Explanation and Prediction in Economics

The basic statements of economics may serve to explain the past but not to predict the future.

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Economics, in contrast to the other social sciences (certain branches of psychology exempted), consists of fairly elaborate deductive systems and relies, therefore, on formal logical-mathematical methods to a much greater extent than they do. Typically, empirical evidence is brought to bear on its constituent statements in a fairly complex manner which is akin to procedures in the natural sciences. This notwithstanding, economics shares with the other social sciences certain fundamental characteristics which set it apart from the natural sciences. These differentiating characteristics become evident primarily at the level of interpretation of the formal systems employed by the economist; in other words, they have to do with the manner in which, and the extent to which, the constituent statements of economics gain empirical content.

A deductive system consists of a calculus side by side with an interpretation of its terms. A calculus is a collection of symbols along with a set of rules for their manipulation. Questions of meaning and therefore of truth or falsity do not arise in the context of a calculus. The calculus is exclusively a device for transforming sequences of symbols according to the rules laid down by the manipulator. When the calculus is coupled with an interpretation of its terms (that is, with a set of rules establishing the meaning of its terms) it becomes a deductive system. In certain deductive systems the rules of interpretation are sufficient to establish the truth or falsity of their constituent statements. Such deductive systems are called *pure,* while their statements are called *L-determinate.*

Logic and mathematics are pure deductive systems. Consider, for instance, the statement "if the cat is black then the cat is black." The interpretation given to the logical connective "if . . . then" makes the statement true independently of any reference to empirical data. On the other hand, the statement "the cat is black" cannot be assigned a truth value (that is, called true or false) on the basis of the rules of interpretation in the relevant deductive system. Such a statement, then, is *non-L-determinate.* The assignment of a truth value to non-L-determinate statements requires a rule of disposition by reference to empirical data. Non-L-determinate statements for which a rule of disposition by reference to empirical data has been formulated are called "factual" statements, and the deductive systems in which they appear are called *applied.*

It should be clear, of course, that economics does not consist of pure deductive systems (*1*). If it did, it would be a branch of logic or mathematics. This does not necessarily imply, however, that it consists of applied deductive systems. Insofar as its deductive systems contain non-L-determinate statements, it is incumbent upon the economist to evolve disposition rules for them by reference to empirical data, thus spelling out the conditions under which they will be accepted or rejected on the basis of evidence. To be sure, it is not necessary to develop a disposition rule for *every* non-L-determinate statement in a deductive system. It is sufficient that the economist establish a rule of disposition for at least *some* of the statements. A deductive system is given meaning as whole, not piece by piece.

A Sample Economic Model

In order to develop some appreciation for the problems that confront the economist in his attempt to formulate empirical hypotheses, it is worth our while to consider a rather simple "model" of national income determination. The "model" is an oversimplified version of fairly standard, textbook-type constructions dealing with the determination of national income and other related aggregates. We begin by writing down a system of equations (postponing for the time being some basic questions concerning the deductive system in which these equations appear).

$$\begin{aligned} &1.\ Y = C + I + G \\ &2.\ Z = Y - T \\ &3.\ C = a + bZ \\ &4.\ I = u + vY \\ &5.\ T = rY \end{aligned} \qquad (1)$$

The system consists of five equations in six variables (Y, C, I, G, Z, and T) and five structural parameters—undetermined constants (a, b, u, v, and r) which fix the form of equations 3, 4, and 5. Y stands for the dollar value of national income, C for aggregate expenditures of households on consumption, I for aggregate expenditures of business firms on capital expansion (investment), G for aggregate expenditures on the part of government on goods and services, T for aggregate receipts from an income tax, and Z for disposable income (that is, the difference between national income and income tax receipts). Equation 1 provides a definition of national income; equation 2 defines disposable income; equation 3 incorporates a hypothesis concerning the consumption behavior of households; equation 4 incorporates a hypothesis concerning the investment behavior of business firms; and, finally, equation 5 gives expression to the income tax law.

The fact that we have six variables and only five equations is not accidental. The presence of "extra" variables in the system is part of the economist's strategy, as is argued subsequently in this section. Assuming that the values of the structural parameters (a, b, u, v, and r) are appropriately restricted, so that the system satisfies the standard criteria of independence and consistency, we may solve for the values of five of the six variables in terms of the values of one. The variables whose values are deter-

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mined within the system are called *endogenous.* The variable to which arbitrary values may be assigned is called *exogenous.* The selection of the variable destined to play the role of exogenous variable is not completely arbitrary. The characterization of a variable as exogenous implies that its value is set independently of the values assumed by the endogenous variables. In our simplified "model," G qualifies for selection as the exogenous variable.

The solution of the system of equations takes the form of five reduced-form equations which give the systemic or solution values of the endogenous variables in terms of the values assigned to the exogenous variable. Thus, for instance,

$$Y = \frac{a+u}{1-[b(1-r)+v]} + G\frac{1}{1-[b(1-r)+v]} \qquad (2)$$

is the reduced form equation for endogenous variable Y, expressing the solution value of Y in terms of G.

Typically the economist does not provide us with quantitative information concerning the values of structural parameters. He is satisfied in merely characterizing their scope or range of variation. Usually, but not universally, these restrictions on the permissible values of the parameters are derived from elaborate "models" concerning the behavior of economic agents (such as households, firms, and government) or from "models" of the functioning of the market mechanism. *Econometricians,* in contrast to general economists, are concerned with providing statistical estimates of the values of the structural parameters. Our discussion here is restricted to the activities of the general economist. The question arises then: Of what use are these constructions if they do not provide us with quantitative information? The answer to this question is the key to much of what economists do.

Let us differentiate expression 2 with respect to G. The result is

$$\frac{dY}{dG} = \frac{1}{1-[b(1-r)+v]} \qquad (3)$$

This expression, known as the income *multiplier* of government expenditures (for the "model" in question), gives us the instantaneous rate of change of the solution value of Y with respect to G. The general economist is primarily concerned with its *sign.* In this instance, its sign depends on the sign of the denominator, $1-[b(1-r)+v]$. We know that the denominator cannot be equal to zero, for otherwise the system of equations has no solution. If $b(1-r)+v > 1$, then the multiplier is negative; if $b(1-r)+v < 1$, then the multiplier is positive. As was indicated above, the economist may be able to argue, on general grounds, that $b(1-r)+v < 1$, and therefore the multiplier is positive. As a matter of fact, considerations of the stability of equilibrium (this concept is discussed in the section on augmented models) force the economist to assert that $b(1-r)+v < 1$. They "force" him in the sense that the whole procedure—including the derivation of the multiplier—would become meaningless unless stability of the system were assumed. We may conclude, therefore, that an increase (decrease) in government expenditures will lead to an increase (decrease) in national income. This may or may not be an interesting conclusion depending on how seriously one takes the "model" in question.

This type of deductive inference which leads to a statement concerning the direction of change of an endogenous variable with respect to the direction of change of an exogenous variable is typical of the branch of economics which is identified as *qualitative economics.* The discussion in this paper is restricted to issues arising in the practice of qualitative economics.

Basic Statements of the Model

The five equations of our "model" are presumably embedded in a deductive system. Equations as such are not statements; they are what logicians call *open sentences,* true for some substitutions for the variables, false for others. For instance, the equation $y=2x$ is true for the substitutions $x=1, y=2$; it is false for the substitutions $x=1, y=1$. In order to convert an open sentence into a statement we must *close* the sentence by appropriate quantification. For instance, we could say "there exists a pair of numbers (x, y) such that $y=2x$." This is a statement, and furthermore it happens to be true. The expression "there exists . . ." is called by logicians the *existential quantifier.* Consider now the equation $x+y=y+x$. In this instance, the open sentence may be closed by means of a *universal quantifier*—for we can say, "for all pairs of numbers (x, y), $x+y=y+x$." This, again, is a statement.

In order to understand how the equations of our simple system are to be quantified, we must introduce one more technical concept, namely the concept of *relation.* Consider again the equation $y=2x$. For some purposes we wish to characterize in a summary way the set or collection of all the points (x, y) which satisfy this equation. This collection is a relation. Symbolically,

$$R = [(x,y)\,|y=2x] \qquad (4)$$

In words, R is the collection of all the pairs of the form (x, y) which satisfy the equation $y=2x$. (R corresponds to the graph of a straight line through the origin with slope equal to 2.) Equations, then, characterize relations.

We are in a position now to write down the relations characterized by our five-equation system.

$$\begin{aligned}
F_1 &= [(Y, I, C, Z, T, G)\,|Y = C + I + G] \\
F_2 &= [(Y, I, C, Z, T, G)\,|Z = Y - T] \\
F_3 &= [(Y, I, C, Z, T, G)\,|C = a + bZ] \\
F_4 &= [(Y, I, C, Z, T, G)\,|I := u + vY] \\
F_5 &= [(Y, I, C, Z, T, G)\,|T = rY] \qquad (5)
\end{aligned}$$

In order to simplify the notation, let us replace (Y, I, C, Z, T, G) by x. It should be clear that if we argue that x satisfies some equation in our system, it must be an element of or belong to the relation characterized by that equation. Thus our five equations may be replaced by five open sentences of the following form:

1. $x \varepsilon F_1$
2. $x \varepsilon F_2$
3. $x \varepsilon F_3$
4. $x \varepsilon F_4$
5. $x \varepsilon F_5$ (6)

where ε means "is an element of" or "belongs to." These are open sentences because for some values of x the sentences become true, while for others they become false. At this point we require appropriate quantification—somehow these sentences must be closed in order to make them into statements (which are either true of false).

The economist would like to argue that, if values of the variables (Y, I, C, Z, T, and G), or simply x, are observed according to some *observation rule* E (which may be very elaborate), the observed x will satisfy the relations F_i, ($i=1, 2, \ldots, 5$). If we denote the observation acts by α, and if we write Λ for the universal quantifier and $\rightarrow$ for the connective "if . . . then," the statements of our deductive system take the form

$$\bigwedge_{\alpha}\bigwedge_{x}[(\alpha, x)\varepsilon E \rightarrow x\varepsilon F_i], \qquad i=1, 2, \ldots, 5 \quad (7)$$

Expression 7 is to be read as follows: For all observation acts α on x, *if* the observation acts on x are carried out according to rule E [that is, the pairs (α, x) are elements of E], *then* these observed values will be elements of the relations $F_1, F_2, \ldots, F_5$ (*2*).

We have succeeded in writing the basic statements (or axioms) of our "model." At the same time we have come face to face with a difficult and fundamental problem in the construction of theory in economics, and more generally, in the social sciences. Our statements are universal in character, and to the extent that disposition rules by reference to empirical data have been formulated, they have empirical content. (We shall come back to this question in the next section.)

It is well-known, of course, that in the case of universal statements a single contradictory instance suffices to falsify them. Is the economist ready to take the consequences, if a single contradictory instance is discovered? That is to say, is he ready to scrap his "model" if such an instance is unearthed? The answer is no. For instance, suppose that we test this "model" in a feudal or a tribal economy, and that we demonstrate the falsity of its constituent statements in that context. The author of the "model" would argue immediately that the "model" was never meant for such an economy. If in turn we ask that the economist specify the economy for which the "model" was meant, we will get a vague answer at best. The economist might say, for instance, that the "model" was meant for the American economy in the 1950's. Such a statement, however, would carry the consequence that the "model" was never meant as a "theory," but rather that it was meant as a description of the economic behavior in an individual instance—namely the American economy in the 1950's. If we wish to claim that our "model" is a "theory," we would either have to accept the statements in expression 7 without qualification, without specification of the context in which the expression is supposed to be applied, or we would have to provide a specification of the context of its applicability in *general* (nonindividual) terms. The term *social space* will take the place of the awkward term *context* in what follows.

In attempting to characterize the social space for which the "theory" or "model" is supposed to be asserted, we must guard against the error of characterizing it in terms of the properties of the relations which make up our deductive system. Were we to assert, for instance, that the "model" is supposed to hold in the social space in which consumer behavior is described by equation 3 of expression 1, and so forth, we would turn our deductive system into a pure system, thus escaping forever an encounter with empirical data. The social space, therefore, must be characterized here in general terms and independently of the information contained in our basic relations. This is a hard task which has never been fully carried out by economists. Indeed it is difficult to say whether any attempts to characterize social space in an appropriate manner will ever be successful. The whole matter needs careful thinking through before predictions on this score can be made.

Empirical Content

We have discovered that the economist is not prepared to commit himself to statements of the form

$$\bigwedge_{\alpha}\bigwedge_{x}[(\alpha, x)\varepsilon E \rightarrow x\varepsilon F_i]$$

without inserting a qualification concerning the relevant social space, while at the same time he is not prepared to characterize it. This implies that the basic statements of his deductive system contain uninterpreted terms. The implications of this state of affairs can best be brought out by introducing social space formally into our deductive system.

Let us denote by k observation acts intended to identify social space, and by K^r the class of all observation acts whose outcome is the identification of the r^{th} social space (*3*). The observation acts k on social space must be carried out in conjunction with but independently of the observation acts α on x. This notion may be expressed compactly by αPk. Thus we may now replace expression 7 by

$$\bigwedge_{k}\bigwedge_{\alpha}\bigwedge_{x}\{(k\varepsilon K^r \text{ and } \alpha Pk) \rightarrow [(\alpha, x)\varepsilon E \rightarrow x\varepsilon F_i]\} \quad (8)$$

This expression reads as follows: For all observation acts k on social space, and all observation acts α on x, and for all x, *if* k is an element of K^r (that is to say, if the observation acts on social space identify the r^{th} social space), and if the observation acts k on social space are carried out in conjunction with but independently of the observation acts α on x, *then* if the observation acts α on x are carried out according to rule E, x belongs to relation F_i.

The fundamental difference between expressions 7 and 8 is this: In expression 7 the flat assertion is made that the observed values of x satisfy the relations F_i. In expression 8 a conditional assertion is made—namely, it is asserted that the observed values of x satisfy the relations F_i, *if* the observations on x are carried out in the r^{th} social space. If economists were in a position to provide an interpretation for all the terms of the antecedent (the "if" part) of the major implication in expression 8, the difficulties would disappear, and it could be claimed that the construction of theory in economics would not present any *sui generis* problems. As a matter of fact, neither K^r nor P can be given appropriate characterization at this juncture in any of the well-known economic theoretical constructs; thus the antecedent of the major implication in expression 8 remains uninterpreted.

The implications of this state of affairs are as follows: Suppose that in some particular instance the observed values of x satisfy F_i. Then, the consequent (the "then" part) of the major implication in expression 8 is confirmed. However, as is shown in Table 1, if the consequent of the implication is true, the whole statement is true, independently of the truth value of the antecedent. Thus, in this instance, the statement as a whole is *confirmed*. Suppose, however, that the consequent of the major implication in expression 8 is *not* confirmed, that the observed values of x do *not* satisfy relation F_i. In this eventuality the truth of the statement as a whole would depend on the truth value of the antecedent. (If the antecedent is false, the statement is true, otherwise false.) Since, however, the terms of the antecedent have not been interpreted, it is impossible to decide whether or not the statement as a whole is true. The consequence seems to be that statements of form 8 are *only* capable of being confirmed by reference to empirical data, as long as terms in the antecedent of the major implication in the expression remain uninterpreted.

Deductive systems whose constituent statements are capable of confirmation but not of rejection will be called *models*. It follows from the argument thus far that economists construct models rather than theories. A theory may be given form 7 or 8. If it is given form

8, K^r and P should be provided with an interpretation. If they are not, then we are dealing with a model rather than with a theory. Interestingly enough, economic models provide explanations of economic behavior in those instances in which they are confirmed, but they cannot be used as predictive devices because the conditions of their applicability cannot be spelled out beforehand. Thus we arrive at the rather odd conclusion that economic models are *strictly explanatory* devices (*4*). Statements in models gain empirical content—but in the restricted sense that only rules for their *acceptance* by reference to empirical data can be formulated.

The fact that economic models are strictly explanatory devices does not mean, of course, that economists are unwilling to make predictions. Indeed, often enough they are called upon to do exactly that. What it does mean is that predictions based on models are necessarily dependent upon the state of mind of the economist about to make a prediction. If I am called upon to make a prediction concerning the effect of some tax measure on income or employment over the next 3 years, say, I will have to choose that model (among many alternative available models) in terms of which the prediction is to be made, which in my opinion stands the best chance of being confirmed by the observations on x over the next 3 years.

We may formalize this process of choosing a model by arguing that the economist about to make a choice of a model is guided by a *subjective ordering* of models with respect to the likelihood of their confirmation by reference to data relating to a given historical individual, such as, say, the U.S. economy for the period 1960–1963. Success or failure on previous occasions will no doubt affect the subjective ordering but will turn out to be a very poor guide if the underlying economic structure is subject to sudden temporal or significant spatial changes. We cannot escape the fact that the ordering of models with respect to the likelihood of confirmation is a highly subjective matter. Forecasting on the basis of models is and will remain an *art*.

Augmented Models

In qualitative economics the statements which are subjected to confrontation with data have to do with the direction of change of endogenous variables in response to changes in exogenous variables. In our sample economic model we arrived at the conclusion that an increase in government expenditures, G, would lead to an increase in national income, Y. Were we to subject this statement to a test, however, we would find ourselves forced to introduce significant additional specifications of our model. It is clear, for instance, that our theorem concerning the sign dY/dG relates to the solution values of Y, to values of Y which satisfy the postulated system of equations. This immediately suggests that the evidence brought to bear on our model should consist of pairs of values (G, Y) taken from observed sequences over time which have the property of being stationary or constant over time. Since time has not been introduced explicitly into our model, the concept of stationariness or steady state cannot be fitted in, unless we stand ready to revise our model by the introduction of *dynamic* considerations. This can be done by dating our variables and introducing appropriate time-lags (*5*).

Table 1. The semantic or meaning rule for $\rightarrow$; P and q are any two statements.

P	q	$P \rightarrow q$
True	True	True
True	False	False
False	True	True
False	False	True

As soon as this dynamization has taken place, the solution values of our variables (in the original, nondynamic model) will turn out to be the steady-state values of the dated variables (in the revised, dynamic version); and the theorem concerning the sign of dY/dG will be restricted to the steady state values of the dated variables. Naturally, a theorem such as this can be derived within the dynamic model only if the dynamic model possesses the property of stability (*6*). This implies that appropriate restrictions must be imposed on the structural parameters of the dynamic model (and, by implication, on the nondynamic earlier version) which guarantee stability. Furthermore, our observation rule E must be revised in such a fashion that only steady-state values of our variables are taken into account.

It is obvious that in the process of testing a simple qualitative statement, we are forced to expand and revise our model in order to make such a test possible. Nor is the revision considered thus far sufficient. Probabilistic considerations must also be introduced. We cannot reasonably expect that the values of our variables will exhibit *exact* constancy over time; rather we can only hope that they may exhibit approximate constancy. Also, when we talk about a change in G or a change in Y, we presumably wish to restrict the argument to "significant" changes. All this requires the introduction of probabilistic statements.

What we end up with then is not the original, *basic* model, but rather an *augmented* model. It should be fairly obvious that a basic model can be augmented in a very large number of ways. Indeed, to every basic model there corresponds a class of augmented models. If the fundamental hypotheses of the economist are contained in the basic model, whereas the augmented versions are viewed as constructions incidental to testing the basic model, it would follow that the economist should not be willing to abandon his basic model unless *all* possible augmented versions have failed to be confirmed. It is clear that under these circumstances the basic model would enjoy a high degree of insulation from the impact of empirical evidence. Indeed it is often the case that the augmentation of the basic model is carried out *after* the data to be explained have been examined and in a manner which insures that the basic model will be confirmed. This is typical, of course, in the work of economic historians. It should be mentioned, perhaps, that econometricians always work with fully developed (augmented) models—and that their models, therefore, do *not* enjoy the insulation from the impact of data which is characteristic of the basic models of the general economist.

Conclusions

In this paper I have attempted to bring out the character and extent of the empirical content of statements in economics. The basic result can be summarized as follows: Economists construct models, not theories. Their models may be confirmed by reference to empirical data, but they cannot be refuted. Therefore, they are strictly explanatory in character. Futhermore, since the models are typically proposed in a basic form, and since to each model corresponds a class of augmented models (whose form is such as to enable the economist to carry out relevant tests), the economist has a great deal of flexibility in choosing the particular final form of the model to be subjected to test. This state of things need not be considered as unsatisfactory, if a liberal interpretation is given to the notion of empirical content. Furthermore, no change in this practice may be expected unless we succeed in characterizing social space and in giving it an appropriate place in the construction of economic theory. Whether this is feasible or not cannot be settled until more effort in this direction has been exerted.

Notes

1. This statement is restricted to what may be called *positive economics.* The branch of economics dealing with normative propositions, known as *welfare economics,* does consist of pure deductive systems.
2. Strictly speaking, the F_i's are not relations because they have not been completely specified. This problem will not occupy us here.
3. The r^{th} social space may be thought of as the r^{th} subset of all the possible states of the world. These are rather involved notions, but it would take us too far afield to attempt further elucidation in this article.
4. In the philosophical literature the distinction between explanation and prediction is of minor importance. An explanatory device is generally considered to be capable of prediction and vice versa. The claim made in this article that models are *strictly explanatory*—that is, incapable of use as predictive devices—stems from the assertion that there exists a class of non-L-determinate statements which may be confirmed, but may *not* be rejected, by reference to empirical evidence.
5. This procedure leads to the formulation of a system of difference equations. We could, of course, achieve the same objective by formulating a system of differential equations.
6. The notion of dynamic stability is far too complex for discussion in this article.

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American Science between 1780 and 1830

The exploration and industrialization of the new nation led to advances in natural science and technology.

D. J. Struik

When, in 1783, the United States had emerged victoriously from the ordeals of the Revolutionary War, liberal-minded men and women on both sides of the ocean held high hopes that the new freedom would bring a flowering of the arts and sciences. Fair Columbia, whose Fathers were sages such as Franklin and Washington, had indeed many leaders who had given proof of their concern with the cultivation of knowledge for the betterment of man. The men around the already well-established American Philosophical Society in Philadelphia, soon the capital—Jefferson, Rush, Rittenhouse, and Bartram, not to speak of Franklin—and the men around the new American Academy of Arts and Sciences in Boston—Adams, Cutler, and Bowdoin—had, even during the war, continued to cultivate the sciences. In all the towns along the Atlantic coast were serious gentlemen of scientific inclination, often connected with the colleges in Cambridge, New Haven, New York, Philadelphia, and Williamsburg.

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Growth from Colonialism

These great hopes were bound to be frustrated, at any rate during the early years of the republic. The primary task of any country emerging from colonialism is to catch up with the advanced part of the world, and the United States had many men only too willing to work for this goal. The main efforts had to be economic and political: improvements in transportation and in industry, as well as the raising of the political position of the country among the nations of the world. These tasks, once undertaken, were successfully carried out despite great handicaps: canals were dug, turnpikes were laid out, factories were built, mass production was inaugurated, steamboats were constructed. The Louisiana Purchase improved the political condition of the country in relation to the French, British, and Spanish empires beyond all expectations, giving the United States, moreover, an entrance into the profitable fur trade. The period which opened with the Lancaster turnpike near Philadelphia and the Middlesex Canal near Boston ended with the extension of the National Road far into the deep Middle West and with the ambitious project of the Erie Canal. Opening with the experiments of John Fitch and Oliver Evans in steam navigation (Fig. 1), it ended with the great successes of Robert Fulton's invention on eastern and midwestern rivers, and even (though these were only tentative) on the ocean. It opened with the feeble experiments of Orr, Cabot, and others in textile machinery and ended in the fulfillment of Samuel Slater and Francis Cabot Lowell's experimentation in the factory towns of New England, and even in the foundation of whole new cities, such as that show place, Lowell. It opened with a few merchant ships setting out from Salem, Boston, and other harbors to China and the northwest Pacific and ended with the American merchant marine all over the globe. This progress, begun rather slowly during the early federalist days, gained impetus with the expansion of the popular forces in the days of Jefferson, Madison and Monroe.

This was progress indeed, even though we must be careful in our use of this