

## Tribute to the Life and Career of Clive Granger

Dear Clive,

The organizers of this NBER-NSF Time Series Meeting asked me, as one of your PhD students, to talk about your research. It is very likely they would like me to talk about your cornerstone pieces:

- **"The typical spectral shape of an economic variable"**, *Econometrica* 1966
- **"Testing for causality and feedback"**, *Econometrica* 1969
- **"Prediction with a generalized cost of error function"**, and **"The combination of forecasts"** (with J. Bates), both in *Operational Research Quarterly*, 1969
- **"Spurious regressions in econometrics"** (with P. Newbold), *Journal of Econometrics* 1974
- **"An introduction to long-memory time series"** (with R. Joyeux), *Journal of Time Series Analysis* 1980
- **"Co-integration and error-correction: representation, estimation and testing"** (with R. Engle), *Econometrica* 1987.

BUT NOT, I will not talk about these papers for two reasons:

1. You don't like repetitions (I discovered that from the second year Advanced Econometrics course you taught us in 1987. You only spent 20 minutes talking about cointegration out of 30 HOURS)
2. The audience know more than me about these papers and no need to get embarrassed.

INSTEAD I have decided to talk about some of your less known papers. Of course I have to decide which ones (this sounds like selecting a model.....). My selection method has been a very simple "naïve" one. I went to my old file cabinet in my office and looked for GRANGER. Presumably these are the papers I most like. I will be missing those papers ordered by topics and many important others (for instance I will miss the paper I co-authored with you about common permanent components). For these reasons, very likely my selected papers (models) would not pass a real evaluation test for many different cost functions (importance of the journal, future impact, etc.) and I know how many times you have repeated to us that models need to be evaluated and evaluated and evaluated; BUT in this occasion I do not think I have to worry about the evaluation step, and I hope you do not mind if I skip it.

I will present this not so well known research following to your already mentioned cornerstone pieces and I hope you allow me to introduce from time to time what I think was your opinion on the different topics.

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### PERSISTENCE

For you Clive TRENDS have always been the most important component of an economic time series. You always commented that being TRENDS so important it is very rare to find a definition of them in econometric textbooks. You were always intrigued that trends are the components that we analyze the least. We estimate it to eliminate it later on. We should analyze the consequences of a bad trend specification on the other components we analyze most. You always thought this is an area with a *permanent* big future (global warming, population, prices, production...etc).

In "**Models that generate trends**" (JTSA 1988) you define them and analyze models that are able to generate different types of trends (slow varying, regular varying, etc)

In "**Non-linear stochastic trends**" (with T. Inoue and N. Morin) (JoE 1997) you analyze under which conditions on  $g(\cdot)$  and  $\sigma(\cdot)$  in

$$X_{t+1} = X_t + g(X_t) + \epsilon_{t+1} \quad \text{and} \quad E[\epsilon_t^2 | X_{t-j}, j=1, 2, \dots] = \sigma(\cdot)^2$$

$X_t$  will grow ( $P(X_t \rightarrow \infty) = 1$ ).

In "**Consideration of Trends in Time Series**" (with Hal White) (2009) you give a big set of operational definitions of different types of trends and consider the important issues that should be analyzed in this area.

Under the topic of persistence I would like to include a paper I like a lot:

"**Stochastic unit roots**" (with N. Swanson) (JoE 1997). In this paper you thought it was too strong that the "unit root parameter" was exactly equal to one and instead decided to model this coef. as a random variable with expected value equal to one. One could think that if you are able to find an explanation for the variability of this new random coefficient you would be finding a reason for the existence of unit roots.

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### CAUSALITY

Under this item I did not find any WP in my GRANGER file. It is true that you did not write much about it after your paper "**Some recent developments in a concept of causality**" (JoE 1988). I wonder why? In any case, I looked in the cabinet where papers were ordered by topics and there under CAUSALITY I found "**Causality in the long run**" (with J. Lin) (ET 1995) where you considered causality at different frequencies (short and long-run causality do not need to go together).

I will adventure and say something more. I have the feeling that you knew your definition of causality maybe was not the definition of "true-causality" and for this reason philosophers ignore your definition until very recently; BUT you knew that your definition should be part of the true one and as an operational definition maybe very difficult to improve. Your definition of causality is a predictability test. So you decided to work on issues related to forecast theory. Of course this is only my opinion.

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### FORECASTING

In my opinion FORECASTING is your big topic, since you declared in High School that you wanted to be a meteorologist. Are there any other guys that forecast more than them? Maybe economists?? No, I do not think so

In my cabinet, under GRANGER, I found four papers:

**"Measuring lag structure in forecasting models: The introduction of time distance"** (1997). Most of the forecasting measures are measures in the vertical axis like the MSPE. In this paper you proposed to use also the horizontal axis and introduce the TIME DISTANCE (the smallest "k" such that  $y_{t+k} < x_t < y_{t+k+1}$ ). With this measure it is found that random walk models are not always the winners as it comes out very often from the MSPE distance.

In **"Thick modeling"** (with Y. Jeon) (Economic Modeling 2004) the ecological principle (nothing is thrown away) of combination of forecasts is extended into modeling. Combination of models is a way of dealing with the uncertainty of modeling specification. THICK versus THIN. In practice we need to trim the worst models. By doing that it is found that a linear combination of non-linear models outperforms combination of linear models forecast wise.

**"Evaluation of panel data models: some suggestions from time series"** (with L. Huang) (1997). For you, panel data models were one of the future topics of research. The profession has to learn how to analyze large data sets that come in at least two dimensions (cross-units and time). You never understood why the panel people did not evaluate their models. You mentioned that it is very common to hear them saying: *....here is our panel data model and it works....* In this paper you propose that panel data models should be evaluated Post-Sample (time dimension) together with Out-Sample (N dimension).

**"Forecasting and decision theory"** (with Mark Marchina) (Handbook 2005). There are two actors in this paper: Forecasters and Decision Makers. Forecasts have to be evaluated according to the use we made of them (utility function) and we have to be aware that behind any loss function there is a corresponding utility function. If this were considered to be absurd then maybe we should not use that loss function for evaluation purposes.

You were worried that we are not forecasting better than 20 years ago. You used to say that we are able to do things to increase the quality of the forecasts a little bit; BUT the big step still was missing. I never knew if you had anything in mind about how to give this big step. Was combination of forecast in different

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set-ups (combination of forecast intervals or of forecast densities, etc) what you had in mind? Was a different way of extracting information from errors of different forecasts? Was the use of survey data? Maybe there is an impossibility theorem that says that in Economics is not possible such a big forecast improvement or quoting Yogi Berra: *The future ain't what it used to be.*

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### SPURIOUS REGRESSION

**"Spurious regressions with stationary series"** (with N. Hyung and Y. Jeon) (1998). This is a simple Monte-Carlo paper very useful for teaching this topic. It is very obvious that spurious regression is a Critical Value issue. With the right cvs (taking into account the serial correlation present in the error term) the spurious regression problem disappears (at least asymptotically). This is the case in the standard stationary world; BUT it is not in the  $I(1)$  case. You always thought you should have written this paper 20 years earlier. I wonder which would have been the consequences of it.

### CO-INTEGRATION

After the famous paper on co-integration you wanted to go a bit further, although not always succeeded (of course at the same level would have been impossible). You succeeded with the papers on **Multicointegration** with Tae-Hwy Lee. Many people pursued that line (for instance the  $I(2)$  people). That was not the case with your paper **"Conjugate processes"** (with J. Lin) (1995) where you looked for linear combinations that are white noise. As a by-product you start re-thinking again on forecasting white noises. Also the paper **"An introduction of co-monotone processes"** (1998), defined as processes such that the sign of their increments is the same, did not achieve what you may have had in mind: to use this concept to analyze non-linear relationships.

Under this topic, a paper I liked a lot and I think you were very happy with is **"Modeling nonlinear relationships between extended memory variables"** (Econometrica 1995). In this paper you show very clear that the concepts of  $I(0)$ ,  $I(1)$  are nice concept for the linear world but inadequate for the non-linear world. In this world we have to worried about the balancedness of our regressions. For this you define the concepts of short and extended memory to analyze non-linear cointegration. And from them on you declared non-linear models one of your research interests for the XXI century.

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## LONG MEMORY

I hope you do not mind I use this topic as an example of how I think you did your research. Your paper with Joyeux (1980) introduced long-memory via aggregation. For many years you did not work in this hot topic because you did not find any good example in economics. Later on in the paper "**A long memory property of stock market returns and a new model**" (with Z.Ding and R.Engle) (JEF 1993), you find that the absolute value of the returns clearly behaves as a long-memory process and get back into this topic (very hot at this time). An example is your paper with Z.Ding "**Varieties of long-memory models**" (JoE 1996). You realized that there are many models that can generate long-memory properties and fractional integrated ones are a very particular case. For instance switching regime models or duration models are other alternatives maybe with more economic sense ("**Occasional structural breaks and long memory**" (with N. Hyung) (1999). You decided that the decision has to be taken on the forecasting ground and in "**Non-stationarities in stock returns**" (with C. Starica) (RStat 2005) you compare the forecast ability of I(d) models with linear models + breaks. The latter seems to be the winners. Was this fact what did take you to be so critical with standard long-memory models? I do not know.

And continuing talking about how did you work let me mentioned to the audience that you always travelled with a notebook, no the computer one but the one with yellow pages. In it you kept writing things. For instance, I remember when we went to Cordoba in a fast-speed train, you were writing stuff about a project on how to forecast things that had never happened before (it was a grant application with Frank Diebold). Were you maybe thinking in the actual financial-economic crisis???

Before finishing let me tell the audience what were you working on + other things.

## RECENT TOPICS you were working on

- Time Varying Models ("**Non-linear models: Where do go next—Time varying parameter models**"). You like non-linear models but did not like their forecasting performance. Maybe time-varying parameter models (via Kalman filter) perform better in this dimension
- How to deal with large sets of information. You thought quantiles was the right "animals" to model. You had this interesting idea, even with one single time series, on modeling the quantiles through time together with a new quantity called spacing (the difference between two quantiles)
- Trends

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A RECOMMENDATION for the audience

Your book **Modelling nonlinear economic times series** with Timo Terasvirta and Dag Tjostheim.

A NOBEL PRIZE FORECAST

Maybe you do not remember; but one day in Madrid (just after you were awarded the Nobel) in the auditorium of my University (U. Carlos III de Madrid), while we were waiting for some questions from the audience I asked you about who do you think could get the Nobel Prize in Time Series Econometrics and after some seconds you said: **Jim and Mark**.

A CURIOSITY

I imagine that you would like to know that your random walk work with Oscar Morgesten got three pages in one of the best sellers of this year: **The Myth of Rational Markets** by Justin Fox. I wonder how would you had appeared in another recent bestseller **Outliers** by Malcom Gladwell about how people become extraordinary people. Let me try:

*.....A pronunciation problem created a Nobel Prize in Economics. Professor Granger wanted to be a meteorologist when he was asked at the High School; BUT because he had problems pronouncing that word he always ended up saying... "I want to be an statistician " UNTIL he became one, and a really good one.*

Later on in the same book the author would write in a deeper sense about

*....how professor Granger was able to create SIMPLE ORIGINAL BREAKTHROUGH very influential econometrics work.....*



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And now Clive I have to finish. The whole profession is already missing you and believe me that is not only because of your original simple influential work, it is also because of your *GENEROSITY* no so common in this academic world, your *GENEROSITY* on sharing ideas (I have not found yet anyone comparable with you in this dimension), your willingness to help your students (always in a hidden way), ..... In a much tinnier dimension knowing that you were working as hard as always even after being emeritus and having obtained everything in the academic world, it would have been great to see the research production of your collaboration with two other UCSD professors: Mark Machina and Hal White (we know how well did go your collaboration with Rob). I will miss that too.

WE HAVE BEEN VERY VERY VERY LUCKY KNOWING YOU and this is what we want to remember (+).

Say Hello to the dolphins surrounding you now in the Pacific Ocean,

And instead of THE END like in the movies I will say

THANKSssssssssssssssssssssssssssssssssss

for having known YOU.

Jesus Gonzalo

(UC3M)