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Theoretical Assumptions and Nonobserved Facts

By WASSILY LEONTIEF*

Economics today rides the crest of intellectual respectability and popular acclaim. The serious attention with which our pronouncements are received by the general public, hard-bitten politicians, and even skeptical businessmen is second only to that which was given to physicists and space experts a few years ago when the round trip to the moon seemed to be our only truly national goal. The flow of learned articles, monographs, and textbooks is swelling like a tidal wave; *Econometrica*, the leading journal in the field of mathematical economics, has just stepped up its publication schedule from four to six issues per annum.

And yet an uneasy feeling about the present state of our discipline has been growing in some of us who have watched its unprecedented development over the last three decades. This concern seems to be shared even by those who are themselves contributing successfully to the present boom. They play the game with professional skill but have serious doubts about its rules.

Much of current academic teaching and research has been criticized for its lack of relevance, that is, of immediate practical impact. In a nearly instant response to this criticism, research projects, seminars and undergraduate courses have been set up on poverty, on city and small town slums, on pure water and fresh air. In an almost Pavlovian reflex, whenever a new

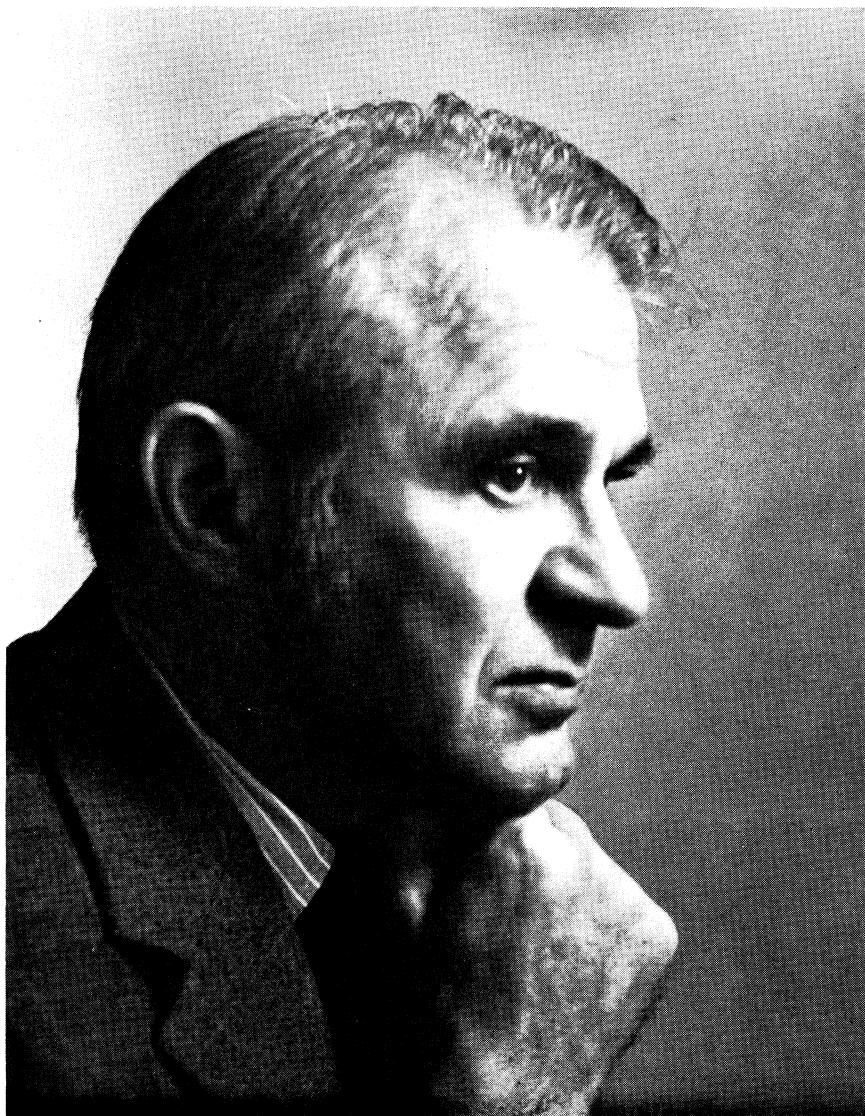
complaint is raised, President Nixon appoints a commission and the university announces a new course. Far be it from me to argue that the fire should not be shifted when the target moves. The trouble is caused, however, not by an inadequate selection of targets, but rather by our inability to hit squarely any one of them. The uneasiness of which I spoke before is caused not by the *irrelevance* of the practical problems to which present day economists address their efforts, but rather by the palpable *inadequacy* of the scientific means with which they try to solve them.

If this simply were a sign of the overly high aspiration level of a fast developing discipline, such a discrepancy between ends and means should cause no worry. But I submit that the consistently indifferent performance in practical applications is in fact a symptom of a fundamental imbalance in the present state of our discipline. The weak and all too slowly growing empirical foundation clearly cannot support the proliferating superstructure of pure, or should I say, speculative economic theory.

Much is being made of the widespread, nearly mandatory use by modern economic theorists of mathematics. To the extent to which the economic phenomena possess observable quantitative dimensions, this is indisputably a major forward step. Unfortunately, any one capable of learning elementary, or preferably advanced calculus and algebra, and acquiring acquaintance with the specialized terminology of economics can set himself up as a theorist. Uncritical enthusiasm for mathematical formulation tends often to con-

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Basil Beaton

ceal the ephemeral substantive content of the argument behind the formidable front of algebraic signs.

Professional journals have opened wide their pages to papers written in mathematical language; colleges train aspiring young economists to use this language; graduate schools require its knowledge and reward its use. The mathematical model-building industry has grown into one of the most prestigious, possibly the most prestigious branch of economics. Construction of a typical theoretical model can be handled now as a routine assembly job. All principal components such as production functions, consumption and utility functions come in several standard types; so does the optional equipment as, for example, "factor augmentation"—to take care of technological change. This particular device is, incidentally, available in a simple exponential design or with a special automatic regulator known as the "Kennedy function." Any model can be modernized with the help of special attachments. One popular way to upgrade a simple one-sector model is to bring it out in a two-sector version or even in a still more impressive form of the " n -sector," that is, many-sector class.

In the presentation of a new model, attention nowadays is usually centered on a step-by-step derivation of its formal properties. But if the author—or at least the referee who recommended the manuscript for publication—is technically competent, such mathematical manipulations, however long and intricate, can even without further checking be accepted as correct. Nevertheless, they are usually spelled out at great length. By the time it comes to interpretation of the substantive *conclusions*, the assumptions on which the model has been based are easily forgotten. But it is precisely the empirical validity of these *assumptions* on which the usefulness of the entire exercise depends.

What is really needed, in most cases, is a very difficult and seldom very neat assessment and verification of these assumptions in terms of observed facts. Here mathematics cannot help and because of this, the interest and enthusiasm of the model builder suddenly begins to flag: "If you do not like my set of assumptions, give me another and I will gladly make you another model; have your pick."

Policy oriented models, in contrast to purely descriptive ones, are gaining favor, however nonoperational they may be. This, I submit, is in part because the choice of the final policy objectives—the selection and justification of the shape of the so-called objective function—is, and rightly so, considered based on normative judgment, not on factual analysis. Thus, the model builder can secure at least some convenient assumptions without running the risk of being asked to justify them on empirical grounds.

To sum up with the words of a recent president of the Econometric Society, "... the achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that there is something scandalous in the spectacle of so many people refining the analysis of economic states which they give no reason to suppose will ever, or have ever, come about.... It is an unsatisfactory and slightly dishonest state of affairs."

But shouldn't this harsh judgment be suspended in the face of the impressive volume of econometric work? The answer is decidedly no. This work can be in general characterized as an attempt to compensate for the glaring weakness of the data base available to us by the widest possible use of more and more sophisticated statistical techniques. Alongside the mounting pile of elaborate theoretical models we see a fast-growing stock of equally intricate statistical tools. These

are intended to stretch to the limit the meager supply of facts.

Since, as I said before, the publishers' referees do a competent job, most model-testing kits described in professional journals are internally consistent. However, like the economic models they are supposed to implement, the validity of these statistical tools depends itself on the acceptance of certain convenient assumptions pertaining to stochastic properties of the phenomena which the particular models are intended to explain; assumptions that can be seldom verified.

In no other field of empirical inquiry has so massive and sophisticated a statistical machinery been used with such indifferent results. Nevertheless, theorists continue to turn out model after model and mathematical statisticians to devise complicated procedures one after another. Most of these are relegated to the stockpile without any practical application or after only a perfunctory demonstration exercise. Even those used for a while soon fall out of favor, not because the methods that supersede them perform better, but because they are new and different.

Continued preoccupation with imaginary, hypothetical, rather than with observable reality has gradually led to a distortion of the informal valuation scale used in our academic community to assess and to rank the scientific performance of its members. Empirical analysis, according to this scale, gets a lower rating than formal mathematical reasoning. Devising a new statistical procedure, however tenuous, that makes it possible to squeeze out one more unknown parameter from a given set of data, is judged a greater scientific achievement than the successful search for additional information that would permit us to measure the magnitude of the same parameter in a less ingenious, but more reliable way. This despite the fact that in all too many instances sophisti-

cated statistical analysis is performed on a set of data whose exact meaning and validity are unknown to the author or rather so well known to him that at the very end he warns the reader not to take the material conclusions of the entire "exercise" seriously.

A natural Darwinian feedback operating through selection of academic personnel contributes greatly to the perpetuation of this state of affairs. The scoring system that governs the distribution of rewards must naturally affect the make-up of the competing teams. Thus, it is not surprising that the younger economists, particularly those engaged in teaching and in academic research, seem by now quite content with a situation in which they can demonstrate their prowess (and incidentally, advance their careers) by building more and more complicated mathematical models and devising more and more sophisticated methods of statistical inference without ever engaging in empirical research. Complaints about the lack of indispensable primary data are heard from time to time, but they don't sound very urgent. The feeling of dissatisfaction with the present state of our discipline which prompts me to speak out so bluntly seems, alas, to be shared by relatively few. Yet even those few who do share it feel they can do little to improve the situation. How could they?

In contrast to most physical sciences, we study a system that is not only exceedingly complex but is also in a state of constant flux. I have in mind not the obvious change in the variables, such as outputs, prices or levels of employment, that our equations are supposed to explain, but the basic structural relationships described by the form and the parameters of these equations. In order to know what the shape of these structural relationships actually are at any given time, we have to keep them under continuous surveillance.

By sinking the foundations of our ana-

lytical system deeper and deeper, by reducing, for example, cost functions to production functions and the production functions to some still more basic relationships eventually capable of explaining the technological change itself, we should be able to reduce this drift. It would, nevertheless, be quite unrealistic to expect to reach, in this way, the bedrock of invariant structural relationships (measurable parameters) which, once having been observed and described, could be used year after year, decade after decade, without revisions based on repeated observation.

On the relatively shallow level where the empirically implemented economic analysis now operates even the more invariant of the structural relationships, in terms of which the system is described, change rapidly. Without a constant inflow of new data the existing stock of factual information becomes obsolete very soon. What a contrast with physics, biology or even psychology where the magnitude of most parameters is practically constant and where critical experiments and measurements don't have to be repeated every year!

Just to keep up our very modest current capabilities we have to maintain a steady flow of new data. A progressive expansion of these capabilities would be out of the question without a continuous and rapid rise of this flow. Moreover, the new, additional data in many instances will have to be qualitatively different from those provided hitherto.

To deepen the foundation of our analytical system it will be necessary to reach unhesitatingly beyond the limits of the domain of economic phenomena as it has been staked out up to now. The pursuit of a more fundamental understanding of the process of production inevitably leads into the area of engineering sciences. To

penetrate below the skin-thin surface of conventional consumption functions, it will be necessary to develop a systematic study of the structural characteristics and of the functioning of households, an area in which description and analysis of social, anthropological and demographic factors must obviously occupy the center of the stage.

Establishment of systematic cooperative relationships across the traditional frontiers now separating economics from these adjoining fields is hampered by the sense of self-sufficiency resulting from what I have already characterized as undue reliance on indirect statistical inference as the principal method of empirical research. As theorists, we construct systems in which prices, outputs, rates of saving and investment, etc., are explained in terms of production functions, consumption functions and other structural relationships whose parameters are assumed, at least for arguments' sake, to be known. As econometricians, engaged in what passes for empirical research, we do not try, however, to ascertain the actual shapes of these functions and to measure the magnitudes of these parameters by turning up new factual information. We make an about face and rely on indirect statistical inference to derive the unknown structural relationships from the observed magnitudes of prices, outputs and other variables that, in our role as theoreticians, we treated as unknowns.

Formally, nothing is, of course, wrong with such an apparently circular procedure. Moreover, the model builder in erecting his hypothetical structures is free to take into account all possible kinds of factual knowledge and the econometrician in principle, at least, can introduce in the estimating procedure any amount of what is usually referred to as "exogenous" information before he feeds his pro-

grammed tape into the computer. Such options are exercised rarely and when they are, usually in a casual way.

The same well-known sets of figures are used again and again in all possible combinations to pit different theoretical models against each other in formal statistical combat. For obvious reasons a decision is reached in most cases not by a knock-out, but by a few points. The orderly and systematic nature of the entire procedure generates a feeling of comfortable self-sufficiency.

This complacent feeling, as I said before, discourages venturesome attempts to widen and to deepen the empirical foundations of economic analysis, particularly those attempts that would involve crossing the conventional lines separating ours from the adjoining fields.

True advance can be achieved only through an iterative process in which improved theoretical formulation raises new empirical questions and the answers to these questions, in their turn, lead to new theoretical insights. The "givens" of today become the "unknowns" that will have to be explained tomorrow. This, incidentally, makes untenable the admittedly convenient methodological position according to which a theorist does not need to verify directly the factual assumptions on which he chooses to base his deductive arguments, provided his empirical conclusions seem to be correct. The prevalence of such a point of view is, to a large extent, responsible for the state of splendid isolation in which our discipline nowadays finds itself.

An exceptional example of a healthy balance between theoretical and empirical analysis and of the readiness of professional economists to cooperate with experts in the neighboring disciplines is offered by Agricultural Economics as it developed in this country over the last fifty years. A

unique combination of social and political forces has secured for this area unusually strong organizational and generous financial support. Official agricultural statistics are more complete, reliable, and systematic than those pertaining to any other major sector of our economy. Close collaboration with agronomists provides agricultural economists with direct access to information of a technological kind. When they speak of crop rotation, fertilizers, or alternative harvesting techniques, they usually know, sometimes from personal experience, what they are talking about. Preoccupation with the standard of living of the rural population has led agricultural economists into collaboration with home economists and sociologists, that is, with social scientists of the "softer" kind. While centering their interest on only one part of the economic system, agricultural economists demonstrated the effectiveness of a systematic combination of theoretical approach with detailed factual analysis. They also were the first among economists to make use of the advanced methods of mathematical statistics. However, in their hands, statistical inference became a complement to, not a substitute for, empirical research.

The shift from casual empiricism that dominates much of today's econometric work to systematic large-scale factual analysis will not be easy. To start with, it will require a sharp increase in the annual appropriation for Federal Statistical Agencies. The quality of government statistics has, of course, been steadily improving. The coverage, however, does not keep up with the growing complexity of our social and economic system and our capability of handling larger and larger data flows.

The spectacular advances in computer technology increased the economists' potential ability to make effective analytical use of large sets of detailed data. The time

is past when the best that could be done with large sets of variables was to reduce their number by averaging them out or what is essentially the same, combining them into broad aggregates; now we can manipulate complicated analytical systems without suppressing the identity of their individual elements. There is a certain irony in the fact that, next to the fast-growing service industries, the areas whose coverage by the Census is particularly deficient are the operations of government agencies, both federal and local.

To place all or even the major responsibility for the collection of economic data in the hands of one central organization would be a mistake. The prevailing decentralized approach that permits and encourages a great number of government agencies, non-profit institutions and private businesses engaged in data gathering activities acquitted itself very well. Better information means more detailed information and detailed specialized information can be best collected by those immediately concerned with a particular field. What is, however, urgently needed is the establishment, maintenance and enforcement of coordinated uniform classification systems by all agencies, private as well as public, involved in this work. Incompatible data are useless data. How far from a tolerable, not to say, ideal state our present economic statistics are in this respect, can be judged by the fact that because of differences in classification, domestic output data cannot be compared, for many goods, with the corresponding export and import figures. Neither can the official employment statistics be related without laborious adjustments to output data, industry by industry. An unreasonably high proportion of material and intellectual resources devoted to statistical work is now spent not on the collection of primary information but on a frustrating and wasteful struggle

with incongruous definitions and irreconcilable classifications.

Without invoking a misplaced methodological analogy, the task of securing a massive flow of primary economic data can be compared to that of providing the high energy physicists with a gigantic accelerator. The scientists have their machines while the economists are still waiting for their data. In our case not only must the society be willing to provide year after year the millions of dollars required for maintenance of a vast statistical machine, but a large number of citizens must be prepared to play, at least, a passive and occasionally even an active part in actual fact-finding operations. It is as if the electrons and protons had to be persuaded to cooperate with the physicist.

The average American does not seem to object to being interviewed, polled, and surveyed. Curiosity, the desire to find out how the economic system (in which most of us are small gears, and some, big wheels) works might in many instances provide sufficient inducement for cooperation of this kind.

One runs up, of course, occasionally against the attitude that "what you don't know can't hurt you" and that knowledge might be dangerous: it may generate a desire to tinker with the system. The experience of these years seems, however, to have convinced not only most economists—with a few notable exceptions—but also the public at large that a lack of economic knowledge can hurt badly. Our free enterprise system has rightly been compared to a gigantic computing machine capable of solving its own problems automatically. But any one who has had some practical experience with large computers knows that they do break down and can't operate unattended. To keep the automatic, or rather the semi-automatic, engine of our economy in good working order we must not only understand the general

principles on which it operates, but also be acquainted with the details of its actual design.

A new element has entered the picture in recent years—the adoption of methods of modern economic analysis by private business. Corporate support of economic research goes as far back as the early 1920's when Wesley Mitchell founded the National Bureau. However, it is not this concern for broad issues of public policies or even the general interest in economic growth and business fluctuations that I have in mind, but rather the fast-spreading use of advanced methods of Operations Research and of so-called Systems' Analysis. Some of the standard concepts and analytical devices of economic theory first found their way into the curricula of our business schools and soon after that, sophisticated management began to put them into practice. While academic theorists are content with the formulation of general principles, corporate operations researchers and practical systems' analysts have to answer questions pertaining to specific real situations. Demand for economic data to be used in practical business planning is growing at an accelerated pace. It is a high quality demand: business users in most instances possess first-hand technical knowledge of the area to which the data they ask for refer. Moreover, this demand is usually "effective." Profit-making business is willing and able to pay the costs of gathering the information it wants to have. This raises the thorny question of public access to privately collected data and of the proper division of labor and coopera-

tion between government and business in that fast-expanding field. Under the inexorable pressure of rising practical demand, these problems will be solved in one way or another. Our economy will be surveyed and mapped in all its many dimensions on a larger and larger scale.

Economists should be prepared to take a leading role in shaping this major social enterprise not as someone else's spokesmen and advisers, but on their own behalf. They have failed to do this up to now. The Conference of Federal Statistics Users organized several years ago had business, labor, and many other groups represented among its members, but not economists as such. How can we expect our needs to be satisfied if our voices are not heard?

We, I mean the academic economists, are ready to expound, to any one ready to lend an ear, our views on problems of public policy: give advice on the best ways to maintain full employment, to fight inflation, to foster economic growth. We should be equally prepared to share with the wider public the hopes and disappointments which accompany the advance of our own often desperately difficult, but always exciting intellectual enterprise. This public has amply demonstrated its readiness to back the pursuit of knowledge. It will lend its generous support to our venture too, if we take the trouble to explain what it is all about.

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