1982

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Citation of this paper:

Howitt, Peter. "Information and Coordination: A Review Article." Department of Economics Research Reports, 8223. London, ON: Department of Economics, University of Western Ontario (1982).

20057

ISSN: 0318-725X
ISBN: 0-7714-0394-1

RESEARCH REPORT 8223
INFORMATION AND COORDINATION:
A REVIEW ARTICLE
by
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November 1982

Axel Leijonhufvud's On Keynesian Economics and the Economics of Keynes (1968), became famous for its argument that by the time the Keynesian Revolution ended up in the Neoclassical Synthesis it had lost track of most of Keynes's important ideas. The book outlined a theory that attributed large-scale unemployment to communication failures in the economic system, of the sort that would arise if you removed the auctioneer from general equilibrium theory. Leijonhufvud called this "The Economics of Keynes" in contrast to the "Keynesian Economics" of the Neoclassical Synthesis.

Since then, Leijonhufvud has written on a variety of topics, including inflation, the interest-rate mechanism, the stability of full-employment equilibrium, Say's Law, methodology, the history of monetary theory, and the sociology of economics. The present collection contains most of this subsequent writing, as well as four essays written about the time of the earlier book, which summarize parts of its argument.

Like the earlier book these essays reveal a broad vision of macro-economic theory, and the faint outlines of a research program embodying that vision, which cannot fail to excite anyone seriously interested in the development of the science. Leijonhufvud has a rare combination of common sense and concern for conceptual foundations. His ideas are rooted in the rich history of monetary economics, and enlightened by modern theory. The general reader will find here a masterly overview presented in an engaging non-mathematical style. The well-tooled theorist will find a gold-mine of ideas awaiting his refinement, along with a lot of wise advice about how to proceed.

This review will attempt to highlight some of the more salient features of this research program by briefly examining a number of macroeconomic issues in the light of "The Economics of Leijonhufvud". Only tangential reference will be made to the historical questions of Keynesian exegesis raised by the essays, a subject which was dealt with at length by reviewers of the earlier book (e.g. Grossman, 1972; Yeager, 1973; Jackman, 1974). Although the review takes exception with Leijonhufvud on occasion, its main purpose is to try to convey a sense of the importance of his writings.

1. Basics

What unifies the essays is primarily the concern that accounts for the title. According to Leijonhufvud the central issue of macroeconomics is how, under what circumstances, and to what extent the economic system coordinates the activities of different agents. The job of coordination requires, above all, a great deal of information, and Leijonhufvud argues that the logical way to address the question is to study the processes by which agents acquire, communicate, interpret, and respond to information in large complex economic systems. He motivates the need for a different approach to macroeconomics by arguing that both general equilibrium theory and the Neoclassical Synthesis, while capable of great subtlety and complexity in other dimensions, address this central issue in far too simple a fashion. One says that coordination (general market-clearing) as is achieved at no cost, through the notorious "tâtonnement" process. The other says never at any cost, (at least in the short run) because of the "spanner-in-the-works" of sticky wages and prices.

Leijonhufvud addresses this issue using the benchmark of full information; a hypothetical equilibrium state of full coordination, in which all exogenous

variables and parameters assume their actual values but "agents have managed to learn all that can be (profitably) learned about their environment and about each others' behavior" (136). He argues that serious departures from full information are usually the result of one group of agents forming beliefs that are inconsistent with those of other agents. With mutually inconsistent beliefs, their plans will be incompatible, and this he calls a state of disequilibrium (p. 140).

To understand any state of disequilibrium, Leijonhufvud sensibly recommends that we begin by asking what has gone wrong. That is, whose belief or action in the actual situation differs radically from what it would be in full information? To proceed from diagnosis to prognosis the key is to discover and analyze the homeostatic, or deviation-counteracting processes generating messages that will re-equilibrate beliefs, and the deviation-amplifying processes that tend to prolong or even worsen the communication failure. If the latter dominate, or if the market adjustments are likely to be slow, then:

"Faced with a diagnosis that pinpoints the "wrong" value of one variable as the source of the disequilibrium of the entire system, the natural impulse is to look around for some policy instrument that would impinge as directly as possible on this variable while, ideally, leaving alone those which are already "right". (p. 74)

He claims that by and large this is the key to understanding Keynes's policy prescriptions. It is also the key to understanding much of what Leijonhufvud has to say.

Another key is his use of the concept of liquidity. Not "the absence of risk", but "saleability at low transaction cost". The lines of communication in a market economy are largely governed by the pattern of transactions.

Transaction costs impose impediments not only to buying and selling goods but to communicating one's willingness to buy or sell. If, as Clower (1967) observed is the case in standard general equilibrium theory, all goods are assumed to be perfectly liquid, then the main impediments to communication have been removed, and it becomes a genuine puzzle how to account for such phenomena as involuntary unemployment, where people are obviously foregoing mutually advantageous opportunities to trade, without resorting to ad hoc rigidities. Such puzzles do not arise for Leijonhufvud, in whose theories the illiquidity of human capital and of most non-human capital plays a central role.

2. Is the Economic System Self-Adjusting?

This is the Big Question that Leijonhufvud wants us to face squarely. Needless to say he does not answer it, but his general approach is ideally suited for addressing the issue in as undogmatic a way as possible, and offers a characteristically sensible conjecture, backed with some tantalizing fragments of analysis.

His conjecture is that the economic system is probably like most other control systems that have either been intelligently designed or survived some test of time; it works well under a range of normal circumstances but there are limits to the size and nature of disturbance that it is capable of handling. The area inside these limits he conceives of as a "corridor" around the full information growth path.

His analysis is clearest with respect to a decline in aggregate demand. Within the corridor the decline is damped by wage and price adjustment. The

potential deviation-amplifying multiplier process is rendered inoperative by buffer stocks of liquid assets that insulate consumption demand from transitory income fluctuations.

But a large and prolonged decline may cause households' liquid assets to be hoarded by bond market speculators, or to dry up in a general monetary collapse. The multiplier process thereby gathers strength, interfering with the messages of the price system. In Clower's (1965) terminology, sellers, instead of receiving the notional demands of full information, receive the lower effective demands constrained by incomes in the absence of liquidity. Likewise the message that savers are willing to supply more loanable funds, which ought to be sent to investors in the form of lower interest rates, at first may be intercepted by bond-market speculators, and later will be snuffed out entirely by the fall in incomes. When such "effective demand failures" occur,

Price-incentives may be effective in all markets and all prices perfectly "flexible", and a market system may still go haywire in its groping for the co-ordinated solution. ...some prices may show no tendency to change although desires to sell and buy do not coincide in the respective markets... Prices may be at their "right" (general equilibrium) levels, but amounts transacted differ persistently from the desired rates of sale and purchase in some markets... Prices that were at their GE values may tend to move away from those values so that the information disseminated by price changes is "false" and makes the coordination failure confusion worse. (pp. 111-112)

Of course this does not constitute a demonstration of global asymptotic instability. What it suggests is instability in terms of some short-run notion, which seems appropriate in view of Keynes's dictum that the only asymptote is death. Elsewhere (1978) I have proposed several such definitions and shown how this kind of argument can be made more formally,¹ in terms of a much simpler model than Leijonhufvud has in mind, but much work obviously remains to be done.

3. Sticky Prices Versus Misperceptions

It is widely believed that the major analytical issue in macroeconomics today is whether aggregate demand causes output to fluctuate because sticky prices prevent markets from clearing, as in the analysis of Barro and Grossman (1971), and in the overlapping contracts approach of Phelps and Taylor (1977), or because the failure of sellers to recognize that the attendant changes in their selling prices are purely nominal, not real, fools them into supplying more, as in the island parable of Phelps (1970) or Lucas (1972), which assumes instantaneous market-clearing on each informationally isolated island. With his emphasis on quantity- rather than price-adjustment, the multiplier process, and removing the auctioneer, Leijonhufvud is often seen as a guiding spirit behind the former approach.

In fact, Leijonhufvud has no use for the "spanner-in-the-works" of price rigidities, which he thinks obscure the serious informational problems that arise even in their absence (see, esp. p. 111, fn. 14), and thinks the fix-price approach grossly understates the homeostatic capabilities of the system within the corridor. His story of how output fluctuates is quite explicitly an island story, with perhaps more emphasis than is currently fashionable on workers being fooled into and out of search rather than leisure (pp. 6, 199, fn. 109).

His analysis makes it clear that there is something seriously amiss with the common view on the importance of this issue. For in fact the two approaches from an operational point of view are almost indistinguishable. Both imply that the short-run effects of unanticipated demand changes will be reflected primarily in quantities with relatively little effect on price. Both imply that the effects on price and quantity of anticipated changes will be reversed. There is some implied disagreement about how long it takes for a change to become anticipated, but even Lucas (1975) assumes that some time is taken for new information to be reflected in expectations.

None of the popular island stories have multiplier processes, but that is primarily because they typically do not include permanent income as an argument of aggregate demand functions. If they did, and if the typical seller misread a nominal drop in aggregate demand as, with some probability, a permanent drop in his relative selling price, then a multiplier process could obviously result for as long as that misperception remained. The misperception would cause a drop in the typical household's estimated permanent income, which would cause a secondary decline in aggregate demand, which could not be fully anticipated by the typical seller, since it results from a shock that he himself is misinterpreting as specific to his own market. Of course these dynamics would be intertwined in a rational expectations model with some optimal adjustment of forecasts, which might be made to converge stochastically on the equilibrium, but only eventually.

The reason for this strong similarity between the two supposedly different approaches is that in fact neither has market clearing in the usual Walrasian sense of one centralized auction market for the entire economy. Nor has either thrown the auctioneer entirely away. One has him operating with a split personality in different localities simultaneously; the other has him working gradually (prices eventually adjust according to some kind of Phillips curve story) but unable to prevent trading at disequilibrium prices.

4. Persistent Unemployment

It must be admitted that the sticky-price story provides a more realistic description of the manner in which job separations occur in recessions. Quit rates definitely do not behave anti-cyclically, but layoffs do. But if that were the only difference it would not be much ground for

preferring one approach over the other, for it is surely not an important primary task of macroeconomic theory to account for these fine details concerning which side of the market makes proximate quantity decisions.

What is an important primary task is to explain not how unemployment arises, but why it persists. The misperceptions approach is widely recognized to be deficient because it can account for persistence only if workers persistently fail to perceive the increase in their real wages resulting from a falling (or decelerating) price level, if their preference for leisure increases, if their marginal products fall, or if firms misperceive their marginal products to have fallen. None of this bears the remotest resemblance to the experience of the 1930's or the 1980's.

But the explanation offered by the sticky price approach is hardly more credible. It asserts that the unemployed worker remains in that state because he does not offer to work for less than the going wage. Either he cannot, because unions have the market cornered, in obvious contradiction with the facts, or he will not, in which case his predicament is voluntary.

What is missing from both approaches is the cost of making effective offers to trade in the labor market. The unemployed worker may have an accurate perception of the going real wage for his services, yet be unable to find out just where the prospective employer is who is willing to pay it. Even once the appropriate contact has been made the two sides must be willing to pay all the other transaction costs; of bargaining, screening, documenting, relocating, and so forth. The labor market is a notoriously difficult one in which to do all this, because of the problems of heterogeneity, indivisibility, inalienability, and moral hazard. Surely it is this difficulty in effecting transactions, especially the difficulty of contacting potential trading partners,

rather than unrealistic wage demands of the unemployed, which accounts for the persistence of large-scale unemployment, as well as for its obviously involuntary character.

In other words, the essential characteristic of human capital that must obviously come into any explanation of persistent unemployment is the one stressed by Leijonhufvud, its illiquidity. "The price obtainable for the services of resource which has become "unemployed" will depend upon the costs expended in searching for the highest bidder. In this sense, the resource is "illiquid"." (p. 6).

This Leijonhufvudian perspective also reveals an important aspect of labor market contracts. They exist in part for the purpose of eliminating the transaction costs that would arise if hiring had to be done afresh every-day, and thus they serve to increase the liquidity of the human capital employed. But by the same token they greatly increase those costs, and reduce liquidity, for the unemployed. For part of most contracts, even the implicit ones, is an understanding that employers will not replace existing employees at the drop of a hat whenever someone shows up willing to work for less. These understandings also limit firms' willingness to undertake the observationally equivalent transaction of hiring a cheap new worker when previous employees are being laid off. They thus reduce the opportunities for the unemployed to make effective offers to work for less than the going wage, especially during a recession.

Now this might be argued to be precisely what fix-price theorists have in mind when they refer to contracts as keeping prices sticky and hence accounting for involuntary unemployment. (See, for example, Solow's [1980] lucid and penetrating account of the labor market.) But it's not in their theories. By the same argument it might be what Lucas has in mind with the ad hoc persistence terms in his aggregate supply function (1973), but it isn't there either. What's

called for is an explicit account of the limited opportunities available to transactors in all markets, not just those for labor services, to contact potential trading partners and to make effective proposals for exchange, of how those opportunities are affected by the presence of a lot of other people looking for the same potential partners, and how they are affected by the sales difficulties in other markets experienced by those potential partners during a recession.

5. Wage and Price Flexibility

The crucial role of liquidity in explaining persistence, and the absence of this role from standard theory, is seen most clearly if we contemplate a historically given situation of less than full employment. Most "Keynesian" models with a (possibly expectations-augmented) Phillips curve attached would imply that the faster the speed of adjustment of money wages the more rapid the return to full employment. Even macro-models where variables other than wages are adjusting over time with possible dynamic instability (for example, Tobin, 1975 or Howitt, 1978) will typically be rendered stable, with unemployment as heavily damped as you like, given a fast enough speed of wage adjustment. By implication, such models imply that the chief obstacle to full employment is the failure of wages to adjust rapidly enough.

Leijonhufvud derides this implication as a return to the pre-Keynesian idea that unemployment is always the result of workers asking too much for their services. He argues that the effects of money wage reductions cannot be studied independently of the diagnosis. If what's wrong (again, compared to the benchmark of full information) is something other than the reservation wages of workers, then wage deflation won't eliminate involuntary unemployment. "The willingness of labor to reduce the money wage will not help..the response of price to excess supply of labor does not bring about a meshing of quantities in that market" (p. 167).

Now Leijonhufvud is far from crystal clear on this question of wage deflation. The above quotation refers to a situation within the corridor (p. 183, n. 87) where what's wrong is the expectations of bond-market speculators who are overestimating the natural rate of interest. Their speculative demand for cash is what's causing unemployment, but he is not invoking either a liquidity trap or an interest inelasticity of investment demand, two of his pet hates. He argues that a "horrendous all-round deflation" (p. 193) that brought the Pigou-effect into play would leave the system "more disorganized than ever" (p. 194) if it failed to cure the basic underlying problem. But he does not provide in the text of the long essay in which this argument appears any explanation of why a less horrendous deflation might not restore full employment through the usual Keynes-effect.

An explanation can be put together using Leijonhufvud's theme of the illiquidity of human capital and his repeated reference to "disorganized markets". Once workers have become unemployed, finding re-employment requires them not only to lower their wage demands but to find new trading partners. New transaction costs must be paid, and partly in the form of extra search time. Reducing reservation wages below the value that would be appropriate for re-establishing equilibrium would permit all the displaced workers to regain employment more readily, but at jobs paying below their potential marginal value product in the best match. If everyone settled on the spot for the best available offer, then measured unemployment might be eliminated, but with professors selling apples and so forth involuntary unemployment would still persist, in the very real sense of people not finding employment at the wage "going" for their services. More generally, as long as labor has not been re-allocated so that society is on its intertemporal production frontier, involuntary unemployment by this criterion will persist. And getting there is a coordination problem that takes more than the willingness to work for less.

This explanation also highlights the importance of the "benchmark" diagnosis, and helps to account for Leijonhufvud's stress on the role of relative prices and their absence from IS-LM (e.g., p. 14; p. 72, n22; pp. 191-4). Suppose that what has gone wrong is the reservation wages of workers. During the period of unemployment some workers will have lost contact with their former employers, some false trades will have been made that require a rearrangement of job matches, and some employers will have been driven bankrupt by the downturn. But before much of this has happened a speedy reduction in reservation wages would permit the re-establishment of former trading relations, and thus a return to the neighborhood of full information. The faster the speed of wage adjustment the quicker the return. The more serious problems arise when someone's beliefs prevent a relative price from assuming its full-information value, in which case the effective demands for labor in different employments will not guide workers into their full-information jobs. It is easy to imagine how this confused state of demand for labor can greatly increase the difficulty of making effective contacts in the labor market.²

6. The Interest Rate Mechanism

According to Leijonhufvud, this mysterious return of Keynesians to the pre-Keynesian diagnosis of sticky wages has gone along with a neglect of the importance of the interest-rate mechanism. When large-scale unemployment arises, he argues that what's gone wrong is usually to be found not in labor markets but in capital markets. Early stages of a recession are usually characterized by a reduction in confidence, which increases the demand for liquidity to provide flexibility,³ hence raising long-term interest rates. The problem worrying Keynes in the early 1920s was the one referred to in the previous section: a drop in the natural rate of interest imperfectly perceived by bond-market speculators. Deep depressions he argues are characterized by a collapse of entrepreneurial sales forecasts underlying the marginal efficiency of capital, which as a result of the experience of a prolonged decline fail to reflect the sales opportunities that would prevail in full information.

All these diagnoses are different, all call for a different prescription, but none of them involve money wages being too high, and none of them are curable by greater wage flexibility. They all pertain to the interest-rate mechanism, not the wage-price nexus. Failure to recognize this, he argues, is what led Keynesians into the untenable position of maintaining the existence of a stable Phillips curve in response to the accelerationist challenge of Friedman. He argues that the proper Keynesian response should have been to assert that "unemployment will not converge to its natural rate unless the rate converges to its natural level--and that the latter condition will not always be fulfilled." (P. 135).

Leijonhufvud's insights are as usual penetrating and illuminating. But his description of "Keynesians" is not really fair. It is true that Solow (1969) and Tobin (1972) tried defending the stable Phillips curve, but it is hard to

believe that the paradigmatic "Synthesizer" Tobin, whose Nobel Prize derived from a lifetime of work on financial markets and their interaction with the real sector, is guilty of neglecting the interest mechanism. Indeed Tobin in another paper (1975) took exactly the line against Friedman's challenge that Leijonhufvud says he should have taken. He posited a model in which the real rate of interest in the short run is affected inversely by expected inflation. When the adjustment of expectations interacts with the expectations-augmented Phillips curve dynamic instability can result. He doesn't refer to the natural rate of interest but it can (naturally) be defined as the real rate when full employment saving equals intended investment. Thus his conclusion "...the adjustment mechanism of the economy may be too weak to eliminate persistent unemployment" (p. 202) is exactly Leijonhufvud's recommended response.⁴

Nor do I find very compelling his call for a return to loanable funds rather than liquidity preference so as to get the interest mechanism right. His arguments here are wide ranging, but one in particular stands out. It is that liquidity preference theory has the implication that the "paradox of thrift" will always arise when the natural rate declines, because a non-zero interest-elasticity of money demand forces the system through a time-consuming deflation during which incomes and hence saving fall. He argues that the only good reason for expecting such interest-elasticity is the speculative demand that arises when the rate of interest falls below that expected by bond-market speculators. But this will occur only if speculators do not perceive the fall in the natural rate, or if they get caught up in Keynes's beauty contest. Consistently with his "corridor" approach, he argues that speculators "normally" spend their forecasting efforts trying to guess the natural rate, not trying to guess each others' guesses. Thus a decline in the natural rate will normally cause no adjustment problems, as expectations will fall point for point with the natural rate.

The problem with this argument is that it seems so obviously to fit into an IS-LM framework, from which the loanable funds mechanism is absent, provide that you include a Hicksian (1937) non-linear LM curve that is vertical above the expected rate of interest and shifts vertically point for point when the expected rate changes, and provided that you "normally" suppose the expected rate changes whenever the natural rate does.

Others of Leijonhufvud's arguments for loanable funds strike me as simply wrong. The conclusion that "If saving and investment are always equal,...the interest rate [cannot] possibly serve to coordinate saving and investment decisions," (p. 135) follows only if there is no difference between "saving and investment" and "saving and investment decisions". Defining the natural rate of interest in IS-LM as the real rate in IS at full employment, assuming that all expectational variables assume their full-information values, may not be the most transparent way to do it, but it refutes the claim that "the denial of the [loanable funds] mechanism makes nonsense of the very notion of a 'natural rate' of interest." (p. 135).

7. What's Wrong With IS-LM?

The problems that Leijonhufvud has revealed in the IS-LM treatment of the interest-rate mechanism seem to me to call not for a move to loanable funds but for a more careful treatment of the expectational variables in IS-LM. Textbook treatments that sweep such variables as expected interest rates, entrepreneurial sales expectations and household permanent income expectations under the rug of constant terms are incapable of addressing Leijonhufvud's basic question of what's "wrong", and of seeing the appropriate connection between diagnosis and prescription. Even when explicit reference is made to these

variables, the assumption that they can be held constant while other exogenous variables are subjected to changes, builds in, as Leijonhufvud observes, a "communication failure" of dubious general validity. This is the message of his "paradox of thrift" argument, and it is essentially a generalized version of the famous "Lucas critique" (1976).

Actually, an IS-LM analysis with the Hicksian LM, and with a more careful treatment of expectational variables can be used to follow Leijonhufvud's "benchmark" approach to the questions of monetary versus fiscal policy. In the deep depression case where what's "wrong" is entrepreneurial expectations, his prescription is for a fiscal policy that will "believe the expectations that demand will continue unchanged at its depressed level and thus set in motion a process of upward revision of demand forecasts." (p. 74). In this case market rate will be below natural rate, because of the "wrong" entrepreneurial expectations underlying IS. If speculators are "right" they will be forecasting a rise in interest rates, and IS will cut the shallow range of LM. The relative inefficiency of monetary policy would follow from the usual textbook argument. But to this Leijonhufvud adds an important dynamic argument. Large open market purchases by the central bank would inevitably induce some speculators to "go along" and lower their expectations. But when the interest rate eventually rose to its natural level, they would suffer capital losses, as "punishment" for having gone along, and "this learning experience would make the Central Bank's task harder the next time a monetary policy à outrance is tried" (p. 35).

In the Keynes-early 20s case of a fall in the natural rate misperceived by speculators the usual IS-LM argument would imply a preference for fiscal over monetary policy, since the solution would be in the shallow range of LM with the market rate below the expected rate. But in this case the fiscal policy would have to be a permanent one, with possible long-term distortions

to saving and investment, and to the size of the public sector, because it would confirm the "wrong" expectations of the speculators.

Furthermore, although Leijonhufvud does not make explicit use of this argument, it is consistent with his analysis of the deep depression case that monetary policy would act directly on speculators' expectations through an "announcement effect". The role of the Central Bank as the largest and most prominent trader in bond markets would make it a natural "focal point" for expectations in case speculation was winning the day over enterprise, or even if enterprise was temporarily confused and in search of guidance. Provided, of course, that the Bank's credibility hadn't been squandered by having been used inappropriately, this announcement effect could make monetary policy much more potent than would be predicted by the usual IS-LM exercises. It would exert downward force on LM that would work even in a liquidity trap. This argument also shows an asymmetry between wage-deflation and monetary policy not captured by textbook treatments, because wage-deflation cannot be expected to have this announcement effect.

8. Inflation

One of the longer essays is on the institutional, legal, and social costs and consequences of changes in the value of money. He believes that we should treat money as an institution, for in all its aspects each person's choices about how much to use, hold, etc., depend crucially upon convention--upon our confidence that others will play by the same rules. "If we construct our explanation of the Social Contrivance of Money from the usual building blocks of the theory of individual choice alone, some aspects of the social institution will escape us in a way that can, embarrassingly, only be covered up by resort to brute tautologies--"Money is accepted because it is accepted," and so forth." (p. 220)

From this point of view he reveals at least three novel economic aspects of the effects of inflation. Because money is our conventional unit for quoting prices and striking contracts, and because there are discrete costs of renegotiating contracts and of changing prices, inflation will necessarily be a ragged process, with different prices rising by discrete jumps at different times, in a way that price-setters will find impossible to coordinate. It has been recognized in the literature (e.g., Sheshinski and Weiss, 1977) that this will cause the dispersion of relative prices to increase with inflation. Leijonhufvud stresses the related proposition that inflation will reduce the ability of the price system to convey important messages concerning resource allocation, as agents find it difficult to distinguish between permanent changes in relative prices that call for long-run adjustments and transitory changes that arise from the jerkiness of inflation. This is, of course, to some extent the message of Lucas (1972), but in the Lucas model the problem arises only if monetary policy is unpredictable; smooth, neutral, steady inflation causes no problems. For Leijonhufvud there is no such thing as a smooth, neutral, steady inflation.

The second novel aspect has to do with capital market distortions. It is commonly understood that these arise with inflation because of its unpredictability, which makes real interest rate calculations increasingly difficult. Again, it is commonly supposed that steady, perfectly foreseen inflation would cause no such problems, trivial Tobin-Mundell effects excepted. Leijonhufvud observes that perfectly foreseen inflation is no more possible than smooth, neutral inflation, because a commitment to stable money is the only possible anchor to people's expectations. Once the convention that "a dollar is a dollar" has gone:

"Observed inflation rates are not "drawn" from a probability distribution generated by a law-abiding mechanism. The appropriate metaphor for this case, I suggest, is that of playing "chess" in the presence of an official who has and uses the power arbitrarily to change the rules--i.e., a man who may interrupt at move 14 with the announcement: "From now on bishops move like rooks and vice versa... and I'll be back with more later." (p. 264)

In such a world he argues that people agreeing to a nominal rate of interest for debt contracts (another convention hard to account for with the logic of choice) cannot hope to be agreeing on a real rate, as efficiency would require.

The third aspect has to do with the demand for flexibility, which increases with inflation and its attendant uncertainty. Not only do people seek to avoid long-term contractual commitments, but, contrary to our normal presumption, they may substitute into nominal assets. For the institutions of a monetary economy make the portfolios with the most short-term nominal assets the most flexible ones. Inflation raises the expected cost of flexible positions but Leijonhufvud points out that it also raises the expected gain.

9. Rational Expectations

Leijonhufvud's approach shares a great deal with modern rational expectations theory in its focus upon signal extraction problems, its view of policy as a process rather than as a series of isolated actions, its implication that textbook Keynesianism deals inadequately with expectations, and its insistence that the self-regulating capacity of the economic system is grossly underestimated by textbook Keynesianism.

But there are two important differences, both of which are revealing in the light they shed on the two approaches. The first concerns the stability assumptions imbedded in the rational expectations approach. Lucas (1981) is not concerned with the process by which people learn about the stochastic relationships between what they can observe and what they wish to predict.

Instead, he proceeds as if it has somehow managed to converge upon a set of consistent beliefs. And not just any consistent beliefs. He makes no use of speculative bubbles, self-fulfilling prophecies, and Keynes's beauty contests, all of which are describable as consistent, equilibrium patterns (see, for example, Brock, 1974; Azariadis, 1981; and Townsend, 1981). Instead, learning is assumed to have converged upon fundamentalist beliefs according to which all that is expected to influence the behavior of a price at any date are the underlying factors of tastes and technology that influence supply and demand at that date independently of expectations.

Much of the interest of Leijonhufvud's work comes from his insightful remarks about the learning process and about the degree to which fundamentalists will prevail in the marketplace. His policy recommendations are generally aimed at aiding the learning process when it gets away from fundamentals, as in the case of the bear speculators who need to be kept in check by the Central Bank. This sort of thing will make little sense to the believer in stable fundamentalist expectations, for whom "what's wrong" is typically nothing that governments will learn about any faster than the market.

Like the fundamentalists, Leijonhufvud stresses the corrective losses that people with wildly wrong expectations will incur. "Normally", he argues that they give a presumption to stability. But here again there is a corridor effect. It's too much to suppose that the market weeds out even those with erroneous conjectures about circumstances that have never been experienced. When large and unusual displacements occur, much of our previous knowledge becomes obsolete, and learning has to start again. Whether or not it converges, and how rapidly, is an extremely difficult question to model, as recent attempts

(for example, Frydman and Phelps, 1983) have demonstrated. Unlike the Bayesian learning of econometric theory, which can usually be shown to converge upon "the truth", macroeconomic learning interacts with market adjustment in complicated ways. The relationships that one person is trying to learn about are affected by the attempts of others to do the same, in ways that often render them incapable of staying put for long enough to be observed. Although nothing much can be said in general, it is clear that unusual events can cause prolonged deviation-amplification, as people fumble about trying as best they can to cope with a changing environment.⁵

The other crucial difference between Leijonhufvud and Lucas is revealed in the latter's (1981) lucid account of the main methodological advantage of his research strategy; namely that deriving everything from the logic of choice is the best way to impose empirical discipline upon our investigations. Most of our accumulated stock of knowledge, both theoretical and empirical, concerns individual decision-making. If we model economic time series as if they were chosen by representative households and business firms, intermediated as smoothly as possible through perfectly competitive markets, we limit what we can take as exogenous (in the methodological not the econometric sense) to tastes, technology, and endowments, and ensure against making ad hoc departures away from what we know, and against using too many degrees of freedom in the form of unexplained "market adjustment" parameters.

Nothing could be further from Leijonhufvud's approach, which is to regard "market adjustment" as the principal phenomenon to be explained.

For Leijonhufvud the "logic of choice" is not to be ignored, but in many cases must take a back seat to the "rules of interaction". This is evident in his treatment of money as an institution, his focus on "liquidity" as an essential ingredient to communication in a monetary economy, the importance he assigns to the question of whether or not market processes will bring different agents' beliefs into harmony, and his insistence that real-world market institutions function very differently from the centralized Walrasian auction house.

What's at issue here is whether institutions fall into place according to the dictates of efficiency, or whether they place independent limits upon our ability to cooperate. No doubt there is an element of truth in both points of view, and there are reasons for thinking that in the long run efficiency prevails. But in the here and now we have no choice about the institutions with which history endows us. Our ability to cooperate in developing more efficient ones when circumstances change, is limited by their existence, and outmoded conventions are notoriously hard to break. How they evolve is not something we understand well. But recognition of our ignorance does not imply that we should reject as ad hoc any theory based upon institutional constraints. Our choice in macroeconomics is often the uncomfortable one between this ad hocery and the foolishness of assuming away obvious, and obviously important, facts of life.

I should add that Lucas and many others cannot really be convicted of such foolishness. Not only is Lucas well aware of the limitations of any policy

implications that follow from very special models, and of the empirical limitations of his theories; but in the class of models he uses to account for the business cycle, institutions do play a crucial role--specifically the institutional requirement that prevents agents from acquiring information on other "islands". Without this rule of interaction, or something like it, these models would have no way of accounting for the effects even of unperceived and unanticipated monetary disturbances.

Thus my point is not that these models ought to be abandoned for their lack of institutional content; indeed I think they are likely to constitute a valuable lasting contribution to our science. The point is that the attempt to purge them of such content to get down to the hard core of tastes and technology will push us in the wrong direction and will inevitably prove to be self-defeating. We can no more do without exogenously specified rules of interaction in macroeconomics than game theorists can do without exogenously specified "rules of the game".

10. The Research Program

The extent to which the economic mechanism is self-adjusting has for too long been either put aside as too loaded with ideology, or else given an implicit, and unbelievably simplistic answer in the initial set-up of models. It's the exciting question that first attracts students to macroeconomics, but our failure in dealing with it drives the best of them on to micro. The main attraction of Leijonhufvud's approach is that it promises a way to bring the question back into the realm of science.

It suggests a way to account not only for the successes of the system upon which Keynesian theory sheds so little light, but also the obvious failures, such as those of the 1930's, which modern classicists like Friedman and Meiselman (1963) and Lucas (1981) have had to recognize as "outliers" to their theories.

But the problems that will have to be overcome before this research program can begin to claim any concrete success are almost as impressive as its promise. If we cannot rely exclusively upon our stock of data concerning individual choice, then new institutional data must be gathered, concerning our rules of interaction. What forms of market organization predominate for what kinds of commodities? What are the implicit rules of contract in different markets? How are prices set? Whose inventories are typically used to buffer shocks in demand and supply? Which groups of agents tend naturally, because of their position in the organizational structure of exchange, to acquire specialized knowledge about what kinds of activities? What are the limits upon agents' abilities to contact potential trading partners and to make effective offers to buy and sell in various markets? By what rules do credit markets ration credit when uncertainty and moral hazard make an equilibrium based upon rational choice difficult to conceive of (see Hellwig, 1977)? Until we can agree on at least a set of stylized empirical answers to these questions our theories will have just the emptiness that Lucas' methodology warns us against.

Even with answers to these questions our theories are likely to be exceedingly difficult unless further simplifications can be found.

The research reported in Frydman and Phelps (1983) referred to above testifies to the complexity of market adjustment processes with even the simplest and tightest of structures imposed upon them. It may be that the only way to proceed at first will be to put the relentlessly critical "logic of choice" in the back seat until we can see how the "rules of interaction" operate on its own, with perhaps the occasional warning shout to keep it out of the ditch. My guess is that choice will be back in the driver's seat before long, but with more interesting company.⁶

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*The author is grateful to David Laidler and Tom Kompas for fruitful discussions.

¹Most of the ingredients of that argument were already presented by Tobin (1975, esp. p. 201) whose paper I return to in Section 6 below.

²This interpretation, if valid, answers the criticisms of Bliss (1975) who argued that Leijonhufvud's earlier book was in error on this point.

³On the connections between these concepts, see Jones and Ostroy (1983).

⁴Nor is it fair to identify Patinkin as the author of a rigid-wage static interpretation of Keynes's theory. Leijonhufvud does this on p. 49, and then goes on to motivate his own dynamic interpretation with the illustration of the "explosive cobweb" cycle (p. 62). This very same illustration was used by Patinkin, in the paper cited by Leijonhufvud (1948, p. 25), to motivate his interpretation, which stressed the dynamic adjustment problems that can arise when expectations and distribution effects interact with wage and price adjustment.

⁵For an example of how this might cause a cumulative decline in output when monetary policy switches from a long-expected inflationary regime to a cold-turkey zero-inflation regime when people aren't sure how firm the authority's resolve will be, see Howitt (1982).

⁶Peter Diamond has recently written two papers (1982a, 1982b) which seem to me to be exactly the kind of thing that this program calls for, and which offer hope that the program is indeed workable. In the context of extremely special models of costly search he shows how low-level equilibria can arise where production is depressed because the cost of search is high, because there are few trading partners available, because no one else is bothering to produce more; all with perfectly flexible prices and perfect foresight!