Lucas and the Austrians

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Review essay on Rudy van Zijp, *Austrian and New Classical Business Cycle Theories: A Comparative Study Through the Method of Rational Reconstruction*, Aldershot: Edward Elgar, 1993. 272 pp., index. Hb. £39.95 . ISBN: 1 85278 6744.

Though few would go as far as David Laidler -- who once argued that Robert Lucas, Thomas Sargent and company should properly be called 'neo-Austrians' -- many writers have seen some degree of parallelism between the Austrian trade cycle theory of Mises and Hayek, and the New Classical macroeconomics. Dr van Zijp offers a critical examination of this relationship. Taking the lead of Karl Popper, he proposes that we focus not on the respective 'theories' as such, *in abstracto*, but on the respective *problems* to which these theories were addressed. Following an introductory chapter in which this general approach is set out, the book contains five chapters on the Austrians, emphasising the development of what van Zijp calls 'the Hayek Problem' and 'the Hayek programme' for its solution, then four chapters on the New Classicals, with a parallel emphasis on 'the Lucas Problem' and 'the Lucas programme'. Two concluding chapters then set out a comparison between these problems and programmes. The gist of the author's conclusion is that while there are important resemblances between their respective theories, nonetheless "the Austrian School and the New Classical Economics study different problems" (p.236) and employ substantially different (if overlapping) methodologies, so that the degree of commonality between the two programmes should not be overstated.

Dr van Zijp's discussion is broad-ranging and stimulating, but unsatisfactory in several ways. In elaborating on the points of interest and the problems, I shall shadow the structure of the book as outlined above. But first a brief comment on the language of the study: professional methodologists may applaud, but most practising economists are likely to be irritated by, the profusion of 'methodologese' in van Zijp's text. One is not surprised by this sort of thing in a discussion of the Austrians, themselves much addicted to methodology, but it seems jarring when applied to the sober, workaday New Classicals. For example, the perfectly standard procedure of setting up an empirical test so as to see whether the data favour one theory over a specific rival -- rather than aiming for an accept/reject decision on a single theory in isolation -- is routinely and otiosely referred to as "adopting a sophisticated-falsificationist methodology". And the procedure of building an initial model based on the assumptions of perfect rationality and full information -- the stock-in-trade of neoclassical economics long before Popper dubbed it "the method of rational model-construction" -- is treated as if it were somehow Sir Karl's gift to Lucas and Sargent. The text is also peppered with prim Popperianisms such as this: "The adoption of the immunizing strategies [read: adhering to a theory when the available evidence runs against it] raises the question whether [Lucas's and Sargent's] 1973 tests were indeed intended to falsify the joint [Natural Rate/Rational Expectations] hypothesis, or whether they 'merely' provided attempts to corroborate it" (147). Why "indeed"? Who ever thought that economists were going to discard a cherished new theory on the strength of a few unpromising regressions?

Philosophy and methodology: general points

As mentioned above, van Zijp starts out with a discussion of Karl Popper's views on the proper way to understand theories. This discussion is based on the essay "On the Theory of the Objective Mind",[1] in which Popper analyses scientific development as a dialectical process. An initial problem situation gives rise to a tentative theory or conjectured solution, which is then subject to critical examination. This then leads to a reformulation of the problem situation, or 'problem shift', which in turn forms the starting point of a further cycle. In the light of this schema, Popper proposes to replace the question 'How can we understand a scientific *theory*?' with the alternative formulation, 'How can we understand a scientific *problem*?'. The latter question, Popper claims, is in general more difficult, more interesting, and also more fundamental, since "science starts from problems" (*op. cit.*, 181).

Thus far, Van Zijp's exposition is both clear and to the point: he proposes to apply this Popperian methodology to characterise the Austrian and New Classical cycle theories in terms of the specific 'problem situations' which called them forth as conjectured solutions. But then things start to go wrong.

The first problem is relatively minor, namely van Zijp's referring to Popper's method as that of Verstehen (p. 4). Since 'verstehen' is German for 'to understand', and Popper is undoubtedly focussed on the question of understanding, this is literally unobjectionable, but it is liable to mislead. Within the philosophy of the social sciences, *Verstehen* connotes the method of the imaginative recreation of another's subjective world, yet Popper is quite emphatic that this is *not* what he intends. In the essay under discussion, he distinguishes what he calls three "worlds": the first is "the physical world or the world of physical states; the second is the mental world or the world of mental states; and the third is the world of intelligibles, or of ideas in the objective sense..." (*op. cit.*, 154). Popper then explicitly claims superiority for his own "third-world method of critically reconstructing problem situations over the second-world method of intuitively re-living some personal experience (a method whose value, limited and subjective yet at the same time indispensably suggestive, I do not wish to reject entirely)" (*op. cit.*, 170 n18). Popper's method is not that of (subjective) *Verstehen* as it is commonly understood.

A more substantial (double) problem emerges when the author floats a possible objection to his own use of the Popperian method of critical reconstruction. Van Zijp suggests that the authorised use of this method is in appraising scientific *progress* (defined in terms of increasing non-refuted empirical content), while he intends to use the method "merely to understand the Austrian and New Classical research traditions" (5) without attempting to appraise their progressiveness. "This may lead," van Zijp observes, "to the criticism that [his book] uses a method for a different purpose than that for which it was originally intended" (*ibid.*). He attempts to deflect this hypothetical criticism with the claim that the (broadly empiricist) Popperian criterion for scientific progressiveness is explicitly rejected by the Austrians,[2] for the case of economics, so that an appraisal in such terms would "distort" the Austrian tradition and "reduce our understanding of it" (6).

Van Zijp is conflating two strands in Popper's work, and the self-criticism that he moots is not valid as stated. Certainly, Popper is concerned with appraising scientific theories in terms of their empirical 'truth content' (an idea spelled out in the second essay in his *Objective Knowledge*). But this project is logically distinct from the project of historical understanding, via the critical reconstruction of theories as responses to specific problem situations. Popper argues that the latter method is a very general one, just as suitable for understanding theories that are now considered decisively falsified as those that are still accepted. Indeed, he suggests that it is equally appropriate for understanding elements of the "third world" of objectivised ideas that lie outside of the scientific domain -- for instance in the history of art (op. cit., 180) -- where the question of increasing empirical truth content does not arise.

Nonetheless, having himself raised the matter of Popper's views on the appraisal of scientific progress, van Zijp's dismissal of these views as irrelevant to the case in hand is surely too cavalier. Two possibilities suggest themselves. (1) Popper is right in his appraisal criterion, from which it follows that the claims van Zijp cites on behalf of Austrian economics are just special pleading; or (2) the Austrians are right about economics, which therefore constitutes a counter-example to Popper's theory of scientific progress. (Alternatively, of course, it may be that *both* Popper and the Austrian economists are wrong.) One cannot slide out of the controversy by saying that Popper is right for the most part, but his ideas just don't happen to apply to economics. Popper's claim is *essentially* a general one; and given one counter-example (i.e., a domain in which scientific progressiveness *cannot* be evaluated in terms of increasing non-refuted empirical content), the theory falls.

Besides, van Zijp's claim to eschew appraisal is a curiously self-denying ordinance. He doesn't rigorously abide by it; but to the extent he does, the effect is to reinforce by default the appraisal of the various theories that is offered by their own authors. One has the impression, at various points in the book, of patently unsatisfactory theories being treated with kid gloves.

The Austrians

Van Zijp's summary, in chapters 2 to 6, of the development of the Austrian school -- from Menger and Böhm-Bawerk, through Mises and Hayek, to Lachmann, Kirzner and the 'Austrian revival' of the 1970s -- is perhaps one of the most useful parts of the book. He gives a clear account of both the common thread in this tradition, and the various differences of emphasis and opinion amongst its adherents, which can be quite confusing to the non-initiate.

In relation to the thesis of the book, however, van Zijp's main aim is to isolate a definite 'Hayek Problem' and corresponding 'Hayek programme', to be compared and contrasted with that of the New Classicals, and in this I think he is much less successful. My main concern is that van Zijp does not pay sufficient attention to the various shifts of problem and position during Hayek's intellectual career. He does, of course, recognise and comment upon the most obvious shift, namely Hayek's turn away from economic theory as such and towards political and social theory in the postwar period, but there is more to it than that. It is noteworthy that van Zijp makes no reference to Terence Hutchison's distinction between 'Hayek I' and 'Hayek II', with a break around 1937. Hutchison's two Hayeks are distinguished on the basis of certain "vital and critical" changes in view on "some quite fundamental and very important points of methodology and philosophy... as also, incidentally, on money and on some issues of employment policy...".[3] If there is any validity in Hutchison's contention, this must throw some doubt on the project of identifying a unique 'Hayek Problem' for the purpose of comparison with the New Classicals. At any rate, one will clearly have to be very careful in identifying such a problem.

In the present context, the most relevant element of Hayek's views to be tested for possible discontinuity over time is probably his attitude towards (general) equilibrium theory, since van Zijp uses equilibrium theory as a lever to pry apart the Austrians and the New Classicals. For the New Classicals, according to van Zijp, "The general equilibrium construct is a heuristic principle, and not a situation to be explained" (214), i.e., the coordinating role of the price system is taken for granted. Lucasian business cycle theory poses itself the problem of how this presumed coordination can be disturbed or distorted by monetary changes. In the Austrian corner, by contrast, the defining 'Hayek Problem' supposedly concerns the *process by which* coordination is achieved. "The central problem in the Hayek programme is the Hayek Problem which concerns the question of how coordination can be achieved in market economies", so that "the attempt to solve the Hayek Problem forms a quest for discoordination dynamics, which explains a situation of coordination as the end result of a sequence of discoordination situations" (213; see also 56).

Two things may be said about this claim.[4] First, even if the above is a valid characterisation of the concerns active in Hayek's later economic writings, it is not at all clear that it accurately represents the problem to which his cycle theory was addressed. Some basic chronology bears emphasising: Hayek's cycle theory was developed in the late 1920s and early 30s (with a footnote being added in the form of his discussions of the 'Ricardo effect' in 1939 and 1942). *Monetary Theory and the Trade Cycle*[5] and *Prices and Production*[6] present the core of this theory. Of these two complementary books (as Hayek described them in the Preface to the English edition of the former), *Monetary Theory* is the more expansive on methodological questions. How do we find Hayek characterising his own project therein?: "It is the task of Trade Cycle theory to show under what conditions a break may occur in that tendency towards equilibrium which is described in pure analysis -- i.e. why prices, in contradiction to the conclusions of the static theory, do not bring about such changes in the quantities produced as would correspond to a new equilibrium situation".[7]

Elsewhere, what Hayek calls "pure analysis" or "static theory" in the above quotation is identified as the moneyless general equilibrium theory of the Lausanne school. This theory is commended by Hayek as "the basis of all theoretical economics"; and in turn the "essential means of explanation" in such theory is "the assumption that prices supply an automatic mechanism for equilibrating supply and demand."[8] The message is clear: in this work, Hayek does not make a problem of the coordinating tendency of the price system. On the contrary, like Lucas, he takes that tendency for granted and asks how it can be *disturbed* by monetary factors. I don't mean to imply that there are no important differences between the Hayek of *Monetary Theory* and Robert Lucas; I am just saying that in one important respect van Zijp's attempt to identify these differences misfires badly. And in this light van Zijp's comment that "Hayek's business cycle theory could not clearly explain why there is a tendency towards coordination" (68-9) can be seen as wide of the mark: Hayek's cycle theory was not *intended* to explain any such thing.[9]

My second objection to van Zijp's characterisation of the 'Hayek Problem' is perhaps of less relevance to the comparison of the Austrians and the New Classicals, but is nonetheless germane to a good deal of his discussion of Hayek's work, and may be of some interest in its own right. Let us reconsider the assumption, hypothetically granted above, that van Zijp's 'Hayek Problem' constitutes an accurate specification of the concerns of the *later* Hayek.

The problem of explaining how, and under what conditions, movement towards equilibrium (or a coordinated state) is *possible*, is indeed prominent in Hayek's 1937 paper "Economics and Knowledge".[10] And one might suppose that this paper adumbrated a research programme. Van Zijp certainly supposes so; but then, considered in this light, he finds Hayek's subsequent work strangely unproductive. He says of Hayek in 1939, that "although he explained the importance of expectations, he did not explicate how agents learn, nor did he give an insight into how they form their expectations. He merely presupposed that economic agents will learn the 'correct' state of the system" (56). That is, Hayek had failed to produce a solution to the 'Hayek Problem'. In 1946, we find Hayek "still unable to answer the question how [a tendency towards coordination] comes about" (67). Indeed, it is finally admitted that "Hayek would not solve the Hayek Problem" (68). He simply gave up on it, and took up political philosophy instead.

What are we to make of this? One possibility is that Hayek bit off more than he could chew: he simply did not have the resources with which to tackle the problem he had framed in 1937. I believe, however, that another factor was at play -- another 'Hayek problem' was *interfering* with the one van Zijp isolates. To understand this, one has to make reference to the socialist calculation debate. Van Zijp quite deliberately sets this topic aside as beyond his purview; but while one appreciates his desire to carve out a manageable field of enquiry, the later Hayek without the socialist calculation debate is truly *Hamlet* without the Prince.[11] Thus, although Hayek, in 1937, expressed puzzlement over precisely *how* the market achieves coordination of economic activities, and argued that standard equilibrium theory did not shed much light on the matter, he had no doubt at all *that* the market produces coordination, of an effectiveness that no socialist planning board could ever hope to emulate. And a good deal of his work in the period following 1935 was devoted to buttressing the latter claim.

Consider, for example, Hayek's 1945 article "The Use of Knowledge in Society"[12] as reasonably representative, we find that Hayek is indeed concerned with the issue of coordination. As he puts it: "The peculiar character of the problem of a rational economic order is determined precisely by the fact that the knowledge of the circumstances of which we must make use never exists in concentrated or integrated form, but solely as the dispersed bits of incomplete and frequently contradictory knowledge which all the separate individuals possess". (519) The true economic problem is therefore "how to secure the best use of resources known to any of the members of society, for ends whose relative importance only these individuals know" (520).

But the 'coordination problem' is here presented as a *practical* issue: Hayek wants to know which system is best at achieving the coordinated use of dispersed information, central planning or the market (although of course he knows the answer already). In this context, the target of his criticism is not general equilibrium (GE) theory as such, considered as a theoretical exercise elucidating the optimality properties of market equilibrium. Rather, the monster Hayek must fight to the death is the *socialist appropriation* of GE. The key point Hayek hammers at is that the elements taken as data from the standpoint of GE theory -- preference functions, technical production possibilities -- may be hypothetically taken as given only for narrow theoretical purposes. They can never *actually* be 'given' to a planning board. Or in other words, the equations to which the GE price vector constitutes the solution can never actually be written down in concrete form.

Instead, Hayek analyses the performance of the market in terms of what one might call a 'price-plan dialectic'. Individual plans are made and adjusted on the basis of both specific, local information (of a sort which Hayek supposes cannot possibly be transmitted directly to a central planning board) and market prices. Market prices are in turn forced to adjust to reconcile the market supplies and demands which emerge from the aggregation of individual plans. Market prices therefore transmit to each individual all he needs to know of (i.e., the relevant summation of) the local knowledge possessed by all other individuals. This price-plan dialectic is a real-time process that cannot, Hayek argues, be short-circuited by the prior collection of all information for central processing.

It should be particularly noted, however, that the continual *convergence* of the price-plan dialectic towards optimality is taken for granted by Hayek. And this is not just an oversight, since making a problem of the convergence of the market process would undercut the primary polemical thrust of his argument. I think it fair to say that the *theoretical* 'coordination problem' of 1937 is *suppressed* in 1945, in the interest of making the most effective polemical case against socialism. This is certainly suggested by Hayek's discussion of the example of the tin market (526-7): his argument here -- that the information conveyed by prices is fully adequate to produce a coordinated response to a change in the 'data' -- would seem distinctly question-begging if held to the standard of his 1937 paper.

My perspective on the various problems governing Hayek's work from the 1920s to the 1940s is therefore as follows. The Hayek problem of *Monetary Theory* and *Prices and Production* (Problem I) was to explain how the strong, automatic equilibrating tendencies of the market could somehow be disrupted, giving rise to the business cycle -- and Hayek thought he had found the answer in an elastic supply of credit money, capable of subverting the 'natural' equilibrium of real saving and investment. Around the mid-1930s, however, two further problems emerged. Problem IIA, if you will, which was set out in "Economics and Knowledge", was more or less what van Zijp describes as *the* Hayek Problem while Problem IIB was to demonstrate the overwhelming superiority of the decentralized market mechanism over socialist planning. And Problem IIB overshadowed IIA.

Returning to van Zijp's statement that "Hayek would not solve the Hayek Problem", the principle of charity suggests that if one finds a certain author repeatedly failing to make any headway on what one supposes to be his central problem, one ought to consider the possibility that one has misidentified the problem. In the present case, it might be a more legitimate criticism of Hayek to say that he *should* have seen a theoretical problem behind the claim that the price system produces a natural tendency towards economic coordination, but either failed or refused to do so, rather than saying that he worked on this problem for years with nothing to show for it.

The New Classicals

Van Zijp's account of the development of New Classical economics -- in its first form of Robert Lucas's monetary theory of the cycle, at any rate -- is for the most part competent and workmanlike. It contains, however, little that is original or remarkable in any way. Good surveys of New Classical economics have been available for many years now.[13] The one aspect of van Zijp's narrative for which originality might be claimed is his lengthy discussion of Thomas Sargent's early work on the Gibson Paradox and the Fisher effect, which van Zijp describes as part of the 'pre-history' of New Classicism. It is particularly disappointing, therefore, to find that this section is not altogether reliable. One instance: Van Zijp states that in 1969,[14] after apparently confirming Fisher's original finding of extremely long lags in the formation of inflation expectations, Sargent explained these lags "in terms of what he called the *extrapolative effect*, which holds that an increase (decrease) in the general price level induces economic agents to expect prices to rise (fall) still further. This hampers the adjustment process, lengthening the transition period" (139). This is quite garbled. The 'extrapolative effect' is the basic mechanism of expectations-formation hypothesised by Fisher himself. If inflation rises from some base rate and then stays at some new rate, the full Fisher effect on the nominal interest rate is realised only once people come to extrapolate the new inflation rate into the future. It is therefore the alleged sluggishness of the extrapolation, not its *existence*, that makes for a long 'transition period'. Sargent's innovation in his 1969 paper was to estimate a short-run 'regressive effect', whereby an observed rise in prices apparently led to people to expect a subsequent *fall*, alongside the standard Fisherian extrapolative effect (by use of a second order rational lag scheme).

I also think that van Zijp misses a trick in his discussion of Sargent. What is particularly striking about the latter's 1972 and 1973 Gibson Paradox papers[15] is the combination of (a) the presumption of optimality in expectations-formation and (b) the use of a Keynesian macromodel including the Keynesian interpretation of the Phillips curve (whereby price movements are the result rather than the cause of deviations of output from its full-employment level -- a view already reversed by Friedman in 1968). Van Zijp says that it's "interesting to note" that Sargent made no reference to Lucas's work in his pre-1973 papers; I find it more interesting that Sargent was briefly, in terms of his substantive analysis, a sort of 'rational-expectations Keynesian'.

One might have expected that a book published in 1993 would differ from earlier surveys of New Classical economics by devoting more attention to the Real Business Cycle theory, since this has been the locus of the 'action' among the New Classicals for several years now, while the original Lucasian monetary cycle theory has fallen into disfavour. But although van Zijp is aware of this shift, he remains focused on the earlier New Classical writings. (In fact, Hoover had a good deal more to say about Real Business Cycle theory in his 1988 book.) This leads to a curious asymmetry. On the Austrian side of his comparison, van Zijp considers a century of work, and by no means confines himself to business cycle theory proper, while on the New Classical side he focuses exclusively on a single decade. The book might more aptly be titled "Austrian Economics and the New Classical Business Cycle Theory of the 1970s" -- still an interesting conjunction of topics, but not exactly what is advertised.

Anyway, what is the 'Lucas Problem' that van Zijp extracts for the purposes of comparison with the Austrians? He identifies a broad and a narrow version. The broad problem is "to explain cyclical fluctuations in economic activity as the 'unintended outcomes' of rational behaviour" (163). At this level of generality, the Lucas Problem seems the same as that of Hayek's cycle theory; it might even be said to fit Keynes's work. But the characteristic twin New Classical assumptions, Rational Expectations and continuous market-clearing, narrow the field considerably. The "Lucas Problem in a narrow sense" is that of explaining fluctuations in output and employment "as the result of unanticipated random expectational errors which are generated by a lack of information about the current values of the relevant global variables" (ibid.). Van Zijp suggests that in his various papers of the 1970s Lucas solved this problem (in outline at least) to his own satisfaction, first by developing the Lucas supply curve on the basis of the signal extraction problem faced by fully rational agents operating in an economy subject to unobservable money-supply shocks, and then by setting out a propagation mechanism whereby uncorrelated shocks might generate a serially correlated business cycle. Fair enough, although of course many Keynesians (and old-style monetarists) found Lucas's constructions far from satisfactory, and they would also be found wanting by the next generation of New Classicals.

Comparison of the Austrian and New Classical theories

The centrepiece of van Zijp's comparison of the Austrian and New Classical cycle theories is the confrontation of the 'Hayek Problem' and the (narrow) 'Lucas Problem'. If I am right in saying that van Zijp's 'Hayek Problem' misrepresents the concerns of Hayek's cycle theory, this is bound to have serious repercussions when it comes to making the comparison. Nonetheless, the specific points made in van Zijp's closing chapters merit attention in their own right. As I have indicated above, I am not unsympathetic to his claim that there are important differences between the two theories; the question is whether van Zijp has put his finger on the true *differentiae*.

As van Zijp correctly notes, Hayek and Lucas share a commitment to the idea of rationality in human action, and also to methodological individualism. More specifically, they share the idea that the business cycle should be analysed as the consequence of *misperceptions* on the part of imperfectly informed agents. Beyond this, however, van Zijp (211 ff.) cites several grounds for distinguishing their theories, among which are (1) that while Lucas analyses the cycle in terms of a sequence of Rational Expectations *equilibria*, Hayek's theory allows for *disequilibrium* states or 'coordination failures'; (2) that Lucas's cycle is triggered by an exogenous shock, whereas Hayek's has an endogenous dynamic; and (3) that while Lucas subcribes to the 'homogeneity postulate' -- his decision-makers are typically 'representative agents' and his capital is homogeneous -- Hayek stresses the importance of the heterogeneity of agents' knowledge as well as the heterogeneity of capital goods. Closely related to the last point is (4) the sharp difference in the respective views of Hayek and Lucas on the possibility of producing a formal, mathematical model of the cycle mechanism.

I shall comment briefly on these points in turn. First, one has to be rather careful in the use of the terms 'equilibrium' and 'disequilibrium'. Van Zijp cites (but disgrees with) the argument of J. Schiede that the difference between Hayek and Lucas in this regard is primarily semantic: what Hayek described as a case of disequilibrium would be subsumed under the heading of rational expectations equilibrium by modern writers. This view has something going for it, and I'm not sure that van Zijp answers it adequately. By 'equilibrium', Lucas means that agents are optimising fully under the constraints they face (including, perhaps, the absence of certain relevant information) while all markets are clearing. In such a state the agents may, however, be working on the basis of incorrect and unsustainable expectations, and once the correct information becomes available they may, with hindsight, perceive their actions as a mistake. Further, if the plans they have made on the basis of incorrect expectations are not quickly reversible (e.g. in the case of capital investments), the agents may, even after they realise their error, find themselves perforce doing things they would not have chosen to do had their expectations been correct at the outset. (This is the essence of the Lucas propagation mechanism, discussed by van Zijp on pp. 156-9.) Now the question is, if all this is subsumed under 'equilibrium', does Hayek's cycle feature 'disequilibrium' in some deeper sense?

Hayek's cycle involves what he calls 'forced saving': intended investment exceeds the aggregate real saving plans of consumers, with the gap being bridged by new credit-creation on the part of the banking system. This may be said to be a more 'discoordinated' state than the sort of imperfect-foresight equilibrium envisaged by Lucas. Interestingly, though, it doesn't necessarily mean that any actual market fails to clear (since there is no actual *market* on which saving and investment confront each other as such). Thus, although I agree with van Zijp that Hayek's economy gets more deeply discoordinated than Lucas's, I don't think he's got to the heart of the matter when he says (a little groggily) "Austrians do not assume that markets and prices clear continuously and instantaneously" (215). I don't see failure of market-clearing per se as crucial to Hayek's cycle theory (and besides, if Lucas's feet were held to the fire, surely he'd relent on the "instantaneously").

This point -- that coordination failure *need* not involve non-market-clearing -- may be reinforced by a simple microeconomic example. Suppose that numerous chip-makers all install new productive capacity in the expectation of a strong demand for memory chips, but without full knowledge of the scale of each other's investments. Then it turns out that they've overdone it: the market is over-supplied and prices fall to unprofitable levels. Firm A's plan to make large profits out of the anticipated surge in demand was inconsistent with the similar plans of firms B, C, etc. In common speech we'd likely call this a case of coordination failure (the chip-makers failed to coordinate their investment plans). But although it is clearly out of Marshallian long-run equilibrium, the chip market may nonetheless *clear* throughout -- at a price that differs from what firms were expecting.

Why might we want to call the above example a case of coordination failure, yet withhold that designation from Lucas's rational expectations equilibrium under imperfect information? Well, Lucas's producers are merely responding in parallel to a common, exogenous disturbance (in the shape of a money supply shock), while the chip-makers are, in a sense, *tripping each other up*. Firm A's plans are frustrated precisely because B, C and D happened to make and implement similar plans, unknown to A. The trouble is endogenous.

This reflection points us towards the second issue mentioned above, namely van Zijp's distinction between Hayek and Lucas on the basis of the endogenous or exogenous nature of the impulse that drives the cycle. This point is valid, but not quite properly in focus. Certainly, the Lucas impulse is exogenous, while Hayek held that any cycle theory that has to rely upon exogenous disturbances is *ipso facto* unsatisfactory.[16] But in what sense is the Hayekian cycle endogenous? Van Zijp (41-2) has it that Hayek's originating impulse is credit expansion on the part of the banks, which then lowers the market rate of interest below the natural rate. But why should the banks suddenly decide to expand credit? This seems little different from the arbitrary action of Lucas's monetary authority. I read Hayek as arguing that, in general, credit expansion is the *second* link (albeit the crucial one) in a chain that begins with a 'real' disturbance to the natural rate of interest.[17] Fluctuations in 'the expectations of profit' (apparently very similar to Keynes's shifts in the marginal efficiency of capital) are endemic to the dynamic development of a capitalist economy. Suppose there is an improvement in these expectations, hence raising the natural rate of interest. And suppose this goes unrecognised by the bankers, who therefore maintain their original lending rates in the first instance. Then lending will expand, and the Hayekian process gets underway.

Ironically, Lucas rejects the specific Hayekian cycle mechanism (as opposed to what he perceives as the latter's general approach) for a reason that is bound up with the very same erroneous interpretation of Hayek given by van Zijp. Thus, after noting that the Austrian theory involved the same idea of "mistaken investment decisions triggered by spurious price signals" as his own, Lucas continues: "However, the price which this theory emphasized was the rate of interest, rather than product prices as stressed here. Given the cyclical amplitude of interest rates, the investment-interest elasticity needed to account for the observed amplitude in investment is much too high to be consistent with other evidence."[18] But if Hayek's cycle is driven by fluctuations in the underlying natural rate, which the market rate fails to track properly, this argument of Lucas's misses the mark entirely. Indeed, the Hayekian disruption will be *maximised* if investment is relatively interest-inelastic, for then it will take a particularly large shift in the (sticky) market rate to compensate for the impact of a shift in 'the expectations of profit'.

The remaining comparative points mentioned above -- concerning the 'homogeneity postulate' and the question of mathematical formalisation -- may be addressed jointly. Regarding mathematical formalisation, there are certainly strong *prima facie* grounds for distinction. Austrian theorising is almost exclusively discursive, New Classical work for the most part tightly formalised. Both van Zijp and Kevin Hoover[19] argue that this is not merely a difference of style, but reflects deep philosophical substance. I find their case less than compelling. Mises' uncompromising rejection of all forms of mathematical economics is well known, but John Moorhouse[20] has recently made the argument that Mises' anti-mathematical claims do not in fact follow from his core philosophical commitments. Besides, Hayek was much less dogmatic in this regard, and it is surely Hayek who bears the closest comparison with the New Classicals.

Van Zijp's basic claim is that, in the Austrian view, the use of mathematics necessarily leads to over-simplification of a highly complex reality: differences among agents are obscured, and radical uncertainty is suppressed, along with the role of entrepreneurship and indeed the 'coordination problem' as a whole. And insofar as the formalisation of New Classical models is geared towards statistical estimation and forecasting, this is said to provide a further ground for distinction, since the Austrians believe that economics (by virtue of the 'complexity' with which it deals) can only hope to provide 'generic schemes' of explanation, with little empirical content. But this is not the message that emerges from the very interesting passages in Hayek's *Monetary Theory* concerning the role of statistics.[21] While insisting on the primacy of theory, Hayek remarks that "there can be no doubt that Trade Cycle Theory can only gain full practical importance through exact measurement of the actual course of the phenomena which it describes" (32). The role of statistical information is to enable us "to grasp existing conditions completely enough for forecasts of the future and, eventually, appropriate action, to become possible" (35). This is very far from a generalised rejection of quantification.

Furthermore, beginning with Sraffa's merciless review of *Prices and Production*,[22] Hayek's critics have doubted whether there is any coherent logical core to his cycle theory: if there is one, surely it ought to be susceptible to formalisation.[23] It may be true that non-homogeneous capital is central to Hayek's argument, but is this really foreign in principle to New Classical analysis? If a technically adept New Classical economist felt that the Hayek mechanism were plausible, couldn't this be modelled -- in a simplified version of course -- by including even two kinds of capital goods (say, those used to produce consumer goods, and those used to produce other capital goods)? To refuse the challenge of formalisation in principle, on the grounds that reality is just too complex and open-ended, is to lapse into obscurantism. Another remark from *Monetary Theory* is *à propos*: "[W]e often find," says Hayek, "an assertion, unfounded on any system, of a far-reaching indeterminacy in the economy." This thesis, he maintains, "is bound to have a devastating effect on theory; for it involves the sacrifice of any exact theoretical deduction, and the very possibility of a theoretical explanation of economic phenomena is rendered problematic" (96).

Conclusion

Van Zijp argues that although there are important similarities between Austrian and New Classical theories of the business cycle, there are also important differences, with the latter in a sense running deeper. I agree that there are significant differences, but I'm not sure they are necessarily any 'deeper' than the similarities. And the differences are exaggerated by van Zijp's tendency to efface the specific theoretical concerns and commitments of Hayek's cycle theory. It is, of course, easy enough to show that the problems and methods of New Classical economics are radically different from those of, say, Lachmann or Kirzner, but was that supposed to be the point?

One parting shot: it is worth noting the context of the remarks in which Lucas himself claims a Hayekian heritage for his theory.[24] These were not the prelude to any detailed examination of Hayek's work; rather, Lucas was simply reaching back in the history of economics to establish some continuity with the state of understanding of the business cycle prior to the transformation wrought by Keynes's *General Theory*. This is quite legitimate: technical matters apart, the New Classical economics represents just such a regression. And if Lucas could not equally well have cited, say, Pigou or Hawtrey as a pre-Keynesian forebear, then why not? Perhaps because the latter were not sufficiently aprioristic, not sufficiently committed to the "automatic adjustment of supply and demand" that is "always present in a state of natural economy".[25]

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NOTES

1 In Karl Popper, *Objective Knowledge* (Oxford: Clarendon, 1972), 153 ff.

2 Although he doesn't say so, van Zijp seems to have Mises in mind at this point. Hayek was much less dismissive of Popper, as van Zijp recognises later in the book.

3 Terence W. Hutchison, *The Philosophy and Politics of Economics: Marxians, Keynesians and Austrians* (New York: NYU Press, 1981), 211. The 'two Hayeks' thesis has been criticised, though not in a very sustained or substantive manner, by John Gray in his *Hayek on Liberty* (Oxford: Blackwell, 1984).

4 Or perhaps three. Might a New Classical not argue that his taking general equilibrium for granted is simply a case of intellectual division of labour? The business cycle theorist looks to the GE theorists to provide the proofs of existence and stability of equilibrium. Surprisingly perhaps, van Zijp offers no discussion of the relationship between the 'Hayek Problem' of convergence towards a coordinated state, and the extensive mainstream literature on the stability of general equilibrium.

5 First German edition 1929, English translation 1933.

6 First published 1931, revised edition 1935.

7 *Monetary Theory and the Trade Cycle* (New York: Augustus Kelley, 1966 reprint), 70-71.

8 Op. cit., 43.

9 There is also a disregard of chronology in Van Zijp's statement that "like Mises, Hayek advocated the 'free banking system' as the best way to avoid the adverse effects of the cycle" (43). It's true that Hayek came to advocate free banking in the 1970s, but at the time of writing his cycle theory he indicated the opposite policy (100 per cent reserve banking, plus abolition of the cheque system) as the only way to prevent monetary disturbances -- see *Monetary Theory*, 190-91.

10 F.A.Hayek, "Knowledge and Economics", *Economica*, n.s., vol.4, February 1937, 33-54.

11 For a detailed account of the matters discussed below, albeit from a somewhat different perspective, see Franco Donzelli, "The Influence of the Socialist Calculation Debate on Hayek's View of General Equilibrium Theory", *Revue Européenne des Sciences Sociales*, tome XXXI, no.96, 1993, 47-83.

12 F. A. Hayek, "The Use of Knowledge in Society", *American Economic Review*, vol. XXXV, no. 4, September 1945, 519-30.

13 See for instance David K. H. Begg, *The Rational Expectations Revolution in Macroeconomics* (Deddington: Philip Allan, 1982), Steven M. Sheffrin, *Rational Expectations* (Cambridge: CUP, 1983), and, for a fuller account with a stronger methodological and historical orientation, Kevin D. Hoover, *The New Classical Macroeconomics, A Sceptical Inquiry* (Oxford: Blackwell, 1988).

14 Thomas J. Sargent, "Commodity Price Expectations and the Interest Rate", *Quarterly Journal of Economics*, vol. 83, 1969, 127-40.

15 "Anticipated Inflation and the Nominal Rate of Interest", *Quarterly Journal of Economics*, vol. 86, 1972, 212-25, and "Interest Rates and Prices in the Long Run", *Journal of Money, Credit and Banking*, vol. 5, 1973, 385-449.

16 See *Monetary Theory*, 145.

17 See *Monetary Theory*, 147-8, 167-8. I discuss this point in my "Hayek's Early Cycle Theory Re-examined" (*Cambridge Journal of Economics*, forthcoming).

18 Robert E. Lucas, Jr., "Understanding Business Cycles", in his *Studies in Business Cycle Theory* (Cambridge, Mass.: MIT Press, 1981), 237 n15.

19 *Op.cit.*, chapter 10.

20 John C. Moorhouse, "A Critical Review of Mises on Mathematical Economics", *History of Economics Review*, Summer 1993.

21 In line with my earlier remarks, I would not necessarily wish to claim that Hayek's views on the role of mathematics and statistics in economics remained unchanged over time. But if the topic under discussion is Hayek's cycle theory, his views at the time of writing on the cycle seem particularly relevant.

22 For a recent discussion of which, see Michael S. Lawlor and Bobbie L. Horn, "Notes on the Sraffa-Hayek Exchange", *Review of Political Economy*, vol. 4, no. 3, 1992, 317-40. Van Zijp has nothing substantive to say about Sraffa's critique.

23 Cf. Hicks's interesting account of the insights he obtained into the problems of Hayek's cycle theory as he attempted to write "the mathematical appendix [*Prices and Production*] seemed to require" -- see J. R. Hicks, *Money, Wages and Interest* (Cambridge, Mass.: Harvard University Press, 1982), 6-7.

24 Lucas, *op. cit.*, 215-6.

25 Hayek, *Monetary Theory*, 101.