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Accomplishments, Opportunities and Needs of Agricultural Economists vis-à-vis Quantitative Techniques

Should we speak of techniques only? Is it not a degradation of our activity? For a short answer to all possible questions, economics has begun to resemble physics in shaping out a formalized language and this happens in the branch of agricultural economics too.

Complaints and criticisms because of formalism and alleged overvaluation of techniques can often be heard. I borrow the words of Waugh: a complex world requires appropriate techniques. Since the world has always been complex for people living in it, techniques as appropriate as possible are always needed. If we spend a little time reading from the history of human activity, especially economics and science, we find that the world by necessity created the quantitative means required. And considering the role of agriculture in the well-organized states of antiquity, we may see that measurement of land and products, grain storage and processing, food distribution, earthworks for irrigation and flood control, distribution of water for irrigation, etc., were the creators of demand for gadgets, known by us as elements of functional analysis or linear algebra, 4000 years ago in the Sumerian state. Similar things can be learned from ancient China and Egypt. The *Concise History of Mathematics* by Struik, *Science Awakening* by van der Waerden, *Science and Civilization in China* by Needham and other books can furnish useful knowledge to economists.

1 FROM THE RECORDS OF ACCOMPLISHMENTS

In the following we shall see something like separate still photographs taken out from a moving picture. They are meant to show segments from the process of building up the present inventory of tools at the disposal of agricultural economics.

1 *Beginnings for input–output relationships*

While natural sciences became the main companion of mathematics, the relations to economic activity developed rather in the background. A sign from 1610: A. Serra mentioned input–output relationships. For agriculture, Quesnay gave a figure according to which 1,000 francs of annual advances were assumed to produce an output of 2,500 francs. We may

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regard it as the parameter of a linear function through the origin. This figure had been used by Saint-Péravy in his *Mémoire*, criticized by Turgot in 1767. Reading thoroughly Turgot's argument, we may recognize a view which later became known as the law of variable proportions, and a cubic parabola lies behind the somewhat complicated verbal description of two segments. Thünen, relying on his own observations at Tellow, stated a diminishing marginal return for manuring potatoes. His description of the procedure for seeking the maximum of a function shows that he knew what he spoke of. (His location theory might rather be mentioned in connection with spatial models.)

2 *The role of farm accounting and cost calculations*
From Thünen's letter to his brother Friedrich (31 December 1820): "Nature answers, in any economy, what I am looking for, and everybody, even the scientifically trained farmer must learn from a long and expensive experience, since one does not take the pains of making records, thus all experience ever gathered goes lost again". [My apologies for the hasty, approximate translation from the German!]

The development of accounting and cost calculation offers good examples in a number of countries. Now I would mention only a few names: that of Laur, Horring, Rheinwald, Preuschen and Heuser, often heard in discussions when the accounting and cost calculation system for a sample of co-operative farms had been set up, more than twenty years ago. Besides their merits for the science and practice of farm management and advisory work, I would like to point to their merits for mathematical modelling. As a result of the activity of experts of agricultural accounting and cost calculations in various countries, mathematical model building found a rich inventory of requisites: sets of coefficients on one hand, systemic thinking, shaping out a model structure, goal setting, formation of possible farming situations on the other. Input–output analysis, mathematical programming, simulation and other techniques would be in a much weaker position without this heritage.

3 *Calculation of effects of weather upon yields*
Dependence on weather of yields in agriculture inspired many people all over the world to search for ways of measuring the influence of meteorological conditions. So was it with a group of outstanding Russian statisticians who, working under Tsarist as well as Soviet rule, made thorough time series analyses of crop yields as influenced by weather. Obukhov, Chetverikov and Yastremsky must be mentioned concerning the huge research work assigned to the Central Statistical Bureau in 1921–23, based upon 1883–1915 time series data for numerous regions. Soviet authorities considered these studies, seeking answers to the following questions, important:

(a) How large are the oscillations between years in the yields of crops in different locations of Russia?

(b) What kind of relationship exists between the changes of yields of neighbouring years?

(c) Is it possible to arrange the territories into regions showing synchronous oscillations of yields?

(d) How close are the correlations between yields of different crops?

The Vestnik Statistiki reported about answers given by the research group to question (c) for two crops and to question (d) for six crops.

In 1923, at a meeting of evaluation of the project, Slutsky emphasized the great *practical importance* of the work done.

A great personality in the organization of mathematical economic research and planning in the Soviet Union, V.S. Nemchinov, also came from this group of economists–statisticians. Starting from time series covering 25 years, collected by Obukhov, he investigated the effect of precipitation and evaporation by phenological phases. In 1937–39, he fitted weather response functions for three strains of summer wheat. In 1934, he made a cross section study based on data from the 35 pieces of land of the acreage under winter wheat in a sovkhoz, using variables for meteorology, wheat variety, inputs and agrotechnical measures. This type of combined analysis had been continued by others, e.g. Peregudov, in later decades too.

Meteorologists and statisticians have done much work of strong mathematical-economic orientation in many countries and numerous economists were engaged in similar activities. To save space, let the Russians represent them too!

4 *A line of quantification from biology to economics*

To Liebig's law of the minimum and its modification by Mayer into a relative minimum Zöller added in 1867 his findings about a square-root-type response to fertilizer. Wollny's experiments led to a statement about an optimal dosage not independent from other conditions, the response being viewed as a kink of two linear segments. In 1898/99 Duclaux gave a cubic response curve resulting from the opposite effects of two factor components. After this came Mitscherlich's statement in 1909 and its formalization often referred to as the Mitscherlich-Baule function. The experimentation led him to an important correction in economic phraseology (although he also seems not to have united the two segments of the response curve, verbally described by Turgot on two separate pages of his *Réflexions*) saying that the increments of returns and not total returns diminish before reaching the maximum of the response curve.

In the USA, Spillman published in 1924 a book containing his views and that of E. Lang on the "law of diminishing returns". The treatment was of a physiological–biological character, with inferences in the domain of economics. In 1933 his bulletin on fertilizer experiments came out and agricultural scientists became acquainted with the Spillman function.

5 *The "big explosion" and examples of a learning process*

Studies in response to prices by Black (1924) and Bean (1929) and an article by T.W. Schultz on research must be mentioned before I try to give a picture about a stream of studies using the devices offered by mathematicians, non-agricultural econometricians and OR specialists. Agricultural economists were eager to make use of the new facilities for a more intensive study of the problems they faced.

Let this stream of early applications be exemplified by American studies in several fields:

(a) Production functions: E. Jensen (1942) on dairy production; Tintner-Brownlee (1944) on derivation of production functions from farm records; Atkinson-Klein (1945) on cattle fattening; Heady (1946) on production functions from a random sample of farms; Monroe (1949) on non-linear systems for estimating animal nutrition requiements; Heady-Pesek-Brown (1955) on response surfaces and optima in fertilization.

(b) Linear programming: Waugh (1951) on a minimum-cost dairy feed; C. Hildreth-Reiter (1951) on choice of crop rotations; Fischer-Schruben (1953) on feed mixing with different prices; King (1953) on applications of activity analysis; Heady (1954) on logical aspects of linear programming; Swanson (1956) on fertilizer mixing.

(c) Risk and uncertainty: Heisig (1946) on income stability in high risk farming areas; Schickele (1949) on farm business survival under extreme weather risks; Schickele (1950) on adaptation to income uncertainty; Heady-Kehrberg-Jebe (1954) on instability and choices with crops; Babbar-Tintner-Heady (1955) on programming with variations in input coefficients.

Such a type of development can be observed in a number of countries, others had a different order of application. In the Soviet agricultural economic research, mathematical programming has had the first place for a long time although they could have learned upon a very important experience gathered in the past for doing production function studies on a broad scale (literature in this field began to pour in the 1970s), and the achievements in national and regional input–output analysis had to a great extent been connected with the tough and wise efforts of V.S. Nemchinov, a person so closely linked with agriculture. As to Hungary, production function studies preceded the application of input–output analysis and mathematical programming in agriculture while for industry and national econony an opposite sequence can be recognized, even considering the start of building of national econometric models. In India, crop response studies came out years before reports based upon other techniques appeared.

To add a non-agricultural aspect to the sketch of diffusion given above, let us have a look at developments in non-agricultural sciences, in comparison with that in agricultural economics, as represented by general input–output, interregional input–output and Markov analysis (many other, maybe better, examples could be found, of course), according to references taken from papers written by agricultural economists.

(a) Spread of general input–output to agricultural economics:

Non-agricultural sciences		*Agricultural economics*	
1951	Leontief	1952	Bachman, Fox-Norcross
1953	Leontief, Cenenery-Clark-Cao-Pinna	1953	Peterson
1954	Dorfman		

(b) Spread of regional-interregional input–output to agricultural economics:

Non-agricultural sciences		*Agricultural economics*	
1951	Isard	1953	Fox
1952	Moses	1956	Schnittker
1953	Isard, Leontief	1958	Schnittker-Heady, Carter, Ram
1956	Chenery	1959	Carter-Heady
1957	Teibout		

(c) Spread of Markov analysis to agricultural economics:

Non-agricultural sciences		*Agricultural economics*	
1952	Müller	1961	Judge-Swanson
1953	Goodman	1962	Judge-Swanson, Bostwick, Padberg
1955	Prais	1965	Lee-Judge-Takayama
1956	Hart-Prais, Sittler	1966	Steffen-Neumann
1957	Bellman, Anderson-Goodman	1967	G. Müller, E. Hanf, Stanton-Kettunen
1958	Goodman, Adelman	1968	Kislev Amiad
1959	Madansky	1969	Hallberg
1960	Howard	1970	Lee-Judge-Zellner
1962	Wolfe-Dantzig	1974	C.H. Hanf-E. Hanf
1963	Telser		
1967	De Gheelinck-Eppen		

Not only a "big bang" period showed the eagerness of agricultural economists to make good use of the means and thoughts produced in mathematics, statistics and other sciences, often rewarding the inventors by elaboration of extensions and presentation of new issues: so was it before, so will it also continue in the future.

The lists above reflect difference between fields, as far as speed of reaction is concerned. These differences may depend on the field of invention and first application: communication and learning is much easier in the case of closely related fields (the timetables for general and interregional input–output show the quick turning of the interest to the issues more directly important for agriculture even in case of the same technique). The requirements in date, computing facilities, demand and fashion also influence the learning process, probably in all fields of science.

6 *A selection from the recent activity in non-socialist countries*

The abundance produced by the 1960s and 70s is amazing. In all parts of the world interesting work has been done, in many cases directly in the service of economic planning, and the findings have often been valuable for natural and social scientists, administrators and politicians. Doing injustice to persons, countries and fields of research by not mentioning them here is unavoidable. This situation leads me to direct attention to a book of great value (although it may not be news for the majority): Volume 2 of a survey in agricultural economics (G.G. Judge, R.H. Dayet et al., *Quantitative Methods in Agricultural Economics*, 1940s to 1970s), published by the Minnesota University Press in 1977. Thus only a few representatives of new steps, rather randomly selected, will be mentioned here in a telegram style, from a few fields.

Input–output: besides the shift to regional–interregional problems, national models have often been used for projections serving purposes of agricultural policies. An example may be the study by Schluter-Heady (1975).

Mathematical programming: large systems were developed, mostly having a space and/or time aspect, for linear models I mention the CHAC by Duloy-Norton (1972), two US crop models by Taylor-van Blokland-Swanson-Frohberg (1977) and an analysis of alternative energy policies by Dvoskin-Heady-Burton (1978).

Behavioural equations were included in the Australian model APMAA reported by Kennedy (1973) and Monypenny-Walter (1976).

Risk and uncertainty have been a favourite topic. A long list may be represented by Renborg (1963), McInerney (1967), Boussard-Petit (1967), Boussard (1969, 1971), Hazell (1971) and Schiefer (1977).

Based upon Day's pioneering work, recursive programming has widely spread, used for projecting more realistic normative supply responses and development paths like in Schaller-Dean (1965), in the USDA project and in de Haen-Heidhues (1973).

From the studies using dynamic programming in a Markov process I mention that of Kislev-Amiad (1968).

Game theory: Dillon (1962) sung the requiem of this technique but its return to life is proven by recent papers by, e.g. McInerney (1969), Hazell (1970), Kawaguchi-Maruyama (1972), Ali ben Zaid Salmi (1976).

Monte Carlo technique: a more efficient procedure had been developed by Carlsson-Hovmark-Lindgren (1969) for farm planning. Dent-Bryne used it for investment planning. Bögemann (1977) compared the Monte Carlo procedure with mixed integer programming.

Simulation: besides the well-known MSU models for Nigeria (Manetsch et al., 1971) and South Korea (Rossmiller et al., 1972), one should mention the GOL, of world-wide coverage for grains, oilseeds and livestock (Rokjo et al., 1978, Regier, 1978).

Some other title would be suitable for a series of studies made by an ISU–Thai team for purposes of development policies for Thailand's agriculture. Demand for food, single crop models, recursive programming for regional and national agriculture, a transportation model and an econometric model for the evaluation of Thailand's rice export possibilities can be found, among others, in this complex.

Computerization of the planning process: as examples, the GEMAGRI from France (Boussard, 1972) and the Computerized Farm Planning from the United Kingdom (James, 1971) may be mentioned.

7 *From the activity of Socialist countries*

Problems emerging in the course of the development of economic planning required analyses different from the traditional ones. These countries had scientists with some experience gained in prewar research, and information came from countries with advanced econometric and OR activity. Now the agricultural economists engaged in OR and econometrics use about the same inventory of tools as in most non-socialist countries.

In 1967, studies from various countries were put together into a book in Moscow. The majority of them dealt with linear programming on the enterprise, farm, regional or national level (one parametric, two of them with non-linear programming, one with CPM, one with input–output, one with transportation, one with information streams, three of them with general problems of modelling).

Linear programming studies being most common and known abroad, I will not go into details. For introducing yield uncertainty into programming, I mention Teresa Marszalkowicz from Poland and Sieglinde Schmuntzsch-Hahn from the German Democratic Republic.

As a continuation of the reponse and yield variability studies of the 1920s–30s, I refer to three books from the Soviet Union: K.G. Tregubov *Mathematical methods of analysis of relationships in agricultural production*, (in Russian), Kolos, Moscow 1972; M.M. Yuzbashev *Methods of studying the dynamics of distributions and relationships*, (in Russian), Statistika, Moscow 1974; O.P. Krastin *Uses of regression analysis in agricultural economic research*, (in Russian), Zinatne, Riga 1976.

It is of interest how agriculture appears in the model systems elaborated for planning the development of vast territories. This is why I direct attention to two books from Kazakhstan: O. Kaldybaev and S. Bayzakov *Mathematical methods in planning and control of regional economy*, (in Russian), Nauka, Alam-Ata 1977; and S. Djandosov, S. Bayzakov and A. Esentugelov *Systems analysis in regional planning* (in Russian), Kazakhstan, Alma-Ata 1976.

The development of the cotton sector in the Soviet Union goes on according to the complex programme HLOPOK (Cotton). Treatment of location of production and processing, irrigation, repair, demand forecasts by systems of models for national, regional, sectoral, intersectoral and agro-industrial-complex levels can be found in a collection of studies edited by N.P. Fedorenko, G.M. Abdullaev et al. *Optimization problems of the development of the national cotton complex* (in Russian), Nauka, Moscow 1975.

There is a co-ordinated effort in the COMECON countries to develop an "automated control system" for agriculture. This would involve information flows, data banks, methods of analysis, model systems, computer hardware and software, training of personnel for purposes of analyses, quick elaboration of consequences of possible policy changes, variants of plans for helping decision-makers from the farm to the government level.

II A FEW WORDS ABOUT NEEDS AND OPPORTUNITIES

Many papers have dealt with this issue viewed from different angles. Let us remember, e.g., the writings by Glenn L. Johnson! Here I would like only to emphasize a few points.

1 *Some needs concerning the use of quantitative techniques*

First of all, agricultural economists must understand each other. We must accept that quantitative techniques involve both quite simple (but very important calculations) and very complicated computations. Different conceptions are often clashing but in a good organization they should reveal their theoretical, political etc. character and not hide behind techniques. Of course, views may differ concerning the treatment of a problem and the choice of the appropriate techniques.

If one starts from the characteristics of the matter to be handled, one can more easily avoid neglect of qualitative judgements and over-emphasising quantifications.

Better understanding between researchers and users of results is needed. The decision-makers, too, should make efforts, but the greater part of the task is on the researchers' side. They must find a language of clarity. However, researchers may also require understanding. If a decision-maker becomes mad when inconvenient outcomes of some planned steps are pointed out by careful analysis it is not the researchers who should be blamed.

To increase understanding between disciplines is a necessity: without this no co-operation in attacking complex problems may be successful.

I have often read complaints about placing too high a value on so-called "sterile disciplinary" results, at the expense of those serving practice. The opposite situation also is a real danger: overpreference for today's utility hampers preparedness to meet tomorrow's problems.

At the Nairobi Conference the issue of non-neutrality of the model builders was touched on. The acceptance of the social and political responsibility of the agricultural economists handling refined mathematical tools is a real need (particularly if we consider working in foreign countries).

2 *Sources of opportunities concerning quantitative techniques*
Mathematicians, statisticians and co-operating natural and social scientists have developed methods and computing facilities, and they do not seem to stop doing so. Thus one can hardly complain about lack of material for learning.

The world-wide expansion of learning and practical experience opens up new territories for activity: local analysts and international teams may be expected to work on a much broader scale.

The growing demand for greater completeness enforces more intensive co-operation between people from various fields. Part of our failures have been due to poor knowledge of achievements in other disciplines but sometimes to lack of a necessary knowledge in a particular field representing a backward linkage, eventually a forward linkage. In genetics and in the domain of physiological processes there is a considerable amount of qualitative and quantitative knowledge to be used by agricultural economists. However, they can ask many questions relevant for their models which require further research in biology.

Sociology, psychology, organization research may also offer valuable information for people using quantitative techniques and may similarly receive questions leading to new lines of research. Since we live in the present world, it is preferable to have such a communication with political scientists too. Efforts toward linkage of national models offer one of possible reasons for justification.

DISCUSSION OPENING – FERNANDO C. PERES

This is a very interesting paper, especially for those of us who do not know how research is done in socialist countries.

Though the paper is entitled "Accomplishments, Opportunities and Needs of Agricultural Economists, *vis-à-vis* Quantitative Techniques" it emphasizes the *Accomplishments* and puts less emphasis on the *Needs*. Because I think it deals with the accomplishments part so well, I will stick mainly with the *needs* of agricultural economists in terms of quantitative techniques.

In the historical review, the paper places more emphasis on the use of mathematics for building the models and less emphasis on the testing. Testing the models requires a lot of mathematics to compute and understand all the statistics.

In fact, one can notice, by reading the paper, that economics can claim the status of a science already. Its methods are being used by researchers working under very different values or ideologies. We may make a "parenthesis" here and add to the review the book by Mishiro Morishima on Marxism which requires a lot of mathematics to be understood. He has shown that Marxist theory and neoclassical theory have a lot more in common than is generally thought to be the case. On the other hand, let me disagree with Professor E.O. Heady when he says that agricultural economists are generally well trained in quantitative methods. I think it is very difficult for many of us to understand Professor Morishima's work and consequently to be able to know many of these similarities between neoclassical and Marxist theory.

With respect to this, there is a point on which I would like to have Professor Petit's clarifying comments. As mentioned in the paper, the development of the economic tools followed different patterns in socialist and in western countries. We can understand socialist emphasis on "normative" models. The point is – is there any reason for research from socialist countries not doing (or starting at a later time to do) what we call "positive" economics? I think this is a relevant question in the sense that from the paper I could not know if statistical testing of the models is considered important by Joseph Sebeystén. If so, that indicates some more requirements in terms of mathematical training.

Let us talk a little more about how much mathematical training is required by agricultural economists. I guess nobody would argue with Waugh, as quoted in the paper, "A complex world requires appropriate techniques".

Specifically, in economics, one may think of what economy in writing and in research time could have been realized if John Maynard Keynes had taken his time and used his known mathematical abilities to put into equations what he *meant* in his writings. (It is said that Keynes did not like to use mathematics in economics, which is a shame.)

The British philosopher Bertrand Russell said, "The scope of all sciences is to reduce it to physics". One can argue that the general trend in our profession is just the opposite; that we should devote some of our time to looking to some *normative* kind of research, such as discussing the equity issues embodied in the work we do. If we accept Gunnar Myrdal's view that the most social scientists can do, in the sense of being objective, is to be explicit in their assumptions, then the need for the use of mathematics in economics becomes clear. The big advantage of mathematics is that it assures us that we will not violate our assumptions if we follow its rules. Of course, the assumptions we make may or may not be good approximations of reality and in this sense our journal editors could play a very important role in selecting papers to be published on a basis of their relevance in terms of realism, instead of elegancy or sophistication. Mathematics should not be blamed for people's lack of realism.

Another important point, which I think should be raised, follows from the fact that we are being urged to do problem-solving research, instead of doing, mostly, disciplinary work. Problem-solving research is, by necessity, interdisciplinary work. In doing this kind of work (which has not been a strong characteristic of our profession), we need a common language and I would like to quote Dantzing when he says "Mathematics is the language of science".

Maybe, I can give a testimony here from the Brazilian experience. Since the origin of our higher education system, it was mainly linked to the French system which was not, by tradition, mathematically oriented.

(I am referring to the social sciences.) We have not been able, until recently, to do team work and I suspect that other scientists did not accept us because we did not speak their language. Now the paper agrees with the need for some mathematics. My point is – I don't see any danger of our research being too mathematical. The way I see it is that one can be blamed for not going deeply enough into the phenomena one is studying. But to put the model into equation format and consequently, to be able to do quantitative predictions is superior (in terms of the advancement of science) to simple description. The point is not one of saying that descriptive works should not be done. What I am trying to say is that after the description of the situation one must put it into equations to be able to make useful predictions. Definitely a system of two equations with two unknowns gives us more information than the statement that X influences Y and X is influenced by Y in an interactive way.

Finally, a word about the big non-normative multisectoral models that are being built, as far as I know, by Western countries – the Fed-Mit-Pennsylvanian State Model, the Brookings Institution Model, etc. As mentioned before, a lot of mathematics is required if one wants to take a position on the relevance (in statistical terms) of these models. Even if one does not agree, based on statistical ground, that there is any relevance in building them, I would argue that they may be very useful in explaining all the calculations we make here in our computer system. Explaining those guesses requires a lot of mathematical training.

The paper also lacks any mention of cost–benefit analysis, maybe because a lot of value judgement is involved.

GENERAL DISCUSSION – RAPPORTEUR: I. TAKAHASHI

Dr Sebestyén's paper covered voluminous accomplishments in agricultural economics from the seventeenth century to the present, in both socialist and non-socialist countries, and in other fields of science which had close links with agricultural economics. The discussion on his paper was, however, mainly concentrated on the narrowly limited area of the *needs* of the agricultural economist, on which Dr Sebestyén's paper put less emphasis.

Two comments were made by others on the discussion opener's opinion of the use of mathematics. The first comment pointed out that mathematics was not always the expression of the whole truth in spite of its value and of its usefulness. The econometrician was faced with two constraints: the first came from his theoretical view of the problem. The results would be different between two methods; one fitting demand and supply curves separately based on the assumption that there was a lag between the action of supply and that of demand, and the other fitting together the two curves by means of simultaneous equations. The second constraint came from the method used by the econometrician. In this context examples were given.

The other comment on the discussion opener's opinion concerned the necessity of balancing quantitative and descriptive works. It was said that the proper balance should depend on the type of problem studied and, in some cases, quantifying attempts might result in some regression rather than progress of knowledge.

Finally, it was suggested that Dr Sebestyén's paper should include a full list of references for publication in the Proceedings Volume of the conference.

In reply, Michel Petit questioned whether we really knew if there was a lack of "positive" economics in socialist countries, as the literature is not readily available. If it is true, we should seek the reasons in the study of the philosophical foundation of investigations done in these countries.

On the use of mathematics he felt that Sebestyén would agree with Dr Peres, although he himself felt closer to Dr Dubos who had called attention to the limitations of all econometric works.

Participants in the discussion included Michel Petit (who read the paper on behalf of the author, who was not able to attend, and also responded to the discussion), Jean Dubos and Laurent R. Martens.