

It would perhaps be generally agreed that economic forecasting doesn't actually work, in the sense of producing accurate pictures of the future. Galloway says that "the forecast may not be accurate for any particular year" and this view is certainly justified by the facts. So if forecasts don't tell us the future, what do they do? One pretty clear and believable answer is given by Ball, where he says:

As matters stand in this general field of large scale model building, which is still as much an art as a science, there is much to be done - enough to keep a large body of researchers extremely busy over the next decade and more.

This idea, that the main purpose of forecasting is to keep a large body of researchers extremely busy (and supplied with food and water) is quite convincing, but does it benefit anyone except those researchers themselves?

A clue is given in the phrase "still as much an art as a science." The object of a scientific approach to forecasting would be to learn from the precise way in which a model failed as to how it could be improved next time round. The only infallible way of spoiling this possibility, and of stopping any learning taking place, would be to adjust the results in the light of commonsense, intuition, experience, general knowledge, inside information, hunch, prejudice and general zeal for improvement. This is, it seems, exactly what happens. Anxiety about being wrong far outstrips the desire for scientific purity. And so we get statements like - "The model, however complex, is generally an aid to judgement of a very specific kind, not a substitute for it."

This inability to decide whether to be scientific (and, as Berdy says, to get the benefit of an iterative series of experiments) or to lean heavily on judgement extends so far that it even leads to contradictions within the same article. At one point Macdonald is advocating a behaviourist approach which essentially involves Newtonian determinism, while at other points he denounces determinism and the Newtonian approach.

So it seems that the researchers doing economic forecasting have a nice little number going. They are like Penelope the wife of Ulysses, who sewed her tapestry all day and undid it again at night. Only it is even more brilliant, because the researcher can be ever so scientific while he is being watched, and leave it to someone else to provide the adjustments which destroy the pretence to science.

Well, supposing that we set up some kind of a self-denying ordinance, and said that scientific method must reign supreme, would that be the answer? Suppose that we gritted our teeth, and forswore subjectivity, and really kept abiding by the rules of the experimental game, would that ultimately give us more useful predictions?

The answer is in the negative. And the reason why the answer is so dusty is because of the Inverse Ratio Law of Forecasting. This law says that once the limits of commonsense have been passed, each extra increment of time and money spent on model-building will give less usable results. Let U be the Utility of the predictions made; let C be all the costs of using Commonsense in whatever way is optimal; and let M be all the costs of making and using large-scale economic models in whatever way is optimal. If the object of the exercise is to maximise U, the equation runs:

$$U = C + \frac{1}{M}$$

An illustration of this law is given in the article by Professor Ball, where he says of the model produced by the organisation he works for that it "produced 25 pieces of information in 1966 as compared to 375 now. This has not meant that the quality of the forecasts has improved in any dramatic way ..." What it has done, of course (as well as making more work for more researchers), is to make any prediction extremely hard to understand or check. And it has been remarked more than once that figures are most sacred when least checkable.

One of the most important reasons why models get worse as they get bigger is that more and more of the figures put in are fiction. This is often not realized, though it comes through in many of the articles in this series. Estimates and guesstimates, assumptions and trends, hypotheses and insights, rules of thumb and generalizations, all come in again and again.

The other key reason why the models get worse is that the number of untenable assumptions increases. One assumption which is frequently built into economic models is that people operate as if they were examples of Rational Economic Man (or REM, for short). REM was invented to make life easier for mathematicians: you can build lovely mathematical models once granted REM. But none of these models apply in the real world because real people do not think like REM, and their behaviour does not correspond to his thoughts. The whole economic theory of consumer behaviour and household behaviour (as put forward by people like Green, Johnson and Devletoglou) is just ludicrous - it ignores all that we have found out about consumer psychology. I often feel that economists would actually prefer to use assumptions and estimates, rather than to use real data.

The fearful danger of using models is that you may begin to believe in your own magic. The Bloom and Stacey article gives a good example of this, where one can see the authors, despite all protestations to the contrary, moving further and further towards a fully automated system which is expected to become more and more believable. The reason why it can never be trusted is given in their own article. They say that causal models (like the interesting one given by David Lowe Watson in his article) are more flexible than extrapolative ones, but add that they are susceptible to the same inherent problem: "new or previously dormant factors can suddenly assume importance". This is of course true; what they do not seem to see is that this means that a dialectical logic is needed, rather than an Aristotelian one. Unfortunately computers do not at present have this facility; only human beings do, and not many even of them are able to use it in any conscious way. This is why, as Ball says - "Identification of these dynamic patterns is a crucial and difficult aspect..."

If I could finish by suggesting a modest experiment, it would be this. Take a forecast given by your favourite economic model, for a period of not less than a year ahead. Obtain an estimate, as accurately as possible, of what it cost you to get this estimate, in the form in which you are using it. Then spend one-tenth of this amount on getting as good a commonsense forecast as you can, using if possible at least one person able to think dialectically. And then compare the two, on whatever criteria seem to you to be the relevant ones. These might include general accuracy; the ability to make necessary decisions; the seriousness of any errors; success in alerting you to key things to look at further, control more tightly, etc. - and in general, any of the aims which forecasting claims to deliver. (For a true experiment to test the law, you would of course have also to order a more expensive forecast prepared by a bigger model.) If, after several trials, it turned out that my cheaper suggestion was the most cost-effective, it would presumably make sense to ditch the formal models completely.

The one thing my alternative would not give you is the ability to boast about what a big model you've got, and how much it costs you to run it. But that wouldn't worry you, would it?

Editor's Note:

The above article was a review by John Rowan of a recent series of articles on forecasting in Admap, a magazine read largely by people working in advertising and market research. This review was not accepted for publication by Admap. We felt that it raised a number of issues of interest to RS readers.

P.S. (from J.E.):

Readers may be interested in looking at the last 3 chapters of John Rowan's The Social Individual, vol. II (Davis-Poynter 1973), which I found a very stimulating introduction to the use of statistics and research methods for not-too-numerate social scientists.