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The Comparative Way in Economics: a Reappraisal

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*In recent times, several leading economists (*e.g. *Granger, Leontief, Malinvaud, Schwartz, Summers) have made a critical assessment of the achievements of formal methods in econometrics. This paper argues that an antidote to such shortcomings is systematic recourse to (analytic) comparative analysis. More specifically, the paper delineates a number of guidelines aimed at improving the effectiveness of comparative methods. For instance, in selecting an issue for investigation, the requirement of potential usefulness for policy purposes should be dropped at least temporarily; instead one should select those phenomena for which there are many recurrent realizations along with adequate statistical coverage; that point is illustrated by the analysis of hyperinflation episodes. Finally, the paper points out that the development of comparative analysis is hindered by many obstacles at different levels,* e.g. *difficulties in data collection or lack of academic incentive. This makes it particularly important to get a clear vision of the route ahead.*

Ces dernières années plusieurs économistes de renom, tels que C. Granger, W. Leontief, E. Malinvaud, A. Schwartz ou L. Summers, ont brossé un bilan plutôt critique de l'utilisation des méthodes économétriques dans la recherche de régularités empiriques.

Cet article suggère que ce ne sont pas les outils économétriques qui sont en cause, mais le fait de les appliquer, comme c'est si souvent le cas, à des événements uniques; le remède passe par l'analyse de

gerbes d'événements. C'est cette approche comparative et analytique qui est développée dans l'article et illustrée par l'exemple des épisodes hyper-inflationnistes.

I. – INTRODUCTION

In 1991 Lawrence Summers published an influential methodological paper; he set forth many seminal ideas and showed quite convincingly that formal econometric approaches often yield disappointing results; he argued that a more pragmatic and informal approach would be more appropriate at least in the current stage of the economic science. It has been noted in several comments (Gottfries, 1991; Grodal, 1991; Malinvaud, 1994) that, while being quite persuasive in pointing out the failures of formal econometric tests, Summers is less compelling when he tries to set up some general rules by which the "pragmatic empirical work" he advocates might be defined. It is the purpose of this paper to make some suggestions and proposals in that direction. There have already been contributions to this debate by several prominent economists; let us mention Granger, 1991, 1992; Leontief, 1982, 1993; Morgan, 1988; Malinvaud, 1991, 1994, 1995; Oswald, 1991 [1]. Wassily Leontief in particular has been a long-standing, staunch proponent of a more empirical approach.

There are two major themes in our paper. Firstly, it insists on selecting the phenomena to be studied on the basis of their expected "simplicity". Let us illustrate what we mean by the following example taken from the analysis of commodity markets. Obviously it is tempting to develop models for commodities which are of cardinal importance in the world economy: petroleum, sugar, copper, etc. These are very imperfect and distorted markets however, in the sense that they are biased by many trading peculiarities: transfer prices, producer dictated prices, tariffs, quotas, fluctuations in exchanges rates, subsidized exportation, etc. (see for instance Radetzki, 1990). On the contrary, the American potato or cranberry markets are free of many of the above bias. Sure, potatoes or cranberries can hardly be considered as being of major importance

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[1] Other contributions should be mentioned even though they adopted rather different points of view: a philosophical perspective is to be found in Rosenberg (1992), Hausman (1992) and Marchi (1992); one shoud also mention the brilliant and vivid contributions of D. McCloskey to the analysis of economic rhetoric (1985, Klamer, McCloskey and Solow, 1988).

for the American economy; but after all the fundamental concepts of modern chemistry have been laid down by analyzing simple chemical elements rather than products such as wood, steel or wine; while being certainly more useful the latter are also much more complex. Similarly, the laws of genetics have first been established for garden peas; their application to more "useful" problems, such as for instance stallion selection, turned out to be far more difficult. Taking this simplicity requirement as a starting point the paper emphasizes the need for extensive comparative, empirical research. From the data shown in Leontief (1982) and Morgan (1988) it can be seen that less than two percent of the papers published in the American Economic Review and the Economic Journal (for the period 1972-1986) are based on data generated by the author's initiative (see also in this respect Fig. 1b); one of the purposes of the journal we advocate would be to rise that percentage to much higher levels of the order of 30 or 40 percent.

First of all let us review and illustrate some of the conclusions in Summers' paper. It focused on *empirical* research in macroeconomics. Incidentally, it is important to realize that such papers represent a rather small proportion of the papers published in economic journals; this point is illustrated by the charts in Figure 1a, b; in the last fifty years there has been a dramatic shift to more purely theoretical papers [2]. Among theoretical papers those which aim at developing new statistical techniques represent probably only a small fraction; they play a major role, however, in so far as they pave the way for formal empirical tests. Now, the great virtue of Summers' paper is to offer a lucid (and rather pessimistic) assessment of the achievements of such tests. In her comment, B. Grodal (1991) observed that "for the time being, these models are the best tools for studying economic policy issues". In our opinion there is no contradiction between Grodal's statement and Summers' pessimistic assessment in the sense that only scanty progress can be expected if the economic science restricts itself to "economic policy issues". As will be shown in the next section, these are difficult problems for which no clear-cut solutions can possibly be expected. Summers' dissatisfaction is quite as understandable as Grodal's realism.

[2] This state of affairs has often been deplored; see for instance Allais (1968, p. 11): "Whether or not it is mathematically formalized, a theory that cannot be confronted with empirical evidence is of no scientific value. (Qu'elle s'exprime ou non sous forme mathématique, toute théorie qui n'est pas ou ne peut être confrontée avec les données de l'observation n'a pas et ne peut pas avoir de valeur scientifique)" or Granger (1992): "It is worth noting that many economic theory papers have no empirical implications; they are thus of very little potential use to applied economists and are not possible to evaluate with data".

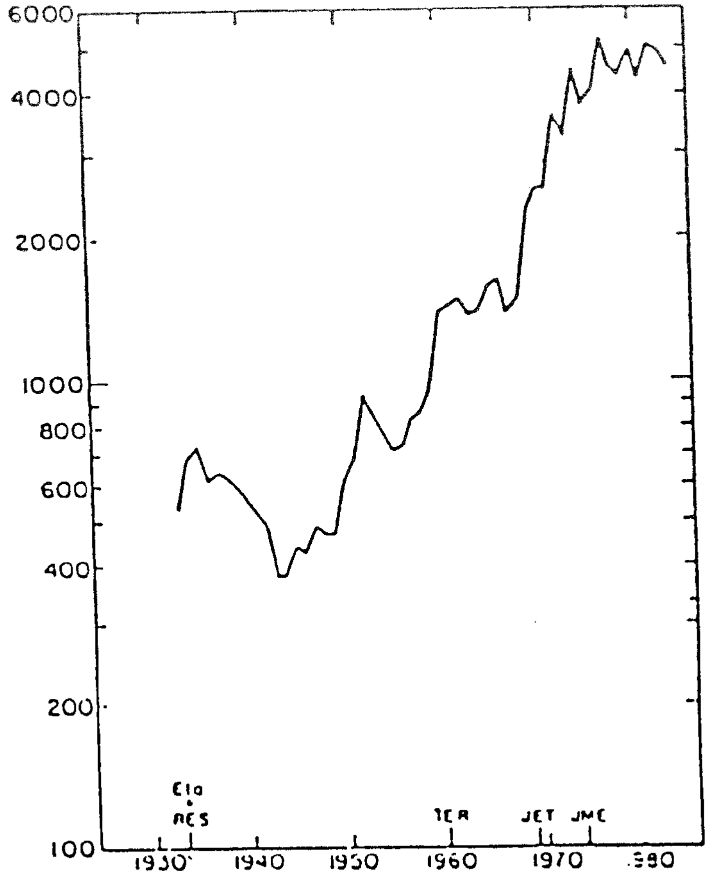

Figure 1a

Number of pages published yearly by the leading journals in mathematical economics.

Source: from Debreu (1986)
The abbreviations are as follows:
Eta: Econometrica;
IER: International Economic Review;
JME: Journal of Mathematical Economics;
JET: Journal of Economic Theory;
RES: Review of Economic Studies.

In this paper empirical statistical research will generally be referred to as observational or semi-experimental [3] research. There are four reasons for that:

1) Attached to the word "empirical" there is a derogatory flavor. On the contrary the words "observational" or "experimental" allude to the highly effective interaction between observation and theory which played such a prominent role in the natural sciences.

[3] The word experimental is also used in reference to group level investigations about cooperative games, bidding processes, Nash equilibria, etc. Given the orientation of the present paper no confusion should arise.

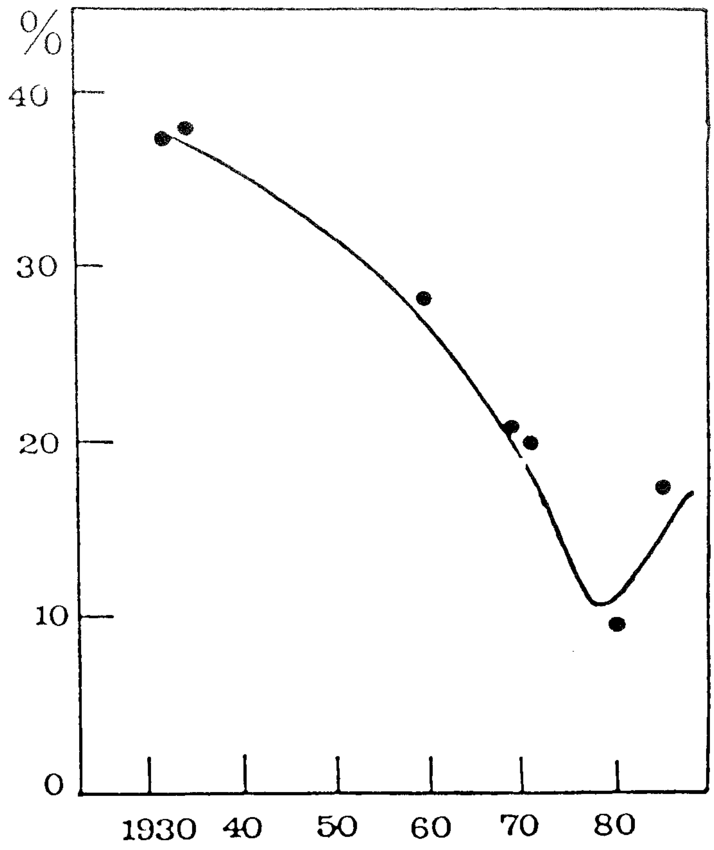

Figure 1b

Proportion of the papers published in Econometrica for which there is a confrontation of the model with empirical evidence.

Source: Roehner (1990)

2) The expression "empirical" has in recent years acquired a technical meaning connected with the testing of econometric models; for instance in Summers' paper it is mainly used with that signification.

3) We believe that there is no fundamental difference between observation either in economics or in such natural sciences as meteorology, astronomy or geodynamics. In all these cases an observation consists in gathering available evidence in a way that enables the researcher to discover new regularity patterns.

4) In the natural sciences interdisciplinarity is a crucial condition of fruitful experimental research. Let us give an illustrative example. The isolation of the first hypothalamic hormone and of the first endorphins were major achievements in medicine; yet these discoveries heavily relied on two circumstances which had little to do with the medical

science; first the delivery of large quantities of animal brains by major American slaughterhouses, second the use of sophisticated methods in chemical analysis. The same kind of collaboration should be encouraged in economics; in Leontief's (1993, p. 4) words: "in the case of agricultural economics, the collaboration may include adjoining fields such as agronomics, soil sciences and ecology".

The paper is organized as follows. In section II, following a track opened by H. Simon (1962), we use a system theory perspective to assess the complexity of the world economy; then we try to conjecture the kind of "basic" mechanisms which may lie behind observable economic phenomena. Section III describes an approach which may prove of some usefullness in identifying significant parameters and in testing new models. Section IV draws attention to the fact that no significant progress should be expected if empirical research is restricted to those questions which may be of importance for policy purposes (directly or indirectly). It is their expected "simplicity" rather than their practical usefullness which should command the choice of the phenomena to be investigated. Section V shows by way of some examples that observational research has not yet been taken as seriously as it should be, in particular as far as accuracy is concerned. In Section VI we advocate the creation of a new journal devoted to observation in economics. Section VII summarizes our conclusions and offers some perspectives.

Before we get to the heart of the matter one more point is in order. May be some of our contentions may seem too categorical. That is almost inevitable, however, for economics has so many facets that objections and counter-examples could be opposed to almost any global assertion. The reader will understand that by omitting many qualifications that would be in order our sole objective is to make our points stand out more clearly.

II. – ECONOMICS AND COMPLEX SYSTEMS

"I do not see why experiments in the natural sciences and econometric work should play a parallel role" (Grodal, 1991, p. 155). It is true that the connection is not obvious; in fact, most of the fields in the natural sciences are unable to provide any insight into the epistemological problems faced in economics. The world economy constitutes a highly *complex system* in the sense that, due to the existence of strong interactions between different elements and between various hierarchical levels, it is very difficult to separate any specific subsystem from the whole without introducing a prohibitive number of exogenous variables. This makes any analogy with Newtonian mechanics (yet the paradigm of classical economics) or other simple physical systems of little utility. Only analogies with other complex systems may prove of some value. Granger (1991, p. 11-12) put forth an interesting analogy with a mammalian brain; it enables him to highlight the inherent limitations of a theory (the so-called representative agent theory) that would consider economic agents to be identical and independent of one another. Another possible analogy (developed in Appendix A) is with meteorology. In contrast with the brain, still poorly understood, meteorology has the advantage of offering a fairly satisfactory theory. Thus it shows us the building blocks of a successful theory, namely: definition of the right variables at the micro-level; definition of the right variables at the macro-level; computation and measurement problems, etc. Even in meteorology, the link between the variables at the micro-level, *i.e.* the degrees of freedom of the molecules, and the basic macro-variables (temperature, pressure, density, humidity rate, wind velocity) is still poorly understood at the dynamic level. In order to get a better understanding of the aggregation stage which is of so fundamental importance in economics (see in this respect B. Grodal's comment (1991, p. 157) or Oswald (1991)), one has to examine how the basic equations of hydrodynamics may be derived from microscopic assumptions at the molecular level; more specifically, one has to show how the Navier-Stockes equations can be derived from known properties of gas molecules. This remains so far an unsolved problem, in spite of the fact that the microstatistical model of a gas is well known and notwithstanding decades-long efforts. Yet, the economist is in a far less favorable situation. For one thing, at the microeconomic level, the number of degrees of freedom is much larger than in the case of gas molecules; secondly, much uncertainty remains as to the definition of the correct model at the micro-level.

One can hardly deny that most of the conclusions we arrived at are rather pessimistic. We prefer to say that they are teaching us a lesson of humility. In addition they suggest a possible route which we describe in the next section.

III. – SOME GUIDING LINES FOR OBSERVATIONAL RESEARCH: ANALYZING RECURRENT EVENTS

Summers (1991, p. 140, 143) gave several clues as to how pragmatic research should be conducted: "to seek inspiration from a wide range of empirical phenomena", "evidence (...) across dozens of countries and a number of decades", "no single test is held out as decisive; many different types of data are examined". The idea behind Summers' suggestions consists, for each different phenomenon, in analyzing a large number of *recurrent events*. This provides an entirely new perspective, a point which may be illustrated by the following analogy taken from P. Veyne (1984, p. 165). "One could try to give a very detailed account of the fall of an apple (the apple was ripe, the wind rose, a gust shook the apple tree, etc.) but never discover the attraction which is a hidden law that has to be discovered. The number of causes that can be separated is infinite for the simple reason that the number of descriptions of the same event is indefinite". With this example, Veyne points out that, whether or not one uses mathematics, the analysis of a *single* event cannot be considered as a scientific issue. For instance, the investigation of the causes of the hyperinflation of 1923 in Germany is very interesting from an historical point of view, but *stricto sensu* it is not, and cannot be, a scientific issue. Reconstructing the causality chain which led to the staggering prices of November 1923 (see for instance Graham, 1930; Laursen and Pedersen, 1964; Webb, 1989) is of rather anecdotal interest: the impact of war reparations, the role of the military occupation of the Ruhr by Belgium and France, the influence of the monetary policy of the German government, the part taken by money creation in major private companies, all these factors are of course unique. A model that operates at that descriptive level can never be expected to capture the "essence" of the mechanism of hyperinflation. The same remarks apply to other much studied phenomena such as the Great Depression or the slowdown of British economic growth by the end of the 19th century; single events simply cannot constitute scientific issues.

The perspective changes completely when *several* episodes of the same phenomenon are taken into consideration. As far as hyperinflations are concerned such an analysis has been undertaken in a pioneering paper by P. Cagan (1956, 1991) and in a book by S.-H. Chou (1963). These studies produced quite a number of "regularities of a kind that theory can seek to explain" (in Summers' words). Just by way of illustration, let us mention some of them. Chou for instance considered four cases: Germany (1914-1923), China (1937-1949), Greece (1939-1944), Hungary (1945-1946); he observed that:

1) There is a strong parallelism between the growth of prices and the increase of note issues.

2) In all but one case (Hungary), prices are multiplied by a factor of about 10^{12}.

3) In all but one case (Germany) the increase in domestic prices precedes the fall in exchange rates.

With these hypotheses in mind one may look at other hyperinflation episodes; the hypotheses may be either confirmed or disproved by the new body of evidence. In any case one has been able to establish a fruitful dialog between statistical data and what can be considered as the building blocks of a possible theoretical model.

From publications which appeared in recent years (*e.g.* Dornbush *et al.*, 1990; Beckerman, 1992; Heymann *et al.*, 1995) it could seem that comparative analysis has now been accepted as the adequate approach. This is only partly true however. As an illustration let us consider the work of Dornbush *et al.* (1990). About 8 instances of "extreme" inflation are considered; yet the sample includes countries such as Argentina, where the inflation rate reached 14 000 percent (1988-1989), and Israel where it averaged 20 percent; clearly a 20 percent inflation rate can hardly be considered as an hyperinflation rate. In other words the essential requirement of a sound comparative analysis (*i.e.* the selection of an homogeneous sample) is missing.

Apart from hyperinflation processes, the approach consisting in studying a number of recurrent events can be employed in many other instances[4]. Summers (1991, p. 141, 143) mentions a number of them: the effect of temporary tax cuts on consumption, the effect of floating exchange rates on real exchange rate variability, the effect on market volatility of news regarding trading operations as opposed to news regarding fundamentals during nontrading periods. Still other examples can be mentioned: the study of real estate price bubbles, the influence of high unemployment rates on the frequency of strikes (Hansen, 1921), the impact of natural disasters (floods, earthquakes) on regional development, etc. In all these cases the first step consists in identifying the relevant statistical sources; most often these sources will be located in different countries with the result that they may not be easily procured. The second step is the selection of statistical evidence. The purpose of the third step is to disclose some significant empirical regularities. As emphasized by A. Schwarz (1995) these steps are very time-consuming without being gratifying in terms of publication opportunities. This is a real problem; it probably explains why such

[4] How may this discussion about recurrent events be transposed to our meteorological paradigm. Consider for instance the phenomenon of the foehn, a warm wind that happens to blow in Switzerland and in Alsace in February or March. By studying different occurrences of that phenomenon one may be able to sort out necessary conditions from causal circumstances.

studies remain so rare. If nothing is done that bias is likely to persist. We shall come back to that point in section VI.

IV. – FOR A BROADENING OF THE SCOPE OF "MEANINGFUL" QUESTIONS

Summers (1991, p. 145) notes that "reliance on deductive reasoning rather than theory based on empirical evidence is particularly pernicious when economists insist that the only meaningful questions are the ones the most recent models are designed to address". Such a bias can probably never be completely eliminated. Human minds have limited adaptability capabilities; accordingly, it is quite understandable that papers dealing with topics which are "fashionable" find their way more easily in economic journals. Such a hidden selection process is certainly also at work in the natural sciences, at least for theoretical papers. Such papers usually are very technical and scientific communication would become very difficult without a consensus about assumptions and formalism. On the contrary, for experimental papers such a screening has no real justification; generally such papers are fairly easy to read, with the relevant information being summarized in a number of charts and tables. In physics for instance, experimental research covers an incredibly large scope of subjects ranging from the simplest devices (see for instance recent publications about avalanches processes in sand heaps) to the most sophisticated experiments in high energy particle physics. There is a general agreement not to discard any new experimental evidence; results for which there is at present no theoretical framework (*e.g.* avalanches in sand heaps) are particularly welcome. This contrasts with the attitude generally prevailing in economics. The scope of "meaningful" questions is narrowed not only by the availability of theoretical models, but also by the requirement that they should be relevant for policy issues either directly or indirectly [5]. While Summers deplores the first constraint, he seems to accept the second [6].

[5] For instance, to pick up an example we already mentioned, a paper about the prices of oil, copper or stocks will gather more attention than one about the prices of potatoes or eggs. Needless to say, there are many domains in economics which are not in the least preoccupied by policy issues, for instance the theory of international trade, most of the sectors in microeconomics, etc. That requirement rather concerns the "applied" studies we have more specifically in mind in this paper.

[6] As an economic adviser of President Clinton and a Treasury-Under-Secretary for Economic Affairs, L. Summers is deeply involved in the task of advising policy makers.

Yet, in the last fifty years several influential economists pleaded for "disinterested work" on simple and "modest" issues. Among the many quotations which could illustrate that assertion, we selected the following four.

• The first one is taken from a paper by Joseph Schumpeter (1933, p. 6) which appeared in the first issue of Econometrica; it can in a sense be considered as defining the objectives of the journal.

> "No science thrives in the atmosphere of direct practical aim, and even practical results are but the by-products of disinterested work at the problem for the problem's sake. We should still be without most of the conveniences of modern life if physicists had been as eager for immediate applications as most economists are and always have been".

• The second quotation is an excerpt from the introduction of Von Neumann and Morgenstern's theory of games (1953).

> "The great progress in every science came when, in the study of problems which were modest as compared with ultimate aims, methods were developed which could be extended further and further. The free fall is a very trivial physical phenomena, but it was the study of this exceedingly simple fact and its comparison with the astronomical material, which brought forth mechanics. It seems to us that the same standard of modesty should be applied in economics. It is futile to try to explain, and systematically at that, everything economic. The sound procedure is to obtain first utmost precision and mastery in a limited field, and then to proceed to another, somewhat, wider one, and so on. [...]. Economists frequently point to much larger, more "burning" questions, and brush everything aside which prevents them from making statements about these. The experience of more advanced sciences, for example physics, indicates that this impatience merely delays progress, including that of the treatment of the "burning" questions".

• The third quotation is taken from a book by Milton Friedman (1953, p. 3). It illustrates and supports a view expressed by J.-N. Keynes[7] in 1891.

It is hardly surprising therefore that he does not advocate the analysis of issues which do not support policy implications.

7 John Neville Keynes (1852-1949) was the father of John Maynard Keynes (1883-1946). His most important contributions to economics were as a methodologist. His chief work on economic methodology: "The scope and methods of political economy" was published in 1891 at a time when the German-speaking world was engaged in the Methodenstreit ("battle of method"). The debate was on the classical question of deductive versus inductive approach; the latter was advocated by the German economist Gustav Schmoller. J.-N. Keynes by contrast insisted that both deduction and induction were required.

"In his admirable book on 'The scope and methods of political economy' John Neville Keynes distinguishes among a positive science, a body of systematized knowledge concerning what is; a normative science, a body of criteria of what ought to be; an art, a system of rules for the attainment of a given end. He comments that confusion between them is common and has been the source of many mischievous errors".

• The fourth quotation is taken from a methodological paper by Edmond Malinvaud (1990) [8].

«Economics has a double purpose: to understand and to advise. The search for objective explanations of economic phenomena corresponds to the first objective. The second consists in providing policy makers with objective assessments of the consequences to be expected from alternative contemplated decisions».

Unfortunately such recommendations turned out to be of no avail. As a result many questions which undoubtedly belong to the field of economics were not deemed worthy of a serious investigation. Let us mention a few examples. The statistical distributions of sizes of firms follow some definite patterns (see in this respect Steindl, 1965); for instance these distributions are more concentrated (in terms of their Gini coefficients) in old sectors of the economy than in new ones; to our knowledge there has been no substantial progress in that direction since the pioneering work of Joseph Steindl. The shares of public expenditures (in percentage of GDP) accounted for by government, state or local authorities are certainly of interest in many respects; yet, this topic has largely been left to sociology (in this respect see for instance Davies, 1970; Deutsch, 1979; Schindler, 1979) [9]. A more recent example is the analysis of the impact of positive feedback in economics (Arthur, 1989). M. Waldrop (1993) gives a lively narrative of the lack of interest shown by the economic community for such "non-standard" issues.

As we tried to show in section III, a true understanding of basic economic mechanisms might best be obtained by a systematic search

[8] Here is the original text in French from which the above quotation has been translated: "Notre discipline a un double objectif: comprendre et conseiller. Comprendre, c'est-à-dire établir objectivement une explication des phénomènes économiques. Conseiller, c'est-à-dire apporter aux décideurs un témoignage objectif sur les conséquences à attendre des décisions alternatives entre lesquelles ils devront choisir".

[9] It may for instance be of interest to note that in the case of Switzerland there has been a sharp turning point about 1945: in the period between 1850 and 1945 the income of the federal government grew much faster than districts' income; after 1945, in the course of a few years, that rapid growth slowed down until district and federal revenues grew almost at the same pace.

of meaningful regularities, whether or not they are of any significance for policy purposes. After all, Galileo founded experimental physics by performing very unpretentious experiments with small cylinders rolling down slightly inclined slopes. Had he right away undertaken to study the trajectories of canon balls or the movements of ships he would perhaps have been able to propose some approximate phenomenological rules, but he certainly would not have paved the way for further progress. Mendel's discovery of the basic laws of genetics is still another illustration of the fact that major paradigms are often brought to light in highly simplified systems.

V. – THE NEGLECT OF OBSERVATIONAL RESEARCH IN ECONOMICS

The following excerpt from a paper by C. Granger (1991, p. 7) provides an excellent introduction to the main issues that we examine in this section: "The vast majority of data used in economic research is supplied by official statisticians in the form of time series, panels or cross-sectional data. There is some discussion with potential users of this data, but what to collect, what to provide and what transformations to use (such as seasonal adjustment) are largely determined by these statisticians. (...) Is it worth pointing out that many potentially important economic variables are not gathered or made publicly available". The fact that most data are supplied by government or international agencies has far reaching consequences. Is it not amazing, for instance, that there is no journal devoted to observation in economics [10]?

We shall argue that this is revealing of a lack of interest in the process of recording and using statistical data. True, quite a number of great economists (*e.g.* Friedman, Frisch, Kuznets, Leontief, Stone) have devoted much efforts to building new statistical time-series, especially in macroeconomics; yet, generally speaking, there is a marked contrast between the rigorous requirements of statistical tests and the carelessness

[10] It is hardly necessary to point out that there are many journals devoted to observations in others fields (geophysics, oceanography, meteorology, etc.). Empirical economic papers can be found in statistical journals; for instance the Journal of the Statistical Society (1838-1889), the Journal of the Royal Statistical Society, the "Journal de l'Institut de Statistique de l'Université de Paris". Such journals are read by only a few economists however; M. Kendall for instance in well known as a statistician, but who knows that he devoted much attention to the statistical analysis of grain prices (Kendall, 1953).

with which statistical data are generated and handled. Let us give five illustrations. The first three observations are of a rather general nature; the last two are specific examples.

1) About fifty years ago, Oskar Morgenstern (1950) discussed the accuracy of various economic statistics: agricultural statistics, unemployement, national income, etc. He showed that their precision is only rarely better than 5%. In spite of his recommendations, economic statistics still are recorded and published without indication of measurement margins. Yet, their inclusion would be of some consequence as far as acceptance and significance tests are concerned.

2) Let us mention the problem of replication in empirical economics. It is obviously of cardinal importance that the regressions carried out by one author could be replicated by another. Yet, it turns out that such replications are very difficult to carry out (Dewald *et al.*, 1986). Locating the right data set used by a given author obviously constitutes a major obstacle. In the editorial which appeared in the first issue of Econometrica, Ragnar Frisch (p. 3) was planning that "in statistical and other numerical work presented in Econometrica the original raw data will, as a rule, be published, unless their volume is excessive". This policy, unfortunately, has not been followed even in the first issues.

3) Comparative statistical studies require particular care in order to ensure true comparability. Yet, it is common practice in that field merely to juxtapose a number of studies by various authors. Most likely this will result in poor comparability. That point is made very clearly by A.B. Atkinson (Smeeding *et al.*, 1990, p. XVII) for the example of income distribution: "Professor X took the household as the unit of analysis, professor Y took the nuclear family; professor X took income without any adjustment for family size, professor Y took income per equivalent adult". As an illustrative example one could mention the study edited by Brenner *et al.* (1991): six countries are examined by six different authors, but almost none of the data are comparable. In fact, as far as cross-national comparison is concerned, the only significant data given in the book are those (in chapter one) reproduced from a pioneering paper by Lindert and Williamson (1985).

4) Historical statistics of main economic indicators are regularly published by the Organization of Economic Cooperation and Development (OECD). Let us consider the index of share prices. The annual figures given for Germany (OECD, 1966) for the years 1955, 1956, 1957 are 31, 29, 29 (they are normalized to 100 for 1960). Thus, because there are only two digits these share prices are subject to an uncertainty of about 4%. Even worse, the 1969 monthly figures for the Tokyo Stock

Exchange vary between 14 and 17; each of these figures is therefore plagued by an uncertainty of about 7%. When such data are used for the computation of volatility or correlation estimates such uncertainties may become quite detrimental. Moreover, the statistics in different countries are not comparable; the figures for the Milan Stock Exchange are *averages* of daily quotations; the figures for Oslo give the quotations on the 15th of each month; the data for Stockholm are quotations at the end of each month. Such figures will clearly lead to very different results in terms of volatility; the standard deviation of the Milan index is likely to be 5.4 (*i.e.* $\sqrt{30}$) times smaller than the standard deviation of the Oslo or Stockholm index. Such inaccuracies and discrepancies are all the more surprising when one considers that share prices are easily available and with great accuracy in any financial newspaper. It should therefore be an easy matter to offer series that are both accurate, reliable and comparable.

5) We took our last example from political science just to show that the problem is not confined to the field of economics. In Singer and Small (1968) there is a table summarizing major international wars for the period 1815-1945. It happens that the "Great War in La Plata" (1865-1870) has been omitted in spite of the fact that, according to Richardson (1960, p. 40), it costed about one million deaths; needless to say the study by Singer and Small records a number of conflicts of a much smaller magnitude. If anything, this example proves that such tables are too rare; otherwise such an omission would have been detected almost immediately [11].

Our purpose with these examples was to show that the publication of reliable statistical series requires no less care and scientific rigor than the testing of models; in a sense, rigor at this stage is even more crucial than in subsequent stages because the latter obviously have to rely on the former. This point is very clearly explained in a statement by A. Schwartz delivered in a recent interview (July, 1995). To the question "You have often criticized the treatment and use of data, especially historical data, in economic research. Do you think the economics profession has in recent years improved its use of data?", Anna Schwartz replies: "No. The main disincentive to improve the handling and use of data is that the profession withholds recognition to those who devote their energies to measurement. Someone who introduces an innovation in econometrics, by contrast, will win plaudits. The fact that it is so easy

[11] For instance, the omission of a major asteroid in a table of the solar system would easily be noticed simply because there are numerous handbooks with which the table could be confronted.

to access data stored in a computer has discouraged familiarity with the problems in the data, let alone an interest in the construction of data".

In order to close this section with an optimistic note let us mention some major contributions which have paved the way for future comparative studies. B. Mitchell's international handbooks (1978, 1982, 1983) are well known. Though perhaps less known, P. Flora's (1983, 1987) handbooks provide detailed and very reliable comparative time series. In our opinion the publication of such statistical handbooks is a very important task which should be encouraged in any possible way by institutions, governments and international organizations.

VI. – PROPOSAL FOR A NEW ECONOMIC JOURNAL DEVOTED TO OBSERVATION IN ECONOMICS

The journal we have in mind should be devoted to empirical findings for instance regarding series of recurrent events. It should be open to all investigations providing the kind of clearcut results which may constitute a starting point for theoretical models, irrespective of any requirement regarding possible implications for policy issues. In other words, it should be an economic analogue of experimental journals in the natural sciences. One could argue that empirical research can hardly be separated from theoretical research [12]; that this is only partly true, however, is shown by numerous examples; such an example is discussed in Appendix B of Roehner (1997) (an extended version of this paper that can be obtained upon request from the author).

Let us explain the objectives of the proposed journal with the help of some specific illustrations.

- In the theory of commodity markets an important task would be to establish comparative statistics about demand/supply elasticities, about transport costs, transit times, storage costs, etc.
- Regarding the topic of speculative bubbles, the journal should aim at the publication of relevant data for *various* speculative processes: stocks, real estate, commodities, rare stamps, coins, collectible autos (for which there has been a tremendous bubble in the early 1990s), paintings (see in this respect Buelens and Ginsburgh, 1993).

One important purpose of the proposed journal would be to avoid the separation that currently prevails between theoretical economics and

[12] In Gottfries (1991) words: "Theory is needed in order to decide what empirical variables should be looked at".

economic history. Gottfries (1991) rightly notes that "every economist must agree that we should use all the relevant evidence in order to evaluate our theories". Actual practice turns out to be very different however. Let us give two illustrations.

• There is a marked split between journals that deal with contemporary economics and journals in economic history. The evidence used in the former is usually taken from the period after World War II; series going back to the first half of the 20th century are rare; those going back to the 19th century are quite exceptional. On the other hand in the main economic history journals (Explorations in Economic History, Journal of Economic History, The Economic History Review) statistical tests are used only marginally (EEH probably being the most sophisticated in that respect); moreover, these journals certainly do not welcome new mathematical models. What is even more regrettable from our perspective is the fact that most of the papers are devoted to *single* events.

• In his pioneering paper about hyperinflation (already mentioned in section III), Cagan (1956) used evidence from seven hyperinflation periods, all of which are posterior to 1922. Yet, there have been many other hyperinflation periods for which statistical evidence is available. Let us mention some of them; references are noted as follows: Altmann, 1976: 1; Bezanson, 1951: 2; Gaettens, 1951: 3; Paarlberg, 1991: 4.

1) The so-called "Kipper and Wipper [13]" period in Germany (1618-1623); references 1, 3.

2) The Swedish money crisis under Charles XII (1714-1716); reference 3.

3) The John Law episode in France (1717-1720); references 3, 4.

4) The American Revolution (1776-1781); references 2, 4.

5) The French Revolution (1790-1796); references 3, 4.

By considering episodes limited to the period 1922-1944 (as did Cagan), there is the risk of restricting oneself to some special phase of the business cycle. On the contrary, by confronting some of the aforementioned episodes with twentieth century crises, one may be able to get a better understanding of the role played by different monetary means.

It could seem odd to advocate a better interplay between economics and economic history. Have R. Fogel and D. North not been awarded their 1993 Nobel Prize precisely for having introduced economic theory

[13] This German expression refers to the process of reducing the value of a currency in terms of gold (kippen = to clip; wippen = to weigh).

into the field of economic history? The work of S. Kuznets similarly blended economics and economic history. Yet, one only needs to browse through the leading journals in each field to become fully aware of the fact that since Kuznets' time the gap between theoretical economics and economic history has been widening not narrowing.

VII. – CONCLUSION

This paper develops a line of thought that has already been delineated in Roehner (1990b). Moreover many of the ideas presented here have been put to work during the past decade in a study about the role of spatial arbitrage in the economics of commodity markets (1990a, Drame *et al.* 1991, 1995, 1996). One can for instance mention the following features: i) The phenomenon of spatial arbitrage has been selected, not because it is "important" but because it is "simple". ii) The study proceeds in two steps: firstly a number of empirical regularities are discovered and pointed out, secondly a theory is put forward which analytically accounts for those regularities. iii) Using a comparative perspective, the mechanisms of spatial arbitrage between distant markets are shown to remain basically unchanged irrespective of the period (20th, 19th or even 18th century) or of the country (France, Germany, United States).

In the past two or three years there has been an astonishing burst of interest for economic problems in the community of theoretical physicists both in Western Europe (especially France, Germany, Switzerland) and in the United States, *e.g.*: Amaral *et al.* (1997), Bouchaud (1997), Buldyrev *et al.* (1997), Feigenbaum and Freund (1995), Ghashghaie *et al.* (1996), Mantegna *et al.* (1995, 1996), Sornette *et al.* (1995, 1997). A clear materialization of that growing interest is the fact that some journals in theoretical physics now have a section devoted to economic issues; furthermore the 20th International Conference on Statistical Physics to be held in Paris in July 1998 has a session entitled "Applications to economics and other fields".

Most of the previous studies display some of the features that we advocated in this paper, for instance: i) The phenomenon under investigation is selected not for its "importance" but for its "simplicity" and because extensive statistical data are available; as a rule these studies rely on huge statistical records; for instance Mantegna and Stanley's paper (1995) is based on about 1.5 million records of the Standard &

Poor 500 index. ii) Empirical regularities are displayed before a model is proposed; that two-step approach is particularly manifest in: "Scaling behavior in economics I: Empirical results – II: Modeling of company growth".

That scholars educated as physicists or engineers may make significant contributions to economics is of course nothing new; one has only to recall such well known names as those of Maurice Allais, Harold Hotteling, Walter Labys, Simon Newcomb or Vilfredo Pareto. Yet the present shift seems to concern the profession itself rather than just a few individuals. If that movement turns out to be more than a passing fad it may perhaps facilitate the evolution we advocated in this paper. Needless to say, such a move is likely to require several decades, if it takes place at all.

APPENDIX

A system theory view about the complexity of the world economy

Analogies are frequently (and sometimes with good reason) considered as misleading or at best speculative. Yet, we believe they can be quite useful if used properly. It is precisely one of the purposes of system theory to tell us how to use analogies. The present discussion is intended to shed some light on the following questions. i) How can the analysis of specific phenomena and special patterns contribute to an understanding of the *global* world economy? ii) What can be said about the link between macroeconomic and microeconomic phenomena?

Granger (1991) has invoked an interesting analogy between an economy and the mammalian brain. This appendix considers an analogy with another complex system, namely the meteorological phenomena in the earth's atmosphere. Neither of these analogies is probably completely pertinent, but both may provide new insights. In terms of connectivity (*i.e.* the number of other neurons to which each neuron is directly connected) the world economy probably ranks between the mamalian brain and the earth's atmosphere. In the brain the connectivity greatly depends on which region one considers; it ranges between a few units in some exceptional regions and a more standard order of magnitude of 10^5; the later connectivity is about a thousand times higher than that of the world economy. On the contrary, meteorological phenomena are by far less complex: only a dozen basic micro-variables are necessary in order to describe the atmosphere at any point in space, while at least ten times more variables are required in economics. What nevertheless makes the comparison with meteorological phenomena interesting is the fact that their basic equations are perfectly well understood.

Let us now briefly list the features that meteorology and economics have in common.

• In both cases, the objective is to provide accurate forecasts in spite of the fact that no real experiments are possible; and in both cases the observational evidence takes the form of multivariate time-series.

• In both fields it is extremely difficult to isolate any subsystem from its environment in order to study it separately (a typical characteristic of complex systems). This is of course a rather obvious observation but it has important consequences at two levels. Firstly, all variables are interdependent; in meteorology, temperature, density, pressure, humidity rate, wind velocity should be studied together; the same is true in economics, with the obvious complication that the number of major variables is much larger. Secondly, in both fields there is an interdependence between spatially separated phenomena. In meteorology a spectacular illustration is provided by the so-called butterfly effect: the movements of a butterfly in Brazil are said to affect the weather even in remote areas. Similarly, in a well integrated market a major event at one point affects the whole market.

• Both meteorological and economic phenomena display long- and medium-term quasi-cycles. In economics such cycles have been extensively studied: let us for instance recall among the earliest studies those of Juglar, 1862; Tintner, 1935; Burns and Mitchell, 1946; Gordon, 1952. In meteorology too, there are short-, medium- and long-term fluctuations; some manifestations of medium-term fluctuations are particularly spectacular, for instance the progression or retreat of major glaciers. Short-term fluctuations, while being less impressive, exist too; they may be related to the position of a given anticyclone: for instance if the Azores anticyclone remains at the latitude of Dakar the summer in Western Europe is likely to be rainy; on the contrary a warm and sunny summer is to be expected when the anticyclone establishes itself over the Bristish Islands.

• Both in economics and in meteorology there are "catastrophic" phenomena characterized by the fact that the state variables suddenly leave their normal variation bounds and assume extreme values. In meteorology this refers to abnormal raining or devastating thunderstorms; in economics one can mention such phenomena as the fall of the general price level during the Great Depression or, at a more microeconomic level, the multiplication of the price of gold and silver by a factor 15 in the period 1975-1981.

What can be learned from that parallel between meteorology and economics? First of all it enables us to get a better idea of how difficult the problem of economic forecasting really is. In meteorology the basic theoretical laws which govern the movements of masses of air are perfectly known. *Static* properties of large masses of air are described by the so-called equations of state (which are generalizations of Boyle's law); these equations may be regarded as the analogs of what production and utility functions are in economics. At the *dynamic* level, atmospheric movements are ruled by the laws of mechanics (for instance the Coriolis effect due to the rotation of the earth) and by the laws of hydrodynamics (the Navier-Stockes equation). It should be noted that these dynamic equations are highly nonlinear. On the statistical side many time series going back to more than one century are available in order to test and to estimate meteorological models. In spite of such favorable circumstances it is still impossible to make reliable forecasts more than two or three weeks in advance. Likewise, forecasting "catastrophic" local phenomena is at best possible a few days in advance. In economics the situation is less favorable. At the theoretical level, the "basic equations" (if any) are not well known; at the statistical level, even in industrialized countries (not to speak of many developing countries or of communist or ex-communist countries) truly comparable macroeconomic series are hardly available even for major variables. Is it so surprising then that the task of predicting interest rates, exchange rates, unemployment rates (see in this respect Fig. 2) or other variables one year in advance remains largely out of reach? Forecasting such "catastrophic" phenomena as Stock Exchange crashes or short-lived commodity price bubbles can be expected to be even more difficult. So far we did not mention the feedback-effect predictions may have on the behavior of economic agents; this is likely to represent an additional and very substantial complication.

Let us assume for a moment that there is something in economics like the basic equations of mechanics and hydrodynamics in meteorology; then, our analogy may help us to assess how difficult it may be to infer such equations from the information contained in statistical time series. It would probably not be too difficult (but by no means straightforward) to derive the equilibrium state equations from meteorological time-series; but it would certainly be an almost impossible task to derive the Navier-Stokes equations from meteorological records even if we assume that the state variables have been correctly identified[14]. H. Simon's famous bowl metaphor (1959, p. 255) already provided an elegant illustration of the fact that non-equilibrium problems are far more difficult than equilibrium problems.

Our analogy seems to lead to rather pessimistic conclusions; but it also suggests possible clues. For instance, by a careful analysis of wind directions from stations located at about the same latitude in both hemispheres one may be able to recognize that atmospheric movements are deviated to the left in the Northern hemisphere, and to the right in the Southern hemisphere. From that empirical regularity it may in turn be possible to infer Coriolis' law.

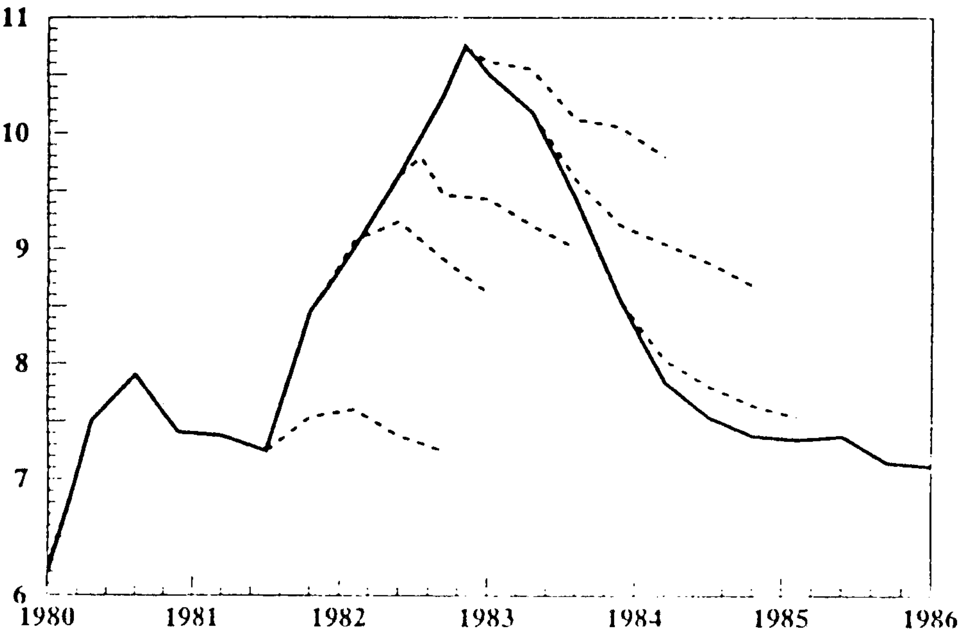

Figure 2
Forecasting the unemployment rate.

Source: adapted from Mankiw (1992).
The solid line shows the actual unemployment rate. The broken line shows the predicted unemployment rate at six points in time. The forecasts are a median average of about 20 forecasters surveyed by the American Statistical Association and the National Bureau of Economic Research. The forecasters missed both the rapid rise and the subsequent fast decline.

[14] Inverse problems of this kind have been studied in mechanics and they proved very tricky.

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