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Author(s): Axel Leijonhufvud
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Adaptive Behavior, Market Processes and the Computable Approach

Axel Leijonhufvud *

Les économies sont des systèmes dynamiques adaptatifs complexes. Pourtant, l'analyse économique est rivée à une méthodologie qui est tout à fait différente de celle mise en œuvre dans d'autres domaines, tels que l'écologie, les sciences cognitives ou l'informatique, où les mêmes types de système sont étudiés. L'analyse économique « calculatoire » tente de réorienter l'économie de telle manière à rendre faisable une interaction plus profitable avec ces autres domaines scientifiques.

Nombre de questions commodément ignorées dans la théorie standard peuvent être prises en considération au moyen de simulations numériques. On peut modéliser des processus économiques plutôt que simplement leur terme présumé, par exemple, et être explicite à propos des structures en réseau des interactions de marché. La modélisation des processus d'échange fait apparaître nombre de questions en relation avec le traitement du temps en théorie économique. Une question particulière, celui de télescopage de la perspective temporelle dans les situations d'inflation élevée, est discutée en détail. L'explication précédemment proposée par M. Allais est considérée et une explication alternative est suggérée.

Classification *JEL* : D4, E

Economies, obviously, are evolving, complex, adaptive dynamic systems. Yet to people who come from ecology or brain research or computer science or other fields in which one deals with evolving complex dynamic systems, mainstream economics seems committed to an alien and unpromising methodology. Computable Economics attempts to reorient economics in a direction that will bring it closer to these other fields in basic outlook and will enable us to learn from the exciting new methods of investigation that have been developed in these areas.

In this paper I intend first to discuss briefly the obvious question : What is it that keeps standard economics from adopting the perspective of complex dynamic systems theory ? Next, I will consider some examples of hitherto neglected modelling issues that we will be forced to tackle in developing computable dynamic models. Lastly, I will discuss some problems drawn from my work

* University of California Los Angeles (UCLA) – Department of Economics, Center for Computable Economics 405, Hilgard Avenue, Los Angeles, California 90024-1477 – États-Unis.

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with Daniel Heymann on high inflations as examples of the kind of economic questions where the computable approach may be particularly helpful in furthering our understanding.

THE AMBIVALENCE OF ECONOMICS

In his *Human Use of Human Beings*, Norbert Wiener described an experiment by W. Ross Ashby in which Ashby had built a number of primitive robots that ran around on the floor responding in simple ways to signals from each other. The flock or school of robots would tend to settle down into stable, stationary patterns that, of course, differed depending upon the number of robots and initial conditions and so forth. These patterns could not be deduced from the properties of the robots. A equilibrium pattern was what we would now call an "emergent property" of the system of robots.

Wiener called Ashby's demonstration "one of the great philosophical contributions of the present day" (Wiener [1956] p. 38) It must be about 35 years since I read this passage. The reader may surmise the reason for my still remembering the gist of it, namely, the emotional reaction of an apprentice economist : "20th century philosophical idea ! Has he never heard of Adam Smith's Invisible Hand ?"

The founding father of modern economics obviously had a perfectly clear grasp of the idea of self-regulating order *emerging* in a complex system from the interactions of numerous simpler components without this "being part of their intentions." So this theme has been in economics for more than 200 years. Indeed, one might make the argument that it is the introduction of it that made economics into a scholarly discipline (and eventually one with scientific pretensions !).

The Invisible Hand is not just an old cliché, suffering from too much repetition. Economists generally are good at explaining the decentralization of decision making in market systems. So we understand about *distributed processing* in complex systems. And we know why central planning doesn't work which is to say we understand the limitations of *top down* control.

Distributive processing, emergent properties, bottom-up organization are passwords among people working on complex systems. And one could elaborate a good deal further on the generalizable insights into complex systems that mainstream economics incorporates. But, if that is so, why do scientists working on other types of complex systems find economics so uncongenial and why are economists, as a rule, so averse to learning from these other fields where the study of complex dynamic systems has advanced so very rapidly in recent years ?

A simple — and perhaps simplistic — answer is this : There is one key principle of complex systems that a great many economists do not understand and that the discipline as a whole does not respect. Let me call it the Principle of the Hierarchical Build-up of Complexity. Less pretentiously : Complex systems are built from simple components.

Of course, we have had economists who grasped the principle perfectly and did so at an early stage. One of the wisest men ever to grace the profession, Kenneth Boulding, wrote a small but very exciting book, *The Image*, back in the mid-fifties, in which this principle was the very core of the entire argument. So, give credit where credit is due. But the real point is the profession's reaction to Boulding. *The Image* was one of several works of his that made a lot of economists dismiss him with that ultimate put-down : "I guess he is no longer an economist."

What then makes economists blind to hierarchical complexity and what are (some of) the consequences ? The answer is that it conflicts with another principle to which economists have the strongest possible attachment, namely, the Rationality Postulate. Socializing students to become economists may begin from the innocuous proposition that the road to understanding people's behavior starts from the assumption that "they do the best they can", but it is not considered complete until the student is convinced that rationality can only mean that the individual understands *all* the objective consequences of his own behavior and, therefore, is able to optimize the objective outcome.

So, economists have developed a fantastic armory of optimizing methods. These methods are in fact very useful in dealing with a wide variety of problems. But optimization can be applied only if all the consequences of alternative actions can be reliably mapped out in advance.

It is this that so often makes the practical man of affairs impatient with economics. He may be forgetting all the tricks his operations research people are actually making good use of down in that Department but, to the CEO, economics seems to be not of much help because, to put it bluntly, *it assumes that people know what they are doing*. It deals with "Incredibly Clever People in Unbelievably Simple Situations"[1]. Business executives know in their bones that the important and difficult decisions are the ones that have to be made on the basis of very incomplete information.

The violation of Hierarchical Complexity lies in this assumption that the typical economic agent fully understands his environment, which is to say, fully understands the higher-order system of which he or she is part.[2] In such models, there can be no room for genuine surprise or for emergent phenomena and consequently no real need for market mechanisms or other forms of feedback governed adaptation.[3]

So this is what I mean by the ambivalence of economics : it is caught between conflicting principles and continues to vacillate between them. From a strong adherence to the Rationality Postulate it is but a short step to the Fatal Conceit and the time is not long past when that was precisely where much economic analysis all too often ended up. Meanwhile, for all the lip service to it, we still do not have adequate models of how the Invisible Hand actually operates.

1. Cf. Leijonhufvud [1993].

2. "If the brain were so simple that we could understand it, we wouldn't." Cf. Kevin Kelly [1994], p. xxx. Society (and the economy) is a system of interacting brains and thus a level higher in the hierarchy of complexity.

3. And, of course, no need for economists...

This is a task for which the computable approach would seem well suited. It might even help us visualize it !

COMPUTER SIMULATION

The nature of the economics discipline today is in large part due to the technical limitations under which it has labored ever since the its inception. Until very recently it simply was not possible to give analytical representation to large dynamical systems, at least not if they had more structure than a volume of gas in Brownian motion. Traditionally, therefore, economists have had to assume, as an article of faith, that their systems would converge rapidly to simple attractors, the properties of which they could analyze.

Advances in computer technology have now made it possible to study representations of dynamical systems that are far too complex for analytical methods. Economists have been slow to exploit the opportunities thus opened up. In order to do so, we will have to deal with some rather neglected aspects of our subject. An example from one of the fields which are far ahead of economics in exploiting computer modelling or simulation may be useful to illustrate what would have to be done in order to adapt the method to economics.

A dynamic computer simulation familiar to everyone is seen daily (in the US at least) on the TV weather report. In giving the forecast for tomorrow, the weatherman will routinely project the movements of the cloud cover forward for 24 hours or so. We have become used to seeing this but we know, of course, that ten or fifteen years ago this piece of animation could probably not have been done, at least not with the ease and at a cost that would make a market for it among local TV stations.

What does this kind of weather projection require ? Obviously, the initial conditions are in part given by a satellite photo which must not be many hours old. Constant satellite monitoring is one of the things that could not have been done some years ago. We may think of the satellite photo as divided into a grid of "pixels". As part of the initial conditions, one then needs to know the current temperature, barometer pressure, and wind conditions at a large number of locations. The theory input into the forecast will then consist in the equations specifying how one pixel will interact with its neighbours, given the temperature, barometer pressure, wind conditions, and heat from the sun prevailing at each. With today's powerful computers one can then compute the evolution over the next few days of this complex dynamic system.

What would we need to do to transfer this technology in a useful way to economics ? My reflections on that question come under three sub-headings. I will give disproportionate attention to the last of the three.

RETHINKING THE INFORMATION SYSTEM

If all one is doing is playing with the equations that specify the interactions among pixels, the result, obviously, is just another Nintendo game. For the TV projection of cloud cover movements to produce useful forecasts, a quite elaborate information system, quite different from what was available to meteorologists twenty years ago, comprising both satellite photography and a very large number of ground stations, is required.

Consider, then, the information system that macroeconomists use in trying to forecast business conditions. It was basically erected in the period from the 1930's into the 1950's, beginning with the creation of the national income and product accounts. Forty years ago, this economic information system of the government was way ahead of anything at the disposal of the private sector. Since then, however, it has hardly developed at all. Indeed, some economists fear that it has, if anything, deteriorated. Meanwhile, the business sector has learned to exploit the drastically reduced cost of recording, compiling and transmitting the quantitative information relevant to the decisions of firms. Economic information systems in the private sector are now in many instances way ahead of those run by the government.

Take GNP statistics as an example. GNP, to begin with, is a highly artifical variable, an attempt to condense a multidimensional welter of sample observations into a single scalar variable. It is produced after the event with a lag of several months and then with a sizeable error that everyone knows will have to be revised several months later still. The number publicized is of little or no direct value to anyone's decision in the private sector, although it may tell managers something about how widespread and general are the changes in demand that they have already experienced.

A macroeconomist friend of mine has spent the last several years as the close advisor to the prime minister of his country. It was a surprise, he told me, that in that job he made virtually no use of the government statistics that had been his stock-in-trade as an empirical macro-researcher for many years. By the time this information was ready, it was to old to be useful. Instead, he spent much time on the phone to friends and colleagues in business and in the financial markets finding out "what's happening."

Contrast this with (the American retail chain) WalMart, for instance. The moment that a tube of toothpaste passes through the check-out register in one of its stores, it is "known" (by a computer) at headquarters. It is not only information collection and transmission that is computerized in such a firm, of course. So are many routine decisions (on when to reorder from manufacturers to central storehouses, for instance).

If it were possible to get hold of "old" data from various firms of this sort, giving timing of sales, prices, inventories, orders, etc., some useful empirical work could be done. If econometricians could retrieve from such data the algorithms actually used by firms in reordering goods or revising prices, we would have probably the first pieces of genuine evidence on dynamic response functions in the economy. It wouldn't be very sophisticated stuff — these algorithms are apt to be pretty simple — but it would be more solid evidence than what we get from fitting more or less arbitrarily specified distributed lags. It would also provide the first pieces for a new kind of macromodel, one of higher dimensionality than traditional ones and utilizing "real time" disaggregated data.

The government of Singapore, apparently, is "wiring the whole economy" so as to have instant access to data on sales and production throughout the system. No doubt they are in this way collecting data that we, in the U.S., will regard as proprietary or private. But we should be able to move in the direction of a macroeconomic information system that could be the basis for better short-term forecasts (and do so without creating a 1984 nightmare).

THE TOPOLOGY OF ECONOMIC SYSTEMS

In my meteorological illustration, the relevant scientific laws deal with the interactions between neighbouring cubes of air. In the physical sciences, the appropriate topology of a dynamic system is most often simple and intuitive. We know what "local interaction" means.

The appropriate topology for the analysis of economic systems is not so self-evident. How should we define "local" in relation to an economic agent ? Nearness in a simple physical sense is not the point. We have to conceive of it rather in communication network terms.[1] For some goods, "nearness" between agents in the network may indeed be defined by physical location, but for others "local interactions" will take place between people on opposite sides of the globe. This problem is only now coming into focus for economists.[2] Until we know how to handle it, the usefulness of such dynamic models from the physical sciences as the Ising or simulated annealing model or various cellular automata models will be of doubtful value at best. The topology of the network of the human brain may be the more appropriate model for the economist to keep in mind.

The standard competitive model conceptualizes "the market" as a single information central to which all transactors are connected at all times. Not even the New York Stock Exchange is quite like that. Of course, all economists are well aware that most actual market structures are very different. But we are wont to assume that, *given the time* for information to flow and for adjustments to be made, all markets structures will behave *in equilibrium* as such a single information central. If a good is traded at more than one location, for instance, we usually assume perfect communication between locations so that price relatives are determined simply by transportation costs ; with those costs "impounded in the *ceteris paribus*", the Hicksian aggregation theorem applies and we are permitted to determine price as if in single market.

But "given the time" is not good enough for genuinely dynamic analysis which ought not to rely on convergence propositions of this kind. Market structure, in the sense of the topology of the network of agents, becomes important in the "short run."

1. De Vany and Walls [1995] deal with an industry in which the physical structure of a network is crucial. Their study of the natural gas industry traces the development of the U.S. gas pipeline network and shows how spot-markets proliferated and how aritrage possibilities increasingly constrained prices as the connectivity of the network increased.

2. Cf. the CEE working papers [10] and [17] by Masanao Aoki and [4] by John McCall.

TIME SCALES AND TIME COMPRESSION

Standard neoclassical models are usually developed as period-models. Individual decision-makers draw up dynamic programs that optimize criterion functions the arguments of which are consumption or production flows over the respective periods. Similarly, all transactors are assumed to act in lock-step and the thus synchronized markets are assumed to equilibrate in each period. But the periods are of unspecified and arbitrary calendar time length and this is thought not to matter. The optimization problems faced by transactors are taken to be formally the same, independently of the length of the unit period.[1] In recent years, it has become more or less standard practice to let the "period" go to zero and formulate intertemporal general equilibrium models in continuous time.

But not every transactor is present in the market at all times. Moreover, even in those cases where consumption and production can be validly represented as flows continuous in time, it is their time-integrals (stocks) that are transacted at discrete intervals. Hence the notion of market excess demand taking a particular value (such as zero) at a point in time is unclear at best. But realism for its own sake is not the point. The question, rather, is whether something of importance gets lost in the continuous time idealization.

If we consider a system in which the solution for $p_i(t)$ is "correct" for all i and all t, and everyone knows it, the exact timing of transactions or the number of people that happen to be "in" the market at any point in time need not bother us. Stochastically occurring "bunching" of transactions may occur, for example, but transactors on both sides of the market stand ready to substitute intertemporally so that prices are basically unaffected. So if the context is "clockwork dynamics", it does not matter. Conversely, it does matter in adaptive behavior models, where agents rely on feedback from current transaction outcomes to resolve endogenous uncertainty.[2] In current parlance, such behavior stems from "bounded rationality" — a constant theme of the computable approach.

Marshall's price theory differed from the neo-Walrasian tradition in being founded on simple postulates of adaptive behavior rather than on optimizing ex ante choice. His trick for doing dynamics with basically static tools was the postulated hierarchical ordering of speeds of adaptation : market day, short run, and long run. This schema, of course, will not allow the arbitrary "compression" of time periods nor, conversely, will intertemporal general equilibrium models accomodate the Marshallian schema.

1. Contrast the attitude to this problem of Sir John Hicks, whose main doubt about his own IS-LM apparatus was whether it made sense to think of the period appropriate to the construction of the IS-locus as of the same length as the one of the LM-locus. Cf. also his comparison of Keyne's and his own period-analysis : uKeynes's [period] was a 'short-period', a term with connotations derived from Marshall ; we shall not go far wrong if we think of it as a year. Mine was an 'ultra-short-period' ; I called it a week. *Much more can happen in a year that in a week* ; ..." etc. Cf. Hicks ([1982], p. 320, italics added) and Leijonhufvud [1986].

2. A. De Vany [1994] develops the problem of lack of synchronicity further and considers the institutional market arrangements that evolve to deal with it.

In postulating this particular ranking of adjustment speeds, Marshall may have been trying to capture what he thought to be an empirical feature of many 19th century markets. But it is also possible that he was mainly seeking a way of escape from nonlinear dynamics that mathematically could not be handled a 100 years ago. Within the short run of a Marshallian market two adaptive processes are at work : market price responds to excess demand and output to the difference between market price and supply price. The combination of two such differential equations is non-linear and likely, therefore, to produce complex dynamics in the two variables. Marshall's trick tames threatening chaos but does so in what is from an economic standpoint a not very satisfactory way.[1]

ADAPTIVE BEHAVIOR, MEMORY AND FORESIGHT

Analytical representations of behavior in economics range between the two extremes of, on the one hand, the infinite horizon (stochastic) optimization models that are the state of the art today and, on the other, simple feedback governed adaptive models with no particular "rational" foundation. Neither extreme is particularly reasonable. The dynamic behavior of infinite horizon models tends to be far too "smooth" and that of the no memory/no foresight adaptive ones tends to go chaotic far too easily. The real world fits somewhere in between.[2] Concessions to that fact have to be made from either end of the spectrum. When the problem of how agents might learn rational expectations is raised, adaptive behavior based on memory is brought in so as to move the intertemporal model away from the purely teleological extreme. When the simple backward-looking adaptive conduct of Marshallian (or Paretian) consumers or producers is "rationalized" as a gradient procedure for homing in on a utility- or profit-maximizing steady state, a teleological element creeps in. But such behavior must also have been learned (in a particular environment) so reliance on memory is implicit here as well.

Modern economic theory tends to frame the decision-problems of agents as exercises in deduction from known premises, slighting the difficulties of establishing by induction what those premises might be. Correspondingly, it has much more to say about expectations than about memory. But foresight can only be distilled from memory, and the reliability of forecasts depends on the informativeness of memory. Between the two extremes on our spectrum of models, we must pay attention to both.

1. Modern dynamical systems theory makes considerable use of the method. Masanao Aoki [1994a] reimports it into economics. In the natural sciences, such a hierarchical ranking of adjustment speeds may simply correspond to the hierarchical order of complexity in the system under study. The lower level components in an order (e.g. atom-molecule-cell-organism-...) must work qualitatively faster than the next higher level for the latter to function appropriately. Marshall's ranking of price – and output – adjustments, however, does not have a clear rationale of this sort.

2. But one should concede that, for the economies enjoying monetary stability that we usually study, the infinite horizon models provide the more useful approximation.

At this point, it may be useful to revert once more to our illustration of the use of the computable approach in meteorology and to ask : Have computers and the improved information systems enabled meteorologists to produce better weather forecasts ? The answer is that for the next 4 to 5 days, forecasts are considerably more dependable than before. Beyond 5 days or thereabouts, local forecasts remain undependable and may remain so forever. The system is highly nonlinear and depends sensitively, therefore, on initial conditions. Chaos theory suggests that we are unlikely ever to be able to determine initial conditions with sufficient accuracy to do much better.

There will no doubt be similar limits to what economic forecasting will ever be able to do. But we have some hopes that meteorologists cannot entertain. Reliance on "rationality" can be overdone in economic theory, but one should not deny the role of reason in human affairs, be it ever so modest. One does not make excessive claims for the cognitive capabilities of economic agents in noting that "reasonable" economic behavior tends to "linearize" the system.

The nonlinear interaction between Marshallian adaptive producers and consumers becomes a bit more interesting if we do not take for granted the simple market topology of centralized information processing. With an unorganized horde of adaptive buyers and sellers transacting the good we might, if information connectivity were low enough, have multiple subsets of traders "hog-cycling" out-of-phase with each other.[1] Price at each trading location would obey some complex dynamic and prices at different locations would not be highly correlated. An agent with optimizing ambitions (but limited means) would find this turbulent environment immensely complex and posing impossible information requirements.

But it is not a normal economic environment, of course, and in the absence of extreme monetary instability, it would not require superhuman cognitive capabilities on anyone's part to bring a simple order out of this complex mess. A relatively few middlemen, striving merely to buy low and sell high, in competition with each other, will eliminate spatial arbitrage profits ; in the process, a market demand function of some stability emerges and it becomes possible to form (fairly) rational expectations about tomorrow's price. The middlemen turn into market makers, posting the price at which they will sell and keeping inventories to buffer variations in sales and heavily dampen price fluctuations over time.[2]

So a modest amount of foresight on the part of a subset of market participants will suffice to make markets that would hog-cycle under short-sighted adaptive behavior behave far more 'smoothly'. This simplifies the environment for everybody and makes it possible also for the less well-informed and sophisticated market participants to plan ahead with some confidence. Simplicity and predictability results from the willingness of some market participants to arbitrage across space and time. In most markets, this will be associated with posted, relatively "sticky" prices. This predictable environment — this market order — will *emerge* from the mutual interactions of adaptive traders. It will do so under a fairly broad range of conditions. But under some conditions it can also unravel.

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1. This, as we will see, is not a bad picture of the fragmented goods markets of a high inflation economy !

2. Cf. Clower and Friedman [1986] for a worked out model of this kind.

HIGH INFLATIONS AND CONTEMPORARY MONETARY THEORY

Daniel Heymann and I have recently finished work on a book entitled *High Inflation*. We define low, moderate, high and hyper-inflation in behavioral terms. Imagine asking people in various countries at different times what the rate of inflation is. We define it as "low" if people have given it no thought, "moderate" if they answer in percent per year, "high" if they reply in percent per month, and "hyper" if they feel unable give a meaningful numerical answer. The telescoping of behavioral time built into these definitions is fundamental in understanding inflation.

Heymann and I argue that the high (and hyper-) inflations exhibit a number of phenomena that are not predicted by the monetary theory that has dominated theoretical attention from Patinkin to Lucas. In this dominant theoretical tradition, one "adds" money to a non-monetary general equilibrium by one or another of several well-known devices. Modern finance theory also proceeds in the same manner by first finding the "real" prices of assets in an entirely non-monetary context and then adding in money by hook or crook (or more seldom both) to obtain their money prices. Theories of this description have narrowly circumscribed the ways in which one imagines that money may be "non-neutral" and affect real behavior.

Among the phenomena exhibited by high inflations that appear unexpected from this theoretical perspective, the following seem the most notable :

1) Domestic money stays in use even at extremely high rates of inflation tax ;

2) The unit of account seems to matter — for accounting and for accountability. No good substitute emerges ;

3) Most intertemporal markets disappear — and many spot markets segment and fragment ;

4) The variability of relative prices increases dramatically.

These phenomena are not just to be taken as remediable omissions of the theory. Rather, we argue, they should be recognized as *anomalies* that seriously challenge our theoretical understanding of the role of money in the economy.

In the present context, we should focus on the last two of these anomalies. The disappearance of markets is a phenomenon that has been neglected in the theoretical literature while the increased variability of relative prices has been the subject of numerous papers. The explanations offered for this increased variability generally interpret it as reflecting the "stickiness" of a subset of nominal prices and for the most part attribute this stickiness in turn to one or another form of "menu-costs." Our interpretation differs ; we see it, in the main, as due to excessive volatility of the various nominal prices.[1]

Note that on the "stickiness" interpretation, increased relative price variability is a phenomenon entirely unrelated to the disappearance of intertemporal markets. Heymann and I, in contrast, see the two phenomena as two aspects of

1. It is not possible to summarize the reasons for our interpretation here. Cf. *High Inflation*, Chapters 5 and 7.

the same adaptation by agents to an environment of monetary instability. Our interpretation of the disappearance of markets and the increased variability of relative prices is that high inflations shift behavior away from the pre-calculated, intertemporal optimizing mode towards the reactive, feedback governed mode.[1] Inflation makes it risky to take positions on future money prices, so future markets thin out and disappear. The higher the inflation, the shorter the maturities and contracts that survive. As time perspectives shorten, the "linearization" of intertemporal price relations weakens or disappears. In the distribution channels for most ordinary goods, sellers are not in lock-step but price revisions occur out-of-synch : where inventory turnover is high cost-increases are pushed through to prices more rapidly. The dispersion of price changes increases as simple mark-up routines have to be abandoned in favor of attempts to anticipate future cost-increases. In markets for consumer goods, spatial arbitrage is also temporal ; in high inflation it becomes speculative and risky and middlemen do less of it. At the same time, comparison shopping by consumers becomes less effective in eliminating pricing discrepancies : the price information gained through consumer search depreciates very quickly. Spatial and intertemporal price relations for the same good, as well as relative prices of different goods, become more and more volatile the higher the inflation. In hyperinflation, the unwillingness to speculate on tomorrow's price will make some traders refrain from trading altogether.

THE TELESCOPING OF TIME

In the above brief account of some high inflation phenomena, the shortening of time horizons plays a crucial role. But why should the rate of inflation (or, more generally, the speed of events) have this effect ? The higher the rate of inflation, it is true, the greater the uncertainty about future price levels. But the disappearance of markets cannot be explained simply by reference to "risk." In standard theory, increased risk will not make an asset disappear from the efficient portfolio, and differences of opinion about future prices should bring higher, not lower, transactions volumes. The question of how we should understand this foreshortening of temporal perspectives does not have an easy, obvious answer.

The telescoping of behavioral time seems to apply equally to past and future. The time-span of useful forecasts shrinks with the time-span of usable memory. This apparent symmetry of memory and foresight also appears in a number of works by Maurice Allais, going back to the 1940's. As summarized by Friedman and Schwartz :[2]

> Allais argues that the rate at which people "forget" the past in judging the future – that is the span of past time on which they base their anticipations – is variable and depends on the course of events themselves. If the relevant magnitude changes rapidly – for

1. In standard monetary theory, of course, this sort of thing does not occur. The "rational" intertemporal optimizing mode always obtains. It certainly does not give way to mere monetary perturbations.

2. Friedman and Schwartz [1982], p. 358. I am obliged to Milton Friedman who in correspondence pointed out the relevance in this context of Allais' ideas.

example, if prices change rapidly – then people also adapt their anticipations more rapidly, "forgetting" the past at a faster rate or using a smaller time span to form their anticipations, and conversely.

Allais' rate of forgetfulness expresses the rate at which the weight of past observations "dies away" in their influence on present decisions. It is postulated to be a constant per unit of "psychological time." The scale of this psychological time will vary in relation to calendar time. The calendar time coefficient of forgetfulness is a weighted average of earlier rates of growth of total outlay with weighting coefficients declining exponentially with distance in time. The coefficient is higher the more the psychological time unit shrinks in relation to calendar time and the psychological time unit is postulated to shrink with the "rate of expansion", that is, with the rate of growth of nominal spending in the economy. Ignoring variations in real volume of transactions, we may associate it here simply with the recorded amount of inflation over a specified calendar time unit.

In Allais' "Restatement of the Quantity Theory" [1966] the demand for cash balances (in relation to income) depends inversely on the coefficient of forgetfulness. Consequently, velocity rises with experienced inflation. His forgetfulness concept is, of course, backward-looking but serves the same function in explaining the behavior of velocity in his theory as does the expected inflation rate (inflation tax) in standard theory.

Heymann's and my work bears a superficial resemblance to Allais' theory in two respects. First, and most obviously, our behavioral definitions of moderate, high and hyper inflations in effect express the shrinkage of psychological time with rising inflation rates that is central to Allais' work. Second, we repeatedly find ourselves asserting a symmetrical relationship between the "timespan of usable memory" and the "timespan of useful forecasts." Allais [1966] asserts a stronger, more precise symmetry, to wit, that people forget the past at the same rate as they discount the future. To this psychological law, we have no counterpart. Still, foresight is linked to memory in both approaches which makes Allais more congenial to us than he would be to economists working with the more purely teleological intertemporal general equilibrium models.

A tentative[1] characterization of how we differ from Allais may be essayed. Allais makes the psychological time unit shrink with the average "rate of expansion" per unit calendar time. (In the present context, we allow ourselves to equate this with the corresponding average rate of inflation.) We have thought of the telescoping of behavioral time as occuring with the increased *variability* of inflation over time *and* across markets. Of course, the variability of the aggregate inflation rate and the variability of relative prices are both highly correlated with the level of inflation. So, for some macroeconomic purposes, the distinction may make no operational difference.

Allais's aggregative approach is obviously perfectly adequate to his main objectives of explaining the behavior of velocity (Allais, [1966]) and the nominal interest rate (Allais [1972]). But it offers no obvious clues to help us understand the microeconomic consequences of high inflation : the disappearance of

1. "Tentative", because a clear delineation of our differences would require both a more extensive and thorough acquaintance with Allais's œuvre and a more rigorous formulation of this aspect of our own work.

intertemporal markets, the fragmentation of spot markets, and the excess variability of relative prices.[1] We believe that the telescoping of behavioral time occurs because of the difficulties of extracting dependable regularities from an increasingly *complex* environment.

Allais' rate of expansion (or average inflation rate) is not a complex magnitude but simply the first derivative of a function, measured by just a single number. A high number means that things "change rapidly", as Friedman and Schwartz express it, but it does not *per se* mean that decision-makers come to face more complex or difficult problems. If agents are able to extract that number, it seems natural to suppose that they would then behave as in the standard theory of anticipated inflation, that is, they would reduce their real balances due to the inflation tax, but would not find their ability to predict the future outcome of actions to be impaired. Consequently, they would see no reason to abandon intertemporal markets.

The difference between moderate and high inflation, that we need to capture, is similar to the distinction between laminar flow and turbulence, to use a hydrodynamic metaphor. Agents find themselves in a high inflation environment that is turbulent in two respects, namely, the non-linear evolution of particular prices and the lack of correlation between price movements in fragmented markets. It is when they find themselves in this kind of setting that old information becomes useless and the far future unpredictable.

By way of illustration, it may help to imagine a graph of a particular function over some calendar time interval. Suppose also that the agent has to predict its value at some future dates. How many data points would he need in order to "fit" the function well enough to make his predictions with tolerable accuracy ? In a (nearly) "linear" environment, extrapolation on the basis of a few observations might serve. Moreover, old observations are almost as informative as recent ones. In a setting where the "linearization" of the economy has evaporated, highly non-linear timepaths require far more and far more accurate information.[2] If such information is costly to acquire and/or to process, the "true" generating function of the data cannot be obtained. The agent becomes an economic weatherman, using only the most recent information to predict only the very near-term future.

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The last section explains some of the differences between a normal "laminar" economy enjoying monetary stability and the "turbulence" of a high inflation regime. It does not explain the nature of the phase transition from one to the

1. These high inflation phenomena are not touched upon in the works of Allais with which I am familiar.

2. Complexity, in the sense that we use it here, may be measured, following Jorma Rissanen, by the *"minimum description length" (MDL)* in binary digits needed to describe the curve to within some specified standard of approximation.

Complexity theory, as well as computability theory, will belong among the foundational subjects of computable economics.

other and it would take us too far afield to attempt to summarize our thoughts also on that subject. Suffice it to say that the computable approach offers the best prospects of improving our understanding of problems of this kind which involve both complex dynamics and complex systems at the same time.

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