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Chapter Author: Wassily Leontief

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Some Basic Problems of Empirical Input-Output Analysis

WASSILY LEONTIEF
HARVARD UNIVERSITY

A. Accuracy of Individual Entries

A voluminous, laboriously compiled collection of statistical data—and a typical detailed input-output table is nothing if it is not that—can be approached with many different questions in mind. Some of the more basic of these are discussed here.

First of all, there is the immediate problem of the numerical accuracy of the individual entries. How closely does the figure stating the dollar value or tonnage of paints and varnishes produced by the chemical industry and absorbed in 1947 by the automobile industry correspond to the actual magnitude it purports to represent? Is the amount of electricity used in steelmaking correctly stated in the entry so labeled?

In answering such questions, of course, one first of all has to make sure that terms such as "paints and varnishes," "chemical industry," or "automobile industry" used by the inquirer refer to the same things that the corresponding entries in the input-output table are supposed to refer to.

When it is asked whether the retail price level has actually risen in a given year by 10 per cent—as the BLS index tells us it has—the meaning of the term "price level" requires, indeed, considerable explanation. There is even good reason to doubt whether such an explanation could ever be unequivocal. The same applies to the definition of, say, the "physical output of manufactured goods" or the "annual rate of real investment."

The less aggregative, the less index-number-like the objects we are trying to measure, the firmer will be our terminological foothold. For this reason, the meaning of the label attached to the individual entry in a reasonably detailed input-output table can be expected to be more precise than the definition of the broader concepts mentioned above. What is even more important, this meaning is bound to gain in definiteness as the number of rows and columns in such a table increases, i.e. as the breakdown of the economy into its separate sectors is progressively refined. To be sure, the index-number problem cannot vanish entirely. Everyone who has actually been engaged in the task of filling the individual cells of the input-output grid will readily testify to that. With progressive subdivision, however, the scale of the aggregative indeterminacy is radically reduced; instead of adding together automobiles and shoes, one has to combine only different kinds of shoes on the one hand and, say, trucks and passenger cars on the other.

B. Dependence on Indirect Estimating Procedures

The question of numerical accuracy unfortunately cannot be answered as simply and directly as it can be posed. In order to know how inaccurate are the figures presented in published tables, one would have to possess the true measures of the magnitudes in question; but if these were available, they certainly should have been used in the first place. In direct observation, an error known is an error corrected.

Actually, none of the figures entered in the present input-output tables were obtained through immediate observation. Most have been arrived at through application of the usual indirect estimating procedures to various well-known sets of primary data. It is not knowledge of the correct answers, but rather past experience with the use of similar indirect procedures based on the same approximate assumptions, that enables one to form a more or less definite opinion concerning the reliability of the final statistical entries.

Compilation of such a large body of organized statistical information involves the use of many special techniques that, even if they are not commonly referred to as theories and hypotheses, still can be and have been tested by repeated application. I speak here not of general textbook precepts for statistical procedure but rather of those informal rules and methods of estimation or interpretation of information pertaining to specific commodities or industries that are usually based on detailed and intimate familiarity with the operating characteristics of these particular sectors of the economy.

On the higher levels of theoretical analysis—in confronting, for example, the problem of general interdependence or the question of economic change—the economist is bound to use concepts and to develop generalizations that are peculiarly his own, and entirely different from the notions and generalizations evolved for purposes of practical decision making and for use in everyday economic transactions. Whenever he approaches the consideration of the more esoteric problems directly from the second floor, so to speak, much of the ground-floor experience gained from everyday economic intercourse and practical decision making, much of the basic factual information buried in private business records and summarized in the unwritten rules of thumb followed by engineers, plant managers, or advertising executives in their particular narrow range of practical experience, will remain beyond the pale of the organized body of economic information.

Most of the data presented in the 1947 input-output tables were produced in what essentially amounts to a second- or even a third-floor operation. Only in a few instances has disaggregation progressed far enough to permit the use of direct ground-floor information. No high-level data, not even those based on corporate balance-sheet records, for example, can yield other than very superficial descriptions of the actual physical structure of various industries. Only a systematic, painstakingly detailed study of actual plant construction and operation practices seems to yield pertinent quantitative data. In other directions, too, the only practicable line of further advance seems to lead through disaggregation toward detailed first-hand information.

C. *Application of Formal Statistical Methods*

The breakdown of the conventional boundaries that have traditionally separated the macroeconomic from the microeconomic type of factual analysis may offer a workable approach toward efficient use of the methods of mathematical statistics in the service of quantitative economic analysis.

I have often been asked why such an ambitious data-gathering operation as that involved in the construction of a large input-output table makes no use whatsoever of the great resources of modern mathematical statistics. Why are our data not presented with proper appurtenances of standard deviations, coefficients of variation, and other appropriate measures of statistical reliability? The answer is that in its present stage this type of quantitative economic analysis can hardly make efficient use of the powerful, but at the same time narrowly conditioned in their application, tools of probabilistic approach.

Quantitative phenomena, as they are observed in various fields of empirical analysis, can be broadly subdivided into three distinct types. The first type is exemplified by problems of classical mechanics. These can be formulated in terms of two, three, perhaps five, or even eight, but in any case a very few, variables. The observed facts are described in exact, nonstatistical terms, and the final solution is stated in the same way.

The phenomena belonging to the second type are mass phenomena. They involve very large numbers of variables—thousands, hundreds of thousands, millions. (In this context, one should count as separate variables the last elements of the explanatory scheme, not the representative stochastic variables one might use to summarize the quantitative behavior of the entire "population.") The classical problems of thermodynamics and some actuarial problems in the study of population are of this kind. Here, the probabilistic approach finds its most direct, simple, and efficient application.

Finally, there is the third, intermediate type. The theoretical formulation of problems presented by it involves the use of from, say, ten to a hundred, or possibly five hundred, distinct variables. This is very many indeed for exact observation and precise solution under the usual experimental or observational conditions, but all too few for efficient stochastic description and reliable statistical analysis. Many problems in biology and meteorology are of this kind, but it is in the field of economics, at least in the part of it associated with the central problems of general interdependence, that this bothersome third type seems to dominate. This is why the numerous attempts to describe and explain the operation of a national economy in terms of a few variables—as if it belonged to the first of the three types of phenomena mentioned above—have always bogged down in the morass of index number difficulties and inconstant (hypothetical) structural relationships. On the other hand, even the most ingenious of the econometric models based on the assumption that the economic system can be explained as a mass phenomenon, in the sense that it is reducible to a combination of a few stochastic variables, have also failed to do justice to the heterogeneity—and what in these simplified terms appears to be the capricious irregularity—of that statistical universe. Moreover, it does not seem likely that some new combination of the same—or perhaps different—five, ten, or twenty variables, emerging from the application of an even more refined formal statistical precedure, will do the job.

One might as well be resigned to the fact that the economic system can be adequately described and its operation satisfactorily explained only in terms of a conceptual scheme involving a very large and statistically irreducible number of operationally distinct but mutually dependent variables. Accordingly, the researcher must formulate a unified conceptual framework and then proceed with the systematic collection and organization of the necessary quantitative factual information.

This is exactly what has been done in recent years in the field of input-output analysis. The theoretical formulation—I shall return to it later—and the large-scale computational manipulation required in this approach apparently do not present insurmountable difficulties. The success or failure of the whole enterprise will depend more immediately on our ability to master the formidable fact-finding task that it involves.

One must begin with the simple and direct collection of facts and figures, stratified in accordance with some unconditionally formulated, over-all theoretical design. The underlying theory may be erroneous and the figures wrong, but irrespective of that, in this first and in a sense most critical stage of the game (if the design is halfway complicated), formal methods of statistical inference can be of little use in improving the theory and correcting the figures. To apply such methods successfully, one must already have provisionally accepted some basic theory—the statistician often refers to it as the basic set of admissible hypotheses—but it is exactly the formulation and empirical implementation of such a conceptual framework that must be accomplished in the first stage.

Mathematical statistics will, however, become very useful, nay indispensable, in the second stage, which begins after all the principal parts of the analytical structure have been erected and one can turn to a more precise fitting and mutual adjustment of its originally rough-hewn components.

In any case, such seems to have been the experience with the application of advanced statistical procedures in those fields in which it has proved to be incontestably successful. In all these instances, the heavy guns of probabilistic formulation were moved into position only after the general layout of the terrain had been thoroughly explored, and the immediate, strictly limited objective of the statistical operation very thoroughly and unconditionally defined—be it the measurement of the effect of a certain fertilizer on the potato yield or the study of the relationship between the average productivity of machine-lathe operators and the length of the labor day. In other words, once this advanced level has been reached, the problem at hand will more often than not fall within the second of the three distinct categories mentioned at the beginning of this paper.

In economic analysis, as in other fields of empirical inquiry, the application of formal statistical procedures can be expected to become really effective only after the analytical, as well as the descriptive, stratification of the factual material has progressed to the level of such specific details that the choice of "admissible hypotheses" can be passed into the hands of the appropriate specialists—be it production engineers, merchandising experts, actuaries, or perhaps social psychologists—and the range of the possible statistical alternatives is thus radically reduced through specific knowledge of the underlying structures.

This does not mean, of course, that one might not occasionally hit upon a strikingly simple and constant distribution or a remarkably persistent correlation well in advance of this particular stage. But these are freakish exceptions that only reaffirm the general rule, and are about as well known to economists as the shooting of a fox at the corner of 42nd Street and Broadway to professional hunters.

D. Theoretical Unification of the Data

After what has been said above, it should be understandable why the compilation of an input-output table cannot be judged simply as a strictly descriptive enterprise but, on the contrary, must be interpreted as the fact-finding component of a rather elaborate analytical venture. This is not the proper occasion for undertaking a systematic survey of all the controversial points that have been raised in the current discussion of the input-output theory.[1] I shall take up only two issues which seem to have immediate bearing on the future direction of empirical research in that field.

The first of these is the question of the general-purpose versus the special-purpose approach. Input-output analysis, with its footing in the classical general equilibrium theory, is definitely oriented toward the former. In it the explanation of what happens in any one part or aspect of the economy is systematically connected with the explanation of what happens in each of its other sectors or its other

[1] One of the discussants of this paper, Rutledge Vining, advances the opinion that the general conceptual framework underlying input-output analysis and the corresponding formal system of general relationships between the structural parameters of the economy, the outputs, prices, final demand, etc., does not constitute a "theory." To satisfy his semantic predilection, I gladly would substitute the expression "conceptual framework" whenever I now speak of theory. But, even if my defenders accepted it with good grace, would not such a change give my other critics, who both at this Conference and elsewhere find things wrong with the "input-output theory," a just reason to complain that in quibbling about words I try to weasel my way out of a serious discussion of substantive issues at hand?

aspects: the study of the price system is based on the same set of factual data as the analysis of the physical quantities; the study of the locational pattern of economic activities, instead of being set up as an entirely independent inquiry, has from the outset been integrated into the common theoretical framework, and is based consistently on the same primary quantitative information.

That radical empiricism—as much statistical as institutional—should be fundamentally opposed to such emphasis on theoretical unification is quite understandable. But arguments in favor of cut-to-order theoretical models and corresponding special-purpose inquiries have recently gained favor also among analytically inclined economists—and I have in mind not the sophisticated lovers of elegant theoretical paradoxes and exotic conceptual formulations but, on the contrary, some of the pragmatically inclined searchers for useful knowledge. Any particular policy decision—so runs their argument—concerns itself primarily, not with the operation of the economic system as a whole, but rather with a specific relationship between two particular variables, one of which constitutes the instrument while the other represents the intended objective of the particular policy decision. To describe and analyze this relationship in the most economical way, we are advised to construct a special model and apply it to a specially collected, or at least a purposefully selected, set of empirical data.

Even if one agreed with the assertion that analytical implementation of specific policy decisions should constitute the primary objective of economic inquiry—which I certainly do not—the foregoing pragmatic argument in favor of a special-purpose approach is open to serious objection. The purported economy of the proposed partial approach turns out to be illusory as soon as one visualizes the immense inventory of special-purpose models with their appropriate complements of specially selected data that one would have to accumulate in order to keep up with the inexhaustible variety of ends and means contemplated in contemporary policy discussions. Moreover, the carefully formulated and empirically implemented general-purpose analytical scheme will be—despite its apparent complexity—not only a more economical but also a more useful aid to practical decision making.

One of the most fatal weaknesses of the contemporary policy-making process is its essential heterogeneity. It results in simultaneous promulgation of contradictory, or at least partially incompatible, policy actions. The wage policy often neutralizes the desired effects of the price policy, the tariff policy militates against the avowed objectives of the foreign aid policy, and the tax policy finds itself at loggerheads with the full employment policy. The clash usually occurs, not between the principal, direct effects of two specific policy measures, but rather between the principal objective of one and the unintended secondary repercussions of the other. The primary effects of any given policy decision can usually be discussed and anticipated even without the use of any complicated scientific aids. To show up the internal contradictions of two or more apparently independent policy decisions, however, one must be able to trace through their secondary and tertiary repercussions. This is a job for a professional economist. Truncated special-purpose models, with their intended neglect of indirect side effects, are eminently unsuitable for such a task. If the difficult assignment can be fulfilled at all, it can be done only within the framework of a general all-purpose analysis, designed along the lines of an unabridged theory of general interdependence and based on as complete and as detailed a set of empirical data as can possibly be obtained.

This does not mean, of course, that simplified special-purpose formulations should not be used in appropriate circumstances. The more they proliferate, however, the more important it becomes to concentrate an ever-increasing part of our exploratory efforts on the development of an integrated, and still not overaggregative, analysis of the economic system as a whole.

E. Advancement toward a Closed Model

In recent years, the development of empirical input-output analysis has been carried much further in some directions than in others. The use of so-called open models has enabled us to concentrate our principal attention on the productive parts of the economy—on manufacturing, agriculture, transportation, and distribution. The study of households, government, and other sectors temporarily fenced off within the bill of goods enclosure must now be brought into line with the rest. Although not yet sufficiently advanced to be discussed at this Conference, the analysis of the structure of the labor force, which constitutes the output side of the household sector, and of the consumption pattern, which represents the input side, has recently been given considerable attention. As the results of these new studies become available for incorporation in the national flow matrix, the entire system will move step-by-step toward greater closure.

A systematic investigation of the input-output structure of governmental operations has yet hardly begun. In the traditional national income analysis, the necessarily summary treatment of the public sector is explained by the impossibility of applying the usual dollar yardstick to the description of transactions that, in fact as well as in principle, are exempt from the control of the price mechanism. The input-output approach, with its fundamental orientation toward the study of technical, institutional, and other basic structural relationships that lie below the visible surface of market transactions and behind the administratively determined operations of public bodies, does not know this limitation. Without change in pace or method, it can proceed also with the detailed factual description of the structural characteristics of those parts of the economic process that happen to lie within the limits of the public domain. Once such information is collected, its analytical incorporation into the over-all matrix of interindustrial relationships presents no special problem. The internal operations of a substantial part of our military establishment happen to be the first large nonprivate sector successfully incorporated into the over-all structure description of the American economy. Analogous approaches can extend input-output analysis to other public sectors of the national economy, such as the educational establishment, health service, and highway construction.

Although within the theoretical framework of the input-output scheme the price system constitutes the logical counterpart of the physical flow system, the study of the former has occupied only a subordinate place in recent research. Such a lag is due first of all to the fact that the set of equilibrium, or so-called shadow, prices corresponding to a given matrix of static flow coefficients represents only the first, possibly a very remote, approximation to actually observed prices. To arrive at the second approximation, one would first have to shift the basis of the entire analysis from the static to a dynamic scheme of interindustrial relationships. This shift involves, in particular, the introduction of stock-flow relationships and of structural lags. Furthermore, a definitive input-output analysis of the price system will require also the incorporation in such a dynamic formulation of such purely monetary transactions as are represented by credit operation, taxes, subsidies, and other unilateral transfers.

Here again the lack of requisite factual information constitutes the principal obstacle to actual experimentation with the available theoretical blueprint. The forthcoming release of the moneyflows study by the Board of Governors of the Federal Reserve System should contribute much to the development of realistic price analysis within the framework of the input-output approach.

F. Use of Constant Input Coefficients

In the light of an ideal general-equilibrium approach, the theory of interindustry relations, as it has been applied up to now in empirical analysis, is indeed very imperfect and quite unsophisticated. The systematic use of constant input coefficients has often been referred to as its principal limitation. The subject deserves a few general comments.

First, it has to be clearly understood that practical necessity, rather than theoretical convenience, is responsible for the dominant position assigned to this limiting assumption in all practical applications of the input-output method as they have been developed up to now. We simply do not have enough detailed factual knowledge of the structural relationships within our own, or any other, economy to warrant any systematic emphasis on changing curvatures—or, more generally, on variable proportions—for purposes of their quantitative description. Even the numerous examples that every economist could cite to the contrary do not change the over-all picture. Many instances of observed nonproportional relationships can, furthermore, be traced back to the indiscriminate summation of dissimilar linear components; judicious disaggregation will often make such structures susceptible to linear description.

Practically, it is an issue of linear approximation. With the improvement in the quality of our basic data, nonlinear relationships, whenever their existence and quantitative significance have been satisfactorily established, will easily be taken into account.

First, the strict proportionality assumption involved in the use of constant average input coefficients can be relaxed through the introduction of a "free" constant term. Instead of a strictly fixed input-output ratio such as is shown in Figure 1, this formulation allows for constant incremental but variable average proportions as shown in Figure 2. This, incidentally, is the graphic mathematical expression of the method by which the "bill of goods" is introduced into the so-called "open-static" input-output computations.

Variable relationships between the level of output and the amount of input of any specific factor can be given an even more flexible and precise description by the use of a broken-line function with discontinuously changing slope, as in Figure 3. The advantage of this particular type of approximation lies in the fact that the numerical solution of systems of simultaneous relationships described in such terms lends itself to computational manipulations very similar to

Figure 1

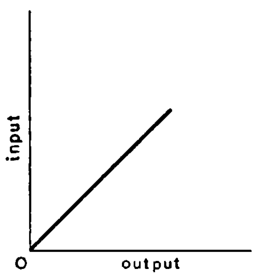


Figure 2

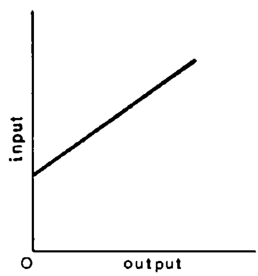
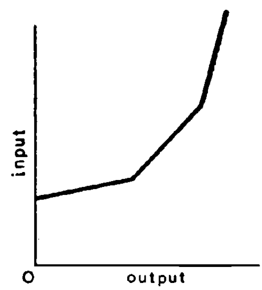


Figure 3



those that were applied so successfully in the analysis of strictly linear input-output problems.

G. Dynamic Input-Output Analysis

While the day-to-day and month-to-month operation of a modern economy is determined primarily by the input-output flows of goods and services among its mutually interrelated sectors, the exploration and explanation of its longer-run developments must be approached through the study of stock-flow relationships, of structural time lags, and of technological change. The theoretical background of dynamic input-output analysis has been described elsewhere;[2] I shall call your attention here to some of the implications one should draw from it in mapping the course of empirical investigation in this particularly difficult area.

The capital matrix, or the somewhat more general stock matrix, of a national economy should, in the study of economic development, be assigned the central position occupied by the flow matrix in static analysis. The analogy is not entirely symmetrical, in that the stock matrix will always have to be used in conjunction with the corresponding flow matrix. An exact conceptual and statistical alignment of the two represents, as a matter of fact, the prime requirement of the purposeful description of economic change.

The principle of disaggregation, which demands detailed classificatory distinction among many different kinds of flows, must accord-

[2] See, in particular, Wassily Leontief *et al.*, *Studies in the Structure of the American Economy*, Oxford, 1953.

ingly and to the same extent be applied to the analysis of the capital structure of the given economy. The stocks of plant and equipment and the inventories of all kinds should be measured and distinguished from each other according to the industry of their origin and the sector in which they are being used.

The durability of many capital goods necessarily ties the study of the stock structure of a given economy to the analysis of its technological—perhaps one should use the more general term structural—change. In the same way that the layers of mineral formations on the steep sides of a western canyon reveal the successive stages of its geological history, the existing plant, equipment, and so-called permanent improvements accumulated in an industry reflect the successive stages of its technological history. The assessment of the productive capacity of an industry (or, for example, of the housing capacity of a given complex of residential property) is the necessary empirical complement of any realistic theory of economic change. Such assessment has to be made in terms of as many separate stock-flow coefficients as there are significantly different technological layers incorporated in the structure of that industry's present capital stock.

New investment undertaken in any particular year presumably incorporates the "current best technological practice." In rapidly expanding sectors of the economy, such investment would usually constitute addition to previous capacity. In older industries, replacement, i.e. substitution of the current best practice for the worst—and probably the oldest—of the past practices still incorporated in the existing capital stock of the industry, will be of considerable quantitative importance.

This is the phase of empirical analysis in which the problem of substitution, i.e. the question of an explicit choice between two or more alternative sets of stock-flow coefficients, becomes really important. In explanation of past, and in anticipation of possible future, technological change, the mathematical procedures developed for computational solution of substitution problems within linear systems with multiple alternatives should certainly find important practical application.

The concept of the "pay-off period," i.e. the length of time it takes to pay off the initial investment in new plant and equipment out of savings achieved through use of the new method of production, I think, should play an important role in the explanation of technological change within the framework of input-output analysis. In particular, it offers an operational explanation of the changing "mix" of different technologies that can always be observed in most sectors of our dynamic economy.

H. Problems of the Product Mix

The question of the "product mix" has also been recognized as one of the central problems in the analysis of interindustrial flows. Not only through its empirical setting but also in terms of its theoretical (or should I say mathematical) formulation and solution, this question is very closely related to the explanation of technological change.

Gradual change in the magnitudes of average input coefficients of some particular industry as reflected in the input-output tables showing the state of a national economy in successive years can, after more detailed examination, be described as a changing mix of two or more methods of production or of several distinct lines of finished products. The word "mix" is somewhat misleading as used in this context, since the individual methods of production (or of consumption, if we face an aggregation of, say, several types of households) are frequently being operated, and consequently can also be described, quite independently of each other.

In some cases the mixing of several heterogeneous products represents simply an inappropriate statistical classification. To solve the product-mix problem in such a case means to obtain better direct information or, if it is not available, to disentangle the artificial combinations and reduce them to their elemental components through the solution of a simple set of linear equations.

In many instances, however, we witness a real shift from one method of production to another. If the productive stocks happen to play only a small role in either of the competing technologies, the substitution will be explainable simply on the basis of current costs, i.e. through comparisons of the competing sets of technical flow coefficients—a comparison that may require knowledge of the entire input-output system, since it will involve the use of corresponding prices. Very often the changing capital structure will, however, be of decisive importance. In such cases, the explanation—and possibly the prediction—of the gradual reduction in the part of the output based on the old set of stock-flow coefficients, and the corresponding increase in production based on the new set, will have to be given in terms of the analysis involving the pay-off period.

The specific circumstances of each case must obviously determine to what extent the variation in the output of any particular sector of the economy will reflect the changes in its internal structural characteristics as against the indirect influences transmitted through the input-output structure from the other more or less remote parts of the economy. Whenever the internal factors have the upper hand but still are inaccessible to direct observation and explanation, all one can do is to describe the effects of their operation through the introduction of some kind of time trend and hope that it will not reverse itself too abruptly. In such cases the usual methods of time-series analysis will obviously pay off better, or at least as well as, intricate input-output computations based on insufficient factual information.

The transition from the old to the new approach must, of course, be gradual; after all, it constitutes a kind of technological change itself, a change in the method of scientific analysis. The new technique requires a considerably larger volume of much more standardized raw statistical material; it demands considerable investment in intricate theoretical machinery. It can be applied effectively only on a comparatively large scale, and demands strict coordination and delicate alignment of all the theoretical and empirical stages of the combined operations. In short, input-output analysis brings with it all the evils of modern enterprise. There is some reason to believe, I think, that it can also offer comparable advantages.

COMMENT

ALEXANDER HENDERSON, *Carnegie Institute of Technology*

There are many points worth making about the development of interindustry studies. Two are of outstanding importance and generality, and I confine my remarks to them.

First is the question of confirmation. Essentially, the system implies an affirmation that, for some important purposes, economic variables can be forecast without explicit reference to prices, that future relations between the output of an industry and its inputs are the important coefficients and can be predicted with reasonable accuracy, and, finally, that the way to predict them is to start from past ratios of input to output. These assumptions can be removed only at the cost of making input-output a particular way of stating a general, and relatively empty, theory of production.

It is not self-evident that these propositions are true. Relative price changes may be fundamental, and they must play some role in the operation of a price system in cases where the "requirements" for a given level of demand are not available. Techniques are, of course, available for dealing with these cases, but if they have to be applied at many points in the system, they destroy simplicity, which is the main attraction of the formal scheme. Input coefficients may not be stable, or they may be stable but unpredictable. There may be other, more stable, relations that are to be preferred for all or most purposes. Finally, prediction of the coefficients may be achieved otherwise than by looking at past data.

Here, the time required for obtaining the data is highly relevant. We have in 1953 a matrix relating to 1947; to estimate outputs in 1953, we can either use this matrix of relations as they were five years ago or we can use other information applying to years up to early 1953. It is perfectly possible that, if the two types of information were of the same vintage, we would prefer the matrix, but that, since the matrix takes a long time to produce, it may be better to use the less pertinent but more rapidly available types of data. The delay in preparing the matrix may well be cut below five years but hardly as low as that for, say, output or price data.

The techniques of input-output are undoubtedly capable of much refinement, but we can reasonably ask now that the faith that has supported the past work be strengthened by a demonstration of its achievements. It is not necessary to ask that the present system of calculation be better than alternatives—for it may be capable of greater improvement than they are. It is necessary to ask that input-output show its paces. At present it is very difficult to find any reasonable test whereby we can appraise the working of the system. Indeed, the best seems to be Wassily Leontief's "prediction" of 1929, using the 1939 matrix.[1] No one would be satisfied with just this example. Yet it is difficult to collect data in the form required for empirical comparisons without the use of government statistical services.

Two reasons have been advanced to belittle this lack of a testing procedure. The first is that there is no way of providing an adequate comparison. The second is that there is no need for confirmation, because certain properties of the matrix guarantee that if it is constructed with reasonable care it cannot lead us seriously astray. Both of these contentions are misleading in the extreme form in which I have stated them.

If we used the system to forecast 1953 levels of activity, it might be a long time before we could compare the forecast with reality,

[1] See Wassily Leontief, *The Structure of American Economy, 1919-1939*, 2nd ed., Oxford, 1951, pp. 152-162.

and we could never be certain that, in the case of an apparent disparity, the error lay with the forecast and not with our estimate of reality. Formally, this problem arises in any scientific experiment, but rarely in as acute a form as in the present context. The forecast would be in 1947 producers' prices and in terms of an industrial classification other than that normally used for current data. Price indexes that are appropriate for these prices and these aggregates are not available, and there is the usual difficulty arising from index number calculations. These and other difficulties are important, but they do not justify failure to make any comparisons. If the input-output technique forecasts magnitudes for 1953 that cannot be related to anything that can, in fact, be observed in 1953, we must doubt whether it is doing anything useful. This doubt would remain even though, around 1956, we could finally come up with figures to show that the forecasts had proved to be very good. The predictions are valued as a guide to action, and action must be framed with reference to currently available facts. It is not sufficient to say that comparison cannot be made; instead, it is necessary to show the relation of the units in which input-output forecasts are made to those of currently available data, or to develop new series for this purpose.

It would be very misleading to suggest that nothing is being done in this direction. The BLS is working in the right direction, though it is gravely hampered by limitations of funds and by security provisions. The emphasis of my remarks is only that the vast statistical operation that has been undertaken is largely wasted if it is not linked to techniques whereby predictions and results can be compared. Moreover, the process of assessment is the most valuable tool for learning how to improve the present system.

I turn now to the other reason for neglecting the problems of verification. It is often argued that the errors in the matrix are compensating and, therefore, less important than might be expected. The row sums and the column sums of the interindustry transactions matrix are known with a high degree of confidence, so that if there is one error in the matrix there are very likely to be countervailing errors elsewhere in the matrix. This statement is subject to question, since the final demand category includes inputs into construction and investment, and the check figure for the column sum is the purchase of materials at prices that are not those used in the matrix. However, it is not the present purpose to question the validity of the rim totals. If these are taken as right, then the compensation can be expressed by saying that, if the value in one cell of the transactions matrix is too large, then the values in at least one other cell in the same column and one other cell in the same row must be too small. Thus, if too much of the copper output is allotted to automobiles, correspondingly less must have been allotted to other industries, and some other purchases of the automobile industry will have been understated. One error implies the existence of at least three others.

The claim is made that this is an advantage, and that because of it the answers obtainable from the input-output system are better than we should expect. It is important to realize the extent to which the claim is true. The offsetting of errors works only insofar as the changes in final demand are proportionate for all the industries whose inputs are affected by a single error.

Suppose we have the basic tables for 1947 and wish to calculate from them the activity levels $[X_1]$ for some different bill of goods $[Y_1]$. If $[Y_1]$ is simply the original bill of goods $[Y_0]$ multiplied by a scalar β, then we do not need the matrix to obtain the answer; it is $\beta[X_0] = [X_1]$. If this is not the case, we can decompose the change of bill of goods by

$$(1) \qquad [Y_1] = \beta[Y_0] + [y_1],$$

where the sum of the values in the vector $[y_1] = 0$. Then the new bill of goods can be obtained:

$$(2) \qquad [X_1] = \beta[X_0] + (I - A)^{-1}[y_1].$$

Now, the offsetting effect of balancing errors does not apply to the second element on the right-hand side of (2), since $[y_1]$ has a zero column sum and the overestimate of the inverse of one element will be accentuated by the underestimate of another element, if the two elements are to be multiplied by y's of different sign.[2]

[2] An example of the importance of the proportionate element is to be found in Jerome Cornfield, W. Duane Evans, and Marvin Hoffenberg, "Full Employment Patterns, 1950," *Monthly Labor Review*, February and March 1947. They postulate a bill of goods $Y_{1950} = 1.80Y_{1939} + y$, where $Y_{1939} = \$83.2$ billion and the sum of y (disregarding signs) is \$19.5 billion at a 32-industry aggregation. Thus, in the calculation of the activity levels, the matrix is needed for less than a quarter of the total bill of goods.

It is possible to calculate the output levels of each industry using the formula $X_{1950} = 1.80X_{1939} + Iy$. This involves using the identity matrix as an approximation to the inverse of $(I - A)$. This produces results very close to those of the authors, and only slightly worse if compared with Barnett's figures of actual output in 1950 (see Harold J. Barnett, "Specific Industry Output Projections," *Long-Range Economic Projection*, Studies in Income and Wealth, Volume

In fact, advantage is taken of this property by which errors may offset each other, because it is easier not to decompose the bill-of-goods vector into the scalar and the residual parts. But this is not essential; insofar as there is cancellation of errors we can dispense with the matrix. Further, if we can expect that the output of those industries whose inputs are affected by the error will vary in the same proportion ($X_{i(t)} = KX_{j(t)}$ for all times t), then the error will make no difference. (The reason is that the industries could have been aggregated to form a single industry without altering results.) Hence, good use can be made of the property of the system that errors may compensate each other. This cannot, however, be used to justify the system itself, for to the extent that there is compensation, the system itself is unnecessary.

The point may seem trivial, but it is not. The offsetting of errors enables fairly reliable forecasts to be made with the help of indifferent data, but we may easily be misled by using the technique in circumstances where the change in final requirements has been nearly scalar. The evidence may suggest that both the technique and the matrix are reliable when, in fact, the matrix itself may have

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Sixteen, Princeton University Press for National Bureau of Economic Research, 1954. Using A. W. Marshall's criterion (Comment on H. J. Barnett's "Specific Industry Output Projections," in the same volume), the distance of the calculated from the observed result is:

	Consumption	*Model*	*Investment*	*Model*
1. Barnett's multiple regression	$5,325			
2. $X_{1950} = (I - A) - IY_{1950}$	7,081		$9,919	
3. $X_{1950} = 1.80X_{1939} + Iy$	7,354	11	10,299	14
4. $X_{1950} = 1.80X_{1939} + 1.59Iy$	7,672	13		
5. $X_{1950} = 1.80X_{1939} + [a]y$	7,961	17	10,465	9

The first figure for each model is the "distance" of the predicted from the observed total output levels for the 28 industries. The second figure is the number of industries for which the calculated figures are closer to the observed values than those obtained by using input-output.

The first line gives Barnett's forecasts using a linear regression of industry output on GNP and time with data for the years 1922 to 1941 and 1946. The second line uses the 1939 input-output matrix. The third line uses the identity matrix as an approximation to $(I - A) - 1$ and applies this to y, the nonproportional element in the change in anticipated final demand. The fourth line uses 1.59 times the identity matrix for the same purpose, where 1.59 is the ratio of the aggregate output of all industries to the final demand for all industries. Finally, the fifth line uses, for each industry separately, the ratio of the output of that industry in 1939 to final demand in 1939 for the same industry.

It is not known whether the closeness of the results, using the matrix or crude substitutes for it, is accidental. This is indeed the burden of the complaints above. However, so far as it goes, these calculations suggest that the gain from using the matrix is slight—and the additional cost is far from slight.

had hardly any effect on the result and has not, therefore, been tested. Difficulty may easily arise when techniques verified under peacetime conditions are used to estimate wartime changes of demand. Moreover, we are concerned as to whether the technique of input-output is of practical value; there would be little point to developing it if its success lay in estimating changes of a type that do not require that technique.

My second point concerns the purpose of the system. As I see it, there are three main purposes that may be sought:

1. Statistical reconciliation of separate data. This is the avowed purpose of the Dominion Bureau of Statistics in Ottawa.

2. Facilitating the formulation and implementation of economic planning. In a modified form this seems to be the objective of the United States government.

3. Forecasting developments in a free economy, which seems to be the aim of Leontief and the Harvard Research Project.

All these are legitimate purposes of economic investigation. But it is doubtful whether an all-purpose system is possible. The more the system is developed, the less the concepts appropriate to one purpose are suitable for another. For planning, we are concerned not with what producers will *choose* to do but with what they *must* do to maintain a given level of future final output; for prediction, we wish to know what they will do now in response to recent levels of demand for final output. We may well believe that what producers must do and what they will choose to do are closely related, if we consider averages over long enough periods of time, but we must also recognize that deviations, e.g. abnormal changes in stocks, may be very important in determining the course of events—for example, by affecting the timing of a cyclical fluctuation and, hence, the level of future final outputs. Thus, for these two purposes, substantially different treatments of the system are required.

Again, consider the entries in the basic transactions matrix. As a technique of reconciling statistics, these entries are usually the result of a complete count of the relevant outputs. They are not stochastic estimates in the usual sampling sense, and their errors are to be explained as due to conceptual differences or to mistakes of counting. For planning purposes, the same entries are important only as a basis for estimating the best input coefficients that the planning authority will be able to achieve at some future point of time. They are now stochastic variables whose distribution may eventually be calculable. Furthermore, since they are to represent not any "possible" input coefficients, but the "best" from the point of view of the planning authority, they must reproduce some formal or informal programing. Finally, for prediction purposes, the matrix represents an estimate of future input coefficients; again, they are stochastic variables, but now they represent what will probably happen, not what can be made to happen. The contrast is still more acute when we consider a matrix of capital coefficients.

The same distinction between planning and prediction can be made for the problem of confirmation. Essentially, this means comparing the results obtainable from input-output analysis with those given by alternative methods in terms of their success in estimating those things we are interested in. If we are interested in good statistics of the past, then the use of the transactions matrix will lead to different figures from those we shall get from another technique. For planning, we need to know the relation between the final demand and the output for each industry. Here, a particular set of alternatives is available, and comparison must be made between them. For forecasting purposes there is more difficulty. However good the relation may be between the final demand and the output of each industry, this is of no avail unless the final demand can be estimated. Hence, the system may be excellent for planning (where the final demand categories are decreed rather than foreseen) and yet useless as a forecasting device. Other relationships that bypass the need for estimating future final demand may then prove more useful for prediction, though inapplicable to planning problems. Hence, the meaning of the system and its criteria of success are different according to the uses we wish to make of it.

My impression is that figures used in the input-output studies are those for which some firm basis can be found, regardless of the purpose for which they are appropriate. Such figures are defensible however irrelevant they may be to the purpose at hand. It is worth emphasizing that a better procedure is to define the purpose to be achieved by any assemblage of figures and then to decide what concepts are relevant to that purpose. Thereafter, compromise will generally be needed in finding approximations to the required figures —but it is often better to use a rough guess of the figure you want than a good estimate of something else.

I suspect that the input-output technique will prove most useful as an aid to economic planning if another war should render such planning necessary. Forecasting the future of an unplanned economy requires too many predictions that are likely to remain very hazardous: stock fluctuation, demand changes, types of invention. So far, perhaps because no occasion for its use exists, little has been done to assess the ways in which this technique could be used for planning. The "feasibility testing" now envisaged is only a small part of what might reasonably be expected. There remain, however, major difficulties in the way of using this technique, and it remains to be seen whether they can be overcome.

The concern here is that those working with the system should decide what they want from it and make sure that it will perform those functions. Finally, they must reconcile themselves to the knowledge that the same system will not perform all possible functions.

RUTH P. MACK, *National Bureau of Economic Research*

I would like to bring up the unhappy question of inventories. Obviously, the importance of the explicit handling of the problem of inventories differs according to the use to which the analyses are to be put. I assume that one of the important uses would be the study of abrupt and large changes in output, such as would occur at the time of war or other national emergency. Under such circumstances, the way inventories were calculated would, I believe, make sizable differences in the results—differences, I would guess, of whole number multiples.

One might choose to use, for example, fixed inventory-output ratios, ratios that changed in accordance with historic experience in the short run, ratios that changed in accordance with historic experience in the long run, or ratios that changed in accordance with current demand as expressed in industry at the time of the emergency. Which of these various behavior patterns was followed would, I expect, make significant differences in the final output figures. Indeed, it seems likely that the differences would be such as to make questions of whether the analyses had been performed according to activity analyses, straight-line programing, input-output coefficients, or fixed rather than stable capital coefficients seem like details.

DAVID ROSENBLATT, *American University, Washington, D.C.*

Input-output analysis has made significant contributions and stimulated research in three fields of inquiry: linear model representations of interdependent economic activities; collation and synthesis of statistical data into systematic tables of accounts; and, lastly, the application of large-scale machine computational methods in the development of conditional economic projections.

In Wassily Leontief's paper, empirical input-output analysis is implicitly defined to embrace interrelated contributions to these fields. This definition would imply that this form of analysis is regarded primarily as a way of conducting organized economic research. The principle—indeed, metaprinciple—underlying the organization of this research is the generalized linear simultaneous treatment of macroeconomic quantities in a national economy. This treatment, as stated, is characteristically descriptive or phenomenological, and is motivated by the interest in undertaking conditional economic projections that satisfy a criterion of "internal consistency."

To fulfill Leontief's principle as formulated, there must exist cross-sections of economic data, systematically organized in accord with certain conventions, that will afford operational empirical constants. These empirical constants are to be introduced into generalized linear representations of an economy in such a manner as to make "efficient" conditional projections possible. The results of these projections are to be evaluated, under test conditions, relative to standard or specially devised statistical reporting systems in accord with certain conventions.

The research principle itself is stated in a relatively open and invulnerable form, so that economic and industrial studies developed under its guidance come to be individually evaluated on pragmatic and "operating-performance" grounds.

In an effort to maintain the operating performance of a particular input-output analytic instrument, the research investigator has at least the following approaches jointly available to him:

1. Development of standard and reproducible means of estimating operational or structural coefficients for different time periods. In principle, estimation based upon probability sampling methods could afford standard estimates and measures of coefficient change, as well as appropriately defined evaluations of statistical reliability. In this last connection, such estimates could be further appraised in relation to the "quality and coverage check" measurements which have come to be associated with the conduct of censuses and large-scale sample surveys. From the standpoint of projection efficiency, determinations remain to be made of the merits of formal statistical methodologies for the treatment of time series of input-output tables, assuming that the requisite data were available.

2. Evaluation by formal as well as experimental means (i.e. simula-

tion studies) of the "accuracy" or "sensitivity" requirements for coefficients in certain types of projection computations.

3. In key industries, or defined "macroactivities," study of the relations existing between patterns of technological choice and coefficient variability, undertaken perhaps with the aid of sponsored activity-analysis investigations at the intra-industry level. Such investigations would have to be carefully designed and located in strategic industrial areas, in view of the complex reporting-system implications of representing technological and administrative choice in activity-analysis models of more than trivial dimension.

These approaches enhance but do not assure the performance prospects of input-output analyses in the sphere of conditional economic projections. Any given input-output analytic instrument brings in its train a structure of specialized statistical measures for the coding of problems and decoding of answers, e.g. systems of price deflators and production-employment transforms. Some of these are structurally circular to the analysis, so that the roles of coefficient assumptions and measurement coding language become indistinguishable in assessing components of performance in test projections. Beyond this, the concept of the set of existing statistical reporting systems as a self-consistent entity must always be recognized as a construction; indeed, a construction not unlike that implied in the design and synthesis of input-output tables.

The promise of Leontief's research principle cannot, on the basis of evidence so far available, be said to be fulfilled, though it has doubtless enriched the vision and scope of the three fields of inquiry noted earlier. The history of ideas, it has been asserted, demonstrates that theoretical constructs eventually come to express an attribute of reality. In the spirit of this notion, the taxonomy of Quesnay and the higher geometry of Leontief have attained fruition in the input-output tables and the projection methodologies of the present day. In the effort to represent "economic reality," Leontief's research program is as yet incompletely formulated; it remains to sketch in the role of theory construction in respect to changes in the defined structural coefficients, and correspondingly to delimit the logistics of data collection on technological and administrative choice.

RUTLEDGE VINING, *University of Virginia*

The following remarks are grouped under four headings: the first part includes a statement of the problem of input-output analysis;

in the second, I attempt to set down what I understand to be the claims on behalf of the "input-output approach" made by Wassily Leontief and his associates; in questioning the bases for these claims, I discuss in the third part my understanding of the requirements to be met by a "theoretical model" for analyzing empirical phenomena; the fourth part includes miscellaneous strictures upon Leontief's comments on "accuracy," "reliability," and the applicability of statistical and economic theory in his research.

A. The Problem of Input-Output Analysis

Consider the firm and the family to be specifiable entities, and regard these as the units of the economic system. Assume that n classes of these units are defined and that an n-by-n table has been formed, the n classes being listed at the margins vertically and horizontally in the same order, there thus being n^2 cells in the table. Consider the firm and the family as producing something recognizable as "output" and consuming something recognizable as "input," and assume that this "input-output" can be quantified in terms of a common unit of measure. Finally, assume that the output of any given firm or family over a specified period may be identified, or accounted for, as parts of the respective inputs of particular firms and families over this period, so that all outputs may be classified by consuming firm or family. The outputs of the firms or families belonging to any one of the n classes of units may now be recorded in the cells of the table in such a way as to show how much of the output of any given class for the period in question is consumed as inputs by the units of any given class; thus, numerical entries may be made in each of the n^2 cells of the table.

The n classes of families and firms having been defined, and a rule having been adopted in accordance with which input-output is to be quantified, the operation of a *system* of firms and families is looked upon as generating numerical data constituting empirical observations as entries for the n^2 cells of the table. Now the problem of input-output analysis, as I understand it, is this: given entries for m cells, to compute entries for the rest of the cells as estimates of the entries that will be generated by the working of the economic system; that is, to discover a "best" procedure or operating rule such that with m numbers arbitrarily determined, $n^2 - m$ numbers may be inferred as predictions of the outcome of an empirical process.

B. Claims Made for the Input-Output Approach

The extent and nature of the claims that are made in regard to this problem by the expositors of the input-output approach are not entirely clear. Many statements made by Leontief and his associates read as though there is already at hand an "efficient" and "reliable" instrument for making these predictions. A reader would hardly be left in doubt in this regard by the discussion of Evans and Hoffenberg. They tell us, using the past and present tenses:

"It has been used to trace the dependence of specific industry production levels on exports from the United States. . . . It is an appropriate tool for the consideration of both long and short range resource limitation and development problems. . . . The approach constitutes an important and powerful new tool for business marketing research. . . . Industrial mobilization analysis is virtually a text book example of the power of and need for interindustry relations methods. . . . The feasibility of a mobilization program can be judged only after translation of the projected delivery schedules into the levels of all supporting activities which they imply. . . . It is this particular job—the determination of the production levels for all industries which are consistent with a given schedule of end-product deliveries—which is done more precisely and in greater detail by the interindustry relations approach than by any previously available method. . . . The data available within the Federal government today, revised currently in various ways, are believed adequate for their intended uses in general industrial mobilization analysis."[1]

This seems to say that there now exists a procedure ("powerful new tool") for performing particular tasks that has been tested and analyzed in such a way that the performance characteristics of the procedure are known, for he speaks of "more precisely" and "in greater detail" in comparing the performance of the new tool with that of "any previously available method." The procedure to which he refers cannot consist merely of instructions for constructing an *n*-by-*n* table. The construction of the table is a detail in the formulation of the problem. The tool or procedure consists of a set of operating rules in accordance with which numerical entries are made in the table as predictions of the results of an empirical process. These rules constituting the "tool" presuppose the definitions of the *n*

[1] W. Duane Evans and Marvin Hoffenberg, *The Review of Economics and Statistics*, May 1952. The above quotations were taken from the mimeographed form of the report, released by the Bureau of Labor Statistics.

classes and the definition of the quantifying rule in accordance with which input-output is expressed in terms of a common unit. These definitions of the *n* classes and of the quantifier are details in the specification of the *n*-by-*n* table and thus in the statement of the problem.

The set of rules constituting the tool or procedure takes the form of instructions indicating the operations to be performed upon specified and available data. W. Duane Evans illustrates the use of an instrument that we may be led to suppose exists by referring to a decision to be made by authorities responsible for the choice of a "mobilization plan": "Checks against . . . manpower, materials, and facilities . . . may show impractical elements in the initial mobilization plan which will require modifications of one sort or another. The type and extent of these modifications can be pinpointed by further use of interindustry relations data."[2]

There is a "feasibility" decision to be made, and the idea implied here is that of a rule of inductive behavior.[3] A mobilization plan is presented to the Division of Interindustry Economics. The rule is applied, and the plan is thereby either rejected or accepted, and if rejected, the required modifications are "pinpointed." There are the two types of error to which the rule may possibly lead, of course: the rule may declare a plan not feasible that, if tried, would be found to be feasible; or it may declare a plan feasible that, if tried, would be found to be not feasible. It is in terms of an understanding and evaluation of how alternative procedures respectively perform, in regard to costs of operation and in regard to the sizes of the two kinds of error attributable to them as operating features, that a meaningful statement may be made to the effect that one procedure is more efficient than "any previously available method." Evans makes this comparative statement, but he does not give an analysis of the operating features of his procedure, and so far as I know there is none to be given.

While the statements of Evans quoted above seem to refer to present capabilities, Leontief's remarks leave doubts in the reader's mind regarding present capabilities, and refer mostly to future potentialities. From the context of his discussion one gathers that

[2] *Ibid.*

[3] That is, a decision function that yields a decision upon a course of action as the dependent variable when specified numerical data are substituted in place of the independent variables. This is the idea that I see in Evans' discussion, ". . . detailed and specific questions may be put to the analytical machinery and equally detailed results . . . may be obtained" (*ibid.*).

there is lacking now only the "requisite quantitative data," that when these are obtained the practical implementation of the input-output approach will be possible. The "theoretical formulation" and the "large-scale computational manipulation" have been or can be dealt with, and the "success or failure of the whole enterprise will depend more immediately upon our ability to master the formidable fact-finding task which it involves." Such statements, in the future tense, suggest that Leontief would be inclined to qualify the claims of Evans regarding practical applications, phrased as they are in the past and present tenses. In discussing the instrument as an aid in the policy-making process, he does not claim that his "analytical scheme" as it stands is equal to the task; but he declares that if this "difficult assignment [tracing through the secondary and tertiary repercussions of policy decisions] can be fulfilled at all . . . ," it will have to be fulfilled in the way that he has in mind fulfilling it. He regards "the theory of interindustry relationships, as it has up to now been applied in empirical analysis, [as] indeed very imperfect and quite unsophisticated." In speaking of what will ultimately be "a definitive input-output analysis of the price system," he states that this will require the incorporation of various sorts of "dynamic formulations," "the introduction of stock-flow relationships and of structural lags." "Here again," he says, "the lack of requisite factual information constitutes the principal obstacle to actual experimentation with the available theoretical blue print."

Some of Leontief's remarks, like some of the remarks of his associates, raise questions in the reader's mind with respect to the nature of the underlying theoretical model that is frequently referred to. Does it now exist, and are these articles describing empirical tests of its usefulness? Or is it only being sought for, and are these articles describing the search and expressing the bright hopes of success? In discussing why he cannot "make efficient use of the powerful . . . tools of probabilistic approach," Leontief speaks of a first stage and a second stage of empirical research and states that we are now in the first stage. "The formulation . . . of . . . a conceptual framework . . . has to be accomplished in the first stage." "Formal statistical procedures" cannot now be applied in input-output research. He proposes to move "the heavy guns of probabilistic formulation . . . into position," to make use of mathematical statistics, after "the principal parts of the analytical structure have been erected [as] rough-hewn components," after "the general layout of the terrain" has been explored, after "the analytical as well as the descriptive stratification of the factual material has progressed" to the level at which "admissible hypotheses" may be formulated. He is stating what he proposes to do, not now while he searches about in the preliminary stage, but after the second stage is reached. Evans seems to be beyond anything like a second stage—rather at the manufacturing stage with a "central agency" doing custom work in constructing models of input-output analyzers designed in requisite detail to meet the demands of the "general users"—but not Leontief. If these reservations are to be taken seriously, we have not yet formulated a procedure for doing what Evans seems to say he can do, and thus cannot begin to study systematically the performance features of anything that can be called a technique or a powerful tool or a piece of analytical machinery.

But the reservations are contradicted by a number of other remarks, and it would seem that Leontief finally maintains that he does have a theoretical model, and a very powerful one at that. The obstacle between him and success in solving the problem of input-output analysis is the lack of "requisite" or of "pertinent" or of "enough" quantitative information. The accomplishment of input-output research to date, if one takes literally the statements made on behalf of this research, lies in the development of this very model, which indicates what facts are to be gathered and what operations are to be performed upon them in order to obtain predictions of the empirical process.

The proponents of the input-output approach are enthusiastic in this regard. One statement has it that "both at present and in prospect, the interindustry relations study technique is the most efficient and comprehensive technique available for studying the total effects of any given program, civilian or military, on the economy and on the several industries in the economy."[4] Leontief expresses the judgment that the input-output approach is destined to be "not only a more economical but also a more useful aid to practical decision making" than any proposed "partial approach"; and he says reservedly that, although it has the disadvantages of modern industrial enterprise, "there is good reason to believe, I think, that it can also offer comparable advantages," which we may suppose to refer to efficiencies. Evans states that "refinements in theory and major practical uses have followed rapidly," and that "these developments seem destined to revitalize important areas of quantitative economic

[4] This is a statement quoted by Ezra Glaser in *The American Statistician*, April 1951, p. 9, from some government report "of a panel of experts."

analysis"; and he uses the phrases "more precisely" and "in greater detail" in making a comparative statement regarding the performance of the "interindustry-relations approach."

Now it is true that these men explicitly point out that when they say "input-output approach" they do not refer to some "single method or technique"; the approach includes "a great variety of methods, each subject to endless modification." This, however, would be taken for granted—just as when we refer to the internal combustion engine we have in mind a class of engine, each member of which is built upon a common set of principles. But this theoretical model upon which the input-output approach is supposed to be based cannot consist merely of the rules for constructing an input-output table. The theoretical problem has to do with the development of a "best" procedure for predicting the results of an empirical process, whereas the table only indicates the form in which the predictions are to be presented. The problem of constructing the model thus presupposes the table, and the table presupposes the definitions of the classes of firms and families and of the method of quantifying input-output. Experimentation with different "aggregation patterns" has to do with defining the n classes of economic units and is thus devoted to formulating the problem rather than to formulating a solution of the problem. Experimentation with various ways of dealing with the index number problem has to do with defining the quantifier of input-output and is similarly devoted to the formulation of the problem. The mathematical problems involved in the "inversion of matrices" are raised when certain computations are made that are required in an application of "the theoretical model"; and work upon this computational detail should not be mistaken for "a development of the mathematical foundations of the theoretical model." The authors that I have quoted use such terms as "most efficient," "most comprehensive," "more economical," "more useful," "in greater detail," and "more precisely." These terms refer to the performance of an instrument in doing something, and they imply that that instrument has been tested empirically or analytically against other instruments.

The underlying theoretical development of a technique, "a powerful new tool," apparently is the accomplishment to which claim is laid. In my view this claim has not been substantiated. What I see presented as a "theoretical model" of observable phenomena does not conform with my understanding of this conception; and I am aware of no analysis at all upon which the appraisals of the "efficiency," "economy," "precision," etc., have been made.

C. Requirements for a Theoretical Model

In order to outline my understanding of a theoretical model in an empirical field of research, I can do no better than to quote from a work of Harald Cramér.

"When, in some group of observable phenomena, we find evidence of a confirmed regularity, we may try to form a mathematical theory of the subject. Such a theory may be regarded as a *mathematical model* of the body of empirical facts which constitute our data.

"We then choose as our starting point some of the most essential and most elementary features of the regularity observed in the data. These we express, in a simplified and idealized form, as mathematical propositions which are laid down as the basic axioms of our theory. From the axioms, various propositions are then obtained by purely logical deduction, without any further appeal to experience. The logically consistent system of propositions built up in this way on an axiomatic basis constitute our mathematical theory."[5]

I shall introduce a question here. Has there ever been presented in any of this input-output work any evidence of a confirmed regularity at all? I am not aware of any, and I know only of expressed intentions of seeking for some regularity by which entries in a formally specified table may be anticipated before the fact. Evans, in expressing "the basic philosophy of the approach," states that: "One must begin by examining the quantitative details relating to specific sectors of the economy, find the elements of stability in their interconnections, and devise methods for applying the results to economic problems. In operations research terminology, the objective is to determine and apply the operational constants of the economic system."[6] What is being presented as the "operational constants of the economic system" that have been "determined"? I am aware of only a searching for these "operational constants," which to date has met with no demonstrated success. The plan of the search seems to me to be somewhat direct, for the constants that the analysts seem to insist upon finding would virtually constitute the solution of their problem, with only routine multiplication left to do. What appears to be in their minds is no more specific than

[5] Harald Cramér, *Mathematical Methods of Statistics*, Princeton University Press, 1946, p. 145.

[6] *Op. cit.*

"the quantitative coefficients relating outputs and inputs." Leontief calls for more and better quantitative data—"pertinent," "requisite" data, "detailed firsthand information," and a "firmer . . . terminological foothold" through disaggregation. Having begged the questions of the degree of effectiveness of his predictive procedure and of how to recognize pertinent from nonpertinent or enough from not enough data, he apparently leaves the way open for the interpretation of unsatisfactory performance of his "analytical scheme" as prima facie evidence for holding that the data are either not sufficiently pertinent or not sufficient in volume. Thus, the outcome of his "experiment" is apparently to be interpreted as a test of the degree of pertinence or sufficiency of the data, or of the "aggregation pattern," or of the input-output quantifier, with the theoretical model being taken for granted.

In appraising Leontief's contribution, Evans refers to his having made something out of the old ideas of political economy and having seen the way to developing them into "a practical instrument for attacking some of the most complex and perplexing real problems of our modern industrial economic environment." In Leontief's words, this would mean something like tracing "through the secondary and tertiary repercussions" of "policy actions," and would involve numerical predictions of the entries in the table cells. What are these ideas that were developed and where is the development? There is a theory of economic organization that has evolved over the ages in the discussion of a certain class of problems of social choice that would persist as problems quite apart from the success or failure of Leontief in his venture. Leontief's problems lie outside this class, and pertain to positive numerical prediction of the outcome of an empirical process. No one need be told that firms and families are interdependent in their operations and that the inputs of some firms consist of outputs of other firms. And there is little contribution involved in writing this out as a formal and unspecified system of equations. An instrument has been or is being sought for making predictive statements, subject to empirical tests.

In Cramér's quotation above, reference was made to the construction of the mathematical model. He goes on to say: "Every proposition of such a system is *true,* in the mathematical sense of the word, as soon as it is correctly deduced from the axioms. On the other hand, it is important to emphasize that no proposition of any mathematical theory *proves* anything about the events that will, in fact, happen. . . . The pure theory belongs entirely to the conceptual sphere, and deals with abstract objects entirely defined by their properties, as expressed by the axioms. For these objects, the propositions of the theory are exactly and rigorously true. But no proposition about such conceptual objects will ever involve a logical proof of properties of the perceptual things of our experience. Mathematical arguments are fundamentally incapable of proving physical facts."[7]

That is to say, if the n classes of firms and families were defined as conceptual objects, and if rules were adopted by which something called input-output is quantified in terms of a common unit, and if confirmed regularities have been established that are interpretable as pertaining to something like these conceptual objects, and to these quantities referring thereto—which of course is not the case—then a model might be constructed, and propositions might then be deduced as properties of the model. But such deduced properties of the model are not by virtue of logical consistency alone to be attributed to what we think we perceive as a system.

Cramér continues:

"Certain propositions of a mathematical theory may, however, be *tested by experience*. Thus the Euclidean proposition concerning the sum of the angles in a triangle may be directly compared with actual measurements on concrete triangles. If, in systematic tests of this character, we find that the verifiable consequences of a theory really conform with sufficient accuracy to available empirical facts, we may feel more or less justified in thinking that there is some kind of resemblance between the mathematical theory and the structure of the perceptual world. We further expect that the agreement between theory and experience will continue to hold also for future events and for consequences of the theory not yet submitted to direct verification, and we allow our actions to be guided by this expectation. . . .

"In a case where we have found evidence of a more or less accurate and permanent agreement between theory and facts, the mathematical theory acquires a *practical value*, quite apart from its purely mathematical interest. The theory may then be used for various purposes. The majority of ordinary applications of a mathematical theory may be roughly classified under the three headings: Description, Analysis and Prediction."[8]

It should be noted that analysis, from Cramér's point of view, has to do with mathematical operations upon the model, by which

[7] *Op. cit.* [8] *Ibid.*

operating properties of the conceptual structure are determined and attributed to alterable parameters of this theoretical structure. Only in a very crude sense can it be said that a theoretical model underlies the "input-output technique." Data gathering is being guided by the form of the table, and various ways of making computations upon data of various sorts are being tried out. In a similar way, the National Bureau has been guided in its data gathering by its conceptual idea of the general form of an economic system, and it has experimented with various ways of revealing behavior properties through reducing the data by computation. Thus, the method of "interindustry relations research" seems no less empirical than the method of the National Bureau. But much more in the way of "theoretical foundations" and practical results has been claimed for the "input-output approach." And the claims appear to be excessive. When the "operational constants" have been discovered, and when a theoretical model has been developed, analyzed, and tested, that time would seem to be soon enough for the announcement of the "power," the "efficiency," the "precision," and the "economy" of the "new analytical tool."

D. Critical Appraisal

Leontief's proposition that "the less aggregative, the less index-number-like, the objects we are trying to measure, the firmer will be our terminological foothold" is subject to more than one interpretation. It may be understood as saying that, as certain operations are progressively performed upon the data ("disaggregation"), there will be observed a larger and larger degree of stability in the measurements that are made over a succession of periods. But no grounds are presented for thinking that the statement is true in this sense, and I would interpret the remark as referring, not to the behavior of empirical phenomena under various modes of treatment, but merely to the difficulties of writing out an unambiguous set of instructions to guide the decisions of routine employees of the data-gathering organization. The definitions of the classes of firms and of products refer to conceptual objects. Data gatherers and computers are confronted by perceptual objects. I take Leontief's remark to mean that the selection of the conceptual object corresponding to an object that is perceived in experience is made easier and more routine by a more detailed description and specification. When it is said that "the meaning of the labels attached to the individual entries in an . . . input-output table . . . is bound to gain in definiteness . . . as the breakdown of the economy into its separate sectors is progressively refined," this "meaning" would seem to refer to what the data gatherer reads as his classification instructions. Progressive subdivision does not lead to anything more real as an entity or toward anything more refined and pure in some absolute sense. It may possibly lead to less ambiguous classification instructions; but there exists no unique and ideal definition of a measure of a conceptual quantity.

There is no real and absolute object that can be identified in experience as an industrial sector or as a commodity, and there is no absolute and ideal way of measuring a price level or a growth rate of real investment. These things and measures have no existence apart from the minds of the participants in the discussion in which they figure. The definition of a commodity or of an industrial sector consists of a specification of a class of conceptual objects, and the definition of a measure of the change in the price level or of the annual rate of real investment is a stipulation of computing operations upon specified numerical data. These definitions and computing instructions are not such as may be called either *correct* or *incorrect*. Rather, they may be more or less *useful* in terms of the performance of the procedure in which they are employed.

Leontief, in classifying these objects and in making these computations, is not merely taking a census and constructing tables for the storage of general-purpose data. He is seeking stable relations for the purpose of prediction. He is striving to formulate a mode of collecting data and of performing computations that will yield numbers that are useful as predictions of other numbers that will be generated by an empirical process. A particular classification of objects serves his purpose if operations upon it yield stabilities that assist decision making. It cannot be taken for granted that subclassifying these objects will be effective in the establishment of the stabilities that are requisite for the predictive purposes he has in mind. The gained "definiteness" of a classification from the point of view of the census enumerator may add nothing to his ability to predict.

Leontief's discussion of numerical accuracy seems similarly questionable. If a class of objects has been defined, a method of making a count of these objects may be devised and conceivably a measure of the accuracy of this method could be agreed upon. If the definition of the class is ambiguous, the idea of accuracy would, of course, have no meaning. But aside from this, the accuracy of the method of counting is not an indication of the effectiveness of the predictive procedure in which the counted data are used. Leontief seems to associate the degree of accuracy of the method of counting with the degree of usefulness of his predictive procedure. The formulation of Leontief's procedure should include a specification of the degree of accuracy required of the data gatherer. But increasing the degree of accuracy of the counting methods is no sure way of increasing the effectiveness of the predictive procedure in which the data are used.

Leontief remarks that he has ". . . often been asked why such an ambitious data-gathering operation as that involved in the construction of a large input-output table makes no use whatsoever of the great resources of modern mathematical statistics. Why are our data not presented with proper appurtenances of standard deviations, coefficients of variation, and other appropriate measures of statistical reliability? The answer is that in its present stage this type of quantitative economic analysis can hardly make efficient use of the powerful . . . tools of probabilistic approach." He then classifies quantitative phenomena into three distinct types. These three types are illustrated by the subject matter of classical mechanics dealing with relations among individual entities, the subject matter of statistical mechanics dealing with population phenomena, and something in between. I am unable to make more of this third type than that it includes phenomena in which no one has yet found stabilities and uniformities that Leontief finds useful for his purposes; and in his comments he clearly begs the question of the effectiveness of his proposals for revealing the heretofore unrevealed stabilities. His statements regarding the stage at which statistical theory may be brought to bear bring to my mind a characterization of mathematical statistics that is excessively narrow. He seems to me to have in mind, not statistical theory, but a collection of devices derived from statistical theory by or for applied statisticians who have had specific problems to solve.

When I think of the more prominent applied statisticians of the past several decades, I do not see them as merely having sought for some way of presenting tabulations of data with the "proper appurtenances" of "measures of statistical reliability." When men such as G. U. Yule and R. A. Fisher have undertaken quantitative research and analysis, the knowledge upon which they have drawn extends beyond any listing of standard computational procedures that have been invented in the past for dealing with special kinds of situations. Statistical theory provides these men with a point of view, frame of mind, and theoretical knowledge upon the basis of which they have invented their own methodology to fit the situation confronting them. In their empirical work, statistical theory has been the source of hypotheses regarding the nature of the phenomena being studied no less than the basis for formulating tests of hypotheses. Their methodology leads them to proceed from simple hypotheses to successively more and more complex hypotheses, testing for the truth or falsity of these statements as they pass from one to another. Whatever the mode of testing they may hit upon, they are aware of the importance of rooting out the begged question.

The ideas of modern statistical theory, if not the operational aspects, would seem to me to be basic in economic analysis. These ideas, broadly interpreted, constitute a proper general background for economic theory. Leontief, in leaving these ideas out, appears to me to give an untenable representation of economic theory. For example, he states: "On the higher levels of theoretical analysis . . . the economist is bound to use concepts and to develop generalizations that are peculiarly his own and entirely different from the notions and generalizations evolved for purposes of practical decision making and for use in everyday economic transactions. Whenever he approaches the consideration of the more esoteric problems . . . much of the ground-floor experience gained from everyday economic intercourse and practical decision making . . . will remain beyond the pale of the organized body of economic information." There is bad theory and confused theorizing, of course. But among the competent, what is supposed to be the nature of the "esoteric problems" that are above or beyond those of "practical decision making"?

Individual decision-making agencies have definite tasks to perform, and they choose some procedure or method for performing them. The method or procedure chosen presumably performs better than any alternative procedure that is known to the chooser. Professionals or specialists are available to advise the chooser, and their competence lies in their ability to analyze the performance properties of alternative procedures and to invent a measure of the relative efficiency of a given procedure for performing a specified task. This is an engineering kind of problem—the designing of a structure (or procedure) so that the structure (or procedure) will have certain properties, the analysis of the performance properties of particular designs, and the comparison of the respective performance properties of alternative designs. This sort of thing constitutes an important part of the subject matter of statistical theory, and while the relevant theory is mathematical and esoteric in the sense of being comprehensible only to those who are trained in mathematics, it is of great practical value to decision makers.

The above refers to individual decision making, where all individual agencies conduct their decision making under a set of rules or laws that apply to each alike. A systematic cataloging of this general set of rules under which the collection of individuals is organized into a system constitutes a specification of an economic system. Such a system also has functions or tasks to perform, and the group must evaluate how well or how poorly an existing system is performing. That is, a group judgment is continually being made upon the performance properties of the existing economic system, and there is a continuing process of group decision making in the group choices that are made among alternative ways of changing the rules in order to *improve* the performance of the system.

It is here that a distinction may be drawn between, on the one hand, the staff personnel who assist the economizer in his economizing and, on the other, political economists who act as staff personnel not for an economizer but for a society of economizers. Engineers, market analysts, sample survey specialists, experts in accounting and comptrollership, all operate as economists in the sense that they assist individual decision makers in their seeking for best courses of action. In addition, there are those who specialize in dealing with problems that confront a group or society of decision makers. The discussion pro and con of the performance of an existing set of laws and rules, under which the individuals making up the group are operating, falls within the field of political economy. The designing of legislation changing the existing rules or laws is carried on with the view of *improving* the performance of the system. The specialists analyze the behavior properties of alternative sets of laws, and the judgment on whether or not a proposed change will constitute an improvement must be made by the group. This analysis of the properties implied in a given set of conditions regarded as alterable by the group may involve powerful methods not comprehensible to persons who have not mastered these methods. But if it has any meaning at all, it has reference to practical decision making by the group.

As I interpret the statements of the input-output analysts, they present themselves as prospective and present staff personnel both for individual decision-making agencies (governmental units as well as firms that operate under a general set of rules not alterable by an individual unit), and for the society of decision-making agencies (which can alter the rules that govern all). The competence that it is incumbent upon them to demonstrate is competence in the analysis of performance characteristics of rules or sets of rules as designed procedures for reaching specified objectives. This is the subject matter of modern statistical theory. Rather than not having reached the stage at which mathematical statistics can be applied, as Leontief says, the input-output analysts lay claims to having solved problems falling within this field. Their work cannot stand as an analytical application of the economic theory that has come through Walras. They have made no operational use of this theory and have derived from it only the modest assumption of interdependence among the actions of economic units making up a system.

D. C. MacGregor, *Department of Political Economy, University of Toronto*

The issues arising in the input-output conference of October 1952 involve matters of high academic policy in the field of research, of government policy in defense and budgeting, and (possibly) questions of professional ethics. These are delicate questions, not easily examined with candor in the company of those whose actions are being appraised.

The intense specialization among economists in some of the largest universities easily leads to activities of the sort under discussion, especially in a period of economic emergency. Little cults of confident researchers emerge, representing themselves as innovators. They make large claims for their work, necessarily in advance of delivery, in order to secure funds.

A research student can hardly be criticized for enthusiasm or perseverance, or even for repeating himself if he is requested to do so. If he has taken too much of the public's purse or of other academic men's time, that is the responsibility of the donors, not his, provided his original representations were honest and competent. But if the work continues without achieving the promised results, the time will sooner or later arrive when the donors will begin to ask whether they have now given enough, or perhaps too much, and whether the confident representations of the researcher or his agent should still be taken seriously.

I have spoken of the researcher as an innovator, but the word innovator here implies research of major importance, and assumes what ought to be proved. The input-output work may be regarded as an advanced *tableau économique*, as an extension of modern studies of general equilibrium and production functions, or simply as a complement to the recent development of aggregates of income and outlay. None of these descriptions implies more than a corrective or, at most, a secondary innovation.

In terms of practical objectives the work may be viewed as a contribution to the art of public administration, either for control of a totalitarian state or as a guide to the policy of a free economy in the emergency of war or economic collapse. As the totalitarian state is not acceptable in North America, researches that contribute to its realization are of negative social value, unless they can be justified for other purposes and be applied to those purposes only. The resemblance to arsenic is striking.

When the work is represented as a contribution to production functions, the writer becomes impatient with the talk about coefficients in view of the inadequate sources and simplified methods employed. The most impressive thing is the dignity and technicality of the language used. It is hard to see how aggregative inquiries in terms of expenses and receipts can give the desired results, and even harder to see how computations based on rough approximations can do so. Special studies seem desirable, as Leontief's recent volume recognizes, and if carefully chosen might prove more useful than the undertaking as a whole. If these are well done, it is doubtful whether it is worthwhile to fill the minor cells of the matrix with estimates that are either conjectural or disproportionately expensive to secure. My doubts about the value of intensive and costly work over the whole field are supported by the recollection that shortages and bottlenecks in World War II were sporadic and of a kind more likely to be anticipated by a few alert, well-informed men than by a speedy mathematical monster operating on outdated figures.

In weighing the cost of the undertaking, much depends on alternative uses of the funds. If the appropriations come from moneys available for other research in the social sciences, the cost in terms of alternatives foregone is enormous. But if they come from appropriations for defense or the general benefit of business, the cost need not be of immediate concern to the Conference in view of the probable low effectiveness of these alternatives. There is an intermediate case also: the funds may be diverted from work that, though not ordinarily considered research, is nevertheless important for economics. Weakening of the industrial or agricultural census, for example, would be a heavy price to pay for input-output research and would, moreover, undermine it; this is the familiar attrition of a long-established government activity that is likely to follow the introduction of a costly new one.

Further, one should not overlook the cost in terms of professional effort diverted to the work. On the project itself a large and specialized staff has been engaged. Less directly involved, but possibly more important in the aggregate, is the effort of others, such as that devoted to holding this Conference, the publication of this volume, and the reading and discussion of it. These, the real costs, are proportionately very great in view of the shortage of competent persons in economics.

Reverting to money cost, what is being paid for among other things is a unique statistical audit. Up to a rather large sum (I doubt whether small sums make much impression in national capitals), the money for such an audit is doubtless well spent, particularly since the staff employed is cheaper than a corps of professional auditors and far better fitted for the task. But such an inquiry need not go on forever. This raises the question of when to terminate funds for research.

It need hardly be emphasized that true research involves so much of the unknown, and hence unpredictable, that its duration and cost cannot be accurately anticipated. There are many false starts and negative results. Further, the money cost of advancing knowledge over different territories varies enormously, from almost zero to millions of dollars. Within this range lie averages of money and real cost per undertaking that cannot be computed and would be almost useless if they could. Decisions in the initial allocation of funds cannot, or at any rate should not, be based on a strict comparison of cost and accomplishment, for estimates of these are all humbug. Rather, they have to be based on the hunches of those who are supposed to be able to recognize the "feel" of promising situations and are prepared to take a chance. When further grants are needed, the same dependence on hunches may continue for some time before the enterprise justifies or obliterates itself by results.

From what I have been able to learn of the results in the present case, my feeling is that the field has been considerably oversold by its exploiters. They have laid undue emphasis on the rather limited theoretical advances involved, while full advantage has been drawn from the popularity of terms such as "empirical" and from the rather mystical appeal of the mathematics. Meanwhile, despite very heavy expenditure, the making of needful *ad hoc* measurements has been neglected until recently. The whole activity has, in consequence, developed an unreal character despite the professed aim of bringing us closer to reality.

My own view is that in a more or less free economy the value of the undertaking, other than its use as an audit, lies in providing occasional bench marks to be used in the event of war or other economic emergency. And since preparation of bench marks is more apt to be skimped than overdone, especially in a period when so much effort is devoted to making estimates for last week and the week after next, complete dismantling of the present research effort would seem unwise. Even so, the proposals for carrying on the work in the minor cells should be examined very critically. As to whether certain bench marks should be preferred to others, a priori judgments may not suffice. It would be well to supplement them by a review of histories of the war, such as those now appearing in Britain, and by conversation with key officials of the period. This should indicate what was most useful in the last war and whether a different emphasis is now required.

EZRA GLASER, *Bureau of the Budget*

My prepared paper for these sessions (not printed in this volume) provided a factual background on the scope and nature of the coordinated program of interindustry economics research by the federal government in late summer of 1952. One characteristic of the research that this Conference has repeatedly discussed is the absence of any plan to provide a conclusive appraisal of the worth of the technique by the comparison of actual and calculated values. Such comparisons would seem to be the simple and direct approach to the appraisal of the interindustry models. In fact, other less direct and less satisfactory means of appraisal have to be used because no "actual" data, in the sense intended above, exist.

Without detailed treatment, let me list three problems in making actual calculated comparisons. Any one of these might introduce errors into a test comparison that would be large enough to condemn the technique in the view of some of us, if they could be attributed entirely to faults in the technique being tested. Some of the problems are discussed in more detail in Frederick T. Moore's paper and in papers supplementing this Conference report.[1]

[1] "Input-Output Analysis: Technical Supplement," National Bureau of Economic Research, Multilithed, 1954.

The computation of an interindustry model yields a set of production levels for all the industries that comprise the economy. If an actual calculated comparison is to be made, these will be matched with actual production levels consistent with a set of historically accurate final deliveries of goods and services by the economy (as defined in the model). But where are the "actual" production levels by industry (or production indexes) that will serve as the standard of accuracy? In general there are none. There is a substantial amount of product data, but only a few reliable current measures of industry output. Of course, all of the production indexes may be improvised in some way from partial data or from the movements of other industries with putative correlated fluctuations. But this is not a standard of accuracy suitable for testing the quality of a technique. It is another set of estimates, very possibly of lower quality for many industries than those of the technique that ostensibly is being tested.

A second problem arises in the provision of the historically accurate final deliveries of goods and services by the economy. These can be subject to substantial error, even long after the close of the period covered by the estimates. For example, any clear detail of military expenditures of the federal government is difficult to obtain and has little assurance of accuracy adequate for testing a "calculated" result.

A third problem concerns the changes in price level over the period under investigation. The actual production levels need to be deflated to some base-year price levels, or the calculated values have to be inflated. Yet there are no very satisfactory price indexes for the output of many industries. Current wholesale price data are for products, and in many industries the weighted aggregate of covered items is a small fraction of the total value of production. This difficulty intensifies the two mentioned above.

These are not the only problems in comparing actual with calculated values of production from an interindustry model; they will suffice to dismiss the idea that a straightforward and satisfactory means of appraising the technique is at hand. We can never be sure an "historical" problem (stated as an actual bill of goods) is really an accurate portrayal of deliveries made; nor can we say (except for a few cases) how well the calculated production levels matched the real-world levels.

This is an unfortunate situation. It would be convenient if two columns of figures could be set side by side in such a way that a few minutes of study would make us all experts on the performance of interindustry models. It would greatly assist in guiding future research on the models. This is not to be possible for any foreseeable date. Appraisal will have to be by much more costly indirect methods, always leaving room for honest questions about what is being accomplished.

Finally, let us return to the problems that induced the federal government to engage in research in interindûstry models. The interest is in considerable detail of economic impact of a set of postulated expenditures by final consumers. A few GNP components, while very useful, do not allow adequate study of such resource limitations as industrial capacity or specialized employment. The need is for a technique in which major changes in the economy—not only the slow drift from quarter to quarter or even from year to year—can be explicitly represented. The world is capable of sudden changes in the actions of government, including the unforeseen outbreak of war, and the expert knowledge of economists to understand and deal with these problems should be augmented. This is the objective of the interindustry research project; the difficulties of the research are enhanced by the absence of a simple, decisive test of results as the work proceeds.