Nicholas Georgescu-Roegen about himself*

Nicholas Georgescu-Roegen§

Two things fill one's conscience with increasing wonder and awe, the stars in heaven and the moral law in oneself.

Immanuel Kant

Proem

It would be superfluous to tell the reader how high my spirits were lifted by Michael Szenberg's invitation to write an account of my life philosophy. But as I starred to think about the task, a fear came to me, the fear that my acceptance would be taken as an implicit presumption that I am a philosopher like Plato or John Dewey, for example. Even though I have not been a professional philosopher, I certainly "have done philosophy" One does philosophy, I think, not when one reasons about the practical problems of a community or the mathematical structure of quantum mechanics, bur only if one treats in a thoroughly free inquiry issues that cannot be rested at a workbench. It is in this sense that I claim to have done philosophy, probably more often than I have exercised any other intellectual expertise. However, although like all who have done philosophy I have asked questions about things, their nature, and their relations among themselves and with the human mind, I have gone further: I have also asked questions about questions.

Everyone's way of thinking is influenced by the events of his or her life. As some have maintained, if was Einstein's experience at the Berne Patent Office that developed his interest in how to test our ideas about things. Not ignoring the considerable difference of proportions, the observation has been especially true to my shattered life. In the country in which I was born and spent the most informative part of my life, Romania, I lived under four dictatorships and three wars. all in my backyard. That history instilled me with a kind of Paretoan view of human societies. Romania was at that time a struggling, overpopulated, peasant dominated culture and economy. And as I came to learn the economics professed in the capitalist world, I was struck by the claims of that discipline that it was a representative

---

* Este artigo está sendo publicado com a autorização da Cambridge University Press. Originalmente, esse texto, *Nicholas Georgescu-Roegen about himself*, foi publicado em Michael Szemberg ed., *Eminent Economists. Their Life Philosophies*. Cambridge University Press, 1992.

§ Nicholas Georgescu-Roegen is Distinguished Professor of Economics, Emeritus, Vanderbilt University.

guide not only for capitalism but for absolutely all economic conditions. I was evident to me that standard economics could not represent an agrarian economy, and hence could not be a guide for it. I thus acquired a special eye for issues ignored by the standard economic persuasion or by ordinary economic analysis. I learned "philosophy" from many consecrated philosophers, but my own philosophy sprouted from two great teachers of mine: Karl Pearson and Joseph A. Schumpeter. From Karl Pearson's splendid *Grammar of Science* and from my listening to him for more than one year, I reached two philosophical tenets. First, contrary to the old epistemology, the stochastic form is not the peripheral but our only possible representation of natural laws. By implication I came to hold further that randomness, not just haphazardness, is an essential ingredient of phenomena. Second, given the human cosmological condition, I construed that for us nature consists of just what we can perceive. Beyond, there are only hypothesized abstractions about which a metaphysician may say anything with complete certitude, since nothing is controllable. Our laws of nature aim not at explaining phenomena, but at saving them, as Pierre Duhem calligraphed the philosophy initiated by Ernest Mach and embraced by Wilbem Ostwald and Karl Pearson. Even Albert Einstein's philosophy was largely Machian: in 1936 be explicitly stated that "physics consists of a logical system of thought [that] can only be arrived at by free inventions. " Not one word about "reality." My philosophy it is spirit Machian: it is a particular kind of epistemology that is little concerned with the science of knowledge, or with the cognitive process itself, but mainly with the problem of valid analytical representations of the relations among facts.

What stirred me mainly in this direction was a frequent sin in mathematical economics. Choose any formula used by a physicist and ask him what it represents factually. He may invite you to his laboratory to witness the actual phenomenon described by that formula. However, in economics there is a vast and growing literature of purely mathematical exercises that correspond to absolutely no facts, not even to physical ones. If one starts only with mathematics, one is trapped inside it and cannot even think of the epistemological issues in my own sense.

A statistician turned into an economist

For my statistical dissertation at the Sorbonne, I followed the line of the smallest effort for a mathematician and thought up a mathematical method for discovering the hidden periodicities of time series. Since economic time series were then, and still are, enjoying great consideration, I felt (as I still do) that economic phenomena are not governed by a mathematical network. If the stock exchange market were governed by a mathematical system, I reasoned, no one would have thought of setting one up. I thus applied my method to

the annual rainfall in Paris.[1] After learning later about the three time series used by the famous Harvard Economic Barometer, I became curious to see it applied to them. I was overjoyed when the Rockefeller Foundation gave me an opportunity to visit that organization. But before I reached the United States, it had closed shop. Seeking some scope of activity, I naturally thought of contacting the professor who taught business cycles, whoever he might have been. This is how, quite unintentionally, I met Joseph A. Schumpeter, the man who directly and through his writings was to have an even greater influence on my thinking than Karl Pearson. Every one of his distinctive remarks were seeds that inspired my later works. In this way Schumpeter turned me into an economist - the only true Schumpeterian, I believe. My only degree in economics is from Universitas Schumpeteriana.

Because of my mathematical preparation, I naturally had to plunge first into the writings of Vilfredo Pareto, whom I learned to value as the greatest mathematical economist ever (with great accent on the noun, for Pareto's mathematics were not laudable: my first paper on mathematical economics was on one of his missteps). Being especially concerned with the valid representation of facts analytically, I felt that the neoclassical utility theory needed a fundamental postulate, listed as Postulate A in my 1936 *Quarterly Journal of Economics* essay: on a continuous move from nonpreference to preference with respect to a given basket, we must pass through a place indifferent to that basket. I affirmed for the first time the necessity of postulating the binary indifference (an idea that has been tacitly adopted by many later writers). But some immediately assailed me: "The postulate is totally superfluous. How can you move from nonpreference to preference without passing through indifference?" That objection helped me later move toward dialectics.

At that time an object of great agitation was a paradox created by a criticism of Pareto by Viro Volterra. As is well known, on a second round, Pareto argued that the same map as that based on binary choices could be derived from the reports of a sleuth who followed the individual in a great number of market situations. Clearly, those data serve to establish a total differential equation in the commodities space:

$$\sum a_i(x)\, dx_i = 0, \qquad 1 \leq i \leq n. \tag{1}$$

It was about this point that Volterra committed a gaffe by countering that equation (1) is *necessarily* integrable if and only if it involves only two variables. The paradox of why Pareto's second method is valid in an economy of only two, but not in one of three,

1 I should mention that my methods was communicated to the French Académie Sciences by Emile Borel, and the full version of my dissertation filled the whole October 1930 issue of *Journal de la Société de Statistique* de Paris.

commodities was still undefeated. After searching through my box of mathematical tools, I concluded that Volterra and all who after him worked on the issue of integrability completely ignored the real snag. I prided myself on having cut a Gordian knot by proving in the 1936 essay that, contrary to what Volterra and everyone else held, even when the integrals of (1) exist, they cannot be identified with the indifference varieties without some additional *factual* reasons. To explicate this point, I considered the integral curves of (1) on a particular two-dimensional space, namely, the budget plane with three commodities. Under simple conditions, those curves always exist (Volterra's point). Two diagrams proved my contention: in one, the integral curves were logarithmic spirals around a singular point, a focus; in the other, they were ovals around a center, also a singular point. The first corresponded to the nonintegrability case in three dimensions; the second, to the standard utility map. In the first case, *even though the integral curves were there, no preference scale can be constructed on their basis.* Fourteen years later (1954), on these two pictures Paul Samuelson based a parable, highly pleasing like all others of his pen.[2]

I further observed that, whether a utility map exists or not, the consumer always tends toward a point of saturation, either absolute or relative to the possibilities of the budget. The budget equilibrium, therefore, is always a point of saturation. Next, I pointed out that, whether or not equation (1) is integrable and whether or not saturation is at infinity, the direction from any point toward the saturation point is always one of preference. On this basis, in place of the principle of decreasing marginal utility or of decreasing marginal substitution I proposed the *principle of perseverance of nonpreference directions;* that is, once a direction becomes one of nonpreference, no good can come from persevering in it.[3]

Because of my tenet that random is an essential element of phenomena, I also initiated the idea of stochastic choice. That study led to several novel by-products, the most salient being that indifference is not necessarily transitive.

Romanian "exile"

If my literary activity during the two short Harvard years (1934-36) seemed rather unusual, it was probably because, as Schumpeter once said, being a novice I was "able to

2 It does not seem at all strange to me that Samuelson was prompted to reread carefully my 1936 essay by na article of H.Houthakker in which independently of my paper, Houthakker argued that no indifference curves may have a spiral form.

3 I expressed the perseverance principle by an inequality on which before long, Paul Samuelson based his epochal idea of revealed preference.

see aspects that trained economists refuse to see and unable to see others that they took for granted." Be this as it may, that performance together with what I had published earlier must have been responsible for Schumpeter's intention to write a treatise on theoretical economics with me, which in rum led to an offer to join the economics faculty. I am now unable to say exactly why, but I simply turned my back on the fantastic chance of being a co-author with Schumpeter and becoming "Georgescu-Roegen of Harvard" I left for Romania.

I looked forward to helping my native land become a happier place for all. The Parcae, however, had decided differently; for being an economist and a statistician as well, I was given the undistinguished, tedious task of organizing the economic statistics at the Central Statistical Institute, followed by that of colligating the daily statistics of foreign trade. (At the time even orthodox countries had foreign trade clearings.) But a truly great wringer was lying in wait for me at the end of the war, the arduous job as general secretary of the Armistice Commission. For about six months it meant long, tedious, and stressful discussions, often lasting the whole night, with the representatives of the USSR Control Commission.

Since the Yalta and Potsdam conferences ultimately shattered all my hopes of seeing the world reorganized on the principles for which Great Britain and the United States had entered the war, I had to flee Romania before I was thrown into a jail from which no one has ever come out alive. According to the Communists' precepts, I was indeed guilty of three capital crimes: (1) being the servant of capitalists as a Rockefeller fellow and then president of the Romanian Association for Friendship with the United States, (2) being a "member" of the National Council of the Peasantist Party, and (3) being an ardent defender of Romania's rights as the secretary general of the Armistice Commission. Since the safest way of escape seemed to be stowing away on freighter, together with my wife I stealthy entered the Constantsa harbor in the middle of the night of February 13, 1948, surrounded by bribed smugglers. I then felt as if the past twelve years in Romania were scooped out of my life.

Before too long, my Harvard friends Edward S. Mason, and especially Wassily Leontief, found a means of bringing me back again. I arrived at Harvard in early July 1948 and what I found boggled my mind for days, for truly great avenues had been opened in economics during my "exile"

A disenchantfed neoclassical economist

Soon after my return to Romania in 1936, I entered into a wonderful friendship with Andrew Edson, the secretary Of the U.S. legation in Bucharest. One day Andy said softly: "Romania is economically underdeveloped because your institutions are silly. The legion of doormen who just sit at the door of every high functionary, public or private, produce nothing to motivate their pay." Andy, a strong believer in the neoclassical dogma, then opened my eyes to a violation in my own backyard of the sacrosanct neoclassical principle of marginal pricing. That icy shower on my religious confidence in mathematical economics started me worrying and thinking and thinking. The solution, when it hit me, was that marginal pricing does not maximize the national product proper - an idea that would undoubtedly strike a standard economist as a ridiculous product of some economic ignoramus. Yet the fact is that only in the lands of plenty does the marginal principle maximize a complex of product proper *and* chosen leisure. In the lands of scarcity, however, people must work as long as they can, to the point of zero marginal productivity of labor, as illustrated by the splendid institution, not too old, of the gleaners. In conditions of scarcity, income distribution is made not according to marginal pricing, but according to some institutional rules (as within most families, yours too, I think). Even in the advanced countries, we should note, the consumer is not guided only by a quantitative set of commodities as standard economics claims. Individual behavior is also affected by how one can realize one's want work for a dollar, beg for it, or pick the cash register, actions judged according to the corresponding social matrix, not affiliated with a quantitative scale.

Another bestirring lesson also came from Romania. Because the peasants always looked to the townies for what to do, the Communists wanted to bring the urban masses to their knees. The plan was to provoke a runaway money inflation so that the decreased value of the bills would stop the peasants from bringing food to the towns. From what I then knew of standard economics, I judged the plan flawless. I cannot describe my surprise when, at a meeting of the National Council of the Peasantist Party, another member, a former village school teacher and an old Peasantist, smugly shot at me, "You do not know the peasants, my friend; they will still sell for any money because money has always been *summum bonum* for them." Nor can I describe my public shame when the developments proved him correct. The Communists then resorted to an unparalleled trick in history: they declared that, on August 15, 1947 all old money was no longer legal tender. Each person (supposedly) received new money of about one U.S. dollar in exchange value.

I had never felt any attraction for monetary theory, and the Romanian peasants convinced me to steer away from its unthinkable quicksands. Yet later, rubbing myself against the

disturbing facts of the monetary conditions of Brazil, I reached another heretical conclusion to which I still cling firmly. Contrary to the general tenet of the professional establishment that inflation is the best strategy for economic development, inflation is the most perverse way of governing. Another invisible hand, a Keynesian this time, picks the pockets of the masses who cannot borrow at a privileged interest rate now and pay later.

Epistemology of economics

Science without epistemology is in so far that it is thinkable at all primitive and muddled.

Albert Einstein

Shortly after my return to the United States I completed several papers consisting of significant results. I am saying this not to boast but to illustrate the special usefulness of epistemology in general.

Why include only one structural component in the analyticalrepresentation of a process when we know that there are many?

This was the epistemological question that struck me first when approaching Leontief's system. I then proceeded to find out what would happen if each industry could choose from a set of recipes satisfying Leontief's basic assumptions: labor is the only primary factor of production, and return to a scales is constant. The theorem that I presented at a seminar of the Harvard Economic Project (March 22, 1949) became known as the "substitution theorem" It states that in equilibrium each industry must operate with only one particular recipe out of its own technological horizon. As I showed by the transparent diagrams reproduced in a paper presented at the December 1949 meeting of the American Economic Association, there are also some singular cases in which, for the equilibrium recipe, the ratio of labor to output is in the limit zero, the catalytic labor that represents the futurists dream of unlimited technological progress.

An analytical representation of business cycle by nonsymmetrical waves

Trough discussions with Professor Schumpeter about my idea that business cycles are not "cyclical" another epistemological began pressing my mind: how can we represent them analytically? My answer was presented at another seminar (April 1949) in the paper "Relaxation Phenomena in Linear Economic Models" In it I debunked the idea that decumulation is the

reverse process of accumulation and proposed that the business turning points are relaxation phenomena when the law of one phase suddenly changes into the other. To represent this conception of business cycles, I used a sequence of two alternating phases", F_1, for the upswing, F_2 for the downswing. Schumpeter, who always attended those seminars, left me breathless when he asked with which of the two phases the process began. He used this sort of question against the theories that explain depression as the product of prosperity and prosperity as that of depression. One must then know, Schumpeter used to say, whether business cycles began with an overproduction or an underproduction of apples in the Garden of Eden.

Vicit *Pareto*

Because Paul Samuelson believed that a Houthakker axiom that assumed away spiral formations provided the definitive liquidation of the nonintegrability puzzle, be keenly endorsed it (1954). Yet the snag of the singularity was still overlooked. To prove the ineffectiveness of the new axiom, I countered with an analytical example (1954) in which the integrals of (1) involved a singular point a node, later renamed pole - which naturally impeded the establishment of an ophelimity index. Within such a map the consumer could move around the node and arrive at the same subjective state as the initial one, a possibility not denied by Houthakker's axiom. A feeling that Pareto was right in a deeper sense than I had shown until then began pressing me. But it was only at the symposium in Pareto's memory (1973) that I presented the ultimate analytical example in which (1) is derived from *community* demand schedules and is nonetheless completely integrable without any singularity whatever. Those integrals do look like the usual indifference map, but we know only too well that they cannot be associated with it. Therefore, to identify integrals with the indifference varieties we must know beforehand that an ophelimity index exists, as Pareto did. This conclusion vindicates Pareto (and by the same coup exposes the behaviorists' folly of rejecting all subjectiveness).

Dialectics versus arithmomorphism

Science began as a trove of propositions describing some observed phenomenon. It still consists of such a trove, though restructured under the continual pressure of the limit of human memory. At first, some propo - tions were classified in groups, as in the Hammurabi Code. Later, some great relief for memory came with writing and the convenient materials for doing so. Taxonomical classification of the kind we find in biology and even in chemistry was the next advance. Ultimately, some ancient land surveyors on the Nile discovered that if one has memorized

(A) The sum of the angles of a triangle is two right angles, one need not also memorize

(B) The sum of the angles of convex quadrangle is four right angles.

That was the germ of the *theoretical* science (not of every science). In a theoretical science all descriptive propositions must be flied, not alphabetically (as in a directory), or taxonomically, but in a *logical order as* in geometry. Through an intricate logical sorting all known propositions can be divided into two classes:

(a) Every α proposition follows logically from some β propositions,

(b) No β proposition follows from any β propositions.

All we have then to memorize is the set (β), for by simple ratiocination we can rediscover all the others. The greatest advantage of logical filing is thus the economy of thought, a point brought up by Ernest Mach and Kari Pearson. For many, though, memorizing is much easier than ratiocinating; many a student prefers courses based mainly on memory. Why are there such sciences?

It was this question that set me on the path to dialectics, for I observed that logic, though a marvelous accessory for our thinking, has its limits set by its own power. Logic works only with a restricted class of propositions, such as

(A) The hypotenuse is greater than a leg but is totally impotent when it comes to propositions such as

(B) Culturally determined wants are higher than biological needs.

Worthy of special note is that all concepts in proposition (A) are as *discretely* distinct as any clear symbol, say, *m*, 2, or ∞. Discrete distinction is the specific property of real members; a number retains its distinct individuality even within the arithmetical continuum. This is why I have proposed to call such concepts *arithmomorphic*. No arithmomorphic concept overlaps with its opposite. The boundary between the two is vacuous: *tertium non datur.* A vast number of concepts, however, overlap with their opposites. That is, A and non-A may be both true (which does not mean *tertium datur).* As Max Rheinstein once remarked, "Even the dictatorship of Hitler... had democratic features, and in the democracy of the United Stares we find certain dictatorial elements." Proposing, to refer to concepts of this kind as *dialectical,* I have obviously followed Hegel, yet only for a short while. In my view concepts are means of expressing our thoughts, not legislators of nature and society as Hegel and Marx (in a switched way) claimed.

Arithmomorphic concepts are absolutely invariant: "square" meant the same thing to Euclid as it does to us today. If in our imagination we alter an angle of a square no matter how little, it is no longer a "square" But we can squeeze an "oval" a great deal before it is no longer an "oval." Dialectical concepts, though not discretely distinct, are distinct. They are separated from their opposites by a dialectical penumbra that is in turn delimited by other dialectical penumbras. A baby will be old when he will be ninety; but no one can say when he will just become old. On this point as well as on similar others, Bertrand Russell argued that one can determine that event by associating it with a convenient number. What he proposed was to define a particular democracy as that of the United States at, say, March 15, 1896, at π o'clock p.m. But my epistemology faults him. In relation with facts we cannot use paper-and-pencil numbers, 1 or π. A pointer-reading belongs to dialectics. This issue recalls one of Schumpeter's incisive protests: "There is no sense in our case in asking: 'Where does that type [of entrepreneur] begin then?' and then to exclaim: "This is no type at all!"

Entities that change qualitatively are necessarily dialectical. The epitome is "species" which is dialectical because, as Charles Darwin put it, "it includes the unknown element of a distinct act of creation." The present temper that insists that "species" is an arithmomorphic concept is tantamount to a return to Lamarck, to species created once for all. Diehard logical positivists naturally would forever insist on exclusive arithmomorphism. Yet these apostles are utterly mistaken,, for not even they could plead any case without using more dialectical concepts than arithmomorphic ones. Is "a sufficiently large sample" or "verifiability," for instance, arithmomorphic? Eminent scholars - like Bertrand Russel and Percy Bridgman - who made a point of honor in combating vagueness offered us the best proof that reasoning with dialectical concepts is not only quite possible, but also indispensable.[4] Only it is far more difficult than doing algebra when, as it often happens, the tip of the pencil may move faster than the writer's mind.

This brings to mind Blaise Pascal's immortal dichotomy *esprit geométrique* and *esprit de finesse* of which the widespread arithmomania of our epoch would rather not hear. Even the bland way in which I put it at a 1955 symposium - "There is a limit to what we can do with numbers, as there is to what we can do without them" - was anathema to the worshipers of the Almighty Arithmomorphic Concept. At the famous David Novick symposium organized by Seymour Harris (1954), Lawrence Klein proclaimed that "nonmathematical contributions to economics [are] fat, sloppy, and vague." A verdict on Adam Smith, Schumpeter, or Simon Kuznets? And because at that time crime and drug

4 For completion, I should add that a still greater economy is achieved by introducing some thought-up propositions (ω), which added to (β) propositions shift many more of these to the (α) set.

addiction were not on the rise, Robert M. Solow could get off unscathed for having reasoned that mathematical economics must be really good because everyday there is more, not less, of it. But the actual crux was exploded later when Salim Rashid produced the document of the time: because of the views of the economic establishment, junior economists, be said, must grind papers by the mathematical engine, lest they perish.

Dialectics and similes

There is justice in the positivist objection that communication with dialectical concepts cannot be precise. With this point in mind, I tried to mirror dialectical concepts by some analytical pseudo-images to which I have appropriately referred as *similes*. While struggling with modern utility theory, my epistemology took offense at the absence of any mention of wants or dislikes - the real movers of our actions. I was thus delighted to discover that wants of all forms had formed the pillars of the older consumer theory propounded by T. C. Banfield and Carl Menger, now fallen from favor. Standard economists have chosen just to putter with the second differential of a nominal, opaque blanket named "utility," a term with which Jeremy Bentham himself was unhappy to the end. The neoclassical rationalization was that want cannot be defined precisely. To be sure, want is a dialectical concept. If want had been a rigid arithmomorphic element, the human species would not have been able to survive under the radically different environments of its long past. Wants even form a dialectical hierarchy· above those that respond to biological needs (which are common to all human) come those that correspond to social propensities (common to all members of the same community), and above these, the purely personal, disordered whims. On the basis of this hierarchy, we can justify two of the most essential propositions about us. First, the principle of marginal utility is just shorthand for the law that any human satisfies his wants in their hierarchical order. And, second, contrary to the most unfortunate fallacy of standard economics, most wants are interpersonally comparable. All humans - the Rothschilds and Hollywood stars included will spend their *only* taller for quenching thirst and assuaging hunger. Of course, interpersonal comparison between the upper wants of two rich people, one enjoying a motorboat, the other a villa, makes no sense. Since standard economics is a discipline of the lands of plenty, Lionel Robbins' famous theses of interpersonal noncomparability fits in place there, bet only there.

After seeing chat Carl Menger's table of wants cannot explain how one distributes a given income with given prices among one's various wants, I proposed a simile diagram in which to every want there corresponds a domain delimited by ordinary fines and located according to the general hierarchy. It was by this factual analysis that I proved for the first time the necessity of lexicographic order for economic theory.

Similes had been occasionally used earlier in other respects. One of the most interesting cases is the theory of probability. The Laplacean, the frequentist, the betting coefficient, and all other definitions tried out are all arithmomorphic similes of probability, which is a dialectical notion in the truest Hegelian sense: it starts and ends with itself. It is because of this dialectical nature of probability that all the mentioned similes have ended in contradictions. The probability associated with natural phenomena is dialectical because its backbone, randomness, is a dialectical notion, for randomness implies irregularity, yet unlike the desultory haphazardness that irregularity is regular. I have captured all this in the following proposition:

> If A is a random event and f_n is an observed relative frequency, there exists an associated number p such that, for any positive, ε and δ, there is an integer N such that
>
> $$1 > \text{Prob}[|f_n - p| < \varepsilon] > 1 - \delta \qquad (2)$$
>
> is true for any number of observations $n > N$.
>
> That p is the probability of A.

The double-barreled production function

In another paper at the Harvard Economic Project (March 22, 1949), I pointed our a serious epistemological discrepancy between the two production models in vogue at the time, Neumann's and Leontief's. Because the first takes into account only stocks, it hides what may have happened between the beginning and the end of the process. Because Leontief considers only flows, he does not allow one to know which of two processes is more efficient. Intrigued, I turned to the standard production function, the definition of which (very strange) has remained in the same vapid form in which Philip Wicksteed introduced it almost one hundred year ago (1894): *"The product being a function of the factors of production we have $P = f(a, b, c...)$"* He said nothing about the kind of "function," or about the nature of the "factors," and no economic luminaries have ever questioned that diction. Some just said that the representation of a process involves only quantities:

$$Q = F(X, Y, Z,...); \qquad (3)$$

others, that it involves only rates of flow per unit of time:

$$q = f(x, y, z,...). \qquad (4)$$

Yet no one seems to have been bothered by this double-barreled view, not even Ragnar Frisch, who used both definitions on the same page (*Theory of Production,* p. 43). Using the elementary *identities* $X = tx$, $Y = ty$, $Z = tz$, ... for any t, I proved that if both (3) and (4) are equivalent representations of a process, the two functions must be identical $F \equiv f$, and, moreover, homogeneous of the first degree. Hence absolutely *all production processes are indifferent to scale!* I presented this astounding result in a formal paper at a 1965 conference of the International Economic Association. According to their rules, only the contre-rapporteur should present the highlights of the author's paper. My contre-rapporteur, Don Patinkin, stated that he could not introduce a paper vitiated by a "fundamental mathematical error" and simply sat down.[5] This high-handed attitude of an asservative economist was an emphatic proof of how incredible my theorem could then be judged by standard economists. Did not Joan Robinson claim with her usual feeling for what economics should be that standard theory of production is an economic miseducation?

Analytical process: flows and funds

"Process" is one of the most frequently used, and also the most abused, term in science. We find no definition of it even in Alfred North White-head's famous opus *Process and Reality.* Its detailed description runs against several paradoxical tangles that are circumvented by a jump in the dark, from dialectical to arithmomorphic (or analytical). What is involves epistemologically is, first that an analytical process is *identified* by a vacuous boundary of double nature: a spatial boundary and a temporal boundary that must not begin or end at infinity. Second, even though inside the boundary things happened in every location - too many to be listed in analysis what a process does is described only by what crosses the boundary. The definitions "input" equals what you put in, and "output" equal what is put out can now be made analytical: input is what crosses the spatial boundary from *outside*; output is what crosses is from *inside*.

5 While writing on this issue for my *Analytical Economics, I* said in passing that a new Aristotle might set dialectical reasoning on as solid a basis as the traditional logic. By a strange coincidence, L A. Zadek ("Fuzzy Sets" *Information and Control* 3 [1965], 338-53) had just claimed to have achieved this. But the claim, endorsed by legions, is spurious. The entire construction, beginning with the membership function $f_1(x)$, is purely mathematical; hence, it has nothing to do with dialectical concepts.

There are only three distinct and exhaustive cases: (1) factors that go in and never come out, (2) factors that come out although they have never gone in, and (3) factors that come out unchanged just as they have gone in. There is also a fourth logical category - factors that do not cross the boundary at all. They are *internal* flows illustrated, say, by "payments of business to business" which smuggles a dialectical concept into analysis. This confusion, also committed by Karl Marx, necessarily ends in errors if nor in a paradox.[6]

The three Production factors just described correspond to the classical Ricardian land, capital equipment and labor power. According to my epistemological view, they are the *agents* that transform the inflows into the outflows. I have proposed to refer to them as *funds* conceived as agents of constant efficiency.[7] Clearly, excepting Ricardian land, everything changes with time. For the purposes of analysis, however, capital may be assumed constant, as Karl Marx first proposed. A separate process the household, aims at maintaining the vital ability of people in order.

What a process does is then analytically represented by a set of functions of time t from $t = 0$, the beginning of the process, to t = T, the end of the process. Each such function represents the cumulative amount up to time t of a flow, entered or exited, or of *service* provided by a fund. This new way of representing a production process is a vector of functions, that is, a functional,

$$[R_i(t), I_i(t), P_i(t), W_i(t); L_i(t), K_i(t), H_i(t)]_0^T \qquad (5)$$

where the alphabetization indicates in sequence: natural resources, intermediate products, products, waste, and next, land, capital, and labor power. Each production process is thus represented not by a timeless vector in the commodity space as in standard theory, but by a *curve* in the same space. A critical difference that I thus introduced is the inclusion in (5) of natural resources and waste, inevitable but totally overlooked factors of any process.[8]

---

6 To be sure, shortly after the conference Dou[illegible]tinkin realized that he had been wrong (probably because he was not aware of the difference between *identify* and *equation*) and requested that his criticism not be published in the *Proceedings*.

7 When Leontief first presented his input-output system, he repeatedly insisted that all diagonal coefficients should be zero, which meant that the matrix should include no internal flow. However, in his later applications he did include them. To make the absurdity of the internal flow clearer, I devised a multiprocess matrix in which there is no empty box into which one could inscribe a coordinate for internal flow. That matrix also enables us to dispense with the helplessly intractable flow diagrams that overlay the recent ecological monographs (in which the frequent use of the "loop" should have exposed the limp concept of internal flow).

8 My concept of fund should not be confused with that of stock. The role of stock is to receive or to generate flows. And contrary to some opinions, the flow-fund model is essentially different from the flow-stock model encountered in the economic literature.

Standard theory makes intensive use of isoquants - *geometrical* curves - to represent the substitution of factors without changing the output. But since neither capital equipment nor laborers can be quantified as needed for the isoquant, if anybody uses an isoquant, one must inevitably admit that one's own framework is essentially dialectical. Some have indeed likened capital to clay or to putty. *I know of no other scientific discipline in which dialectics is as indispensable as in most sectors of mathematical economics.*

Patterns of production processes

My epistemological search has also led me to the new fact that production takes place in several and entirely different types of processes. The simplest type is that which not only is represented by (5) - all are - but can be represented only by it. It is the process of a single craftman's workshop, where at any time work is applied to only one unit (or one batch) of the product and units are produced *in series.* And if we tease out any other kind of process, we find that all consist of some arrangement of such simple processes, which I *called elementary* processes. For an agricultural product within a uniform climate, the elementary processes are arranged *in parallel.*

One point now deserves unparsimonious attention: in any elementary process virtually all funds are idle over certain periods, and this idleness cannot be completely eliminated by technology. Think of the plough in the temperate zone or of the saw of a cabinetmaker. If in the latter's shop an additional craftsman is brought in, the two could use the same tool alternately and thus decrease the idleness of each fund. The production would thus be speeded up many times more, which would require an equally increased intensity of the demand, a finding that analytically vindicates Adam Smith.

In the industrial sector we also find that production - of one spacecraft, for instance - is an elementary process. But that sector is dominated rather by the factory system, so common a view, yet so totally disregarded. Like money, that process is a purely economic invention, not a technological discovery. A theorem I proved states that any set of commensurable tasks that would constitute an elementary process can be arranged in a pattern that would eliminate all idleness, a commonplace illustration of which is any assembly line. This is the superiority of the factory system, which has still another economic advantage. Its production needs no waiting. If Bali Island (where uniform climate would permit it) agriculture used the factory system, it could be said that the Balinese eat the rice sown that very moment. This peculiar property is due to a specific capital item, goods in process or *process fund* (my preferred term).

Production by factories, though, needs waiting too: building the plant itself and priming it may rake years. Contrary to Piero Sraffa's celebrated thesis, production of commodities in general needs not only commodities as such, but factories too. And because there is no factory to produce factories, some waiting is irreducible in the case of growth.

Curiously, the simplest analytical representation is that of the factory process for which the arguments of functional (4) are simple linear homogeneous functions of t. We may thus put in a generic form $A(T) = aT - A^o$ for every element of (5), the notation A^o standing for quantities and the lowercase a for rates with respect to time. For the representation by quantities, there is a hitch: one hundred pounds of nails says nothing about T, which might have had almost any value. For this reason, in passing from the functional to its degenerated form - the quantitative vector - we must include T in the new form

$$(R^o \ I^o \ Q^o \ W^o; L^o \ Q^o \ H^o: T). \qquad (6)$$

By contrast, the representation by a vector of rates need not explicitly contain the time coordinate:

$$(r, i, q, w; l, k, h). \qquad (7)$$

Formulas (6) and (7) dissolve immediately the paradox of the double-barreled production function. The epistemology of the propounder of production function (2) failed to realize that quantities are not time dimensional. Another common epistemological fumble is the indiscriminate use of "flow" in saying, with Marxians and the legion of energetists, that the sewing needle, not only the cloth, flows into the pants.

After examining the blueprints L, K, H of a factory, specialists could say (1) how large would be the output q and (2) what production flows would be necessary for it. The factory process must be portrayed by two similes:

$$q = \Phi(L, \ K, H) \quad \text{and} \quad q = \Psi(r, i, w). \qquad (8)$$

Factory production, therefore, is affected by a strict limitationality: it cannot produce more shirts by increasing only the sewing machines or only the input of fabric. A production function of only funds and flows, therefore, is total nonsense. Yet with the recent discovery of resource scarcity, numberless economists have used the function $q = F(H, r)$ for selling the newest economist's conjuring trick. If that formula were epistemologically valid, we could at whim substitute capital equipment for iron ore while increasing even the production. Another fictional function is $q = F(K, H; t)$, where t supposedly represents

technological progress. Several standard economists have used the partial derivative of F with respect with t, completely ignoring when they were trapped the fact that neither the function F nor the arguments K, H, are the same in 1980 as in 1960: $F_{1980}(K'^{80}, H'^{80}) - F_{1980}(K'^{60} H'^{60})$ is not a difference on which the derivative is based. This fumble proves that even in mathematics we cannot do without epistemology.

Growth versus development

Ever since John Maynard Keynes' *General Theory,* growth has been conceived as a purely monetary phenomenon sustained by the century's monetary witchery: if government spends more for itself, all people will grow economically. For the simplicity of the diagram with the 45° line, Keynes became the darling of economists and, before long, of the politicians, who could now rationalize moving mountains without increasing taxes.

In spite of my uneasiness in approaching monetary facts, my epistemological penchant found, nonetheless, some delight in the problem of growth because of Neumann's and Leontiefs models of production. The mathematics of even the generalized Leontief system are rather simple (and I thought to have them all included in my article "Some Properties of a Generalized Leontief System" presented at the memorable conference in Chicago [1949] by the Cowles Commission and reproduced in my *Analytical Economics,* Chap. 9). They tell us that LeontieFs system in which labor is the only primary fund needs only a definite amount of labor power, L^i, for the production of an additional unit of commodity C_i. Is this not a secret of growth? Reluctantly, I must disappoint the adherents of this viewpoint: the principle is true mathematically but not operationally.

A point I can hardly overemphasize is that *in the initial Leontief system, just like in Marx's labor theory, labor is necessary but not sufficient for production.* A pesky question therefore confronts would-be planners: given that coal is necessary to produce iron and iron is necessary, too, to produce coal, where does any growth plan based on Leontiefs system get the necessary amounts of these commodities? An authentic story pinpoints this antinomy: a cookbook advised cooks to prepare stock A with some of stock B and the latter with some of the former. Even the king's cooks could not prepare anything.

It is very simple to calculate the matrix in which all net outputs are increased, yet how to pass from the initial to the last matrix is one o the most stubborn economic problems. Even Karl Marx, who considered both a stationary and an expanding stare, never showed how to pass from the former to the latter.

Of course, instantaneous growth can be achieved at will if we increase the working hours of the shift. But this trivial solution is inapplicable in Leontief's system, which ignores that coordinate. And it would also be unacceptable to the temper of this era, which refuses to recognize the primary truth upon which I have repeatedly insisted: the economic progress of the West was fostered by a very long working day.

On paper, we may start by saving some of one commodity and, by ramification, stage by stage reach a higher net income for all commodities. In Leontief's dynamic system even growth by stages is beset by several snags. In that system, because growth also requires increased funds, an awful snag is that either bulldozers are consumption commodities or yogurt is a producer good. The morale cling to the idea that all facts involve irreducible structure.

Another strong epistemological uneasiness of mine concerns the use of differential equations to show how growth can be implanted in a poorly growing economy by fitting it to a growing differential system. Of course, this is the acme of self-deception.

For a final word, we should not delude ourselves that mere accretion poses no intricate epistemological issues. Remember that biologists had long been tormented by the problem of how the accretion of simple cells occurs. Only the double helix, by being its own negative template, cleared the mystery. Yet notwithstanding the claims of eminent biologists, development has still not been explained. My epistemology prompted me to dissent: the DNA of a zygote warrants only its identical self-reproduction, not its metamorphosis into, say, a nerve cell. How much more mysterious, then, should economic development be? I was taken to task for having said that the most valuable clues for that issue come only from economic history, but Professor Schumpeter saved my soul by avowing his own confidence in history in *History of Economic Analysis*.

Feasible recipes versus viable technologies

I came to realize the indissoluble dependence of the survival of humankind on scarce resources by the combined influence of two sources: Emile Borel's monograph on statistical mechanics (alias thermodynamics), which as a student of statistics I read in the 1920s, and the problems of an overpopulated agrarian economy, of which I became fully aware during my Romanian exile. I then became convinced that nothing can solve the problems of an exploding population except, as Maithus argued, the population itself. The thought that, even if the population stopped growing, its predicament would still remain came to me one day as I watched a big Romanian river running in its bed furiously and with a chocolate color. There goes, I said, our daily bread of tomorrow.

Homo sapiens, *the exosomatic animal*

As a witness of the political events during my Romanian exile, I realized that Romania could not remain neutral (as virtually everybody wished) in World War II From history I also learned that this was true for World War I. The only impediment was Romania's possession of great reserves of oil, which neither Germany nor Russia was willing to let the other control. Other animals, however, do with just what they have. The quintessence of this view was recently expressed by Andrew Rooney, who watching a chipmunk at work observed that it never stopped to go to the hardware store to buy a tool, as humans must do. In all activities of life, all animals (humans included) use their organs with which their somata have been endowed by birth - *the endosomatic* organs, a term coined by the unusually perceptive biologist Alfred Lotka. And it is through changes of the endosomatic organs that every animal becomes better (or even less) adapted to life. But this mode of evolving is extremely slow. The human species alone found a far speedier way. Even some ancestors of *Homo* began using and finally making detachable *exosomatic* organs: first, stone hammers to bit harder; recently, airplanes to fly higher and faster than any bird. This does not justify the popular definition of humans as the only toolmakers. But as Henri Bergson first observed, humans are the only animals to use tools to make tools or, as Schumpeter used to say, to make machines, to make machines, to make machines, on end. The exosomatic human can do things that could not be done before. But exosomatism, I should stress, is not an unadulterated blessing. It is the root of inequalities within the same society as well as among societies. Because the production of exosomatic organs has to be planned and supervised, human societies have been divided into those who work (the ricksha men) and the governing individually (the mandarins). And, not to forget, exosomatism has also made us thoroughly addicted to the exosomatic comfort - hence almost completely dependent on the finite mineral dowry of our abode.

Thermodynamics and economic scarcity

While men of science were still interested in celestial affairs and in explaining them by the laws of mechanics, Sadi Carnot, a young French officer, published in 1824 an immortal memoir on the efficiency of the steam engine, the seed f the science of thermodynamics. For these reasons I argued that Carnot was the first genuine econometrician and that thermodynamics is in essence a physics of economic value.

Thermodynamics has had an agitated history, and its theoretical structure is now beclouded by a swarm of mathematical fantasies. My own struggles with the vacillating literature have led me to a very simple and clear conception of it. One of its four laws, the

entropy law, as engendered endless controversies. Yet its content boils down to a commonplace known from the time when man was not yet *Homo sapiens.* It simply states, as Rudolph Clausius put it in 1856, that beat always goes *by itself* from the hotter to the colder body, never in reverse. Or as I put it in a more homely way, it is the hand touching a hot stove that is burned, not the stove. Since such transfer of heat cannot be prevented between bodies in contact, it follows chat everywhere in the universe bot things *continuously and irrevocably* become colder and cold things, hotter. Clausius cast this fact into "The entropy of the universe tends toward a maximum." But what exactly is entropy?

My view is that the entropy law, like thermodynamic laws, reflects basic limitations of all living creatures. Let me begin by noting that a formulation of the entropy law that denies the possibility of converting the energy from a single source of uniform temperature into work, although well established in the literature, is not true; a piston and cylinder, by absorbing heat from such a source, can do it, as happens during the first expanding phase of a Carnot cycle. Yet we cannot take advantage of this splendid "engine" because of our limitation *in space.* Even after a small course, the piston must be brought back to its initial position. For this we must spend the same amount of energy as the work gained when the piston moved forward. There remains no surplus work for us. The solution to the impasse is one of Carnot's keen propositions: bring the piston back by a colder route than the first and you will get some surplus work. All steam engines must therefore work in cycles between a hotter and a colder temperature. In general, we can obtain work only from a source that involves a difference of temperature, of electrical or chemical potential. Only such energy is *available* (useful) to us as humans, homogeneous energy is *unavailable* (useless) to us. These fundamental thermodynamic concepts are clearly anthropomorphic. They justify my earlier contention that thermodynamics is a physics of economic value.

Any direct conduction of heat from hot to cold therefore robs us of available energy. Another robber is friction. However, friction does not produce heat if the motion is infinitesimally slow, in which case any movement would take a virtually infinite time. That possibility is off bounds for us, because we are limited *in time* as well.[9]

Those who have an unrestrained confidence in the power of science to fix anything keep preaching that science would help us get rid of even the entropic degradation. As a simile for the entropic degradation, I once used an hourglass assumed not ever to be turned upside down. Paul Samuelson followed with the remark *(Economics,* 11th ed.) that "science can temporarily turn the glass over." Yet I would not advise anybody to settle in an aentropic

9 These results were presented twenty-five years ago when pollution had not yet hit us in the face, nor had the embargo of 1973 made us aware of nature's niggardliness.

world (if one existed); for if one took a bath there, one might have the neck scorched and the toes frostbitten by a redistribution of heat. Not would I enjoy living in a frictionless world where I could not write or walk in the direction I wanted.

I have further argued, and very strongly, that matter, too, is subject to entropic degradation, that available matter in the bulk (say, the rubber of automobile tires) degrades irrevocably into the unavailable form of the rubber particles dispersed by friction on the pavement. We delude ourselves if we trust the popular belief that matter, unlike energy, can be completely recycled. What we can recycle is only *available* matter that is in an unusable form: broken glass, old papers, worn-out motors, and the like. I have stated this as the impossibility of perpetual motion of the third kind, defined as a closed system that could exchange only energy and would perform work at a constant rate forever. I have referred to it as the Fourth Law of Thermodynamics. The reaction to it has been thin and also very strange, for some have simply asserted that the law has long been known, while others have objected that it is not true. The latter critics have committed the same error as Galileo in claiming that the air offers no friction to the flying arrow on the ground that its existence could not be detected by any instrument of that time.[10]

The root of economic scarcity, hence of economic value as well, lies in the entropic degradation of energy and of matter in bulk. A different kind of scarcity is represented by Ricardian land, which is scarce because it sets a limit to the daily carrying capacity of the earth. The scarcity of mineral resources sets no reasonable limit to how much of them we can use during one day, but it sets a more dreadful limit, a limit on the survival of the human species on this planet. This was my message of twenty years ago, which, though pessimistic, did not spring from a pessimistic *Weltanschauung,* but from known facts. Yet a tidal wave of writers, new and old, have sought public admiration by opposing an ultra-optimistic battle cry to my pessimistic message.

10 A few critics, typically, represented by Carlo Bianciardi, Paolo Degli Espinoza, and Enzo Tiezzi "Ma la materia há una storia." *SE Scienza Experienza* 4 (july 1986), 40-1. Argued that with a magnet it is possible to reassemble all iron filings into the original piece. But they did not specify the instrument that would guarantee that the iron particles dispersed during the proposed experiment, and *only these*, would be picked up by their proposed magnet. Naturally, I think my law is true, yet I would not opposed any scientific denial of it. As I told Ilya Prigogine during a symposium sponsored by the U.S. Department of Transportation, we must know whether my proposed perpetual motion is feasible or not; we have answered this question for other perpetual motions.

The promethean destiny of humankind

Confronted with the recent symptom of the scarcity of our environmental dowry, the 1973 oil embargo, economists in particular have reacted according to Disney's First Law, "Wishing will make it so," as William Miernyk aptly put it in hiss piercing contributions to the problem of the exhaustibility of fossil fuels. Economists have authoritatively advised us to go home and sleep tight in our beds assured that "come what may, we shall find a way" as we have done ever since the time of Tutankhamen. Robert M. Solow even declared in his Richard T. Ely Lecture that "the world can, in effect, get along without natural resources, so that ex-haustion is just an event, not a catastrophe." And legions have argued that "solar energy is here, we can use it now," as Denis Hayes, a very sound student of the problem, proclaimed in the *Washington Post* a few years ago. Standard economists have made a defensive circle around the dogma that the market knows best, that prices will take care of any economic turnabout. I have strongly dissented from this economic fantasy. Its advocates have completely ignored that we could not let the polluting driver pay: instead, we have enforced the use of the catalytic converter by law. Are not the whales on the way to extinction precisely because the price of their meat is right? The same also goes for the deforestation of all time, especially of that in Brazil now.

There now rages a fashion to fancy one or another alternative to the current technology. None (yes, none) is worth anything because none has taken account of the fundamental condition of a viable technology, to which I now turn.

The number of production recipes used by humans ever since they became exosomatic animals is so enormous that even a lifetime would probably not suffice to compile a complete list of them. However, surprising though it may seem, only three of that vast number have effectively pushed on our exosomatic progress. In chronological order they are husbandry, the mastery of fire, and the steam engine. I have called these recipes *Promethean* on remembering the old legend that Prometheus, a Titan, stole the fire from the gods and gave it to humans. With just the spark of a match we can set on fire a whole forest, nay, all forests. This property, although not as violent, characterizes the other two Promethean recipes. It is a commonplace that a seeded grain of corn will normally yield a surplus of a handful of grains.

The steam engine, however, needs further discussion along with the prodigious story of its invention. Helped by the Promethean fire, humans were able to keep warm, cook food, bake ceramics, and above all smelt metals. An era of vigorous technological progress thus began. But given human impatience, any recipe that increases our power over things is self-defeating. We would normally use it oftener and oftener so that its technology would spread at a fantastic speed. In this way, by the middle of the seventeenth century the technology

based on Promethean fire ran out of its fuel: wood. Deforestation was advancing so fast that even in Norway legal restrictions onto cutting trees had to be introduced. Coal had been known as another source of hear ever since the thirteenth century. But the energy of coal, *thought available,* was not profitably accessible. Below even a moderate depth, underground water floods any mine and must be drained off, which requires an appreciable amount of energy. At the time, this posed an insuperable problem. Operators of mines asked even Galileo for help. He advised them to use a pneumatic pump; nature, he explained, abhors vacuum. But after they reported back that, no matter how hard they pulled our the pump, the water would not rise above some ten meters, Galileo reflected that perhaps nature abhors a vacuum only up to that height. The situation was in all respects like that of today. Fate had it then, that Prometheus II - two mortals, Thomas Savery and Thomas Newcomen - saved the day by inventing the steam engine. This engine, too, is a Promethean recipe: with just a little coal under it we can drain the water completely from a mine and also bring out far more coal than that used by the engine, nay, enough to operate other mines as well. Yet the steam engine has its entropic limitations, too. What the energetist tyros seem to ignore is that absolutely no recipe can produce additional available energy or available matter. From what? I would ask. Let us assume that another earth would possess vast reserves of bituminous coal located 10'7 feet below the surface. Since it would take more than the energy of one pound of coal to mine one pound, no steam engine could then be Promethean for that coal. The object lesson of this parable for the self-styled energetists is that a recipe that works well in the laboratory (as many do) may not necessarily support a viable technology.

From what I have said in this section, it is obvious that no viable technology can exist unless it is supported by a Promethean recipe. A new Promethean recipe, not just any fancy contrivance, is what the present crisis needs (a point totally ignored by those who exalt, individually or within the ever-growing number of global associations, one's own alternative solution). The stern question now is: will Prometheus III come in time to save our souls by a new Promethean recipe? Some claim that two Promethean recipes are already at hand. One is indeed: the breeder that produces more fissionable fuel than it consumes - hence its uniquely alluring name. Its hitch is the genetic danger, of its waste. The second (alluded to earlier) is the *direct* harnessing of solar energy. With sanguine hopes but with accomplished technological knowledge, a serious attempt was made by Solarex, Inc., to construct a solar "breeder" The result of that experiment was categorical: the amount of solar energy captured by a number of silicon cells does not suffice to reproduce them all even if all the necessary materials are obtained gratis from elsewhere. Automobiles and airplanes have been propelled by solar cells. But as my epistemological obsession forces me to observe, the energy that produced the cells, the automobiles, and the planes came

from nonsolar sources. At this time, harnessed solar energy is, like electricity, a parasite of other energies.

Faced with the present hovering c·'sis, what could humankind do? In strictest logic the answer is: practice "conservation", which, nor to remain just a word é must be examined epistemologically. Two important factors emerge. Th first is the necessity of reducing consumption so as to slow down the depletion of our vital resources to the minimum compatible with a reasonable survival of our species. A prominent economist challenged me to set a number for the reasonable consumption rate, an inept challenge. Are there numbers *set* for the rates of taxation, hospitals, schools? Undoubtedly, we must adopt some austere program (nor to go back to nature, as some haw wrongly read into my writings). Besides renouncing all kinds of instruments for killing ourselves, we should also stop overheating, overcooling, overlighting, overspeeding, and so on. Most important, we should cure ourselves of the morbid craving for extravagant gadgetry, such as the contradictory golf cart and two-garage cars. I thin!: that we could stop following fashion, that disease of the human mind, as Abbot Fernando Galliani described it in 1750.

The austerity program should, of course, apply primarily to the lands of plenty, certainly not to economically wanting people, say, to Bangladesh. But nations with a growing overpopulation should make all efforts to stop growing in numbers. In a facile way we all speak of overpopulation without realizing that, if the United States were as densely populated as Bangladesh, its population would be just over 6 billion, the present population of the world! This is what overpopulation is.

Conservation would also allow more time for Prometheus III to emerge, and should he or she fail to come in useful time, we will be capable of sliding without social convulsions into a technology not identical with, but very much like, the old one based on wood. Unfortunately, two obstacles rise against this plan. First, no human would voluntarily give up luxuries or even conveniences to help some future generations have plowshares. The human species seems determined to have a short but extravagant existence. Second, conservation is not a program for a club, a town, or even a whole nation. It requires the participation of all in a world organization that would administer the use of the worldized (a word I coined after the manner of "socialized" and "nationalized") resources. But perhaps the human race will pass into extinction segregated economically. It cannot be ruled out that some of the last people should die in penthouses, the others in hovels. *Chi vivra verra.*

Bioeconomics and evolution

Economic life is a unique process that goes in historical time and in a disturbed environment.

Joseph A. Schumpeter

After learning of Alfred Lotka's idea that the role of our tools is analogous to our biological organs, I began thinking that a greater analogy exists between exosomatic and endosomatic organs. On that trail I saw that exosomatism was the fountainhead of the economic process. Since the exosomatic organs offer unique advantages to their users and also are detachable, they began being traded and being produced for trade. Production for trade ultimately led to large social organizations. This development brought down upon humans the irrevocable predicament alluded to earlier: the social conflict. Societies of other species do not know such a conflict. The periodic killing of drones by worker bees is a biological, natural action, not a civil war. The reason for the difference is the fact that the role of the individual in the latter societies is decided at birth. The ant doorkeeper, for instance, has a flat head with which it blocks the entrance of any foreigner to the gallery, and he would not like (as we can judge from experiments) to do anything else. But in our case would not a ricksha man, for example, prefer to be a mandarin? And would he not struggle to become almost one?

The exosomatic organs evolve just as the biological ones do, though much, much faster. And just like the latter, they may be deleterious to the species: enormous deer antlers and the automobile that "attains one hundred miles per hour before the cigarette lighter gets hot," as a topical advertisement praises it. Our incurable addiction to even futile exosomatic organs complicates further our existence with problems that belong to *bioeconomics*.

Bioeconomics reminds us of Alfred Marshall, who first envisioned the sisterhood of economics and biology. Although he repeatedly preached that biology, not dynamics, is the Mecca of the economist, he himself hardly practiced that teaching. The economist who developed a general framework to represent evolution everywhere, nor only in economics, was Professor Schumpeter. Let me explain this great contribution of his to science, for it still needs to be explained. Schumpeter's vision of development, as he termed it for the first time in opposition to accretionary growth, anticipated by some thirty years a salient idea thought up in 1940 by a prominent biologist, Richard Goldschmidt. Schumpeter's view was that economic evolution is constantly fostered by discontinuous innovations, the product of the continual inventing faculty of the human mind, whereas Godschmidt contended that biological evolution fares primarily through successful monsters. Inspired by Schumpeter, in my bioeconomics I assimilated the emergence of palpable endosomatic changes with his

chain of innovations. Both are essentially unpredictable, not even randomly regulated, a point that exposes the fantasized attempts, such as Trygve Haalvelmo's, to equate evolution with an arithmomorphic mechanism.

In a firmer way than the biologists, Schumpeter maintained that economic evolution is irreversible just as the biological one is. But several biologists who believed in the supremacy of mechanics argued against irreversibility by pointing out that the color of the drosophila's eye changes back and forth constantly, like a pendulum. In this opposition there lies Schumpeter's piercing idea, which he relegated to a footnote (p. 81). There, Schumpeter excluded from innovations small changes, reversible changes akin to drosophila's changes of eye color. It was at that point that Schumpeter was confronted (for the first time, I believe) with an issue of a dialectical nature: What change is small? And there, as on other occasions, his answer was that you and I know when a change is small, although neither of us is able to say exactly when. By taking this position, Schumpeter implicitly opposed the neo-Darwinism account of evolution; for as be splendidly put it, "Add successively as many coaches as you please, you will never get a railway engine thereby." The railway engine compared with the horse cab is a monster, but such a successful one, as Richard Goldschmidt might have put it thirty years later. Of course, in an ocean of Darwinists and neo-Darwinists, Goldschmidt's thesis could not be accepted. Yet very recently, Stephen Could, one of the most active minds in biology, has rehabilitated Goldschmidt's theory , adding, interestingly, that no explanation of evolution can dispense with dialectical reasoning. Economists, however, have failed to pay any attention to the greatness of the conception of evolution first thought up by one of them.

Against some current?

Don't be modest, you are not that great.

Golda Meir

The question brings up the relativity of motion; for one may feel one is moving against a current although one just stays put, as many men and women did because they could not do anything else to oppose the Nazi onslaught. And there is the symmetrical case in which one may feel movement against a current even though one is just moving in a placid milieu, in which one might hear the whisper "*Sh! vous reveillez Monsieur*", as happened to me on a few occasions.

To try to ascertain whether I have ever moved against an objective current is not smooth sailing, for it inevitably entangles me in what, in line with my epistemology, I prefer to call

the *sociology of scientists.* This term correctly describes the discipline now known as *sociology of science,* as Karl Mannheim called it first; for sociology necessarily refers to living individuals: humans, chimpanzees, bees, horses. It would be nonsensical to speak of the sociology of books or of differential calculus. As the unorthodox sociologist Florian Znanieki argued, we can speak only of the role of people in acquiring and spreading knowledge. It is by reorienting Mannheim's view that Robert K. Merton set the "sociology of science" on a better track. There is, in particular, Merton's magnificent studies of the Matthew effect, of the multiple discoveries, or of plagiarism, copies germane to what scientists do rather than to what science is.

We may not *all* be aware of the most striking illustration of the Matthew effect; "*E pur si muove"* is ordinarily attributed to Galileo, although those words were the last ones uttered by Giordano Bruno on the burning stake! To descend to common people, my theorem of substitutability of Leontief's static system is usually not connected with my name but with Samuelson's, although Samuelson himself has always acknowledged my priority. (Maybe, Samuelson can be modest.) In the economic literature we also encounter a veiled plagiarism when an author lists only very recent works, two or three years old, avoiding any reference to Adam Smith, Karl Marx, Vilfredo Pareto, john R. Hicks, or others just as great. The aim is to place oneself within the tidal wave of pseudo-innovators. Newton thought this practice to be an academic crime of which he accused Galileo for failing to mention Kepler. When I receive one elegant flier after another about future large congresses organized by energetists who have never referred to my contributions, I always ask, "Why do they send these fliers to me?"

I should list now some of my strange ideas that have a connection with the question of my running against a current. For a start, I thoroughly deny that money is an economic factotum. By itself, it creates impediments for the customary international aid consisting only of money. More often than not, such aid has filled the pockets of the privileged with still more money and has developed the industry of luxury goods instead of much needed wage goods. This wrong is aggravated by the fact that the wanting people usually are toilers of the soil, using either inadequate methods or inappropriate tools. We could train industrial workers by bringing them in successive groups to a huge teaching workshop, but we could not do the same with people occupied in husbandry. Northeast Brazil is the strongest case in point. With this idea in mind, after a 1965 meeting on subsistence farming I declared to a Honolulu newspaper that the best way to help the undeveloped countries was to send not gushers of money, not a peace corps, but a peace army. Would sending a peace army instead of one fully armed be an inept idea?

When the UN General Assembly met in Stockholm in 1972 to consider the problems of the environment, I participated in the meeting of the Dai-Dong Association, the sole

organization acknowledged by the UN. As Tom Artin tells in his *Earth Talk,* a delectable report about the general events of that occasion, I offered several motions that immediately upset the other members. One motion was that all natural resources should be worldized. My aim was to preclude increasing scarcity from accentuating the extant international inequalities and from eventually fomenting wars. In an interview with the *New York Times* (December 1979), I insisted that, if the use of resources is still to be at the whim of the market, missiles will fly for the possession of the last drop of oil. What recently took place in Kuwait was, fortunately, only a rehearsal, but a rehearsal in full dress. My second tabled motion was to abrogate all passports for international travel. It was another bioeconomic idea to aid the people of undeveloped countries by allowing them to move freely where there is a much greater opportunity for the use of their hands, instead of resorting to the conventional, but extremely difficult operation of bringing capital equipment into their native countries. These ideas certainly were utopian, but I would plead guilty and with pride to that incrimination. There is hardly any social or economic practice of which we are proud now that was not a distasteful, though fully sensible, utopia once. Yet I did not feel that by the foregoing thoughts I was running against a current; there was no current opposing me. I just made my interlocutors conscious of their latent opinions, which happened to oppose mine.

In my earliest contributions I even ran with the current, which was then to expand the legitimate use of mathematics in economics, a program in which I have never ceased to believe and for which my exemplar is Sir john Hicks. My opposition is to the abuses of mathematics, although they have not caused the greatest harm. The greatest harm could come from the prevalent orientation that allowed as a leading item in the *American Economic Review* a paper about rats (which compelled me to resign from the American Economic Association).

If I finally realized that 1 was running against one current or another, it was not from any crossing of intellectual swords with my fellow economists, who have systematically shunned such an encounter, but from their personal attitudes toward me. I was a darling of the mathematical economists as long as I kept contributing pieces on mathematical economics. Several things radically changed their mood, especially that of the econometricians.

First, there was my contention that marginal pricing is the worst policy for an agrarian overpopulated economy. Soon after returning to the United States, I informally presented that idea at an after-dinner chat at the University of Chicago. How well I remember that there were absolutely no questions at the end! Those good friends wanted to spare me the embarrassment of being exposed as a neoclassical ignoramus. My position in the profession

worsened irreparably when, owing to the grace of George B. Richardson, my agrarian paper appeared as a leading item in *Oxford Economic Papers* (1960), not only for having thus touched the sacrosanct neoclassical dogma, but especially for pointing out that the much lauded proof by Kenneth Arrow and Gerard Debreu of the existence of a solution of the Walrasian system was irrelevant in practice because it was based on a fantastic premise; that every individual already had an income sufficient for life. My disclosure was hardly mentioned by subsequent writers, *et pour cause.* Yet it must have succeeded *in sotto voce* to alert others to the danger of breaking intellectual bread with Georgescu-Roegen. When quite recently I proposed collaboration on a significant agricultural project to a colleague, he turned me down explaining that he could not renege on his neoclassical testament.

Second were another series of irritating blunders. In *Analytical Economics* (1966) I stated that not all things can be made with the aid of numbers. And in a paper read at the meeting in honor of Corrado Gini (also in 1966), I dared to expose the ineptitude of predicting economic futures by econometric models. That was like signing my death sentence as a fellow of the Econometric Society (to which I had been elected in 1950 when election to fellowship was extremely selective). It was after expressing those articurrent ideas that I received identical treatment from two coeditors of *Econometrica,* E. Malinvaud and J. Dréze. Each sent me a paper critical of one of my articles. In their letters both stated categorically that they *had decided* to publish those papers and that I might, if I so wished, write a small reply (which I did). To my great surprise, both later sent me new versions with notes saying that, after seeing my reply, my critics had modified their initial versions. From Malinvaud I received even a third version together with a pronouncement that I had no proper right to a reply since my critic's paper was not aimed at my own work. After I pointed out that even in that relatively small third version my name appeared not less than twenty-two times, the strange tug of war had to end with the publication of my last reply, but, probably a unique case in the scientific literature, with an additional *replique* by that critic (1963). I am completely correct, I think, in believing that those two coeditors decided to publish the first critical versions because they thought that (without much care) they represented irrefutable blows to my scholarly reputation. But the greatest message of ostracization on the part of my fellow econometricians came on the occasion of my Richard T. Ely Lecture entitled the "Theory of Production" (1969). The Fellows of the Econometric Society scheduled their annual meeting at exactly the same hour as my feature, a machination that I dissected as a prelude to the lecture. This is just one symptom of the modern sociology of scientists.

Third, my idea that has irritate nor only the immense new crop of energetists, but especially most of the economists, was made known at a Distinguished Lecture at the

University of Alabama (1970). It was then that I raised my voice against the neoclassical dogmatic belief that the free mechanism of prices is the only way to ensure rational distribution of resources among, all generations. One pillar of that belief was (and still it) that the interests of future generations are taken care of by the fact that we care for our children, our children for their children, and so forth and so on. Our economic interests have been taken care of (so it seems) by this algorithmic sequence from the time of, say, Julius Caesar - nay, much earlier. Yet none of those propounds thought of asking whether the relation "take care of" is transitive.

I firmly believe in the philosophical idea chat our understanding in any domain (including, yes, mathematics) needs both dialectical and arithmomorphic concepts. I cannot even get near the irascible reductionism - everything can be reduced to numbers - that especially dominates the thought of this century. Naturally, I cannot see in a computer anything other than a device to *calculate with numbers* (please, mark those words well) much, much quicker than our brain. About the time I was writing *The Entropy Law and the Economics Process,* a big din was being made about a computer that calculated 1 million decimals of π in eight hours.

As I was writing the present essay, another computer printed out 1 billion decimals! Besides greater speed, nothing has fundamentally changed. In both cases, I believe, the computers used Leibniz's infinite series for p/4. And as I said in my volume, if Leibniz had had to calculate by paper and pencil just 1 million decimals, it would have taken him thirty thousand years. How much ink, how much paper, how many quills? Now I wonder whether even the presupposed life of the universe would have sufficed Leibniz for calculating 1 billion decimals. But I am certain that the discovery of any new important theorem, Gödel's, for example, will remain the appanage of the human brain.

Today, "artificial intelligence" is a name so dressed up as to make us easy believers in the fantasy. In my 1971 volume, in considering the claim of that marvelous brain of A. M. Turing, that one day we will no longer be able to determine whether an interlocutor hidden by a screen is a human or a computer, with the proper apology 1 said that reading Turing's paper convinced me that it may have been written by a computer, that Turing only signed it. I recently sent the same punch to the editor *of Scientific American* in connection with an overenthusiastic article by a staff member. They naturally did not publish it: apparently, the press is free but only for those who own it.

I also contend that the impossibility of relating every function of the brain to some digital or chemical phenomenon is salient proof that we cannot do everything with numbers. The extraordinary experiment by the famous brain surgeon W Penfield pinpoints the mystery.

When Penfield told a patient under brain surgery not to raise his arm if Penfield touched his brain with m electrode, the patient just used the other arm to keep the impulsed one down. Surprised, Penfield then asked what electrode caused the second arm to move. We still wait for a nonfantasized answer.

My epistemological addiction is the reason I am against arithmomorphia. I have only words of protest for the typical assertion of a physicist that it is not necessary to explain phenomena before dealing with them mathematically. If one starts only with mathematics, one is likely, as I said, to be trapped inside it. A superb illustration is the theorem of some mathematical economists that the market tends to an equilibrium even if the traders are more numerous than the continuum power. Being trapped, they could not even dream of asking what actual space could have room for so many actual traders.

This has been the story about my claim that I have indeed run against a current, why and how. Other scholars and philosophers have also run against a current. To my knowledge they are Isaiah Berlin, Paul Feyerabend, and Gunnar Myrdal. By comparing their conditions with mine, after long years I have concluded that for the results of one's struggle the place from which one runs against a current matters enormously.

Relevant contributions

"Leontief's System in the Light of Recent Results" *Review of Economics and Statistics, 32* (August 1950): 214-22.

"Toward Partial Redirection of Econometrics: Discussion". *Review of Economics and Statistics, 34* (August 1952): 206-211.

"Mathematical Proofs of the Breakdown of Capitalism". *Econometrica, 28* (April 1960): 225-43.

"Measure, Quality, and Optimum Scale" *Sankhya,* Ser. A, 27 (March 1965): 39-64.

Analytical Economics: Issues and Problems. Cambridge, Mass.: Harvard University Press, 1966.

The Entropy Law and the Economic Process. Cambridge, Mass.: Harvard University Press, 1971.

Energy and Economic Myths: Institutional and Analytical Economic Essays. Elmsford, N.Y.. Pergamon Press, 1976.

"Methods in Economic Science". *Journal of Economic Issues* (June 1979): 317-27; (March 1981): 188-93.

"The Measure of Information: A Critique" In: J. Rose and C. Bileiu (eds.), *Proceedings of the Third International Congress of Cybernetics and Systems*. New York: Springer Verlag, n.d., 3: 187-217

"Feasible Recipes Venus Viable Technologies". *Atlantic Economic Journal* (October 1983): 21-31.

"Hermann Heinrich Gossen: His Life and Work in Historical Perspective" In: R. C. Blitz, tr., *The Laws of Human Relations and the Rules of Human Actions Derived Therefrom*. Cambridge, Mass.: MIT Press 1983, 11-140.

"An Epistemological Analysis of Statistics: The Science of Collective Description and of Rational Guessing" In: M. C Demetrescu and M. Iosifescu (eds.), *Studies in Probability and Related Topics*. Montreal: Nagard, 1983, 221-59.

"Man and Production". In: M. Baranzini and L Scazzieri (eds.), *Foundations of Economics*. Oxford: Blackwell Publisher, 1986, 247-80.

"Interplay between Institutional and Material Factors". In: J. A. Kregel, Egon Matzner, and Alessandro Roncaglia (eds.), *Barriers to Full Employments*. London: Macmillan Press, 1988, 297-326.

"Closing Remarks: About Economic Growth A Variation on a Theme by David Hilbert". *Economic Development and Cultural Change*, 36, Suppl. 3 (April 1988): S291-S307