MACROECONOMIC PERSPECTIVES

by

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I cannot help citing here the Talmudic dictum that - in certain cases of private law where the formal consent of an individual is required - the court is permitted "to coerce him until he says 'I'm willing.'

§1 . INTRODUCTION

Money, Interest and Prices (henceforth *MIP*) has just been made available in a new, albeit truncated, version. We, relics of another age, applaud this. That it may sufffer the fate of all classics and be quoted but seldom read is, perhaps, inevitable.

It is, surely, no exaggeration to say that the MIP research agenda laid the groundwork for a crystallized macroeconomics. Macroeconomics (2) , as an independant discipline, was forged out of the older subdisciplines monetary theory, public finance, trade theory, capital theory and business cycle theory. The many fundamental attempts to integrate value theory with these subdisciplines mark the various stepping stones towards an independant macroeconomics. The Ricardian tradition, via Heckscher-Ohlin and then Samuelson attempted the integration of value theory and international trade theory ; the Wicksell-Lindahl tradition and the attempt to integrate value theory and public finance ; the Wicksellian tradition in capital theory ; and in this great lineage stands Patinkin's remarkable attempt to integrate value theory and monetary theory. The most recent of these attempts is, of course, the Lucasian endeavour to integrate value theory and business cycle theory.

Value theory is, quintessentially, equilibrium theory. Hence the Lucasian insistence on equilibrium business cycle theory and thus there is no scope for 'involuntary unemployment'. This is not an anomaly ; it is squarely in the tradition of the integration of value theory with the other subdisciplines : (Wicksell-) Lindahl equilibria in public finance and the Ricardian equivalences ; neutralities in monetary theory ; factor-price frontiers and steady-state dynamics in capital theory ; and 'natural rates' in equilibrium business cycle theories. The Talmudic dictum got it right first time around.

The celebrated chapter XIII of MIP, now a part of the folklore of the 'microfoundations of macroeconomics' revolution - together with the mystique of Clower-Leijonhufvud, Phelps-Friedman, and Stigler - Mc Call - was an attempt to cast the value theoretic net

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2) To the best of our knowledge the word "macroeconomics" was first used, in the published literature, by Lindahl in his famous "Studies in the Theory of Money and Capital" (cf. Lindahl, 1939, p. 52). However, Frisch had been using the term, in his lectures in Oslo, at least since 1931.

wider : to try to encapsulate unemployment. However, whereas the value theoretic integration of the other subdisciplines maintained equilibrium fundamentals (cf. above : Lindahl equilibria etc.) the incorporation of unemployment in monetary, capital, public finance and business cycle theories (in their respective integration with value theory) inevitably led to unemployment disequilibria - as Patinkin emphasazied in MIP. The Lucasians refuse to accept this for one reason ; Keynes for another.

The General Theory (GT) , customarily considered a contribution to a Monetary Theory of Production, especially since the publication of the famous 'Tilton Papers' as vol. XXIX of Keynes's *Collected Works* is, perhaps, more an attempt to provide the value theoretic underpinnings for a theory of unemployment equilibrium. Whether it is so or not, money, and employment remain the intractable core of macroeconomic analysis at the frontiers.

A quarter of a century since the appearance of the second edition of MIP on the eve of the expectational revolution which was to make the rational agent a 'signal processor' and not merely ([3]) the 'utility computor' of Patinkin's individual conceptual experiments has seen whole schools of macroeconomics bud, blossom and even wilt away ; some, still flourishing by other names, wholly inspired by the analytic structure and the central themes of MIP.

New 'theoretical technologies' have a way of helping the process of being sceptical about sacred cows. The new classicals would have us go 'beyond demand and supply curves in macroeconomics' (Sargent, 1982) and derive equilibrium configurations in terms of consistent 'decision rules'. Fundamental results from mathematical economics cast even more doubts on the usefulness of excess demand functions as the basis on which to attempt to bridge the micro-macro divide. Even deeper results from computability theory make empirical nonsense of demand functions whilst deep results in metamathematics call into question the standard formalism of preference theory. Coupled to these are the

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[3]) Anyone acquainted with the elements of computability theory will of course know that, fundamentally, they are one and the same thing : finite automata as, say, language acceptors.

fascinating developments in dynamical systems theory giving new insights into the dynamics of iterative processes with direct implications for tatônnement ([4]).

Few would quibble with the assertion that in MIP the real balance effect is *the* central lever that calls forth the forces of *supply and demand* to act, via *tatônnement* (and the constraint qualifications of *Walras's law* together with nonsatiation) on the *excess-demand function* to equilibrate departures from desired optima.

If the new 'theoretical technologies' force us to abandon these central constructs - in particular, the excess-demand functions - then must we also call into question the usefulness of the real balance effect. Surely that cannot be the case. The real balance effect and its counterpart, the intertemporal substitution effect, are fundamental *behavioural* constructs in a monetary economy. Perhaps, then, the new 'theoretical technologies' can encapsulate these behavioural constructs in empirically richer and more illuminating ways. Implicitly this is one of the issues we wish to discuss in this paper. In the next section some broad remarks on the agenda that was implicit for macroeconomics in MIP is interspersed with equally broad - almost kaleidoscopic and even obscure - doctrine historical remarks. There is no pretence that our sweeping remarks are based on the deep scholarship that is required for such an endeavour - especially in a volume to honour one of the modern masters of the history of doctrine. However, there is also an analytical case to §2. It is an attempt to make clear some of the fascinating dynamic -indeed, business cycle- insights buried in the comparative static framework of MIP.

In §3 we try to suggest new frontiers when the conventional concepts are interpreted in the light of new 'theoretical technologies'. We address, quite specifically, the perennial issue of separating the real from the nominal, in market signals, by the rational agent,

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[4]) It must be remembered that Walras did, after all, correspond with Picard and that Picard was the first to apply the method of successive approximations to ordinary differential equations (and to many cases of partial differential equations too) - and that Edgeworth was personally acquainted with Hamilton and Clifford. It may not be long before solitons enter the vocabulary of mathematical economists and then via the sine-Gordon equation even Clifford algebras will find a place and then detected somewhere between lines in Mathematical Psychics !

Patinkin's 'utility computor'. Thus, the section is subtilled : 'Utility Computor's - and all that (5)!

In the final section the thoughts on the glimpses beyond are, hopefully, more constructive than speculative.

§2 . THE MIP AGENDA AND ITS UNFOLDINGS

MIP brings to completion the great tradition in monetary economics that had been developed in the interwar years by Hawtrey, Robertson and Keynes in Cambridge ; Lindahl, Myrdal and Ohlin (6) in Sweden ; and von Mises, Schumpeter and Hayek in Austria (7). They all had at least one aknowledged indebtedness in common : the legacy of Wicksell. 'The Wicksell connection' - to use Leijonhufvud's felicitous phrase - in the development of interwar monetary macroeconomics is a well-mined research topic, not least by Patinkin himself. There is very little we can or wish to add to the massive and distinguished literature on various Wicksellian connections except to express our own conviction that an adequate appreciation of MIP is impossible without some understanding of the doctrine-historical background to the analytical core of interwar monetary macroeconomics. Much of this background can be gleaned from Patinkin's own

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5) With apologies to the witticism of D. H. Robertson's famous collections of essays titled 'Utility and all that' - though the context is not unrelated.

6) The traditional enthusiasm of the profession to include Ohlin in this trio, on the same footing as Lindahl and Myrdal, should, perhaps, be moderated. Even as 'late' as in 1932 Lindahl was writing to Lundberg :

> "Even Ohlin is now trying to *learn* monetary theory..."
> (även Ohlin söker nu lära sig penningteori ...')
>
> Letter from Lindahl to Lundberg, 13th March 1932 ; italics added.
> Our translation from the original Swedish.

7) Contrary to note (5) above it is customany not to include Schumpeter in this trio. His early work in monetary theory and, in particular, his famous criticism of Böhm-Bawerk's interest theory are important, but unwritten chapters, in the development of Austrian monetary and business cycle theories.

scholarly output since MIP ; but a broader perspective illuminates the structure of MIP in ways, perhaps, not even contemplated when it was written.

The agenda of MIP is not - and was not meant to be - the 'whole' of macroeconomics. As we remarked earlier, although only indirectly, capital theory, trade theory, public finance and business cycle theory are not part of the main theme of MIP.

Elements of these subdisciplines are, however, discussed in relevant places and the impact of results from within the analytical structure of MIP on them are also mentioned where necessary ([8]).

The analytical agenda implicit in MIP seems, however, to have been quite broad in that an attempt was made to encapsulate, within the broad framework of an integrated value analysis of a monetary economy, the issues of : *unemployment disequilibria* , *effective demand, neutrality, real balance effects, temporary equilibria* in a stable setting with the necessary uniqueness against the backdrop of the tools and theoretical technologies implicit in setting in their paces *expectations, tatônnement, comparative statics* (correspondence principle) and, above all, the *macroeconomic consequences of the behaviour of optimizing agents* .

The strengths and weaknesses of MIP can be traced back to the interaction between these few potent issues and the tools and 'theoretical technologies' that were to give them analytical content. Thus the 'dogma' of the stability ([9]) of tatônnement and the expectational foundations of temporary equilibrium ; individual and market 'thought experiments' of optimizing agents to generate the excess demand functions which lie at the core of all of the comparative static exercices in MIP ; and other permutations ([10]).

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8) The careful reader of MIP would have discerned the rudiments of the discussions around 'Are Government Bonds Net Wealth ?' in chapter XII, §4 - complete with the story of the conversation between Christ and Friedman !

9) We have it, on Samuelson's authority, that it was a dogma (cf. Samuelson, 1974, p.10). MIP is, almost in entirety, an analytical structure confined to linear and stable systems even when functions were written in full nonlinear generality.

10) Whimsically, but not entirely frivolously, the story of macroeconomics is also the story of Wicksell effects, the Fisher effect, the Keynes effect, the Pigou effect and the Tobin effect. The weaving of a macroeconomic tapestry by these effects required embroidery by other 'whimsical' elements as well : the 'original' natural rate and its 1967 imitation - more than anything else.

The precise unfolding of the MIP agenda has two strands : one, the well trodden path, is the story of fix-price macroeconomics : the other, less told story, is the affinity with two strands of old-fashion Keynesian business cycle theory.

The former is the macroeconomics of the interregnum between the demise of the neoclassical synthesis and the rise of Lucasian or new classical macroeconomics. It is a story, well told by many, which we have also tried to tell (cf. Fitoussi, 1983 and Fitoussi and Velupillai, 1987) A succint way of descripting the roots in MIP of these fix-price endeavours is to say that it was the expectational macroeconomics of general and temporary equilibrium economies with value rigidities. The most recent off-shoots of this version of macroeconomics, with clear roots in MIP are, of course, varieties of so-called new Keynesian macroeconomics. In these 'varieties' the attempts to rationalize the rigidities are in a setting of rational expectations. The central conceptual economic issues remain substantially the same : neutrality and the real effects of nominal disturbances ; real balances vs. intertemporal substitution in accounting for macroeconomic fluctuation, etc. However the visions have broadened and the 'theoretical technologies' have deepened. Much of the old wine is couched in terms of 'propagation and impulse' mechanisms and less in the words of transmission mechanisms and irregularities. Imperfectly competitive markets and increasing returns technologies for economies in a growth setting peopled by agents with rational expectations and consistent decision rules is almost the canonical macroeconomic model.

Events have, to some extent, as they must, caught up with MIP - but not quite.

In relation to the issues we want to emphasize in the next and subsequent sections the most significant contribution of the macroeconomics of the interregnum was the reopening of the question of *the analytical* nature and scope of *effective demand* (and, hence, the multiplier process : spillovers, etc.) and *involuntary unemployment*. The macroeconomists of the interregnum had to come to terms not only with the traditional preoccupation with the existence of equilibrium and the paraphernalia that goes with it : stability and uniqueness, above all ; but also, in view of the need to interpret the meaning of *effective* and *involuntary* had to reopen the debate on the significance of *tatônnement* and *Walras's Law*. Thus the old Patinkinian concerns reappear, dressed in new 'theoretical technologies'.

The 'less told story' is, broadly speaking, the connection with the monetary part of Hicks's 'Contribution to the theory of the Trade Cycle'. Indeed the whole of the macroeconomic part of MIP, viz, part two of the book, is a variation on a theme based on that connection (and with the other two classic pieces by Hicks, 1937 and Metzler, 1951). To bring out this connection explicitly we proceed by way of correcting *a slip in MIP*.

In analyzing the dynamics of the bond and commodity markets Patinkin proceeds to set up the equilibrium relations as follows (all relations exactly as in MIP unless otherwise qualified) :

For the bond market, from the household demand function :

$$H\left(Y, \frac{1}{r}, \frac{M_o^F}{p}\right) \qquad \text{..........(1)}$$

and the firm supply function :

$$J\left(Y, \frac{1}{r}, \frac{M_o^F}{p}\right) \qquad \text{...........(2)}$$

in equilibrium :

$$B\left(Y, \frac{1}{r}, \frac{M_o^F}{p}\right) = H\left(Y, \frac{1}{r}, \frac{M_o^H}{p}\right) - J\left(Y, \frac{1}{r}, \frac{M_o^F}{p}\right) = 0 \qquad \text{..........(3)}$$

For the commodity market, from the demand relations :

(α) The household demand for consumer commodities :

$$C = g\left(Y, r, \frac{M_o^H}{p}\right) \qquad \text{.........(4)}$$

(β) The demand by firms for investment commodities :

$$I = h\left(Y, r, \frac{M_o^F}{p}\right) \qquad \text{..........(5)}$$

and (γ) The constant demand of government for consumption commodities :

$$G = G_o \qquad \text{..........(6)}$$

giving the aggregate demand function for commodities :

$$E = F\,(Y, r, \frac{M_o}{p}) = g\,(Y, r, \frac{M_o^H}{p}) + h\,(Y, r, \frac{M_o^F}{p}) + G_o \qquad \text{...........(7)}$$

In slightly less cumbersome terminology one would refer to (4) and (5) simply as the consumption function and the investment function. The reason for laboriously setting up these familiar functions will become clear soon.

The aggregate supply function in the commodity market, for the economy as a whole, is given by :

$$Y = S\,(\frac{w}{p}, K_o) \qquad \text{..........(8)}$$

and, in equilibrium :

$$E = Y \Longleftrightarrow F\,(Y_o, r, \frac{M_o}{p}) = Y_o \qquad \text{..........(9)}$$

for a fixed level of output, Y_o.

To analyze the special case of distribution effects due to the 'forced savings' brought about by price increases Patinkin proceeds as follows :

(i) : assume differential savings propensities between 'lenders' and 'borrowers ;

(ii) : assume a price increase ;

(iii) : in view of (i) a possible effect of (ii) would be a decrease in the demand for consumption commodities (cf. (4)) and increase in the demand for bonds and money (cf.for eg., (1)) ;

(iv) : hence money will not be neutral.

For the case of a negatively sloping equilibrium locus of p and r for the bond market, introducing 'an index of the distribution effect which results from a price change' (MIP, p. 498) :

$$h = h(p) \qquad \text{..........} \quad (10)$$

we have (denoting M_o by M) (3) and (9) rewritten as :

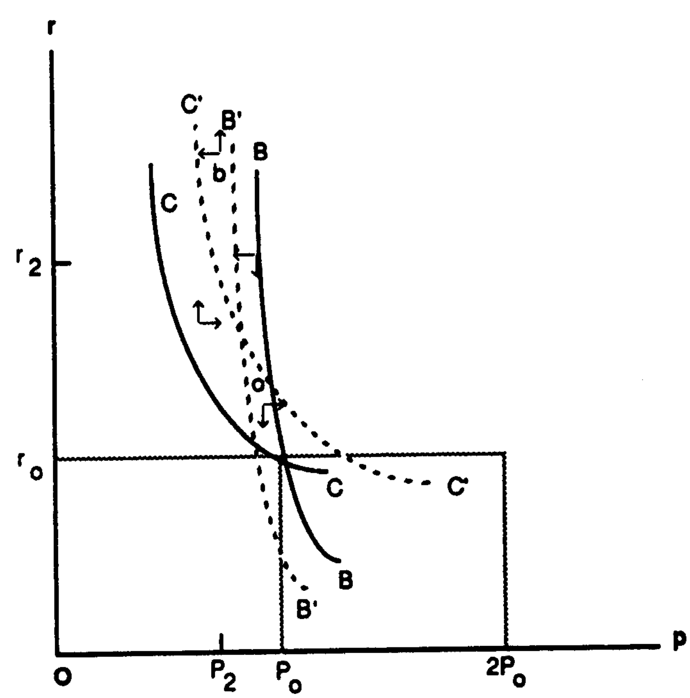
$$B\left[\, Y_o, \frac{1}{r}, \frac{M}{p}, h(p)\right] = 0 \qquad \text{..........}(11)$$

$$F\left[\, Y_o, r, \frac{M}{p}, h(p)\right] - Y_o = 0 \qquad \text{..........}(12)$$

with : $h'(\,) > 0$, $B_4 > 0$ and $F_4 < 0$

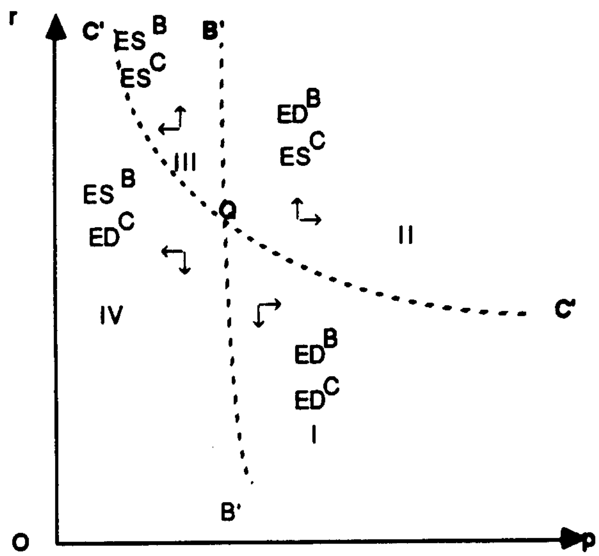
We reproduce below figure A - 8 on p. 502 of MIP :

Figure 1.



For the case of one set of loci in the p - r space for the two markets a simple version of fig. 1 would be as in fig. 2.

Figure 2.

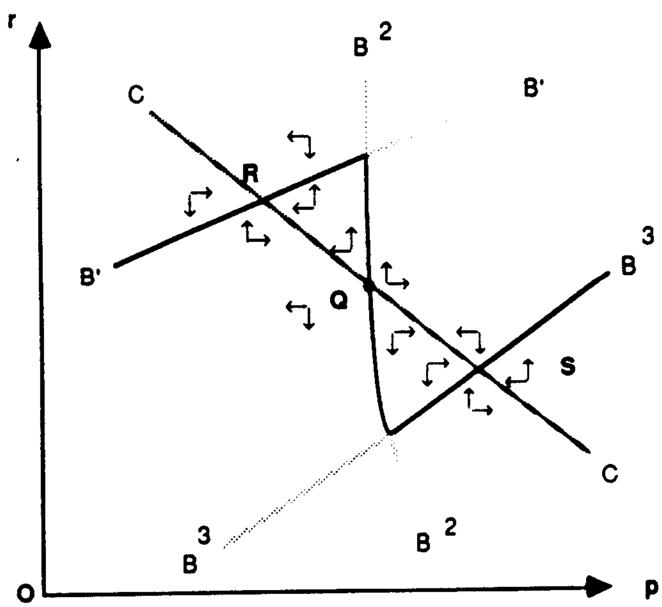


ED : Excess demand
ES : Excess supply
The superscripts denote the markets, bonds (B) and commodities (C).

The economic slip in MIP comes about as follows. As we indicate in fig. 2 in regions II and IV we have asymmetric market conditions : excess demand for the bond market in region II, and excess supply, in region II for the commodity market ; vice versa in region IV. The apparent paradox of excess supply in the commodity market, in region II, driving prices up and excess supply in the bond market driving interest rates down in region IV is the cause of the slip in MIP. In view of the negative slope of B'B' it is clear that the real balance effect in the bond market, due to a price rise for example, is dominated by the distribution effect brought about by the assumption of differential savings propensities (leading also to asymmetric behaviour in the commodity and bond markets : hence the instability of the equilibrium point). The economic essence of instability is, of course, when the 'law of supply and demand' works perversely : driving prices up when there is excess supply and driving prices down when there is excess demand. In the present context this is generated by switching around the normal effects of the real balance process and the distributional mechanism.

But, of course, such an anomaly or, perhaps, an abnormality comes to an end by a breakdown in economic activity or, more likely, due to 'endogenous' mechanisms that restore the normal ranking between the real balance and distributional effects. This means multiple equilibria and nonlinearities and, diagrammatically, is best illustrated by assuming that the unstable part of the equilibrium locus in p - r space for the bond market is sandwiched between stable loci, as in fig. 3, where R and S are stable equilibria and Q is an unstable equilibrium.

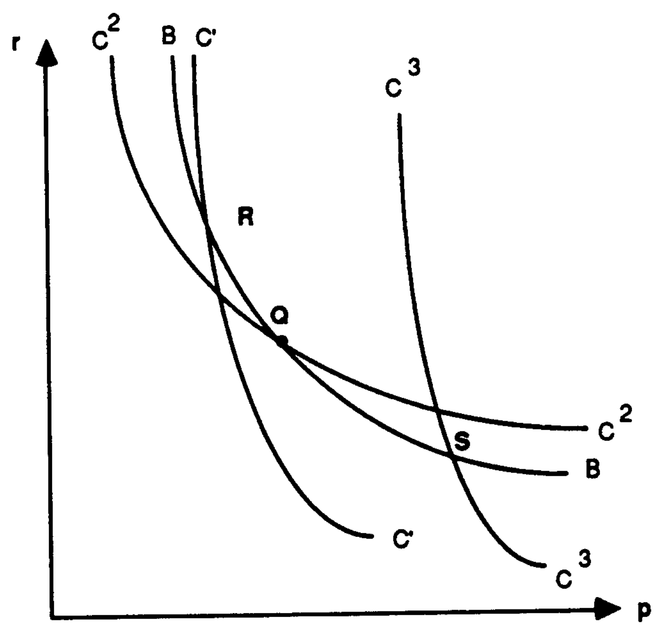
Figure 3



Harnessing any number of elegant theorems and approaches from the massive literature (even in economics) on planar dynamical systems it is easy to demonstrate the existence and uniqueness of a meaningful 'distribution cycle'- or better : *Patinkin's Monetary Distribution Cycle* ([11]).

Alternatively we can have the commodity market equilibrium locus in the p - r space nonlinearly patched up due to the real-wage term in the supply function (8) :

Figure 4



Patinkin was, of course, well aware ([12]) of the economic rationale underlying the possible instability of the equilibrium point Q, for example in fig. 2. However he was, obviously, handicapped by an inability to harness results from the 'theoretical technology' of nonlinear dynamics and, perhaps, the weight of the 'dogma of stability'.

To continue with the theme that the 'less told story' is the connection with Keynesian business cycle theory of an older - and, in our opinion, nobler-vintage and to link it up

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11) This is vintage stuff ! From Wicksell through Lindahl, Hayek and Keynes to Kaldor and Pasinetti - and, now, via Patinkin too.

12) See, in particular, MIP, p. 503.

with the discussion about involuntary unemployment in the famous ch. XIII of MIP is the aim of the rest of this section.

First and foremost Patinkin is unequivocal - correctly in our opinion - on the issue of the central dividing line between Keynesian and classical economics : 'the efficacy of an automatically functioning market system with flexible money wages in eliminating *involuntary unemployment* ' : (MIP, p. 315 ; italics added).

Secondly, contrary to Keynes, Patinkin identifies involuntary unemployment as a *disequilibrium* configuration.

Thirdly, again correctly, that therefore involuntary unemployment cannot be analyzed by the methods of static equilibrium analysis. The implication being that disequilibrium analysis is inherently dynamic.

Fourthly the behavioural underpinnings for the dynamic path from a disequilibrium configuration towards the equilibrium position is given by making a distinction between an actual position in phase-space and the desired position. The latter being the optimum configuration derived by the optimizing principles of rational behaviour. Referring, therefore, to point G in figure XIII-2 of MIP, Patinkin is able to say that it represents a point of 'excess of desired over actual supply' (MIP, p. 321).

Against this background it is easy to see how it links up with vintage Keynesian models of the business cycle where the capital stock adjustment principle plays a significant role. An inspection of the supply function (equation (8), above) shows that the *desired* configurations are derived for given amounts of the total fixed capital equipment of the economy (K_0 ; MIP, p. 203). But a point such as G, as Patinkin quite unambiguously states, represents a situation of *excess productive capacity* . This idle capacity is allowed to persist throughtout the adjustment process which takes the economy from a disequilibrium to an equilibrium - i.e., optimum configuration. This, surely, is nonoptimal behaviour especially if the adjustment process is 'unduly' long. Patinkin, again, is perfectly aware of this anomaly. In fact the brilliant summary, whilst identifying Keynesian economics with the 'economics of underemployment *dis*equilibrium' (MIP, pp. 337 - 338 : italics in the original) cataloguing the complexity of the adjustment process is unequivocal :

> "(Keynesian economics) argues that as a result of interest-inelasticity, on the one hand,and distribution and expectation effects, on the other, the (disequilibrium) dynamic process... - *even when aided by monetary policy* - is unlikely to converge either smoothly or rapidly to the full employment equilibrium position. Indeed, if these influences are sufficiently strong,*they may even render this process unstable* . In such a case the return to full employment would have to await the fortunate advent of some exogenous force that would expand aggregate demand sufficiently."
>
> (MIP, p. 338 ; italics added)

Expectations, distribution and interest inelasticity effects on the expenditure side is the stuff of which vintage Keynesian business cycle models were made. It is clear also that it would be unrealistic and unaesthetic to maintain the assumption of persistent excess capacity during a convergence process which is neither smooth nor rapid - and, moreover, which may well be unstable. Again Patinkin is more than clear on these possibilities and permutations :

> "But it is not necessary to go to either of these analytical extremes (i.e., unstable equilibrium or non-existence of an equilibrium). As already indicated, even if monetary policy could definitely restore the economy to full employment, there would still remain the crucial question of the length of time it would need. There would still remain the very real possibility that it would necessitate subjecting the economy to an intolerably long period of dynamic adjustment : a period during which wages, prices, and interest would continue to fall, and - what is most important - a period during which varying numbers of workers would continue to suffer from involuntary, unemployment. Though I am not aware that he expressed himself in this way, this is the essence of Keynes' position. This is all that need be established in order to justify his fundamental policy conclusion that 'the self-adjusting quality of the economic system' - *even when reinforced by central bank policy* - is not enough."
>
> (MIP, p. 339 ; italics added)

If the expectations of the firms are such that investment expenditures are postponed (MIP, p. 337) surely the assumption of a given and unvarying K_o is no longer tenable. The desired level of capital equipment will be re-computed if the length of the disequilibrium configurations become sufficiently long and, in particular, if it is expected to be long. This would lead to an adjustment of the *actual* capital stock (K_o, to begin with) to be adjusted downwards to the newly computed *optimum, lower, desired* level of capital equipment. Patinkin puts the whole burden of quantity adjustment on inventories. This may will be acceptable and even realistic if the adjustment process is rapid - or, again, expected to be so.

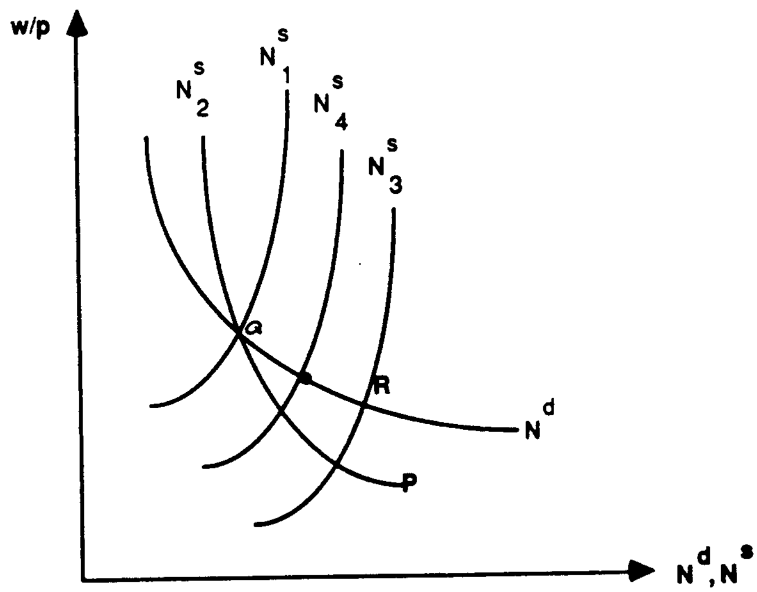
The disinvestment from K_0 to the optimum, lower, desired level K^* would proceed simultaneously with the adjustment due to the real balance effects (and monetary policy). At some point, not necessarily simultaneously with the efficacy of the real balance effects, K_0 would tend to equality with K^* and disinvestment would tend to cease. But this is, implicitly, nonlinear behaviour and is exactly the capital stock adjustment principle of vintage Keynesian business cycle models. However, there is a new twist. If the real balance effect continues to work, the new, lower K^* would *appear* - to the firm - to be too low and they would update their expectations and, once again, recompute a new, *higher*, optimum desired level of capital equipment, $K^{**} > K^*$, net investment would increase. The cycle will repeat. Only in the unlikely event - in fact, extremely unlikely - that value adjustments and the stock adjustment process synchronize would tranquility prevail. The 'unlikeliness' is encapsulated by an instability which can come about by a combination of expectations, interest inelasticity and distribution effects on the expenditure side in any of the vintage Keynesian ways ; for example, the classic nonlinear investment relation which is interest inelastic at very low and very high levels of the interest rate (the famous Kaldorian s-shaped curve). We will, however, suggest a different nonlinearity which is consistent with Keynes's definition of involuntary unemployment and meshes smoothly inside the system of relations postulated by Patinkin in MIP given above.

Just to recapitulate (one of) Keynes's definition of involuntary unemployment was expressed as follows :

> "Men are involuntarily unemployed if, in the event of a small rise in the price of wage-goods relatively to the money-wage, both the aggregate supply of labour willing to work for the current money-wage and the aggregate demand for it at that wage would be greater than the existing volume of employment."
>
> (Keynes, GT, p. 15 ; The whole quote is in italics in the original)

To encapsulate this, within Patinkin's system in MIP, against the backdrop of the discussion above on the capital stock adjustment principle in terms of desired and actual levels we must flesh out figure IX-I (p. 204, MIP) in the following way :

Figure 5



The equilibrium point Q is unstable and the relevant position of the supply curve is given by the non-dashed curves. QP is relevant in the recovery period from K* to K**. Thus equation (4) on p. 203 of MIP, written here as :

$$N^S = R\ (w\ /\ p) \qquad \text{..........(13)}$$

incorporates at least one of the 'well-known reservations' in assuming that labour 'supply is an increasing function of the real wage rate' (MIP, p. 203). It is not that agents *choose* to supply more labour as the real wage rate falls ; invoking the Talmudic dictum and reflecting upon the economics of the new Rabbis (Lucas, Barro and co !) agents may have had so much 'leisure' by being unemployed for so long that a point may come at which leisure is inferior. It is not difficult to find utility functions rationalizing the market results of figure 5. This means the supply curve (equation (8) above) is nonlinear quite asymmetrically. If we now collect the various disparate elements together and set it up as a dynamical disequilibrium we get :

$$\frac{dY}{dt} = \Omega \{[g(Y, r, \frac{M^H}{p}) + h(Y, r, \frac{M^F}{p}) + G_o] - S(\frac{w}{p}, K)\} \text{.........} \quad (14)$$

$$\frac{dB}{dt} = O[H(Y, \frac{1}{r}, \frac{M^H}{p}) - J(Y, \frac{1}{r}, \frac{M^F}{p})] \quad \text{..........} (15)$$

$$\frac{dN}{dt} = O[Q, (\frac{w}{p}, K) - R(w/p)] \quad \text{..........} (16)$$

where : $N^d = Q(w/p, K)$ .... (17)

(cf. MIP, p. 203).

Because of the nonlinear interactions between the capital stock adjustment principle and the 'perverse' part of the labour supply relation and because of interest inelasticities in, say, h and J, highly complex dynamics can be generated by this system of relations(13) The particular question of underemployment ***equilibrium*** vs. underemployment *disequilibrium* can be studied by making a small compromise between Keynes and Patinkin (14). We can grant Keynes the point Q as the relevant point because a fall in real wages increases labour supply ; we can grant Patinkin the *dis*equilibrium part by assuming that R is the desired level. Then the most effective way to understand, analytically, the compromise seems to be the following :

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13) In fact the best way to study the problem of ***involuntary*** unemployment as an underemployment *equilibrium* would be to consider Q to be "metastable" ; i.e., stable in some directions and unstable in others. This requires the full force of dynamical systems theory and, in particular, the consideration of homoclinic trajectories. In work that is in preparation we are proceeding along these lines. This will also remove the *mathematical* infelicity depicted in figures 1 and 2.

14) We must assume that R is the ideal equilibrium.

(α) : Determine the level of capital stock consistent with equilibrium in the labour market at point Q.

(β) : Using the production function, for that level of capital equipment, determine the level of desired supply, S in (14).

(γ) : Now determine whether the subsystem so determined :

(i.e. :

$$\frac{dY}{dt} = \Omega \{[g (Y, r, \frac{M^H}{p}) + h (Y, r, \frac{M^F}{p}) + G_o] - Y^S \quad .... (18)$$

and (15) ;

where Y^S is based on (β) above) is stable. Since an initial disequilibrium is remedied by variation in the level of income and by adjustments in the bond market we feel able to say that MIP contains the true core of GT.

(γ) : On the other hand to be true to Patinkin and the general equilibrium spirit of his analysis we must consider the full system of equations simultaneously, in which case involuntary unemployment turns out to be a disequilibrium phenomenon.

Thus the Marshallian roots of Keynes become evident and the Walrasian basis of Patinkin also become clear.

In concluding this section we wish to make the following observations :

(i) : From the system of relations postulated by Patinkin in the macroeconomic parts of MIP we can derive every one of the Keynesian models of business cycles developed between Kaldor in 1940 and Benassy in 1984.

(ii) : The most illuminating way to study the system of relations (14) - (16) would be by simulation. In view of recent results in the theory of dynamical systems we can expect exotic dynamics.

(iii) : Embedded in the system (14) - (16) is also a Minsky (-Irving Fisher)- type debt - deflation theory of economic fluctuations.

(iu) : There is, finally, a puzzle : the obvious similarities between the monetary parts of Hicks's 'Trade Cycle Theory' and the dynamics of the macroeconomic part of MIP is nowhere discussed by Patinkin. This is as much perplexing as the absence of any mention of the Hayek-Sraffa debate on 'neutral money'.

Our basic aim in this section was to indicate two things : one was the fact that the issues at the conventinal frontiers of macroeconomic analysis owe much to the framework and concepts pioneered by Patinkin in MIP ; the other was to point out issues and frameworks in MIP that remain unexploited but have become a part of the heritage of the subject without explicit acknowledgement of parallel possibilities starting from MIP. This is unfortunate because the nonmonetary dominance of vintage Keynesian business cycle theory could have profited a great deal by being developed within the framework in MIP. And so much has been left unsaid. That is a measure of Patinkin's scholarship ; it is also a measure of our inadequacy ([15]).

§3 - 'UTILITY COMPUTERS' - AND ALL THAT :

Three of the most fertile metaphors in macroeconomics have been Patinkin's 'utility computer', Lucas's 'signal processors' and Phelps's 'island paradigm'. Patinkin conceptualized the central construct of his analytical framework - the excess demand function - by imagining the existence of a utility computor in a thought experiment ; Lucas and the newclassicals made the signal processing agent the centrepiece of their equilibrium macroeconomics ; the perennial problem of the dichotomy between real and nominal was given an analytical lift, after the expectational revolution of the mid-sixtees, by Lucas's imaginative use of Phelps's island paradigm.

[15]) It is possible that Lucas's monocausal view of the interaction between conceptual developments and 'theoretical technologies' has some substance to it. Had Patinkin been aware of nonlinear dynamics in some detail many of the slips in MIP could have been avoided - particulary because his economic intuitions seem always to overcome the mathematical infelicities. For example the system (11) and (12) with an index h (p) could have been more elegantly analyzed using bifurcation theory. Results were available - and even used in economics - but Patinkin was writing at the dawn of the mathematization of macroeconomics. Indeed he did more than alomost anyone else - with the possible exception of Paul Samuelson - to mathematize macroeconomics.

Macroeconomic themes at the frontiers still grapple with the problem of nominal impulses, plausible propagation mechanisms and possible real effects. Thus neutrality remains a central issue and monetary macrodynamics must resort to 'frictions' in the dynamics of propagation (cf. Sargent, 1987, ch. 4, § 4.2). And thus too we have the rational agents recursively filtering noise from signals - to separate real from nominal. The signal processor is depicted as a Kalman filter and learning enters the scene via the innovations term and rational expectations equilibria find a 'natural' formalization (cf. Appendix 1) ([16]). The full force of the developments in the 'theoretical technology' of one type of applied mathematics is harnessed to make newclassical macroeconomics a formidable fortress of mathematical formalism - to some eyes. But all is not well. The cracks show, in particular, in the paucity of meaningful learning mechanisms that transcend the aridity of simple and general recursive least squares. Above all much of the lip-service paid to so-called recursive economics does not come to terms with the central issue in recursive analysis : recursive enumerability and computability. For example, the massive volume on 'Recursive Methods in Economic Dynamics' (Lucas et. al, 1989) is based on a probabilistic structure, which is central to the whole philosophy of Lucasian macroeconomics, which is neither recursive nor computable. The concept of randomness employed there - and, indeed, the whole of mathematized macroeconomics - is not generated by a recursive mechanism. Surely a consistent recursive analysis of an applied area of research should base the probabilistic structure on computable foundations and define randomness in a similar way. It will not do to appeal to Kolmogorov type axiomatic probability theory and a definition of randomness without any relation to its

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[16]) With characteristic intellectual candor Sargent expresses these thoughts in a clear way :

> "Economists more quicky learned and used the techniques of intertemporal optimization developed by mathematicians and control scientists than they did the tools of optimal prediction and filtering of stochastic processes. From a technical viewpoint , this is peculiar since the optimal control and optimal filtering problem are dual to one another and involve essentially the same mathematics. In the 1950's and early 1960's, much literature occurred in which agents were imagined to solve optimal control problems using the calculus of variations, but also to use static or adaptive expectations generating mechanisms. Given the belief in the usefulness of optimizing economic theory that the first part of this research plan reflected, it was only a matter of time and technical understanding that optimizing theory would also be applied to restrict the formation of expectations."
>
> (Sargent. 1982, p. 382, f.n.1)

recursive generation and claim that the agent is a signal processor recursively updating decision rules and learning the true parameters of a rational expectations equilibrium. It is, if we are permitted to be quite blunt, absolute nonsense. Of course if it is a philosophy of methodological ad-hockery then we have no objections.

But a school that prides itself in doing hard, consistent and empirically meaningful macroeconomics and castigates Keynes and his motley group of followers of ad-hockery brings to mind the old saying about 'people in glass houses ...'.

The point we wish, however, to make in this section is the following : every metaphor that has been suggested, in formal economic theory, to model Rational Economic (Wo)man, (REM) has a representation that can be formalized in terms of a finite Automaton ([17]). The implications of this assertion ([18]) are many :

(i) Patinkin's utility computor cannot be fed *any* arbitrary sequence (sic !) of prices to obtain, as output, 'a corresponding sequence of "solutions" in the form of specified optimum positions' to 'conceptually generate the individuals' excess-demand function' (MIP, p.7). The sequence of prices, or the domain over which prices are defined, must possess a certain mathematical structure, for example, recursive enumerability for the feasibility of *effective* computation of optima - or, in fact, any computation at all. This way of approaching the genesis of the excess-demand function may well be useful in circumventing the ostensibly devastating consequences of S-D-M ([19]) type results (discussed in Mc Call-Velupillai, 1990).

(ii) Similarly, Lucasian signal processors cannot process *any arbitrary* signal. This is an *easily* shown consequence of a variety of results from metamathematics and computability theory.

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17) It will be useful to remind readers, in view of the sequel, that Finite Automate (FA) were used, first, by Mc Culloch and Pitts (1943) in their seminal paper to model neural nets. For simplicity we use the term FA generically and, thus, the conventionally more powerful Turing machines are also FAs.

18) Partially demonstrated in Rustem and Velupillai (1990) and more fully explored in Mc Call and Velupillai (1990).

19) S-D-M denotes Sonnenschein-Debreu-Mantel.

(iii) Above all it is not the case that any arbitrary signal can be deciphered for patterns - systematic elements - by demonstrable learning algorithms - and most certainly NOT by the standard formalisms of newclassical macroeconomics.

All the formalisms for the assertions have been developed in Rustem and Velupillai (1990), Mc Call (1989), Mc Call and Velupillai (1990). Here we try to illustrate the utility - indeed, ultimately, the necessity - of viewing REMs as FAs with one concrete, simple, example which is paradigmatic - almost.

The example we wish to consider is the problem an agent (REM) faces when confronted with the necessity of separating real (R) from nominal (N) signals. Formally it is the problem of separating R from N : i.e., the problem of the exclusive OR ; R or N but not both. Since fundamental to any choice theoretic paradigm of economic decision making is the assumption that rational decisions are based on relative values and free of money illusion it would be interesting to know the nature and constraints of the computational architecture that can achieve the separation, of R(eal) from N(ominal). In the appendix some background notation and the building blocks are defined and explained with simpler examples ([20]). We look at it geometrically first, then at the equivalent algebraic problem and, finally, the neural network that achieves the necessary computation.

The Geometry :

The signal (imput) can contain R, N, both or neither. Thus : the agent would have to identify four possible imputs and assign truth values to the output signals appropriately. In a tabular form :

[20]) There is, by now, a vast literature on this topic. A particulary readable text is the one by Arbib (1987).

INPUT		OUTPUT	
R	N		
0	0	-->	0
1	0	-->	1
0	1	-->	1
1	1	-->	0

Table 1

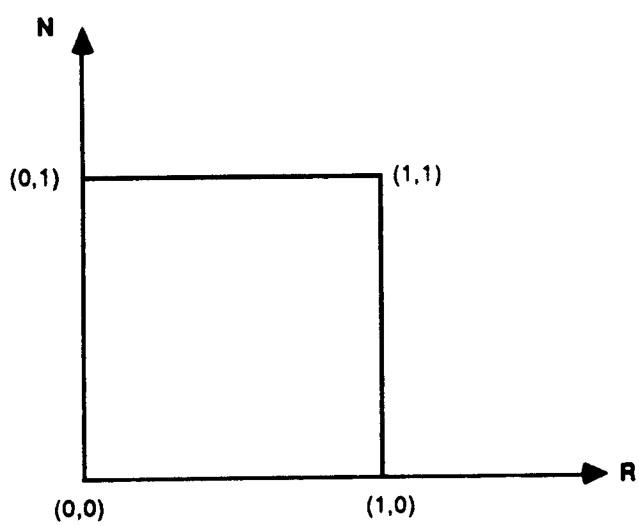
Where : O : false

1 : true

Geometrically :

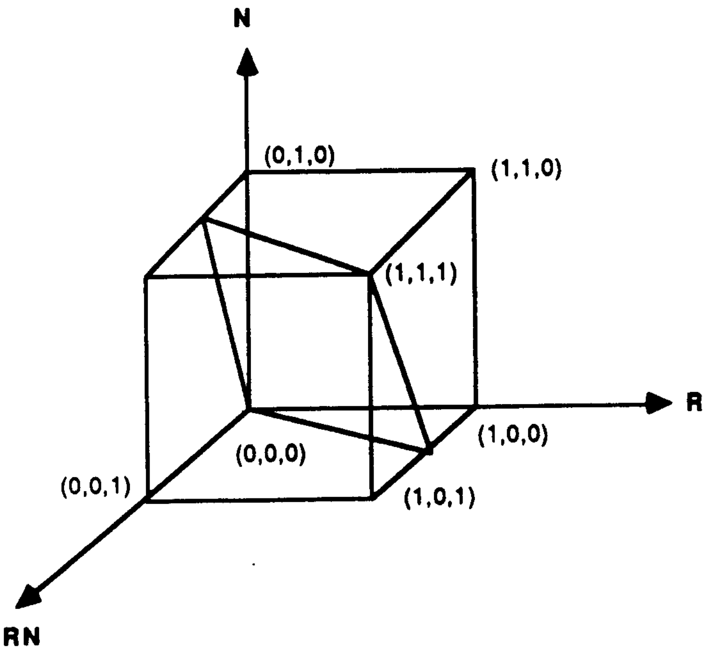
It is easy to see from figure 6 that no 'line' can be drawn such that <10,01> is on one side and <00,11> is on the 'other' side ; i.e. the two sets of input signals are not (linearly) separable.

Figure 6



However, if we allow second-order predicates we can visualize a possible separation as follows :

Figure 7



By introducing the second-order predicate RN we are able to separate R and N in 3-space.

The algebria : (cf. Appendix for notations)

$w_1 R + w_2 N > \theta$

where : $\theta \geq O$

$w_1 > \theta$

$w_2 > \theta$

and : $w_1 + w_2 \leq \theta$

which is impossible. In the language of the notation in the appendix, Real OR ELSE Nominal (the exclusive-or) cannot be evaluated (computed) by simple Mc Culloch-Pitts units. We have to nonlinearize the problem (a slight novelty in a profession which linearizes everything on sight) in the following way :

$$w_1 R + w_2 N + w_{12} RN > \theta$$

Where : $\theta \geq O$

$w_1 > \theta$

$w_2 > \theta$

and : $w_1 + w_2 + w_{12} \leq \theta$

A possible solution is given by :

$$\theta = O, w_1 = w_2 = 1 \text{ and } w_{12} = -2$$

Separability and nonlinearity go together. Before we proceed to the architecture that can achieve this evaluation a few remarks must be made :

(α) Reverting to a general formulation and putting R = X and N = Y the problem we posed was whether evaluation, in some sense, was possible for X OR ELSE Y. The negation of X OR ELSE Y is *not*< X OR ELSE Y>,

i.e., X OR ELSE Y = (X v Y) ^ (~(X ^ Y))

= (X ^ ~ Y) v (Y ^ ~ X)

Thus ~ (X OR ELSE Y) = ~ [(X ^ ~ Y) v (Y ^ ~ X)]

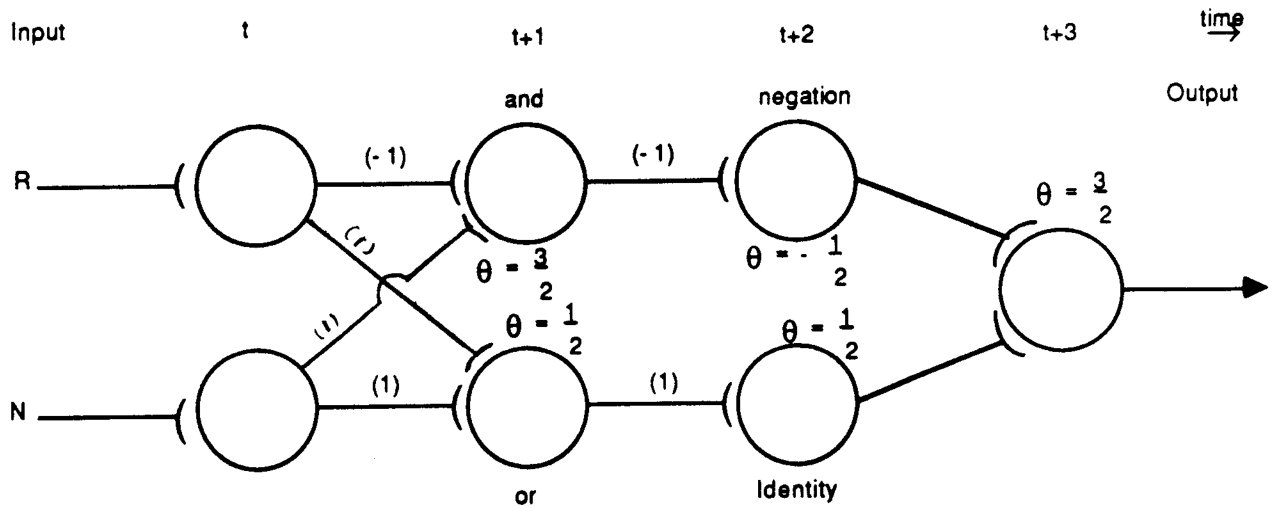
Now, ~ [(X ^ ~ Y) v (Y ^ ~ X)] is a ***universal proposition*** i.e., every proposition (in the predicate calculus) can be expressed as a string of ~ [(X ^ ~ Y) v (Y ^ ~ X)].

In other words it is computationally universal in Turing's sense.

(β) From the algebra and geometry of the resolution of the problem it is clear that *NONLINEARITY* is essential to construct a universal computing machine.

A Neutral Network for R OR ELSE N :

Figure 8.



Remark 1 : The function of the 'identify' operator is to store R v N while R ^ N was being evaluated-in this case negated.

Remark 2 : No choice of weights and thresholds would allow the neuronal network to compute (evaluate) R OR ELSE N in a SINGLE step.

From the geometry and algebra we derived the necessity of NONLINEARITY. From the computational architecture we derive the necessity of TIME LAGS (if such a demonstration is necessary - although in a profesion obsessed with *time to build* it may be salubrious to respect *time to think*).

What about learning ? Conventionally all units - and collections of units - between those that have been initially identified as input and output groups of units are called HIDDEN units. The learning process is that of imputing a set of weights to the hidden units such that for given input signals an algorithm can be devised to achieve the desired outputs (by assigning appropriate weights to the hidden units). Formally :

$$dy_i / dt = - \alpha y_i + \beta f_i \; [\Sigma w_{ij} \; y_j] \qquad ....(19)$$

where :

y_i : activity of the i th unit

w_{ij} : connection weights between units i and j

(cf. also appendix 2)

α, β : positive scalars

From the 'simple' example of R OR ELSE N we know that the system must be a dynamical system in R^n, $n \geq 3$ and that f_i must be ***nonlinear***. The attractors of (19) for :

$$\alpha y_i = \beta f_i \{\Sigma w_{ij} y_j\} \qquad ....(20)$$

can be viwed as the solution to a nonlinear algebraic equation with some integer constraints which makes it as hard as any nontrivial diophantine problem ; which also means that general purpose algorithms (Lucasian 'steody states') do not exist and, also, the problem is NP - hard The very fact that the dynamical system is nonlinear and in R^n, $n \geq 3$ means also that the space of attractors contain, in addition to the conventional limit points and limit cycles, strange attractors and Wolfram's 'fourth class' of attractors delicately balanced on the borderline between limit cycles and strange attractors.

Now, *learning* in our context is to find *algorithms* to adjust the weights w_{ij} assigned to the hidden units such that from any given initial state for y and a collection of input signals, say σ (a vector of signals) leads to a solution of the dynamical system (19) with a limit set with desired output configurations. Given the nonlinear nature of the dynamical system (or the diophantine from of the algebraic system) we know that the *algorithms must be heuristic*

If, therefore, agents have learned, by 'trial and error' a set of weights w_{ij} which partitions the solution space of the dynamical system (19) into limit points, limit sets, strange attractors and Wolfram's 'fourth class' then the equivalence classes of the partitions are the 'steady states' of decision rules to evaluate preferences. However, the second part of the 16th of Hilbert's famous 'Mathematical Problems' remains unresolved ([21]).

Assume now a disturbance to the economic system manifested in wholly new input signals which must be transmitted to the agent. Quite apart from the nonlinearity and time-lag noted above the agent may now have to re-partition the solution space - i.e., assign new sets of weights.

Would it be unreasonable to assume that agents will first try to interpret the new signals with the 'known' algorithms to assign a set of weights such that the output corresponds to the equivalence class ('basins of attraction' in the language of dynamical systems) of some existing attractor. If it is inappropriate the consequences will force the rational agent to repartition by 'relearning' new weights. Thus signals that lead to a confirmation, in terms of consequences, of the existing procedures, weights and attractors will tend to strengthen the adopted weights. This is a generalized statement of the Hayek-Hebb rule of learning. Conversely, signals that call forth modifications may well be resisted in proportion to the costs incurred in reaching a particular partitioning in the first place.

The technicalities of all the above assertions, remarks and interpretations are developed in the companion piece by Mc Call and Velupillai (1990)

§4. CONCLUDING NOTES

We began with a citation from MIP where Patinkin invokes Talmudic wisdom to illustrate a particular vicious circle in economic methodology. The Rabbinic zeal with which the

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21) In Hilbert's own words (which may sound a bit archaic to the purists at the frontiers of dynamical system theory) "...The question as to the maximum number and position of Poincaré's boundary cycles (cycles limites) for a differential equation of the first order and degree of the form

$$\frac{dy}{dx} = \frac{Y}{X}$$

where X and Y are rational integral functions of the nth degree in x and y".

(Hilbert, 1902, p. 465)

When even this remains unsolved how can be expect to partition R^n, $n \geq 3$, in the attractor space !

high priests - and the low ones - of Lucasian macroeconomics preach this particular Talmudic wisdom has all but emasculated imagination and choice from the domain of economic analysis.

The rise of expectational economics brought with it another danger - complacency. The somnambulating effects of believing that if only we ***know*** everything we can compute anything will be the first casualty of advances in 'theoretical technologies' - particularly the advances in computability theory from the point of view of a variety of disciplines ranging from metamathematics to psychology. The closely related idea that if we can calculate anything all we need for somnambulation is to know everything is equally absurd - but seems, prima facie, easier to believe. Mercifully we have not quite come to believe that if we can compute anything we can know everything - although there are reductionists defending that corner too. The expectational revolution and its consistent incorporation of information in analytical macroecnomics is only *part* of the story. There is a computational aspect to information and randomness which, if not consistently incorporated in so-called recursive economics, makes nonsense of the extreme assumptions - the 'somnambulating effect' - of the rational calculus of REM. Neither the metaphor of the signal processor nor that of the utility computor can stand the analytic weight placed upon them.

And that is how the Keynesian insistence on coordination failures due to the inadequacy of price signals comes to play a significant role. Not any arbitrary signal can be processed even by the most complex of finite automata ; there are ultimate theoretical bounds. Implicit in the formalisms we have tried to present in §2 and §3 are such issues. To work them out in detail requires much more space than even the excesses we have used.

At the frontiers of macroeconomics Patinkinian themes are alive and well. Other issues have also a place : learning, computability, increasing returns, imperfect competition - all in a growth and cycle context. That there was, implicit in MIP, a cycle context is clear. Where Patinkin's 'theoretical technology' was inadequate his economic intuitions enabled him to sail through treacherous conceptual seas ; where advances in 'theoretical technologies' pushed the frontiers of macroeconomics, Patinkinian themes resurface again and again : neutrality, dichotomy, real-balance effects, tatônnement.

We have, in §2, tried to draw out some of the dynamical implications in MIP that Patinkin, somehow, did not find necessary to emphasize. It was seen that a rich structure of business cycle implications can be inferred. The book, in fact, bristles, with unexplored dynamics even although it is couched in comparative static terms.

In §3 we have tried to indicate new horizons starting from the conceptual basis in MIP and standard economic theory.

The somewhat pretentious title to this paper, viz 'Macroeconomic Perspectives' needs a word of explanation. Lucas, in his elegant Yrjö Jahnsson lectures, ends with a 'prophecy'.

> "The most interesting recent developments in macroeconomic theory seem to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of 'microeconomic' theory. If these developments succeed, the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will become superfluous".
>
> (Lucas, 1987, p. 107)

We might even agree - in spite of the optimism we wish to convey with our title. Then that we may agree with Lucas is for precisely the opposite reason. The ad-hockery that macroeconomists - particulary the school we endorse in the spirit of Patinkin - have been accused of is not half as serious as the ad-hockery of the choice - theoretic paradigm. This becomes evident as soon as we prise open the black-box that is preference (or technology). In trying to provide a glimpse into that black-box, in §3, we came out with nonlinearities, time-lags, uncomputabilities, diophantine monsters and attractors delicately poised in the indeterminate region between stable regimes - as if undecided on which side of the fence to jump.

This is the stuff of which human decision-making is made even in the Marshallian world of mankind in the ordinary business of living : ambiguities, undecidabilities, heuristics, mistakes and regrets.

To frame them we must ask new questions, develop new 'theoretical technologies' and frame new answers.

The profession is ripe for a new Patinkin

APPENDIX 1

The new classicals, Kalman Filter and computability

Somewhere, buried in the works of Gauss, there must be a recursive formulation of least squares - thus, Thomas Kailath is reputed to have said ; it has since been unearthed, translated and published in Peter Young's recent book on "Recursive Estimation". Time-domain formalisms and the recursive structure of estimation problems formalized by Kalman on the one hand and the recursivity of dynamic programming formalisms to tackle control problems on the other have excited economists ever since the late 50s. The plain excitement of the 60s seems to have become a passion-perhaps evan an obsession, of late. Some measured caution is required. The reasons - some of them, are spelled out in this section.

There is a widespread belief - a dogma, to add to those of stability, existence etc, that anything recursively formulated is computable. This is incorrect ; We will not spell out the details here (cf. Mc Call and Velupillai, 1990) but only point out that this mistaken belief - dogma - seems to be the main cause for the popularity of Kalman filtering formalisms. *There are , at least, three interrelated issues : recursive estimation, separation theorems and the analytics of decision rules.* Our discussion is, ultimately, a criticism from the point of view of computability theory in its full generalisation but remains implicit here (for details see Mc Call and Velupillai, op. cit). First on the Kalman filter formalisms.

(i) Claims have been made, within the metaphor of the agent as a "signal processor" that this formalism has descriptive realism and analytical tractability to formalize the so-called signal extraction problem : separate noise from information ; real from nominal. The references are to Lucas's derivation of the supply function in his influential 1972 paper and the whole formalism of his parametrized equilibrium business cycle model of 1975 (cf. Lucas, 1981, p. 192 for eg. and Cuthbertson, 1988, p. 225). This is the pure filtering problem.

(ii) There is also the related claim that "its usefulness would seem to stem from an interpretation of the Kalman filter as mimicking a learning process by agents" (Cuthbertson, op. cit., p.225).

For concreteness let us take a simple formalism : the general linear case. Let the system be described by the linear vector difference equation :

$$x(t+1) = F(t)\, x(t) + G\, \omega(t+1), \qquad t = 0,1 \qquad \text{..........(A-1)}$$

where : $x(t)$: n-vector state at time t ;

$F(t)$: an nxn nonsingular matrix ;

G : is nxr ;

and $\{\omega(t)\}$ is an r-vector white Gaussian sequence $\omega(t)$ $N(O,Q(t))$.

Assume now that discrete "linear" observations are taken at time instants t according to :

$$y(t) = H(t)\, x(t) + v(t). \qquad \text{..........(A-2)}$$

where : $y(t)$: an m - dimentional observation vector ;

$H(t)$: an mxn matrix

and $\{v(t)\}$: an m-vector white Gaussian sequence, $v(t)$ $N(O,R(t))$

and $R(t) > O$

in addition it is assumed that $x(o)$, $\{\omega(t)\}$ and $\{v(t)\}$ are independant

The Kalman-Bucy optimal filter equations for (A-1) and (A-2) are equations of evolution for the state estimate x and its covariance matrix P.

Define : $\hat{x}(t \mid t) \; \Delta \; E\{x(t) \mid Y(t)\}$ ....(A-3)

where : $Y(t) \; \Delta \; \{y(1), y(2), \ldots, y(t)\}$ ....(A-4)

In view of (A-1) we have :

$$\hat{x}(t+1 \mid t) = E\{x(t+1) \mid Y(t)\} = F(t)\,\hat{x}(t \mid t) \qquad \text{..........(A-5)}$$

The covariance matrix, at observations, corresponding (A-3), is defined as :

$$P(t \mid t) \; \Delta \; E\{(x(t) - \hat{x}(t \mid t))\,(x(t) - \hat{x}(t \mid t)^T \mid Y(t)\}$$

$$= E\{(x(t) - \hat{x}(t \mid t))\,(x(t) - \hat{x}(t \mid t))\} \qquad \text{.......... (A-6)}$$

with P (o) is an initial condition of the filter. The covariance matrix between observations, corresponding to (A-5), can be written in view of (A-1), as :

$$P(t+1 \mid t) = E\{(x(t+1) - \hat{x}(t+1 \mid t))\,(x(t+1) - \hat{x}(t+1 \mid t))^T \mid Y(t)\}$$

$$= F(t)\,P(t \mid t)\,F(t)^T + G(t)\,Q(t)\,G(t)^T \qquad \text{.......(A-7)}$$

The optimal filter equations are, then (A-5), (A-7) and :

$$\hat{x}(t \mid t) = \hat{x}(t \mid t-1) + K(t)\,\{y(t) - H(t)\,\hat{x}(t \mid t-1)\} \qquad \text{..........(A-8)}$$

$$P(t \mid t) = P(t \mid t-1) - K(t)\,H(t)\,P(t \mid t-1) \qquad \text{..........(A-9)}$$

Where the Kalman gain K(t) is given by :

$$K(t) = P(t \mid t-1)\,H(t)^T\,\{H(t)\,P(t \mid t-1)\,H(t)^T + R(t)\}^{-1} \qquad \text{.......(A-10)}$$

Substitution of (A-10)- in (A-9) gives the nonlinear discrete-time Matrix Riccati equation. As mentioned earlier most of the excitement surrounding the use of Kalman filtering formalisms is based on the apparantly "simple" recursive structures in (A-5), (A-7), (A-8) and (A-9). But is this excitement against the interpretation (i) and (ii) above justified ? The idea is that the innovations term in (A-8) weighted by the Kalman gain updates the system state as new information arrives sequentially. What are the problems ?

(1) Within the new classical system the characterization of agents as REM = < ≤ ,e, >
where : REM : Rational Economic (Wo)man

≤ : preference relation (binary)

e : endowments

must be supplemented by an additional information assumption : all agents are equally 'perceptive'. This is what comes out of the extreme newclassical assumption of 'one true model'. One true model, all know there is only one true model, and know it or will learn it. How ? By using system (A-5), (A-7), (A-8) and (A-9). But since the initial variance - covariance matrices are given, all learning is pseudo learning against the backdrop of RE, one true model and uniform perception. Even if we grant all this there is trouble ! The Matrix Riccati equation is a *nonlinear difference* equation ! Even in the ultimate simple situation of time-invariant coefficients such equations can describe exotic behaviour - as we all know, or should know, by now. Invoking the dogma of stability to get asymptotic solutions by imposing restrictions on the matrices (essentially in terms of *generalized* Perron-Frobenius results) is most unrealistic.

(2) Even if the objections of (1) can be bypassed there is a deeper question. When the order of the system is even moderately large, say 5 or 10, an agent - representative or not - is supposed to solve n^2 or $n^2 / 2$, i.e. 12 to 100 simultaneous nonlinear equations at each point in time ! Indeed matrix inversion is *not* a local operation that can be ascribed to neural architecture.

A school whose philosophy has been uncompromising in its call for rules at the level of macroeconomic policy imputes, to an agent, a computational burden without even asking whether there are neural representations consistent with the space and time constraints of decision problems. Would we, with equal nonchalance formulate production relations that violate, say, the second law of thermodynamics !

(2') Much of the enthusiasm to use the innovation term in conjunction with the Kalman gain does not face a simple - conceptual - issue : surely learning the true model must also imply learning about the Kalman gain ; learning not only by doing but by thinking ! There are, after all, bulls and bears in the system. Why ? Suppose A Bull and B Bear start off as Lucasions and compute $\hat{x}(t \mid t)$ according to (A-8) (having inverted matrices, got all the initial values precisely correct and uniform for AB and BB etc). But being A Bull and B Bear, after the very first shock, would tend to have different views (perceptions) - perhaps the 'sources' of the impulse are 'uncomputable' (in the Turing sense). Would they not try to "rationalize" their views ? How ? Surely by weighing the new information differently - i.e., updating the Kalman gain itself. Can this be formalized ? The answer is yes. Assume, (say for A Bull), after computing $\hat{x}(t \mid t)$ (s) he compares it with his 'vision' : $\hat{x}_e(t \mid t)$.

Define : $\delta \triangleq \hat{x}_e(t \mid t) - \hat{x}_e(t \mid t)$ ....(A-11)

Assume $P(t \mid t)$ is nonsingular and let $\bar{\delta}$ be the solution of

$$P(t \mid t)\,\bar{\delta} = \delta \qquad \text{.......... (A-12)}$$

(We can, of course, interpret the covariance matrix as a projection operator).

The retort of hard-core newclassicals would be based on the white Gaussian nature of the 'disturbances'; hence, if $\hat{x}(t \mid t-1)$ had been equal for A Bull and B Bear there is no rational reason for $\hat{x}(t \mid t)$ to be 'inconsistent with expectations' ! But suppose A Bull is an immigrant - new one ! Since x (o), (w (t)) and (v (t)) are assumd to be independant, how is (s) he supposed to have a view about x (o) ?

This point makes explicit yet another implicit assumption in the newclassical vision. Only by fluke will A Bull's x (o) at calendar time t equal B Bear's $\hat{x}$ (t | t - 1) which, of course, implies in general that their $\hat{x}$ (t | t)'s will be different. The essence of being 'an immigrant' - and thus having the relevant $\hat{x}$ (o) $\neq$ $\hat{x}$ (t | t - 1) is what calls for updating of the Kalman gain too. The assumptions of the newclassicals amount to stationarity in every sense of the word.

We must now show a link between δ and the Kalman gain via modifications of the covariance matrix. For illustrative porposes we show the link via the utilization of rank-one updates of the Hessian. *We conjecture that neural processes are analogous in procedure although not in the exact method.* Use δ to compute a rank-one update of P (t | t - 1) :

$$\bar{P}(t \mid t-1) = P(t \mid t-1) + \alpha P(t \mid t-1)\,\delta\ \ \delta^T\ P(t \mid t-1) \qquad \text{..........(A-13)}$$

and replace, in (A-5), (A-7), (A-8) and (A-9) P (t | t - 1) by $\bar{P}$ (t | t - 1). (α : a scalar).

We conjecture that α's role can be made explicit only in terms of neural processes - possibly encapsulating a sort of distance from stationarity. This conjecture is based on some work we are doing on neural networks as well as recent reinterpretations in filtering theory by Kailath and others (cf. Kailath, 1981 esp. Appendix 1).

Second, a few notes on the so-called separation theorem. The separation principle - well known, in one form, as the certainty equivalence principle - allows decision problems to be solved in two stages : a first estimation stage and then control. In other words within the metaphor of the 'agent as a signal processor' the idea is that the agent :

(α) 'Looks' at the observations, processes them, and produces an estimate (like Patinkin's 'utility computor' generating an excess demand function).

(β) Then decision rules are implemented as functions of the estimate of stage (α).

Is this an artefact of the analyst ? Or is it what the 'utility computor' or 'the signal precessor' actually does ? Before we comment on these questions a formalism of the problem is appropriate. Since we aim to cite results that enlarge the set of theoretical technologies currently being utilized by (newclassical) economists the formalism is in continuous time (cf. Wonham, 1968 ; Davis, 1976). The ensuing stochastic differential equations are all to be interpreted in terms of Ito equations.

Given the following linear stochastic differential equation :

$$d\,x(t) = A(t)\,x(t)\,dt + \beta(t, u(t))\,dt + G(t)\,dw(t) \quad \text{.......(A-14)}$$

with x (o) given as x_o.

The observation dynamics given as :

$$d\,y(t) = F(t)\,x(t)\,dt + H(t)\,dv(t) \quad \text{................(A-15)}$$

with y (o) given as y_o

The assumptions will be sandard : w, v being standard Wiener processes in appropriate dimensions. The difference between this formalism and the pure filter formalism is of course, the addition of the control term ß which has the effect of shifting the mean of the state vector x (t) - i.e., the conditional distribution of x(t). In this formalism the problem is literrally crying out to be separated : because we have already classified the variables into two classes. The state variables : x (t) ; and the control variables : u (t).

Given (A-14) and (A-15) the control formulation is the following. Choose a $\{u(t)\}$ as a function of the observations $\{y(s) \mid O \leq s \leq t\}$ such that :

$$J(u) = E\left\{ \int L(t, x(t), u(t))\,dt \right\} \quad \text{...............(A-16)}$$

is minimized.

L : an appropriately defined real-valued functional.

Technically, then, the statement of the separation theorem is that an optimal control exists, in a subset of the controls, which depend only on the expected value of the current state given past observations.

So far as we know every appeal to the separation principale in (new claasical) economics assumes that L must be quadratic.

But such an assumption is not necessary. Results by Wonham (op. cit) and Davis (op. cit) have been available for almost as long as the Kalman filter has been used by economists. So why, in a profession so eager to exploit advances in theoretical technologies, has this result remained unexploited. Our conjecture is the following :

(α) : Wonham's result depended crucially on assuming that the dimensions of x and y were the same. This condition seems to be crucial in showing the existence of a solution to the standard Hamilton-Jacobi-Bellman equation. This seems to be an unttractive assumption.

(ß) : Davis, using the framework of Girsanov solutions, was able to circumvent the case of dim y (t) < dim x (t) by assuming that a certain class of additional observations can be made available. This seems arbitrary.

But are these assumptions unattractive or unrealistic. They are, after all, analogous to joint cost imputation problems and joint product pricing problems when for example, fewer processes than products are activated. Surely *it is intellectually more rewarding and descriptively more accurate to investigate whether, in fact, separation is a characteristic of rational decision making ; and, if so, to study the processes that lead to separation* rather than assume, from the outset, so-called tractable formulas and then claim universal validity for such results. This is one of the reasons for us to have recast the problem of signal processing in terms of neural architecture in finite automata in §3.

Finally to the question of decision rules. There is some ambiguity in the newclassical use of this phrase. For example in a stimulating article a few years ago Lucas observes that :

> "In general terms, we view or *model an individual as a collection of decision rules* (rules that dictate the action to be taken in given situations) *and a set of*

preferences used to evaluate the outcomes arising from particular situation-action combinations. These decision rules are continuously under review and revision, new decision rules tried and tested against experience, and rules that produce desirable outcomes supplant those that do not. I use the term 'adaptive' to refer to this trial - and - error process through which our modes of behaviour are determined".

(Lucas, 1986, p. S. 401 ; italics added)

and,

"Technically, I think of economics as studying decision rules that are steady states of some adaptive process, decision rules that are found to work over a range of situations and hence are no longer revised appreciably as more experience accumulates".

(Lucas, ibid, p. S. 402)

Are we, then, to specify REM as :

$$\text{REM} \;\Delta\; <\Omega, \leq, e> \qquad \text{....................(A-17)}$$

where : Ω : decision rules

If so is the agent subject to Church's thesis ? If not, why not ? And, in any case, the remarks do not square-up well with standard newclassical practice (cf. Sargent, 1982, 1987 and Plosser, 1989) (13). The standard version has the following broad structure (14).

Preferences

$$U_t = \sum_{s=0}^{\alpha} \beta_{t+s} \, U\,(C_{t+s}\,,\,L_{t+s}) \qquad \text{..................(A-18)}$$

for an economy populated by identical agents who live forever (cf. our remarks when discussing invariant Kalman gains).

C_t: level of consumption in the single produced good.

L_t : level of 'consumed' leisure.

and U (.) is assumed to have all the 'nice' properties.

Production : $Y_t = \theta_t \ F(K_t, N_t)$ ....(A-19)

Where F is CRTS and Y_t is the single final good.

and K_t : capital stock chosen at t - 1.

N_t : labour input in period t.

θ_t : shift factor for altering total factor productivity (dropped by helicopters from Minnesota and Chicago)

Clearly F is supposed to satisfy standard "Inada conditions".

Capital Accumulation : $K_{t+1} = (1 - \delta) \ K_t + I_t$ ....(A-20)

where I_t : given investment

and δ : given depreciation rate.

Resource constraints : $L_t + N_t < 1$ ....(A-21)

and $C_t + I_t < Y_t$ ....(A-22)

and L_t, N_t, C_t, K_t : non negative

Thus the newclassical Crusoe optimizes :

$$L = \sum_{t=0}^{\infty} .\beta^t \ \{U(C_t, 1 - N_t)\} + \Sigma \lambda_t \{\theta_t F(K_t, N_t) - C$$

$$- K_{t+1} + (1 - \delta) K_t\} \quad \text{..........(A-23)}$$

The first order necessary conditions are, then :

$U_1(C_t, 1 - N_t) - \lambda_t = 0$ ....(A-24)

$U_2(C_t, 1 - N_t) - \lambda_t \ \theta_t \ F_2(K_t, N_t) = 0$ ....(A-25)

$\beta \lambda_{t+1} \{\theta_{t+1} (K_{t+1}, N_{t+1}) + (1 - \delta)\} - \lambda_t = 0$ ....(A-26)

$$\theta_t \ F(K_t, N_t) + (1 - \delta) K_t - K_{t+1} - C_t = 0 \qquad \text{..........(A-27)}$$

We shall not state any of the more obvious implications and assumptions. Our purpose does not quite require much more than the above. Now (A-24) - (A-27) are nonlinear difference equations. If we have learnt anything then surely it is that even simple nonlinear difference equations have deceptively complex dynamic behaviour. In particular whether steady states (limit points) completely characterize the limit set is an alomst unanswerable question for some of the simplest equations in one and two dimensions. It is, therefore, with astonishment that we read the following in Plosser :

> "...it is usually impossible to solve this maximum problem analytically for the optimal decision rules of Robinson Crusoe. Consequently, real business cycle research finds it necessary to compute approximate solutions to Robinson Crusoe's choice problem. These approximation procedures typically result in decision rules that are linear in K_t and θ's"....
>
> "The first step in approximation procedure is to *choose a point to approximate around. The natural choice is the stationary point or steady state* ... The second step is to express the four first-order conditions in terms of the percentage deviations from the stationary values...and *then take a linear approximation* to each condition. This results in a set of linear difference equations in percentage deviations from the steady state".
>
> (Plosser, ibid, p. 74 ; italics added)

What if, as is most likely, the equations possess limit sets that are strange attractors. Would it not have been much simpler to postulate linear decision rules (sic !) from the outset and use them to account for the dynamics of key aggregate variables ? The newclassical answer would be that their procedure enables the analyst to relate parameters to meaningful individual specifications !

We think the time has come to study these nonlinear dynamics directly without paying too much emphasis on closed form solutions, steady states and the like. With this in mind the following remarks are made.

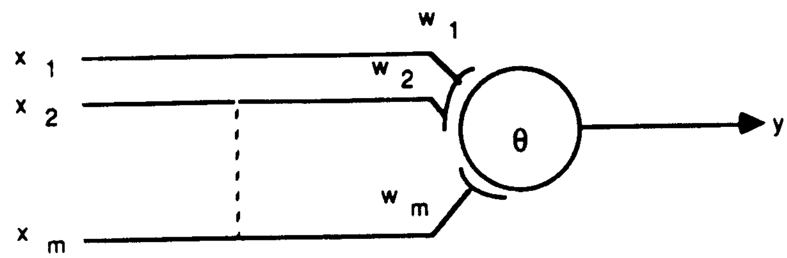
Since the production function satisfies the usual 'Inada conditions' and the utility function likewise U_1, U_2, and F_1 can be evaluated. Whatever the functional forms of λ_t and λ_{t+1}they can be solved in terms of C_t and N_t . By assumption 'K_t is the predetermined capital stock (chosen at time t - 1)' (Plosser, op. cit, p.72). Thus from (A-25) N_t can be represented in terms of C_t and parametrized by θ_t (and K_t, if necessary). Semantics aside, if K_{t+1} is chosen at time t, there will be little difficulty in solving C_t via (A-27). Even if not 'semantics aside' it is easy to see (A-24) and (A-25) can be used to solve for C_t in terms of N_t and parametrized by θ_t (and K_t, if necessary). Then (A-26) and (A-27) can either be viewed as a system of first order difference equations in K_t and N_t or, if K_{t+1} is assumed to be given at t, one first order nonlinear difference equation in N_t. Surely numerical explorations of the nature of the dynamicals of this equation should first be attempted before the calisthenics of steady-states, linearizations etc., are even contemplated. Such an approach requires an attitude free of the standard dogmas.

We can return to the point about decision rules now. Clearly the derivation of decision rules from utility functions and constraints is one thing ; to consider them to be one of the primitives is quite another thing. That the latter is the more fruitful strategy from aesthetic and realistic points of view has been discussed quite exhaustively in Rustem and Velupillai (1990).

APPENDIX 2

Elementary Notes on Neural Nets

Figure A-1.



A Mc Culloch - Pitts Neuron (M - P neurons)

M - P neurons are characterized by :

(i) m + 1 numbers : inputs --> $x_1, x_2, \ldots, x_m$

output --> y

(ii) thereshold number : θ

(iii) 'synaptic' weights : $w_1, w_2, \ldots, w_m$

(where w_i is associated with x_i)

The refractory period is taken as the unit of time.

Assumption : $y(n+1) = 1$ iff $\Sigma \omega_i \; x_i(n) \geq \theta$

i.e., the neuron fires an impulse along its axon at time n + 1 if the weighted sum of its inputs at time n exceeds the thereshold of the neuron.

$w_i > 0$ --> excitatory synapse

$w_i \leq 0$ --> inhibitory synapse

Definition : Neural net

A *Neural net* is a collection of M - P neurons, each with the same refractory period, interconnected by splitting the output of any neuron ('axon to the dendrites') into a number of lines and connecting some or all of these to the imputs of other neurons.

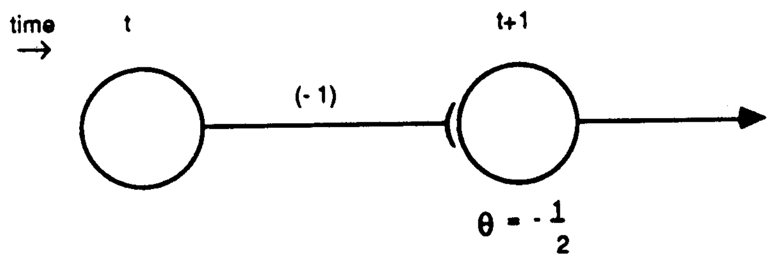
Proposition :

Any Neural Net is a Finite Automaton.

Three simple examples of how neural nets evaluate Boolean propositions prepares the groundwork for the main, universal, example of the text.

(α) *Negation*

Figure A-2.



(i) The left neuron emits a 'spike' at time t if some proposition A is *true*.

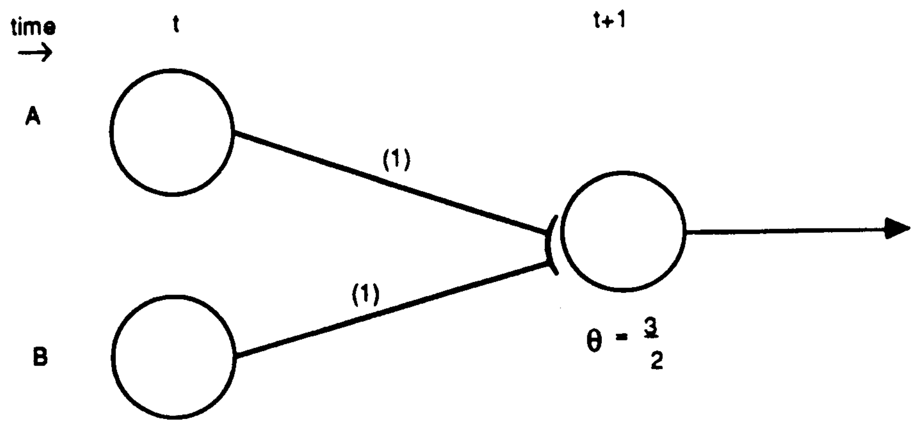
(ii) If the transmission channel has weight, w, (-1), and the right neuron has the theresold $\theta = -\frac{1}{2}$,

then, at time t + 1, the right neuron will not 'spike' : i.e., A --> ~ A

(iii) Conversely, when A is *false* : ~ A --> A

(β) *Conjunction*

Figure A-3.



The right neuron will spike at t + 1 iff *both* neurons on the left spiked at time t indicating that the propositions tested by these two neurons have both been true. Thus :

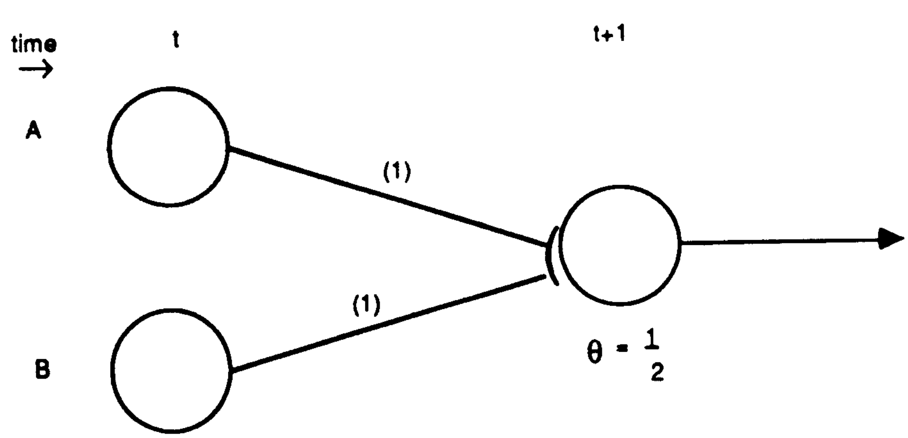
$$Aw_1 + Bw_2 = 1 \times 1 + 1 \times 1 = 2 > \theta = \frac{3}{2}$$

when A and B are true. (Note the danger of diophantine formalisms). If, on the other hand, A is true and B is false :

$$1 \times 1 + O \times 1 = 1 < \theta = \frac{3}{2}$$

(γ) *The Inclusive OR*

Figure A-4.



If at least one of the left neurons spike at time t then the right neuron will spike at t + 1.Thus, if A is true (1) and B is false (0) we have :

$$1 \times 1 + O \times 1 = 1 > \theta = \frac{1}{2}$$

Nothing more than a collection of these, and equally simple - simple-minded !! - elemental units are the basis for a neural network architecture to compute anything that a Turing machine can compute

REFERENCES

ARBIB, M.A., (1987) : *Brains, Machines and Mathematics,* Second edition - Springer - Verlag, New-York and Berlin

CUTHBERTSON, Keith (1988) : 'Expectations, Learning and the Kalman Filter' *The Manchester School of Economic and Social Studies Vol. LVI Number 3 September pp. 223 - 246*

DAVIS, M.H.A., (1976) : *The Separation Principle in Stochastic Control via Girsanov Solutions* SIAM J. of Control and Optimization - Vol. 14, N°1, January, pp. 176 - 188

FITOUSSI, J.P. (1983) : "Modern Macroeconomic Theory : An Overview" in : J.P. Fitoussi (ed) : *Modern Macroeconomic Theory* - Basil Blackwell, Oxford

FITOUSSI, J.P.and VELUPILLAI, K. (1987) : "Studies in Business - Cycle Theory : A Review Essay" *Journal of Money, Credit and Banking* - Vol. 19, N°1, February, pp. 122 - 130

HICKS, J.R. (1937) : *"Mr Keynes and the Classics : A suggested Interpretation"*- Econometrica, Vol. V, pp. 147 - 159

HILBERT, D. (1902) : "Mathematical Problems"*Bulletin of the American Mathematical Society*
Vol. 8, July pp. 437 - 479

KAILATH, T. (1981) : *Lectures on Wiener and Kalman Filtering* - Springer-Verlag, New-York and Berlin

LINDAHL, E. (1939) : *Studies in the Theory of Money and Capital* - George Allen and Union Ltd., London

LUCAS, R.E. Jr. (1981) : *Studies in Business Cycle Theory* - The MIT Press, Cambridge, Mass

LUCAS, R.E. Jr. (1986) : *"Adaptive Behaviour and Economic Theory"* - Journal of Business - Vol. 59, pp. S 401 - S 426

LUCAS, R.E. Jr. (1987) : *Models of Business Cycles* - Basil Blackwell, Oxford

LUCAS, R.E. Jr. and STOKEY, N.L.
with Edward PRESCOTT (1989) : *Recursive Methods in Economic Dynamics* Harvard University Press, Cambridge, Mass

Mc CALL, J.J. (1989) : *"The 'Smithian' self and its 'Bayesian' Brain"* - Mimeo., UCLA, February (Revised Version Forthcoming)

Mc CALL, J.J. and VELUPILLAI, K. (1990) : *"Constructive Foundations for Economics : The Emperor's Old Clothes"* Mimeo., UCLA, April (Revised Version ForthComing)

Mc CULLOCH, W.S. and PITTS, W. (1943) : "A Logical Calculus of the Ideas Immanent in Nervous Activity" - *Bulleting of Mathematical Biophysics,* Vol. 5, pp. 115 - 133

METZLER, L.A. (1951) : "Wealth Saving and the Rate of Interest" - *Journal of Political Economy*, Vol. LIX, pp. 93 - 116

PATINKIN, D. (1965) : *Money, Interest and Prices : An Integration of Monetary and Value Theory* Second edition Harper and Row, New-York

PLOSSER, Charles, I. (1989) : "Understanding Real Business Cycles" *Journal of Economic Perspectives*, Volume 3, No 3, pp. 51 - 77

RUSTEM, B and VELUPILLAI, K. (1990) : "Rationality, Computability and Complexity" *Journal of Economic Dynamics and Control* - Vol. 14, pp. 419 - 432

SAMUELSON, P.A. (1974) : "Remembrances of Frisch" *European Economic Review* - Vol. 5, pp. 7 - 23

SARGENT, T.J., (1982) : "Beyond Demand and Supply Curves in Macroeconomics" *American Economic Review : Papers and Proceedings* - Vol. 72, N° 2, May ; pp. 382 - 389

SARGENT, T.J., (1987) : *"Dynamic Macroeconomic Theory"* Harvard University Press, Cambridge Massachusetts

WONHAM, W.M., (1968) : "On the Separation Theorem of Stochastic Control" *SIAM Journal of Control* - Vol. 6, N° 2, pp. 312 - 326