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## NOTES ON COMPARATIVE ANALYSIS OF NATIONAL ECONOMIES

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### **The Strategy of Comparative Study**

Economic teaching and research in the United States have suffered from a myopic preoccupation with Anglo-American ideas, institutions, and problems. From a scientific standpoint, it seems self-evident that sound generalization about economic behavior requires examination of a wide range of national economies. We can have, in effect, the benefit of twenty or thirty laboratory experiments instead of only one or two. This "geographical stretching" of the scope of economics is at least as necessary as the "historical stretching" that is rightly urged by historians and others.

In grappling with policy problems, too, we can gain wisdom by studying the way in which these problems have developed and been met in other countries. Many of the issues that are important in the American economy are important in other industrial countries as well, and some are virtually world-wide in scope. Examples come readily to mind: the possible inflationary consequences of strong unionism; the effectiveness of monetary policy in promoting stability of prices and employment; the relation of tax structures to economic incentives and economic growth; and the proper balance between agricultural and industrial prices and incomes.

While I am strongly in favor of comparative study, I feel that the "comparative economic systems" approach, which has been fashionable in recent decades, has probably done more to retard scientific progress than to advance it. This is not just because most work in this area has been qualitative and unanalytical. It is because the effort at a botanical classification of economies as "capitalist," "socialist," etc., is itself misguided. The economies ordinarily labeled as "capitalist" in such schema are dissimilar in many important respects, while economies that fall in different boxes—such as the United States and the U.S.S.R.—may show strong resemblances. A classification of economies based on output levels ("developed," "semideveloped," "underdeveloped") or on rate of growth ("stagnant," "growing," "rapidly growing") makes more sense but is still not very useful. Development is a matter of degree, and there are no sharp

breaking points. Most countries of the world can be classified as "underdeveloped," but this does not mean that they are structurally similar. I conclude that the effort at simple categorization of total economies is a blind alley, and that progress must be sought in other directions.

In curricular terms, I believe that progress lies through a massive infusion of foreign data and experience into the standard functional branches of economics—money and banking, public finance, industrial organization, labor economics, and the rest. Every course in the curriculum, at least at the graduate level, should be implicitly comparative. In research terms, progress lies through developing sharp hypotheses about selected aspects of economic behavior and testing these hypotheses with quantitative data from as many national economies as possible. In the present formative stage of economics, qualitative description should not be entirely excluded. Problems which can be framed in quantitative terms, however, give greater promise of definite results which may be added on to those of other studies using comparable data and techniques.

### **A Word About Data**

There would probably be widespread agreement on the types of national economic data that are desirable. If we had for each country of the world the economic statistics now available for the United States, and if these series extended backward for a hundred years, we should be in an economist's paradise. The actual situation is, of course, much less satisfactory. Large gaps appear in the current data for most countries of the world. The gaps widen rapidly as one proceeds backward in time. Problems of reliability and comparability are very serious. Conclusions drawn from such data require extensive footnoting and qualification.

One could argue that the first need is for improvement of national statistics, that effort should be concentrated on data accumulation, and that until much more has been done on this front, research efforts should be deferred. There is a good deal to this, but it overlooks the reciprocal relation between research and data accumulation. Framing hypotheses helps to determine what types of data are most useful. Aggressive research often develops new sources of data and new approaches to measurement problems. Research on very crude data may stimulate the production of more refined measurements and their subsequent maintenance on a current basis. The proper conclusion is not that research should be deferred, but rather that we must put up for the time being with wobbly data and tentative conclusions.

Accumulation of national statistics on a mass scale is the task of national governments, aided by the specialized agencies of the UN and by other intergovernmental bodies. But a private research group could do useful work in suggesting improvements in the quality and comparability of national statistics, particularly in economic accounting. There should be some place in the United States where a small group of Ameri-

can experts could work steadily on conceptual problems over a period of years. They might be joined from time to time by experts from other countries with advanced systems of economic accounting, such as Britain and Scandinavian countries. The center could also provide internship and training services for economic statisticians from the less developed nations, and thus have a gradual but cumulative impact on the content of statistical work in these countries. The objective would be not to do substantive research or even to accumulate large amounts of data, but simply to work toward standardization of concepts and methods of measurement. An outstanding example of the utility of this sort of work was the National Accounts Research Unit, directed by Richard Stone at Cambridge University, which developed a uniform system of accounts subsequently taken over and maintained currently by the OEEC countries.

### **Key Issues for Comparative Study**

It was argued earlier that any problem in economics can and should be analyzed on a comparative or multinational basis. Thus to ask, "What subjects are most deserving of comparative study?" amounts simply to asking, "What problems are most important in present-day economics?" Individual answers to this are bound to differ. I have selected for emphasis here a number of issues in two broad areas: (1) the evolution of national economies through time, and (2) the structure of national economies, by which I mean essentially the mechanism of resource allocation. This selection of issues is deliberately incomplete and does not mean that I regard other problems as unimportant.

### **The Evolution of Economies Through Time**

This is an area often designated as "development economics" or "growth economics." "Development" has a connotation of emphasis on low-income economies, while "growth" suggests a focus on total output and other national aggregates. My terminology is intended to cover detailed as well as aggregative changes, and to leave the door open for a certain amount of institutional description along with statistical measurement.

This area might also be termed "sophisticated economic history." It is significant that the most important generalizations about long-run change in the American economy have been developed, not by economic historians, but by economic theorists and statisticians. I have in mind the work of Kuznets, Schumpeter, Burns, Abramovitz, Fabricant, Stigler, and a number of others. One can make a good case that this *is* the new shape of economic history and that it *is* destined over the long run to supersede conventional economic history in the graduate curriculum.

The issues that impress me as most interesting are listed below in question form. This is not meant to imply that they are entirely unanswered. Much obviously has been done, most notably by Simon Kuznets.

Any attempt to consider the state of research on each topic, however, would run far beyond the limits of a brief paper.

*Growth of total and per capita national output.* What have been the long-term rates of increase, country by country, in aggregate and per capita output?

Can inter-country differences in growth rates be related to differences in rate of population increase, rate of capital accumulation, and other quantitative variables?

Within a particular country, is there typically a period of accelerating growth, followed by stable and perhaps eventually declining rates of growth? Is there evidence for Rostow's "take-off point" hypothesis or for the "mature economy" hypothesis?

*Capital accumulation in relation to growth.* What has been the course of gross and net investment, relative to GNP and NNP, in various countries over long periods?

How does one account for inter-country differences in investment levels, and for changes within the same country over time?

What is the relative importance of government saving, business saving, and personal saving as sources of investment finance in various countries, and what trends are observable over time?

What evidence is available on average and marginal capital-output ratios, and on changes in these over time?

*Internal changes accompanying aggregative growth.* What generalizations can be drawn about the changing importance of various output sectors as total output expands? The secular decline of agriculture is well established. Are there other trends of equal reliability?

Is there a "normal sequence" in the growth of manufacturing in an economy? Do certain industries typically lead or is there wide variation of leading industries?

Do changes in relative labor and capital supplies in the course of development have the effects that they "should have" on production methods, productivity ratios, factor prices, and labor-capital shares?

What are the long-run tendencies in the size distribution of personal incomes? Is there support for the hypothesis of gradual leveling of incomes in mature economies?

*Other questions about long-run change.* What has been the course of the price level in various countries over long periods? Is there a tendency toward chronic inflation under economic planning, whether of the Soviet type or the looser "development program" type? Is there evidence that the Western industrial economies are also more biased than formerly toward secular inflation?

Do developing economies show any regular pattern of change in their debtor-creditor position, the volume and composition of exports and imports, and other aspects of their international economic relations?

## **Economic Structure: The Allocative Mechanism**

The term "structure of a national economy" can be used in a variety of senses. It can mean the complex of physical input-output relationships. It can mean the financial mechanism through which monetary changes are transmitted to the productive system and vice versa. It can mean the parameters of the key equations in an aggregative economic model. Each of these is obviously an interesting and important object of research.

I use the term here in the traditional sense of the arrangements for pricing and allocation of productive resources and outputs. More specifically, my emphasis is on the mixture and interaction of "market" and "administrative" controls. "Structure" in this sense is believed to have important effects on the operating efficiency of an economy, the level of welfare which it yields, the nature of its short-term fluctuations, and the rate of capital accumulation and output growth.

I have tried to select a few aspects of economic structure which seem intrinsically important and on which one might hope to marshal quantitative evidence.

*Inter-industry distribution of resources under private enterprise.* It is often said that some capitalist economies are more competitive, flexible, and market-controlled than others (France, for example, is contrasted with the United States). What quantitative indicators can be devised which would lend greater precision to such statements?

In most economies the marginal product of labor is substantially lower in agriculture than in industry. How can one explain the persistence of this phenomenon over long periods? How can one explain inter-country differences in the gap between agricultural and urban productivity?

Is there any secular tendency toward over- or under-allocation of resources to other sectors? (Retail trade, for example, is allegedly over-manned in many countries.)

Schumpeter and others have advanced hypotheses about the relation between industrial structure (age of an industry, size of establishments, competitive or monopolistic product market) and the technical progressiveness of the industry. What can be done with this problem from a quantitative standpoint?

Does the distribution of the employed population among major occupational strata conform broadly to competitive principles? Or are there serious barriers to upward mobility, leading to distorted allocation? How substantial are inter-country differences in this respect, and what are the trends over time?

*Resource use in the public sector of capitalist economies.* How great is the inter-country variation in the scope of the public sector, *i.e.* the percentage of national product produced under government auspices? Is there a secular tendency toward expansion of the public sector?

Are there systematic differences in the price, output, and investment

policies of government-owned "public utilities" compared with those of private enterprises in the same industries?

Is there any tendency toward over- or under-allocation of resources to public industries providing services on a nonprice basis? Are public services "starved" or "swollen"?

*Resource allocation and use under central planning.* What has been the broad allocation of output between private consumption, communal consumption, and investment in Soviet-type economies? What are the major differences from capitalist economies, and what have been the significant trends over time?

Can one say that agriculture is generally "exploited" in these economies? What are the mechanisms used and how effectively have they operated?

What is the significance of cost and price calculations as a basis for, or an aid to, administrative controls over production? Examples would be use of cost calculations in decisions about production methods and in control of production efficiency, and use of retail prices as a rationing device and conceivably as an indicator of consumer preference.

What are the significant similarities and differences between the operation of labor markets in Soviet-type and in unplanned economies? Does the wage mechanism operate in substantially the same fashion? Are there differences in the barriers to penetration of the upper occupational strata?

How does the blend of market and administrative controls in the Yugoslav economy differ from that in the Soviet model? What difference does this make in the day-to-day operation of the economy? What light does experience in Yugoslavia and elsewhere shed on the feasibility of "market socialism"?

*Development programming in low-income economies.* What is the scope and logic of the "five-year plans" and "development programs" now used in many countries of Asia and Latin America? Do they contain an appreciable element of central planning in the Russian sense? Have these programs had an observable effect on inter-industry resource allocation, productivity, saving and investment levels, and rate of increase in output? Perhaps more properly, why have *some* national programs had such effects and others not?

### **A Comment on Research Needs and Strategy**

The enormous scope of these topics illustrates the point made at the outset, i.e. comparative economic analysis is not a branch or subdivision of economics. It is a point of view, a way of approaching any significant problem in economics.

The range of important problems deserving comparative study is very wide. The available data are quite crude for most countries and will improve only gradually over the decades to come. Few talented people are exploiting even the data presently available. On many problems there has

not been enough exploratory work to develop plausible hypotheses. The main need at present is for more exploratory studies of a rather broad, rough, and preliminary character. It is a great contribution at this stage simply to lay out techniques for attacking significant problems, to expose major inadequacies in the data, and to develop hypotheses that may prove useful for subsequent work. "It is better to ask some of the questions than to have all of the answers."

The fact that quantitative analysis and comparison of national economies are still in such formative stages argues for a large measure of individual entrepreneurship over the foreseeable future. It is important that more good economists be persuaded to undertake this kind of work and that they approach it with a variety of techniques and substantive interests. They should be encouraged by research support on a scale adequate to their personal interests and needs. It would seem profitable to link their efforts through a standing research committee under proper institutional auspices. In addition to exchanging information on their own plans and activities, the members of such a committee could help to "develop and plant" research ideas in promising locations and to recruit new talent into the field.

It may be that something would be gained by going beyond this and bringing several able scholars together for continuing work in the same physical location. This is not an issue of principle but a practical question which could only be decided on the basis of answers to a number of sub-questions, including:

1. In what areas, if any, have we reached the stage—both in terms of theoretical structures and availability of data—at which the main need is facilities for large-scale data control and processing? (Economies of scale in social science research seem to be mainly associated with handling of mass data.)

2. Who would be willing to manage such a center? Would the reduction of the manager's own research effectiveness because of administrative duties be more than offset by an increase in the productivity of his colleagues?

3. Where would the center be located? Could scholars of the desired quality be persuaded to leave their present locations and settle in the new one? And would the gain in efficiency more than offset the dislocation costs?

4. Related to this, what are the advantages of university as against non-university locations? (I must confess to some preference for university locations, partly because a graduate school provides a ready supply of research apprentices, and partly because of the advantages of exposing one's half-formulated hypotheses and conclusions to a critical audience of students and colleagues.)

5. Wherever the center might be located, how could it be made serviceable to scholars throughout the United States and in other countries?

6. What assurance of continuity in financing would be desirable and feasible? (The prevalent practice of financing supposedly long-term ventures through short-term foundation grants presents obvious difficulties to all concerned.)

These questions and others would need to be faced realistically in order to appraise the feasibility of the center idea. Certainly a good deal of additional thought and planning would be necessary to arrive at an informed conclusion.

In the long run, the main hope lies in interesting good young economists in applying refined theoretical and statistical methods to comparative economic analysis. One way of doing this is through research seminars or workshops for advanced graduate students. I have developed at Yale a "Workshop in National Economic Organization" with precisely this purpose in mind. The group consists each year of 15 to 20 second- and third-year graduate students, about half of whom are usually from foreign countries. The workshop technique has proven feasible and stimulating, and a number of student projects have developed into thesis subjects. The main problem is that foreign field work is usually desirable, and this requires financing. I consider foundation support in this direction even more important than support for the work of mature scholars. A small investment in an able young man can yield remarkably high returns.