [p.39]. (4) The revolution in economics was a methodological revolution only: “In emphasizing mathematics and neglecting data collection, econometrics was trying to imitate the appearance of physics without following its actual procedures” (Wiener, 1964, pp. 90–91) [p.39]. (5) What Swann calls “trained incapacity”. According to Wiles (1984, p. 293), the main thing wrong with economics is its disrespect for fact: failure to gather primary data, neglect of others’ research findings, excessive use of axioms, excessive honor to theory, tendency to disregard contradictory evidence, preference for algebra over arithmetic, interest in puzzles not problems, use of stylized facts (convenient half-truths). (6) Economics is isolated from other disciplines, as a result of its preoccupation with econometrics. (7) Bricks from straw. “Economics is a one or two digit science” (Morgenstern, 1963, p.116, based on a comment to him by Wiener) [p.42]. (8) Investment in mathematics and econometrics has reached the point of “vanishing returns” (Friedman, 1991, p.36), in part because this is easier than investment in data collection.

When the signal of the cause-and-effect relationship vanishes in the noise of the system being modeled, econometrics has a problem. Some of this we know about. The effort to date has centered on fixing the problem associated with one definition of noise: the set of all other influences on the endogenous variables besides the explanatory variables of interest. Swann provides another definition of noise: the error of measurement (defined broadly) in the explanatory variables of interest. Applied economics in general, and econometrics in particular, has largely ignored the problem of measurement error. Griliches (1986, p. 1507) produces a nice example of the nature of the challenge: in 1982 the U.S. trade deficit with Canada was either \$12.8 or \$7.9 billion depending on which country’s publications one referred to. And these are countries with reasonably well-developed data collection services. Along with Morgenstern, Swann regards the low signal-to-noise ratio caused by measurement error as one of the biggest practical problems in econometrics. Unfortunately, his manifesto has very little prescription of what to do about it.

When measurement error is combined with multicollinearity, the result (according to Swann) is parameter estimates that are nothing more than noise,

because the errors caused by inaccurate measurement greatly exceed those from equation error. His solution to the problem is to increase the signal-to-noise ratio by imposing *valid* restrictions on regression models so as to reduce their dimensionality (*italics in original*). If this were easy, it surely would have been done, and it is to ways to accomplish this that Swann next turns.

Possible approaches to overcoming the limitations of econometrics include experimental economics, simulation, economic engineering, surveys, case studies, use of economic history, and interview. Swann appears to take a rather jaundiced view of the experimental economics approach. His criticism hinges on the legitimate concern that laboratory experiments, usually conducted with undergraduate students, do not carry over to real markets with real businesspeople; that is, they lack parallelism. His evidence of lack of parallelism is a single study by Burns (1985) concerning a simulated wool market. Students and professional wool buyers reached different market solutions, and did so because the rules of the game were insufficient to capture the institutional setting of the real wool market. The results differed because of different innate preferences of young students and older experienced professionals. I think the criticism misses the point of most experiments: they are to measure how changes in institutions (loosely the “rules of the game”) matter. This is different from trying to discover what are the innate preferences of individuals (from which, presumably, one could then predict their behavior in any given situation).

Survey questionnaires can elicit information from respondents that could not be collected any other way, for example purchase intentions, or valuations of non-market goods. But surveys, as is well known, are subject to various biases: hypothetical, interviewer, vehicle. The skills of questionnaire design and survey implementation are to either minimize biases or permit them to be measured. Inevitably that has limitations; for example, the concern of “hypothetical answers to hypothetical questions” is difficult to quantify without creating an actual market.

Finally, Swann calls for a change in attitudes (chapter 23). If the limitations of econometrics and the dangers of isolation are better understood, several benefits could occur: (1) We could increase the domain of research by searching for soluble problems

beyond those only amenable to econometric analysis. (2) By recognizing that econometrics is not the “universal solvent”, we could stop trying to apply it to problems for which it is not suited. (3) We could discover that other techniques being used in combination with econometrics can enhance the results. (4) We could reduce isolation, both from other disciplines and from vernacular economics. (5) Innovation in research techniques could be enhanced. (6) Finally, we could be reminded that research problems are multifaceted, and that different approaches are needed to explore different facets.

Should this book be on every econometrician’s bookshelf? Perhaps not, but it certainly would not hurt if it was there to be taken down once in a while. The practicing econometrician would be reminded that econometric analysis is not the only method, nor always the best method, and could usefully accept help from one or more of the other approaches that the book discusses.

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Econometric analysis of financial and economic time series, *Advances in Econometrics, Volume 20, Part 2*, Thomas B. Fomby, Dek Terrell (Eds.), Elsevier Ltd., (2006), 352 pages, Price, \$105, ISBN-10: 0-7623-1273-4, ISBN-13: 978-0-7623-1273-3

This book is homage to Sir Clive W.J. Granger and Robert F. Engle III for their gigantic contributions to econometrics, and at the same time congratulating them for receiving the 2003 Prize in Economics in Honour of Alfred Nobel. A group of (mostly younger) authors have contributed studies that in one way or another continue the work of the laureates.

The reader first meets an impressive list of the contributions of the two laureates in a formal dedication. Following the editors’ introduction, the laureates present their short accounts of how they perceive the creative processes they have been involved in. A quote from Engle’s comments: “There is nothing in our chosen career that is as exhilarating as having a good idea. But a very close second is seeing someone develop a wonderful new application from your idea”.

Nine of the thirteen essays are about modelling financial time series. They seem to be crowding out macroeconomic data as objects of econometric research. The reasons are obvious:

- (i) you can get thousands of observations, whereas with macrodata, a few hundred observations is a rare luxury, and
- (ii) the data are exact; no revisions are needed.

The long financial series are particularly useful for testing non-linear models using methods that do not require second-order stationarity, i.e., methods that have their roots in Granger’s and Engle’s work.

The first essay is by the well-known authors T.G. Andersen, T. Bollerslev, F.X. Diebold and G. Wu. The subject is the *beta* coefficient, measuring the risk of an individual equity as compared to the general market risk. Beta is simply the regression coefficient between the price of the asset and the market index. A debate has been going on for decades about whether these betas are constant or varying over time. The authors eloquently show that, although individual variances and covariances are highly persistent, the betas of