Those Dynamic Years 1930-31-32*

1. - The three-year period from 1930 to 1932 has a particular significance for my professional life. Having obtained the first scholarship from the Rockefeller Foundation to Italy, I lived through the exceptional events of these years in great cultural centres in the United States, England and Germany. It was for me an experience of exceptional importance for the richness of the intellectual stimuli received by me from the thoughts and judgements of outstanding economists and of distinguished figures on the economic scene of various backgrounds and from the comparisons between different orders of economic culture. In these recollections, I shall dwell in particular on these comparisons which may offer analytically significant indications.

2. - At the time of my departure for the United States, I combined two strands of Italian doctrine to which I owed a great deal of my future scientific work.

The first line of thought stemmed from Ca' Foscari in Venice where I took my second degree. In Venice, the teaching of Gustavo Del Vecchio was for me of fundamental significance. He had a knack of analyzing with incomparable insight the thought of the greatest monetary thinkers of the age — I. Fisher, K. Wicksell, L. von Mises, F. von Wieser, R.F. Hawtrey, D.H. Robertson and naturally J.M. Keynes, but Keynes of the earlier manner — and he did not fail to point out (especially in conversations and private correspondence) that their particular theories, so different from each other and alternatives to each other, constituted a very damaging cricitism " of each of them " and hence called for a general and complex theory which embraced them all. This according to " the

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infinitesimals of systematic tension" which characterized them and taking account of the momentum given to theoretical analysis by new disciplines such as the statistics of monetary flows and even of accountancy or book keeping, the latter of these being a discipline of "estimates" as opposed to, or complementing, economics, which was a discipline "of prices."

Although not completely sharing these articulations (and graduations), I always broadly adhered to them, first in the outline of my graduation thesis in Venice (which was later published in book form as *Le teorie monetarie e il ritorno all'oro - Monetary Theories and the Return to Gold* - 1928 and 1964) and subsequently in other writings up to the *Trattato di logica economica - Treaty of Economic Logic* - (Padova, Cedam, three vol., 1962-1974) in which, especially in the second and third volume, money and its systematic relations are theorized thanks to the concept of "monetary, banking and financial propagator" (one of the ten propagators which have the task of substantiating the permanent exogenousness of every economic problem).[1]

For this theoretical development, I am again indebted to Del Vecchio, who, in some of his writings, and especially in *Le nuove teorie della moneta - The New Theories of Money* - (1909), recognizes its validity, especially when the economic system is regarded holistically. However, according to Del Vecchio, that should be confined to certain dates "in time", or ought to occupy only "secondary" positions (as was to emerge in the *Beiträge zur Oekonomischen Theorie,* 1930, directed by A. Aftalion, L.V. Birk, H. Mayer, A.C. Pigou, F.W. Taussig, F. de Vries and by Del Vecchio himself and edited by E. Lederer and J. Schumpeter, in which the opening work is precisely that one by Del Vecchio called *Grundlinien der Geldtheorie*).

I am greatly indebted to Ca' Foscari in addition for the dynamic conceptions formed, or at any rate advanced, there. From these I received the impulse to write my essay, *Studi sull'attività dell'imprenditore moderno - Studies on the Activity of the Modern Entrepreneur* - (1929), in which, however, I emphasized the slight importance of Schumpeter's *Kreislauf* (cycle) beyond which the dynamics would be carried to subsequent peaks only by the "waves" of new entrepreneurs, and indicated limits and reservations, both of which I repeated in the *Saggio sugli studi di dinamica economica - Essay on Studies on Economic Dynamics* - (1930), in the more thorough study, *Sul concetto di tempo - On the Concept of Time* - (which went through a number of editions) and in the item which I wrote for the *Italian Encyclopedia* called *Dinamica economica - Economic Dynamics.*

As I owed it to Del Vecchio if, on the one hand, I set out on the path of monetary economics and on the other I received an extremely strong incentive to go more deeply into "general economic dynamics," in the same way I owe it to my Turin masters if, in tackling the problems of social economics, I evolved a mentality free from ideologies and prejudices. I am not referring to the sociological stances of R. Michels, who was merely continuing the work of Gaetano Mosca on political parties (interpreted as a new area of power in every economic system), or even to Achille Loria whose studies on scientific socialism had, even a considerable time back, provoked the corrosive criticism of Benedetto Croce.[2] I am on the contrary referring to the men continuing the positive line of the "Cognetti-de Martiis Laboratory" regarding not only all research on theoretical and real economics, but also the manifestations of that wide sector of scientific economics which is exclusively composed of methodology — an approach, therefore, which does not at all follow, but is "diametrically opposed" to O. Spann's "Universalismus."

[1] According to my theory, the term propagator indicates the extra-economic reality, which is always present and conditions the whole of economic activity. In the analysis in the first parts of the third volume (devoted to exogeneity) of my *Trattato di logica economica* (1974), I distinguish ten propagators or large complexes of exogenous variables, i.e., demographic, psychological, technological, institutional, trade union, monetary (including the banking and financial subpropagators), international, distribution of the kinds of firm, catallactic, distribution of income (and ownership). On the contrary, the exogenous non-permanent originators of economic changes are formed of "entelechians" and of "anti-entelechians" (which are discussed in the last parts of the third volume of the *Treaty* under reference). The former are unforeseen and original events which bear down on the economy and are recognizable and measurable only *a posteriori,* such, especially, as wars, revolutions, famines, epidemics, violent transformations of tastes and interventionist policies; entelechians are not therefore recurrent, like seasonal movements, nor can they be assimilated to erratic movements of a casual nature. A long series of researches have ascertained, thanks to a complex statistical method, that the duration of the "entelechians" from the fifteenth century on is equal *on an average* to that of the "anti-entelechians", by which term I mean the economic standstills which generally follow the entelechians and are accompanied by a general fall in prices and multidirectional rates of change in the other economic variables (due in the main to the unequal behaviour in time of the ten propagators). In short, no economic event is due to a single cause, but depends on a vast number of causes; hence an equally vast number of predicates.

[2] These criticisms were echoed forty years later in CROCE's admirable essay *Come nacque e come morì il marxismo teorico in Italia - How Theoretical Marxism Was Born and Died in Italy* - (1938).

Luigi Einaudi, Pasquale Jannaccone, Attilio Cabiati, Giuseppe Prato and in part R. Bachi and V. Porri at that time formed at Turin a veritable school of economic concreteness. I have specially in mind the "industrial relations" studied by them in a pluralistic world of free trade unions and in a system applying coercion in which there is only one trade union (as was the case in Italy during the Fascist Corporative State and in Australia at that time).

At Turin, thanks mainly to P. Jannaccone and G. Prato, it had become possible to apply, with excellent results, the theories of Marshall on reciprocal demand to these labour relations. For my part, I based my speech inaugurating the Bari academic year in 1930 on that very theory. Subsequently, Einaudi, who was not entirely convinced, had done me the honour of speaking at length on it in the review *La riforma sociale* and then to nominate me for the Rockefeller Scholarship, mentioned above. This led to an extensive volume of mine of over 500 pages on the *Economia del lavoro* (*Economics of Labour*) which I still have in proof, but which was never published because the 1943 bombing destroyed it when set up in print.

At Turin I was fundamentally cured of unrealistic ideologies. *Not* by observing the failures of the popular disturbances in Turin in 1917 and 1922, but by discussing with Einaudi for three hours one winter morning the concrete possibilities of the lasting extension of the first Soviet Five-Year Plan to European countries including Italy. (I had then met, also at Turin, Hans Staehle, the future author of the table of the budgets of various European and non-European countries.) From my Turin masters, then, I learned how dangerous it was to allow economic life to develop in accordance with plans established by government ideologies or by parties, and applied in practice according to the manoeuvres of the inevitable bureaucracies, almost always as mysterious as they are arrogant.

For the problems of pure economic theory and for those connected with money and international trade, too, I believe that my Turin masters were second to none in profundity of reasoning and elegance. I recall, in particular, Jannaccone's *Relazioni tra commercio internazionale, cambi esteri e circolazione monetaria - Relations between International Trade, Foreign Exchange and the Circulation of Money* - (1927) and his treatment of dumping in 1914, in which imperfect competition is theorized long before it was in well known foreign models; Prato's long studies on the *Effetti economici del contratto collettivo di lavoro - Economic Effects of Collective Labour Agreements* - (1916) and those on the wars of the eighteenth century from which I drew so generously both in my theory and in the related verifications of the "entelechian" factors (identified many years later by over six hundred working groups in the Bocconi University and published in three volumes of *Ricerche di cinematica storica - Research on Historical Cinematics* - 1968, 1968, 1972); Porri's acute analyses and proof of the similarities between foreign and domestic trade, which anticipated the universally known works of Ohlin; and lastly Cabiati's studies. Even now I am surprised that Cabiati's teachings on the solution of problems of foreign exchange, gold and money are ignored by the new generations, although these are rich in new theoretical insights and were in advance of contributions by D.H. Robertson, F. Lutz and Don Patinkin (in the framework of the relative "general economic equilibrium" which he always bore in mind).

I would like to make reference, too, to two singular forms of personal preparation which I have always regarded as being positive in my case. The first of these flowed directly from the noisy and almost daily encounters in the café where we continuously argued about social and historic and cultural problems. Luigi Einaudi has already recalled the tradition — a veritable "oral history" — of the "mirrors' café" of Turin and of the "third room" in Aragno's in Rome which continued, although to a limited extent, that of the *Risorgimento*. But it is also worth recording the encounters in which I often took part, although in a subordinate role, at the San Pietro in Bologna and Biffi's café in Milan where the "elect" pontificated and the discussions led to salutary second thoughts and not infrequently helped certain attitudes to take root which were to last for a whole lifetime.

The second of these singular preparations flowed from my personal and even more radical rethinking (in relation to the university of the time) of *Das Kapital* by Marx which I had acquired in a French translation in 1924 (by J. Molitor, in four volumes). Although agreeing with the whole of Ricardo's criticism (as he had considered "naïvement," as it was put in the first French translation of 1872, "the opposition" between the wage, profit and rent categories "as if it were a natural law of society"), this constant harping on "historic missions" on the part of the working masses and of an "opposition" between small properties and large ones, but without having gone into the proofs, seemed to me unscientific, or as showing a most dubious *a priori* attitude, since it showed no concern for establishing up to what point all that could be adapted to the real world. In the same way, I was shocked, but even more deeply, by the Hegelian interpretation of history, and I always remained true to this critical position despite the fact that a similar interpretation was also advanced in a recent work of J. Hicks.

In my intellectual baggage I did not take with me on my trip to America the teachings of Maffeo Pantaleoni, although I was his distant successor at Bari University where he conceived the famous *Principi - Principles* - (1889). This little book is a valuable work, but I felt that it had certain defects, given the reversal of the theoretical terms caused in me by the familiarity, as a result of lengthy studies, with L. Walras' work. When, later, I read the *Saggi* (*Essays*), I felt dissatisfied when faced with the real or presumedly scientific methodologies of Pantaleoni, based on "contrasts" and "rapprochements" between factors and relations, material or not, which were categorically different. (At a later date, I openly denounced their fallaciousness or their imperfection, though recognizing their refinement.)

And, to conclude on the matter of my intellectual baggage, I will add that, whether from a flair or from an obscure instinct, my interest in Turin and at the Ca' Foscari in American economic literature was almost solely concentrated on J.B. Clark, I. Fisher, H.J. Davenport, J. Viner and F.H. Knight.

3. - The thinking of American economists at that time was openly influenced by the pragmatism of John Dewey, who had published in 1927 *The Public and its Problem*. For some, it was very close to the "rugged individualism" of President H. Hoover who, from his famous *American Individualism* (1922) on, had been a supporter of the individual's freedom of choice. Hence, a series of "Thou shalt nots." These, however, were vigorously denounced in the economic and literary world, e.g. by T. Dreiser and J. Dos Passos.

At Harvard, I did not greatly profit from the "general" teachings of A.N. Whitehead and P.A. Sorokin. The latter's *Social Mobility* (1927), to be considered inevitable and continuous, struck me as mechanistic and hence incomplete. And Whitehead's interpretation, with its "eternal objects" present in the realm of the "possibilities," for whom every event always belongs to the "becoming," seemed to me abstract and generic and, at bottom, reductivist. Indeed, his *Process and Reality* (1929) made me regret the work, written in collaboration with B. Russell, *Principia Mathematica*, which was abstract but completely logical *à la Peano*. J. Schumpeter was absent — in Japan — and thus there were only two great professors of economics present at Harvard. The first was the famous Henri Lee Professor of Economics, F.W. Taussig (1859-1940). But, contrary to my expectations, he seemed to me only to repeat the same essential themes of his 1911 *Principles of Economics* and his 1924 *International Trade*. His credo, thus, was free trade and individualistic *à la Einaudi*, of whom he was a convinced admirer. Yet, during this very period of the dynamic years from 1930 to 1932, he was obliged to make public amends for his free trade convictions as regards the extension of the "most favoured nation" clause. As to his pure theory, I felt that Cabiati was confirmed in his view which reproached Taussig with not linking "the indispensable mathematical concept of general economic equilibrium" with the exclusively literary apparatus of his theories, and hence with not being able or willing to extract himself from the "immobilism" of Ricardian hypotheses. In short, Taussig, as a determined realist, left pure speculation to the "professional economists."

Today, the economic concepts of S. Slichter (1892-1959) are not very much alive. He was then the second most important economics figure at Harvard, and his image was that of the advocate of better industrial relations — a field which for me formed a must, given the purpose of my scholarship. Slichter was in close contact with L. Wolman (1890-1961), P. Brissenden (1885-....), D. Lescohier, whom I got to know — but later, at Columbia University — and Broadus Mitchell whom I also got to know, much later, at the Johns Hopkins University at Baltimore. All these scholars, in their studies of industrial relations, behaved like J.M. Clark in the field of business and industry, that is, they examined them in concrete terms from all social and economic sides — the problems of excessive wages and too low wages, which led to "wage distortion unemployment," the problems of collective agreements and the mobility of labour, especially in the case of

Brissenden and of Don Lescohier, and the Deep South, a problem analyzed in particular by B. Mitchell who saw exactly what would happen to the Old South ("No one can calculate what the South will one day be like without grasping the tremendous implications of the factory"). He also warned how much, in the future, a Mississipian, a South Carolinian or a Georgian would be able to do and did in fact do, that is, with a completely different capacity from that of the "degenerate Southerners" who "treated industry with fear and contempt" (cf. *The Rise of Cotton Mills in the South*, 1921). Slichter, in this group of labour economists of the East, stood out by the modernity of his views regarding the best form of trade union organization and the best labour policy, but also as an academic. He was already of considerable standing when I frequented him in his house with its merry swarm of children.

This widespread tradition of a high level of economic culture in labour relations was backed by a vast specialized literature. I found it very advanced, both because of the far-reaching analytical discussions and of the successes obtained in the political campaigns flowing from them — in the battle, for example, against the labour injunctions or in defence of arbitration, and in the one for the creation of adequate public welfare bodies for the unemployed (even if in certain states, such as that of New York, it was only at the end of 1931, a year of grave crisis, that the Governor decided to earmark considerable sums for aid to the unemployed who had risen in total to more than six million. The aid was, however, obtained from "private economic power").

I also found the behaviour of almost all the American universities exemplary in entrusting several courses on labour economics to the most qualified trade unionists. Thus, for example, Columbia University called on S. Hillman, who advocated the creation of labour exchanges for all States of the Union and was the organizer of the "locals" for the Amalgamated Clothing Workers, and on J.B.S. Hardman, a typical trade unionist, who dealt with the relations between the Unions and the authorities. These courses drew a large audience and were constantly brought up to date. I could not help comparing them with what was happening in Italy. With due respect to our experts on labour economics, it was possible at that time to argue that our university literature, though advanced as regards theory, took second place when it came to the concrete teaching of the problems of labour economics.

I would like to add another observation. During the dynamic years from 1930 to 1932, Harvard University gave the impression of not being completely prepared to tackle economic problems from all the points of view of economic science, and especially of being only inclined to consider these problems from the mechanistic, short-term side. This impression seems to me to be confirmed by the huge success at Harvard of Keynes' *General Theory* and by the way Harvard supported the type of economic policy flowing from that work.[3] For this reason, among others, I decided to turn elsewhere in search of genuinely new scientific approaches which would justify the title given thirty years later by the editors of *Fortune* to one of their books, *America's Rise to the Forefront of World Science* (1961).

4. - My first and most important purpose in New York[4] was to come as close as possible to the greatest scholar on economic dynamics, H.L. Moore (1869-1958), whose works, and especially *Synthetic Economics* (1929), I knew when still in Italy. At that time it was common to define economic dynamics as a mainly or even solely mechanistic theory; hence all that was needed was to group together, in the dynamic period, endogenous variables, parameters and constants of different dates in order to break out of the bounds assigned to statics — a theory supported in particular by J. Tinbergen in Europe. Moore's theory, however, was presented in a pioneering spirit with the concept of mobile trend, provided the determinacy of the secular movement was admitted, and hence not only its possibility but also its full equilibrium in time. Unfortunately, Moore's research was far from solving the problems of "existence" and "uniqueness" which are the indispensable conditions to be respected in these cases. Another and not less important question has been tackled by Moore, but only in part as a pioneer, as will be shown below when I come to talk of H. Schultz.

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[3] In this connection, I cannot but cite the opinion of P.A. Samuelson *à propos* of J. Dorfman's *The Economic Mind in American Civilization* (1946), which he judged a "monumental study": "Only if spending could be kept on a high plane, could capitalism survive."

[4] For the reminiscences of this period I am unfortunately without my notes, as they disappeared as a result of the bombings of Turin in 1943. These are only partially made up for by photocopies of the letters I sent to Einaudi and preserved in the Einaudi Foundation Archives.

In those dynamic years 1930-32, new steps forward were being prepared in the theories of knowledge in general, and, as a result, in economic doctrines, too, of which I was extremely curious. They exercised a marked influence on my way of approaching general economic theory. The *New York Times* of that period made public, with a series of excellently presented reports from Berlin, the theoretical apparatus of the new theme contained in the work called *Einheitliche Feldtheorie* of less than five pages, on which A. Einstein had worked for ten years, with the purpose of unifying the laws of gravitation and those of electro magnetism, thereby demonstrating not only that matter cannot be formed without electricity, but also that the whole of matter, called " space " by him, is one, though with different aspects, such as gravity, light, electricity, inertia, space and time. Hence — and that was at the time regarded as of extreme importance by some economists — just as there appeared not to exist absolute space and mass, so absolute time would be inconceivable, which, as a vessel, would contain all matter — the sun, the plants etc. (including man). Moore, too, followed these discussions.[5] Unfortunately for economic science, he was to reveal himself as what he had always been — even in his earlier books, in particular, *The Differential Law of Wages* (1908), *Laws of Wages* (1911), *Economic Cycles* (1914), *Forecasting the Yield and Price of Cotton* (1917) and *Generating Economic Cycles* (1923) — that is, as something not very different from the typical scholar with preconceived ideas following mechanistic doctrines (which, for that matter, is sum-

[5] I have carefully consulted the relevant correspondence and the unpublished material in the 44 boxes (cm. 9 x 26 x 45) donated to Columbia University by Moore's heirs and his wife, Jane, in 1959. The most important correspondents were J.B. Clark, A.A. Cournot, F.Y. Edgeworth, A. Marshall, E.R.A. Seligman, F.W. Taussig and L. Walras, but the correspondence is of no special interest. There is more to the unpublished manuscripts, *Good Life in a Progressive Democracy and Moral of Mediocrity* — all in his precise handwriting, either in ink or in pencil, perfectly orderly and vertical — and to the notes on cuttings or reproductions from the works of numerous economists (including Italian ones, such as Pantaleoni, Pareto, Borgatta and Cabiati) characterized by frequent underlinings. On the whole (including the quotations which are also very numerous), I derive two impressions: 1) this is mainly material, and not the definitive approach to the composition of a book; and 2) none of the correspondence justifies us in thinking that Moore, *at the time of the relevant dates*, was regarded as an outstanding economist, as was believed in France, for example, by H. Guitton, in Italy by G. Del Vecchio and part of his school, A. Bordin, A. De Pietri Tonelli, A. Amoroso, V. Moretti (especially); and, in the United States itself, by C.F. Roos, H. Schultz, G.C. Evans and W. Mitchell.

med up in the passage placed by L. Brunschvicg at the beginning of *Synthetic Economics*). And this, in my view, is because of a serious mental illness which prevented him, after 1929, from going back to teaching in Columbia and hence from removing not only the ostracism which he had imposed on those aspiring to positions in the university who opposed scientific formulations of the type put forward by L. Brunschvicg (i.e., that of " synthetic unification which transforms a discontinuous plurality of facts into a continuous network of relations," which leads to the principle of the scientific validity of economic forecasts), but also from continuing to bind to himself those economists who, from his earliest publications, regarded him as their leader. Apart from H. Schultz, to whom I will revert below, I would like to recall two scholars who are among the most highly esteemed of his followers.

The first was C.F. Roos, the first director of the famous Cowles Commission and the author, in addition to the study on the demand for cars, of *Dynamic Economics* (1934), and subsequently of *Dynamics of Economic Growth* (1938). Having read them at the time and having known their author personally, I can here affirm that their aim was substantially that of measuring the " imminence " of what is called the cyclical ups and downs in order to furnish a theory which would also be useful for practical purposes such as that of interesting even department stores (for example the Filene of Boston). A similar aim had been pursued by the authors of the famous " three Harvard curves," which, it should be added, were disproved by economic facts in the second decade of the present century.

As for R.W. Souter (1897-....), whom I also knew personally for a long time, the main part of his book, *Prolegomena to Relativity Economics. An Elementary Study in the Mechanics and Organics of an Expanding Economic Universe*, was not formed of references to the " organics " of Marshall, but of relations with Moore's theoretical formulation, since the partial oscillations were attributed to minor causes in line with the general conditions of the economic equilibrium envisaged by Moore. In short, both for Roos and Souter, the main theorization, even if at the time it was explicitly abstract, corresponded completely to the positions of mobile equilibrium expounded by Moore. On the deficiencies of this scientific approach, I have come out on several occasions elsewhere, and more extensively and consistently in the three volumes of the *Trattato di logica economica* (*Treaty of Economic Logic*), and it would be boring to repeat myself here.

A second aim of my long stay in New York was to follow closely the scientific activity of certain other professors of Columbia in order to verify the analogies with, and differences from, the scientific work carried on in Italy by my masters. I was already familiar with several essays by H. Hotelling (1895-1973). When I took his courses at Columbia, I had occasion to get to know one of the best qualified personalities produced by the American universities in the field of economic statics. For that matter, his theorems are still at the heart of the most advanced statics formulas, although they represent the definitive conclusion of an era — the one opened by the Lausanne School, after which there is little if anything to be added despite the flood of literature which has accompanied it for very many years, even if expressed in so many different — but mainly mathematical and econometric-statistical — ways (as in the case of the *Mathematical Introduction to Economics,* 1930, by G.L. Evans).

I have always differed profoundly with this approach. If I may so express myself, then as for that matter now, in Hotelling's essays the question remains open of the absence in the main of a formulation and quantification at least explicitly diachronic, if not a historical one, which, having also resort to some of the most refined methodologies, would make it possible to "fix" the substance and the horizons of the analytical relations, and these should not then vary, in such a way as to overcome once and for all the antitheses and juxtapositions furnished on the matter by hundreds and hundreds of studies and of ingenious but frequently unsatisfactory images. These reserves of mine, some of which were frankly advanced in the seminar at that time, affected only three works of Hotelling: *The Application of the Theory of Error to the Interpretation of Trends* (1929), an essay written in collaboration with H. Working, *Stability in Competition* (1929), and *Edgeworth's Taxation Paradox* (1932). However, save for these reservations, such works, like others subsequently published, deserve well of the world of more responsible economists, since they bring out certain disconcerting and useless disputes of the "literary" economists, as in the case of the overingenious (and somewhat ridiculous) prosopopea of the Austrian economists as regards the theory of subjective values.[6]

On the scientific activities of J.M. Clark (1884-1963), my reminiscences concern only *The Economics of Overhead Costs* which goes back to 1923 (translated by me into Italian jointly with A. Piana). In Italy, too, there was often talk of fixed expenditures and costs for different purposes and situations, including supplementary costs, overhead costs, full cost and so on; but the only two typical cases developed (in a fairly generic way) were those in which marginal costs became greater than average costs, and vice versa (as often happens in agriculture, which results in competition becoming extremely keen). J.M. Clark, on the contrary, felt the need of a longer analysis. In particular, for individual industries, and then on the knowledge of the "accounting principles" present in every society, but often differing among themselves. Without this double reference, it is impossible, Clark affirms, to establish not only the particular magnitudes of economic components, but also the meaning and extent of their flow, both in the single moments and in the dynamic context (and especially in growth). Since, if the "accounting principles" change — partly legal and partly contractual — and they are replaced by "accounting principles" formulated outside any specific agreement, every economic fact assumes different dimensions and directions.

I do not believe that Clark had read *Il costo di produzione* (1901), by Jannaccone, or the little set of dynamic jewels formed by Pantaleoni's essays. If he had, Clark's dynamic analyses, although solidly anchored to real economic facts which were however regarded as present everywhere, would have offered different and much more heuristic points of departure than his usual ones (represented by "business attitudes," "new ways of life," "pathological conditions," etc.). At a lecture, I remember taking the liberty of pointing out to him that the points or causes of dynamic departures were treated only as imponderables and on the same theoretical plane, thus furnishing a set of cases drawn from obser-

[6] Cf. the study of about the same date, *I tre tipi fondamentali della teoria del valore soggettivo* (*The Three Fundamental Types of the Theory of Subjective Value*) by O. MORGENSTERN (1931) which was handed to me personally at Bari by the author and which was so different from the work which was to be written a decade later, in collaboration with von Neumann, on the games theory.

vation, but deprived of general motivations which could not be introduced solely by the supercategories of the " groups " and " industries " (in the Marshallian sense), as if the analysis of what happens on the part of their " components " were enough to explain the " presence " and the " succession " of the market and production shares, number and proportion of giant firms and the others, etc.

To put the point in a phrase that is now current, in the famous *Economics of Overhead Costs* the logic prevailing is that of understatement, and hence the explanations always suffer from serious shortcomings. This logic remained unchanged both in *Competition as a Dynamic Process* (1961) and in the preparatory work, *Toward a Concept of Workable Competition* (1940), although greater space is given in it to certain analytical instruments (called " dynamic ") — such as the demand and supply functions connected at the moment in time and the uncertainty functions — in order to follow more precisely the effects of competition within stable and unstable (or in general dynamic) industries.

I remember Clark as if it were now. Averse from long discussions, cold, reserved and not very erudite, a collaborator and reader of the *Towncrier* of Westport in Connecticut, where he lived. He followed only events in America, and rather minor ones at that. Perhaps he was timid. Perhaps he felt bound to look only at these and at the numerous small and circumscribed questions. He fought shy of generalizations. Convinced as he was that goods are only " bundles of utilities," he gave the impression that ruptures of equilibrium should be attributed to different factors, from which even the opposite consequences might be expected, and that it was not necessary to take a further step in order to arrive at a total view in time of the " transpositions " of the economic functions which continuously succeed each other. In short, with J.M. Clark, one was in a scientific climate linked in the main to static concepts or to concepts of comparative statics, and only to a very small degree non-endogenous, and in any case always coming within the American tradition of the case-system, which economists of very different extraction, such as F. Machlup, ended up by echoing (not to speak of the book by E.H. Chamberlin, *The Theory of Monopolist Competition*, 1933).

5. - If at this point I glide lightly over my recollections of Professors Seligman and Willis, also of Columbia, it is not because they were lacking in scientific personality as scientists, but because their personality was similar to that already outlined of Taussig, and in that sense they too were neither the prototype of the new university professor or an example of the man who is out of line with the times.

E.R.A. Seligman (1861-1939) was simply a worthy defender of the interests of the middle classes and a firm but not *a priori* opponent of those who ran down the American capitalist system, whether it was a question of his almost endless scientific and academic output on theoretical finance in the narrow sense, his main territory, or double taxation, or railway tariffs and international debts, the economy of instalment sales, or the technique and effects of the unfair burdens which weighed on agriculture which, like other economic activities, should, he felt, have been in a position to be undertaken in a free economy. Unfortunately, I did not see much of him, although he professed admiration for Jannaccone and Einaudi, and especially for Antonio De Viti De Marco, of whom he particularly appreciated the theory of the " cooperative state " as the main contribution to public finance. As regards financial logic, his ideas were, however, opposed to those of Taussig, since he maintained, basing himself on De Viti, that taxes could, in total, act in a productivistic sense on family budgets. If I made use of this theory of " the state as a factor of production " when I dealt with the three principal ends of the Modern Social State (in a book of that title: *Stato sociale moderno*, 1946), I refined on it thanks above all to the work of Seligman and to the reworking of it as expounded in his courses at Columbia.

H.W. Willis (1874-1937) was extremely well versed in the credit policy of the Federal Reserve Banks (he had been the Secretary of the Federal Reserve Board). He was the most brilliant professor of the School of Business, and, if it is true that the theoretical fabric of Irving Fisher at Yale was much richer than that of Willis, the latter in his turn was superior in the intuition of the ways in which money and credit policy should be handled and even in practical taxation. Willis could never have endorsed Fisher's pamphlet, *Stable Money* (1934), or even the previous one, *Booms and Depressions* (1932), according to which the capitalist system would always tend to " boom the booms and to bust the busts ".

At that time, the only work of Keynes to be known was the *Treatise on Money* (1930) which, however, had not had any great influence on writers on money of the period — Robertson, Meade, or even Willis, as is proved by his essays of 1933 and 1934 (which were discussed beforehand in his lectures). The enthusiasm for Keynes came later. Willis regarded the great depression of these years as not very different from the classic cases. He therefore differed from the Governor of the Bank of England, Montagu Norman, who, in a letter dated 26 July 1931 and addressed to his French opposite number, Moret, wrote: " Unless drastic measures are taken to save it, the capitalist system throughout the world will be wrecked within a year," and he added: " I should like this prediction to be filed for future reference ".[7] Willis called for the continuation of the great American experiment of the Federal Reserve Banks. He was therefore far from wishing to entrust many responsibilities to mixed banks.

My observations (at lectures) tended to cast doubt on the traditional American policy of sterilizing gold and the policy, which was not so serious, of maintaining the gold exchange standard, both of which were all the more incomprehensible since in fact their practical aim was to give rise to conditions regarded as favourable to producers, especially to farmers (the policy of the Farm Board). Obviously, as the author of *Le teorie monetarie e il ritorno all'oro - Monetary Theories and the Return to Gold* - (1928), I was unknown in America. But that did not prevent me, in discussions, from always expressing amazement at the lack of interest on the part of practical and theoretical American writers on money (many of them on company boards) as regards the problem of the return to gold, while pursuing, on the contrary, certain effective but short-term solutions. These monetarists were also contrary to the " qualitative control " of credit called for in my volume, a policy which is not simply the one defended, many years after, by R.V. Roosa, but breathes the same principles as those of the monetary reform in Federal Germany after 1948, which makes practically impossible for the Federal Republic and the Länder any serious inflationary or deflationary skid. An approach of this type constitutes the fulcrum of the " Soziale-Marktwirtschaft " and is based on the " overall " stability of a series of parameters regarding the relations between public and private expenditures, potential and actual national income, issue of money and creation of credit and demand, etc.

[7] I take the passage from a quotation supplied by me to Einaudi in one of my letters deposited with the Einaudi Foundation.

I approached W.C. Mitchell (1874-1948) at the Fayerweather (the building of the Faculty of Political Science where History, Economics and Public Law were taught) before he was called to Oxford to give a course (for the academic year 1931/2). His theoretical " types " were very erudite biographical pictures of D. Ricardo, J. Bentham and J.S. Mill, of the English radicals, and so on. Hence, they were not such a draw as Mitchell expounding his theory of economic cycles or as the scholar who verified them (in a long series of writings, some of them together with A.R. Burns and other members of the National Bureau of Economic Research) or as the interpreter of American economic policy after the crash of the stock exchange on 24 October 1929.

As to the first and second activity, my scepticism was in part " di scuola." According to G. Del Vecchio, these economists who cultivated the theory of crises had neglected the dynamic phenomena of the " second type " (i.e., which could not be foreseen, even by recourse to highly advanced analytic and empirical devices). Mitchell and his collaborators argued — I think — indipendently of that distinction, or else, while accepting it, they proceeded solely on the basis of the consideration of the dynamic phenomena of the " first kind ". In any case, their diagrams, boosted by detailed statistic and econometric calculations, were valid only for limited periods of time and for certain countries, and never for a continuous length in time or space, although they did not accept that.

I have always considered as incomplete or inexact — or even as derived from the mysteries of imagination — the statistical and econometric " verifications " in question, from those, far back in time, supplied by J. Tinbergen and collaborators to the *Société des Nations* at Geneva to those of E. Wagemann, K. Pribram and so many others, first of all N.B.E.R.; and this I have also proved and had proved.[8] For that reason I could not even accept the affirmations of J. Schumpeter, inserted in the 1934 English edition

[8] *Cf. Ricerche di cinematica storica - Research into Historical Cinematics op. cit.*

of his fundamental work, that there were at least "three" types of cyclical movements and "probably more".[9]

6. - This brings me to the strange developments which accompanied, and were at the same time contributory causes of, the stock exchange crash of 24 October 1929, whose extremely serious consequences in America I witnessed from 1930 on. That "black Friday," and above all what happened in the following two months and later in 1930 and 1931 in the labour policy sector, constituted, in my opinion, a set of unpredictable causes, most of them exogenous, which in my terminology connote a complex "entelechian fact".[10] This is the complete antithesis of the usual "entelechian factor" which is directed at gradually increasing levels of prices and has exceptional general causes (war, revolution and so on). An "entelechian factor," therefore, which, in addition to being sharply distinct from the traditional type, was extremely corrosive as regards almost all the world's economies, especially because, under the impact of completely unrealizable illusions and not very salutary countermeasures, individuals and the large economic and political complexes committed endless mistakes as if they were a prey to unbounded irrationalism, and hence incapable either of taking bold reequilibrating decisions or of organizing adequate points of resistance.

Thanks to the assistance derived from a good deal of information, especially the material in the newspapers of that time, I have recently been able to recapture that atmosphere, with all its errors and anxieties pervading almost all the economic sectors.

Here I will only recall the main points, passed over by the vast literature on the Great Crisis, and which, in my opinion, functioned as its main or contributory causes. 1) Although the volume of transactions on the New York stock exchange had already, in the third quarter of 1929, risen to over four million, that is, double

[9] Fortunately, for over twenty years no authoritative theorist has talked of "periodical" crises. Let us hope the N.B.E.R. will therefore make a thorough revision of the original inductive methodology, further buttressed by the "modelistic" dogmatism, as well as by R. Frisch (1933), T.C. Koopmans (1947), J.R. Hicks (1949), F. Modigliani (1949), S. Kuznets (1952), E. Lundberg (1955), G. Haberler (1956), A. Smithies (1957), W.W. Heller (1957), M. Friedman (1959), J.R. Schlesinger (1960), M. Abramovitz (1961), J.J. Polak (1962), G.H. Moore (1962) (to mention only the major works appearing in this sense since the end of the second world war).

[10] See footnote 1.

the volume of the previous year and three times that of two years earlier, the most responsible banks and bankers continued to believe that this was due solely to two events: *a*) the "burden of financing the autumn international trade" (particularly in agricultural commodities); and *b*) the increase in the "British rediscount rate." As a result, the stock exchange brokers "were congratulating themselves on the comparatively small number of margin calls which it was necessary to send out." They also believed on the average that "the customer with a fifty per cent margin has little to worry about." 2) Even a year after the stock exchange crash, the political atmosphere was still characterized by complete uncertainty as to the course to be pursued. On the one hand, President Hoover felt that he could postpone ("may be worked out later") not only the expenditure of several billion dollars which were thought to be capable of limiting the spread of unemployment, but also the very first session of the Employment Committee appointed by him under the pressure of public opinion. As against this, Governor Roosevelt continually attacked the President whom he regarded as guilty of being completely unaware of "the extent of the rapidly growing condition of unemployment," and hence of being deliberately inclined, not to take account of that fact, but even of thinking that he could keep it concealed, and continuing to follow the completely unjustifiable practice of issuing "one optimistic bulletin after another", "a desperate and futile attempt to restore prosperity by means of proclamation from Washington". 3) Even in the late autumn of 1930, there was no determination to assemble the new Unemployment Commission presided over by the Secretary for Commerce, R.P. Lamont, and even less to appropriate federal funds for public works and to increase the credits for consumption, since trade revival had been predicted by numerous "industry chiefs." 4) In several industrial sectors of the country, there was a tendency on the part of the trade unions at the various levels of employment, to insist on obtaining higher wages. Indeed, this was the time when the practice of giving a bonus became of cardinal importance, being added automatically to wages (although, to start with, it was treated as provisional).

I would never end were I to continue to record the main points about the Great Crisis. At the time when I was observing it in America, phenomena 2) and 4) were to the fore, while the unemployed in the large cities were allowed to sell "Delicious" apples at 5 cents a piece amidst the general indifference. However, I feel that the gist of the entangled series of these hesitations and contradictions both in theories on economic crises and in economic policy, and even in private behaviour, can be summed up objectively by stating that the relevant logical models, both of doctrine and action, were worked out not only accidentally, but also as circumscribed and short-term models. There were already large numbers of them, and these have swelled with the passage of time. Quite a few obstacles which delayed economic recovery and even the growth of academic culture stemmed from these models, of which the first was the one worked out by G.U. Yule in 1927. All such models were positivist and most of them were "locally" linear and devoid of the indispensable general relations and articulations.

7. - I did not have a chance of observing at first hand the New Deal experiment launched by F.D. Roosevelt in 1933, but, during the two previous years, I was present as a spectator, and almost took part in its spiritual and political gestation. In those years, Americans began to be very concerned at the worldwide progress of Communism, even in their own country. This was obvious both in some popular reviews and in the public or semi-academic lectures which aimed at presenting it as the panacea of the current economic crisis. In certain trade union centres, too, these and other questions of social reorganization were debated. The argument was stimulated by the reports from Soviet Russia of several outstanding American newspapers, as well as by the debates promoted by their correspondents visiting or holidaying in New York (with one of which I had long discussions). There were also certain minor papers of the avant-garde, which could be read only in particular places, and various books which came out at that time, both providing information or putting forward lofty speculative theories.[11] As opposed to the present situation, however, we intellectuals could claim that we were neutral, and therefore could be remembered like the men in the Fourth Act of Shakespeare's *Henry the Fifth*:

We few, we happy few, we band of brothers.

If in 1932 President Hoover could still maintain that "there is no relief to the farmer by extending government bureaucracy to control his production and thus to curtail his liberties," the fairly coercitive and "progressive" process of "levelling" democratization, in addition to being initiated by the famous Homestead laws (which guaranteed the head of the family concerned exclusive access to not more than 160 acreas against payment of the extremely modest sum of 10 dollars), went resolutely ahead in Parliament. Hence the Norris-La Guardia Act of 1932 (which made the injunctions on the trade unions illegal, thus widening the area of collective bargaining in the teeth of the old guard Republican tradition which stemmed from Jefferson and was sharply anti-state), the Agricultural Adjustment Act and the implementation, but only in 1933, of the New Deal.

Among the opponents of explicitly coercitive "progressive" tendencies, I had occasion to get to know — besides Taussig, Willis and Seligman — Alva Johnston of the *Saturday Evening Post,* Henry Hazlitt, the future author of *The Failure of the "New Economics"* (1959), whom I later met at the Mount Pelerin Society, and above all, A.A. Berle and J. Means, the authors of the famous *The Modern Corporation and Private Property* (1933), as well as the group of anti-Protectionists and anti-State interventionists who were subsequently to become anti-Keynesian, and hence F.H. Knight (whom, however, I met at Chicago). A very homogeneous group operating in defence of civil unity and of the enhancement of the capacity of individuals through the practice of freedom and a free economy.

As for the supporters of "progressivism" and the "Progressive Era", it should be noted that, at Columbia University, liberalism (in the European sense) was no more the logic that reigned supreme. I have already spoken of Mitchell. But, working in the same direction were his close collaborator at the National Bureau of Economic Research, A.F. Burns (the future Chairman of the Board of Governors of the Federal Reserve System) and, in a

[11] As I was staying at the International House (financed by the Rockefeller Foundation) which housed over a thousand university students from all over the world and quite a few scholars of various origins, there was never any lack of arguments and people to argue with. In one of these winter months, I recall having gone, from pure curiosity, to the semi-clandestine cell of the *American Daily Worker* and of the American Communist party which was in downtown New York and which could be reached only by a shaky lift which was open to rain, wind and snow. The very modesty of the setting convinced me of the hopelessness of that party making headway in the United States.

crescendo of antiliberalism, the two young Burns (A.R. and Eveline M., both Directors of Research at Columbia, whom I first met at their fine house in Riverside Drive, and the authors of numerous publications, the first, of *Money and Monetary Policy* — 1927 — and *The Decline of Competition* — 1936 —, the second, of studies on insurance and social welfare), A.S. Johnson, Editor-in-Chief of the *Encyclopedia of the Social Sciences* (1931-35), the leading light of the University in Exile (where, thanks to him, several German Jewish refugees found a chair, such as E. Lederer, K. Brandt and G. Colm), in general the staff of the University in Exile, i.e. the New School for Social Research, situated in downtown New York, and especially the new institutionalists (whose main aim was to modify the Constitution in order, I think, to bring it closer to the Russian Model) with R.G. Tugwell at their head. With him, I had a most instructive symposium, organized by the two Burns, on the political situation in the American universities, which were at the time subjected to numerous investigations on the part of the individual States, especially the University of Washington at Seattle, for "alleged anti-American activities." Tugwell, whom the Italians got to know in Rome in 1934 as the American representative to the Institute of Agriculture, founded by D.L. Lubin senior, was regarded by the most authoritative critics as the inspirer of the New Deal.

In this context, my attention was also attracted by the position taken up by Gerard Swope, the head of the American industrialists and of General Electric, who called for a new "industrial structure" which was panic-proof (perhaps with certain connections with the work of Karl Brandt, professor of agrarian economics at the *Berlin Landwirtschaftliche Hochschule* and director of the *Blätter für Landwirtschaftliche Marktforschung*); and I dealt with it at some length in my article "Un plan américain pour la stabilisation volontaire de l'industrie," which I wrote for the *Revue économique internationale de Bruxelles* (1932).

8. - My research in the three years 1930-32 was rounded off by long stays in four cities — Madison, Chicago, London and Berlin — and other short periods in Washington (to make contact with the Brookings Institution, which had just been founded, in 1927, and with its heads, Lorwin and Lubin), Newcastle-upon-Tyne (which I was invited to visit by the trade union of the unemployed in that large mining centre), Heidelberg (to meet J. Marschak), Frankfurt (to listen to K. Pribram's views on the advent of Hitler to power, which proved to be completely wrong), and Geneva (to study the organization of the Bureau of Labour).

At Madison, I learned two things. The first was that one could, by a "local" legislation (that of the State of Wisconsin), defeat tuberculosis in cattle by the creation of agricultural cooperatives, especially for dairy products.[12] The second lesson was about the birth of the famous American institutionalism which was traditionally attributed to T. Veblen, whereas it might equally well be assigned to J.R. Commons (1862-1945), the author of a host of studies (especially *Distribution of Wealth* (1893), the "assault on laissez-faire," as was later observed by L.G. Harter, 1962).

Around Commons there were, however, people of all approaches, even radicals. From this group, therefore, there emerged not only the well known theory on collective attitudes of the various economic regimes, which, in my judgement, allowed American scholars in that discipline to disengage themselves from Jevons, Pigou and Marshall, but also to provide "men of good hope" with the theoretical foothold for visions such as that of H. George.

Naturally my heart warmed to Commons as the greatest student of the history of labour (*History of Labour in the United States*, 1918, 1935), and to the fine group of his young collaborators (including D.J. Saposs and S. Perlman, 1888-1959, whom I knew). My familiarity with the Madison school helped me to develop the theory of the "trade union propagator" (both workers and owners) which was advanced in the *Logica della produzione e dell'occupazione* (1950) and further elucidated in the *Trattato di logica economica*, Vol. III (1974).

9. - At Chicago, where I arrived in the torrid summer of 1931, the necessity had been emphasized of discovering new paths to economic research in that great university centre. One of these paths was opened up by the visit of J.M. Keynes, who was then known as the author of the *Treatise on Money*. Another was the important econometric research of P.H. Douglas, which was to

[12] This served as a basis, twenty years later, for my general report on *The Difficult Evolution of the Wheat/Beef/Dairy Products Economy* and economic measures for its progress, submitted to the Meeting on the animal husbandry sector at Cremona (1952).

form a prelude, but only eight years later, to the transfer of the Econometric Society from its first headquarters at Colorado Springs to the Social Science Building of the University of Chicago (with Alfred Cowles as Secretary of the Society) and hence to act as a countermove to that (which had even proved sterile) of the three time curves of Harvard as barometric measures of economic time. A third approach consisted in the call to Chicago of Henry Schultz (1893-1938), a "Columbian" since his Ph. D. thesis on the statistical law of demand had been discussed at Columbia University (nor, for that matter, were the barely five pupils of the summer course held by him on A. Cournot Chicagoans either).

Although I was involved in the revision of the abridged Italian translation (the first in the world) of the *Theorie der Wirtschaftlichen Entwicklung* by Schumpeter (I devoted four hours a day to it), I regularly followed Schultz's course and was sometimes his guest at lunch in the Quadrangle Club (at which I was to have the added good fortune, almost twenty years after, of being Enrico Fermi's guest, too, and, still some years later, of F.H. Knight as well) and where, in private, he talked to me of his studies on the "interrelations" of demand, with or without the hypothesis of the constancy of the marginal utility of money. These interrelations were, it should be added, drawn from the erroneous assumption that it is possible to derive, causally and linearly, the partial derivatives of prices from those of the quantities or vice versa, an objection which is fundamental in my writings of the time and was later developed in my *Trattato di logica economica,* Vol. I, *La catallattica,* 1962.

Schultz at that time was at the height of his fame, but his computations — which were later recast in the ponderous volume, *The Theory and Measurement of Demand,* published the same year as his tragic death — of the elasticities related only to the two arguments of E. Slutsky, did not, in my view, provide a systematic important key to economic behaviour since it is essential to consider, in addition and above all, the "shiftings," as I contended in *Sulla teoria delle trasposizioni dinamiche delle curve di domanda e offerta* (1940), a study which was subsequently developed by the mathematician C.E. Bonferroni in the *Giornale degli economisti* that same year.

For Schultz, on the contrary, the Slutsky criterion with which the elasticities were connected, was sufficient to explain the influences of the social stratifications, nor it was possible, except with that criterion, to make headway. As I shall note later, I found the same mentality — a completely mechanistic one and characteristic of Schultz's master, L.H. Moore, too, as well as of W.C. Mitchell — in London, where, indeed, that approach is still the dominant one in economic analysis.

10. - On disembarking at Plymouth from the "President Harding," just at the end of 1931 I ran into a cold spell, which did not abate at my first hotel in London, where only by putting half a crown in the slot machine in the fire place was it possible to extract the merest suspicion of warmth. Subsequently, too, what subtle and unexpected disappointments was I to encounter, redeemed, though these were, by certain particularities of English manners and economic thinking for which I had a profound admiration! In England my main task was the preparation of the long study on labour economics; but that did not prevent me from being greatly concerned to make the close acquaintance of certain very young logicians of economic cycles such as R.F. Kahn, P. Sraffa, M. Kalecki, O. Lange, and D.H. Robertson himself. I did not meet Robertson at Cambridge, where he was Reader in Economics, nor at the London School of Economics, where he was to occupy with such *éclat* the Sir Ernest Cassel Chair for Economics (which was to have "special reference to currency and banking"), but at the Liberal Club in London where, as elsewhere, he was well known for his brilliant literary wit and his personal ideas on the 1930-32 crisis.

Even then I was very sceptical about the cyclical theories which attribute depressions to the difficulties of reconciling progress with the stability in economic relations. It might perhaps be justifiable in certain circumstances, such as those in America, to speak of a "glut of capital goods" — the *Ueberfüllung* of the German writers — but I contested as being unrealistic the theories which considered the economic cycle as one of the three "general" constituents of the specific dynamics of every economic structure (the other two categorical constituents being the secular tendency and the extremely varied set of seasonal movements). I felt equally sceptical about the dynamics of M. Kalecki (and also of R. Frisch) of functional cyclical relations of the "adverse flows of fixed capital," that is, in the terminology of V. Furlan, what are called "ensembles renouvelés," also and above all in cases where that is put in sophisticated equations, so that the imaginary and real roots correspond to the cyclical movements and to the illusory trend movements (the whole, in an atmosphere of erratic shocks, sometimes of overwhelming force, but in the long run not deforming). This scepticism as regards excessively rigid mathematical approaches had been, almost unexpectedly, confirmed after a long visit to the Royal Greenwich Observatory, where I had learned of the inexistence of solar cycles in the period 1645-1715 (which was perhaps never taken into account by W.S. Jevons, the first theorist of the periodically recurring cycles). As to R.F. Kahn's *multiplier,* I will deal with that later (while I will speak of Sraffa on another occasion, and, in the same way, of O. Lange whom I met several times, when he was acting as number two of the Polish Government and was more and more detached from his own theoretical writings on Marxist economics).

The winter of 1932 was for England the Third Winter of Unemployment, to use the title of a fat report by J.J. Astor, A.L. Bowley and other experts, although the title referred to the winter of 1922, which had also been a specially hard one. I got to the heart of the problems raised by that winter, not only by studying the highly detailed *Twentieth Abstract of Labour Statistics* (1932), but also by observing its social consequences during my visit to the mining centre of Newcastle-upon-Tyne. True, England was not then permeated by the present mainly ideological trade union mood which will perhaps decide the future of the Labour Party and even of English society (by destroying and leading several sectors of its economy into an absolute decline), nor was it disturbed by the present communist-type intoxication. However, it is equally true that it was already engulfed in the dole (a dangerous dilemma to the *high* wages because of the consequences for the whole of the country's production as well as for the sector concerned). This attitude was poles apart from that of the American trade unions, which, as was to be emphasized by the Madison school itself, had learned, after tragic setbacks, the optimal limits of trade union action, the limits which had recently prompted the 3,700 pilots and mechanics of the Braniff International to reach agreement with remarkable celerity on a 10 per cent reduction of earnings in order to avoid putting the firm once and for all in the red. The *Zeitgeist* of old Europe was even then very different from that of America.

In that winter-spring of 1932-3, I spent three mornings every week in the National Gallery copying a large picture by Rembrandt (or, as is now affirmed, by his school). I devoted a small part of my time to the courses at the London School of Economics or at Cambridge, particularly those of Keynes and Pigou, the latter of which was stuck in the tortuous maze of wage-goods and non-wage goods (hence the *Theory of Unemployment,* 1933).

If I did not have the good fortune to meet R.G. Hawtrey, a most skillful defender of the sound bank rate tradition, and hence a forerunner of the present-day monetarists, I was in an excellent position to breathe the atmosphere of continuous somersaulting, later repeated pedantically, which these very young logicians (referred to above) performed.

It seemed to me that even the logical *diableries* of the stages of production were going too far, as dished up in the evening seminars of the London School by F.A. Hayek (and, to a certain extent, with the personal endorsement of L. Robbins) as among the most effective intellectualistic instruments for distinguishing capitalist methods. That skillful and completely adiabatical approach on the part of the brilliant future author of *Use of Knowledge* and *Individualism and Economic Order* (1949), according to which endogenous variables create, and are created only by, endogenous variables, not only never led, at least in my case, to realistic conclusions, but took one to eminently abstract arguments, like the painted nail on the wall mentioned by Etienne Gilson on which only other formal compositions can be hung.

An even higher degree of abstraction characterized the contributions of the young logicians who were coming up in England at that time, both in the macro and micro-economic field of theory. Apart from the advanced mathematic and probabilistic expressions which in themselves are only verbal constellations and from their frequent hermeticism, what was involved was in the main logical paintings outside historical time and which hence neglected completely or were unaware of the very first principles according to which "initial data" are formed in economics. Mathematically speaking, moreover, they were ergotic constructions in which it is sufficient to know the situation at the last time, that is, exactly like the Markov processes, as if in reality economic movements were something analogous to Brownian movements.

With these alchemic superfictions, with this *bonne à tout faire* apodictics, thanks to which, by simply altering postulates and initial conditions, everything can be argued as a solution of the problems of consumption, catallactics, production, unemployment, one underlines, on innumerable occasions, only marginal aspects, or as if they existed at all times, only local laws, which can be expressed as pure differential equations, or as if no importance — whereas it is always preponderant — attached to the time structure of the economic world. It was Marshall himself (but also Bagehot, Cairnes, List, Knies, Wagner, etc.) who took up a cautious and sceptical stance when faced with this kind of speculation which is utterly unsuited to a world of continuous change.

Thus, even in the years 1930-32, the logical conditioning of economic problems was, both in the analysis and in the theoretical assessment of the young English economists, devoid of any explicit basis in time. In addition, into these strange models lacking as they do the indispensable temporal totality and therefore simplist (and which cannot even be proved by recourse to the classical coefficient of correlation, even where this is extremely high), there were introduced certain conditions of optimization (expressed as a rule by equally simplist functions) as if the aim of economic mankind was in this type of perfection.

Many and many a time I wondered, at that stage and after, whether the irresistible flow of this type of model, whose elaborate, extremely uniform writings remain in the memory only for some years — and I need merely cite, as an example, Pigou's *The Economics of Stationary States* (1935) — is really genuine and not rather a mere scholastic exercise. My hope is that all this will fade away and make room for realistic and constructive economics.

11. - In Berlin I did not find what I was looking for in the circle of those who, in the field of scientific economics, were opposed to or supported the dying Republic of Weimar, that is, a theoretical activity different from the static logical paintings and which would buttress general systematic principles designed to solve the extremely serious economic problems of the age — the problems of money, unemployment, and the great economic movements.

The last months of 1932 were one continual " happening " of highly dangerous situations. Early in September, the Government of Chancellor von Papen had issued a much debated plan for economic recovery, followed by the hard facts of increasing unemployment. At the end of 1932, the unemployed had risen to almost six million. One had only to walk to the Alexanderplatz to obtain a clear idea of the advanced state of social disintegration. At the Sportpalast, too, every evening witnessed tens and tens of thousands of demonstrators give vent to their political passions for or against Hitler or Thälmann, the Communist leader, but without any signs of genuine constructive intentions.

Occasionally, I was present at all these events in company of an American economist, K. Bopp (who subsequently became President of the Federal Reserve Bank of Philadelphia, and at that time was a very careful student of international banking problems). We went back to the Hegel Haus, an international centre a few steps from the Hauptstrasse which led to the Friedrich Wilhelm University, well aware of the imminence of an inexorable turn in history in one sense or another, not only because of the collective disintegration, but also because of the disintegration of the Ego itself. This development had in fact already been foreshadowed in the works of Brecht, Döblin, Kafka and Grosz, and, in the academic field, it was obvious from the lessons of the then almost seventy-year old W. Sombart (1863-1941). These lessons were delivered in a huge hall crammed with students who spontaneously split into two opposing factions, always in a state of agitation, as if they were from one moment to another about to come to blows — a completely different state of affairs from the polite debates of the Schmoller-Menger polemics on economic historicism which in the previous generation had provided a vital yeast for science in that very hall.

12. - I owe another economist of Berlin University, E. Lederer (1882-1939) the Director of the Berlin review the *Archiv für Sozialwissenschaft und Sozialpolitik,* which was once run by Max Weber and E. Jaffé, and the author of *Planwirtschaft,* a completely different " verbiage " from the one contained in the book — also published in 1932 — by F. von Gottl-Ottlilienfeld (*Der Mythus der Planwirtschaft*), if I may end this long article with two reminiscences of a certain interest.

The first is about my studies on indeterminacy which started thanks mainly to direct contact with the works of W. Heisenberg and C.H. Weil, continued in London and Berlin, and then enriched by the frequentation, in the latter city, of certain young university mathematicians. This led to various publications of mine (some of them dated 1933 and 1934). From professor G. Del Vecchio, I had received the demanding assignment of dealing with the book by R. Frisch, *New Methods of Measuring Marginal Utility* (1932), with a view to preparing, in addition, a review in the *Giornale degli economisti*. The careful reading of this remarkable work led me not only to apply my ideas on indeterminacy (which are recalled in a letter of mine to L. Einaudi of 27 February 1932 from Berlin) to the case of the individual supply of labour, but also to reconsider the whole question of the behaviour of that supply. Anyone who has had the patience to read my article on Frisch in the *Giornale degli economisti* (1933) will have noted that my conclusions (which are indeterministic) differ markedly from the deterministic point of view of Frisch, both as regards the non-mechanistic approach and as regards mathematics, apart from the fact that the conclusions paved the way for my definition, which remains unchanged, of logical indeterminacy, static indeterminacy and dynamic indeterminacy.

My second reminiscence from my Berlin days is of interest to the historians of economic doctrines. It is about the multiplier, which is called an "employment" one if referred to the works of R.F. Kahn, and a "general" one when it relates to investments and hence to the national income; for the general multiplier, and only for that, Keynes came to the forefront as an author. In my view, there is a still more general multiplier, and it is the one which I did not fail to point out in various books (and in my lectures). It is the idea of an assistant of E. Lederer, M. Mitnitzsky. (This moltiplicator was logically recomposed by me by setting up an analytic distinction between a positive and a negative multiplier, all this in my 1936 lectures and in subsequent writings.)

At that time, I was trying to follow closely the studies of certain assistants of E. Lederer at Berlin University, among whom there were some outstanding figures. Certain of them resided in Berlin, and others were already teaching elsewhere such as J. Marschak in Heidelberg and E. Altschul and K. Pribram at Frankfurt. All of them fled to America after Hitler came to power. They were kind enough to discuss their scientific problems with me. These were obviously also my problems, and all the more significant since I was fresh from my American impressions.[13]

One of these brilliant assistants was this very Mitnitzsky. As a university economist, he is no longer listed, not only because, when he fled to America, he changed his profession (and is now a senior executive of Shearson, Loeb, Rhoades of Park Avenue in New York), but also because he changed his name too — to Mark Millard. Yet his original name should be cited in any good history of economic doctrines, at least for his precursory article on the multiplier which appeared, for the first and possibly only time, in *Social Research* (1934) — the economics review of the New School for Social Research. The article bears the title "Economic Effects of Changes in Consumers' Demand" (pp. 199-218). This sets out the theory of the discrepancies between the flow of costs and the flow of prices, based, however, on the condition that the increase in the demand for consumer goods is not limited by the original expenditure produced by the new investments, but is greater during the period of time involved. Only thus will there be a lasting economic expansion.

13. - Perhaps the reader who pays special attention to the long term in history would have preferred something different from the exposition of a roving report which is limited to the dynamic years 1930-32. However, I would draw attention to two circumstances. The first is that the years are right in the middle of the period 1926-1939 which Professor Shackle has termed "years of high theory." It is true that he has mainly in mind the work of P. Sraffa (1926), D. Robertson (1926), R.F. Kahn and Joan Robinson (1932) and J.M. Keynes (1936) to whose oral tradition I have made only a passing reference. However, and without in the slightest trying to effect a comparison (which in any case would be impossible) between levels of high theory qualitatively different on the heuristic side, I feel that the years 1930-32 were years of very elevated economics pedagogy. It was thus that, precisely

[13] In this connection, I recall that, even in the immediate past, the links between American and German economists had become fairly close, as is clear from the article by F.A. FETTER called "America" and the one by SCHUMPETER "Deutschland" in *Wirtschaftstheorie der Gegenwart*, Vienna, 1927.

at that time, I parted company with the economic thinking which now predominates and which was already characteristic of the period 1926-39.

On the level of concrete economics, too, — and this is the second circumstance — that three-year period was one of exceptional originality as regards the historic events already under way or about to materialize in the whole world, an originality which perhaps oral historical tradition enables us to clarify better than its written equivalent.

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