An Emigrant from a Developing Country: Autobiographical Notes - I *

When asked by Paul Schilpp to write his *Autobiographisches*, Albert Einstein, at 67, felt as having to write his own obituary. One of the numerous things that differentiates me from Einstein is that even at 82 I do not feel that these biographical notes represent my auto-obituary. Those who have had as splintered a life as mine are likely to invert the old adage — *dum spiro, spero* — *dum spero, spiro*. Only by hoping and hoping could I live through four dictatorships as well as three wars all in my own backyard.

I have written these recollections only because they could serve as an instructive case study of life during the world's convulsions and upheavals that marked this century. The disconnected episodes threaded by my life are indeed relevant for such a purpose, even though I can describe them rather imprecisely, for I have never kept records of my daily happenings, never had a good memory for names, not even for faces (a failing that ruined many of my social ties).

I grew up and lived for many years in places and among a people that were not well-known outside their border at that time. History has changed all beyond recognition. Certainly, children in Romania no longer play, as I did, with antique coins gathered from almost any excavation in the town of my childhood. Having been a witness to that old world of seventy, sixty, or even forty years ago I considered that even some details of my past should have their place in this account.

I wish that I could but I cannot begin with the typical ingratiating preliminary by mentioning the notable feats of some ancestors. I never met my paternal grandparents who died before I was born; nor have I learned what exactly they used to do in life. My mother came from a humble family; her mother and three of her five siblings were illiterate.

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* First part of a contribution to a series of recollections on professional experiences of distinguished economists. This series opened whith the September 1979 issue of this *Review*.

My maternal grandmother lived with her unmarried daughter in a small adobe house half of which was below the ground level, a traditional design which was both inexpensive and thermally efficient. It was still standing as a lone relic when I visited Bucharest twenty years ago.

Constanţa, the town where I was born in 1906, had then only 25,000 inhabitants (now, more than 300,000). It was then an ethnic mosaic of Romanians, Greeks (some of whom may have descended directly from dwellers of the ancient Greek emporium, Tomis), Germans, Jews, Armenians, Turks, Tartars, and a few Bulgarians. Each nationality lived by its own precepts for felicity, but there never was even the smallest racial strife. I had classmates who wore the fez and turned toward the south during the morning prayer. Because the environment of my childhood was truly cosmopolitan, my ethos has remained so ever since.

My parents had a decent income, yet stringent in many ways. My Western friends are often flabbergasted on learning that I cannot ride a bicycle. But for my parents' income a bicycle meant a golden Cadillac. They could not buy me even shoes with laces which were one *leu* (then exactly 20 U.S. cents) more expensive than those with buttons, although I yearned for the former. My mother, an amazing addict to work, taught needlework in a trade school for girls until the 1930s. My father was an army captain when I was born. A couple of years later, he happened to come upon a higher rank officer slipping away with some meat from the soldiers' stock. During the ensuing altercation my father struck him. For striking a superior my father should have been court-martialled, but in view of the nastiness of the situation he only was pressed to retire, a typical face-saving procedure of bureaucracy. He had thus time to turn toward me to guide my development with immense love and patient understanding. Effortlessly I learned to write and read by the time I was four. My earliest occupation was to write the sequence of numbers from 1 to 99 on any piece of paper I could find (a sign of budding mathematical inclination, according to Lord P.C. Snow). My first mathematical discovery was how to write "one hundred", 100, and beyond. My second mathematical discovery, about which I can still remember my father's happy smile, was the Eratosthenes sieve. I was eight when he died and I have missed him not only as a father but also as a *man* that would prepare me for the plunge into the future harsh life.

In 1913, I became acquainted for the first time with war. Romanian armed forces had been sent into Bulgaria to put an end to a lingering episode of the Balkan war. No shots were fired, but the returning soldiers brought with them two threats. One was cholera, plenty of it. That was why, as it was explained to me, we had to boil the water. The other, which was political, I realized it much later via my economic studies of Romania. Because of its relevance for the cunning of economic phenomena, it deserves a parenthesis here. Bulgaria had been an integral part of the Ottoman Empire whose religious precepts prevented the emergence of private landed property, implicitly, of landlords. The Romanian soldiers, mostly peasants, thus found out that landlords need not exist and after their return commented openly about it. An aristocratic but wise Parliament voted with quasi unanimity to set the stage for a radical agrarian reform, an action, curiously, taken under no pressure whatsoever from Communism.

Nothing much happened during my first years of school. I only had less time to play around the Roman sarcophagi excavated from old Tomis. But I despaired of always having my hands stained with ink from the inkwell we had to carry to school. In my third grade there came a new teacher, Gheorghe Rădulescu, who was to influence my educational development in a profound way. He endeavored to stimulate children of ten or eleven, like myself, by problems that normally require algebra. Just for a deserving record: "Good morning hundred geese". "But we are not one hundred", objected the gander. "We would be one hundred if we were twice as many, one quarter more, and plus one goose". Whatever inborn mathematical aptitude I had was not outstanding, but that early training developed my lasting love for mathematics.

At the time, a newly founded military *lycée* was in tremendous demand for its unique excellence. Admission was through exams. According to the general principle of that time for the allocation of public educational funds, only children of parents of modest means could enter the exam for scholarships, of which there were only 23; the exam for 30 fee-paying places was held separately. Rădulescu insisted on preparing me to try for a scholarship, saying that if I win all he wanted was a demijohn of one of the famous wines of the region. For the exams I went with my mother to Bucharest. The competition for scholarships was frightful, more than 900 pupils. For the written tests, tables were arranged in a huge military *manège*. I learned that I got the 23rd scholarship (the last) from the results that came out the very day Romania entered the war against the Central Powers (August 27, 1916). On our return to Constanţa we saw the name of my teacher on one of the earliest lists of "killed in action". My child's soul began to be terrorized by the thought that perhaps, as in a mythical old Romania legend, his life had to be the price paid for my success.

Before long, under attacks from all sides, we had to leave Constanța in the greatest hurry by the last train of refugees. We spent the next two years in Bucharest under German occupation, living first with my grandmother in her old house. After schools opened, my scholarly duties were overtaxed by my lining up from midnight to dawn for the bread rations, and by my earning a little money selling newspapers on the street, moving the rubble of interrupted constructions, tutoring even grown-ups. During whatever free time I had, I read a few well-known authors, but since I was limited to secondhand paperbacks, I hardly benefited from my choices. From books I also learned chess but could only replay old games by myself. My love for chess has never left me, although no significant talent for it has ever graced me.

I must not fail to mention an enduring trauma caused at that frail age by the sight of blood dripping from the moribund wounded piled up like sardines in horse carts that kept coming from the front to a hospital across the street. It was worse than if, as a grown-up, I had been on the front line.

With the cessation of hostilities late in 1918, the military *lycée* returned from the unoccupied zone to its old home. A newspaper notice advised the students who had remained in the occupied zone to join the school. From some discarded wood pieces with an old rusted handsaw I made a chest for my belongings. My mother and my younger brother rode with me to the railway station in a hired horse cart. There was none of the usual emotional quivering at the separation: we believed that two years as a refugee under German occupation had tempered me sufficiently for an unknown and disarrayed reality. That we were not immediately confuted, as was natural to be, was only because I joined an educational institution, well-supervised, disciplined, and, moreover, virtually isolated from the outside world.

The *Lycée* of the Monastery on the Hill was so called because it was situated on the top of a hill, around a monastery church dating from 1499, of moderate but harmonious proportions and adorned by delicate, sober stone carvings. The buildings of the school, some of which were erected on the foundations of the old cells, consisted just of classrooms, a dormitory, a mess hall, a gymnasium, an infirmary, a couple of homes for teachers, a barrack for soldiers, a stable for the work horses, and a power plant. Briefly, a nearly self-sufficient settlement virtually isolated from the outside world. Most teachers lived in a nearby town and shuttled every day by a horse carriage. The students were not permitted to leave the school at all except for the summer vacation or for the shorter ones, at Christmas and Easter.

Everything, from meals, books, and clothing was provided free to the scholarshippers and, because of the inflation, practically to others, too. We wore a uniform and were submitted to a mock-military discipline. Each class was supervised by a distinguished officer, a few of whom later became university lecturers or professors. Between reveille at six and breakfast (black bread and tea) there was a half-hour of sustained trotting up and down the hill, save in case of blizzard. Except Sunday, five hours were devoted each morning to fundamental courses, with two hours of physical exercises in the afternoon (which together with *solfeggio* sessions formed my indescribable terror) and three hours of study after another tea. There was nothing else one could do but study even in the few nonprogrammed hours. And this is what I kept doing. Many teachers had a Ph.D. and were eventually called to university chairs. I may mention Octav Onicescu because he is known especially in Italy for his work on probability.

My aptitude for mathematics was noticed and sustained by two teacher officers, alumni of the *Lycée*. When I was thirteen (in the eighth grade) they introduced me to *Gazeta Matematică*, a review that even during the World Wars never failed to appear on the 15th of every month ever since it was founded (and financed) by a handful of enlightened engineers in 1895. It was a periodical devoted to medium-level articles, short notes, and especially proposed problems and their solutions. It covered matters up to the level of the average bachelor's degree in the United States or of the European special *lycées*. One may imagine my elation when so early in life I saw my name in print under a solution, a proposed problem, and later a note. *Gazeta* was a remarkable institution that fostered the mathematical interest of the youth also through stringent national competitions for some four to five prizes. It was not only because of luck that I got the second in 1922 and the first in 1923. My addiction to mathematics did not interfere with my other studies. Each year until graduation in 1923 I was ranked first in my class. The human capacity for pride is usually limitless, and my pride was certainly so when seeing my name carved on the marble of the honor list. Of course, I regret not having a photograph of that scholastic testimonial which has been blasted into dust by the present regime as it has done with many truly important memorials.

Although the heavy program of my *lycée* covered far more than the normal curriculum for seven years — more mathematics and more Latin — I had to take separate exams for some of the next grade's courses. By the end of the 1923 summer, I had also passed the exam for the "baccalauréat", a killing wringer, which was necessary (and sufficient) for the entrance to the university. After a sustained curriculum of thirty hours of solid classes per week and my additional efforts, my general knowledge was an operational toolbox. When in 1950 I told Harvie Branscomb, the Vanderbilt Chancellor, that as much as seventy-five percent of my working knowledge came from my secondary education, he branded me as facetious.

But in a distant retrospect, the system of the *Lycée* of the Monastery on the Hill was a blessing but not an unadulterated one (a thought that probably will displease the still living alumni). To live, year after year, isolated from the ordinary society, following a program on which one had no influence of any kind, sitting in class next to one and the same classmate, and sharing the same dormitory room with an almost invariable group was a most inadequate conception for preparing one to meet other people and develop fruitful relations according to the opportunities of each case. The vacations spent with the family did not help either. Given the situation, everyone endeavored to wait hand and foot on the "tourist". I do not doubt that because of this long extrasocietal period of my life I did not master the vital art of developing auspicious social ties with new acquaintances, or even the manners of cultivating the ties I fortuitously made. That is a far from small defect. Of two scholars known to me, one a much better scientist, the other a far more amiable personality, an Ivy League university chose the latter because he sold himself quickly and to more people.

The issue of where I should go to the university came up tragically, not for me but for my mother. Friends of my parents when paying some attention to me as a child used to ask what I wanted to be in life. My invariable answer for as I can remember was: "Mathematics teacher". That had remained my life dream. So, I entered the mathematics department of Bucharest University. But my poor mother had dreamed to see me an engineer, a rich man without material worries, and cried bitterly over my foolish decision not to go to the Polytechnic School (where I was exceptionally invited to join without the entrance exam). But I could not renounce my own dream (so I thought then).

Public education, the overwhelming institution in Romania, was free and welfare-minded. This time, too, I was allowed to enter a competition for a modest first-year scholarship open to students from families of low means — which I won. The department of mathematics consisted then of six professors and two lecturers. From the competitions of *Gazeta Matematică* I knew four of them. The academic demeanor was like that usual on the Continent at the time: the professor behind the desk, the students seated on their benches, ordinarily no dialogue. Together with a classmate I edited and lithographed the lectures on analytical mechanics of Professor Dimitrie Pompeiu (who was already world-famous for his contributions to the theory of functions), but he could not have cared less for our enthusiastic project. The only men who descended into the student's milieu were Professor Traian Lalescu and Albert Abason, a lecturer. They helped us form a student association for scientific activities.

The requirement for licence was four courses per year for three years. There were no graduate courses. After the licence anyone could submit a dissertation for a doctor's degree. The curriculum was specifically classical; for example, it included a full year of elliptic functions but not a single lecture on modern algebra or topology. There was little variation from year to year. An exception: while I was in my last year, Anton Davidoglu, who ordinarily taught mathematical analysis, offered at his pleasure a special seminar on the singularities of differential equations. What I learned from his masterly exposition helped me to arrive at the peculiar results of my 1936 paper "The Pure Theory of Consumer's Behavior". Curiously, in 1926 I did not think that it could be of value to me. Ordinarily, only George Ţiţeica, a founder of the differential projective geometry (a highly interesting field now fallen into oblivion), used to vary the topic of his free course for the third year. One that I cannot forget because of the beauty Ţiţeica instilled into it was the isoperimetric problem in its most general conception. Ţiţeica's lecturing taught me an affix to what I had learned from my *lycée* teachers. His lectures went so smoothly, so clearly, so impeccably that many students left his classes with the illusion that they had understood absolutely all and need not study any more. How unhappy must have been that great teacher to see that too many students failed his courses. All perfection, so it seems, has its drawbacks.

At the university there was nothing that could be called a library; however Carol I Foundation had a good, convenient, library to which we had free access. But the new books, which in the postwar years came out in waves, were available only from bookstores. To be able to buy some and also to supplement the meager support from my mother I did some tutoring. In my last year I even accepted to teach in a *lycée* newly established in a small town for the special purpose of making secondary education accessible to peasant children. On that occasion, witnessing the peasant's staunch yearning for learning was an impressive revelation for me: rain or snow, pupils came on foot from villages miles away. In that year the university courses were often interrupted by student demonstrations, symptoms presaging worse things to come.

In June 1926, I graduated with the highest grade: *foarte bine*. And for the coming academic year, I got a position at my old *Lycée*, where I could prepare in seclusion for the next hurdle: the "aggregation". That term, without a correspondent in English, denoted a special exam to qualify as a secondary teacher. It consisted of a deeper test of the licence materials and of the ability to teach secondary classes. The committee was presided over by Samuel Sanielevici, a professor at the University of Iaşi where the exam took place. (As my mathematics teacher, ten years earlier during the German occupation, Sanielevici boxed my ears although he seemed to like me very much. Curiously, the only other teacher to have done so was my fateful Rădulescu). I again ranked first for men, and in the competition for women so did my future wife, Otilia Busuioc, who had been my classmate from the first university year. I chose the teacher position at the boys *lycée* of Constanţa and applied for a scholarship to study for a Ph.D. in Paris.

On that occasion, in October 1927, I went to see Traian Lalescu, toward whom I rightly felt most attracted. Lalescu was a remarkable mathematician who along with Vito Volterra and Erik Ivar Fredholm broke new ground for the theory of integral equations. But he also was interested in the welfare of his country. Repeatedly he endeavored to submit economic problems to scientific methods. Everybody knew of "Lalescu's curve" for salary adjustment. Having felt very badly about the lack of statistics, with great pathos he succeeded in convincing me during that visit to study statistics further, not pure mathematics. "Mr. Roegen, when you come back from France, we will have to do some great work together", he said. In November I left for Paris and, as planned, registered myself at the *Institut de Statistique*. After completing my course requirements (for which I ranked first) I returned home for the first time in 1929. Naturally, with a heart jumping with felicitous expectations, I went, immediately to see him, only to learn that he had died just a few days earlier. For a while, I was unable to turn away from the housekeeper who informed me of still another tragic loss in my life.

In the fall I was supposed to return to Paris to complete my dissertation. A snag came up that deserves mention because of its interesting sociopolitical object lesson of the kind Pareto's *Mind and Society* is full. The rule was that the scholarships for studies abroad be given from a special fund by a commission of university delegates on the basis of academic recommendations. Until 1928 the Liberal governments respected that rule, so my first two scholarships came automatically. But the new government of the National Peasantist Party (the party I joined years later with great zeal!) began distributing scholarships to their own favorites by administrative decision in disregard of the rule. The head of the office for the scholarship fund decided one day to indiscreetly inform me that the fund was almost exhausted. I went to plead my case to the Secretary General of the Treasury who, shamefully, told me that only the "commission" distributes the scholarships. Troubled, my mother found that a former colleague of hers could obtain for me an audience with Mihail Manoilescu, the renowned advocate of industrial protectionism, then Minister of Industry. But Manoilescu barely looked at my credentials from the University of Paris and he, too, sent me to "the Commission". Time and again, my mother approached Al. Lapedatu, an elementary school teacher from Constanţa, then a National Peasantist deputy in the parliament. A pure idealist, he could not believe my story but promised to try. Next day he handed me my application with the administrative resolution of that very Secretary General: "It is approved". The only time, I believe, when I got a scholarship by favoritism. The work for my degree was thus saved.

For many students life in Paris was not easy. There were no dormitories nor mess halls for students. We had to rent rooms in hotels (old apartment houses) with which the entire Latin Quarter was studded and ate in fixed-price restaurants, most of which sold books of meal coupons at some discount. With the unavoidable *pourboire* the simplest meal was from five to six francs, and the continental breakfast at the "bistro", about two. Monthly rents (no bath) varied around 300. My stipend was 800 francs per month, which amounted to about 32 US dollars of that period. Since what remained after food and shelter was next to nothing, I had to rely on some money from home. Fortunately, there was, just like in Romania, absolutely no tuition fee. With holes in the soles and even the uppers of the shoes and with worn-out shirts, students in general were nevertheless happy, very happy to be able to learn and learn. That episode of my life often came vividly to my mind in connection with the situation of past decades when almost everywhere scholarships have been viewed just as salaries.

Contact between professors and students was, just as in Romania, at arm's length. On a couple of occasions I was able to speak with Professor Lucien March at the General Bureau of Statistics where he was a director. Alfred Barriol, a man who was the incarnation of good will, taught financial mathematics and also was a director of the PLM, the greatest railway company in France. He graciously lent me an old calculating machine to use in preparing my dissertation. The machine was two feet long and so heavy that I had to transport it by cab! However, I believe that even though all who studied in Paris would agree with the poet who said that

> *Tout homme a deux patries,*
> *La sienne et puis Paris*
> (Everyone has two fatherlands,
> one's own and then Paris),

all have also remained with a strong bitter aftertaste because of the way French students jeered at their foreign colleagues by calling us *métèques*, a deriding term meaning "stranger" in ancient Greek.

In addition to my obligatory courses, I attended regularly two others. The first was an uninspiring course on finite difference equations offered by Henri Lebesgue at *Collège de France*. The second was the epochal course of mathematical analysis taught at *Sorbonne* by Eduard Goursat, another great. Bent with age at 70, Goursat entered the classroom after, as was the *Sorbonne* decorum, an *enchaîneur* wearing a neckpiece with the university seal announced "*Messieurs et Mesdames, Professeur Goursat*". At that point we all stood up. Men like him deserved that and would deserve it even today. What Goursat, moving slowly and speaking very softly, wrote on the board, just as in Ţiţeica's case, never needed the sponge for correction.

Occasionally, I also attended some conferences bearing on the theory of probability offered by Maurice Fréchet at the *Institut Henri Poincaré*. Because Gaston Julia was a rising star in mathematical analysis I also went to one of his lectures. On that occasion, seeing that the man who entered the classroom had a black globe as a head, I was shocked to the point of fainting. Julia had to wear such a total masque with just small holes for eyes and mouth because a shrapnel had exploded on his face during World War I. He was what the French called a *gueule cassée*. Burning with patriotism, young French intellectuals, like him, fought on the first line of fire. Their momentous story was told by Julienne Félix in a luminous epigraph about her brother

Robert, "who, at the age of four, discovered the rule of casting out nines; and, at the age of seventeen, was a student at the *Ecole Normale Supérieure*; [and] who, at the age of nineteen, died for France". Mathematics being taken as a case in point, after the old greats — say, Borel, Cartan, Darboux, Fréchet, Goursat, Hadamard, Lebesgue, Picard, Poincaré — departed, there was hardly anyone to replace them. I have kept wondering ever since whether the highest stratum of intelligentsia should not be exempt from going on the battlefront. The answer is difficult and no cost-benefit analysis could settle it.

My only other extracurricular contact was with Fortunat Strowski, an expert on Blaise Pascal who had a seminar on that philosopher. Şerban, the son of my former professor of astronomy, Nicolae Coculescu, introduced me to it. Şerban (Pius Servien, by pseudonym), was developing into a critical philosopher of probability and an initiator of statistical analysis of languages. He took me to the seminar because many participants, even Strowski, had trouble with Pascal's argument for the incommensurable lottery advantage of believing in God. Incredibly, even after several meetings I was unable to convince them that, if e $\rightarrow 0$, while G $\rightarrow \infty$, e x G may be finite, even infinite. Together with Şerban, they all demurred at an analytical definition of probability.

Because the novelty of the field kept my interest alive, I attended my required courses scrupulously. As one would expect, I never missed a single lecture of Emile Borel, even though his course did not add much to his available textbooks on probability. Borel, just like Lalescu, had multiple extracurricular activities and often came unprepared. Once he was stuck, unable to think of the next move for some embarrassing minutes that seemed like ages. When I spoke to suggest a way out, not only Borel but also everyone present at once fixed their eyes upon me as if spelling out "Who are you to help Borel?".

One of the most valuable experiences for me was the unusual *Cours de statistique*, recently introduced by Albert Aftalion. An outstanding economist among the French, Aftalion proved himself in that task a devoted and talented teacher as well. He had used statistical data in his studies of business cycles and of money. But after hearing of Karl Karsten, Warren M. Persons, and the so-called Harvard Economic Barometer (all mentioned often in his lectures), he became convinced of the need for a systematic method. To be sure, the title of his course stood as a curious nonsuch on the graduate program posters of the Faculty of Law to which he belonged. However, Aftalion, although without much mathematical training, must have taken great pains to prepare his lectures, for the task of teaching exponential trend to law students was Herculean. So, all his lectures were to a tee. And to judge from the fact that the University Presses of France published a lithograph of his course in 1928 and followed with several reprints, his program kept attracting students. Aftalion endeavored to make us see what is actually obtained by the use of a statistical formula rather than to know how to manipulate it. Witness his splendid metaphor I have always used in my classes: partial correlation serves to put out the sun, as it were, so that we can see the relevant stars. Admirable, penetrating thoughts such as these have disappeared from the French writings with the disappearance of the Aftalions, the Borels, or the Poincarés. When I was a visitor at Strasbourg University in 1977/78 it chagrined me to see that, as my colleagues told me, the fashion was to write *à la* Lacan (the psychoanalyst), that is, in a verbose, foggy style. For instance, J. Attali, the economic counsellor of M. Mitterand, mixed even music with economics in his *Bruits* (1977). After a glowing review of it in *Le Matin*, I wrote the editor wondering how can M. Mitterand know when his counsellor speaks of economics and not of music. Harry Johnson's blunt (and certainly too strong) verdict, that France has no genuine economists anymore, was not surprising.

I also paid a visit to François Divisia, another intellectual wounded on the battlefront, who was lecturing at some of the special schools in Paris. His remarkable *Economique rationnelle* appeared the same year and in the same collection as Darmois' *Statistique mathématique*. In a friendly atmosphere, we talked about his dynamic index and about his solution for the industry whose average cost was constantly higher than the demand for its product (a point that especially struck me). But knowing then only little economics, I was unable to benefit much from that encounter. The third economist with whom I came in contact was Jacques Rueff, who taught the regular course on monetary phenomena, which he limited to statics. A second volume of his *Theorie des phénomènes monétaires* (1927) supposed to deal with dynamics never came out. Rueff already moved in highest society, he was Inspector General of Finance, one of the most powerful bureaucratic posts, and the right-hand of Charles Rist in the frequent international monetary arrangements (including Romania's). From his course it seemed clear that the science of economics was not his true calling, a point confirmed by his ending as the only economist to be elected to the prestigious *Académie Française* which could include only forty "immortal" *littérateurs* and by the great success of his, a ballet-comedy, *La création du monde*.

The course I avidly absorbed was mathematical statistics, not only because of my great interest in it but also because of the immense delight it offered me as a mathematician. It was taught by Georges Darmois, who had succeeded Borel in that chair. Darmois was both a consummate mathematician and an inspired and devoted teacher who had a warm faint smile for all of us. His *Statistique mathématique* that just came out in 1928, reflected his exceptional comprehension of the particular nature of statistical analysis. Because of the extent of the problems covered as well as the elegant mathematical treatment, that treatise was by far the best at that time and, in my opinion as an old hand, a lasting paragon. Darmois's contributions to statistics pertain to many other branches, but his *Statistique* did not count little in his election as President of the International Institute of Statistics (1953-60).

The reader does not need to be told why I chose a topic of mathematical statistics for my dissertation: "On the problem of finding out the cyclical components of a phenomenon". As was the practice then, when I thought it to be good enough I deposited a copy of the manuscript with the secretary of the Faculty. Naturally, Darmois was nominated the chairman of my committee, which also included Alfred Barriol, M. Huber, Lucien March, and Jacques Rueff. My defense on June 27, 1930 was received with the highest qualification, *très bien*, but the committee liked my work so much that all members signed my diploma adding "*avec les félicitations du jury*".

On July 7, Borel communicated to the French Academy of Sciences (C.R. pp. 15-17) a *résumé* of my periodogram method and the October 1930 issue of *Journal de la Société de Statistique de Paris* contained nothing besides my dissertation in full. My method permitted the discovery of all the numerical parameters of a time series of the form

$$y = Q(t) + \Sigma_i B_i \cos(a_i + \omega_i t) + e_i,$$

where $Q(t) = \Sigma_k A_k t^k \exp(b_k t)$, and e_i, an independent error. As I found out after I could read English, my method has some points of contact with that proposed by Arthur Schuster in 1898 which served as a basis for virtually all subsequent writers. But my approach still is superior, I believe, to all similar periodogram analyses because, after the replacement of the above formula by an equation of finite differences, it takes into account the random covariances resulting from that transformation. The fact that Professor Joseph Schumpeter used it in his 1939 *Business Cycles* and that an ampler presentation in English appeared in the *Proceedings of the International Statistical Conferences* (1947) and was reprinted in *Econometrica* (1948), ought to have arrested the attention also of those who were not acquainted with the French sources. But references often follow a devious link. It is because Herman Wold probably did not know my French sources that he did not mention my work in his 1938 dissertation, a less powerful method than mine. And, by inertia, he did not correct the omission even in his 1968 article in IESS, in this way influencing virtually all subsequent writers. So, I must content myself with Borel's and Darmois's recognition of more than fifty years ago, which is a long time indeed. As many have rightly exclaimed, the scholar's only award is that one's peers should know what one has done — good or, even, bad.

Having learned from Darmois some of the epochal contributions of Karl Pearson, I began longing for the possibility of studying with him. Luckily, I obtained a scholarship for that — about 15 pounds per month, far more than for Paris because of London's higher cost of living. The trouble was that I did not know even what "good bye" meant. Luck helped me. In Paris I had known a young Englishman, a master of French, Leonard Hurst. In late November 1930 the economic crisis was biting deeper and deeper, two Hurst youngsters were out of work, the small bedroom of Leonard's father, who had died recently, was vacant. So, the Hursts were willing to take me as a paying guest, seventeen shillings and a half (a little over four dollars) per week. That was very little, indeed. But the Hursts were a working class family, living in a realtor's small house on Leicester Road, the working class section of Putney which was separated by a common from the other, of the highest class. We subsisted usually on potatoes, cabbage and gravy, with bread and lard for breakfast. However, for more than one year, the Hursts surrounded me with great attention and warm consideration, things that cannot be bought. Priceless indeed was the patience of Mrs. Hurst, a retired elementary teacher, who helped me learn the new language which for a long time kept sounding to me as an uninterrupted sequence of diphthongs without consonants. Once, in despair I thought of giving up and returning home.

After I could command a few basic words, I gathered all my courage and went to see Karl Pearson. He received me so naturally that I immediately felt as in heaven. The Galton Laboratory at the University College was a small institution set primarily on research; almost all visiting fellows had a doctoral degree. One could see Pearson almost any time, unannounced through a secretary. His office was long; between his desk and the door there was a couch and some chairs around a tea table. Every working day around three o'clock, tea and biscuits were served for Pearson and anybody who wanted to join him. News about one's own or other's work were then passed around. In addition to meeting Karl Pearson I soon had other surprising experiences for my Continental background. The first was the possibility of checking books out of the library. I must confess that I said then to myself "this library would not last long". Yet it has. I made another discovery when one morning a fellow standing at the foot of the stairs in the Laboratory stopped me to ask where I was going. After I told him that I was going to Professor Pearson's class, he replied that for that very reason I must see the bursar. I did, only to find out that I had to pay tuition, in my case ten guineas or so per semester. I felt outraged — even in Romania one did not have to pay for going to school! Another surprise was that everybody at University College was very friendly. Before coming there I even asked myself what word they would use instead of "*métèque*".

What is truly great you did not learn from Pearson's classes; you learned it from the immense number of his contributions to which he would inspiringly direct you. Pearson was a unique scholar, amazingly prolific in a vast range of interests. In addition to laying single-handed the proper foundation and forging the basic tools of statistical analysis as we know it today, he made important contributions to applied mathematics, the theory of elasticity, anthropology, sociology, eugenics, biometry, and, with a singular insight, to philosophy through his *Grammar of Science* (which, regrettably, is not properly appreciated by present tastes). To any topic that would be touched in a casual conversation, Pearson could add an enlightening observation. He was also a hard worker *sans pareil*: his writings number almost seven hundred. He was a stern, acidulous at times, defender of his positions. The man who appeared under this face in several bitter controversies — theoretical, with R.A. Fisher, and political, with many others (including J.M. Keynes and A. Marshall) — was nonetheless a most considerate and warm teacher. He invited me to spend a weekend at his country home in Surrey, and wrote with a quill (as he always did) a detailed travel schedule for me. It could not have passed through my mind as a possibility, but when I arrived he was on the station platform. Damning that there *had* to be a wedding that very day to hire the only horsecab in the village, Pearson, at 75, insisted on carrying my valise over a couple of miles to his home.

One of his greatest prides was his method of determining distributions of random variables — observed or theoretical — by moments. The idea had its roots in the Laplace transform of the characteristic function of the moments m_i. I already knew that from Darmois, from whom I also learned Pearson's method of using only the first four moments for constructing distributions of virtually universal application. As I see it in retrospect, Pearson's method is analogous to an arithmetic approximation, say, of π as 3.1416, *i.e.*, to retain only the most relevant moments, m_1, m_2, m_3, and m_4 from their infinite sequence. Pearson's stroke of genius consisted of the additional observation that every one of these moments represents an important structural characteristic of the frequency curve: the location, the dispersion, the asymmetry, and the kurtosis (one of the several terms coined by him). The seven types of distributions obtained by that method found splendid applications in the studies of sampling distributions by Student and R.A. Fisher. But Pearson expected that one may go much further than that, namely, to arrive at some general formula for the moments of sampling, moments which could lead to an analytical expression of the generating function for some particular cases at least. The problem of moments seemed to dominate the day. Several statisticians of note — *e.g.*, V. Romanowsky, C.C. Craig and even R.A. Fisher — had already dealt with it. So, I decided to try my hand at it, too.

In the conference I had with him Pearson encouraged me and even seemed pleased that I would work on his *Lieblingsthema*. The result of my steady effort over a full year, during which I could hardly do anything else, was a long memoir that occupied 43 pages of the May 1932 issue of *Biometrika*. But the long list of tedious, repelling formulae for the moments of sampling moments (six quarto pages of them!) did not seem to reflect any regularity; they could only dispense other students from sweating if they needed any. The only meaningful result was a proof of the fact that the semi-invariants of large samples from normal distributions are uncorrelated, and, further, that this property characterizes the normal distribution. (This last theorem, for any sample size, was proved later by R.C. Geary, also by using moments.)

Science often advances by negative results, such as the impossibility of perpetual motion or of a speed greater than that of light. Perhaps, the long list of formulae in my memoir served as proof that Pearson's expectation was not realizable. Be this as it may, Pearson's method of moments, even that of four moments, has not prevailed but not because its role has been taken over by that of the Baysian analysis. The real reason, I submit, is to be found in the sociology of the literati. The tone of R.A. Fisher in referring to Pearson's method, which undoubtedly was a valuable innovation of a man his senior by more than thirty years, who also had ploughed and sown where Fisher was then harvesting, certainly wounded Pearson deeply. The result was an unsavory rift that marred the relationship of those two great minds. So, after he followed Pearson in the Galton chair in 1933, Fisher felt no inclination (as it might be natural) to propound or support any idea of his foe. Nor did the statisticians of the later generations, who were interested rather in maintaining good bridges with the new pontiff of *Biometrika*.

The shelving of Pearson's idea of four moments is most regrettable. To wit, Sir John Hicks in a paper summarized in *Econometrica* (April 1934) proposed to use it for determining the frequency distributions of investment risks. Stochastic dominance could also be applied with much greater precision if the involved distributions were determined by Pearson's method rather than relying only on the stochastic intervals established by the standard deviations. Particularly nowadays computers could be programmed to print out the proper frequency curve without waiting. But there are sufficient signs that statisticians now return to Pearson's conception of statistical analysis.

Pearson was a Machian who did not like to erect science on pure creations, such as those used by R.A. Fisher. Nothing, in my opinion, would constitute a better portrait of Pearson as the forceful writer he was and of his profound attachment to the method of moments than the first stressed sentence of his very last (and greatly enlightening) paper published in *Biometrika* of June 1936, a couple of months after his death:

Wasting your time fitting curves by moments, eh?

Once, Pearson suggested that I pay a visit to the London School of Economics where Arthur Bowley was lecturing. But I had already studied the French translation of Bowley's textbook. Above all, I said to myself that I wanted at first to be a pure mathematician, instead I have become a statistician. I have nothing to do with economics and I do not wish to become an economist, never!

However, sometime in 1931, someone from the Rockefeller Foundation came to the Laboratory to explain to me the program they had then for postdoctoral fellowships and to test my interest in a fellowship in the United States; I suspected that no other but Pearson was behind that move. In my dissertation I applied my method to the series of rainfall in Paris because I was convinced that the economic cycles are not symmetrical like the trigonometric cosine (a conviction that was the source of a paper of mine to be mentioned later). Yet when shortly thereafter I received the application form, optimistically, I already saw myself working with Warren M. Persons at the Harvard University Economic Barometer on the application of my method to their three curves system. Perhaps some property of a Fourier series may save the day. After my return to Romania I received the favorable notification of the award of a Rockefeller Fellowship for the academic year 1933. Two reasons stood in the way of my leaving at that time: my mother was seriously ill and my first substantial project, *Metoda Statistică*, was not yet ready for the printer. The volume of over 500 pages, combining the viewpoints of Pearson and Darmois, appeared in 1933. It still could serve in any course aimed at explaining the significance of concepts rather than at manipulating formulae. On the side, I wrote a couple of didactical pieces and also a paper on a problem broached in a particular case by H. Poincaré. Over hurdles of differential and functional equations I proved that only for three distributions (one of which is the normal one) are the most probable and the probable values of a characteristic parameter equal (1932).

It may be well to explain now that, just as A. Burk became A. Bergson, my present legal name appeared first on *Metoda Statistică*. In all my previous papers and the dissertation my name was Nicholas St. Georgescu, as it appeared in Schumpeter's *Business Cycles*, and, strangely, was kept unchanged by R. Fels in his condensed version of that work. In the first edition of M.G. Kendall's *Advanced Theory of Statistics* I was indexed even as "St. Georgescu".

By October 1934 I was ready for the voyage to "the Moon", as it may have seemed at that time to anyone from Romania. The Rockefeller Foundation booked me on the President Roosevelt, a United States steamship of only 11,000 tons but apparently very sturdy. It trailed the Mauretania through a ferocious fall storm of the mid-Atlantic without any damage, whereas the Mauretania had a mast broken and several wounded. We docked two days late and, moreover, on a Saturday. Until Monday when the Foundation offices would open, I hesitated to walk farther than the corner of the hotel: New York stunned me not as much by the skyline from the ship as by its internal agitation. Stacy May, the director for the social sciences, was a kind, friendly person, highly competent for that job, who contributed in great measure to making my work and life during my stay totally enjoyable. The Rockefeller Foundation continued to be the splendidly oriented institution it has always been.

Cambridge seemed quite companionable in comparison to New York; no agglomeration of any kind then. After finding easily a room next to the Harvard Yard, I tried to locate the offices of the Harvard Economic Barometer and was quite intrigued to find that no one seemed to know anything about that organization. Asking and searching, I finally found out that the organization petered out soon after the 1929 Black Tuesday because just the week before it predicted that all was in perfect order. The organization thus no longer existed when I counted on it in my application, but I had no way of knowing that. The whole thing, I feared, was vitiated *ab ovo*. Once more, I felt the earth sinking under my feet.

Thinking that something could nonetheless be saved by contacting the person who was teaching statistics, I obtained an appointment with Professor W. Leonard Crum, a highly respected statistician. I was stopped cold when he routinely just asked me: "What can I do for you?" Ill-at-ease, I told him my reason for being there and hazarded to ask whether I could visit his research outfit. After the obviously discouraging answer, which I had expected, I left with my heart heavy with despair. (Later, I learned that Crum not only had written a neat paper on periodogram analysis for the *Handbook* of H.L. Rietz, but he also had been an important associate of W. Persons.) There was only one solution left, I thought, to ask Stacy May to send me back to Bucharest.

But after sleeping on it, it occurred to me to approach the professor in charge of business cycles theory. After all, it was Albert Aftalion, a specialist in that field, who had made me aware of the importance of periodogram analysis. At that point, praying more than hoping, I asked for an appointment with Professor Joseph A. Schumpeter, a rather strange and completely unknown name to me then. Like Traian Lalescu seven years earlier, but in a much subtler manner, Schumpeter trapped me in his own scientific bailiwick. Instead of simply asking what he could do for me, he wanted to know what I had done and what I wanted to do. On hearing about the topic of my dissertation, he immediately called E.M. Hoover, then his assistant, to see together how my method could be used for Schumpeter's planned *Business Cycles*. Learning that I had a degree in mathematics as well, without any ado Schumpeter invited also Wassily Leontief who at the time taught mathematical economics. You can remain here — I said then to myself — to see how your method would work on economic time series and, in addition, try to learn more than you knew from Divisia's *Economique*. Leontief's lectures were a model of clarity. We quickly became intimate friends, and so did later his wife, Estelle, and mine.

Schumpeter was a great teacher but only for those students who were sufficiently advanced to see the gist of his remarks, almost every one a valuable suggestion even for a doctoral dissertation. Naturally, I stayed out of his classes for some time to come. But Schumpeter had still other attractions for any would-be student of his. He was still single, living as a guest in Professor Frank Taussig's sumptuous house. Taussig even seemed to look after Schumpeter as after a younger brother. I once saw Taussig pulling Schumpeter's collar up as they were walking together in the Yard on a chilly day. Schumpeter was then the darling of all Harvard Houses where he was constantly invited for an enchanting afterdinner speech. Above all, he found time to preside over the weekly meetings of a circle of Rockefeller Fellows from Europe who happened to be there at the same time with myself. They were Oskar Lange, Fritz Machlup, Gerhard Tintner, and Nicholas Kaldor (who arrived one year later). The meetings concentrated on mathematical economics and lasted endless hours after dinner. Leontief, Ed Hoover, and Paul Sweezy (another Schumpeter assistant) also were among "the regulars", nonetheless the foreign accent still dominated. Machlup usually attended only when the topic involved diagrammatical analysis in which he was a deft expert. Kaldor was a special figure in the group. When he first learned about its main interest, he demurred joining it. "I do not understand mathematics", he said. He came nonetheless by curiosity once and continued as an active participant; he was so well trained in economics that often he was able to better by a verbal argument a mathematical point. Almost every mathematical paper published by the members of the group during that period had first gone through the severe wringer of our meetings. Schumpeter always came forward with a helpful, inspiring suggestion, although a favorite student of his once taxed him as a fake because, as he claimed, in Schumpeter's formulae on the black board some sign was always missing. The circle's star of mathematical economics was Lange. He was a very friendly colleague always ready to spice the conversations with little stories and much humor. There was a great deal of unassuming socializing among the fellows of the circle, including Schumpeter. Now and then, Schumpeter would offer a dinner, with a menu composed by him, in an upstairs room of the Harvard Club. Those memorable dinners lasted so late in the night that we were the last to lock up the Club.

At that time the fashion in the United States was that statistics serve mainly business needs. The most advanced level was represented by Edmond Day's textbook. Crum's course followed an elementary text he had co-authored. So, there was no reason to approach him again. I attended just a few of Taussig's classes because my economics was not yet up to their level and also because each session consisted of continuous dialogues between Taussig and the students about the previous assignments, a rather typical teaching style in the USA. It made me recall my gaffe in Borel's class. I found two other professors of great help. Edward V. Huntington, the author of *Continuum* (1917), one of the earliest monographs on order types, was a man born to be a kind and efficient teacher. He taught a related course from which I learned for the first time things I used twenty years later in some papers on the nature of pure uncertainty. E.B. Wilson, the devoted pupil of J. Willard Gibbs, then connected with the Harvard School of Public Health, crossed over to the Department of Economics where he lectured on mathematical economics as a sort of preparatory acclimatization for the graduate students of that time. He was familiar with Pareto since his substantial review of the *Manuel* in 1912. In QJE his papers were intercalated with mine and often he passed the chalk to me for the class lecture.

Sometime in May 1935, Stacy May sent me a flat traveling stipend urging me to visit other universities so that I should not go back to Romania "without knowing America". Although not happy with the idea of leaving my Harvard nest, I drove across the Continent as far as the small community of Palo Alto; Stanford did not yet exist as a town. On the way I stopped first in Chicago, where I had the good fortune to meet Henry Schultz; he died together with all his family a few years later in a car accident. Schultz was a very pleasant as well as interesting host with whom I explored his renowned notion of statistical demand and his recent incursions into Paretoan theory. In Bloomington, Indiana, where I stopped to see C.C. Craig, who had written on moments, I also met Harold T. Davis, who was to become a great mentor of dynamic economic systems. Davis had a niche at the turn of the staircase in his home where he kept a copy of Newton's *Principia* behind a continuously burning candle. It characterized the spirit of that prolific writer who advanced knowledge in several directions.

No one could have thought of bypassing Colorado Springs, the seat of the Cowles Commission. Alfred Cowles, III, a former broker who had escaped unscathed by the 1929 crisis but became seriously ill, established that institution so that he could be busy but without any worry. It was a highly attractive visit, which combined Cowles's almost daily entertaining with exciting meetings at the Commission. The prominent members were Charles F. Roos and Victor von Szeliski. The Commission's project was to find a mathematical formula for predicting the stock exchange market. A few papers on that topic appeared in the first volumes of *Econometrica*. Our discussions easily reached a heated level for I was strongly opposed, as I have always been, to describing historical, that is, unique processes by a mathematical, necessarily ahistorical formula.

The persons I wanted to see in California were Holbrook Working, a statistician mentioned by Aftalion, and Griffith C. Evans. Evans, a mathematician famous for his signal contributions to a domain developed by H. Poincaré, D. Hilbert, and V. Volterra and now almost deserted, had also written a highly appreciated monograph in which he considered the equilibrium of a market that depends also on the price change. At that season, Working was on vacation. Evans was giving a summer course to a small class of students with little, hybrid knowledge, which once he turned into a seminar for my work on Pareto, probably with little effect. In the ultimate analysis what we got out of California was just a touristic interest and gasoline at 7 cents a gallon.

On the trail back, I stopped first in Princeton where I met a Romanian scholar, David Mitrany, who had been the editor of *Manchester Guardian* and was a specialist in agrarian problems. Being invited to join the Institute of Advanced Study, he was able to arrange for me to see Albert Einstein. Naturally, I did not feel much enthusiasm for that since I did not see what I could say to Einstein. Just to say something on meeting him, I mentioned that in my model of stochastic choice I used a hyperbolic quadratic form similar to that of his ds^2. Visibly unimpressed, he passed on to ask me whether I played any musical instrument. Probably disappointed by my negative answer and out of kindness toward a strange youngster, at the end of the visit he said that mathematical economics must be a right endeavor. I missed a great opportunity: to ask his opinion about the entropy law. But at that time I did not know, what I learned thirty years later, that he was a great defender of it.

I next stopped in New York and New Haven. In New York I saw Harold Hotelling, whose name was already on the lips of every statistician and mathematical economist. Not having kept notes, I cannot

remember what precisely I talked about with that great, yet modest and gracious, scholar. But circumstances being as they were then I could not have thought of asking his possible afterthoughts about the paper he had written on natural resources in 1931. I thus missed another unique opportunity.

In New Haven, I called on Irving Fisher, already retired. A convinced vegetarian because that regime had saved him from tuberculosis in his youth, he invited us to a Sunday dinner when everyone had turkey except him. He then conducted me through a basement full of shelves with numberless reprints of which he liberally gave me many more on diet and his ideas about 100 per cent stable money than on economics. But some helped me to become aware of his solid economic contributions. Like many *emeriti* nowadays, Fisher must have been unhappy with his younger colleagues, for when I told him that I had not yet come in contact with any Yale economist, he shot back, "Don't worry, there is none".

My stay in the United States allowed me to know also the multiple faces, good or bad, of that great, soul-searching and soul-stirring nation. To begin with, my wife (who having been ill could not travel together with me) landed in New York on the day before the Thanksgiving of 1934. I had reserved a room at the Iroquois Hotel, recommended to me as clean and moderately priced. After the Thanksgiving dinner my convalescent wife felt sick and I asked the room service to bring us two teas and two rums (an old wives' recipe). But the switch girl harshly admonished me: "We do *not* serve liquor in this establishment!" We had to make do with tea only, but I kept wondering thereafter how I could have known that the Iroquois was dry. Next day as my eyes fell on two brass plaques on each side of the entrance, one with "Iroquois", the other with "Restricted", I thought that I solved my puzzle. It was only an illusion, for when I told my story as a joke on tourists, my Harvard friends assured me that "Restricted" meant "Only Caucasians". My surprise turned into a shock when next year I saw the main street of Brookline (a suburb of Boston) dressed up with huge placards full of Nazi slogans. And I was bewildered when a professor's wife confidentially told us that anyone with Jewish ties stood a poor chance of promotion at Harvard. According to some, Henry Schultz in retaliation had turned down the invitation to occupy a special, heavily endowed chair recently created at Harvard. But apart from the shocking discrimination against the blacks and isolation of the Orientals, in my journey across the whole country I found no other symptom of racial hostility. America had to be then, as always, a melting pot.

At the time, motels did not exist even as a name. Rarely there were "cabins". Most of the time we stopped in homes with a sign "overnight guests". At 75 cents on the average, including at times a farmer breakfast, we were received friendly, our accent notwithstanding. On some occasions, the host adjusted my carburetor or even washed my car. They would also make tea for my wife when she happened to be sick.

By and large, the United States enchanted me sufficiently to tell my flabbergasted Romanian friends how marvelous it would be if Romania could by the impossible become the 49th state. I also was happy to serve as president of the Romanian Society for Friendship with the USA. I was often taxed as a traitor, neither then nor now an unusual type of reaction almost everywhere.

During my stay of one year and a half at Harvard I published in quick succession four papers. It was from Schumpeter's remarks that I began to admire Pareto's work on mathematical economics to which I later added with enthusiasm his overpowering sociological edifice. From the outset I realized that Pareto's mathematical skill wavered from highs to lows. In his 1896 *Cours*, for example, he was up on Irving Fisher who in a review of the work wrongly censured him for a point concerning the integrability of total differential forms. On the other hand, in his *Encyclopédie* article (1911) Pareto claimed to derive the indifference varieties from the simple, common assumption that the quantities demanded depend on the price constellation and the quantities initially possessed. The claim involved an undetected mathematical slip which I exposed in my first paper on economics (*QJE*, 1935), in which I also touched upon the integrability issue as well as on that of time in choice. In the next paper (*RES*, 1935) I attacked a new problem, the pricing of limitational factors, in connection with which I endeavored to define "limitational" more broadly than Ragnar Frisch, the originator of the term, had done. Kaldor (*RES*, 1937), as if bent on confirming my above-mentioned opinion about his talent, pointed out that I had overlooked one limitational type. As there were no *RES* in Romania, I learned about Kaldor's point only after my return to the United States in 1948, when it helped me arrive at a general analysis of limitationality in opposition to the kin concept of limitativeness I introduced then (*AE*, chaps. 7 and 10).[1]

[1] *Analytical Economics: Issues and Problems*, Harvard University Press, Cambridge (Mass.), 1966.

I wrote the third paper (*QJE*, May 1936) at the invitation of Taussig, the editor, to serve as an honest broker on a controversy between A.C. Pigou and Milton Friedman concerning Pigou's recipe for measuring the elasticity of demand. In his criticism, Friedman adopted a strict interpretation of mathematical constant, while Pigou's argument considered a *nearly* constant elasticity of demand. What I wrote was a lecture about the absurdity of qualifying a dimensional entity as small or as large and also showed how the assumption of a very large number of demanded commodities may lead, depending on the type of utility function, to either a zero or a finite value for the income elasticity. The verdict was against Friedman. As we all know, if you disagree with him however little, Milton Friedman would clobber you: "You are *totally* wrong!" So I felt immensely gratified when Milton introduced me before a lecture at the University of Chicago as the only economist who had proved him wrong. Of course, my lecture, on Brazilian monetary inflation, "was *totally* wrong".

The leading article in the August 1936 issue of the *QJE* was my fourth essay "The Pure Theory of Consumer's Behavior". It was a solid essay not because of its size (48 pages) but because it covered several entirely new aspects of that subject. I began by formulating a special postulate (later known as the continuity postulate) and showed it to be indispensable for the existence of indifference varieties. So, I regret to have to disappoint those who have kept attributing my postulate instead to Herman Wold. True, in his 1943/44 articles he did formulate a similar postulate but failed to cite my article although it had been advertised by Paul Samuelson in two 1938 articles on "revealed preference". In my article I also related the consumer choice to indifference directions subject to a few transparent axioms which entail the peculiar property that a nonpreference direction, if prolonged, retains that quality. Topologically, it means that the indifference planar elements form a convex structure relative to the origin of coordinates. In one section I dealt with another novel problem, the stochastic binary choice (between two alternatives). The way I approached it led to a stochastic distribution of some particular varieties at every point of the commodity space. One surprising result was that stochastic indifference is not transitive, a point which formed the object of numberless lucubrations until very recently. The model also cast light on Taussig's famous description of actual demand as a penumbra. Probably because its mathematics is hard going, subsequent writers have preferred a scheme based on a stochastic distribution of the ordinary indifference varieties. However, my exposition was not faultless. An error, which remained undetected until 1958, was exposed in my contribution to Ragnar Frisch's *Festschrift* (*AE*, chap. 5). On that occasion, I established a few relevant theorems and an unexpected result: assuming that A is the most frequently chosen in the binary alternatives (A, B) and (A, C), A may nonetheless be the least frequent choice in the multiple one (A, B, C).

I consider the 1936 essay as one of my salient contributions to economic theory and, to judge by Paul Samuelson's repeated praises of it, a pathbreaking development of the theory of choice. The piece of evidence is the clarification of the integrability puzzle which originated with the lesson Vito Volterra wanted to teach in his critical review of Pareto's *Manuale*, namely, that although a total differential equation is *always* integrable for two variables, this is not true for more than two. Hence the paradox, which tormented Pareto thereafter: why the indifference varieties can always be derived from market data for a world of two commodities but not necessarily for one of more than two. Volterra has never made a more infelicitous intervention and no other mathematician seems to have duplicated Volterra's example of the disorienting muddle one can create in a field by taking account only of mathematics. Volterra did not see that integrability is not the economic issue, the economic issue is whether the market data entail a transitive order of *binary* choice, the cornerstone of Pareto's theory. Volterra confused a mathematical concept — the integral varieties of a total differential equation — with an economic one — the indifference varieties. This is what I have denounced in several places and, in an accentuated way, in the conclusion of that paper. To prove my point I showed that the integrals of $Xdx + Ydy = 0$ may also represent a family of spirals around a focus, in which case no ordering can be established even in a purely formal way. Furthermore, to forestall the objection that my argument was relevant only for a two-dimensional case, I pointed out that the intersections of the three-dimensional indifference elements, $Xdx + Ydy + Zdz = 0$, with a budget plane form a two-dimensional structure in no way different from that of the two-dimensional $Xdx + Ydy = 0$, and demonstrated that nonintegrability of the former would correspond to spirals, and integrability to closed curves.

Schumpeter realized that, because of what I had published before coming to Harvard and the four articles worked out during my short stay there, at the age of 30 I was a promising scholar. Due primarily to his judgement, Harvard wanted to keep me on. Schumpeter also wanted to write an economic analysis in collaboration with me. But incredible as it must seem, I declined — not the only time when I fouled up my scholarly career, but the worst such case. Schumpeter, together with the others at Harvard seemed to hope nonetheless that after looking at conditions in Romania I would return and arrangements for that possibility were made. The day before our sailing, sometime in late May, Schumpeter came to New York and took us to dinner at the Waldorf Astoria (still in splendor then) to convince me to accept his outstretched hand. Only after many years was I able to comprehend how hurt he must have been by the refusal of an inconsiderate youngster. Yet he later wrote me several times urging me to change my mind. Perhaps, it is for the better that those letters, testimonials of my maleficence, have been destroyed together with all my old files by the Communist regime. Our cabin on the SS Washington was covered with so many flower bouquets that at first I thought it was for another couple, a honeymooning one. My Harvard friends wanted to say "Bon voyage and come back".

That happened more than fifty years ago and I cannot recall, not even imagine why I made that inconceivable gross blunder. The Georgescu-Roegen of that time appears to me now as another individual, another mind. One reason that interfered with my vision was that all my education had been supported by the public funds of Romania and that even my Rockefeller Fellowship counted on a spot earmarked for Romania, just as the other Fellows were for each country. I ought therefore to serve in the capacity expected of me. Yet I cannot believe it was just the call of the wild.

On my way back to Bucharest I stopped for a while in England via Paris. Hitler had remilitarized the Rhineland just a couple of months earlier; France could not react alone and Great Britain was not willing to indispose the Führer. Under the government of Léon Blum, in Paris almost every package of cigarettes or of matches contained a small label with "*pour qui et pourquoi*", a slogan painted even on the sidewalks. All that gave me the goose pimples.

At LSE, I just caught the tail of a series of seminars held by F.A. von Hayek. My knowledge of monetary matters was quite slim then, but after listening to Hayek's splendid and methodical exposition I became aware of one infirmity of my mind, that of not being able to feel at home in monetary theory. After earnest efforts all I could gather from the revered literature are the endless controversies not only between the Keynesian and the monetarists, but also between the members of one and the same fraternity. Monetary theory has no leading analytical thread. Bankers and financiers are generally successful only because they ignore the economists. The monetary domain appears to me as a "phantasmagoria", to use Irving Fisher's characterization of the economic world with the difference that I do not believe that it can be clarified by the torch of mathematics. Max Planck, as we know, found even economics too entangled for his mathematical propensity.

Understandably, I have written only one paper on monetary matters, "Structural Inflation-Lock and Balanced Growth" (*EEM*, Chap. 7).[2] In my personal experience with the galloping inflation in Romania and with the troubles caused by the slowing down of inflation in Brazil after the fall of the João Goulart government I found inspiration for that paper. Its moral, in which I have never ceased to believe, was that in most cases inflation is a perverse way to govern: it switches real income from some people to others surreptitiously and without divulging the pickpocketing. Because the essence of his theory can be explained by one of the simplest economic diagrams with the 45° line, Keynes immediately became the darling of economists. But he became even a dearer darling of politicians who could thus fulfill their demagogic promises by government spending without any tax increases, now a catholic policy. To me, Keynes and especially the Keynesians seem to have believed that because a diabetic feels better with shots of insulin, everybody else would also feel better if given such shots. This fable pinpoints Keynes's definite impact on economic epistemology: all that counts in economics are the aggregates, structure is irrelevant.

At LSE I was looking forward to meeting R.G.D. Allen, whom I admired for his remarkable paper on utility theory. But he seemed rather cool. Perhaps, in a somewhat un-British way, he resented the exposure of a mathematical slip of his in the Appendix of my 1936 paper.

During short stops in Oxford and Cambridge, I met Jacob Marschak, in whom I was to find great support later, and the Hickses, kind, approachable and interesting as they have always been. Sir John thought that the issue of nonintegrability was a will-o'-the wisp, as he put it later in his *Value and Capital*. I was unable to move him an iota, for like all British economists he was to a large extent a Marshallian (as he has always confessed) and, hence, the existence of a utility function was a matter of pure faith for him.

[2] *Energy and Economic Myths: Institutional and Analytical Economic Essays*, Oxford, Pergamon Press, 1976.

It was a great surprise for me to find Vienna as enchanting as ever despite her retrogressions from World War I and the shock she suffered through the 1929 crisis. Young statisticians, economists, and philosophers would meet informally in small groups to settle the universe anew. Tintner was already back from the United States at the Institute for Business Cycle Research. The Institute, headed by Oskar Morgenstern, followed a similar conception to that of Ernst Wagemann, the founder of *Konjunkturforschung*. One member of the Institute was an exiled Romanian mathematician, Abraham Wald. He worked out a few vignettes of mathematical economics, but later was the inventor of sequential analysis. The whole atmosphere contrasted with the gun powder I could already smell.

Nashville

NICHOLAS GEORGESCU-ROEGEN