Eastern Economic Journal, Volume XVII, No. 1, January–March 1991

A Scientific View of Economic Data Analysis

Cornelis A. Los*

". . . econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system."

(Leontief, 1982)

WHAT IS THE PROBLEM?

The presumption of the regression method, so widely used in economic and financial analysis, that the model is known even before the data are analyzed presents a severe problem when viewed from the viewpoint of the customary method of science. Ragnar Frisch who shared the Nobel Prize with Jan Tinbergen in 1969 and Tjalling C. Koopmans who shared the Nobel Prize with V. Kantorovich in 1975, proved that if a number of historical data series are observed and covaried, an equal number of mathematically equivalent, ordinary (or as they called them at that time: elementary) regressions can be based on the same data covariance matrix. More recently, R. E. Kalman (Kyoto Prize winner in Advanced Technology in 1985; 1987) has again drawn the attention of the econometrics profession to a finding which should have at least given pause to the indiscriminate application of ordinary least squares (OLS) and its various derivatives, such as principal components. Yet, econometricians and other economic analysts proceed with the *a priori* selection of one plausible "equation" or "model" and indiscriminately apply the conventional least squares regressions scheme to compute "estimates" of the parameters of such assumed linear relations. The results of the alternative, or reverse, regressions are ignored. That is regrettable from a scientific point of view, since the results of these reverse regressions often conflict disturbingly with the results of regressions selected on the basis of *a priori* theory. The implication is that scientific evidence that might (and often does) falsify a theory is being ignored.

Let me illustrate the problem by a recent empirical example that relates to a proposed correction of the 1988 second quarter GNP growth rate estimate from 3.0% to 3.8%.[1] The claim was based on the computational results from a single equation quarterly GNP regression model that contained five variables. The new estimate was within the range of plausible forecasts derived from the conventional GNP-component "build-up" procedure and was widely considered non-controversial. Yet, it is easy to show that a model consisting of one linear relation could have produced five different estimates by regressing each of the five different variables on the other four, using exactly the same historical data, exactly the same regression methodology and exactly the same input statistics for the second quarter of 1988. These five estimates for second quarter GNP growth in 1988 would have been −0.2%, 1.8%, 3.8%, 6.6% and 7.4%, at annualized rates. In other words, using the same data, methodology and inputs, the model estimates actually ran from a growth recession, −0.2%, to a feverish inflationary growth pace of 7.4% real GNP growth.

Two interesting observations may be made: first, the range of growth projections encompassed virtually all the second quarter GNP growth rate estimates offered at that time. Second, the selection of

*The Netherlands Chamber of Commerce in the United States, Inc., One Rockefeller Plaza, New York, NY 10020, and Research Associate, Center for Mathematical System Theory, University of Florida, Gainsesville, FL 32611

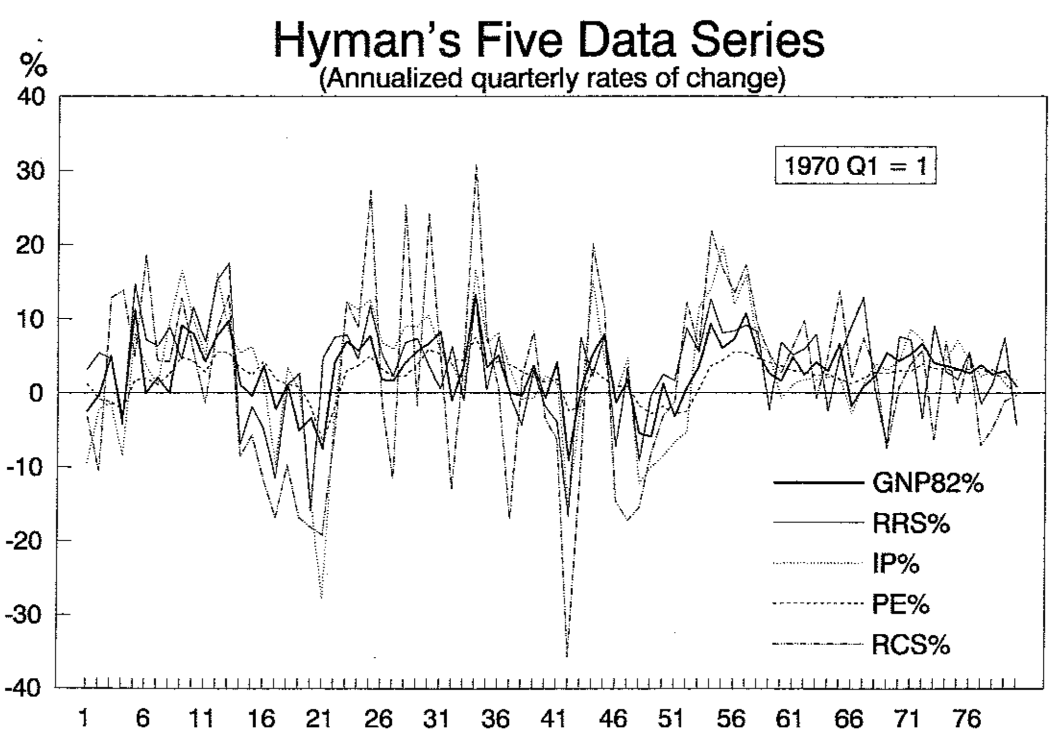
The comments and suggestions of Rudolf E. Kalman, Jan C. Willems, Joos Vandewalle, Emmanuel Parzen and the Editor, are gratefully acknowledged.

the middle estimate as being the most plausible was prudent, but based on personal judgement. Reliance on plausible selection is not unique, as any economist or financial analyst can testify. But the widespread and deep misunderstanding of the regression methodology customarily used to analyze "noisy" economic and financial data is a problem. To understand why estimates can differ so much when the data, methodology and inputs are exactly the same, it is necessary to distinguish three separate phases in the scientific method: (1) measuring and collecting the data, i.e. the measurements of the signals which are contaminated with noise; (2) organizing the data and "purifying" them from noise to recover the signals, i.e. the exact parts of the data; and (3) analyzing the exact signals to find a logical (more precisely, mathematical) explanation for their generation.

It cannot be emphasized too often that, in science, *the data* are, literally, *the givens* that cannot be altered or "enhanced" by extraneous assumptions.

Scientific methodology often, but not always, combines phases (2) and (3); data are often decomposed into their exact and noisy parts simultaneously with the identification of the mathematical system, or model, that explains the exact parts of the data. The problem is that the regression method starts with phase (3) and *presumes* the model that ought to be identified from the data. Furthermore, it can easily be proved that regressions *always* "fit" the data, since they always produce the (artificial) noise, i.e. the residuals that make the presumed model compatible with the data. It follows that individual regressions are useless for scientific research. However, it may come as a surprise that regressions can still produce useful results when used "comprehensively," i.e. when all the possible regressions are analyzed together. They can then be used as a test to see if the underlying system is a single equation system or a multi-equation system. In addition, we know that they can be used to determine how many independent linear relations the underlying system, that generated the data, actually has.

In the case of the empirical example, the five data series are the time-series of the quarter-to-quarter annualized percentages change of real GNP, real retail sales, industrial production, payroll employment and real construction spending, plotted in the following graph.



The five corresponding regressions are presented in Table 1, together with the projected estimates they produced, using the statistical inputs for the second quarter of 1988, as published.[2] In terms of the conventional terminology, these results are the original regression, together with the four respective reverse regressions, each normalized on the GNP growth rate for the purpose of comparability.

TABLE 1

Five Conflicting Model Results Obtained by Ordinary Least Square Regression from Historical Data

Models:	Possible Q2 Real GNP Growth Forecasts:
1. GNP82% = 0.7 + 0.1 * RRS% + 0.2 * IP% + 4.0 * PE% + 0.1 * RCS%	3.8%
2. GNP82% = 2.4 − 0.4 * RRS% − 0.2 * IP% + 0.3 * PE% + 0.8 * RCS%	6.6%
3. GNP82% = −10.4 + 0.4 * RRS% − 1.2 * IP% + 5.8 * PE% + 0.1 * RCS%	7.4%
4. GNP82% = 4.7 − 0.3 * RRS% + =.8 * IP% − 3.0 * PE% − 0.1 * RCS%	1.8%
5. GNP82% = −3.3 + =.8 * RRS% − 0.4 * IP% + 1.6 * PE% + 0.3 * RCS%	−0.2%

Data: (quarter to quarter % at annual rates, 1970 Q1–1988 Q1)		Hyman's Q2 Inputs:
GNP82%	= Real GNP Growth	—
RRS%	= Real Retail Sales Growth	0.9%
IKP%	= Industrial Production Growth	4.6%
PE%	= Payroll Employment Growth	3.4%
RCS%	= Real Construction Spending Growth	7.1%

Most economic analysts who look at this table will be inclined to accept the first equation as "plausible" and discard the signs and magnitudes of the coefficients of the other four, because the coefficient values in these four equations are thought to be "not in agreement with economic theory."

This is a grave mistake, since *economic theory has nothing to say about the signs and magnitudes of these single equation coefficients.* Although it is plausible to assume that retail sales, real construction spending, industrial production and payroll employment are positively related to real GNP growth, economic theory does not specify this quantitatively, nor does economic theory dictate the presence of one simple relationship among these five variables, or a system of multiple relationships.

WHY SHOULD WE BELIEVE ECONOMISTS?

Based on historical data and using regression methodology the forecast of 3.8% real GNP was false, even though considerable professional judgment had been used to select a plausible value from the wide range of estimates allowed by the data and a single linear relationship.

Personal ability to make prudent judgments are held in high esteem by the many who pay considerable sums for investment advisory services. But, the question *why should anyone believe the estimates of any economist* can be raised, *if they are not based on the data alone* but also on the judgments of economists and other financial analysts, which are accepted because they are "experienced," "established" or "wise," and because they have, for some mysterious reason, "authority." That "authority" cannot be derived from their theory, since that theory can now be shown to have no scientific foundation, only a speculative philosophical one. Is their "authority" then based on the use of special econometric methodology? It has been established algebraically that statistical methodology is seriously flawed (Kalman, 1987, 1990; Los1989a). We have to conclude that the judgement (good or bad) involved in the example, and in similar cases, is not scientifically established. In many respects, we are still in prescientific times, as far as economic research, analysis and decision-making is concerned. (Hendry 1980).

The above example was selected not because it is professionally worse than others. On the contrary, the example was produced by someone who is part of a very large society of professional economic forecasters, who continue to use this flawed regression (and equivalent) methodology. These shortcomings have been extensively documented. Professional economists and market analysts often differ in their opinions, even when confronted with the same data and using the same methodology (McNees 1981, 1986; McNees and Ries 1983; see also Mahmoud 1984). Other examples abound. For example, in *The Wall Street Journal* of December 31, 1987 the leading article had the revealing headline: "Fickle Forecasters. How Three Analysts, After Crash, Revised Economic Predictions. Their Views Differed Greatly, *But All Changed Them Without Computer Aid*" (Emphasis added). "Reputable, well-educated economists examine the same event, yet reach often strikingly different conclusions," the author of the article commented (Wessel 1987).

It is clear to most people that economic forecasting still amounts to little more than educated guessing, despite the aura of precision created by computerized models of the economy. Not enough research has been done to understand why this costly and wasteful situation persists, notwithstanding the enormous expansion of the capabilities of computer hardware and software and the developments in linear algebra (Los 1986).

This exceedingly simple and recognizable example helps to lift a tip of the veil of our ignorance: most economists, financial analysts and other statisticians do still not understand the intrinsic logical properties and consequences of the computerized number-crunching apparatus they routinely implement. Historical research shows that some of these disturbing intrinsic properties were exposed already more than fifty years ago (e.g. Frisch, 1934), but then were conveniently ignored, particularly after the acceptance of probability theory as a major tool in economic data analysis (i.e. after Haavelmo, 1944).

To return to the example, from an objective algebraic and scientific point of view, the additional four reverse regressions are not only as legitimate as the original regression chosen, they are *necessary to obtain complete identification of the complex system* that generated these particular covariance data. Only by running all five regressions and observing the conflicting signs it is established that the system must consist of more than one simultaneous, independent, linear relationship. The reverse regressions are legitimate, because regressions are straight-forward algebraic procedures implemented on the computed covariance matrix of the original data series. The *a priori* assertion that some variables are free of noise, i.e. the subjectively chosen "regressors," while some variables contain noise, i.e. the subjectively chosen "regressands," is scientifically indefensible.

The computational burden of the new comprehensive procedure is minimal: in practice, one does not have to run the regressions: by just inverting the data covariance matrix and arbitrarily normalizing on one of the data variables, one can check the discordance of the column, respectively, row signs (See Los 1989b for a detailed example). Strictly speaking, checking the adjoint of the data covariance matrix is already sufficient. The choice of the normalization is immaterial; one could just as easily normalize on any of the other four variables. Normalization is only for the trivial purpose of comparison. The sign-values intrinsic in the data do not change.

Just postulating a single linear relation based on a judgmatically chosen part of the historical data, combined with statistics of the present to forecast the "future" is insufficient, since such a single relation can be shown to *not completely* summarize the complexity of the system that produced the original data. *Such a single equation can not "realize," or reproduce, all ten observed data covariances.*

It should now be clear that the current institutional arrangement of the financial markets resembles a huge racetrack with heavy betting on certain horses e.g. the investment advisors' recommendations, complete with "bookies" (now euphemistically called "account executives"), since scientific economic analysis, in the true sense of these words, still does not exist. It is not much different from the days of John Maynard Keynes, except that he used the naive analogy of a game of "Snap, Old Maid, or Musical Chairs" to describe the phenomenon (Keynes 1936). The critical system identification of the complex economic and financial data is still left subject to the prejudices of "tribal economic and financial seers." Most of these "seers" appear *not* to have an understanding of the actual problems encountered in the *scientific* identification of the systems that produce the noisy data, although they sometimes exhibit a strong intuition about how the systems work.[3]

THE POINT OF MODELING

Let us now put these issues into a somewhat broader context. If there is a point to collecting data at all, building models from data is clearly a necessity, not only in economics, but in all fields of research. Models serve to condense and clarify data, for insight into the mechanisms generating data, and for predictions, synthesis, *et cetera.* Modeling is still a quasi-monopoly of statisticians i.e. professional data analysts who tend to give *a priori* a probabilistic interpretation to the data. In the field of economics, econometricians have been performing this function, since Haavelmo (1944) suggested the use of probability theory for economic data analysis. It is ironic that in 1989, forty-five years later, Haavelmo received the Nobel Prize in Economics for this erroneous suggestion. His suggestion is erroneous, since statisticians have clearly not solved, or even understood the noisy identification problem. *The fundamental problem of identification from noisy data is a matter of mathematical system's analysis, not of statistics.*

The present paradigm used by statisticians is to look at all data as a sample output of a (hypothetical or even metaphysical and certainly not verified) probabilistic "machine." Statisticians ask "is the sample big enough?", "Should I get more data?" The tool for this type of work is elementary (Kolmogorov) probability theory, which clearly does not address the issue of system complexity. However, we should be interested in what the data can tell us about the underlying system, not in the noise overlaid on the data. (It is highly questionable to assert that noise is stochastic). The questions of the system engineer are different: "Do we have enough information about the system in the data; are the data complete?", "What is the signal/noise ratio?" and "How are the data related to the system?" The tools for this kind of research are pure and applied mathematics (not statistics) and powerful computers. It is hard to explain this unpleasant fact of life to professional practitioners, e.g. to modeling and data analysis groups at banks, investment firms and other financial and economic institutions.[4] Shortly after the article on the Fickle Forecasters was published, *The Wall Street Journal* published another lead article: "What Becomes of Data Sent Back From Space? Not a Lot, as a Rule." Very few people noticed the connection between the two articles, because we are now so used to compartmentalized thinking, that problems, that affect several fields of the human research enterprise, tend to fall between the cracks. Kneale, the author of the second article, wrote: "The mismatch between raw data and real analysis, worst in space science, also shows up in energy exploration, aircraft design, medical imagining, engineering *and other fields.*" Indeed, the mismatch is horrendous in the meteorological, biological, economic, financial and other fields dealing with extremely complex systems.

The need for more data and better data processing is clearly illustrated by the progress made by meteorology in the past three decades. Meteorological forecasts serve many purposes, but usually they have a clear cost/benefit analysis attached to them in both the agricultural and touristic industries. These cost/benefit evaluations caused the US government to spend substantial amounts of money on improving forecasts by increasing the density and the quality of meteorological data collection (e.g. by bringing more weather satellites into orbit). This development has demonstrated the importance of accurate scientific measurements for forecasting in the so-called non-experimental, observational sciences (von Neumann 1955; Kerr 1985). But the current forecast accuracy proves very hard to improve further. The last ten years of research and several new generations of supercomputers have pushed the horizontal limits of medium-term forecasting, which is wholly dependent on computers, from five days out to just seven days. But beyond seven days, forecast chaos takes over, partly because the data are not sufficiently accurate and partly because the weather system dynamics are still not correctly identified from these historical data (Kerr 1989).

Millions of economic and financial data are now stored in computer files, but they contain very high noise levels, since most of them are, in one form or another, based on survey samples. Much higher signal/noise ratios are required than currently provided. For example, anyone who has monitored the U.S. Department of Labor releases of the nonfarm employment data in 1989 and the subsequent gigantic—sometimes more than 100%—revisions, has had first-hand experience with extremely low economic signal/noise ratios. Higher signal/noise ratios can be achieved by further increasing the density and comprehensiveness of the data collection and applying higher standards of accuracy. However, the recent developments in weather forecasting make it clear that higher signal/noise ratios *per se* are insufficient for better understanding. Higher signal/noise ratios are only a necessary ingredient for less ambiguous identification.

In the second phase of scientific research, high technology, improved (super-) computer hardware and software are now combined to summarize these voluminous data in tables, charts and, increasingly, in interactive, three-dimensional high-resolution graphics, that can be easily visualized, digested and acted upon (Bell, Miranker and Rubinstein 1988; Markoff 1988). Three examples will illustrate these advantages of scientific visualization: a macro example from global meteorology, a micro example from pharmaceutical biology and a meso example from foreign currency trading.

First, the Antarctic hole in the earth's ozone layer was discovered from the enormous amount of data transmitted from space by presenting the ozone densities, measured by spectrometers carried by satellites, in three-dimensional projection on the earth's globe (Kneale 1988). Second, we can now look at the three-dimensional structure of serum albumin, the most abundant plasma protein of the human circulatory system (Carter *et alii* 1989). A detailed knowledge of the 3-D structure of this serum is imperative to understand how it binds substrates, such as calcium, copper, amino acids, hormones and therapeutic drugs for rational drug design. Third, visualization can contribute to enhancing the efficiency of financial markets. Real time expert systems, such as TARA, now assist currency traders by automatically analyzing five series of financial data for trends, by displaying on-screen the essential facts in two dimensions and by making recommendations for trades (Waldrop 1989).

We tend to better understand data in a visual, topographic form. However, system theorists know already for a long time that two- or three-dimensional pictures are of little help for understanding systems whose complexity exceeds what can be represented as a three-dimensional order. The eight generic ways in which computer scientists are able to visualize the data, mentioned in *The Wall Street Journal* article referred to above (Kneale 1988), are insufficient to deal with multidimensional noisy identification problems. This implies that algebraic, non-visual analysis of data remains crucial to gain understanding of the structure and operations of complex real (economic) systems. Two-dimensional visualization of aspects of system behavior, like produced by TARA, is incomplete and therefore insufficient. Systems can behave in ways that can not be inferred from two-dimensional graphs.

LESSONS FROM THE EMPIRICAL EXAMPLE

Let us return to the example, because it should be now clear that in a complete science we *have to solve* the problem of how to identify simple, algebraic models from the summarized multidimensional noisy data. Computing covariance matrices is one approach to eliminate some of the noise in the data. But it is clear from many empirical examples that further decomposition of noise from such data matrices is necessary to get the exact, or reduced, covariance matrix that implies the exact (linear) model.

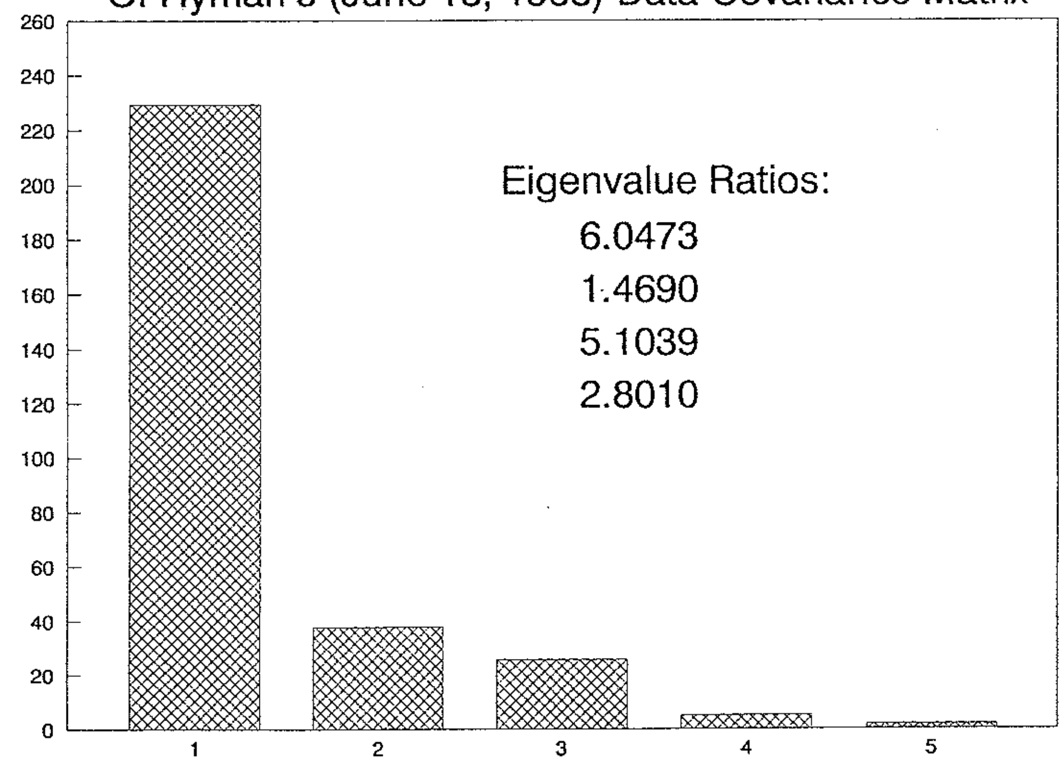
In our example, the five historical data series of each 73 data points are summarized in a 5×5 covariance matrix. The scientific problem is to identify from this noisy matrix the exact system of linear algebraic equations that describes the exact covariance matrix in an objective fashion, without human judgment. In other words, ideally, when these data are fed into a computer, only the computer software should separate the noise from the data and automatically identify the linear system, without human interference. This is now possible in principle for relatively low noise data. For example, the computer can now answer the obvious but crucial question: how many independent linear relations are necessary to completely reproduce the exact part of the covariance data?

As we observed, the data tell us very quickly that there must be more than one independent linear relationship, and of course, less than five relationships, to describe the covariance data completely. For a complete analysis we have to look at *all* other possible regressions. Thus, if we initially presume that there is one linear relation, we can run the $5!/1!4! = 5$ regressions and check for the sign consistency. If there is no sign consistency, as is the case here, then we presume next that the system consists of two independent linear relations. Consequently, we have to check the results of the $5!/2!3! = 10$ regressions with two independent linear relations; and, if still no sign consistency is found, the $5!/3!2! = 10$ regressions with three independent linear relations; and, if there is still no sign consistency, we have to check the final $5!/4!1! = 5$ regressions with four independent linear relations, and check for sign consistency.

Kalman's Theorem (Kalman, 1990) states that if the presumed number of independent linear relations is smaller than the true number that produced the data, the parametric results will lie very close to an identifiable hyperplane. In other words, the set of regression results show a "noisy" linear dependence. (A simple plot of all the regression results can often be extremely revealing to show this). If the presumed number of independent linear relations is correct, there is total sign consistency (That is the result we try to find). If, finally, the presumed number of independent linear relations is larger than the true number of independent linear relations, the computed results show a large degree of "noisiness," and can lie virtually anywhere in the hyperspace of possible coefficients.

For our example, it turns out that the implementation of Kalman's Theorem shows that the true number of independent linear relations has to be four to be sufficient to show sign consistency. In other words, based on the data alone the complexity of Hyman's system is 4/5, four independent linear relations based on five variables, and not the presumed 1/5, one linear relation based on five variables. The remaining uncertainty caused by the noise is then expressed by the allowed, but limited and strictly bounded range of the computable model coefficients. This exact conclusion is independently corroborated by the screeplot of the eigenvalues resulting from the spectral decomposition of the data covariance matrix, which shows one large eigenvalue, indicating that the five series, indeed, vary closely together.[5] However, it should be emphasized that such a screeplot is by itself insufficient to establish our result, since the eigenvalues are computed from the noisy data covariance matrix and not from the exact reduced covariance matrix and, therefore, contain noise themselves. Often, in cases where the eigenvalues lie close together, the "break-off" point can not be established in unprejudiced fashion.

Eigenvalues From Spectral Decomposition
Of Hyman's (June 18, 1988) Data Covariance Matrix



The implication for the economic data analysis of this example is rather ironic: each of the four indicators, the annualized quarterly growth rates of real retail, industrial production, payroll employment and real construction spending (for which monthly data exist and also good end-of-quarter estimates of quarterly sums or quarterly averages), can be used separately to compute the annualized quarterly growth rate of real GNP, and thus also of each other. All five variables are just noisy multiples of each other! There is no real gain from taking a sort of weighted average of all four, as was done in the original example. The only relevant criterion should be the relative tightness of the four individual independent linear relationships, which can be derived from the computed ranges set by the regression LS(5,4) boundaries on the system coefficients; i.e. the boundaries of the five least squares regressions with four independent linear relations. The reason is that least squares regression does find the minimum amount of noise compatible with a given set of system coefficients. Using that criterion, the tightest ("best") preliminary indicator is industrial production, with annualized quarterly growth rates between 1.5 and 2.3 times those of real GNP, followed by second "best" payroll employment, with growth rates varying between 0.3 and 0.8 times those of real GNP. Notice that each of the indicators is positively related to GNP growth (as we expected).

TABLE 2

Possible LS(5, 4) Coefficient Values a_i^j

j	i = 2 RRS%	i = 3 IP%	i = 4 PE%	i = 5 RCS%
1.	1.279	1.696	0.413	4.048
2.	0.696	2.120	0.804	1.961
3.	0.984	2.335	0.589	2.232
4.	2.622	1.676	0.329	2.865
5.	0.885	1.508	0.419	1.984

Notes: (1) There are 5!/4!1! = 5 LS(5,4) regressions (j = 1, . . . , 5)
(2) The system consists of 4 independent linear relations as follows, normalized on GNP82%:

$$\begin{aligned} \text{GNP82\%} &= a_2^j \cdot \text{RRS\%} \\ &= a_3^j \cdot \text{IP\%} \\ &= a_4^j \cdot \text{PE\%} \\ &= a_5^j \cdot \text{RCS\%} \end{aligned}$$

It is now clear that even this simple problem of system identification from five noisy variables produces quite complex scientific questions that cannot be quickly answered from running only one single equation regression. This suggests that one can safely dismiss the exaggerated claims of those who build large-scale econometric models with thousands of equations based on only a few hundred variables, using the conventional methods of regression, principal components or factor analysis, and their multitudinous variants (Loss 1989a & b).

CONCLUDING COMMENTS AND SOME ADVICE

Where does this leave investors and policymakers, who necessarily rely on economic and financial forecasts? Not much different from where they were on December 29, 1930, 14 months after the Black Thursday stockmarket crash, when Frisch, Schumpeter, Fisher and 13 other sympathizers founded the Econometric Society in Cleveland, Ohio.[6] These founders formed the new Society with the specific mandate to identify the economic systems ("laws") that govern our lives, from the available noisy economic and financial data (Frisch, 1970, p. 226; see also Christ, 1984). Of course, there are some differences: more data than ever before is being collected and displayed, more computer power than every before is available. More importantly, some progress has now finally been made, as demonstrated by Kalman's recent work. Although the essential noisy system identification problem has not yet been completely solved, neither in economics, nor in any other scientific discipline, despite the valiant efforts of the members of the Econometric Society in the past 60 years, the first important steps have finally been taken towards a new and truly scientific paradigm.

This paper has proceeded from Leontief's (1982) complaint that none of the conventional economic modeling schemes can pass the mathematical system analyst's test of *objective* modeling: feed data into a computer and let the computer identify what system generated these data, *by algebraic means only,* subject to the condition that it should not matter in what order the various variables are being fed in. Since objective modeling has not been practiced, economics as a science has not progressed. This stringent objectivity criterion eliminates the currently popular method of "state-space modeling" from being scientific, since variation in the order by which the data are fed in produces different identification results. It also renders "neural network modeling" unscientific since the adaptive neural networks still need human judgment for calibration to produce their identification results.

For the moment, investors and policymakers should remain fully aware that the thousands of forecasts floating through the global markets today, are still based on contestable personal judgment, "tribal wisdom" and "established authority," or (too often) on ephemeral "rumors," and *not on scientific evidence,* no matter how impressive the glossy packaging. There is, indeed, a very good reason for monitoring the track-record and experience of our economic "seers," as McNees has done in the past decade. And it remains strongly advised, to take competitive polls among these "druids of tribal economic wisdom," as is now regularly done by *The Wall Street Journal, Business Week* and other popular publications. These polls show two results:

1. forecasts continue to differ widely, despite the availability of the same data and processing technology, *ergo* the individual forecasts must be subjective; and
2. there is a "consensus" median expectation, reflecting the best available "tribal wisdom," not to be ignored in the markets.[7]

Contrary to some earlier pessimistic visions (Worswick 1972; Mayer 1980), I am optimistic that economics as a science is a realistic goal. Kalman's and my own research, which uses artificial intelligence programs, e.g. symbolic algebra, to analyze the data, now strongly suggest that progress is possible. But that progress will come as in meteorology or as in pharmaceutical biology: slowly, haphazardly and piecemeal, propelled by the forces of technological development. Recently, simple cost/benefit analysis has created strong financial incentives to obtain better and more accurate economic forecasts in the private sector. But, paradoxically, the main obstacle to this progress in economics is the conventional pseudo-scientific methodology of econometrics adopted in the 1940s and 1950s. The conclusion is clear: first the problem of objective identification from noisy data has to be solved. We have made already a new beginning. But, currently, *economists still practice rhetoric, not science* (McClosky 1985).

NOTES

1. In the June 18, 1988 issue of *Weekly Economic Data,* Edward S. Hyman of C.J. Lawrence & Co., a well-known investment advisory firm in New York. On June 27, the official preliminary GNP estimate was released by the Department of Commerce. Later, after many subsequent revisions, that estimate rests at 3.7%.
2. Anyone who tries to replicate these results using the current revised figures, will notice small differences in the computed coefficient values, but not in their signs. The relevant data, as of March 30, 1990, are available from the author on request.
3. It is only recently that the public has been taking notice of this deplorable situation, but it has already cast a clear vote. In the past five years, some of the largest econometric forecasting services and economic departments of large "Wallstreet" firms have been forced to restructure, to merge, to be down-sized and sometimes be phased out altogether. The public's demand for economists/econometricians has considerably weakened, while that for mathematicians has commensurately strengthened. In addition, the consumers require now a more sophisticated "unbundling" and pricing of the investment advisory services (What are they really worth?). Will these

restructurings really change the deplorable situation? In my opinion *not,* because the essential system identification problem has not been solved. The financial industry has still not learned its lesson. It has just appointed another group of "seers," the so-called "quants," who use a slightly more sophisticated form of "freebase modeling."

4. In fact, a very awkward situation has arisen. Although the research problem of system identification from noisy data is very clear, its research must be undertaken against considerable entrenched inertia of those who would most benefit by new knowledge. This is not a new situation. It has happened before in history (e.g. genetics) and it is happening in fields other than economics and finance.
5. As mentioned earlier, it is not necessary to run all these computations. In fact, it is rather surprising that the only real computation is the inversion of the data covariance matrix, and, strictly speaking, the determination of the adjoint of the data covariance matrix is sufficient. In other words, possible numerical problems, like extremely small determinants of the data covariance matrix, do not occur. The identification problem boils down to the tasks of implementing correct combinatorics and of the checking of signs. Both these tasks, as well as the computation of the data covariance matrix, are accomplished by computers extremely well and with lightning speed.
6. Irving Fisher was elected the first president. The first European meeting of the Econometric Society, with 20 participants, was held in Lausanne in 1931. The first volume of *Econometrica* appeared in 1933, financially supported by the investment adviser Alfred Cowles 3rd. Cowles had discontinued his investment service in 1931 following his failure to foresee the stock market crash of 1929. His first article in the new *Econometrica* (Cowles, 1933) was entitled "Can Stock Market Forecasters Forecast?" Its contents were neatly summarized by the terse, yet complete, abstract: "It is doubtful" (Cowles Commission for Research in Economics, 1952), a judgment that still stands. It is regrettable that in the course of its existence, the Cowles Commission has not fulfilled its own mandate, that is summarized in its logo, "Science Is Measurement."
7. Partly to protect myself against the wrath of my colleagues, I emphasize that it is still better in the long run to consult a professional economist like Hyman, than a non-schooled crackpot. (Of course, I do also not mind that my colleagues and friends make a living with "economic storytelling"). There remain three acceptable reasons for consulting professional economists that are similar to why you would consult professional, licensed physicians if you suffer from an ailment: (1) professional economists are, at least, familiar with the data. Crackpots, per definition, aren't; (2) professional economists respect the "consensus" of their peers, i.e. the "tribal wisdom," that can positively influence the markets. Crackpots, per definition, don't; and (3) professional economists have a "feeling" for the complexity of the economic systems. Crackpots, per definition, haven't. Of course, listen always to the advice of *minimally three* economists before making a decision of substance.

REFERENCES

1. Bell, C.G., Miranker, G.S. and Rubinstein, J.J. (1988), "Supercomputing for One: Interactive, High Resolution Graphics and Vector Processing Combine for the First Time in These Desk-Side Units," *IEEE Spectrum,* Vol. 24, No. 4, pp. 46–50.
2. Carter, D.C. *et al.* (1989), "Three-Dimensional Structure of Human Serum Albumin," *Science,* Vol. 244, pp. 1195–1198.
3. Christ, C.F. (1984), "The Founding of the Econometric Society and Econometrics," *Econometrica,* Vol. 51, pp. 3–6.
4. Cowles, A., III. (1933), "Can stock market forecasters forecast?", *Econometrica,* Vol. 1, pp. 309–324.
5. Cowles Commission for Research in Economics (1952), *Economic Theory and Measurement: A Twenty Year Research Report, 1932–1952,* Waverly, Baltimore, p. 7.
6. Frisch, R. (1934), *Statistical Confluence Analysis By Means of Complete Regression Systems,* Publication No. 5, University of Economic Institute, University of Oslo, Oslo.
7. ______________ (1970), "From Utopian Theory to Practical Applications: The Case of Econometrics," in *Les Prix Nobel 1969,* Nobel Foundation, Stockholm, pp. 213–243.
8. Galton, F. (1886), "Regression towards mediocrity in hereditary stature," *Journal of the Anthropological Institute,* Vol. 15, pp. 246–266.
9. Haavelmo, T. (1944), "The Probability Approach in Econometrica," *Econometrica*, Vol. 12, Supplement.
10. Hendry, D.F. (1980), "Econometrics—Alchemy or Science?", *Economica,* Vol. 47, pp. 387–406.
11. Hyman, E.S. (1988), *Weekly Economic Data,* C.J. Lawrence & Co., New York, June 18.
12. Kalman, R.E. (1987), "The Problem of Prejudice in Scientific Modeling," in *Recent Advances in Communications and Control Theory* (Eds. R.E. Kalman, G.I. Marchak, A.E. Ruberti and A.J. Viterbi), Optimization Software, Publications Division, New York, pp. 448–461.
13. ______________ (1990), *Nine Lectures on Identification,* Springer Lecture Notes on Economics and Mathematical Systems, Springer Verlag (to appear).
14. Kerr, R.A. (1985), "Pity the Poor Weatherman," *Science,* Vol. 228, pp. 704–706.
15. ______________ (1989), "Telling Weathermen When to Worry," *Science,* Vol. 244, pp. 1137–1139.
16. Keynes, J.M. (1936), *The General Theory of Employment, Interest and Money,* Harcourt, Brace and World, Inc., republished in 1964, New York, pp. 155–156.

17. Kneale, D. (1988), "Into the Void: What Becomes of Data Sent Back From Space? Not a Lot as a Rule," *The Wall Street Journal,* January 12, pp. 1 and 33.
18. Koopmans, T.C. (1937), *Linear Regression Analysis of Economic Time Series,* Netherlands Economic Institute, Haarlem, pp. 99–101.
19. Leontief, W. (1982), *Science,* Vol. 217, p. 107.
20. Los, C.A. (1986), "Quality Control of Empirical Econometrics: A Status Report," Research Paper No. 8606, Federal Reserve Bank of New York, New York.
21. ______________ (1989a), "The Prejudices of Least Squares, Principal Components and Common Factor Schemes," *Computers & Mathematics With Applications,* Vol. 17, pp. 1269–1283.
22. ______________ (1989b), "Identification of a Linear System from Inexact Data: A Three—Variable Example," *Computers & Mathematics with Applications,* Vol. 17, pp. 1285–1304.
23. Mahmoud, E. (1984), "Accuracy in Forecasting: A Survey," *Journal of Forecasting,* Vol. 3, pp. 139–159.
24. McNees, S.K. (1981), "The Recent Record of Thirteen Forecasters," *New England Economic Review,* September/October, pp. 5–21.
25. ______________ (1986), "Forecasting Accuracy of Alternative Techniques: A Comparison of U.S. Macroeconomic Forecasts,", *Journal of Business & Economic Statistics,* Vol. 4, pp. 5–23.
26. ______________ and J. Ries (1983), "The Track Record of Macroeconomic Forecasts," *New England Economic Review,* November/December, pp. 5–18.
27. Markoff, J. (1988), "Supercomputer Pictures Solve the Once Insoluble," *The New York Times,* October 30, pp. 1 and 26.
28. Mayer, T. (1980), "Economics as a Hard Science: Realistic Goal or Wishful Thinking?", *Economic Inquiry,* Vol. 18, pp. 165–178.
29. McCloskey, D. (1985), "The Loss Function Has Been Mislaid: The Rhetoric of Significance Tests," *American Economic Review,* Vol. 75, pp. 201–205.
30. von Neumann, J. (1955), "The impact of recent developments in science on the economy and on economics" (summary of speech before the National Planning Association). *Looking Ahead,* Vol. 4, p. 11; reprinted in the author's Collected Works, Vol VI, pp. 100–101.
31. Waldrop, M.M. (1989), "AI is the Able Assistant," *Science,* Vol. 244, pp. 1017–1116.
32. Wessel, D. (1987), "Fickle Forecasters. How Three Forecasters, After Crash, Revised Economic Predictions," *The Wall Street Journal,* December 31, pp. 1 and 28.
33. Worswick, G.D.N. (1972), "Is Progress of Economic Science Possible?", *Economic Journal,* Vol. 82, pp. 73–86.