CHAPTER 10

(1966)

Further Thoughts on Corrado Gini's *Delusioni dell'econometria*

One of the many academic enterprises which Corrado Gini, the scholar whom we are honoring today, encouraged or supported with his scientific prestige is the Econometric Society. For Gini, it is proper to remember on this occasion, was a founding member of that society. But Gini did not have to wait for the Econometric Society to be founded. He had been an « econometrician » in the true sense of the word for a long time before this term was coined. Therefore all the greater is the significance of the message which Gini, during the later part of his life, wanted to send to his fellow econometricians and which he crystalized in the title of a short article : « Delusioni dell'econometria » (1). Coming from a scholar who had already devoted many good years to devising new quantitative tools and to applying them successfully to the analysis of many social phenomena, the message cannot be interpreted as a denial of the value of quantitative analysis in economics. Gini wanted only to impress upon us the danger created for the economic science by the ostentatious yet decidedly false claims econometricians ordinarily make for the scientific superiority of many of their procedures. In one place (2), he even alluded to an additional danger, of far greater consequences, which may confront the student of economics if the ostentation of the econometric *Akademia* turns into scientific intolerance — a thought which though not absurd

(1) *Giornale degli economisti*, Anno XV, 1956, pp. 174-177. See also his "Au sujet de l'utilité et de la limitation de l'emploi du calcul des probabilités en économie politique", *Economie appliquée*, t. X, n. 1, 1957, pp. 49-55.

(2) "Delusioni", p. 176.

seems highly unrealistic. Be this as it may, the consummate scholar wanted to warn us for the sake of the very science which he served with such high honor.

Gini's message about the delusions of econometrics has already been taken up and amplified by a few of my distinguished colleagues from Europe. In support of the same message I wish to add a few observations concerning some specific points which by their nature belong to four distinct aspects of the problem. These observations may suggest that my message is stronger than Gini's — and perhaps it is. But I wish to make it perfectly clear that they are offered only with the hope that they will orient further discussions toward the constructive end which certainly Gini had in view.

I. — Excess of Mathematical Formalism.

It is certainly not because of some fancy of modern mathematicians that formalism has fared in mathematics as splendidly as it has. Its success derives from the immense economy of thought it created for mathematicians themselves, who are continously invited by special sciences to solve this or that particular problem. Mathematicians have thus begun to study, for instance, the properties of relations in unspecified terms, $x R y$, because these properties could serve equally well to a sociologist for whom x and y may stand for individuals and R = « related by blood », or to an economist for whom x and y may represent productive processes and R = « more efficient than ». This very idea implies that the sociologist and the economist should fill the empty boxes of mathematics with some specific empirical content proper to the one's own field of endeavor. But because of the well-known difficulties of getting down to empirical brass tacks in economics, many a student has found it more comfortable to continue the formalism of mathematics or, as this is often put, to substitute mathematical exercises for economics. This type of pseudo-economics is particularly prevalent in the most recent contributions on utility theory where often the word utility appears only in the title. The text itself speaks only of an undefined relation, of its being upper or lower continuous, or of some other abstract properties which have hardly any relevance for the study of consumer's behavior in the real world. Curiously, none of the respective authors seem to be aware that a proposition based upon the continuity of a mathematical system cannot be tested empirically any more than the irrationality of $\sqrt{2}$ can be established on the workbench. Whether the consumer can be indifferent between two different commodity combinations is an issue that can be settled only by general introspection, not by testing theorems on lower and upper continuity through laboratory experiments on behavior. Such absorbing preoccupations with the mathematics called for by a particular problem are responsible in a great degree for the fact that econometricians in general tend to forget that, apart from engineering economics, economic models are not blueprints of the reality. They are only schemata or similes (³), which are useful only if handled with the delicate touch demanded by the very nature of economic phenomena. There is certainly no harm but gain in using a simile in which the entrepreneur is assumed to know the probability of every future market coordinate, for the purpose of illustrating the main thread of one's argument. But to identify such a simile with actual behavior in a world of pure uncertainty and, moreover, to use the mathematical expectation formulae as a guide for « rational » behavior is an irrational position against which John Maynard Keynes, Frank H. Knight, and Corrado Gini have, apparently in vain, raised their protests. To quote Gini:

> En économie politique [il y a] des problèmes (et des nouveaux en surgissent tous les jours) dont les données ne peuvent pas être chiffrées et mesurées comme l'économétrie le présuppose, mais dont l'importance est incomparablement supérieure, au point de vue théorique et pratique, aux raffinements, sans doute élégants et parfois aussi notables, que l'économétrie est en condition d'apporter (⁴).

(³) Cf. Gini, " Delusioni, " p. 174. See also Nicholas Georgescu-Roegen, *Analytical Economics : Issues and Problems*, Harvard University Press, 1966, pp. 117f.

(⁴) Gini, " Au sujet de l'utilité et de l'emploi du calcul de probabilités," p. 53. For a proof of the nonmeasurability of uncertainty see my *Analytical Economics*, pp. 208-211, 263-275.

Admittedly, formalism has been part and parcel of mathematical economics from its inception, but its excess has become alarming only in modern times. Surprising though it may seem, the most glaring illustration is the ultra-familiar concept of production function, which even in the most recent works is described indifferently as the relationship between input and output *rates* or between input and output *quantities*. This patent indifference towards the empirical content of the symbols used has prevented economists from seeing that if the two definitions are equivalent — as they have been always regarded — then all production functions must be homogeneous and of the first degree.

Let

$$q = f(x, y, \dots, z) \tag{1}$$

be the « production function » expressed in terms of *rates of flows per unit of time*, q for output, and $x, y, \dots, z$ for inputs. Let

$$Q = F(X, Y, \dots, Z) \tag{2}$$

be the « production function » expressed in terms of *quantities*. Since the process these are supposed to describe is a steady-going (static) process, we have $Q = tq, X = tx, \dots, Z = tz$, where t represents an arbitrary time interval. Clearly, if the modes of representing a production process, (1) and (2), are equivalent, we must have

$$Q = qt = tf(x, y, \dots, z) = F(tx, ty, \dots, tz) \tag{3}$$

for any nonnegative values of $t, x, y, \dots, z$. Making $t = 1$, we obtain

$$f(x, y, \dots, z) \equiv F(x, y, \dots, z). \tag{4}$$

From (3) it then follows that all production functions are homogeneous of the first order, as said. However, traditional satisfaction with the formal representation is so deep-rooted that at two recent international meetings I have encountered unusual difficulties in making some of my fellow econometricians see this most elementary point.

Still more important is that because of the same penchant to formalism, in standard — as opposed to Marxian — economics the time element, i.e., *the working day*, is completely absent from the description of a productive process. From what has been said above it can be shown that its correct formula is

$$Q = tf(x, y, \dots, z),$$

where Q is the « daily » output, $x, y, \dots, z$, the input flow rates and service rates of which the productive system is capable, and t is the length of the working day. This is not the proper place to dwell on the far reaching consequences which the omission of the time factor has for pure theory and especially for policy recommendations ([5]). Nor is it within the scope of this communication to cite other minor instances where the gale of formalism has driven sound economics aground. But one particular consequence of the curious fact that even for a non-mathematical economist mathematics comes easier than economics, should invite our attention next.

II. — The Loss of the Social Dimension.

It was Irving Fisher who first argued that the modern utility theory is not based upon a hedonistic philosophy of human behavior ([6]). The argument, challenged by a few, is long since an article of faith of the neoclassical economist. But if one tries patiently to penetrate the veil of words — at times, mangled, at times, empty — in which the argument is enveloped, one would discover that the whole edifice is rather specious. Indeed, to say with Fisher that utility is not synonymous with pleasure but only with the desire of pleasure does not absolve utility theory of hedonism. And to say, also with Fisher as well as with

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([5]) For further details the interested reader is referred to two of my papers, "Process in Farming vs. Process in Manufacturing: A Problem of Balanced Development," chapter 5 in this volume, and "Chamberlin's New Economics and the Unit of Production," ch. 2 in *Monopolistic Competition: A Study in Impact, Essays in Honor of Edward H. Chamberlin*, ed. R.E. Kuenne, New York, John Wiley & Sons, 1966 (in press).

([6]) Irving Fisher, *Mathematical Investigations in the Theory of Value and Prices*, Yale University Press, 1925, pp. vii, 11.

Pareto, that « each individual acts as he desires » (7), is a type of empty talk which leaves one in complete empirical darkness. What one would certainly like to know is *what* the individual desires. One reads the answer of the utility theory to this question only as he gets to the second page: the individual desires *only* commodities. Hence, an ordinal measure of his desire is a function $U(x, y, \ldots, z)$ only of the amounts of the various commodities he may possess, and with this the way is clear for all kinds of mathematical elaborations and conclusions by which we now generally swear.

No doubt, we do so because, like the great founders of utility theory, we have in mind a Civil Society where the actions of the individual are determined only by utility, in the ultimate analysis, by commodities. But the exceptions to this rule, few though they are, in the urban stratum of the Western world should have made us see that in its general form economic choice is not between two commodity vectors, X and Y, but between two complexes $(X; A)$ and $(Y; B)$, where A and B denote the actions by which each vector may be obtained. After all, contrary to what utility theory assumes, the economic choice is not a culturally *free* choice. One chooses between available complexes on the basis of the values which the actions have in the corresponding cultural matrix *and* the values which the commodities have on the personal utility scale. The fact that in some societies — which are generally referred to as « traditional » societies — the first element weighs heavier in the balance than the second, does not mean at all that their members are irrational. Only their behavior is not susceptible of being cast into a purely arithmomorphic model (8). But this is no reason for us to proceed like Procrustes or to throw up our arms in despair at their « irrationality », should we be called to make policy recommendations for a traditional society.

III. — The Abuse of Statistical Theory and Tools.

Every econometrician knows that all statistical inferences and tests are based on some special assumptions in addition to

(7) Fisher, *op. cit.*, p. 11; Vilfredo Pareto, *Manuel d'économie politique*, Paris, Giard, 1927, p. 62.

(8) See further *Analytical Economics*, pp. 124-129.

one general assumption, that of randomness. Yet in many cases there seems to be a great gulf between what one knows and what one does. I shall discuss these cases one by one.

1. — As we all know, the entire edifice of statistical theory rests on the general assumption that the relation between any sample and the parent population is homeomorphic to that produced by a random mechanism. Most econometricians have assumed all along — implicitly as well as explicitly — that *all* economic data fulfill this condition and yet no justification other than mere verbalism has been offered in support of this position. Perhaps the predicament of the econometrician is that, since in the domain of social sciences only in a few cases can we point to the parent population, a proof of the randomness of economic data is impossible. In agronomy, for istance, we are justified in regarding any group of observations as a random sample because we can experiment with the *same* type of fertilizer on as many plots selected at random as we wish. But what is the reason for treating, say, the occupational ratios in all the counties of a state as a random sample or, still worse, as a random sample from a single universe (9)? *Irregularity*, it should be repeated again and again, is not necessarily the same as *randomness*. The last decimal digits in a five decimal logarithmic table certainly form an irregular pattern. But it is equally certain that they are not determined at random. Besides, the idea that the distribution of the economic features of the counties in a state is the result of God's tossing some special dice over the state is certainly bizarre. I need not mention the more obvious fallacy of treating time series data as a random sample because this fallacy after being duly exploded seems now extinct, at least as far as respectable works are concerned.

2. — No theorem on which a statistical test is based is valid in a vacuum. All such theorems — even those pertaining to the so-called nonparametric tests — make some assumptions con-

(9) To be sure, hardly any economic data is not affected by errors of observation resulting from the imperfection of statistical registration. But this is not what an econometrician means by the economic data being a random sample.

cerning the parent population. For instance, the most popular tests invoked in support of the realiability of an econometric model, the t-test, the F-test, and the z-test, all require that the sample be chosen at random from a *normal* population. Consequently, even if one would deal with data that can be safely regarded as constituting a random sample, before applying any of these tests one also needs to make sure that the parent population is normal. Unfortunately, the extremely few cases analyzed in the literature do not encourage us at all to expect economic data to be normally distributed [10]. A number of doctoral candidates, who at my insistence have tested the normality of some of the data used in their dissertations, have all obtained decisively negative results. In this situation, to claim the validity of an econometric model on the basis of, say, the F-test is tantamount to claiming that a patient does not have cancer because his blood test for sugar has come out negative.

3. — Because this point concerns a muddle for which statistical theory shares part of the blame, it deserves to be discussed at some length. For a while statisticians and econometricians were troubled by the question of which of the two regression lines of a bivariate distribution represents the « true » relation between two variables. The question has as much sense as if one would ask which of the two polar circles is the equator. It is not surprising therefore that it led to the idea that the difficulty of the answer lies in the existence of more than *one* regression line. Apparently, everyone ignored the fact that the existence of regression lines does not necessarily imply the existence of a « law » between the theoretical values of the variables. Or to put it differently, the point that was ignored is that a scatter of observations may reflect different stochastic structures.

A two-dimensional scatter may be the random image of a *point*, as is the case of the gun shots spread around the target point. It may also represent the results of shooting at a target which shifts on a given *curve*. Such a scatter is the random image

([10]) E.g., IRVING FISHER, *The Making of Index Numbers*, Boston, Houghton Mifflin, 1927, pp. 408-10.

of a curve, in general of a k-dimensional variety ([11]). We may refer to the element of which the scatter is the random image as the kernel of the scatter.

Any bivariate scatter, whatever its kernel, has two regression lines. In case the kernel is a curve, neither of these coincides with the kernel curve — except in quite special cases. The point that seems to need stressing is that it is the kernel curve that represents the relation between the two variables as this relation is conceived in any theoretical construction. For example, for a scatter of prices and quantities observed in different actual market situations where demand is the same, it is the kernel that represents this demand, $p = f(q)$. The idea is that each observation p', q' corresponds to some p, q representing the theoretical equilibrium which the market would have reached in the absence of any imperfections. That is, $p' = f(q) + \xi, q' = q + \eta$, where ξ and η are random variables with zero mean.

Let us take the simple case where the « true » (or the equilibrium) position $P(X, Y)$ shifts at random on the straight line $Y = aX$ and that X is a variable with zero mean and variance σ^2. Let (ξ, η) be the deviations from P:

$$\overline{\xi} = \overline{\eta} = 0, \overline{\xi^2} = \sigma_1^2, \overline{\eta^2} = \sigma_2^2, \overline{\xi\eta} = \rho\sigma_1\sigma_2. \quad (5)$$

For the observations

$$x = \xi + t, \; y = \eta + a t \quad (6)$$

we have

$$\overline{x} = \overline{y} = 0, \overline{x^2} = \sigma_1^2 + \sigma^2, \overline{y^2} = \sigma_2^2 + a^2\sigma^2, \overline{xy} = \rho\sigma_1\sigma_2 + a\sigma^2 \quad (7)$$

If the regression of y on x is linear, we have

$$BG(y/x) = \frac{\rho\sigma_1\sigma_2 + a\sigma^2}{\sigma_1^2 + \sigma^2} x \quad (8)$$

([11]) NICHOLAS GEORGESCU-ROEGEN, " Sur un problème de calcul des probabilités avec application à la recherche des périodes inconnues d'un phenomène cyclique, " *Comptes Rendus de l'Académie des Sciences*, July 7, 1930, and " Further Contributions to the Scatter Analysis ," *Proceedings of the International Statistical Conferences*, 1947, vol. V, pp. 39-41.

kernel

scatter analysis

which represents the « best guess » of y for any *given* x. It is immediate that this line represents the true law if and only if

$$\rho\,\sigma_1\,\sigma_2 + a\,\sigma^2 = a\,(\sigma_1^2 + \sigma^2), \tag{9}$$

i.e., if $a = \rho\,\sigma_2/\sigma_1$ or if $\sigma_1 = 0$. The first condition means that $BG\,(\eta/\xi) = a\,\xi$, the second that *the observed x is the true value X of the corresponding equilibrium position*. Needless to add, both these conditions represent very special cases. But one widespread fallacy connected with the second case must be denounced. The fallacy confuses the fact that, as happens often, x is measured with a high degree of approximation with the fact that x is the « true » value. This confusion is at the bottom of the prevalent position that (8) represents the theoretical law.

The famous « regression law » of Francis Galton provides an excellent illustration of the points made above. As biologists later learned, for a population consisting of a pure biological line, the height, for instance, of the parents, x, and that of the off-springs, y, are uncorrelated identical normal distributions:

$$\bar{x} = \bar{y} = M,\ \overline{(x - M)^2} = \overline{(y - M)^2} = \sigma^2,\ \overline{(x - M)\,(y - M)} = 0 \tag{10}$$

The kernel of the distribution is a *point*, $X = M$, $Y = M$. Now, if for the sake of simplicity, we take a population formed by a normal distribution of pure lines, M becomes a normal variable with

$$\overline{M} = M_0,\ \overline{(M - M_0)^2} = \Sigma^2, \tag{11}$$

The kernel of the new distribution of x and y is $X = Y$ which, clearly, represents *the true heredity law*. The regression line is

$$BG\,(y/x) = \frac{\Sigma^2}{\Sigma^2 + \sigma^2}\,(x - M_0) + M_0, \tag{12}$$

the slope of which is $\Sigma^2/(\Sigma^2 + \sigma^2) < 1$. It is thus seen that Galton's error was precisely that of the modern econometrician: to take the regression line for the true law. A far more instructive way of describing this error is this: *ex post* we can say that Galton reasoned as if the *observed* height of the parent represented the characteristic height of the pure line to which the parent belongs. But in a mixed population the individual's height, be it measured with perfect accuracy, does not reveal to which pure line he belongs, the reason being that such a height is the sum of two unknown random components. Similarly, an observed price, for instance, even if known with perfect accuracy should not be confused with the equilibrium price at the time of the observation.

Let us consider the scatter (ξ_1, η_1) such that its kernel is a point, the origin, and such that

$$\overline{\xi_1^2} = \sigma_1^2 + \sigma^2,\ \overline{\eta_1^2} = \sigma_2^2 + a^2\,\sigma^2,\ \overline{\xi_1\,\eta_1} = \rho\,\sigma_1\,\sigma_2 + a\,\sigma^2. \tag{13}$$

Obviously, without outside information, it is absolutely impossible to distinguish between the structure of this scatter and that of (7) the kernel of which is a straight line. Also, without some outside information — seldom available in the domain of economics — we cannot discover the true law $Y = a\,X$, for the simple reason that the last three equations of (7) cannot *identify* five unknowns: $\sigma, \sigma_1, \sigma_2, \rho$, and a. We cannot even affirm that $Y = a\,X$ lies within the acute angle of the two regression lines. All the more useful therefore is to mention a particular situation which may be encountered in some special problems where σ_1^2, σ_2^2, $\rho\,\sigma_1\,\sigma_2$ are known except for a constant factor of proportionality λ [12].

In this case the last three equations of (7) become

$$\overline{x^2} = \lambda\,K_1 + \sigma^2,\ \overline{y^2} = \lambda\,K_2 + a^2\,\sigma^2,\ \overline{x\,y} = \lambda\,K_{12} + a\,\sigma^2, \tag{14}$$

and a is determined by

$$\begin{vmatrix} 1 & K_1 & \overline{x^2} \\ a & K_{12} & \overline{x\,y} \\ a^2 & K_2 & \overline{y^2} \end{vmatrix} = 0\ ^{(13)}. \tag{15}$$

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[12] See the references given in footnote 11.

[13] To take care of the fact that we generally do not know *ex ante* whether the kernel passes through the origin, relation (7) must be replaced by

$$\bar{x} = x_0,\ \bar{y} = y_0,\ \overline{x^2} = x_0^2 + \sigma_1^2 + \sigma^2,$$
$$\overline{y^2} = y_0^2 + \sigma_2^2 + a^2\,\sigma^2,\ \overline{x\,y} = x_0\,y_0 + \rho\,\sigma_1\,\sigma_2 + a\,\sigma^2. \tag{7a}$$

Moreover, in economics it is more realistic to assume that the distribution

The difficulty in determining the true law with the aid of a scatter is analogous with that of the identifiability of the coefficients in the so-called shock-models. Now, if according to logic a certain system of data cannot provide the kind of information we would like to have, that is the end of the matter. There is no good purpose in overrunning logic by a makeshift and presenting the makeshift as the product of the highest form of scientific procedure. Curiously, the point has been immediately accepted in the case of nonidentifiable coefficients but completely ignored in the case of nonidentifiable parameters of scatter distributions.

IV. — Curve Fitting Fetish.

This last point pertains to the confusion between discovering a quantitative law from a series of data and merely fitting a mathematical formula to the same data. The confusion thrives on the characteristic fluidity of the phenomenal domain of economics : almost any economic phenomenon is a *potential* element of change for almost any other such phenomenon. That is why we profess the highest esteem for general equilibrium theories. In this we are, no doubt, right. But the case of econometric models — which generally aim at formulating precise quantitative macroeconomic laws — is quite different. One should endeavor to explain a macroeconomic phenomenon by only a few variables chosen for their theoretical affinity and their predictive qualities surmized from simple analysis. The contrary happens in many an econometrician's shop. There, as I once put it, we see him selecting his tools with a single purpose in mind : to cut the log on his workbench in such a way that he

of the equilibrium positions on the theoretical law, instead of following a random law, is *irregular*. For this second alternative,

$$\bar{x} = T_1, \bar{y} = y_0 + a\, T_1, \overline{x^2} = \sigma_1^2 + T_2, \qquad (7b)$$

$$\overline{y^2} = y_0^2 + \sigma_2^2 + a^2\, T_2, \overline{xy} = \rho\, \sigma_1\, \sigma_2 + a\, T_2,$$

where $T_1 = (\Sigma\, t_i)/N$, $T_2 = (\Sigma\, t_i^2)/N$, and N is the number of observations. The necessary modifications of (15) present no difficulty.

may be able at the end to exclaim triumphantly « I told you that inside that log there was a beautifully carved Madonna ! ». Since I used this metaphor I came across a research project which, sad to say, is equally piquant : to find which of the numberless ways in which « money » items can be combined in one item fits Keynes' system of equations. The project differs little from the more conspicuous form of pseudo-scientific endeavor, namely, that of seeking among a large set of variables those that would make the regression equation pass with flying colors the « statistical test ».

Admittedly, not all econometric shops follow exactly this recipe. But with the increasing facilities of the computer centers the practice is likely to become predominant, at least by numbers.

There is no denial that progress in many special sciences has often consisted in adding a new variable in the functional relationship expressing a law. A most convincing illustration is the addition of lunar nutation in the formula for the position of the earth. But econometricians seem to ignore the fact that a better fit obtained by adding a new variable does not mean at all that the formula is also a better law. For a formula to represent a law it is not sufficient that it should fit well the available observations : the acid test is the fit for all other observations. Perhaps, another predicament of the econometrician is that in the case of a stochastic law with a high variance this acid test requires a large number of additional observations, which in turn requires a new census or a great number of annual observations. But then one should display greater skepticism and far less assurance as regards the significance of the quantitative laws derived by mere fitting. On the other hand, if, as has appened in a not small number of cases, the acid test came out negative, the excuse that meanwhile evolutionary changes took place boomerangs with disastrous effect : it is tantamount to admitting that evolutionary factors play a substantial role and yet cannot be caught in an arithmomorphic scheme.

V. — Concluding Remarks.

Some of the observations made in this paper seem to point to the illusions of those who profess the science of econometrics, others to the delusions of the discipline itself as it appears in the actual endeavors. Now, illusion is the very thing upon which a scholar's devotion to science feeds. He must have the illusion that what he thinks is right. Should he begin considering all the doubts that may exist about what one can say « correctly » in his own domain of study, progress would come to a standstill. After all, mathematicians themselves at one time entertained such illusions as the duplication of the cube or the differentiability of any continuous function. And no one can say what other illusions are still hidden within the body of modern mathematics. The point which seems to characterize the econometric profession and which has created a source of irritation between that profession and the rest of the economists, is that, contrary to what happens in other analytical sciences, the econometricians tend to cling to their illusions even after they have been duly exploded. It is because of this attitude that illusions — a necessary companion of any scholar — turn into delusions. Recalling an earlier metaphor, we may compare the resulting consequences with those which would prevail if medical science would not fight — as it does — its delusions as they become certified. Perhaps, we, the econometricians, can get by with our delusions either because most of our models pass into oblivion as soon as we have finished dressing them up elegantly or because — and this is the saddest part of the story — the consequences of their adoption by a policy maker, even if they can be traced back to the real culprit, become manifest only after such a long time that there is no longer any purpose in indicting him. By then the culprit may be busy extolling the qualities of another model. An epistemology which disregards entirely the acid test of a formula is certainly strange for a discipline which at the same time claims to believe only in objective science. The consequences have been so admirably described by Corrado Gini that I find it appropriate to close my communication by quoting him :

> La condition essentielle pour faire des progrès par le procédé des modèles, et en général par la méthode hypothético-déductive, est que les conséquences qu'on en deduit soient dûment vérifiées. [Les modèles] méritent confiance seulement si on peut s'assurer que les hypothèses schématiques qu'ils impliquent ne portent pas à des conclusions trop divérgentes des faits. A mesure que des faits nouveaux sont mis en évidence, le contrôle doit être renouvelé. ... C'est ce qu'on a fait et ce qu'on fait systématiquement dans les sciences physiques ; dès qu'on a construit un modèle, des milliers d'expérimentateurs, dans les laboratoires du monde entier, se hâtent de le contrôler. Ce n'est pas ce que l'on fait et que l'on a fait en économie politique, et en particulier en économétrie, et c'est bien à cause de cela que les sciences physiques font des progrès par la construction des modèles, tandis que l'économie politique et en particulier l'économétrie n'en font pas, ou, du moins, pas autant qu'elles pourraient. Les économistes, et surtout les économètres, n'hésitent pas à lancer sur le marché scientifique de nouveaux modèles sans se donner la peine d'écarter ceux qui sont déjà en circulation, ni de contrôler sur les faits si les nouveaux modèles sont préférables aux anciens. De cette façon, les modèles se multiplient, s'amoncellent, s'enchevêtrent, se contredisent et suscitent la méfiance et le scepticisme [14].

[14] GINI, « Au sujet de l'utilité et de la limitation de l'emploi du calcul des probabilités », p. 54. Also " Delusioni ", p. 176.