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A Review of Input-Output Analysis

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A. *Introduction*

Input-output economics can be regarded as a vast collection of data describing our economic system, and/or as an analytical technique for explaining and predicting the behavior of our economic system. The *sine qua non* of empirical input-output work is the input-output table, reminiscent of Quesnay's *tableau économique*. The table has one row and one column for each sector of the economy and shows, for each pair of sectors, the amount or value of goods and services that flowed directly between them in each direction during a stated period. Typically, the tables are arranged so that the entry in the rth row and cth column gives the flow from the rth sector to the cth sector (here r and c refer to any two numbers, such as 1, 2, etc.). If the sectors are defined in such a way that the output of each is fairly homogeneous, they will be numerous. The amount of effort required to estimate the output of each sector, and to distribute it among the sectors that use it, is prodigious. This phase of input-output work corresponds in its general descriptive nature to the national income accounts.

The analytical phase of input-output work has been built on a foundation of two piers. The first pier is a set of accounting equations, one for each industry. The first of these equations says that the total output of the first industry is equal to the sum of the separate amounts sold by the first industry to the other industries; the second equation says the same thing for the second industry; and so on. Thus, the equation for any industry says that its total output is equal to the sum of all the entries in that industry's row in the input-output table.[1]

The second pier is another set of equations, at least one for each industry. The first group of these equations shows the relationships

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[1] Inventory holding is regarded as an industry here, so that the output of each bona fide industry can be completely accounted for by its equation.

between the output of the first industry and the inputs it must get from other industries in order to produce its own output; the others do the same for the second and all other industries.

Work in input-output economics may be purely descriptive, dealing only with the preparation of input-output tables. Or it may be purely theoretical, dealing with the formal relationships that can be derived under various assumptions from the equations just mentioned. Or it may be a mixture, using both empirical data and theoretical relationships in the attempt to explain or predict actual developments. By "input-output analysis" I mean the latter two of these three types of input-output work, and it is to them that this paper is devoted.

It seems to me that the most fruitful areas to concentrate on for a review of input-output analysis are these: the theoretical position of input-output, the analytical implications of the data and techniques used in constructing input-output tables, the question of computation and errors (which has supported much speculation), and the question of the worth of input-output analysis (which has supported even more speculation). This paper is divided into four major sections corresponding to these four areas.

B. The Theoretical Position of Input-Output Analysis

1. THE GENERAL NATURE OF INPUT-OUTPUT

Input-output analysis is essentially a theory of production, based on a particular type of production function. Its key relationships are technological, involving quantities of inputs and outputs in productive processes. It does not present a *theoretically* complete picture of either the supply or the demand side of the economy, in that it does not envision optimizing behavior on the part of economic organisms faced with alternative courses of action.

Optimizing on the supply side is precluded by the characteristic and controversial assumption that the quantities of inputs used are directly proportional to the quantity of output, which implies that there is only one "recipe" by which to produce a given product.

Optimizing on the demand side is precluded in one of two ways, depending on whether one considers closed or open input-output models. In the closed models, which were developed first but are little used now, consumers or households are regarded as an industry whose output is labor and whose inputs are food, medical care, books, and other consumer goods. The household industry is treated just as the others are, in that its input-output ratios (technical coefficients) are assumed constant, so that for every man-hour of labor output, a fixed amount of each consumer good is required as input; that is, consumers are treated as a technologically determined production process, and not as choice-making organisms.[2]

In the open input-output models, the assumption of constant input-output ratios is dropped for one or more industries (households are almost invariably, and investment, government, and foreign trade are usually, in this group), and the input provided to these industries by any other industry becomes a component of the so-called bill of goods, or final demand. Then, if for each industry whose inputs are not included in the bill of goods the input-output coefficients are known and the total output is specified, the bill of goods can be determined.[3] Alternatively, if the input-output coefficients are known and the bill of goods to be produced is specified, one can determine the total output required of each industry whose inputs are not in the bill of goods. The term "final demand" is applicable to the latter case, in the sense that certain amounts of the outputs of one set of industries are specified as being desired by the rest of the economy, but even so there is no explicit optimizing; the final demand is chosen arbitrarily and is merely a part of the initial conditions, as far as input-output analysis is concerned.

Thus, like other production functions, input-output analysis per se deals with technology only, not with the preferences of economic organisms among different states of affairs.[4]

2. THE ASSUMPTIONS OF INPUT-OUTPUT

The characteristic assumption of input-output analysis is really a double one, and it is well to distinguish its two constituents, be-

[2] One might argue that the constant ratios of consumption of each good to the output of labor need not be assumed but can be derived as a result of an optimizing procedure on the part of consumers. But this, if true, would require a utility function wherein all goods are perfect complements, so that no good has any marginal utility, except in combination with the right proportions of all others. The argument must be rejected, because its premise is implausible and also because its conclusion—fixed ratios of consumption to labor output—is contrary to fact.

[3] If any component of the bill of goods turns out to be negative, this is a sign that the original set of specified outputs is inconsistent.

[4] It enables one, at best, to tell which combinations of inputs and outputs are possible and which are efficient in the linear programing sense, i.e. which are on the boundary of the production possibility range so that a reduction in any input or an increase in any output requires an increase in some other input or a reduction in some other output.

cause they are logically independent and because they differ in the attacks and defenses that can be brought to bear. One is the assumption of constant returns to scale. The other, and the more controversial, is the assumption that no substitution among inputs is possible in the production of any good or service. Alternative ways of putting the second assumption are: There is only one process used for the production of each output, or, the level of output of a product determines uniquely the level of each input required.[5] This second constituent assumption is sufficient to exclude any optimizing from the supply side, because it excludes all choice about the proportions in which inputs are to be combined in the production of a given output. With such a production function, all inputs are perfect complements, the marginal product of every one of them being zero, except in appropriate combination with all the others.

The assumption of constant returns to scale is contested on the ground that functions more complex than simple proportions are necessary to describe production processes realistically, particularly in industries like the railroads and utilities, where at least one large installation (such as a railroad track, a dam, or a telephone line) must be provided before any output appears. It is defended chiefly on grounds of simplicity. One need observe a productive process just once, say at one point in time, to obtain estimates of all the parameters of a simple-proportion production function,[6] and computations are simpler with this form than with almost any other. It is quite possible that analyses based on it will lead to empirically satisfactory results in some problems—of course this remains to be seen; if they do, that will be a splendid defense. Another defense sometimes offered is the argument that not enough is known to suggest what type of function should be used if proportions are rejected.[7]

The assumption that there is no substitution among inputs is often attacked because economists expect to find, and do find, substitution

[5] The idea of a process here is the same as that of a process or activity in linear or nonlinear programing. A process can be defined in terms of a particular set of proportions among inputs (if all inputs are doubled, the process is being used more, but output may not double; if the input proportions are changed, then by definition a new process is being used). Therefore, within any process there is no substitution among inputs.

[6] This explains why observations in a single year enable one to estimate input-output coefficients numerically.

[7] An obvious possibility to try is the ordinary linear function. If this is not very helpful, as I suspect it will not be, then observations at several different times on each industry might suggest possible alternative forms, but there is always the risk that technological change may have intervened to cloud the picture.

among inputs when relative prices change. This assumption too is defended on the general grounds of simplicity: data gathering and computation are much easier if one can regard an industry as a single process with fixed technical coefficients. In addition, analyses based on this assumption may yield empirically satisfactory results in certain problems, but this remains to be seen; I will come back to the problem later on, in section E.

Some economists, instead of defending this assumption, have scrapped it and admitted the possibility that each product may be produced by several different processes. If this modification alone is made, the assumption of constant returns to scale being retained, then the open form of input-output analysis turns into linear programing.[8] Substitution among inputs in the production of a particular output is then explained by shifts from one productive process to another.

A third assumption underlying input-output analysis is a kind of reverse of the first. It is that no process produces more than one output or, in other words, that there are no joint products. Two comments may be made here. First, if a process produces two or more outputs in nearly constant proportions, such as hides and meat, one can define a single new output for the process, consisting of all the original outputs together, and thus satisfy the assumption. Second, joint products cause no difficulty in linear programing, because every input is regarded as a negative output, so that there are already at least two "outputs" of each process anyway, and adding a few more positive ones will not change the character of the problem.

A fourth assumption, for the static models, is that only current flows of inputs and outputs are important, i.e. that problems of capacity and capital can safely be ignored. This assumption is not necessarily characteristic of input-output analysis. In fact, it has been replaced in several studies by suitable dynamic assumptions, with the aid of which the open input-output model can be made to "foresee" and provide for the capital requirements associated with a given future time-pattern of final demand.

[8] This is strictly true only if the number of alternative processes for producing a product is finite; if the number of alternative processes is infinite, and they form smoothly curved production surfaces, then open input-output analysis becomes the familiar continuous production theory instead of linear programing. See Tjalling C. Koopmans, editor, *Activity Analysis of Production and Allocation*, Wiley, 1951.

3. SAMUELSON'S THEOREM ON SUBSTITUTION

Apropos these assumptions, Paul A. Samuelson has proved an interesting proposition about substitution, namely: The absence of substitution among inputs in open input-output analysis need not be *assumed*, for it is already implied in the assumptions of efficiency in production, constant returns to scale, absence of joint products, and the existence of just one scarce primary resource (i.e. a scarce resource that is not produced).[9] The theorem says, in effect, that even though the production functions *allow* substitution among inputs, it does not take place, no matter how the final bill of goods is changed, because the achievement of efficiency in production always leads to a unique set of input ratios for each industry.

If it could be proved without the assumption that there is exactly one scarce primary input, the theorem would be truly incredible: Open input-output analysis would be vindicated in the face of the most troublesome and embarrassing theoretical charges that have been leveled against it, for the very assumption for which it is most criticized would be deduced as a logical consequence of the position taken by its critics! But the theorem *without* this assumption does not hold, whereas *with* the assumption it is obvious, and so unrealistic as to be uninteresting for input-output analysis. To see the situation intuitively, consider an economy like that assumed for the theorem: if there is just one resource to be economized on, then of course the efficient process for producing any final output is the process that uses least of that resource per unit of output, taking into account direct and indirect requirements together; and the set of processes (one for each output) that are efficient for *one* bill of goods must be efficient for *any* bill of goods, because of constant returns to scale and the absence of joint products. The unrealism lies in the fact that in our economy there are several scarce primary resources, not just one. Our mineral resources, for example, are important inputs that are not the output of any industry.

The conclusion to be drawn from this discussion is that, if input-output analysis is to meet the charge that it assumes away substitution among inputs, it will have to do so through results empirically

[9] See Paul A. Samuelson, "Abstract of a Theorem Concerning Substitutability in Open Leontief Models" in Koopmans, *op. cit.*, pp. 142-146. Actually, Samuelson assumed further that the production functions have continuous derivatives, but Koopmans and K. J. Arrow, in the two succeeding papers in the same volume, have extended the proof to the discontinuous cases of three and *n* variables, respectively.

arrived at, not theoretically arrived at. I shall return to the point in section E.

4. INPUT-OUTPUT AND GENERAL EQUILIBRIUM

Wassily Leontief has characterized input-output analysis as a general-equilibrium system, in contrast to partial-equilibrium systems.[10] This is correct with respect to the "general" part, but not with respect to the "equilibrium" part. Input-output analysis is like general-equilibrium theory in that it encompasses all products and industries, rather than singling out one or a few for study and relegating the others to the pound of *ceteris paribus*. Thus, the impact of a change in any corner of the economy can conceivably find its way via indirect effects through the input-output chart to every other industry. However, input-output analysis is unlike general-equilibrium theory in that it is not in itself an equilibrium system, any more than is any other production function. It is possible to incorporate the open form of input-output analysis into a general-equilibrium system by introducing utility functions or demand schedules or something of the kind to reflect the preferences of those who control the use of the outputs.[11] The same, of course, can be done with other production theories as well. At its best, open input-output analysis can tell us the cost, in terms of labor and primary resources, of producing each possible bill of goods. It cannot rank those bills of goods in order of preference. That is the function of the preference scales of those whose decisions control the economy —in our case, individual consumers and firms and government agencies. They give rise to demand functions that, together with the input-output equations, can determine an equilibrium bill of goods.[12]

[10] Wassily Leontief, *The Structure of American Economy, 1919-1939*, 2nd ed., Oxford, 1951, pp. 33-34, 203-204. The material in the latter pages also appeared in "Recent Developments in the Study of Interindustrial Relationships," *The American Economic Review*, May 1949, pp. 212-213.

[11] Let the assumption of constant input-output ratios be accepted for the moment, so that the adequacy of input-output as a production function is not questioned; then the introduction of optimizing on the demand side is sufficient to yield a general-equilibrium system incorporating open input-output analysis as its production function.

[12] A notable example of a general-equilibrium system built around an input-output production function is presented by Jerome Cornfield, W. Duane Evans, and Marvin Hoffenberg in "Full Employment Patterns, 1950," *Monthly Labor Review*, February and March 1947. The authors first projected labor force, wages, and hours for 1950, and so obtained a projection for full employment national income in 1950. They then projected the quantities of goods and services that would be demanded from each industry by households, businesses,

C. Data and Techniques in Constructing Input-Output Charts

It is impossible to divorce a discussion of input-output analysis from a consideration of the types and quality of data on which the analysis is to be based. Presumably, it is for this reason that most of the papers on the program of the Conference are concerned mainly with problems of data and techniques used in constructing input-output charts. The problems are legion: whether to use producers' values or purchasers' values; how to approximate a commodity basis for defining industries when most of the available data are on an establishment basis; how to treat secondary products; how to handle imports of goods that are also domestically produced; where to charge transport costs; how to handle scrap; and so on and on. And there is the ever-present problem of where to get more basic information. To review the manner in which such problems were actually solved or avoided is a prodigious task. For this reason, it is fortunate that the papers on data and techniques have discussants. I shall leave the task to them, except for one general observation.

Any dissatisfaction that one may feel about how all these conceptual and practical problems have been solved—and there is certainly room for some—should be traced to its sources. For one thing, it may be plausibly argued that the BLS staff did not find the best solutions to all of its problems, that a wiser or more careful or more experienced team of economists could have done better at certain points. To the extent that this is true, it is cause for criticism of the data and techniques used. But there is another point. The very decision to construct an input-output table, and thus to cast the economy in the framework of a set of industries of which each is supposed to produce a more or less homogeneous output with the aid of inputs purchased from other industries, immediately raises a

and government at the projected level of income. (For this they implicitly used consumption and investment functions embodying choice making.) They then used these quantities as a final bill of goods, and applied them to the 38-by-38 1939 matrix to estimate the total outputs required of each industry. They then used projected labor-productivity figures to estimate the total amount of employment required to support the estimated industry outputs. If this required level of employment had turned out to be just equal to the projected labor force (actually it was less), the study would have predicted that full employment would be the equilibrium level of employment in 1950, for it would have shown that the income generated by full employment would lead to sufficient demand to maintain full employment. Thus, the study built up the input-output model into a general-equilibrium system by adding to it a set of relationships describing the choices of income receivers.

host of problems, some of which do not have really satisfactory solutions, so that one would feel critical no matter what the BLS did about them. This is the case with questions of classification, with the phenomenon of joint production, and with many other problems.

To put the matter another way: It seems to me that in most instances the BLS economists have adopted the most reasonable approach and have indeed been quite ingenious on some points, but that even if they had been supermen, they could not have arrived at wholly satisfactory solutions at every point. It is in the nature of the input-output scheme, as of any other abstraction, to create some problems that cannot be solved but can only be compromised with.

D. Errors and Computation

Input-output analysis soon ran into computation problems too big for the ordinary desk calculating machine. In the open form, when a bill of goods is assumed, and the levels of output required of each industry in order to produce it are calculated, a system of linear equations must be solved, one equation for each industry. In order that the industries used have some semblance of internal homogeneity, they must be small and numerous. For example, the 1939 input-output matrix, prepared by the Bureau of Labor Statistics and inverted shortly after the war, has 38 industries. Two 1947 matrices prepared by the BLS and recently inverted have 44 and 190, respectively.[18] The BLS is now collecting data for a table having some 450 industrial categories.

The number of two-factor multiplications involved in obtaining the inverse of a matrix with n rows and columns is approximately n^3; thus, for a 38-by-38 inverse about 50,000 multiplications are required; for a 190-by-190, about 7,000,000; and for a 450-by-450, about 90,000,000. Such large-scale computation jobs require electronic calculators like the Univac, which has been used for most of the input-output calculations. They also raise serious questions regarding computational accuracy, for as has been remarked repeatedly, when a large matrix is inverted, or a large system of linear equations is solved, small errors can sometimes accumulate to such

[18] The 44-by-44 matrix and its inverse appear in W. Duane Evans and Marvin Hoffenberg, "The Interindustry Relations Study for 1947," *The Review of Economics and Statistics*, May 1952, facing p. 142. The 190-by-190 matrix and its inverse were distributed at this Conference and presumably may be obtained from the BLS.

an extent that the results become useless. The nature of input-output matrices is such, however, that much narrower limits can be set on the possible errors in inverting them than is the case with ordinary matrices.

I shall now discuss four types of error mentioned by John von Neumann and H. H. Goldstine in a paper that deals chiefly with one type.[14] First, since a computing machine has a limited number of decimal places in its "memory," the results of multiplications and divisions (not additions or subtractions) performed early in the computation may have to be rounded off before they are fed into the later stages, and large errors may result from the compounding of this process. Second, if the first few terms of an infinite series are used as an approximation formula for a quantity that is difficult to compute exactly, an error will enter because not all the terms of the infinite series are used. Third, the data fed into the computation process, i.e. the input-output coefficients in our case, may contain errors that will affect the result. And fourth, the linear equations used may not represent exactly the reality they are meant to describe.

1. ERRORS DUE TO ROUNDING

The paper of von Neumann and Goldstine is concerned with the first type of error, that due to rounding. They set out to find a relationship between the order of a matrix (i.e. the number of rows or columns) and the number of *extra* decimal places that a computing machine must carry—places in addition to the number desired in the result—to insure that the inverse of the matrix can be computed with negligible chance of error from rounding. They wished to be conservative, that is, to overestimate the required number of places. Therefore, they made several conservative statistical assumptions about the values of the upper and lower bounds of an unspecified high-order matrix,[15] and assumed conservatively that the rounding error introduced by each separate multiplication and division during the inversion is the maximum possible, namely, 0.5 in the last retained decimal place. Their result is that errors due to rounding will be likely to have no effect in the inversion of an n-by-n

[14] John von Neumann and H. H. Goldstine, "Numerical Inverting of Matrices of High Order," *Bulletin of the American Mathematical Society*, November 1947, pp. 1021-1099. I have reversed their order of presenting the four types of error.

[15] Their assumptions are even more conservative when applied to input-output matrices, because the latter have certain special properties, described in section D3.

matrix so long as the number of extra places carried in the computation and dropped at the end is at least as great as the common logarithm of $2{,}000n^4$. This means that to invert a 44-by-44 matrix, ten extra places must be carried; to invert a 500-by-500, fourteen extra places must be carried. Since accuracy to three or four decimal places in the inverse is sufficient for practical purposes, at most seventeen or eighteen places must be carried altogether. This requirement is well within the range of modern electronic computers, so that errors due to rounding present no cause for apprehension.

2. ERRORS DUE TO APPROXIMATION FORMULAS

The second type of error, due to the use of truncated infinite series, may arise in inverting an input-output matrix, as follows: The basic input-output equations can be written in a simple form, if A represents the square matrix array of technical coefficients, X represents a column of total outputs, one for each industry, and Y represents a column of final demand, one component from each industry. Thus,

$$X = AX + Y, \tag{1}$$

i.e. total-output-of-each-industry (X) equals output-delivered-to-other-industries (AX) plus output-delivered-to-final-demand (Y). This is equivalent to

$$X - AX = Y. \tag{2}$$

When X is factored from the left side, the result is

$$(I - A)X = Y, \tag{3}$$

where I is a matrix that behaves among matrices as the number 1 behaves among numbers; it is called the identity matrix. Now, in the usual input-output problem, all the components of A (the technical coefficients) and of Y (the final demand) are known or assumed, and it is desired to find X, the total outputs of each industry that are necessary to produce the given final demand. In ordinary algebra, when one has an equation like $(1 - a)x = y$ and wishes to solve it for x, one divides through by the expression $(1 - a)$ and gets $x = [1/(1 - a)]y$, or as it is sometimes written, $x = (1 - a)^{-1} y$. That is just what one does with a matrix equation like (3), and the result is written thus:

$$X = (I - A)^{-1}Y. \tag{4}$$

The expression $(I-A)^{-1}$ is another matrix, and is called the inverse of the matrix $(I-A)$. (Note that the inverse of A itself is *not* used; this is true throughout.) The computation of $(I-A)^{-1}$ is what takes so many multiplications; that is not really surprising when one thinks that in this operation is performed most of the work of solving at one stroke all of the sets of n linear equations in n unknowns that can be obtained from the given matrix of coefficients (A) by changing the constant terms (Y) that are used.

The ordinary geometric progression suggests a simple method for calculating the approximate inverse of $(I-A)$. The progression $1+a+a^2+a^3+a^4+\ldots$ has an infinite number of terms, but if a is less than 1, then the terms get smaller so fast that there is a limit to the value of the progression; the more terms one adds, the closer the sum gets to the value $1/(1-a)$, and one can make it as close as desired by taking enough terms. Thus, one can use the first few terms of the progression as an approximate value for $1/(1-a)$.

The matrix analogy holds true, with a modification necessitated by the fact that a matrix is not a number and so cannot be compared with 1 to see which is larger. If a matrix A has no negative numbers in it, and if the sum of the numbers in each column is less than or equal to 1, then the progression $I+A+A^2+A^3+A^4+\ldots$ has as its limit $(I-A)^{-1}$, the inverse we need.[16] Because of the way

[16] A proof for the case where the sum of the elements in each column of A is *strictly less* than 1 is given by Frederick V. Waugh, "Inversion of the Leontief Matrix by Power Series," *Econometrica*, April 1950, pp. 142-154, especially pp. 145-148. This case is also treated by Harold Hotelling, "Some New Methods in Matrix Calculation," *Annals of Mathematical Statistics*, March 1943, pp. 1-33, especially pp. 13-14, and by Robert Solow, "On the Structure of Linear Models," *Econometrica*, January 1952, pp. 29-46. A proof for the case where the column sums of elements of A are merely less than *or equal to* 1 can easily be extracted from "A Fundamental Property of Systems Characterized by Non-Negative Matrices Whose Column Sums are Less Than or Equal to Unity," a paper presented by John S. Chipman at the meeting of the Econometric Society in Chicago, Dec. 27, 1952, abstracted in *Econometrica*, July 1953, p. 467.

Y. K. Wong of Princeton University has pointed out to me that these proofs date back to C. Neumann's work in 1877, *Untersuchungen über das Logarithmische und Newtonsche Potential*, XVI, Teubner Verlagsgesellschaft, 1877, p. 368. Neumann showed that:

$$\frac{I}{\lambda}+\frac{A}{\lambda^2}+\frac{A^2}{\lambda^3}+\frac{A^3}{\lambda^4}+\ldots=(\lambda I-A)^{-1}$$

where A is any square matrix and λ is any number greater in absolute value than the largest modulus among the characteristic roots of A. If all the characteristic roots of A have moduli less than 1, then we can choose $\lambda=1$, and the above equation becomes

$$I+A+A^2+A^3+\ldots=(I-A)^{-1}.$$

The recent proofs by Chipman, Solow, and others use this theorem plus another they are obtained, all of the BLS input-output matrices A do have the properties required by this theorem.[17] Therefore, the first few terms of the progression can be used as an approximation to $(I-A)^{-1}$, and the approximation can be made as close as one desires by using enough terms of the series. The virtue of this method is that it is computationally simpler to obtain and add together a few low powers of a matrix than to obtain its exact inverse directly.

Upper bounds for the error incurred by stopping after the kth power term have been worked out,[18] and they show this method to be a practical one. That is, the number of terms required to get acceptable accuracy is small enough so that this method is cheaper in terms of computational effort than is the direct method of computing the inverse.

As a measure of purely computational accuracy in inverting any particular matrix such as $(I-A)$, one can compute the product $(I-A)(I-A)^{-1}$. If the computation is entirely accurate, this product will be equal to the identity matrix I. Hence, the difference between the product and I will be a matrix whose elements indicate the size of the computing error. For the 38-by-38 1939 matrix, the largest element in this difference was less than .000000028; for the 44-by-44 1947 matrix, it was less than .000000042; and for the 190-

to the effect that in a Leontief-type matrix (having all elements zero or positive, and all column sums less than or equal to 1) the characteristic roots all do have moduli less than 1.

[17] Proof: Quantities of product in an input-output table are measured in units of $1. The tables prepared by the BLS have the property that the row and column sums for any industry not in the final demand sector are the same, which is to say that the total value of the output of an industry in the base year is just equal to the total value of *all* its inputs. (The inputs of capital and entrepreneurship to any industry are valued in terms of the profits of the industry, which are included with wages and salaries in the household row; hence, the equality of row and column sums holds even for industries experiencing profits or losses in the base year.) The element in the rth row and cth column of an input-output matrix A is obtained as the base-year value of product flowing from the rth industry to the cth industry divided by the base-year value of the total output of the cth. Hence, no element of A is negative. Further, the sum of elements in the cth column of A is the fraction of the total value of the base-year output of the cth industry that went to buy the inputs included in A (i.e. the inputs not produced by the final demand sector). Since all inputs (including labor, capital, and entrepreneurship) are included in a closed matrix and not all in an open matrix, either all or less than all input costs of the cth industry are accounted for by any matrix. Hence, the cth column sum must be equal to or less than 1. The same applies to every column of A.

[18] Waugh, *op. cit.*, pp. 148-153.

by-190 1947 matrix recently inverted, it was less than .00000037.[19] These errors are effectively zero, indicating that the pure *computing* errors, arising from rounding off during the operations or from using approximation methods, are no problem at all.

3. ERRORS DUE TO INACCURATE DATA

The third source of error, inaccurate data, is of a different kind because the calculation of $(I-A)(I-A)^{-1}$, as described just above, cannot throw any light at all on its importance.

The problem arises in the following way: First, suppose that a particular technical coefficient is estimated from past data to be .150; that while this is not regarded as absolutely correct, the men who obtained the estimate feel, on the basis of the data available to them, that it cannot be off by more than 10 per cent either way; and that all other technical coefficients are exactly known. The questions then arise, How large is the possible error in each element of the inverse matrix, i.e. over how wide a range could each element of the inverse matrix shift, if this particular technical coefficient in the matrix A is allowed to vary from .135 to .165? And how great can be the resulting error in each component of X, the column of predicted total outputs, obtained from the inverse and the final bill of goods by equation (4)? Then, suppose that *every* technical coefficient has a range of uncertainty such that one feels sure only that its true value is in the range. Again the questions arise, How large is the possible error in each element of the inverse? And how large is the possible error in each element of X, the column of predicted total outputs of each industry? These are realistic questions, for those who work on the preparation of the input-output charts usually have a rough idea of the limits within which the correct value of each element of the matrix A is sure to be.

There are 1,936 entries in the 44-by-44 1947 matrix, of which about half are not zero, so that the task of obtaining from the Bureau of Labor Statistics and using a separate pair of limits for each entry would be cumbersome, to say the least. It is much simpler to assume limits of uniform size for all the coefficients; they may be stated either in absolute terms (e.g. plus or minus .005) or

[19] From a paper presented by Herbert F. Mitchell at the meeting of the Econometric Society in East Lansing, Mich., Sept. 3, 1952, abstracted in *Econometrica*, April 1953, pp. 344-345. Actually, each of the figures given above in the text is the square root of the sum of the squares of all the elements of the difference, so the largest single element of the difference is still less.

in percentage terms (e.g. plus or minus 2 per cent) and can be made large enough so that they do not understate the uncertainty in any coefficient. (They will probably then overstate the uncertainty in a great many.)

In the case of unrestricted matrices, one cannot set very narrow limits on the errors in the elements of an inverse that are due to errors in the original matrix. Fortunately, the input-output matrix A has some special properties that help a great deal, as we shall now see. As already noted, each element (technical coefficient) is positive or zero, each column sum is less than or equal to 1 (less for the open matrices we now consider), and the series $I + A + A^2 + A^3 + \ldots$ converges in the limit to the inverse $(I - A)^{-1}$.

From these it follows that each element of the inverse is positive or zero; actually, most are strictly positive.[20] Also, if any element of A is understated, then as a result some of the elements of the inverse will be understated, and some (depending upon the location of the zeros in A) may be unaffected, but none will be overstated; similarly, if any element of A is overstated, then as a result some of the elements of the inverse will be overstated and some may be unaffected, but none will be understated. This follows from the fact that the inverse can be expressed in the form of the power series in A, the fact that all elements of A are nonnegative, and the rules of matrix multiplication and addition.[21] Therefore, if there are errors in different directions in different elements of A, they will partially compensate each other.[22] Or, equivalently, if each element of A is known to be off either way by not more than an absolute amount α (or a percentage π), then the outside limits within which

[20] To begin with, each element of I is either 1 or 0, and each element of A is either positive or 0. Hence, each element of A^2 or any higher power of A is either positive or 0. Furthermore, the zeros dwindle in number as one gets to higher powers. A zero in the rth row and cth column of A means that some one industry (the rth) does not sell to some other (the cth); a zero in the rth row and cth column of A^2 means that some two industries are so unrelated that one of them (the rth) does not sell to any industry that sells to the other (the cth); etc.; a zero persisting in the rth row and cth column of every power of A would mean that two industries are so completely unrelated that none of the output of one of them (the rth) ever finds its way, after further processing, into the other (the cth) without passing through the final demand sector.

[21] Even though some elements may be overestimated, the economic character of the matrix guarantees that they are not overstated so much as to make any column sum greater than 1, and thus spoil the convergence of the series; see note 17.

[22] This point is made independently by W. Duane Evans in a mimeographed paper, "The Effect of Structural Matrix Errors on Input-Output Estimates," which was circulated at this Conference. See *Econometrica*, October 1954, pp. 461-480.

the true values of the elements of the inverse must lie are given by two modified inverses of $(I - A)$, one obtained when every element of A is diminished by an amount α (or a percentage π) and the other obtained when every element is increased by an amount α (or a percentage π). Of course, to compute both of these inverses is twice as much work as finding the inverse of $(I - A)$ in the first place. What is wanted is a shortcut that avoids the necessity of computing any more inverses and that leads to an estimate of how large, at worst, the errors in the computed inverse can be if limits on the size of the errors in A are known.[23]

Frederick V. Waugh has succeeded in expressing the difference between the true and computed inverses as a product of the computed inverse with another matrix, thus providing a shortcut.[24] Let A^* be the true matrix of input-output coefficients, and A be the estimated matrix. Let C^* stand for $(I - A^*)$, and let C stand for $(I - A)$. Then C^{*-1} is the true inverse, and C^{-1} is the computed inverse.[25] Define E to be the matrix of errors in estimating A^*; thus, $E = A^* - A = C - C^*$. Define D to be the difference between the true and computed inverses; thus, $D = C^{*-1} - C^{-1}$. Define an auxiliary matrix, $F = EC^{-1}$. Then, provided F is such that the sum of the absolute values of the elements in each of its columns is less than 1 (this is the same condition that was used above for the existence of a limit of a power series, and it will be true if the error elements of E are not too big), the difference D can be expressed as a product of C^{-1} with another matrix; thus,[26]

$$D = C^{-1}(F + F^2 + F^3 + F^4 + \ldots) . \tag{5}$$

[23] The reader not interested in the mathematics of this argument may wish to skim or skip the next paragraph and some of the footnotes below.

[24] Frederick V. Waugh, "On Errors in Matrix Inversion," unpublished typewritten note of six or eight pages. Waugh wrote that I might quote this note. Since then he and Paul S. Dwyer have published a joint paper containing this result and several others related to it (*Journal of the American Statistical Association*, June 1953, pp. 289-319).

[25] I proceed as if C^{-1} were the exact inverse of C, obtained with no computational errors. This is justified by the accuracy exhibited at the end of section D. Thus, in the following discussion, the only errors assumed are in measuring the elements of the matrix.

[26] Proof:

$$\begin{aligned} D = C^{*-1} - C^{-1} = (C - E)^{-1} - C^{-1} &= [(I - EC^{-1})C]^{-1} - C^{-1} \\ &= [(I - F)\,C]^{-1} - C^{-1} = C^{-1}(I - F)^{-1} - C^{-1} . \end{aligned}$$

Now, if each column of F has a sum of absolute values of its elements less than 1, $(I - F)^{-1}$ is equal to $I + F + F^2 + F^3 + F^4 + \ldots$, and the last expression above becomes

$$\begin{aligned} D = C^{-1}(I + F + F^2 + F^3 + F^4 + \ldots) &- C^{-1} \\ &= C^{-1}(F + F^2 + F^3 + F^4 + \ldots) \text{, Q.E.D.} \end{aligned}$$

Waugh continues by assuming that every technical coefficient in A is off by no more than the absolute amount α, i.e. that the absolute value of each element of the error matrix E is less than α. He then shows that, if α is not too big, the error in the element in the rth row and cth column of the inverse, which error he calls d_{rc}, cannot be greater in absolute value than the product of α times the sum of the elements in the rth row of the computed inverse times the sum of the elements in the cth column of the computed inverse, all divided by the following expression: 1 minus the product of α times the sum of all the elements of the computed inverse. In other terms,[27]

$$(6)\quad |d_{rc}| \leq \frac{\alpha\,(\text{sum of } r\text{th row of } C^{-1})\,(\text{sum of } c\text{th column of } C^{-1})}{1-\alpha\,(\text{sum of all elements of } C^{-1})}\,.$$

[27] Proof:

a. From (5) and the fact that C^{-1} has all positive elements, the elements of D will be largest in absolute value when the elements of F, and therefore also of E, have the maximum positive values.

b. To be conservative, we assume the worst, i.e. that every element of E is equal to α.

c. Hence, F, which is EC^{-1}, becomes $\begin{bmatrix} f_1 & \dots & f_n \\ \vdots & & \vdots \\ f_1 & \dots & f_n \end{bmatrix}$

where $f_c = \alpha \sum_i (C^{-1})_{ic}$ and $(C^{-1})_{ic}$ = the element in the ith row and cth column of C^{-1}.

d. Define $s = \sum_c f_c = \alpha \sum_i \sum_c (C^{-1})_{ic}$.

e. Then $F^2 = \begin{bmatrix} (\Sigma f_c)f_1 & \dots & (\Sigma f_c)f_n \\ \vdots & & \vdots \\ (\Sigma f_c)f_1 & \dots & (\Sigma f_c)f_n \end{bmatrix} = (\Sigma f_c)F = sF$.

Similarly, $F^3 = s^2F$, $F^4 = s^3F$, etc.

f. Then $(F + F^2 + F^3 + F^4 + \dots) = F(1 + s + s^2 + s^3 + \dots)$.

g. Then, if $s < 1$, the preceding expression is equal to $\left(\frac{1}{1-s}\right)F$.

h. Then the maximum possible absolute value for d_{rc} is found, by using (5) to get $D = C^{-1}\left(\frac{1}{1-s}\right)F$, to be

$$\max\left|d_{rc}\right| = \frac{\sum_j (C^{-1})_{rj}f_c}{1-s} = \frac{[\sum_j (C^{-1})_{rj}]f_c}{1-s}$$

$$= \frac{\alpha[\sum_j (C^{-1})_{rj}]\,[\sum_i (C^{-1})_{ic}]}{1-s} = \frac{\alpha[\sum_j (C^{-1})_{rj}]\,[\sum_i (C^{-1})_{ic}]}{1-\alpha\sum_i\sum_j (C^{-1})_{ij}}, \text{ Q.E.D.}$$

Note that the condition that α not be too big turns out to mean that α must be small enough to insure that every column sum in F will be less than 1 so that (5) will hold, *and* that s will be less than 1 so that step g can be made.

I shall now apply this result to the 44-by-44 1947 matrix, and try to answer in turn the two questions raised above, i.e. how large can the errors be in the elements of the inverse and how large can the errors be in the components of X, the predicted total outputs in each industry? The argument supporting (6) holds only if α is small enough so that every absolute column sum in F is less than 1, *and* so that α times the sum of all the elements of the inverse is less than 1. Since the maximum column sum in the inverse is approximately 2.9 (column 42),[28] the maximum column sum in F is 2.9×44, and so the first condition requires that α be less than $1/(2.9 \times 44)$, i.e. less than .0078. Since the sum of all elements in the inverse is approximately 96, the second condition requires that α be less than .0104. Hence, the formula holds if α is less than .0078. It will be useful to find the widest error limit in the inverse and to evaluate it in terms of α. From formula (6) one can see that it belongs to the element in the row and column of the inverse whose sums are the largest, which turn out to be row 43 (undistributed, with a sum of 7.2) and column 42 (scrap, with a sum of 2.9). Table 1 shows the calculation of max $| d_{43,42} |$, the error limit for this element, for various possible values of α, and also the average error limit for *all* elements of the inverse.[29]

How large an α is it reasonable to assume, and hence, how large a maximum error in the elements of the inverse is it reasonable to expect? Consider that the largest element of A is .44 (row 9, column 38), and that only sixteen others exceed .20. An α of .004 then represents an error of about 1 per cent in the largest element of A, between 1 per cent and 2 per cent for sixteen other elements, and over 2 per cent for all the rest. This would mean at worst (see Table 1) that the least accurate element of the inverse might be off by as much as .14, and the average element might be off by as much as .032. I would argue that .004 is an unreasonably small value to assume for α, at least for any elements of A greater than .10 or so, since so small an α implies a percentage error of less than 4 per cent for those elements. On the other hand, I would argue that the limits

[28] Note that in the tabulated inverse, published in Evans and Hoffenberg's paper in *The Review of Economics and Statistics* for May 1952, the rows and columns have been interchanged. I refer here to the noninterchanged form.

[29] The average column sum in the inverse is 2.2, as is the average row sum; hence, the average product is $2.2^2 = 4.84$. The largest product, as noted, is $7.2 \times 2.9 = 20.9$. Hence, from (6) the average error limit is 4.84/20.9 times the largest, max $| d_{43,42} |$. The element in row 43, column 42 is .135. The average element is $.135 \times 4.84/20.9$, or .050.

shown in Table 1 are unrealistically wide, on the ground that they are obtained by assuming all errors in A to be in the same direction, whereas input-output matrices actually are not likely to have all their errors in the same direction, and, as already noted, errors in opposite directions partially compensate each other.

To explore the possibility of compensating errors, let us look at the manner in which errors in the technical coefficients may arise. In the preparation of an input-output flow chart, the row and column totals representing the total base-year outputs of the various industries, designated by $\overline{X}_1, \overline{X}_2, \ldots \overline{X}_n$, are obtained first. (The bars above the symbols here indicate that base-year quantities are meant.) They are used as control totals in estimating the amounts of the output of each industry that went to the various other industries. Thus, if we use $\overline{X}_{rc}$ to stand for the base-year amount of the rth product going to the cth industry, and $\overline{Y}_r$ for the base-year final demand from the rth industry, the distribution of the rth product among its direct uses in the base year is

$$\overline{X}_r = \overline{X}_{r1} + \overline{X}_{r2} + \ldots + \overline{X}_{rn} + \overline{Y}_r . \tag{7}$$

The estimation of each $\overline{X}_{rc}$ can be thought of as having two parts: first, the estimation of the total base-year output of the rth industry, $\overline{X}_r$; second, the distribution of this amount to all industries, in proportions that we may denote by $k_{r1}, k_{r2}, \ldots k_{rn}, k_{ry}$, where the sum of all the k's for any industry (i.e. the k's having the same first subscript) must of course be 1 because all output must go somewhere. Consequently,

$$\overline{X}_{r1} = k_{r1}\overline{X}_r; \quad \overline{X}_{r2} = k_{r2}\overline{X}_r; \quad \ldots ; \quad \overline{X}_{rn} = k_{rn}\overline{X}_r . \tag{8}$$

Now each $\overline{X}_{rc}$ gives rise to a corresponding technical coefficient when divided by the total output of the receiving industry X_c; thus,

$$a_{rc} = \frac{\overline{X}_{rc}}{\overline{X}_c} = \frac{k_{rc}\overline{X}_r}{\overline{X}_c} \tag{9}$$

Hence, one may distinguish two components in the estimation of any element a_{rc} of the input-output matrix A: the obtaining of the quotient of the total base-year outputs of the two industries involved, $\overline{X}_r/\overline{X}_c$; and the estimation of the proportion of $\overline{X}_r$ that went to industry c, namely k_{rc}.[30] Now what about the errors in each?

[30] I realize that in practice one does not establish the industry totals firmly and then distribute them; actually, they may be adjusted as a result of the

TABLE 1

Error Limits for Elements of the Inverse for 44-by-44 1947 Matrix as a Function of a

a, the maximum absolute error in any element of A	.0078	.0075	.004	.002	.001	.0005
$s = 96a$	1.00	.72	.38	.192	.096	.048
$a/(1-s)$	∞	.027	.0065	.0025	.0011	.0005
max $\vert d_{43,42} \vert$, the error limit for the element in row 43, column 42, of the inverse	∞	.56	.14	.05	.023	.011
max $\vert d_{43,42} \vert$ as a per cent of the element in row 43, column 42, of the inverse	∞	420%	104%	37%	17%	8%
Average (max $\vert d_{rc} \vert$) over all elements, the average error limit in the inverse	∞	.13	.032	.012	.005	.0025
Average (max $\vert d_{rc} \vert$) as a per cent of the average element of the inverse	∞	260%	65%	24%	10%	5%

It is *impossible* for all the quotients $\bar{X}_r/\bar{X}_c$ to be in error in the same direction, since the reciprocal of every quotient is also among the quotients; for example, if $\bar{X}_2/\bar{X}_5$ is too large, then $\bar{X}_5/\bar{X}_2$ is too small. Note, further, that *no* error enters into the quotients $\bar{X}_r/\bar{X}_c$, if all the total outputs for various industries are off by the same percentage; the quotients can only be off if the percentages of error in the various total outputs differ.

It is *unlikely* that all the proportions k_{rc} will be in error in the same direction, as the following argument shows. The sum of all the k's in any row or column of the chart (including the k's for final demand) must add up to 1, so that in a closed matrix any error in a particular k_{rc} must be offset by opposite errors in the same row and in the same column. The only way in which all the k_{rc} in an open matrix could be off in the same direction would be for too much (or too little) of the output of every single industry to be allocated to final demand.

Therefore, it is likely that the errors in the input-output matrix A will not be predominantly of the same algebraic sign. This means, as noted a few paragraphs above, that for any given α the resulting errors in the elements of the inverse will not be as large as the error limits in Table 1. Furthermore, it is likely that some of the elements of the inverse will be overstated and some will be understated, since, as noted, the sign of the error in any element of A is the same as the sign of any resulting errors in the inverse.

There remains the second of the questions raised earlier: How large will be the errors in X, the column of predicted total outputs of the individual industries? Each industry's predicted total output is the sum of pairwise products of elements of its row of the inverse with the column Y of final demands, according to equation (4). Hence, the error in the predicted total output of an industry is a weighted sum of the errors in the corresponding row of the inverse, the weights being the final demands, and it is likely to be smaller percentagewise than its largest component error, since, as shown in the preceding paragraph, its component errors are likely to be of mixed algebraic signs.

On the basis of these considerations, I am willing to hazard the generalization that the errors in the inverse caused by errors of

distribution. This is irrelevant to my point, however, for the estimation of the technical coefficients may still be viewed *as if* it consisted of the two components described. The reason for so viewing it is to show how unlikely it is that all elements of A are off in the same direction.

given size in the input-output matrix are probably not as much as an order of magnitude larger than their parent errors in the input-output matrix, and that the resulting percentage errors in the predicted total outputs of the various industries are even less important.

4. CONCLUSION

The fourth type of error, that due to discrepancies between the mathematical structure and the reality it is intended to describe, is properly the subject of section E.

The conclusion of this section on computation and errors, then, is that pure computing errors are of no importance for input-output analysis, and that errors resulting from the cumulation of errors in the input-output coefficients are not likely to be very dangerous.

E. Evaluation of the Worth of Input-Output Analysis

The greatest practical value that an intellectual tool can have is the ability to make good predictions of important facts. Accordingly, this section will be concerned chiefly with the question of how good a tool input-output analysis is, or gives promise of becoming, for predicting important economic magnitudes. The question can be attacked from a theoretical point of view and from an empirical point of view, and the results can and should be compared and evaluated. Thus will the last three parts of this section be occupied. But first, a few remarks on a lower level of aspiration.

1. THE INPUT-OUTPUT TABLE AND THE ANALYTICAL TECHNIQUE

The input-output table, showing the flows of goods and services from each industry to each other industry for a specified period, is to be distinguished from the analysis that is usually associated with it. One may construct an input-output table for 1947 or any other year without having any confidence in "input-output analysis," without making any assumptions about substitution or constant returns to scale or joint products, without having any production function in mind at all. All one needs is an industry classification scheme, a great quantity of data, and time and patience. An input-output table, or a series of them for several years, certainly has value apart from input-output *analysis*, simply as a contribution to our factual knowledge of our economic system. The national income accounts of the Department of Commerce are valuable to many people who perform no mathematical analysis with them, and the same would be true of a series of input-output tables showing not only the totals of final goods and services but also the intermediate flows.

2. THEORETICAL EVALUATION

I have already indicated in section B that input-output analysis must stand condemned from the point of view of accepted economic theory. The "law" in this case is the proposition that the proportions in which inputs are combined in production depend upon the relative prices of the inputs; the charge is that the defendant has openly violated the law by assuming that the input proportions are fixed technologically. Counsel for the defense do not deny that the input-output assumptions are contrary to the law; in fact, they admit it freely.[31] Their defense is based on two other, more pragmatically oriented, claims. One is that, even though substitution among inputs takes place, it is unimportant enough to be ignored in many instances. The other is that an input-output analyst need not really believe in these assumptions, since he can adjust his technical coefficients to take account of any important substitutions that he expects to take place.

The essence of both of these claims is that input-output analysis should be regarded as a first approximation to reality, which is good enough to act upon in some problems, and which can be made good enough to act upon in other problems if appropriate adjustments or corrections are made. Its proponents contend that it is particularly valuable in situations where in a short period of time great changes occur in the amounts of final goods and services produced, such as in wartime.

The issue cannot be resolved on a theoretical plane; it clearly must be appealed to the higher tribunal of empirical evidence.

3. EMPIRICAL EVALUATION

Everyone from Leontief[32] on has agreed that only empirical testing can determine the areas of practical usefulness, if any, of input-output analysis. Let me review, then, the few empirical studies that I have been able to learn about: those of Leontief for 1919 and 1929, of Hoffenberg and the BLS for 1929 to 1937, of Harold J. Barnett for 1950, and of Evans and Hoffenberg and the BLS for 1951.

a. Leontief's test for 1919 and 1929. Leontief[33] took as his problem

[31] See, for example, Leontief, *op. cit.*, pp. 38-39. [32] *Ibid.*, p. 40.

[33] *Ibid.*, pp. 216-218. (Also available in "Recent Developments in the Study of Interindustrial Relationships," *The American Economic Review*, May 1949, pp. 223-225.)

the prediction of the total outputs of each industry in 1919 and in 1929, when the actual final bill of goods for each of those years is known. He compared the results of three methods, using 1939 as a base year for each, with the actual total outputs. First was the input-output method, described by equation (4) of section D2 of this paper. For this he used a 13-by-13 input-output matrix, derived from the 38-by-38 1939 matrix by condensing it. Second was the so-called final demand blowup method, which assumes that for each industry the ratio of total output to final demand is the same in every year as it was in the base year. Thus, it predicts that for any industry the total output in a given year will be the final demand in that year times the total output in the base year divided by the final demand in the base year. Third was the method of GNP blowup, which ignores the distribution of final demand and assumes that for each industry the ratio of its total output to GNP is the same in every year as in the base year. Thus, for any industry it predicts the output in a given year as the GNP in that year times the industry's output in the base year divided by the GNP in the base year. For comparability, Leontief used the same thirteen industries in all three methods. It should be noted that all the given quantities must be converted into the same units before any calculations are made; the units used here were 1939 dollars, with separate price indexes constructed for each of the thirteen industries for 1919 and 1929.

Leontief compares the results of the three methods in terms of the standard error of prediction, which is defined as the square root of the average of the squared differences between predicted and actual total outputs of all thirteen industries, measured in dollars. (The figures for the actual total output and the bill of goods of each industry, of course, are really *estimates* based on both price and output data from the *Census of Manufactures*, the Board of Governors of the Federal Reserve System, etc., and may be in error themselves.) Table 2 shows the outcome: The input-output method has a much smaller standard error for both 1919 and 1929 than the other two methods.

Leontief does not show in his account of this test the actual 13-by-13 matrix he used or the predicted and actual values of the industry outputs. However, he does show a 9-by-9 matrix for 1939, and a similar set of predictions of 1929 outputs based on that, together with the actual 1929 outputs of those nine industries.[34] And

[34] *Ibid.*, pp. 149, 155.

TABLE 2

Standard Errors of Prediction of Three Methods of Predicting Industry Outputs

(billions of 1939 dollars)

	1919	1929
Input-output	.38	.24
Final demand blowup	2.02	1.54
GNP blowup	1.36	1.74

as G. Diran Bodenhorn pointed out,[85] the standard error of the 9-by-9 1929 input-output predictions is $1.65 billion, which is essentially the same as the standard errors reported by Leontief for the two alternative methods.

b. The Hoffenberg-BLS Test, 1929 to 1937. Hoffenberg[36] took as his problem the prediction of the total outputs of each industry in 1929, 1931, 1933, 1935, and 1937, when the actual final bill of goods for each of those years is known. He compared input-output predictions and those of final demand blowup and GNP blowup (using 1939 as the base year) with the actual total outputs, as Leontief did. For his input-output predictions he used the 38-by-38 1939 matrix, but since his actual total output figures for the odd years from 1929 through 1937 cover only 25 of the 38 industries, the analysis of his results here is confined to those 25 industries.

Hoffenberg's comparisons show that the input-output and final demand blowup predictions are approximately equal in quality, neither being good enough to arouse enthusiasm, while the GNP blowup predictions are markedly worse.

One comparison of the three methods is in terms of the simple average of the percentage errors made by each method in each year. For each method, the 125 predictions (there are 25 industries and 5 years) are compared with the actual total outputs, and the percentage errors are computed; these results are shown in Table 3 for the two methods other than the GNP blowup. Then, for each method and each year, the 25 percentage errors from the different

[35] G. Diran Bodenhorn, review of Leontief, *op. cit.*, in *The American Economic Review*, March 1952, pp. 172-173.

[36] Hoffenberg and the BLS undertook an historical study of input-output based on the 1939 table and the odd years from 1929 to 1939, but it has not been finished. Hoffenberg very kindly gave me access to, and permission to quote figures from, the work sheets of this study in the files of the BLS, and it is from them and a few calculations I made thereon that I draw this account.

TABLE 3

1939 Industry Outputs and Percentage Errors of Two Methods of Predicting Industry Outputs, 1929-1937

INDUSTRY	*1939 Output* (*$ Bill.*)	PERCENTAGE ERROR *Input-Output* 1929	1931	1933	1935	1937	Avg.[a]	*Final Demand Blowup* 1929	1931	1933	1935	1937	Avg.[a]
1. Agriculture	$9.8	− 9	−16	−12	−11	−10	10	− 1	− 2	+ 1	− 1	− 3	2
2. Food processing	13.2	+17	+18	+19	− 7	− 7	15	+19	+19	+21	− 7	− 7	15
3. Ferrous metals	2.5	− 2	+32	+ 6	+ 5	− 5	10	+ 6	+39	−16	−10	+ 1	15
4. Iron & Steel foundry	.5	−30	+ 4	+42	+30	−18	25	− 5	−52	+41	+14	−25	25
6. Agric. machinery	.4	+ 6	+39	+80	−12	0	30	+ 9	+26	+36	−13	+ 3	15
8. Motor vehicles	2.5	−13	− 6	− 6	− 7	− 5	8	−10	− 9	−10	− 4	− 2	9
10. Transp. equipment (n.e.c.)	.3	−18	−13			− 5	12	− 9	−23			+ 7	12
13. Mdsg. machinery	.3	+21	+12	+12	+ 2	− 7	11	+26	+ 8	+ 9	+ 4	− 5	10
15. Iron & Steel prod. (n.e.c.)	2.2	+ 6	+11	−14	+ 5	+ 1	7	−50	+34	− 5	− 9	−10	20
16. Nonferrous metals	1.5	− 8	+12	+17	+18	− 1	9	−12	+14	+ 2	+20	− 2	9
17. Minerals (n.e.c.)	2.1	+ 1	+15	+12	+ 1	− 9	8	+ 6	+21	+ 6	− 3	−11	9
18. Petroleum	4.7	+14	+13	− 3	− 1	− 5	7	+ 6	+11	− 3	− 5	− 6	6
19. Coal and Coke	1.7	−15	− 5	− 4	− 3	− 6	7	− 2	+18	+26	+16	0	12
20. Elec. and mfd. gas	2.8	+12	+ 5	+ 3	+ 4	0	5	− 9	+ 6	+ 6	+ 5	− 4	6
21. Communications	1.5	−18	−20	−17	−15	− 8	15	−17	−12	− 9	−14	−11	12
22. Chemicals	3.3	+80	+64	+31	+11	+ 1	35	+81	+71	+39	+11	− 4	40
25. Paper	1.7	+21	+15	+ 2	+ 4	+ 1	9	+ 4	− 3	−19	− 7	− 1	7
26. Printing	2.2	− 3	− 2	+ 1	− 5	− 7	4	− 4	− 4	− 2	− 4	− 7	4
27. Textiles	3.1	+21	+19	− 9	− 1	0	10	+ 9	− 5	−19	−15	0	10
28. Apparel	3.4	+11	+ 3	− 6	−11	−14	9	+11	+ 4	− 6	−11	−15	9
29. Leather	1.0	+36	+24	+ 4	− 1	+ 2	13	+39	+27	+ 5	+ 2	+ 3	15
30. Rubber	.9	+29	+25	− 1	+ 9	+10	15	+67	+61	+16	+11	0	30
31. Manufacturing (n.e.c.)	1.6	+21	+29		+ 7	− 8	15	+24	+28		+ 2	−10	15
33 & 34. Misc. transportation	3.0	+30	+21	+ 3	− 1	− 2	11	+52	+104	+10	+ 5	0	35
35. Steam railways	4.2	−20	−10	− 9	− 6	− 8	11	0	+ 4	− 4	− 3	0	2
Absolute total[b]	$70.4	468	437	317	181	143		478	605	311	196	131	
Algebraic total[b]	70.4	184	285	155	11	−113		340	489	125	20	−103	

[a] Approximate absolute average; rounded to nearest multiple of 5 if over 15 per cent.
[b] Totals for the 25 industries shown.
n.e.c. = not elsewhere classified.
Source: Marvin Hoffenberg and the Bureau of Labor Statistics, an unfinished study.

industries are averaged. Actually, two kinds of average are possible: the average of algebraic percentages, in which positive and negative errors offset each other, and the average of absolute percentages, in which signs are ignored and there is no offsetting. The absolute averages are the better indicators of accuracy.[37] Table 4 shows that, according to the absolute averages, input-output and final demand blowup are about equivalent, each erring by 12 to 14 per cent on the average and by 6 to 7 per cent over the short period from 1935 to 1939, while the GNP blowup is much poorer. The bottom of Table 4 shows the algebraic averages.

TABLE 4

25-Industry Averages of Percentage Errors of Three Methods of Predicting Industry Outputs, 1929-1937

METHOD	1929	1931	1933	1935	1937	1935-1937 Average	1929-1937 Average
				Absolute Averages			
Input-output	18	17	13	7	5	6	12
Final demand	19	24	14	8	5	7	14
GNP	23	26	35	10	10	10	21
				Algebraic Averages			
Input-output	7	11	6	0	−4	−2	5
Final demand	14	20	5	1	−4	−2	7
GNP	1	20	24	1	−8	−4	8

Source: Table 3.

Another comparison is in terms of the total dollar value of the errors of each method, or, equivalently, in terms of a weighted average percentage error using industry outputs as weights. Here again, input-output and the final demand blowup perform about equally well. The calculations to support this statement can be made from Table 3, which shows the 1939 total outputs of each industry in billions of 1939 dollars in addition to the percentage errors. A quick examination of the "average" columns will show that input-output did markedly better in five industries (numbers 3, 15, 19, 30, and 33-34), while the final demand blowup did markedly better in three industries (numbers 1, 6, and 35), but that the dollar value of 1939 output in the five industries where input-output did better is $10.3 billion, while the dollar value of 1939 output in the three

[37] The algebraic averages indicate whether a method is high or low on the average over all industries, while the absolute averages (like standard errors) indicate whether it is good for specific industries.

industries where the final demand blowup did better is $14.4 billion. These are not important differences in performance.

Another comparison is the one used by Leontief, in terms of the standard errors of prediction. For the year 1929, the standard error of both the input-output and the final demand blowup predictions in the Hoffenberg study is $0.58 billion. This compares unfavorably with $0.24 billion, the result reported by Leontief for his unpublished 13-by-13 1939 matrix (see Table 2), but favorably with $1.56 billion, the result obtained from Leontief's published 9-by-9 1939 matrix.

The pattern of magnitudes and signs of errors in Table 3 is worth looking at. The magnitudes are not surprising; in general, the closer a year is to the base year, the smaller its percentage error. This is to be expected, because both the input-output and the final demand blowup methods must work perfectly in the base year, and the fact of continuity (*natura non facit saltum*) in economic affairs insures that errors will be small for periods near the base period, even if they are not for periods far away; given time enough, technology and relative prices can change the input-output ratios. In some cases, the 1931 or 1933 error is greater than the 1929; this is probably due to the depression.

The signs in Table 3 are interesting, however. Except for a few industries (agriculture, motor vehicles, other transportation equipment, coal and coke, communications, and steam railways, representing together about one-fourth of the total output of the 25 industries here included) the errors for the years 1929 to 1935 are predominantly positive, while those for 1937 are almost all negative. Since a positive error is a prediction too high, total production in 19 of the 25 industries here included was overestimated for the period 1929 to 1935 on the basis of actual final demands in those years and the 1939 matrix. In other words, the degree of indirect use of the outputs of these 19 industries, i.e. their use as intermediate products, must have been increasing over the period 1929 to 1939 (skipping 1937). This is to be expected in the case of the chemical industry and others whose products were being put to new and larger-scale uses, replacing other products. But it appears to be a characteristic of the greater part of the economy, and suggests that on the whole production was becoming more indirect. Theoretically, this might have been due to a shift in the composition of final demand from industries having low indirect requirements to industries having high ones. However, there was practically no shift in the industrial composition of final demand between 1929 and 1939 (by far the largest shift was in food processing, which rose from 11 per cent of total final demand in 1929 to 14 per cent in 1939; only one other industry's share changed by as much as 2/3 of 1 per cent). Hence, this trend toward indirect production was apparently characteristic of most of the individual industries.

c. Barnett's Test, 1950. Barnett[38] chose as his problem the prediction of total output of each industry in 1950, not knowing the actual final bill of goods for 1950 but knowing instead a forecast of it prepared in 1947. Like Leontief and Hoffenberg, he made predictions on the basis of input-output analysis, final demand blowup, and GNP blowup. He used the 38-by-38 1939 matrix, and took 1939 as the base year for the other two methods. He made predictions by a fourth method also, namely, regressions for the years 1922 to 1941 and 1946 of the total output of each industry on GNP and time. He compared his predictions with the actual 1950 outputs, and found the results from regressions best, from input-output and final demand blowup next best and about equally good, and from GNP blowup worst. His work, however, depends not only on input-output analysis and the data-preparation process together, as Leontief's and Hoffenberg's did, but also on the final demand projections that were made in 1947. His average input-output errors were typically more than twice as large as Hoffenberg's;[39] two of the possible reasons are that errors may have occurred in projecting the 1950 final demand, or that large shifts may have occurred from 1939 to 1950 in our economy's input-output ratios. It is difficult, if not impossible, to check the latter possibility by comparing the 1939 table with postwar tables, because so many of the definitions and conventions used in preparing the postwar tables are different from those used for the 1939 table.

d. The Evans-Hoffenberg-BLS Tests, 1951. Evans and Hoffenberg and the BLS have undertaken to predict the total 1951 output of each industry with the aid of the actual 1951 final bill of goods, using two different 190-by-190 input-output matrices. One of these is the 1947 matrix "as is," and the other is the 1947 matrix with certain alterations in the coefficients that seem to the BLS staff to be warranted for 1951 in the light of specific industry information. Evans

[38] Harold J. Barnett, "Specific Industry Output Projections," *Long-Range Economic Projection*, Studies in Income and Wealth, Volume Sixteen, Princeton University Press for National Bureau of Economic Research, 1954.

[39] I do not present Barnett's results in detail because his projections are not based on actual final demand.

and Hoffenberg then plan to compare their predictions with the actual total outputs of each industry in 1951. A preliminary version of this work has been completed for the 1947 matrix as is, but it has not been released. The BLS informed me that this is because of its security classification. The 1947 matrix itself is, of course, not classified; the 44-by-44 miniature of it (with the inverse) has been published and widely distributed, and the 190-by-190 matrix and inverse were distributed at this Conference.[40] The apparent reason for the security classification of the results is that the 1951 bill of goods includes a detailed picture of the military bill of goods.[41]

From the point of view of this Conference, it is unfortunate that we do not know the results of the latest test. The others discussed earlier are all based on the 1939 matrix, which the proponents of input-output have abandoned as insufficiently detailed and as inapplicable to the postwar period. Upward of half a million dollars has been spent in obtaining the 1947 matrix,[42] and it represents the best product to date, according to the Division of Interindustry Economics. If the economics profession is to be able to judge fairly the performance of input-output analysis, it must have all the available evidence, and in particular it must have results based on the work that the proponents of input-output analysis regard as their best so far.[43]

[40] See note 13 above.

[41] If the 1951 predicted total outputs for each industry were released, then the bill of goods that was used for making the predictions could very easily be calculated from equation (3). Since the civilian portion of the bill of goods could be fairly accurately surmised, the military portion could be deduced fairly accurately as a residual.

[42] According to a BLS memorandum of Feb. 3, 1949, "Proposed Inter-Industry Relations Study by the BLS," the agency spent about $100,000 to prepare the 1939 table. According to a rough estimate given me informally by the BLS staff, the 1947 matrix cost approximately $600,000, and the annual budget of the program is now about $250,000 (1952). Since then the program has been substantially reduced in scope.

[43] At the close of the Conference, I learned that certain aspects of the preliminary results of the test of the 1947 matrix are *not* classified. The procedure in the preliminary test was as follows. The inverse of the 190-by-190 1947 matrix was applied to a (classified) preliminary 1951 bill of goods expressed in 1947 prices, according to equation (4) above, to obtain the (classified) predicted 1951 total output for each industry. Each predicted output was then compared with the Board of Governors of the Federal Reserve System's measurement of actual 1951 total output for that industry. Both predicted and actual outputs are in terms of 1947 prices. The percentage deviation of the predicted 1951 output of each industry from the actual 1951 output of that industry, as measured by the Board of Governors, was then computed. The *frequency distribution* of these percentage deviations has no security classification. Evans has informed me that the BLS intends to release it after certain studies of it are completed. It is to be hoped that this will be in the near future.

e. On Testing Input-Output. All the tests of input-output analysis so far described have been of the same type. A bill of goods is assumed or known, an input-output matrix is obtained and the appropriate inverse is computed, the total outputs of the various industries required to produce the given final demand are predicted by equation (4) of section D,[44] and the predicted total outputs are compared with the actual. The rationale of the test is straightforward: if the predictions are good, input-output analysis is thereby shown to be a useful tool even though it does violate certain theoretical propositions; if the predictions are bad, input-output analysis is thereby shown to be an impractical tool. But the matter is not this simple. Granted, if the predictions are consistently good, input-output is useful. But on the other side, it would be quite possible for the predictions to be bad even if input-output analysis were a useful tool, if enough error or inconsistency were introduced in preparing the original matrix and all the final demand and total output figures for the prediction year, and converting the latter to base-year prices. Thus, the tests described so far are really tests of the data as well as of the input-output technique, and if they indicate failure, they do not indicate where the cause of the failure lies. For the purpose of finding out whether one can now make good predictions by using input-output analysis, it does not matter where the failure lies. For the purpose of directing future research in economics, it matters a great deal.

The way to localize and eliminate the cause(s) of trouble is to confine tests to a small area, instead of encompassing a wide range of factors. Along these lines, in 1948 a committee of consultants on the problem of what kind of input-output research to undertake offered the following suggestions with respect to strategic areas.[45] First, consideration should be given to production variations that are possible with existing techniques. For example, how much is it possible to alter input-output ratios and increase output in the steel industry by reducing the life of equipment, preparing the coal, altering the charges of the furnaces, using oxygen and pressure, and so on? And again, is it possible to substitute coordinating activities for capital equipment and operating labor in the railways? Second, the known bottlenecks to expansion of output in any and all in-

[44] Sometimes the inverting and the predicting via equation (4) are combined in one step; if the inverse is not going to be used again, this is more economical.

[45] Solomon Fabricant, Edwin George, Robert Landry, and W. Allen Wallis, "Comments on Studies of Inter-Industry Relations," National Security Resources Board, Mimeographed, Nov. 24, 1948, pp. 12-14.

dustries should be catalogued, together with the known ways of trying to eliminate each one, and then the results should be analyzed to find the implications of each for input-output ratios and total output.

Studies of this sort, confined to a particular industry or productive process, can be expected to provide small-scale tests of how well the assumptions of input-output analysis stand up in situations when certain components of final demand are suddenly increased, and can help to indicate the economic stimuli under which such situations may arise, and the kind of corrections that must be applied to the results of input-output analysis in such situations to make them more accurate.

4. CONCLUSION

The preceding appraisals of input-output analysis from theoretical and empirical points of view are clearly inconclusive. The theoretical appraisal cannot be conclusive by itself because the question is one of usefulness in the real world, and the empirical appraisal cannot be conclusive until more evidence is known. I plead for the preparation of data on the final bills of goods and the total outputs of industries for several different years, so that it can be seen how well the 1947 matrix (and any future one) works, not for just one year, but for several years.

In attempting to evaluate input-output, one should apply the dependable old economic principle of considering the available alternatives. The problems for which input-output is called upon are chiefly those of guiding the allocation of resources in wartime, and of guiding the economy so that it enjoys something like full employment and so that investment and resource needs are continuously foreseen. Indirect effects are of great importance here, and hence, a scheme is needed that can follow many changes simultaneously through their ramifications in the economy.

The input-output technique is certainly better than no technique. What are the alternatives? Clearly, a real general equilibrium system would be the best, barring cost, but that is not a realistic alternative. Clearly, linear programing would be better than input-output if the relevant data were developed to the same degree, for it would have all the advantages of input-output plus the advantage of being able to deal automatically with substitution among inputs. It is not a practical alternative at present, even though it may become one. I believe that input-output is the best technique now available for handling problems that require a picture of the production function of the entire economy, and that its results can serve as first approximations from which to start making corrections where special information permits or experience demands.

A disadvantage of both input-output and linear programing as practical techniques is that they require very large staffs and fairly rigid coordination of research activities to fit the framework of the particular industry definitions used and the conventions adopted for handling the problems mentioned in section C. The result of this is likely to be a centralization of research in one or a few coordinated programs. However, it is possible to publish procedures and results and to farm out projects to small research groups, so that many economists can know what is being done and so that the decisions that do have to be made uniformly for a whole project can be made after a maximum of interchange of ideas. The current input-output research program is being handled fairly well in this respect. Whatever residue of centralization remains is the price that must be paid for the undertaking of such large projects. On the brighter side, there is little danger that input-output and similar large-scale techniques will crowd out all other economic research, because far from all our problems are economy-wide in character.

Another question about input-output and linear programing that may come up in our complex world of uncertain freedom is what effect they will have on the prospects for a free society. They are tools that can easily be turned to use in establishing direct controls in increasing scope and force, and therefore may constitute a danger. However, they *are* tools, and as such are neutral, and I believe they can and should be used on the side of a free society. "Indeed, the ability to predict, with reasonable accuracy and in reasonable detail, the consequences of a given *general* policy [italics added] may be precisely what is necessary to make possible the accomplishment of socially desirable economic objectives *without* resort to regimentation."[46]

COMMENT

MILTON FRIEDMAN, *University of Chicago*

I find myself in substantial agreement with the admirable paper by Carl Christ that has been assigned to me to discuss, so I shall comment on some general issues in input-output analysis rather than

[46] Loring Wood, "Comments on the Inter-Industry Relations Technique (with particular reference to the report of the consultants)," National Security Resources Board, Mimeographed, February 1949, p. 5.

attempt a detailed criticism of the paper. In the process, I shall indicate a few minor disagreements.

Like Christ, I want to emphasize at the outset the distinction between the input-output *table*, regarded as a statistical description of certain features of the economy, and input-output *analysis*, regarded as a means of predicting the consequences of changes in underlying circumstances. The table is clearly an extremely useful and ingenious construction that can be regarded as a natural extension of national income accounts. As such, its value and usefulness are largely independent of the success of input-output analysis as a predictive technique. Any dispute about the table is only whether it is worth its cost, not whether it is worth having at all. This unanimity about the value of the table explains why the rest of my comment, like the major part of Christ's paper, is devoted to input-output analysis; it is here that controversy rages.

Viewed as a predictive device, input-output analysis specifies a method of predicting the total output of a series of industries from the so-called "final-demand schedule," or "bill of goods." Its actual use to forecast total output for a future year involves, first, forecasting final demand, then predicting total output from this final demand; any error in the forecast can, in principle, be separated into (1) the error in the forecast of final demand and (2) the error in the conditional prediction of total output, given final demand. In judging the analytical validity of input-output analysis, only the accuracy of the conditional prediction should be taken into account, for errors in forecasting final demand cannot be attributed to defects of input-output analysis. True, in using the method to make actual forecasts, even extremely accurate conditional predictions may be of little value if the forecasts of final demand are very bad. But that is a separate issue, certainly at this stage of development, when the analytical validity of the input-output technique is by no means established. On this point, I share the view expressed by Christ.

The central feature of input-output analysis as a predictive device is, as has been repeatedly emphasized, that it proceeds to make predictions *as if* all coefficients of production were fixed, *as if*, in each defined industry, the amount of each input per unit of output were rigorously fixed, regardless of relative prices, levels of output, and so on. Now, it is obvious that coefficients of production are not rigorously fixed, that all sorts of variations are possible and do occur. But it is a mistake to suppose that the lack of descriptive realism of fixed coefficients of production is by itself an objection to input-output analysis. Quite on the contrary, *if* input-output analysis worked, *if* it yielded good predictions, the fact that it neglects changes in production coefficients would be a decided advantage, for it would greatly simplify the making of predictions by making it unnecessary to take such changes into account. Indeed, the fact that it does hold out the hope of being able to neglect changes in coefficients of production that are known to occur is precisely the reason why there is such extensive interest in input-output analysis.

The crucial question is not whether coefficients of production are, in a descriptive sense, rigorously fixed—quite obviously they are not—but whether treating them as if they were yields good predictions; whether, that is, treating them as if they were involves neglecting factors that are only "minor" disturbances or involves throwing the baby out with the bath water. It is sometimes argued that, if predictions that neglect changes in coefficients of production turn out to be bad, this "difficulty" can be overcome by complicating the analysis, for example, by introducing changes (generally by unspecified methods) into the entries in the input-output table before using it for prediction, or by substituting linear programing models. This seems to me highly misleading: substituting linear programing or the other devices is a retreat, not the surmounting of an obstacle; it means that this particular simple hypothesis has been a failure, and that we are therefore forced to turn to more complex—and by the same token less useful—models, or to try to achieve simplicity in some other way.

But how can we judge whether treating the coefficients of production as fixed is likely to yield good predictions—by which, of course, we mean better predictions than alternative methods that are less or no more costly? As I see it, there are only two ways in which we can form such a judgment: indirectly, from experience with the use of similar assumptions in economic theory in general or in related contexts in particular; directly, by trying out this method in particular problems of the kind for which we hope to use it.

The indirect evidence does not, I believe, justify any confidence that input-output analysis will work; rather, it suggests that it will not. Except for a limited class of extremely short-run problems for which Marshall's strict joint-demand analysis has been useful, I know of no examples in economics in which it has turned out to be acceptable to neglect changes in the coefficients of production, especially when what is in question are average coefficients of production for aggregates of the elementary technical units, rather than for the latter themselves. This negative indirect evidence explains, I believe, both the tendency for almost all "outside" or "independent" economists who have carefully examined input-output analysis to be highly skeptical of its usefulness, and the apparently greater readiness of noneconomists than of economists to have confidence in advance that it will work.

Of course, the inventors and innovators of input-output analysis will not accept this negative judgment. And it is all to the good that they should not. It is natural for a parent to overstate the merits of his progeny, and, indeed, in this day and age of high-pressure advertising and loose use of superlatives, he must do so if his progeny is to get his just due. Some of us may feel that, in this particular case, the claims have been pitched rather high and based on rather little, but there is clearly much room for sharp differences of opinion on such questions. The important thing is that indirect evidence cannot be regarded as conclusive, that many inventors have seen their brain children scorned by others who regarded themselves as competent to judge, and that strong faith despite the negative opinion of others has been an important ingredient in many great advances. Given, therefore, able men who have faith that input-output analysis will work, it behooves the rest of us to withhold final judgment until direct evidence is available, and, moreover, to urge that such direct evidence be gathered and made available.

But in urging that direct evidence be obtained, I feel I should enter a qualification to avoid misunderstanding. I do not mean to be expressing agreement with, or support for, the present enormously extensive program of interindustry research as a means of providing such evidence. It seems to me fantastic that input-output analysis should have gone almost directly from the drawing board to full-scale production with almost no pilot production, and certainly no outstandingly successful pilot production. The kinds of tests that would be justified are small-scale tests for part of the economy, crude breakdowns, and the like—not the expenditure of hundreds of thousands, and perhaps millions, of dollars a year.

But whether justified or not, such a full-scale program is underway, and it was my hope that it would have gone far enough so that at this Conference reasonably direct evidence would be available on a large enough scale to permit a fairly definitive judgment as to whether input-output analysis does or does not yield better predictions than equally simple or simpler alternative theories. Unfortunately, this hope has not been fulfilled. The proof of this pudding is in the eating, and we still have very little to eat. And what we have is not, I fear, very digestible.

The proponents of input-output analysis are inclined to pooh-pooh tests based on earlier input-output tables because they think their brand-new de luxe model is so much superior. But in default of evidence for the new model, we must base our judgment on what we have. Christ has summarized the various tests that have been made on the basis of earlier input-output tables. These tests are almost wholly negative; they reveal no systematic tendency for input-output analysis to yield better predictions than other vastly simpler methods. Perhaps further evidence will reverse these results; until it does, input-output analysis must be regarded as an hypothesis that has been contradicted by the data so far available.

In judging the accuracy of input-output predictions, I should like to urge two points of detail: (1) that percentage errors be based not on total output for an industry, but on the difference between total output for that industry and the part of its output classified as final demand, for it is this difference, and this alone, that is predicted by any of the various methods; (2) that an even simpler naïve model be used for comparison purposes than those implied in the final demand blowup, GNP blowup, or regression methods, namely, estimating the excess of total output over final demand in each industry in the year in question by applying to the excess in all industries the percentage distribution of the excess among industries in the closest preceding year for which the data are available.

Against my highly skeptical and negative position, it will be argued, as indeed Christ does argue, that "the input-output technique is certainly better than no technique"; that it "is the best technique now available for handling certain problems"; that we must make predictions of the kind it claims to be able to make, and that we have no other way to make them. I find it hard to understand or to make sense out of such statements. If input-output analysis does no better than "naïve models," then these naïve models are "other" and equally good techniques. And what does it mean to say we "must" do something we cannot do? If input-output analysis gives no better results than we can get by the equivalent of tossing a coin, and if, indeed, it is the "best technique," then the simple fact is that we cannot, in any meaningful sense, make predictions of this kind with the present state of our knowledge. Willy-nilly, then, we "must" do without them.

Rather than closing our eyes to this necessity, pretending that we can do what we cannot do, and adopting techniques of administering and organizing economic mobilization whose success depends critically on having accurate predictions that we cannot make, it would seem far more reasonable to develop and use techniques that do not depend for their success on such predictions. Even if we could make, with reasonable accuracy, the kind of predictions that input-output analysis is designed to make, I would myself prefer, on many grounds, to rely primarily on the price system, rather than on detailed physical planning for organizing the use of our resources, whether for peacetime purposes, defense mobilization, or total war. This is unquestionably a minority view. But if the predictions required to make physical planning at all efficient cannot in fact be made, is it not incumbent upon the majority to take account of this fact, and cut their coat to the cloth available?

One final comment is suggested to me by reading the papers presented to this Conference and listening to the discussion of them. I am greatly impressed by the amount of work done under the guise of interindustry research the value of which is completely independent of the success of input-output analysis as a whole. Let a large group of able, imaginative, and hard-working people be set to work on any project, no matter what it is, and no matter how ill-advised or well-advised, how successful or unsuccessful as a whole, and a large number of important and unintended by-products will emerge. This seems to me the case with the interindustry-research program.

I venture the prediction that contributions made through this program to the understanding of particular industries, of the process of technological change, and like matters, which at this stage seem like unimportant by-products, will turn out to be the lasting and important contributions of the program to economic knowledge; that they will be with us long after the grandiose dreams of predicting by input-output analysis the detailed consequences of major changes in the economic environment have been abandoned.

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This commentary was presented in part at the 1952 Conference on Research in Income and Wealth as a discussion of Ronald Shephard's "A Survey of Input-Output Research."[1] The original discussion has

[1] Shephard's paper was prepared in July 1952 under the auspices of the Rand Corporation (Paper *P-309*) as a review of various phases of the input-output program. Since this paper was both a review of W. W. Leontief's second edition of *The Structure of American Economy, 1919-1939*, 2nd ed., Oxford,

been broadened somewhat to include comments on the work of other reviewers of input-output research, particularly the paper presented in this volume by Carl Christ, "A Review of Input-Output Analysis."

Addressing myself for the moment to the articles of both Christ and Shephard and not venturing to judge whether Wassily Leontief would agree that the discussions of his work and his theories (particularly the review of his book by Shephard) are the best possible or even whether they are fully adequate, I believe that a student in the field of interindustry economics research would benefit greatly from these generally objective presentations of both Leontief's pathbreaking findings and other more recent work in this and related fields. There are all too few such surveys presenting an integrated view of the many diverse activities that are all part of input-output research.[2] The economist not fully acquainted with the data activities in this field, and certainly the data gatherer also, would benefit from these critical summaries of the work being done by the numerous organizational groups concerned with the making and using of input-output charts and related interindustry models.[3]

The following discussion is aimed primarily at those aspects of Christ's and Shephard's papers referring specifically or relating closely to the work of the Bureau of Labor Statistics or to the broader federal government interagency program, which includes the BLS interindustry-relations work. To the extent that these papers lead to points discussed in Rutledge Vining's paper or other papers either presented or referred to at the Conference, this discussant

1951, and a survey of government and outside agency work in interindustry economics and linear programing, it was included in the program of the 1952 Conference on Research in Income and Wealth. This author was asked to review this paper, particularly from the viewpoint of Shephard's discussion of the BLS program. Shephard's paper was not included in this volume because it was expected that it would be published in a professional journal. At the time this volume went to press it had not, however, appeared. A very limited supply of the original (unrevised) article is available at the Rand Corporation in Santa Monica, California.

[2] Throughout this paper, as is generally the case in this volume, the words "input-output" and "interindustry" or "interindustry relations" are used interchangeably. Specifically, however, the term "interindustry economics research" is often used to designate the interagency research program in this area undertaken by federal agencies in recent years.

[3] Christ's paper goes into these matters in a general fashion with only limited discussion of specific facets of the program. Shephard discusses each of the agency programs in substantial detail, and relates the objectives of each program and his criticisms to the entire area of research.

will extend or generalize his remarks to include reference to these others.

An evident difficulty of many reviewers of the input-output field has been their inability to secure an intimate understanding of the enormous volume of data collected (in terms of operational significance and conceptual framework) as well as the large body of associated theoretical work that has been developed in the past few years. This shows up in misstatements that often appear in otherwise scholarly presentations. The difficulty is understandable, for no single person can possibly be intimately familiar with all the ramifications of the numerous programs that have recently engaged the attention of so many different groups in the field. I think that Christ and Shephard have done exceedingly well in encompassing as much as they did in the short time each concentrated on his review of input-output research. Nevertheless, I wish to show one of these misconstructions, for, in addition to straightening out the record, it illustrates the kind of problem faced in such research. There is no point in presenting other such statements, for they are not of great significance.

Shephard states in his original paper,[4] "The construction industry . . . is a hodge-podge of different kinds of production activity. . . . A minimum desideratum . . . is the subdivision of maintenance-repair and new construction into separate industries, but data on the inputs to the many different structural types are not available and a refined breakdown of the construction industry is not statistically feasible." This statement is correct only to the extent that it indicates the great difficulties entailed in securing definitive inputs for each of the different construction activities. In actuality, BLS has defined twenty-six separate construction sectors for use in the interagency emergency model, and has detailed data for even finer breakdowns. Perhaps the fairest statement that could be made is that the materials expenditure data for the various maintenance sectors were generally weak in quality, necessitating, in many instances, use of material gathered specifically for one sector as a guide for estimating parameters applicable to other sectors.

Various reviewers discuss the work of Leontief and his Harvard group, of W. Duane Evans and the BLS group, and that of other agencies as if there were no distinction among them. To the contrary,

[4] It is only fair to indicate that Shephard's paper was very preliminary in the sense that he had no chance to submit it to others for review and appraisal, or for specific consideration of certain points that may have been incorrectly stated.

it is quite important to distinguish clearly among their separate activities. Vining, for example, speaks of apparent inconsistencies between Evans and Leontief in their statements concerning the state of readiness of interindustry work. He indicates that Evans talks about this new research tool as being currently available, whereas Leontief is apparently more cautious and talks about its future availability after much more data refinement and consideration of many additional problems. These statements are actually not at all inconsistent, for Evans refers generally to static models, such as those presented in the past that are based on the 1939 table and similar ones being considered for the present 1947 table. Leontief's discussion about lack of "requisite" or "pertinent" or "enough" information refers almost entirely to refinements necessary for dynamic uses of interindustry models. As noted in the Duesenberry-Grosse paper prepared for this Conference, the data stages for capital coefficients, lead times, etc., are well behind the data stages for structural coefficients. I believe that in the papers of Shephard[5] and Christ these aspects are presented more properly. For instance, the Harvard project and the BLS work are clearly differentiated. However, there is a slight confusion in Shephard's work as to the basic BLS research program and the special data developed for the so-called emergency model. One should not conclude from the decisions made in constructing the emergency model that the same approach must always be applied to essentially static models. Apparently, both Shephard and Christ thought the emergency model was the basic project of the BLS, for they both indicate that the BLS is "now working on developing more detailed information, possibly on a 500-industry basis." This is the wrong impression, for in reality the original BLS data were developed on a 500-industry basis (which Shephard so indicates in his last section) and have since appeared in, and have been applied in the form of, summary presentations, such as the 50-sector charts published in 1951 and the 200-order charts shown at this Conference. It is only because of the difficult presentation problems associated with a 500-sector chart that a 200-sector chart, corresponding with the emergency-model classifications, was presented instead.

The availability of 500-industry detail resulted from a decision early in the 1947 interindustry-relations project that, since it was in fact more efficient to carry on individual studies on a detailed rather

[5] Copies of this paper, presented at the Conference but not published, are available at the Harvard Economic Research Project.

than an aggregative basis (essentially 4-digit SIC rather than 3-digit or less), there would be a conscious attempt to keep industry identity on a detailed basis. In general, the kinds of data available determined the level of aggregation. Various additional criteria, such as similarity of input structure, were used in a limited number of cases. The basic consideration was to employ a classification system that would be flexible enough to permit later combinations for specific purposes. The type of problem to be considered with the help of an interindustry model would determine the logical combinations. The interagency emergency model, with some 200 sectors defined, is an example of a special-purpose model aimed primarily at industrial mobilization aspects of alternative defense programs.

Reviewers of interindustry work invariably bring out the point that proponents (and onlookers) present the input-output technique as a powerful new research tool with almost unlimited horizons for application. They further observe that there has been no satisfactory proof of the power of this technique for problems for which it seems most suited, much less for the host of problems, which seem more amenable to solution by other analytical techniques. The comparison standard of the reviewers seems to be the same—whether input-output techniques are better than others for purposes of forecasting. In trying to make comparisons, the reviewers discuss the few tests that have been made, all based on the 1939 matrix, and decide that the balance is not clearly in favor of input-output over other techniques, and in fact possibly the other way round, although there is substantial doubt that any positive conclusion can yet be made.

Deferring the question of whether input-output analysis stands or falls on its predictive power alone, I think it necessary to examine the tests referred to by the various reviewers and decide whether their apprehension is warranted. Barnett's test presented to this Conference in 1951[6] compared input-output results with results from the use of regression methods and certain "naïve" methods. He found no conclusive answer, for too much depended upon such factors as what production indexes should be used for comparison, whether sector results should be given different weights in getting a combined result, whether base- or current-period weights should be used, whether the mean square of the squared deviations or the

[6] Harold Barnett, "Specific Industry Output Projections," *Long-Range Economic Projection*, Studies in Income and Wealth, Volume Sixteen, Princeton University Press for National Bureau of Economic Research, 1954.

average of absolute deviations from "actual" should be used as the yearly statistic, etc. Selma Arrow (Rand paper *P*-239, April 1951) used testing methods essentially similar to Barnett's, but she used estimated "actual" bills of goods for the odd years between 1929 and 1939 (*Census of Manufactures* years) and applied them to the 1939 matrix of coefficients. She, too, arrived at no conclusive answers, but at least her methods were not subject to a criticism that has been applied to Barnett's; he used, for the input-output model, a bill of goods developed for an essentially different purpose, i.e. that developed for the study "Full Employment Patterns, 1950,"[7] a study not at all intended as a forecast of the actual 1950 situation.

These tests, and one or two others, which have not been published, were all based on a matrix (that for 1939) that was preliminary, admittedly incomplete, and very condensed. Even if proper tests could be applied (and I am certain that Christ, Shephard, and the others would agree that those mentioned above are not fully satisfactory), it is doubtful that the 1939 matrix would show very dramatic results, either good or bad. It is expected that the 1947 matrix will provide better answers, but the problem of developing proper tests remains. These tests will have to encompass the entire apparatus—bill of goods development as well as coefficient determination.

At this time, I wish to emphasize a point made in the Evans-Hoffenberg paper prepared for this Conference that the existence of input-output charts does not automatically imply use for prediction purposes alone, although there are predictive elements in each application. Section D of that paper, which discusses the matter fully, helps to place a proper focus on the relation of input-output analysis to forecast models. Generally, the predictive elements in the many models that can be drawn are of two types, discussed in the following paragraphs.

First, assumptions are made about the economy, usually in very general terms; then, bills of goods consistent with these assumptions are prepared. The predictive aspect here is evident when the complete bill of goods is prepared, for the analyst must usually apply forecasting methods to approximate those areas not determined specifically by the initial assumptions. These assumptions will, of course, condition some of the stipulations; in some instances, the necessity for forecasting will be entirely eliminated. Those parts

[7] By J. Cornfield, W. Duane Evans, and Marvin Hoffenberg, *Monthly Labor Review*, February and March 1947.

of the bill of goods that have to be projected in a predictive sense (rather than a prior stipulation sense) may be estimated by several methods—the analyst may insert his intuitive judgment as to what the nonspecified world will be like during the years under consideration, or he may use a more formal mechanism than intuition for making these decisions.

The second element of prediction is associated with the structural coefficients used in the mathematical model. These coefficients, some of which may have been adjusted to reflect current and future conditions, are the analyst's estimates of the determining factors in the economy that will react best to the specified independent variables. These coefficients may be a conglomerate of base-year relationships, the current situation, or predicted structures, but *in toto* they represent the belief of the analyst that these are the best ones consistent with his state of knowledge and the assumptions explicit in the model.

The problem of testing these predictive models is then one of testing the various predictive elements included in the mechanism. If they can be properly tested all at once, much the better, but I doubt that the present condition of economic statistics is suitable for such a task. For example, before production indexes of the Board of Governors of the Federal Reserve System can be used to test production levels implied by an input-output model, it is necessary to determine whether they are proper indicators. The question of suitable price indexes is a most difficult one, and one that cannot be divorced from the general testing problem. A host of other questions too numerous to mention also come to mind.

Possibly the most relevant general question at this time is whether this technique (not including dynamic extensions) offers improvements over existing methods for solving important problems that are essentially economy-wide in scope. At present, interindustry analysis seems to be the only large-scale method for simultaneously measuring total effects, direct and indirect, upon the economy of specified requirements in a consistent (balanced) fashion. This type of problem, then, seems to be properly within the sphere of input-output analytical techniques. The relevant additional question then becomes: For what further problems is such an approach more desirable than others? Certainly, this costly apparatus should not be applied to relatively small problems that do not justify the time or effort, nor should it be applied to other problems that could be solved better by direct means, and for which satisfactory established methods already exist. There is, unfortunately, no clear-cut answer to which remaining problems are suitable for this approach. It is hoped that the ensuing months will provide additional understanding of the system and its full range of application, but pending that, and possibly depending upon it, the next step is to determine how suitable the technique has been for the larger-scale problems for which it has been and is being used. This is important in itself, for similar problems are being considered for the near future.

It may seem to beg the question to say that no good answers are available. They will not become available until a detailed testing program has been applied by those intimately familiar with the detailed workings of the model and the data within it. The program will necessarily have many facets, for the test will not be of a machine as a whole, since all of its working parts are of importance in themselves alone. Thus, many things need testing, and not, for instance, only the implied gross national product in a certain model. To some extent, this testing program is under way, but it can use help. It is hoped that the members of this Conference will provide suggestions that can lead to better tests or that can stimulate their development.

Turning to the more specific points of the various critics, it is evident that they have strong doubts that input coefficients can be changed to fit more properly a given analytical situation. The criticism is generally made that substitution possibilities, changes in scale, and technological innovations tend to render these coefficients invalid. Individuals working in the Interindustry Relations Program agree wholeheartedly that in our complex economy there are numerous factors tending to bring about alterations in the transactions relations among sectors. There are, however, differences of opinion as to the extent that appropriate allowances can be made for these forces, and the relative importance of the resulting changes in the total structure. These differences can be resolved only by additional empirical research, which can be substantially implemented by more current data resulting from a positive program in that direction.

The writers at times seem to suggest that the necessity for revising coefficients for interindustry analysis is a disadvantage and a distinct drawback to use of the system. At the same time, by indirection, it seems that they believe that other methods of analysis can make allowances for these conditions in their parametric system. No method of analysis will take account of such effects, unless specific allowances are introduced in the apparatus. The input-output technique actually provides an advantage in this regard over other methods, since it readily permits the introduction of revised coefficients. Other methods often have no means of introducing such revisions in parameters, or the actual incorporation of them may be extremely difficult.

No method of solving problems by complete models, or even by partial systems, can really give positive answers to the question of how far one must go in anticipating the future. You put into a system what you feel most strongly belongs. If you have improved information, your answers improve. The advantage of input-output techniques is that this improved information can be used simply, completely, and with logical consistency.

Furthermore, there is continued criticism of the fact that, even without passing judgment on the current quality of input-output studies, the results are so long in coming that they are out of date before they can be used. It is true that the problem is a difficult one, but it is certainly not insoluble. Once a basic chart has been developed for a nonabnormal year, and it has been refined to give the best possible chart that available funds can produce, it is a much simpler matter to bring other charts into up-to-date terms. Once such a program has been clearly proposed on a continuing basis, it is conceivable that concurrent programs in other agencies could provide basic data for revision of inputs and development of control totals. The current Annual Survey of Manufactures of the Bureau of the Census is an excellent base for this sort of work.

In general, it seems that an enlightened interest in input-output research should lead to more ready availability of better data, so that the quality of relationships among data may rapidly improve. Assuming this happy state of affairs, it may be possible to incorporate more than porportionality into the parameters describing technical relationships. It may also be possible to achieve functional relations through time series of coefficients, so as to develop insight into the accuracy of the proportionality assumption now widely in use and the kind of projections that should be made to replace it, whenever necessary. For example, the temporal drift of materials-use relationships may help to provide limits within which the proportionality assumption is satisfactory. In any event, it seems reasonable that the economic world should not condemn an infant without giving it a fair chance to develop. Christ's attitude on this point, as expressed in his closing paragraphs, is, I think, a reasonable and fair one. I know from personal discussions that Shephard's viewpoint is essentially the same.