

THE ECONOMIC  
AND SOCIAL  
RESEARCH INSTITUTE  
MEMORANDUM SERIES  
NO. 141

THOUGHTS ON PERUSING A RECENT PAPER ON THE  
ECONOMETRICS OF TIME SERIES

R. C. Geary

**Confidential: Not to be quoted  
until the permission of the Author  
and the Institute is obtained.**

Thoughts on perusing a recent paper on the econometrics of time series

I use the word "perusing" as milder even than "reading", still less "studying", because I understand only parts of the paper I base these thoughts on. It is true that any worthwhile paper has parts hard to understand. I have not made this effort in this case, for reasons I hope to make clear.

I refrain from citing title or authorship of the paper, since this might be unfair, and certainly ungracious, on the part of an old man about the work of probably young authors, who must not be discouraged. The particular paper is still in typescript. I have found the things I complain about here in many published papers. I want this comment to embrace them all and finally to come up with useful suggestions. An old researcher must also be on his guard in his criticism of new work, which may be based on his resentment at having to study afresh, or plain old age jealousy.

A citation of principle: in the social sciences we are engaged in improving the welfare of mankind. Improvement implies the prior ascertainment of the relevant statistics and other facts. These have to be analysed, often using mathematics and other abstruse methodology and terminology, in a word scholarship. But improvement means decision-making and doing, in turn involving often vast expenditures. Decision-makers, doers and spenders should be able to rely on the afore-mentioned works of scholarship, not exclusively, for they may have to take into account political considerations to which scholarship may not extend. As Tinbergen has remarked, the contribution of the science of economics to the solution of the economic problem is necessarily small. But the point is that doers etc, who have to possess their own expertise, cannot be expected to understand all the scholarship in the preparatory studies. Nor can they be expected to accept the findings in fact and the recommendations towards action of the scholars, without understanding at some level. There is, of course, a real difficulty here, Scholars cannot in

every study go back to first principles, in mathematics or anything else. Almost invariably the latest paper depends largely on the scholarship of others. Summaries in simple language, i. e. adapted to the mentality of doers, may do serious injustice to the work of scholarship, may even be misleading. Intermediaries may be necessary between scholars and doers, persons who can understand the scholarly works, who also know the problems of the doers and who therefore may be able to exact from the scholarly papers what the doers need to know, in the doer's language, and without undue distortion of scholarship.

Of the understanding necessary for action, scholars' understanding of the doers' needs must be easier than the doers' understanding of works of scholarship. It seems to follow that the scholar should take over a larger part of simplification than would be expected of the doer in regard to mathematics etc.

The author of a recent volume of abstracts of socio-economic papers informed me that, in general, authors were not good at summarizing their own papers. Perhaps authors should submit themselves to guidance in this important regard.

A simple rule for the scholar in the social sciences would be as follows (1) citation in the simplest terms, of the problem tackled, (2) methodology adapted to the solution of the problem, (3) citation of this solution in plain language. The problem may be the ascertainment of facts, involving merely orderly presentation and simple analysis, and not the solution of a social problem, but the threefold categories can be adapted to the latter type. As to (2) above, if significant results have been forthcoming from very abstruse methods, the researcher will be wise to try to obtain them using simpler methods. From his/her point of view this may act as a check on unexpected results, and full comprehension will tend to break down potential doer - resistance. The simpler the result the more likely its acceptance for action.

This preamble has gone far further than intended. Back to the paper, but repeat, for general inference.

The problem tackled is a very important one: is A the cause of B, or vice versa, A and B being time series? It might be thought that if there be relationship but if A precedes B in time but no relationship in the inverse case, then A is the cause of B.<sup>+</sup> One can be by no means sure if this proposition is accepted by the present authors or their authorities, of which there is a multitude: there are over 30 references. There is a mathematical introduction with formulae not explained, i.e. in highly symbolical terms, first to achieve white noise residuals by lag operation on transformed original variables. In a section entitled "Empirical Results" there is citation by author's name and date of methods of filtering but with a statement "Serious doubts, however, have been expressed about the appropriateness of using these statistics to determine the relationship between two series ... Moreover, we have found that the detection of the presence of a relationship between [A] and [B] using the cross-correlation coefficients is sensitive to the choice of joint filter used to generate the innovation". The last part of this quotation seems to damn a large part of the methodology of the paper, which contains other remarks of this derogatory kind. At the end of page 7, there is a joint white noise model of two equations expressing the residuals for log first differences of A and B in terms of fourth order autoregression terms. Of the four terms in each, only the coefficients of one in each is significant by the t-test (if this is what the bracketed figures under the coefficients - as is usual - mean). Why then use the other six terms? On page 9, there are auto - and cross-correlation coefficients, all negligibly small to the eye, for 19 lags and simultaneous (lag 0), 80 figures in all, of which only 8 are asterisked as significant at .05 N.H.P.\* Then why the rest?

There is page after page of figures with little, if any, attempt at interpretation, except that they are somebody approach to something or other. Though the to - do is about time series there is no mention of DW. Has this fallen

---

\* In a large series of figures a few conventionally "significant" may not be significant at all.

+ A colleague points out that action A taken now may be in anticipation of B in future so that precedence in time is not necessarily causal.

on evil days? Another offence: original data, for only two variables, 62 quarters, not supplied. Worst offences of all, perhaps, are a few  $\bar{R}^2$  of value .999, indicative in time series of spurious relationship only.

And the conclusion of all this brou-haha: (1) A and B "are not independent", (2) A "is informative about, or useful in predicting" B, (3) "There is also evidence, however, that causality runs from" B to A ... "By this we mean that [A] does not rise in response to a higher [B] " ... "Doubling [A] eventually doubles [B] ". There are a few other inferences, none important. Knowing A and B, some of these statements of results are so guarded as to be nearly self-evident; what is certain is that they could have been derived by far simpler statistical methods than those used.

And yet it is quite evident that the authors are au fait with relevant modern scholarship; to repeat, there are over 30 author-references to their paper and it is reasonably clear that the authors are familiar with the papers they quote, which notoriously is not true of all authors in citation of references. What may be wrong is the scholarship itself in this field. One must sympathize with young researchers. Their work must be derivative. It must start at the level of the current work of others, hopefully adding a little to it. There are thousands of researchers the world over of whom few will make a real breakthrough. It was different during the earlier statistical period. All we had to do was to help abreast of R.A. Fisher, the greatest statistician who ever lived.

The writer must be frank and admit that he is not familiar with modern statistical scholarship. He is nowadays more interested in social problems. He has never been at a loss for a statistical (methodology learnt long years ago) for dealing with problems he has tried (not that he has often solved them!) and severe critics of other aspects of his work have never faulted him for use of outdated statistical methodology.

The practice of reproducing in papers voluminous computer printouts with little comment should be discouraged. The computer is marvellous, particularly for an old researcher who had to do most of his calculations himself. But something has been lost compared with the old days, though the net gain (from the rapid, vast capacity of the computer) has been immense. Most people who have done their own calculations have found that during the process they have noticed or suspected relationships or simple arithmetical facts (e.g. an exceptional figure in a sequence - why?) which might not have occurred to them on viewing a table of figures prepared by others. The fact remains that computer printouts should be a means and not an end.

At the risk of spreading this note beyond its intended limits the writer may mention that he has recently read a paper in an international economic journal ostensibly (by title) on the measurement of real output of public services. As a one time government statistician, baffled by this problem of estimation of volume of all services I read the paper eagerly, for enlightenment. I was disappointed. The paper contained not a hint of statistical methodology. Of course the problem raises general economic and social problems, and the paper in question had many remarks on these aspects to my seeming rather trite generally, but not a word about statistical methods.

In this case this paper contained exactly 30 references, some of which may have been statistically enlightening but, if so, the author did not say so. It is another case of voluminous referencing designed to make the paper scholarly respectable and to make it acceptable for publication. I should add that there is one case in which referencing is essential, namely in those invaluable papers giving an account of the up to date state-of-play in some branch of research, papers unfortunately rarer nowadays in statistics than formerly, perhaps because of number of papers all over the world to cope with.

It is all very well to recommend that elaborate citation and even genuine knowledge of relevant authority should not mean automatic acceptance of papers for publication, but to know what to do about it. Scholars in the particular

discipline must be the judges and usually they will be the first to admit that they may be wrong. They can have no prior knowledge about the interest the paper may create amongst other scholars, probably the best ultimate test of the paper's worth. Heaven forbid that this writer should involve himself in a discussion on the value of knowledge, where judgement may be determined by aesthetics, e.g. the use of the word "elegance" in connection with mathematics. Judgement of value may be easier in the social sciences by having regard to the simple rules outlined in this note.

In a word, name dropping is not enough, - and editors and responsible readers must have the courage to say so, in regard to social science submissions. Young researchers should be encouraged to tackle problems occurring to (or suggested to) them on their own in the first instance, i.e. without consultation of authority. They may make real advance this way alone but what is important is that, having done so, in most cases they will know their problem better. It is at this comparatively advanced stage in their thinking that they should look up references. Then they will do so critically, comparatively, selectively and economically, for they will know what they are looking for, to help solve their problem. So originality of treatment and result in significant degree may be attained.

10 October 1980.

R.C. Geary