Recollections of Professional Experiences*

1. Introductory

For somewhat over fifty years I have had the privilege of being engaged in scientific work in the field of economics. I welcome the initiative of the Banca Nazionale del Lavoro in inviting a number of economists to put down in writing their "Recollections" on their most significant professional and personal experiences. These recollections may be of some use in (i) the systematic presentation of lessons for ourselves and others; (ii) preserving for our younger colleagues facts and developments in thinking which are not generally known to them, and (iii) avoiding the spread of myths regarding the history of economic science. A systematic approach seems preferable to, say, a chronological one. Yet the system chosen contains a subjective element, and we should therefore follow G. Myrdal's (1930) suggestion that the value system underlying it be mentioned as explicitly as possible. For my part, I have chosen a teleological system. The objective of scientific work should generally be, as I learned from my own physics teacher, P. Ehrenfest, to formulate differences of opinion in a "nobler" way than merely as conflicts. His favourite formulation was cast in the general form: if $a>b$, scholar A is right, but if $a<b$, then scholar B is right. The statement applied to a well-defined problem, and both a and b would generally be sets of values of elements relevant to the problem treated, with possibly a number of components of a qualitative nature. Such a "nobler" form of conflicting ideas was, I felt, the one chosen (around 1960) by Japanese opposition (socialist) parties who formulated their criticism of the ruling liberal (in the European sense) party in the form of an alternative development plan to that of the government. My formula for dealing with the East-West controversy proposal is on the same lines.

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* Second contribution to our plan of "recollections" by distinguished economists. The first of these — by Professor Hicks — appeared in the September 1979 issue.

This approach has the further objective of finding out the "truth" about the operation of an economic system or of the economic aspect of a societal system. Its final goal should, in my view, be the "discovery", or rather the "construction", of an ideal economy — to be preceded, of course, by its (subjective) definition.

I have been guided by these considerations in presenting my recollections. In addition, I have chosen to describe them as a set of contacts with persons on a particular problem. Each point in the following essay will therefore be a problem-related personal encounter. The choice of the points to be included is based on my experience with important persons I had the privilege of meeting, listening to or discussing with. For economists, important persons are not only fellow economists, or fellow scientists; they are also people with economic activities, including representatives of pressure groups, social workers or reformers and politicians, or finally just outstanding individuals. As regards economic science, my own interest is in economic substance rather than in methodological econometric issues. This point of view is reflected in my selection of problem-related personal issues in these recollections. Hence, in certain respects, I have foregone a number of opportunities simply because they did not come within this focus. Finally, my encounters reflect my luck in meeting certain eminent persons and my misfortune in not meeting others. Thus, I just missed meeting J.A. Schumpeter on a number of occasions.

Finally, I want to apologize to the many persons to whom I am greatly indebted but who are not mentioned in these recollections. As usual, any errors committed are my own, to adopt the usual formula.

The main structure of my essay follows a not uncommon subdivision of economic thinking which happens to coincide roughly with a subdivision of my own activities in certain broad periods. I first deal with short-term economic movements and policies to influence them; next, with the explanation of economic structures and long-term movements, and then with the problems inherent in comparisons of alternative structures. Whereas these three sections (2, 3 and 4) deal with the substance of economic science, a very modest Section 5 reports a few encounters of a methodological and a didactical nature. Finally, Section 6 discusses issues relating to communication between economists and between scientists generally and to multidisciplinary research.

The men from whom I have benefited most were those with whom I was fortunate enough to collaborate on a daily basis — for example, J.B.D. Derksen, P. de Wolff, W.H. Somermeijer, C.A. Oomens, J.J. Polak, M.H. Ekker, F.L. Polak, J. Sandee, G. Stuvel, A.I.V. Massizzo, R.J.P. van Glinstra Bleeker, D. Groenveld, E. van Cleeff, H.C. Bos, H. Linnemann, L.B.M. Mennes, J.G. Waardenburg, P.A. Cornelisse, and J. van Ettinger. Given the daily discussions of numerous problems that are inevitable in such a relationship, these friends do not fit into the problem-related formula.

2. Short-term Economic Movements and Policies to Deal with Them

By short-term economic movements I mean those extending over a few years and at most a decade. They are usually referred to as cyclical. It was after the First World War that their systematic study was initiated, with the aid of such concepts as the Harvard Business Cycle Barometer, by an American group and by a group of London and Cambridge economists, in which W.C. Mitchell and A.L. Bowley were respectively the leading figures. Mitchell took a highly empiricist line, and was later sharply criticized by T.C. Koopmans (1947). Bowley (1924) was also interested in, and an authority on, problems of mathematical economic theory, but kept both fields somewhat apart. In Germany, towards the end of the nineteen twenties, E. Wagemann had succeeded in bringing together, in the Institut für Konjunkturforschung (Institute of Business Cycle Research), a number of able young scientists, some of whom are mentioned below. These engaged in a type of research which, from 1930 on, was to be called econometric in the Anglosaxon world. The Great Depression starting in 1929 gave a marked fillip to quantitative research of an integrated theoretical and numerical character, and the Economic Intelligence Service of the League of Nations, headed by A. Loveday, assigned to G. von Haberler the task of writing a survey of the numerous theories of the business cycle formulated in the course of the previous fifty or so years. The fruit of these labours was the famous book, *Prosperity and Depression* (1937).

Most of these theories had never been tested statistically, and it was even an open question how such testing could be carried out.

Up to 1935, single relationships, such as the demand function, had been the subject of testing. The standard two-dimensional diagram, with quantity demanded, x, plotted against price, p, was constructed with the aid of actually observed figures for a number of years for a given commodity, starting with all agricultural products. Such a diagram could only be accepted as representing a demand function, if the influence of variables other than p could be neglected or eliminated. Otherwise, the function could just as well be claimed to be a supply function. The other variables appearing in a demand function are usually called "demand factors". The most important of these for consumer goods is income y of consumers. Similarly, supply factors are defined, unit cost of production c_1 and production capacity c_2 as important concrete examples. In Germany A. Hanau, one of Wagemann's collaborators, made a statistical analysis of the market for pork (1928); and, in the United States, the Agricultural Adjustment Administration produced large quantities of demand functions for agricultural products. Somewhat later (1938), H. Schultz published his standard work, *On the Theory and Measurement of Demand*. Agricultural and fishery products were characterized either by a low income elasticity (for basic necessities) or by extremely wide fluctuations in quantities supplied, which made it possible to neglect influences other than that of price. In many cases, too, the quantities supplied could be considered as given (which implies that their price elasticity was negligible).

An elegant example is the Dutch anchovy market, for which data are available over the period from 1855 to 1930, when the closing of the Zuiderzee disturbed the functioning of the mechanism. Annual catches vary in the ratio of 200 to 1, and a biological explanation of these fluctuations was provided by H.C. Redeke. The main factor involved was the March temperature in the Marsdiep (part of the sea around the city of Helder, that part being the nursery of the anchovies). A second interesting feature is that, that fish, after being salted, has to be stocked for four years to reach top quality and to be fit for canning. As a consequence prices clearly vary negatively with the total of the last four years' catches. This reflects the influence of total 4-year catches price. These findings have never been published.

A somewhat different example of a supply lag was implied in Hanau's pork market analysis. Farmers need some time to react to a favourable (or unfavourable) price, and the growth process of the pigs causes a further delay. In all, about 1-1/2 years after a period of high prices, a large quantity of pork comes on the market, which then depresses the price as a consequence of the demand function. The result — apart from some complications caused by price changes of fodder — is a roughly 3-year cycle in the pork market. This mechanism, known as the cobweb theorem (because of its graphical presentation), constitutes the simplest example of a family of explanations of cycles in individual markets. The longest cycles of this type were found by Roth (1929) for the coffee market in the latter part of the nineteenth century and the beginning of the twentieth century. This mechanism is somewhat more complicated because the time taken by the coffee trees to ripen creates a capacity to produce coffee for the life of the tree. The pig is killed, but the tree is merely stripped of its beans.

The main problem of the business cycle was not connected with a single market, however, but with the complete economic process of investment, production, consumption, and price and income formation. J.M. Keynes' model leading to the famous multiplier concept did not try to explain the period of the main cycle (also called the Juglar), which was found to be around eight years (with a tendency to shorten from about ten to about seven years between 1800 and 1930). Two main features of business cycle theory testing, consequently, are that (i) a whole system of relations had to be formulated and tested statistically, and (ii) it was essential to find out which of the elements in these relations could explain the relative constancy of the cycle's period.

The systems of equations or models came into existence in 1936 (Tinbergen, 1936) with a system for the Dutch economy. They may be said to be extensions of the Keynesian equations of income formation and consumption expenditure. Additions were made by the splitting up of money flows into a price and a quantity factor. Further additions consist of the introduction of imports and exports, and of employment and wage rates. Investment was not equated by definition to savings. In a number of the equations, lags were introduced. Consumption as well as investment were assumed to lag behind their explanatory variables such as incomes or profits. Prices were assumed to lag behind import prices as given by the world market and quantities demanded. The Dutch example was followed by one for the United States (Tinbergen,

1939), first by the present author in the framework of the Loveday initiative and as a follow-up of the Haberler assignment, later by L.R. Klein (1950), and still later by the Brookings Institution with a team of American econometricians including Klein (Duesenberry *et al.*, 1965). One relationship suggested for the explanation of the fluctuations in investments may be mentioned separately. It is called the "acceleration principle" and was initially illustrated by Haberler, using the relation between the demand for shoes and that for investments in shoe factories. In the illustration, full capacity production in the shoe industry was assumed to be permanent. Clearly in those circumstances, investments are proportional to the rate of increase in shoe production. Taken literally, the acceleration principle would imply cycles in investment of much larger amplitude than, and preceding by a quarter of the cycle's duration, the movements in consumer good production. Later, it was shown that the principle did not give a very precise picture of reality (Tinbergen, 1938), and subsequently the principle of the "flexible accelerator" was developed. This new principle implied that investments were equal to a fixed portion of the deviation between required and actual production capacity, and required capacity equal to the demand for the consumer good concerned. A major contribution to this discussion was made by the late L.M. Koyck (1954). On another occasion, an attempt to apply the rigid acceleration principle to investments in stocks was somewhat more successful (Tinbergen, 1942).

The problem of the explanation of the relatively stable length of the cycle had two potential answers. One type of element making for cycles was that of the various lags appearing in the model. This was tried out for the first US model, and around 1951 for a model for the United Kingdom for 1870-1910. An unsatisfactory property of these models is the extreme sensitivity of the period to changes in the lags and to the coefficients appearing in the equations.

The alternative explanation of the period is due to the so-called "echo theory", formulated by S. de Wolff (1929). Here it is the average (economic) lifetime T of capital goods which determines the cycle's duration. If, for some reason, at some time t an investment boom occurs, there will be an echo of this boom at time $t+T$. This will only lead to an undamped cycle if all capital goods have exactly the same T. De Wolff was critized by Vos (1938) and

others, who showed that the (technical) life time T' of machines showed a considerable dispersion, leaving practically no second echo after a very weak first echo. It can be easily shown, however, that an undamped cycle can be obtained if the time of replacement is determined not only by technical life time T', but also by the cyclical phase of the economy.

A problem related to those just discussed is the number of cycles which a model produces. This number depends on the number of roots of the characteristic equation of the model, which are either negative (leading to cycles of a period of two time units) or complex. This problem has not received much attention except in a theoretical model, owing to the role of random components which will now be discussed.

Observations had made it abundantly clear that — in contradistinction to some cycles observed in biology (the movement of certain animal populations over time) and of numerous phenomena in physics and astronomy — economic variables show highly irregular or "disturbed" cycles. In other words, random or stochastic components occur in all economic variables.

Some variables may even be said to be practically entirely random in character. This applies to agricultural yields (crop per hectare) and approximately also to total crops, because weather conditions are of overwhelming importance here and are themselves practically random. The question may even be posed whether or not the so-called Kitchin or short "American" business cycle can be largely explained by the random character of crop figures, which are certainly important for the American economy. Assuming that crop figures have a symmetrical probability distribution, it can be shown that the probability of the occurrence of an upper turning point in the broken line of annual crop figures is $\bar{W}^2 = 1/4$. Here, W constitutes the cumulated probability of a crop figure above a given crop x, assumed to obey $\bar{x} < x < \bar{\bar{x}}$ and, because of the symmetrical probability distribution of x, $\bar{W} = 1/2$.

Purely random variables are an extreme case, however. Most variables in an economic system are composed of a systematic and a random component, the former being determined by the systematic terms in the system's equations. Econometric estimation procedures for these equations are as a rule based on this pattern. One exception to this approach is the one suggested by the brilliant Dutch engineer-economist, J. Goudriaan (1934), who considered the ec-

onomic system to be indeterminate between an upper and a lower level for each of the variables. I made an attempt to incorporate his idea and J.G. Koopmans' concept of multiple economic equilibria (J.G. Koopmans, 1932) in a single scheme (Tinbergen, 1944).

Another aspect of the role of stochastic elements in the business cycle is that a quasi-cycle can be obtained by the cumulation over some time interval $0<t<T$ of random figures r_t. Any economic variable x could then be written as:

$$x_t = a_0 r_{t-T} + a_1 r_{t-T+1} + \cdots\cdots + a_T r_t$$

This is not essentially different from the econometric models described earlier: the systematic elements being now represented by the a_t (t=0, 1, ..., T).

A remarkable generalization of the simple models dealt with so far was offered by B. Chait (1938), who introduced systems of markets connected either vertically or horizontally. By the former term, he indicates markets of raw and processed materials; by the latter, the markets of products made from the same raw material (for instance cotton yarns of different numbers).

In a number of early American models, the monetary-financial sector was oversimplified. Only one paper asset, money, was considered, and bonds, stocks or short-term paper such as bills left out of consideration. Although ample attention was paid to the markets of these assets in the League of Nations model, in close collaboration with J.M. Fleming, these elements of our model were never mentioned in the reviews. No reactions, either, were received to my attempt (1947) to build a bridge between flow and stock approaches to markets of financial assets. Only much later was the monetary-financial element introduced into business cycle models; at present, close attention is paid to them in various models, including the Brookings Quarterly Econometric Model of the United States (F. de Leeuw, 1965).

Whereas Haberler's standard work was entirely devoted to the primary aim of economic science (the explanation of cyclical movements), the econometric models were from the start also intended to suggest policies. This distinction between the use of the same model for explanatory (analytical) and normative (policy) purposes was the subject of a few of my publications (Tinbergen, 1952, 1956), and the definition of four types of variables is chosen as the

starting point (data, target variables, instrument variables and "other" or "irrelevant" variables). The difference between the analytical and the political problem was seen in the groups of variables which are the unknowns and the given groups. Several authors, when quoting me as the "inventor" of these definitions, overlooked the fact that they are due to Frisch, who introduced them in a little-known paper for the Employment Commission of the United Nations (Frisch, 1949).

I gave an example of such a model and its use for political purposes in the latter of the two publications just quoted (Model 16 and Problems 161 and 162). It appeared that the coefficient matrix of the political problem shows an almost triangular structure, which, in H.A. Simon's (1953) terminology, means that it is of a very high "order", in contradistinction to the matrix of the analytical problem using the same model. A high order of the matrix and the solution implies that the unknowns can be determined in a logical succession of solutions; in the case of the highest order, each subsequent solution determines one additional unknown. This again means that, for the knowledge of the first unknown, only the coefficients and given variable of one equation is needed; for it, all other coefficients and given variables are indifferent. For the determination of the second unknown, only the coefficients and the given variables of the two first equations are relevant, and so on. Using more concrete language, the first instrument variable sought depends only on one target and two coefficients; the second instrument depends only on two targets and at most five coefficients. In contradistinction, cases are conceivable where only simultaneous determination of all unknowns is possible. This happens to be the case in the rather realistic model discussed (Nr. 16) for the analytical problem. Here, Simon would speak of one order only, implying that the explanation of the unknowns of the problem (the target variables and the "other" variables — those which the economist wants to explain in terms of the data and the instrument variables) is possible only with the aid of all given entities (variables and coefficients). This illustrates the well-known proposition that in an economy "everything depends on everything". This statement, therefore, has limited validity, as Problem 162 shows. Problem 161 is an example of a problem which is insoluble, even though the number of equations equals the number of unknowns. What happens there is that one part of the equation contains more unknowns

than equations and is indeterminate, whereas the other part contains fewer unknowns than equations and is overdeterminate.

In contradistinction to the statement that "everything depends on everything", some economists and politicians think that each of a number of targets can be attained by the manipulation of one instrument. As a rule, this is not correct; it requires more than triangularity of the matrix of coefficients. It requires that, after the elimination of the "irrelevant variables" (those that are neither targets nor instruments), we are left with a system of equations whose coefficient matrix is diagonal — a very rare case indeed.

For present-day discussions about the validity or non-validity of neo-classical economics, I would like to put on record the fact that, from the start, my models contained certain equations which are not necessarily neo-classical, but rather of the "satisficing" type, i.e., not derived from the maximization of some utility. The "price-setting" equations, which are comparable with supply equations, are of that type; they express the well-known "satisficing" device that prices charged by industrial producers are of the cost-plus type, where the profit margin need not correspond to maximum profits.

I should point out, incidentally, that the models in use at present in the Netherlands Central Planning Bureau are far more complicated than those in use around 1955, as illustrated by my Model 16 — even at that time a simplified model. Credit must be given to numerous colleagues, including P.J. Verdoorn, C.A. van den Beld and H. den Hartogh, to mention only a few Dutch economists, and to innumerable other European and American model builders as well as to some from the Third World [S. Chakravarty (1959), N.V.A. Narasimham (1956)].

To conclude this section, I should mention a number of models for single market or market complexes, e.g. those for transportation by ship [Tinbergen (1931), assisted by B.G.F. Buys], or by tanker [T. Koopmans (1939)], those explaining share prices (O. Donner, another staff member of Wagemann's institute, and later a Director of the IMF) and one for beef prices (Dutch Central Bureau of Statistics, 1931, 1933).

3. Explanation of an Economy's Structure and Long-Term Movements

It has been customary for business-cycle research to use the phrase "structure" in two somewhat related, yet different, meanings — one being the more or less lasting structure in the sense of the relative proportions of the parts or sectors of the economy, in all senses, and another in the context of structural movements or long-term, usually slow, changes. On some occasions, a revolution may bring about a sudden change in structure or a structural break. Much later, J. Pen (1974) made an attempt to compare the manifold meanings given to the phrase "structure" in various sciences.

An appropriate link between the present section and the preceding one is the suggestion by J.G. Koopmans (1932), already mentioned, of the possible existence of more than one equilibrium position of an economic system and the subsequent possibility of the economy sliding into a lower equilibrium, a point which several economists were wondering about in the Great Depression, when that appeared to be so persistent. Might not the economy be brought back to the higher equilibrium (especially of production and employment) with the aid of a "big push"? Perhaps this is a consideration deserving study in our present-day (1979) situation as well.

An important feature of the world economy or of parts of it, such as the South and the West, in present-day *jargon,* is the international division of labour. A long series of studies on this topic, often based on the concepts of comparative cost and international and interregional trade [the Heckscher-Ohlin concepts, cf. B. Ohlin (1933)] started with G.D.A. McDougall's (1951,2) analysis of American-British competition, which was studied extensively by H. Giersch, J.B. Donges, G. Fels, A.D. Neu and others of the Kiel Institut für Weltwirtschaft (Institute of World Economy) and by many other economists. It is difficult to single out names, but B. Balassa (1979), C.P. Kindleberger (1954), B. Herman (1975), and A.H.M. Mahfuzur Rahman (1973) were those I came across most often in empirical work. Their work has brought out the essential features of the world division of labour and the deviations from an optimal division. The Third World or group of Less Developed Countries (LDCs) was originally forced into the

production of agricultural and mining products (''primary'' commodities), but is now in the process of becoming increasingly competitive (again) in a number of manufacturing industries (producing ''secondary'' products), mostly those requiring relatively large inputs of (their own) raw materials and of less skilled labour, and preferably not requiring much capital — physical or human. It is natural, therefore, and in the long-term interest of all, developed and LDCs, that such industries be shifted from the former to the latter. This process is hampered, however, by the lobbies of vested interests in the developed countries.

Whereas many problems of the international division of labour and of international trade have been discussed in theoretical studies without empirical backing, the authors quoted have contributed in the latter respect, and thus have helped to elaborate more concrete ideas about trade policies. An example of the irrelevance of some purely theoretical work is the famous factor price equalization theorem. This states that factor prices (and hence, wages and interest rates) can be equalized among countries not only by free migration and capital transfers, but also by free trade in products without free migration and capital transfers. This is only possible, however, if the differences in capital per capita required by various industries is of the same order as the differences in capital per capita endowment of the countries of the world. The latter differences are far larger, however, than the former.

Shifts in international trade can be induced by changes in price levels of the goods traded. A famous book by J.M. Keynes (1919) tried to show that the war indemnity required by the Allied Nations from Germany after the First World War was completely unrealistic, since the export possibilities of Germany were limited as a consequence of the limited price elasticity of the demand for Germany's (and any country's) export goods. In this study, Keynes took this elasticity to be −2. His argument would have been weakened considerably if this elasticity had been assumed to be, say, −10, or to be equal to the theoretical value of minus infinity. Hence, some of my collaborators in Holland and myself undertook a series of econometric studies in order to estimate the elasticity's value. We actually found values around −2, and I told Keynes so, expecting that he would consider this to be a strengthening of his position. His reaction was different; ''how nice for you to have found the correct figure!'' Sometimes, indeed, intuition constitutes a basis for new scientific results. It should be the intuition of a genius, however. For simpler souls, intuition may be less reliable!

An important complement to the theory of international division of labour is the concept of non-tradables as distinct from tradables, used by I.M.D. Little and J.M. Mirrlees (1968), and using the terms of national versus international commodities in my own studies. It becomes particularly important when estimates show that roughly one-half of most countries' national product consists of non-tradables. Concrete examples are very heavy goods, such as buildings, roads, railways, and a series of services which have to be available to the consumer at the point where he is located (personal services, retail trade, schools, etc.) or to be part of a national unit (government, with its many ramifications and levels). One important consequence is that a reduction in national expenditures — required in order to re-establish balance of payments equilibrium — implies a reduction in national product equal to the reduction in expenditures on non-tradables. (For a simple algebraic treatment, cf. Tinbergen, 1965.)

The existence of non-tradables also affects the composition of investment projects. Assuming the full use of productive capacity in all sectors, a new shoe factory cannot be established as an isolated object. It requires an extension of the production of electricity or of some other form of energy, of transportation of products, raw materials and ancillary goods. Hence, investment projects are bunches of investments, and the criterion of eligibility as part of a nation's development must be applied to such a bunch. In the example of a shoe factory, an extension of leather production is not necessary, though, since leather can be imported. Here, the use of the classical input-output analysis is not desirable, since its tendency is to force upon the bunch all activities linked with, in our case, shoe production. Excluding the tradable inputs also simplifies the calculation of the composition of the bunch. The rather extensive discussion around this so-called semi-input-output method has been very capably integrated by A. Kuyvenhoven (1978).

The international division of labour can be seen as part of an extended type of economics to be called space economics. Here space is not (yet?) outer space. For the time being, a two-dimensional surface space will suffice. Space economics implies that a geographical location (by, say, two co-ordinates) is attached to each economic variable. Production is split up into production in a

number of sub-areas of the economy considered. Prices may be different in these sub-areas. Transportation may have to be specified as transportation between any two locations distinguished, and so on. This implies an enormous increase in the number of variables entering into the models. This new science is only in its beginnings. It is beyond my capacity to describe the present state of this art. Leading names include those of W. Isard, L.H . Klaassen and J.H.P. Paelinck.

A second dimension of an economy's structure is its income distribution. An impressive early contribution to this subject was made, again, by R. Frisch (1932), where he presents a theory of rent in a given geographico-agricultural environment. It is another brilliant piece of analysis and exposition by this author.

Most other studies on income distribution deal with distribution between factors, or, going a step further, between individuals. Factor income distribution started with a simple subdivision of factors into three or four, (i) nature or "land", (ii) labour, (iii) capital, and (iv) entrepreneurs. The latter were assumed to organize production (the source of all income) by hiring quantities of the other factors and to compete for these factors. An implicit assumption added was homogeneity of the other factors, though not necessarily of land. Since the countries for which figures were available were industrialized countries (DCs), where the role of primary production has become small, nature as a factor was often neglected; or the analysis was restricted to the secondary and tertiary (services) sectors. The outcome of the analysis outlined here is that labour and capital are being paid their marginal product. Whatever remains as a residual is appropriated by the entrepreneurs. The marginal product can be derived from the production function, and so the two subjects of a) factor income distribution and b) the estimate of production functions are closely connected. The pioneer in the field was undoubtedly P.H. Douglas (1934), in cooperation with C.W. Cobb. The production function proposed by them was characterized by constant elasticities of product with regard to each of the production factors hired. As a consequence, these factors as a whole would receive a constant portion of national product. Figures collected by Kuznets (1966) show, however, that, between around 1850 and 1950, income from capital in Britain and France fell from about 40 per cent to little more than 20 per cent; and in the USA at the same pace between around 1900 and 1950. For other reasons,

too, the Cobb-Douglas production function is less satisfactory. Hence, a considerable series of other functions have been tried out. Some of the best known among them are the CES function (which has a constant elasticity of substitution between factors, but not necessarily with the value of -1 shown by the Cobb-Douglas function) and the translog function (which has a higher degree than one in the logs, valid for the Cobb-Douglas function, when written in log-form). Recently a real industry of inventing more sophisticated production functions has developed. Very often, they have only been tried out on the assumption of homogeneity of such a factor as labour, while sometimes introducing energy as a separate factor — which is useful because of the energy scarcity (cf. E.R. Berndt and D.O. Wood, 1979).

Alongside the distribution of income among factors, personal income distribution has attracted the interest of economists, with a well-known contribution by V. Pareto (1897). This contribution, like the one by Champernowne (1953), belongs to the stochastic category, which lacks an economic interpretation for individuals or occupational groups of individuals, and, as a consequence, lacks the possibility of giving rise to an economic policy directed at reducing income inequality — a generally accepted aim of socio-economic policy. This has induced me to present a theory in which the homogeneity of labour as a production factor is replaced by the introduction of a considerable number of types of labour, characterized by the value of a number of relevant qualities. Simultaneously, I have introduced the distinction between the demand and the supply side of each of the many compartments into which the labour market is then subdivided. Although the theory was put forward in 1956, attempts to check it with empirical data only started after 1970, and were summarized by me (1975). Part of this verification used the assumption, rather unpopular among present-day economists, that utility can be measured, and here I gratefully made use of the material collected by B.M.S. van Praag (1971, and later). Other attempts at verification, in close co-operation with J. Hartog (1978), who had developed a multicapability theory with impressive empirical results, did not use the assumption of measurable utility. Very little support was found for what I called the "tension theory" of utility, that is, the hypothesis that utility is negatively affected by a difference between the intensity of a quality required for a job and the intensity of that quality present with

the person on that job — whether the difference is positive or negative. R.H. Haveman (1977) raised a considerable number of questions about the testing procedures I had used, most of them justified. This was a repetition of T. Haavelmo's (1944) criticism of my business cycle models, where I had used weak, if not wrong, methods of statistical testing (least squares for each equation). In both cases, my lack of interest in the mathematical-statistical aspects of testing wreaked its revenge. T.C. Koopmans, H. Theil and many others developed the more appropriate methods now available, known as maximum likelihood, two- and three-level least squares etc. R.H. Haveman had further criticisms to make of my 1975 *Income Distribution: Analysis and Policies*, and I profited greatly from these. I think I have found the correct answers to some of his queries, for instance the one regarding my neglect of the costs of education in my first attempt to find an optimum distribution of education (cf. Tinbergen, 1978).

Work of an empirical character undertaken by J. Hartog, J. Berkouwer and myself brought us into contact with students of education, for instance, J. Dronkers (1978/9), who followed Jencks (1972) and Fägerlind (1975). The result of this collaboration was the conviction that geneticists and sociologists had neglected the relevance of certain non-cognitive qualities (such as leadership or sense of responsibility), whereas income distribution economists had not grasped the importance of distinguishing between the demand and the supply side in labour markets. For the time being, unique empirical material on both issues has been collected by J. Berkouwer (still to be published) with the aid of the Dutch Gallup Institute (NIPO) and the personnel department of the Dutch transnational enterprise AKZO.

Finally, R. Haveman, J. Pen (1977) and T. Huppes (1977) have made it clear that, regardless of demand and supply power factors, as reflected in social security schemes resulting from trade union pressure also affect income distribution. Another example may be managers' incomes, if only because their incomes are the result of demand and supply being in the hands of the same sociological groups. Kolm (1978) may well be right in his contention that managers' incomes are higher than is needed to induce them to continue their work.

In the present situation where almost all social groups try to raise their incomes as much as they see fit, readers may be reluctant

to believe that, in the Netherlands, between 1945 and 1960, trade union leaders were willing to accept real wage reductions in order to safeguard employment. Apart from the national solidarity prevailing after the Nazi occupation which was essential if the badly damaged Dutch economy were to be rebuilt, it is perhaps also of great importance that trade union leaders, A. Vermeulen and D. Roemers, were university-trained economists, who had a complete understanding of the arguments of the Central Bank President M.W. Holtrop, or of ministers H. Vos and W. Drees.

At that time, the ILO had difficulties in evolving from an institution essentially created by trade union interests of the developed countries (to protect wages against "social dumping") to an institution defending the interests of all workers and poor in the world. By 1969, at its 50th anniversary, however, it had launched the excellent idea of developing a "World Employment Programme", with much greater emphasis on employment in LDCs.

So far, we have dealt with structure as an instantaneous concept. Let us now take up the related concept of structural or long-term movements. In the early phases of the development of economic science, this subject was dealt with in the form of what W.J. Baumol (1951) later called "magnificent" classical dynamics, which was only slightly connected with measurement. Statistical information was limited in those days. Measurement came in with the desire to break down time series into a number of components, often called trend, cyclical, seasonal, and random components. The trend component was given a mathematical form which seemed best fitted to the long-term movement of the series; from a straight line to a parabola or from a different algebraic power curve to an exponential one (increasing "geometrically"), or finally more complicated functions such as a logistic one, that is, a curve with a lower and a higher horizontal asymptote. There was no theory behind the choice, hence, no "theory of development". Elements of such a theory were available in theories of population with a constant birth rate (leading to an exponential curve), or theories of population with a given and limited food base (leading to a logistic curve); or theories assuming a growth of capital as the consequence of a constant saving rate (again providing us with an exponential). These elements had to be combined, and as a rule the result would be more complicated curves. Almost simultaneously, Colin Clark and the present author presented (in 1942) a first attempt to formulate

such a theory; but the theories were in fact different. Clark chose to forecast the world economy's situation in 1960 and expected agricultural prices to have almost doubled over pre-war days. J. von Neumann (1935/6) integrated labour as an input and an output, and hence as an endogenous variable, one of his well-known findings being that the interest rate and the rate of growth are identical. T.C. Koopmans (1970) and M. Inagaki (1970) paid attention to the difficult problem, already posed by E. von Böhm Bawerk (1884), of the psychological discount rate to be applied to future consumption in the choice of the savings rate. Both authors used very ingenious constructs, and it is a pity that a comparative evaluation of the two approaches has never to my knowledge been published. In passing, we may remind ourselves of the hard truth that a nation may be endowed with too little capital to survive without outside help. This may apply in particular to a country whose natural endowments require a long roundabout way of production, for instance forests.

An interesting problem of the roundabout way of production arises when the "specificity" of capital goods is introduced; that is, the fact that, as a rule, each consumer good is produced by capital goods of a specific character: tissues by (weaving) looms, clothes by sewing machines, furniture by woodworking equipment such as saws and planes. Theoretically, one might introduce capital goods of the first order which themselves have to be produced by capital goods of the second order, etc. If specificity went on indefinitely, the production of more of certain consumer goods might involve a time lag of infinite length, which fortunately is not the case. Theory should therefore contain the statement that, at a certain point, non-specificity comes into the picture. Lathes may be one name given to the typical non-specific capital good; machine tools another (cf. Tinbergen, 1960). The author knows of no empirical research on whether a boom in investment as a whole is preceded by one in machine tools.

Similar situations are to be found in investment in "human capital" — a concept due to J. Mincer (1957) and his colleagues G.S. Becker (1964) and T.W. Schultz (1963). Teaching requires teachers, and teaching the teachers requires teachers of the "second order". In practice, three main levels of teaching or schooling can be distinguished, and by now third-level education in most countries is the level at which non-specific education is lumped together

with the otherwise very specific forms of higher education. Models of a smooth path of development adapted to a given smooth path of general production of commodities were set up by H. Corea and this author (1962), and models of the process of shifting from one smooth path to another added somewhat later. A far more sophisticated model including a model for income distribution was constructed by J.M.M. Ritzen (1977).

Schumpeter's definition of an entrepreneur as somebody who innovates production processes ("brings new combinations" of production factors) (1912) applies to a relatively small part of those who actually manage production. An excellent, but rare, example is the Belgian firm Picañol (Messrs Steverdinck) at Ieper, Belgium, which transformed itself from a textile exporter to a textile machine (simple automatic looms) exporter. Similarly, the Dutch State Mines (DSM) switched from coal mining to chemicals. Large transnational enterprises behave more in accordance with Schumpeter's ideal than many family firms. The latter often take the easier path of asking for commercial protection — against the long-term interests of both the firms themselves and their competitors in LDCs.

The development of LDCs has become the major issue of economic dynamics after the abolition of colonialism. As is well known, P.N. Rosenstein Rodan has been the pioneer in this field, by devoting attention to the possibilities for development of South-Eastern Europe even during the Second World War. He continued to take a leading role by estimating the necessary capital transfers for development from DCs to LDCs (1961). It is impossible to do justice to all those who in recent decades have contributed to this debate. Let me mention only E. Mason, H.W. Singer, and R. Prebisch as a few who greatly impressed me by their personalities in more respects than their scientific talents. I will revert to the subject in a broader context in Section 4.

A burst of scientific innovation was elicited by the discovery of the "new scarcities" by ecologists (or ethologists), later on brought to the notice of a much wider public by the Club of Rome, whose driving force was and is A. Peccei, surrounded by a few others, including A. King. It was by their work that modelling methods seeking to forecast world development over several decades came to the fore. The main protagonists were J. Forrester (1972), the Meadows couple and collaborators (1972) and, along different lines, W. Leontief. Whereas the latter are all Americans, Europeans too made

contributions, including the Sussex group and the Berlin Wissenschaftszentrum, with ramifications in both the USA and Europe.

Most of these newer developments point to possible "limits to growth" which are questioned by a relatively small group of optimists such as W. Beckerman and H. Kahn.

4. Alternative Structures Compared

The socio-economic structure characterizing the industrial nations in the middle of the nineteenth century was baptized as capitalist by Karl Marx (1867). The overall view developed by Marx was that society was not a constant structure but a changing one. For him, capitalism was preceded by feudalism and to be succeeded by socialism. Power would shift from landowners to the owners of capital (or man-made means of production) and later to labour. His ideas have had an enormous impact on social life and international politics. These ideas have been elaborated by a number of other thinkers, most of whom claimed to be Marxists, or the only true disciples of the Master. Marx was an original thinker, and there are good reasons for calling him a scientist. Among his ideas was the thesis that socialism as he saw it would necessarily ("naturnotwendig" in German) emerge out of capitalism. Among the Marxists there has increasingly developed a state of mind which is religious rather than scientific. Those who followed Lenin — e.g. the present rulers of Eastern Europe — considered militarism to be necessary. It is difficult to ascertain whether this militarism is defensive or offensive. In the latter case, the automatic development towards socialism would have been questioned by the Eastern-European leadership; and Dubček's overthrow would seem to confirm this. Among the communists in Western countries, Togliatti and Berlinguer are clearly the most independent politicians, together with the Yugoslav leaders,who are regarded as being non-committed to either East or West.

Socialism is defined in different ways, ranging from a structure in which all means of production are owned by the community to one where the ultimate responsibility for socio-economic policies and institutions lies with the community. In all these senses, socialism is an alternative to the liberal order (or "laissez-faire"). The way the author has attempted to defend his own socialist approach

is by using the scientific instruments developed by the proponents of that order in order to show that laissez-faire is not optimal; the main instrument for this defence being welfare economics, as created by Pareto (1897) and summarized and elaborated impressively by J. de V. Graaff (1957). The central problem dealt with by welfare economics is the determination of the conditions to be fulfilled in order to maximize social welfare. One fundamental question dividing economists and constituting a major issue of scientific strategy is whether personal satisfaction (also called utility or ophelimity) is measurable. At present — it has not always been so — the majority of economists denies measurability and they have to employ long and exhausting procedures to arrive at theses which are correct independently of measurability. The present author prefers a strategy based on the assumption of measurability of satisfaction. This strategy permits important savings in mental activity in the formulation of political conclusions. One important advantage of the strategy defended is that its alternative — the assumption of non-measurability — leaves a number of political decisions to people not trained in economic science. Another advantage is that the assumption of measurability of utility enables the economist to give a sense to the concept of equity or justice in distribution (of income and sacrifice).

The thesis of the non-optimality of laissez-faire is independent, however, of the question of measurability. Welfare economics is able to show that laissez-faire cannot be proven to satisfy one set of optimum conditions, namely the conditions that the marginal utilities of all citizens must be equal.

In the debate between communist regimes and Western politicians, a question of terminology arises which can cause confusion and calls for an effort to avoid subjective statements. It is the question how to define the present structure of Western industrialized societies. The term "capitalist" is a misnomer, consciously maintained by leftist and rightist extremists. The present structure of Western societies may be given various names, but "capitalist"suggests an identity with nineteenth century conditions. For lack of a better name, we may choose the adjective "mixed"; even better, perhaps, would be "on its way to democratic socialism", or "on its way to optimality".

The latter definition leads us to the famous discussion of the theory of "convergence" launched by the sociologist, P.A. Sorokin

(1960), and taken over — at first with a question mark, but later on without it — by the present author (1961, 1966) together with H. Linnemann and J. P. Pronk. For obvious reasons, the theory came under heavy fire from both West and East, though what extremists on both sides want is in fact convergence — in their own favourite structure! What was overlooked by many critics, too, was that (i) the theory was based on a Marxian view and (ii) that convergence need not imply convergence on identical positions. A major contribution to the scientific debate was made by J. van den Doel (1971), who pointed out several weaknesses of the original presentation and added new analyses. This was only a preludium to later work by that author (and by Mrs. T. Grondsma van den Doel) which extended methods of economic science to political science. The results include a convincing plea for an incomes policy, which applies, to be sure, not only to wage earners.

The thesis that the mixed order itself will undergo a further evolution is now stressed by a considerable number of authors of rather diverging opinions. G. Adler Karlsson (1975), who introduced the concept of "functional socialism", as well as N. Macrae (1976), are among the most imaginative authors, along with a number of environmentalists too numerous to do justice to them all. One economic feature, a further reduction in income inequality, was stressed by J. Pen and this author (1977). Economists may well have to realize that the more important forces needing analysis may well lie outside the traditional economic field, *n'en déplaise à Marx*. Some features of cultural degeneration, long ago described by G. Duhamel as "American", now apply to all Western and perhaps partly to Eastern societies. Here E. Eppler (1975) is certainly right in his plea for "conservatism in moral norms and progressiveness in policies". Perhaps my generation should give the benefit of the doubt to part of the younger generation, following the example of Th. van der Waerden vis-à-vis younger people in the 'thirties who propagated the Dutch Labour Plan, impressed by H. de Man, the Belgian politician who, notwithstanding his political blunder during the War, deserves our admiration.

The further evolution of our structure has some far-reaching implications which we cannot ignore. One is the geographical dimension, with the question of the optimal level of decision-making. For many problems, the solution can be found in decisions at a "low" level, thus facilitating a maximum of participation. For a

minor, but very important, part of the problems which the world population has to solve, only "high" (i.e. supra-national up to world) levels are a technical necessity. The reason is the presence of external effects, which means that decisions by a national government also affect the wellbeing of citizens of other countries. It is amazing that so many citizens and politicians are so short-sighted as not to understand this. This myopia is a terrible threat to our future. It damages the vitally necessary co-operation in European (and other continental) as well as in world affairs. Only certain of our greatest politicians, such as J. Monnet, have understood this, and have continually attempted to convince their less clever colleagues: in Churchill's terminology, he was a statesman, as compared with to-day's leaders most of whom are only politicians. Another statesman is R. Prebisch, who laid the foundations for many institutions needed for supra-national decision-making. Some of the other far-sighted personalities have been co-operating in drafting, at the request of the Club of Rome's chairman A. Peccei, the report *Reshaping the International Order* (or RIO in short). Their work has been partly preceded by that of the United Nations Development Planning Committee, chaired by G. Salgado.

Another dimension of the alternative world order needed to survive has been brought out clearly by A. Herrera, who, in the framework of the Fundación Bariloche, chaired the group which also reported to the Club of Rome under the title "¿Catastrophe o Nueva Sociedad?" (Catastrophe or New Society?). The dimension stressed is that of a sober material level of living. In Asia, Mahatma Gandhi and in Africa Julius Nyerere and A. Tévoédjrè have stressed this dimension of a desirable world order.

5. Methodology and Didactics

As already observed, this part of my recollections will be of a modest nature. In fact, it consists of a few scattered remarks on my encounters in methodology and didactics.

One aspect of methodology has been dealt with already. It refers to measurement as an integral part of scientific activity. It is generally realized that the amazing progress made in the natural sciences is due to no small degree to the continual confrontation of thinking and measuring. I have already stated my belief that the

acceptance of the measurability of welfare constitutes part of my strategy in working. Some of my allies here are B.M.S. van Praag, already mentioned, and Lore Scheer, of the Vienna Arbeitsgemeinschaft für Lebensniveauvergleiche, whose activities I consider to be very important. I would like to add R. Stone, who, apart from an impressive number of penetrating econometric analyses (especially of demand for non-durable versus durable goods), laid the foundations for the national accounting systems now officially used on the United Nations recommendations. Equally in the framework of the United Nations, I would put on record my encouraging recollections of many stimulating discussions (in the United Nations Research Institute for Social Development) with J. Drewnowski (1974) and the development of the social indicators "movement", a good example of which is provided by S. Levy and L. Guttman (1975).

Another element of methodology is the use of formal similarity between relations found in other sciences as a basis of a theory in the science under consideration (in the present case, economics). My plea is that this source of scientific activity be used with caution. I am aware of having used it, and of having transplanted elements of my training in physics into economics. Several of my colleagues probably think I have been guilty of not being cautious enough. Apparently the limits laid down by them for what is permitted are stricter than mine. The examples in which my own limits have been transgressed are to be found especially in the attempts to explain income distribution with the aid of stochastic processes. Perhaps similarities between processes in some natural sciences and production processes in economics should also be mentioned. Some of the similarities do make sense, however, especially when processing industries are involved. Here, there may not be similarity but outright identity; and then caution is not needed.

Turning to the subject of didactics, I was deeply impressed by the brilliant didactic capabilities of my physics teacher P. Ehrenfest. Like no other scientist I have met, he was able to reduce a subject to its absolutely simplest core, and by so doing made the essence of some relationship understood. I have found it a useful exercise to work out the simplest form in which some concept makes its entrance into a series of generalizations of some highly simplified initial model. The well-known example in economics is the use made of Robinson Crusoe; another is the use of two countries in a

model of foreign trade. The cobweb theorem mentioned in Section 2 is another well-known example. J.E. Meade is, I think, in all his work the ablest "simplifier" among economists. The earliest evidence I have is Meade (1936); and we all know how many of his crystal-clear publications have since appeared. Not only does he offer simple models, it is also his admirable use of the English language that contributes to his optimal didactics. Every concept is rendered concrete by an example. Each sentence expresses only one act or one relationship. The reader is never side-tracked. One train of thought at a time is described. Complicated issues are divided into pieces; the pieces are then tackled one by one and subsequently put together to show the whole. Meade has this in common with the physicist H.A. Lorentz. The only drawback may be that, while listening to such a teacher, you think the subject treated is simple. Back home, you discover that this is far from being the case. Fortunately, if the text happens to be available in print, you can read it once again.

6. Communication and the Organization of Multidisciplinary Research

Ironically, classical economists and many non-classical ones coming after them used to assume that their *homines oeconomici* were fully informed about everything relevant to their activities. The key importance of the amount of information available and the frequent lack of relevant information have been dealt with only in the last decades. L. von Mises and F.A. von Hayek can rightly be regarded as pioneers in this connection, and to-day the computer is the subject of preoccupation of citizens afraid of possible abuse of information. Without going into the many discussions of this type, I would like to report on the lack of communication experienced by me, strangely enough only in the later stages of my work. For me, the awareness of the communication gaps became clearly visible when I compared two survey articles on income distribution published in 1970, one by an American and one by a European scholar in the field. The former centred on the human capital approach; the latter mentioned almost all other aspects but the human capital approach. (I am exaggerating slightly.)

After 1970, I discovered a series of colleagues who had pu-

blished on the subject — at the rate of one a month or so. My respect for the state of Wisconsin and its university grew sharply. (Yesterday I had something of a shock when reminded of the state's one-time senator, McCarthy.)

Among the discoveries during my reconnaisance trip in search for more information was the concept of path analysis used by sociologists and geneticists some twenty years before econometric modelling started, which is so similar that I felt like having talked path analysis for about fifty years without realizing it. At this point, the lack of communication between different sciences began to impress me, although I remembered that economists dealing with production functions and engineers failed to contact each other for a long period.

Finally, I increasingly noted that younger colleagues had, inevitably, not read many of the authors I knew from my own earlier years. On reading about A. Einstein (A.P. French, ed., 1979), whose lectures I had the good luck to attend at Leiden University in the nineteen twenties, I thought of an interesting and somewhat different communication problem. Einstein in his student years hardly ever followed courses, but read the physics classics. K. Wicksell, whose counsel I had asked for during my switch from physics to economics, recommended me above all to read the economics classics, much in line with Einstein's behaviour. In fact, I did not follow Wicksell's advice, which may be revealing. The weak defence I recently put up for my behaviour is that somebody with an empirical preference is more in need of discussions with contemporaries.

As I said earlier, everybody is now aware of the overwhelming importance of the communication problem in scientific work. For two reasons, I think. On the one hand, more than ever do we need interdisciplinary research. This requires new forms of communication, and perhaps even of education. On the other hand, we are faced with a serious problem of overpublishing. The number of documents which we ought to read is continually greater than those which we are able to read, especially if we want to do our own thinking. This unhappy situation must be changed. We have already made it compulsory to prepare a summary of each publication; and we have introduced surveys. Perhaps we should require each author to submit, not only a summary, but also an indication,

in that summary, of what he or she regards as novel in the publication, especially when the publication is lengthy.

In conclusion, let me try to indicate the guide lines for scientific work to which I attach the greatest importance. I venture to suggest that:

(i) we try to minimize dogmatism and subjectivity;

(ii) we remain as close to empirical data as possible;

(iii) we work in interdisciplinary teams; and last, but not least:

(iv) we choose areas relevant to the most pressing problems. These I consider to be:

(a) to organize a peaceful world;

(b) to strengthen solidarity with those living in poverty;

(c) to take thought for future generations.

The Hague

JAN TINBERGEN

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