# DEBATES

## *New Approaches in Economic History and Related Social Sciences* *

**Elias H. Tuma**
University of California, Davis

### I. Introduction.

Economic historians have during the last two decades been subjecting themselves to a sort of self examination with the objective of elevating their discipline to a scientific level. This self examination has resulted in controversies and, more importantly, in a long series of publications characterized as New Economic History.

Similar controversies and attempts at methodological innovation have been common in other social sciences. This should not be surprising since other social scientists treat problems similar to those handled by economic historians, at least those of the traditional vintage. It should, therefore, be enlightening to explore the common bases of controversy and the various attempts at methodological innovation. In this paper only a partial view of these issues can be undertaken. I have concentrated on Sociology, Anthropology, Political Science, and Psychology, leaving out economics and history which are within the domain of economic history.

The relevance of these related social sciences may be seen by looking at the factors they have in common with economic history, as follows: 1) All of them deal with human behaviour, even though from different points of departure. All face highly complex problems of interpreting human behaviour which is based on motives that may be hard to discern, except within the framework of the given culture and institutions. 2) All

* Written for the « Roczniki Dziejów Społecznych i Gospodarczych ». Except for minor updating this paper is based on my book, *Economic History and the Social Sciences. Problems of Methodology* (University of California Press, 1971); for space limitations, footnotes have been kept to a minimum.

these disciplines are involved largely in observing, measuring, and explaining dynamic behaviour. Yet often the data are not accessible because controlled experimentation is out of the question. 3) They all have to cope with two levels of interpretation, that of the individual and that of the group. Transference of theory or experience from one to the other is not easily implemented. 4) In studying human behaviour, they need to integrate the common-sense knowledge of everyday behaviour into the scientific analysis of that behaviour for deeper understanding. They need to reconcile the apparent regularity of behaviour which suggests a certain degree of determinism with the alleged free will of man in society. 5) These various disciplines must take into consideration what other disciplines have to say about their subject matter. Therefore, they often wonder how interdisciplinary their studies should be. 6) Fnally, they usually face the challenge of methodological achievement in the physical and natural sciences and try to imitate, innovate and explore how they can achieve the same levels. Yet, they are frequently handicapped by the inaccessibility and non-quantifiability of the data, compared with the physical sciences.

As a point of departure, let us look at the New Economic History: what it is, what it aims at, and how it has fared in achieving its goals. This will be followed by a survey of the controversies and new approaches in the related social sciences. In the last section, I shall try to evaluate the impact and the potentialities of the new trends.

## II. The New Economic History.

Although much writing has been done in the framework of the « new » economic history, there is no precise definition of the new approach. Rather, it is identified by the tools it uses, the subject matter being the same as in traditional history. Put in a nutshell, the innovations in method expounded by the new economic historians include: 1) More extensive use of economic theory in the analysis of historical data; 2) the use of econometric tools and statistical theory, rather than dependence on descriptive illustrative analysis; 3) keeping to the rules of scientific method; 4) precise specification of the model, hypothesis, and criteria relevant to the research; and 5) the explicit use of counter-factual hypotheses as bases for reconfirmation of the results.[1] The failure to apply these tools by

[1] The bibliography on the New Economic History is already extensive; in addition to the references in my book, the following should be mentioned: Robert W. Fogel and Stanley L. Engerman (eds.), *The Reinterpretation of American Economic History* (Harper and Row, 1972); R. L. Andreano (ed.), *The New Economic History* (John Wiley and Sons, 1970); the 1971 summer issue of « The Journal of Economic History », XXXI, No. 1 — the whole issue is relevant.

traditional historians has rendered their conclusions, according to the new history, to be less scientific than they could be. The new economic historians must try, therefore, to correct the errors of the non-scientific approach of traditional historians.

To a large extent, the charges of the new economic history are justified. Traditional historians have made little use of economic theory, of statistics, or of the method of hypothesis. They have also failed to specify the model and the criteria they use. Consequently, they have often derived vague and broad generalizations which cannot be subjected to testing or confirmation. They have also failed to apply formal and systematic approaches in analyzing the data. Therefore, their results have usually been broad and imprecise. The new economic historians, in contrast, have tended to use models based on the logic of economic theory, though sometimes only by imposing simplifying assumptions to make the theory usable. They also have depended extensively on quantitative data which would allow the application of statistical theory and analysis. Should quantitative data be unavailable, new economic historians would try to estimate and generate data, or they might give up the study. Thus, most studies in the new economic history have resulted in fairly precise and verifiable conclusions.

One of the major innovations of the new economic history is the use, explicitly, of the counterfactual hypothesis method as a tool of analysis. Rather than estimate the significance or contribution of a given factor to the economy or the process of production, the counterfactual hypothesis asks how different would the economy or the production process have been in the absence of that factor, or in the case of applying its next best alternative. For example, if two processes can be used to produce a commodity, the contribution of either process to the economy would be the difference between its efficiency and the efficiency of the other process which might exist. If only one process is feasible, its contribution would be assessed as the difference between economic conditions with and without its product. It is assumed, of course, that counter-to-the-fact conditions can be determined with the help of economic theory. Probably the best way to illustrate the methods of the new economic history would be to review some studies in that approach.

In his pioneering « new » study of railroads, Robert Fogel questioned whether railroads were « indispensable » for American economic growth, or whether the railroads were responsible for the take-off of the American economy.[2] To test these hypotheses, he set out to measure the incremental contribution of railroads over the next best alternative means of transportation, the canals. The incremental contribution is the « social saving »

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[2] ROBERT W. FOGEL, *Railroads and American Economic Growth: Essay in Econometric History* (Baltimore: Johns Hopkins University Press, 1964).

generated by the railroads. Social saving is defined for any one year as the difference between the actual cost of shipping by railroad and shipping exactly the same collection of goods between exactly the same set of points without railroads. Fogel's main hypothesis is the following:

> « Railroad connections between the primary and secondary markets of the nation were a necessary condition for the system of agricultural production and distribution that characterized the American economy of the last half of the nineteenth century. Moreover, the absence of such railroad connections would have forced a regional pattern of agricultural production that would have significantly restricted the development of the American economy ».

As for the take-off thesis and the impact of railroads on industry, Fogel tries to estimate the contribution of railroads to the growth of coal, iron, machinery, transportation equipment, and lumber on the assumpion that most purchases by the railroads came from these industries.

After detailed calculations of the social savings, Fogel rejects the hypothesis of indispensability of railroads to growth because the estimated social savings (in the year 1890) amounted to between 6.3 and 7.1 per cent of the Gross National Product, which is not considered significant. He also rejects the hypothesis that railroads were responsible for a take-off or that there was a take-off between 1843 and 1860, as had been thought before, because the demand generated by the railroads could not have been large enough to induce a take-off.

Fogel's study has been subjected to numerous evaluations,[3] which cannot be reported here. While his results are neat and precise, his hypotheses have been subject to criticism as unrealistic; the concept of social savings has been criticized as inadequately representative of railroad contributions, being not comprehensive enough in coverage, factually and conceptually. The non-economic impact of railroads has also been ignored. Hence, the results can at best be tentative and only suggestive, as was the case with results of the traditional research.

A different approach to railway contribution in Italy reaches much more drastic conclusions. Stefano Fenoaltea measures the impact of railroads as the value added to the steel industry, the engineering industry, manufacturing as a whole, and to the national income between 1861 and 1913. Considering what these values would have been without the railroads, he concludes that Italy's industrial history would not have differed had the railroads not been expanded. He even suggests that the impact of the railroads might have

[3] See my Chapter 9; for a more recent and comprehensive critique of railroad studies see PETER D. MCCLELLAND, *Social Rates of Return on American Railroads in the Nineteenth Century*, « Economic History Review », 2nd series, vol. XXV, 3, August 1972, pp. 471-488.

been negative because of their effect on the tariff policy. Therefore, « until Italy's political history is further clarified, the very sign of the total impact of railway growth must remain a matter of conjecture ».[4]

Another good illustration of the new economic history is a study of the impact of tariffs on income distribution in pre-Civil War America.[5] After surveying previously-held positions on the impact of tariffs in the economy prior to the Civil War, it is established that there are questions still not answered. The author, therefore, proceeds to set up a model for the study, with the help of simplifying assumptions, among which are the following:

> ... all factors of production are fully employed; there is perfect competition in all markets; tariff revenues are distributed to consumers in lump-sum payments; the marginal propensity to consume each good is the same for all groups of factor owners; for any given commodity, foreign and domestic supplies are perfect substitutes; the elasticity of substitution in each of the production functions is unity; and there is a trade balance with no international flows.

The author finally reduces the equations to fourteen, with fifty-four parameters, which are « simply hypothetical values selected to illustrate the impact of a particular change in the tariff rates ». Fifty parameters are to be estimated or specified. The structural parameters, such as labour force distribution, shares of consumption or production, or income shares are estimated directly from census data. The behavioural parameters, such as elasticities of demand and supply functions must be estimated indirectly from other data. The author wisely tells us that

> ... the time and effort involved in such estimation is beyond the scope of this study. Further, the paucity of data for the ante-bellum period makes such estimation difficult, and the estimates would necessarily be extremely tenuous. Rather than attempting estimates of these parameters, estimates of others will be adjusted to conform to this model. Obviously, the errors in the values assigned to the parameters will make the results subject to some error. All that can be expected from the model is the direction of income changes (due to tariff) and some idea as to the relative magnitude of these changes in the income of the various factors of production.

It is not evident, however, that the findings are an improvement over previously held views. The major conclusion seems to be that interaction between history and theory is fruitful.

Many other studies have been conducted as new economic history. In general they have been revisionist: they have reconsidered questions of the

[4] STEFANO FENOALTEA, *Railroads and Italian Industrial Growth, 1861-1913*, « Explorations in Economic History », 9, No. 4, Summer 1972, pp. 325-351.

[5] CLAYNE POPE, *The Impact of the Ante-Bellum Tariff on Income Distribution*, « Explorations in Economic History », Summer 1972, vol. 9, No. 4, pp. 375-421.

viability of slavery, the impact of the Navigation Laws on the American colonies, the economic efficiency of the canals and other waterways and means of transportation, and the mechanization of agriculture. Precision, formalism, and the expectation to be scientific have been characteristic. In general, however, these studies have been based on some degree of simplification and have been restricted to issues that can be studied quantitatively and scientifically. Only recently have attempts to resort to broader and less precise and formal approaches been made.[6] The impact of the new trend will be explored further after we take a look at happenings in the related social sciences.

## III. Controversies and New Tendencies in Related Social Sciences.

Two main controversial issues at least should be noted before exploring the new tendencies: One major controversy has been the conflict between holism and reductionism and whether one or the other should be adopted in social science. The issue may be formulated as follows: Should we study the unit as one whole or should we disaggregate it and study it in parts more intensively? Is it possible to understand the whole through the parts or must we look at the whole as a unit to be able to comprehend it? These questions are related to the broader and more fundamental issue as to whether one should look for generalizations with the hope of discovering laws, or only to explain events in the immediate environment. The conflict has not been resolved and both approaches have proponents in the various disciplines.

The second important controversy centres around the role of the historical approach, the significance of data accumulation, and the descriptive or reconstructionist approach to research. The substance of this controversy is whether the « traditionalist » approach is viable, as has been asked in economic history. While the traditionalist is identified with the historical, sociologists go farther by questioning the role of history in the study of human behaviour. But the conflict between sociology and history should be distinguished from the conflict within sociology between what has been described as the « old fashioned » and the « modern » diagnosis of social problems. The old-fashioned approach is similar to what has been described as the historical, which studies behaviour as consisting of unique events. The old-fashioned approach holds that while sociology tends toward the

[6] In the form of a review, see William N. Parker, *From Old to New to Old in Economic History*, and Albert Fishlow and Robert W. Fogel, *Quantitative Economic History: An Interim Evaluation. Past Trends and Present Tendencies*, in « Journal of Economic History », XXXI, 1, March 1971.

study of general structures and the prediction of future developments, it does not ignore the unique or single events. Hence, it may be appropriate to consider the difference between the old-fashioned and modern schools as a division of labour and hardly a matter of conflict.[7] Scepticism regarding the historical approach has been common to political science, anthropology, and to an extent to psychology.

This negative attitude toward the historical and descriptive has been in direct contrast to a keen interest in facts and data collection. Rather than depend on documents and secondary sources, as was common in traditional research, interest has shifted toward directly observed facts, field work, surveys, questionnaires, and laboratory experiments. Consequently, interest has shifted from the past to the present on which such data may be gathered.

Probably few would question the significance of gathering data. The conflict, however, has persisted as to whether emphasis should be on quantitative or qualitative data. The opposition to historical, institutional, and descriptive research has extended to qualitative data which are hardly subject to systematic analysis. However, this attitude has been challenged. It has been noted that quantitative data are appropriate only for mechanistic processes which are presupposed by mechanistic models. However, non-mechanistic processes can be handled only analytically, by qualitative data. It has been pointed out that quantitative research frequently leaves out counter evidence by analyzing only supportive data. Furthermore, the stress on quantification tends to generate hypotheses for which only the relevant data are collected when in fact the attempt to explain any given amount of data may generate a number of conflicting hypotheses.[8]

In general, there seems to be a common rebellion in the social sciences against the traditional, historical, descriptive, and qualitative approaches. This rebellion has signalled the introduction of new, theoretical, precise, and presumably predictive approaches, to which we turn next.

The New Trend in Social Science includes three main tendencies which may be described as: increasing interest in theory, more emphasis on measurement and measurability, and increasing interest in formalism.

1. *Interest in theory.* That theory is an integral part of the scientific approach is an accepted fact in the social sciences. The relevant questions are how to derive a theory, what are its functions, and how to validate it. The trend in sociology has been described as the natural science approach

[7] Barrington Moore, Jr., *Sociological Theory and Contemporary Politics*, « American Journal of Sociology », LXI, No. 2, September 1955, p. 10.

[8] Carl J. Friedrich, *Man and His Government: An Empirical Theory of Politics* (New York: McGraw Hill, 1963), pp. 60-65.

which emphasizes symbolism, observation, and measurement. This behaviourist approach considers symbolism or abstraction and precision as prerequisites for scientific sociology. As an elaboration of these views, it is suggested that « science advances in so far as it can find adequate procedures for simplifying and ordering its factual materials », free from common sense and the facts as observed in nature and based on creative imagination as exemplified in the work of the natural and physical scientists. To use creative imagination it is necessary to construct « 'ideal types' and mathematical models of portions of social reality ». Hence, the scientific approach stresses strict laboratory and experimental condtions. Understanding current problems can be stressed only as a « mere by-product ».[9]

The attitude towards theory in psychology has been well expressed in the debate between Skinner and his critics, a debate which has lasted for almost two decades. Skinner questions the usefulness of theory unless one is studying an inner system which cannot be observed, but it adds little in the study of empirical behaviour On the negative side, theory tends to channel observations into narrow areas focusing on the theory or the hypothesis and thus overlooks many relevant aspects of the subject matter. In other words, Skinner's objections seem to be aimed at deductive analysis rather than at theorizing in general. Skinner recommends experimentation, depth, and a search for explanations of behaviour that take the purpose of behaviour into consideration. This approach does not need theory, sophisticated statistics, or a large number of cases for observation.[10]

Skinner's views have been criticized severely as « anti-theoretical », « ultra-empirical », « non-theoretical », and « radical ».[11] Scriven insists that theories are necessary to explain why a particular law should apply in a given situation and hence they are indispensable. The significance of the debate may be reflected in the observation that commitment to scientific method has continued to rise, and has rendered research in psychology almost sterile and irrelevant to human problems. The method has tended to guide research away from human problems, into the narrow channels determined by the theory, the hypotheses, or the statistical tools available

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9 BARRINGTON MOORE, JR., *op. cit.*, p. 108.

10 B.F. SKINNER, *Are Theories of Learning Necessary?*, « The Psychological Review », 57, No. 4, July 1950, pp. 193-216; also *Operant Behaviour*, « American Psychologist », 18, 1963, pp. 503-515; for a more recent general survey and bibliography see BENJAMIN B. WOLMAN, *Does Psychology Need its Own Philosophy of Science?*, « American Psychologist », 26, 2, 1971, pp. 877-886.

11 For a detailed critique, M. SCRIVEN, *A Study of Radical Behaviorism*, « Minnesota Studies in the Philosophy of Science », edited by H. Feigl and M. Scriven (Minnesota University Press, 1956). For a comparison of Skinner with Scriven, see R. J. JOHNSON, *A Commentary on 'Radical Behaviorism'*, « Philosophy of Science », 30, 3, July 1963, pp. 274-285.

to the researcher, as Skinner had predicted.[12] Nevertheless, the trend seems to have continued.

A little over a decade ago it was possible to say that « most political scientists, although perhaps a diminishing majority, are skeptical of both the possibility and importance of systematic theory... ».[13] The movement towards scientific politics begun in the 1920s was somewhat stopped in the following decade. Objections were aimed against the proposition that the study of politics should be value free and against impractical theorising and scientism. In fact it was proposed that political scientists are duty-bound to lead the way to better statehood and better societies.[14] However, a revival of interest in theory has been apparent in the last two decades, which has been due to an interest in convergence of political science toward a unified discipline.[15] Since then an increasing number of political scientists have turned to theorizing and systematic rather than pragmatic or problem-oriented research. This is what has been known as Behaviouralism, which concentrates on behaviour rather than on political philosophy or institutions. The objectives of behaviouralism include « the development of a systematic body of theoretical propositions; ... the acquisition of analytical skills and data collecting procedures for relating empirical uniformities and deviations to our theoretical concepts and categories... », on the basis of « relatively microscopic studies which have undertaken the tedious task of identifying and validating the conceptual variables... ».[16] In addition, behaviouralism postulates that « truth or falsity of values (democracy, equality, freedom, etc.) cannot be established scientifically ». Therefore, political science should pursue pure research, abandon the « great issue », and remain value free. At the same time, there should be more scrutiny of method and more interaction with other disciplines in search of better techniques, theories, and concepts.[17] Yet, opposition has been sustained. A serious indictment of modern political science notes the lack of theory, or the lack of theory of a given type. It charges that the recent trend has been away from politics toward what is called pseudopolitics. The main argument of this

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12 NEVITT SANFORD, *Will Psychologists Study Human Problems*, « American Psychologist », 20, 1, 1965, pp. 192-202.

13 AVERY LEISERSON, *Problems of Methodology in Political Research*, in HEINZ EULAU, SAMUEL J. ELDERSVELD and MORRIS JANOWITZ (eds.), « Political Behavior » (Free Press, 1956), p. 57. Most of the arguments for the new trend in Political Science are represented in JAMES N. ROSENAU, *The Scientific Study of Foreign Policy* (New York: The Free Press and London: Collier-Macmillan, 1971).

14 ALBERT SOMIT and JOSEPH TANENHAUS, *The Development of American Political Science: From Burgess to Behavioralism* (Boston: Allyn and Bacon, 1967), Ch. IX.

15 DAVID B. TRUMAN, *Disillusion and Regeneration: The Quest for a Discipline*, « American Political Science Review », 59, 4, December 1965, p. 870.

16 A. LEISERSON, *op. cit.*, pp. 57-58.

17 SOMIT and TANENHAUS, *op. cit.*, pp. 178-179.

indictment is that the new politics has removed itself from the normative and the real into research which glorifies the status quo and thus distorts the real objecives of political science.[18]

Most of the above arguments and counterarguments have been repeated in anthropology. In a recent symposium, anthropologists listened to a plea for modernising their methods and increasing the validity of their findings, as the number of speakers, each of whom agreed with a part and disagreed with another of that plea, as a summary of the debate would show.[19]

Looking at these various fields, we find certain characteristics common to their recent developments. They all stress theory, the need for systematic and formal presentation of the data and conclusions, and the necessity of validation. They also urge for the use of symbolism or abstraction, measurement, and reductionism as indispensable. And to be scientific, they emphasize the need to follow the pattern of theory-hypothesis-data in research so that the hypothesis and the theory from which it is derived may be tested by the empirical data. However, while these features have been generally accepted as essential, two fundamental questions have persisted: Is it legitimate to predict human behaviour without getting involved in the problems of causality? And, how to justify theory building and prediction, both of which imply determinism, and how to reconcile such action with individuality and volition of behaviour? While these questions have continued to sustain a debate, proponents of the new tools of analysis have gone ahead with their innovations. The nature of causality in the new trend has been modified such that multiple causation is sought and probabilistic conclusions are the ideal, thus avoiding the more fundamental questions raised above.

2. *Measurement.* Social scientists are aware of the difficulty of attaching quantitative values to many of the variables they deal with. Nevertheless, they have made attempts to quantify and often to limit their research to the quantifiable. To render quantification and measurement possible, social scientists have compromised on several criteria of classical measurement. For example, they have accepted as impossible the attachment of cardinal or ratio scales to the variables they deal with. At best, they are content to use ordinal and interval scales, although exceptions may be noted. Secondly, they have redefined many concepts « operationally » to make quantitative measurement possible. Thirdly, they have frequently depended on derived measurement in order to avoid the problem of observing what may not be observable. Finally, they have usually described their findings as tentative, depending in part on the reliability of the measures they have obtained.

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[18] Christian Bay, *Politics and Pseudopolitics: A Critical Evaluation of Some Behavioral Literature*, « American Political Science Review », 59, 1, March 1965, pp. 39-51.

[19] William McEwen, *Forms and Problems of Validation in Social Anthropology*, « Current Anthropology », 4, 2, April 1963, pp. 155-183.

Economic historians and economists in general have led the way in this new trend.

For example, political scientists have defined liberalism, for purposes of measurement, in terms of voting behaviour on a variety of issues which are classified according to a given standard or scale of liberalism. Thus, the more frequently one votes YES on certain issues, the more liberal he would be considered. The issues may be classified according to fixed intervals on a continuum such that the more liberal issues tend to one end and the less liberal to the other.[20] While the issues are classified in an ordinal fashion, the scale is an interval scale so that the more or less on the scale may be defined in fixed intervals. Political scientists have also resorted to indirect measurement. For instance, political power or influence has been measured as a function of the distribution of income, wealth, or property, often with the help of a Lorenz Curve as used by economists. The same approach has been applied to the study of the distribution of power among social groups.[21] Unfortunately, such attempts inherently oversimplify the issues to make them measurable, as in the measurement of social power. To be able to measure social power, Dahl defines the power of A over B to be the extent to which « A can get B to do something that B would not otherwise do ».[22] Harsanyi modified this approach by considering power as the « difference of two probabilities, and therefore is directly given as a *real number* » for each action X. The total power of A over B for all actions is then a vector rather than a single number, unless aggregation is possible.[23] However, the resulting measurements are subjective in the sense that the probabilities must be assigned by the investigator. While these values are numerical, aggregation would have little significance as far as total power is concerned. Nor would aggregation be possible if we agree that power is cumulative: the more successes A obtains over B, the higher the relative power value of his resources and the easier it becomes for him to influence B. The probabilities assigned will then vary according to the number of successes and failures and the relative significance of the individual actions performed.

An easier approach might be to use an efficiency measure as the indicator of power, by comparing the probabilities of A and B independently having action X performed in a given period of time and with a given amount of resources. By means of this approach it would be possible to assign empi-

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[20] H. R. ALKER, JR., *Mathematics and Politics* (Macmillan, 1965), pp. 23-24.

[21] ALKER, *op. cit.*, p. 30 ff.; also B. M. RUSSETT, *Inequality and Instability: The Relation of Land Tenure to Politics*, « World Politics », 16, 1964, pp. 442-454; G. WILLIAM DOMHOFF, *Who Rules America?* (Prentice Hall, 1967).

[22] See JOHN HARSANYI, *Measurement of Social Power*, in M. SHUBIK (ed.), « Game Theory and Related Approaches to Social Behavior » (New York: Wiley, 1964), p. 184.

[23] *Ibid.*, p. 185.

rically derived probabilities by investigating the political history of the action under consideration.

While political scientists have continued to search for measures of democracy and power, psychology has advanced even farther, partly because of the experimental nature of that field. Under controlled conditions behavioural data may be measured on predetermined scales. This obviously is difficult in social psychology and the psychology of personality, the fields closest to social science. When arguments against precise measurement have been raised, they usually have been concerned with quantification and its feasibility, rather than with its usefulness or desirability.[24] Nevertheless, serious attempts at quantification have continued, mostly in search of interval scales, and by seeking operational and feasible measurements. Psychologists have put little emphasis on whether the resulting scores are additive or not.[25]

An example would be the « equal-sense-distance » scale: the subject is exposed to two widely-separated sound frequencies and is given control of three keys on a pitch scale, which he can adjust to intermediate frequencies. The subject is instructed to vary the frequencies of these three keys until the five frequencies have been separated by psychologically-equal intervals. Thus an equal unit can be derived and calculated.[26] Such a scale, however, is limited in usefulness to the specific operation for which it was devised and must be understood as a subjective or psychological scale. Other measures include the centile scale which ranks the performers on a centile scale, but in which the numerical differences on the scale have a rank significance in interpreting the performance; this is an equal-unit scale with an arbitrary zero point. There have been suggestions that ratio scales for psychological research may be devised, but it is not clear how useful these scales would be.[27]

Measurement in sociology has been a continuing problem. Attempts for cardinal measurement have been made although the results have been limited. One of the most common approaches has been the survey approach which depends on counting and which results in descriptive explanation. Counting, however, is inadequate for measurement of qualitative variables such as attitudes or values which may vary in direction with greater or lesser intensity. The same is true of sociological concepts such as anomie, cohesion,

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24 For relevant arguments and counterarguments, R. PERLOFF, *A Note on Brower's 'The Problem of Quantification in Psychological Science'*, « Psychological Review », 57, 4, 1950, pp. 188-192.

25 ANDREW L. COMREY, *An Operational Approach to Some Problems in Psychological Measurement*, « Psychological Review », 57, 1950, pp. 221-222.

26 Devised by STEVENS and VOLKMAN; see *ibid.*, p. 225; also FRED ATTNEAVE and RICHARD K. OLSON, *Pitch as a Medium: A New Approach to Psychophysical Scaling*, « American Journal of Psychology », 84, 1971, pp. 147-166.

27 JAMES S. COLEMAN, *Introduction to Mathematical Sociology* (New York: Free Press, 1964), p. 63 ff.

prestige, or social status. In such cases, attempts have been made to attach numerical values to the manifestations associated with these concepts. For example, group cohesion has been measured by an index which is the ratio of the number of times « we » was used to the number of times « I » was used in various groups. Such an index is not a fundamental measurement. It may, however, be operational at a cost, since indexing reduces the amount of information that can be embodied in the index and therefore leaves room for ambiguity.

Some indexing operations have managed to avoid dimensions that are not subject to counting and observation. An ambitious attempt by Allen and Bentz to measure sociocultural change falls in this category. The authors specify 32 indicators of change that seem relevant and measure the percentage change on each of them over the period 1940-1960 for 48 of the 50 United States. These indicators cover population, government and politics, communications, transportation, education, agriculture, health, welfare, crime, and the family. In all cases, however, the data are measurable by counting since the authors seem to have left out all dimensions that cannot be counted such as attitudes, class relations, and political views and values. To take an example, change in government and politics is measured only by the percentage changes in per capita state government employees. Having measured the percentage changes of these indicators, the authors compute intercorrelations between them, apply factor analysis, and end up with four main factors of change: a rising standard of living, population growth, industrial-technological-urban development, and increasing education. Each of these factors represents a complex of indicators from among the original 32 indicators. Finally, the authors search for an overall index as used by economists, and like the I.Q. used by psychologists. An arithmetic average of the revelant indicators is such an index in which the indicators are given equal weight. A variant of this is a weighted score index which, however, is found to be highly correlated with the unweighted index. Thus, the authors find it appropriate to rank the individual states according to the index to determine their relative position in sociocultural change.[28]

While the authors may have succeeded in computing a general index, it is not certain that they have succeeded in overcoming the problems of measurement of sociocultural phenomena. Not only have they left out the non-quantitative dimensions of change, but they seem to have defined change in a tautological manner. Social change implies a change in the standard of living, among other criteria, and, a change in the standard of living implies social change. Measuring one means measuring the other. There is also a high degree of over-simplification. What significance does an

[28] Francis R. Allen and W. Kenneth Bentz, *Toward the Measurement of Sociocultural Change*, « Social Forces », 43, 4, May 1965, pp. 522-532.

increase in the size of government mean? Quantitatively it is a change, but how does it imply social and cultural change? To ignore these questions could render the whole procedure of measurement insignificant. In other words, by leaving out information and by operationally defining change, quantitative measurement may be made feasible but also sterile.

By their own admission, anthropologists have made less use of quantitative data and measurement tools. Althought the material they work with is similar to what the sociologist and psychologist work with, their lagging behind has been attributed to two factors: first, anthropologists handle a relatively wide range of subject matter, both in terms of space and of content. Second, they study people who are less quantitatively minded than the people studied by the sociologists and psychologists. Therefore, quantitative data tend to be less accessible. Nevertheless, many attempts have been made to introduce quantitative analysis in social and cultural anthropology, extending back to the early years of this century.

The problems of measurement in anthropology are similar to those of the other social sciences and, therefore, there is no need to go into these problems in detail. However, a few differences may be noted. Anthropologists tend to use a different unit of study. They deal with tribes, clans, or families and kinship groups instead of individuals. It is more difficult to observe and record behavioural events of tribes than of individuals. Anthropologists also deal with cultural units such as folklore which can hardly be manifested in observable behaviour, even though folklore may be the source of such behaviour. As a result, measurement in anthropology seems to have been reduced to a matter of counting or the frequency of observation of a behavioural event. Studies of folklore are reduced to the classification and inventory of relevant stories. Social and political traits are tabulated and classified in attempts to establish a developmental stage pattern. The purpose of most such studies seems to have been the application of statistical analysis to the data. In search of validation of the finding, anthropologists have resorted to correlation and factor analysis, and to probability theory, all of which require numbers. This objective has been pursued actively even though frequently at the cost of oversimplification, since counting has usually ended in merely establishing the existence or non-existence of a trait, regardless of the intensity of its manifestation. The desire to quantify has reached almost completely unrealistic proportions in the attempts to quantify personality tests.[29]

In conclusion, social scientists in all the related fields have become measurement-conscious. While accepting the fact that cardinal measurement is beyond their reach, they have persisted in quantification even when dealing

[29] For examples see Harold E. Driver, *Statistics in Anthropology*, « American Anthropologist », 55, 1953, pp. 42-54.

with qualitative unobservable concepts. To do so, they have resorted to operationalization of the concepts, to devising special scales, and to the construction of indices, even though frequently at the cost of oversimplification and the loss of information.

3. *Formalization.* The use of numbers and statistical tools has been the main approach of social scientists to precision and verification of their findings. However, even where numbers are not accessible, formalization has been advanced as a substitute for the vague and diffuse description of the relations observed and phenomena analyzed. This has been one of the justifications for introducing mathematics into the social sciences. And to compensate for the difficulty of experimentation and comprehensive or exhaustive empirical observation, and as a source of hypotheses, social scientists have resorted to pseudoexperimentation in the form of gaming and simulation.

The use of mathematics has been advocated as a way of formalizing relationships and as an abstraction from observed data, both as a source of economy of words, and as a powerful tool of transformation by which logical conclusions may be deduced beyond the observed relationships. Mathematics is advocated as a neutral language with certain properties that render it more useful than the language of words for research purposes. The language of mathematics has an advantage in being neutral so that once a system of relationships has been perfected, it can be applied to any situation in which such relations may exist. And, once the objects have been represented by mathematical symbols, these objects can be manipulated experimentally by proxy in order to predict their behaviour, which may not be feasible in the case of the objects themselves. It is in this area that game theory and simulation are useful. The main precondition for such manipulation is that the initial conditions of the system containing these objects must be well known.[30]

The use of mathematics as an abstraction of observable behaviour and as a tool of analysis is no longer a point of debate. The main arguments and counterarguments centre around the feasibility of its application, given the inadequate knowledge of initial conditions and relations on which the analysis may be based. As theory and data become more accessible, mathematics and statistics seem to acquire additional significance. One area, however, which needs elaboration is the use of mathematics for prediction by hypothetical models which can neither be verified nor are they complex enough to approximate reality. This is the case of game theory and simulation models which have become fairly common in social science research. These approaches may be considered as substitutes for comprehensive observation and for experimental techniques which are inapplicable to the social sciences.

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30 COLEMAN, *op. cit.*, p. 3 ff.

Game theory offers an important opportunity to formalize behavioural relations, to deduce additional relations, and to formulate hypotheses that may be subjected to testing in empirical situations. In other words, gaming serves an heuristic function which relates to reality at least in essential features. For example, diplomats in international relations, military commanders in the field, labour unions engaged in bargaining with industry, or players of poker all behave in a manner which may be represented in games. Even though certain elements of the situation remain absent, the theory of behaviour derived from such gaming may eventually be modified to cope with reality. Thus, in the absence of sufficient grounds for induction, and to avoid pure intuition, gaming offers a good source of hypotheses of behaviour in somewhat similar situations. Economists and political scientists have recognized these positive aspects and have taken advantage of them.[31]

Simulation, especially computer simulation, has been carried out in psychology, sociology, economics, and political science. An impressive example is the simulation model constructed by Bernstein and Abelson, dealing with political controversies in local communities. The model specifies the general assumptions on which behaviour is analyzed—how voting on an issue may be influenced. The model also allows for resistance to the possible influence on the voting decision, for varying influences and types of exposure, for the lapse of time between the beginning of the campaign and the voting date, and for variation in the characteristics of the voters. The model was tested in a limited way by surveying the effect of influences on the vote in a mock fluoridation campaign. This kind of simulation is called by the authors prognostic simulation, in contrast to process simulation, since it carries « present statistics into anticipated future statistics in one leap without concern for the details of the intervening process ».[32]

The significance of such models is obviously limited since frequently neither the theory nor the data are available. Often the influences and factors that are relevant are not easily identifiable. Therefore, the results can hardly be indicative of the real behaviour in similar situations. Yet, such results can be invaluable as heuristic sources of hypotheses and theories. The question is whether such processes are worth the costs they incur. The variability of behaviour and the volitionalistic nature of social relations are such that simulation of complex social situations is highly imperfect,

[31] M. Shubik, *op. cit.*, especially Ch. 1; also Samuel Z. Klausner (ed.), *The Study of Total Societies* (Garden City, New York: Doubleday, 1967), pp. 30-44; for more recent examples: W. H. Riker and W. J. Zavoina, *Rational Behavior in Politics: Evidence from a Three Person Game,* « American Political Science Review », 64, 1, 1970, pp. 48-64; D. W. Rae, *Political Democracy as a Property of Political Institutions,* « American Political Science Review », 65, 1, 1971, pp. 111-119.

[32] R. A. Abelson, *Lectures on Computer Simulation*, in « Mathematics and Social Sciences », compiled by S. Sternberg, *et al.* (Mouton and Co., 1965), vol. I, p. 446.

in contrast to simulation in the physical sciences where the environment and the elements can be controlled. Nevertheless, in the absence of more useful technique, simulation serves an important function which explains the expansion of its application in the social sciences. As Brody put it, simulators

> ... share a common commitment to the proposition that social realities can be understood *via* the study of scaled-down versions of these realities... Whether programmed for high-speed computers or set into motion by human decision-makers enacting roles, a simulational model requires attention to operational theory that is unusual in social research. In the end, this development of operational theory may be simulation's most significant contribution.[33]

## IV. The Impact.

Given the space limitations, only a few impressions can be presented in an attempt to assess the achievements of the new methodology. The new economic historians and other social scientists have succeded in drawing attention to the need for specification and explicitness in stating the problem and method of research. They have aroused interest in measurement, validation, abstraction, and limited hypotheses. The degree of success in each of these areas has varied according to the field and the period under study. However, it may be proposed that the failures in these attempts may be as important as the successes. Actually a certain degree of reversal may have resulted from these failures, as has been apparent more recently.[34]

The trend in the social sciences, including economic history, has reflected a division between those who lean towards a scientific approach and those who are contented with a less precise and more introspective approach. Both groups, however, have shown interest in some kind of theory, either to guide the analysis or to be derived from it. Both have accepted the possibility of innovation in method. It is these innovations that should be of interest to us.

To begin with, formalism has become an integral part of modern research. The building of models and the use of mathematics have become central in the training of professionals, often to the extent of making the procedure more important than the subject matter. The same observations apply to the restriction of research to limited hypotheses. In order to test the validity of hypotheses scientifically, the tendency has been to formulate limited and testable hypotheses. It is apparent, however, that a certain

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[33] R. A. Brody, *Varieties of Simulation on International Relations Research*, in « Simulation in International Relations », Harold Guetskow, *et al.* (Englewood Cliffs: Prentice-Hall, 1963), p. 220; see also Ithiel de Sola Pool, *Computer Simulation of Total Societies*, in Klausner, *op. cit.*, pp. 45-65.

[34] W. N. Parker, *op. cit.*

degree of reversal away from reductionism has taken place. The reason has been the difficulty of comprehending the relevant situation when reduced to a limited framework; which may explain the apparent trend back to holism and interdisciplinary approaches. The social sciences have found it necessary to deal with policy matters which can be treated only in an interdisciplinary framework. While economic history is not concerned directly with policy, its implications to economic development and growth are such that all the relevant variables must be considered in the analysis.

A significant difference between economic history and the other social sciences is that those sciences have objectives and tools that do not fit in historical explanation. The social sciences have adopted a behavioural approach which treats observable behaviour. Past behaviour which is neither observable nor comprehensively recorded cannot be studied in the same fashion. Consequently, history must depend greatly on indirect indicators and on analytical and interpretative devices. Behaviouralism as practised in political science, sociology, and psychology cannot be applied to historical analysis. The same complication arises when considering gaming and simulation as potential tools of economic history. These tools have been used for heuristic purposes in policy contexts. Furthermore, these tools require complete knowledge or specification of the rules of behaviour and of the initial conditions in order to predict what decisions will be made. Economic history might be concerned with decisions which have been made in the past, but if the rules of behaviour and the initial conditions were fully known, there would be no need for gaming or simulation of the past. And even if gaming and simulation were applied, there would be no way of testing the models constructed. In the other social sciences testing might be possible at least by estimating the predictive power of the model. There is no place for such a test in economic history. Therefore, it would be presumptuous to imitate these innovations, unless economic history is reduced to a study of economic growth in the contemporary world.

Economic history can learn from the other social sciences in the area of measurement. These sciences have generally settled on interval and ordinal scales and have rendered their concepts measurable by operationalizing them. While counting has remained the main objective of measurement, special indices and scales have been devised for more limited objectives. Economic historians can apply similar approaches by operationalizing their concepts. Operationalizing the concept by decomposing might be inelegant, but it is useful for rendering the concept measurable. This would be true with concepts such as utility, economic change, development, and welfare.

Looking at these comparisons from the standpoint of the new economic history, it becomes clear that the new history has taken the same road as the modernists in the other social sciences. The cost, however, has been relatively high. Vast areas of interest to the historian have been slighted because the method has tended to determine the subject of research. Neither the new historians nor the modernists in the other social sciences have resolved the problems facing historical research. Systematization and precision have been at the expense of holism and interdisciplinary tendencies.

Will this be the trend in economic history? Most probably not; economic history will probably become more flexible and accommodating and allow for a meaningful division of labour. Those who follow the traditional approach will continue to be in the majority. They will accumulate data, formulate ideas and propose broad explanations on the strength of qualitative and relatively unsystematic observations. Their views will remain comprehensive and cover wide areas of subject matter in terms of both time and space. The meaning of scientific method will also be regarded flexible enough to cope with the approaches that have characterized historical research for a long time. The new economic historians, on the other hand, will gradually unite with theorists of development and growth but put their emphasis on the past. They will benefit from the work of the traditional historians to test hypotheses and reunite theory and history. The impact, however, can be expected to remain limited because the tools they use are of little relevance to historical research. Probably the most promising achievement would be for the traditional and new economic historians to restore their mutual respect for each other, join forces, and reunite to form effective research teams in search of knowledge and relatively valid explanation.