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Peter Howitt – A Keynesian Still in Recovery

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David Laidler

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Peter Howitt – a Keynesian Still in Recovery*

by

David Laidler

Abstract: Peter Howitt is best known for his contributions to growth theory, but his work in short-run economics, which began with his Ph.D thesis and still continues, is important and deserves attention. It lies firmly in the Keynesian macro-disequilibrium tradition of Clower and Leijonhufvud, and for a long time has been overshadowed by New-classical and New-Keynesian orthodoxy. However, the development of agent based modelling and behavioural economics will perhaps give disequilibrium macroeconomics a new lease on life.

Key words: equilibrium, disequilibrium, money, New classical Economics, New Keynesian Economics, Keynes, Lucas, Howitt, Clower, Leijonhufud, Phelps.

JEL Classifications: B22, B59, E12, E13, E31, E32.

*This paper formed the basis of a presentation made at L'Universite Cote d'Azur, Nice, on September 8 2022 as part of a *Doctorat Honoris Causa* Ceremony honouring Peter Howitt. My debt to personal correspondence with Peter is immense. Nevertheless the view of his contribution to short-run macroeconomics that I set out here is entirely my own responsibility.

I *Introduction*

It is a great honour and an even greater pleasure to discuss some of Peter Howitt's contributions to macroeconomics. I had heard praise of Peter from Bob Clower even before we became colleagues in 1975, and though he left the University of Western Ontario long ago, we have never lost touch with each other. We have written only one paper together, and an obscure one at that (Howitt and Laidler 1979), but our association has been much deeper and longer lasting than this statistic might suggest. It has been one of the most fruitful of my career. Peter's contributions to short-run macroeconomics (hereafter just "macroeconomics") attract far less attention than those to growth theory, and far less than they deserve. This is surely because they have been unfashionably Keynesian. Fashion can change, though, and this brief essay explains why it should and still might.¹

II *The Keynesian Recovery that Wasn't but Still Might Be.*

I shall start my story towards its middle, on May 29th, 1986, to be precise, when Peter delivered the annual Harold Innis Lecture to the Canadian Economic Association's Winnipeg conference. His title was "The Keynesian Recovery" (Howitt 1986b): pointed and provocative, given the recent dominance of the ideas of New Classical Economics (NCE) over academic macroeconomics. The lecture's message was: that Keynesian Economics, whose demise had been proclaimed by Robert E. Lucas Jr. and Thomas J. Sargent (1978) just a few years earlier, was showing renewed vigour at a time when NCE was beginning to weaken in the face of empirical evidence; that it still provided the soundest basis for the sub-discipline's future development; and that it would soon regain its dominance. And he used the same title a little later for a collection of his essays on short-run macro topics (Howitt 1990a).

As we all know, despite this display of persistent optimism, Peter's third proposition turned out to be far off the mark, leaving the other two moot. Mainstream macroeconomics did not re-establish itself on reconstructed Keynesian foundations in the 1980s. Instead, it maintained continuity with NCE and entered a long phase of what a Kuhnian would call "normal science," going on to develop a body of analysis, occasionally characterized as the New Neo Classical Synthesis (NNCS), whose only link to Keynesian economics lay in the perversely misleading

¹ And with brevity comes selectivity. It should be said explicitly that this essay does not purport to be a complete survey of Peter's work in macroeconomics. In particular, the emphasis here is on his contributions to our understanding of the role of monetary exchange in short-run systems. In the '80s, he also made significant contributions to the search theoretic literature that followed the lead of Peter Diamond (1982a&b), exploiting then-new equilibrium concepts derived from game theory (see Howitt 1985, Howitt and McAfee 1987, 1988). In my view and *from the perspective of Peter's own work*, these are best seen as *technical* precursors of his contributions to growth theory, where the deployment of similar equilibrium concepts is fundamental. With the sole exception of Howitt (1988), he did not try to follow up their substance in the context of a money-using economy. See also footnote 13 below. As Clerc and Raymond dos Santos-Ferreira (this symposium) recount, however, a distinctive non-Walrasian, but also non-monetary, type of short-run macroeconomics, has developed from the search-theoretic foundations laid in the 1980s.

“New-Keynesian” (NK) label that has more usually been applied to it in recent years.² In the real world, memories of the Volcker disinflation faded as the ‘80s progressed (the severity of Canada’s downturn in the early ‘90s was a local phenomenon), medium term inflation targeting was widely adopted in the ‘90s in preference to the pursuit of short run stabilization goals, the Great Moderation set in, and, in Western economies at least (Asia and Russia were different matters), the macroeconomic environment remained tranquil for almost two decades.

When this peaceful era came to an abrupt end with the financial crisis of 2008-9 and the recessions that followed, it turned out that NNCS/NKE had surprisingly little to say about these events. And before the sub-discipline had caught its intellectual breath, the widespread and still-ongoing inflation that followed the policy-improvisations prompted by the arrival of the coronavirus pandemic in 2020 faced it with a new set of inflationary challenges to which it once again failed to rise. Perhaps, then, as Peter himself hopes, the Keynesian Recovery that he looked forward to so long ago might only have been delayed for three or four decades: hence the title of this essay.

III *A Keynesian Education that Definitely Was*

Peter’s education in macroeconomics was robustly Keynesian, and very different from that to which Robert E. Lucas Jr. (2004) would attach the same label when describing his own training in early ‘60s Chicago.³ In the late ‘60s there were still important economics departments where the textbook based “standard curriculum” had not penetrated, where graduate students were few and far between, and where the later stages of the undergraduate programme provided the venue where faculty took students to those parts of the subject’s frontiers that particularly interested them. McGill was such a place. The macro component of Peter’s final honours B.A. year (1967-68) saw him working through not one of the numerous texts based on the Samuelson-Hansen Neoclassical Synthesis (NCS) and centered on the ubiquitous IS-LM diagram, but *The General Theory* itself, as well as the second edition of Don Patinkin’s *Money Interest and Prices*, under the guidance of Joan Robinson’s student Tom Asimakopulos and of Tom’s other mentor, Jack Weldon.

His next stop, the one-year M.A. program at the University of Western Ontario was more conventional, but it nevertheless included a one semester course in monetary economics taught by Joel Fried, a recent student of Clower. At Western, Peter was thus exposed (among other things, of course) to Clower’s (1965) “Keynesian counter-revolution” paper, which along with

² The New Keynesian label was first applied in the late 1970s to models that relied on sticky prices to generate their results. The aggregate demand sides of later models, whose supply sides were based on Real Business Cycle analysis, were directly descended from these, and brought this label along with them. By the late 1990s the NNCS and NKE labels were being used synonymously.

³ Lucas received this education mainly from Martin Bailey, author of the fine (1962) textbook *National Income and the Price Level*, and Harry Johnson, the outline of whose course followed quite closely that of his once famous (1962) “Survey of Monetary Theory”. Joan Robinson, the mentor of Peter’s teacher Tom Asimakopoulos would surely have applied her famous qualifier “bastard” to the version of “Keynesianism” represented here.

Chapter XIII of Patinkin would in due course provide the basis for Robert Barro and Herschel Grossman's (1971) "General Disequilibrium Model of Income and Employment". And it was while at Western that he bought his copy of Axel Leijonhufvud's (1968) *On Keynesian Economics and the Economics of Keynes*. Small wonder that, in 1969, his preferred destination for a Ph.D. was Northwestern, with Clower as his thesis supervisor, or that when, having followed Clower to Los Angeles in 1971 to complete that thesis, he "thought he had gone to heaven" when he found himself a member of a group that included not only Clower, but Leijonhufvud and Joseph Ostroy as well.⁴

IV *Macroeconomics in Disarray 1972*

Peter absorbed this education at a time when there was much disarray in macroeconomics.⁵ In 1972, when he took up his first academic appointment back at the University of Western Ontario, the extent to which Hansen's (1954) *Guide to Keynes* had ever described a useful route to understanding *The General Theory* was still under debate, but its claims here had already been subject to much skeptical scrutiny. The NCS that Hansen had helped codify around the IS-LM framework, and had formed the basis of Lucas's education, was spawning ever larger and more complex and disaggregated macro-econometric models, but gave no guidance to anyone wishing to link macroeconomic to microeconomic theory. Patinkin's (1956) *Money, Interest and Prices*, whose first edition appeared only two years after Hansen's textbook, had tried to remedy this deficiency. Its subtitle, it is worth recalling, was *An Integration of Monetary and Value Theory*.

Keynes's own discussions of the micro-foundations of his *General Theory of Employment, Interest and Money* were opaque, thanks to that book's Marshallian pedigree, and the subsequent absorption of formal General Equilibrium Theory in the tradition of Walras and Pareto into the English language literature, following the efforts of John Hicks (1939), Paul Samuelson (1947), and eventually Kenneth Arrow and Gerard Debreu (1954), had not eliminated the challenges posed by this opacity. As I recall Clower putting it sometime in the mid-'60s, the Walrasian model consisted of a set of equations describing the entire economy, all of which had quantities on their left-hand side and prices on the right and yielded a full employment equilibrium solution; the algebraic representation of what should have been that same economy summarised in IS-LM, had quantities on the right hand side of some of its equations, and yielded a full employment solution only by remote accident: why the different configuration of these equations, and how was this related to their different solutions? What further issues might an answer to these questions raise?⁶

⁴ Personal correspondence with Peter Howitt 14/06/22. In his oral presentation at this conference, Peter also included Armen Alchian on this list.

⁵ My appropriation of the title of Karl Brunner's (1986, published 1989) Henry Thornton Lecture, to which the profession paid insufficient attention, is deliberate. His use of it to describe the state of this unruly sub-discipline in the early 80s is equally apt when applied to the beginning of the '70s.

⁶ It is not co-incidental that Clower was Hicks' student. Hicks' own half century of intellectual progress from J.R Hicks, author of *Value and Capital* (1939), and originator (1937) of the IS-LM diagram (which he labelled SI-LL) to

As we all know nowadays, the answers here turned on the fact that an “auctioneer” presided over the Walrasian economy. Only when this entity had found a set of prices that cleared all markets, communicated them to all agents, and arranged all necessary contacts among them for multilateral barter exchange to take place did economic activity begin. Thus, agents had all the *information* needed to make mutually consistent plans as well as complete assurance that, when it came to their execution, their *co-ordination* would be costless, before anyone actually did anything.

The Keynesian economy, on the other hand, lacked an auctioneer, and required agents to cope with such problems for themselves. They had somehow to set prices, disseminate information about them, devise ways to find others with whom trade would be possible and desirable, and then try to harmonise the outcomes of each individual bilateral transaction with all the others, etc. etc.. In this Keynesian economy, which bears a remarkable resemblance to the one we actually inhabit, acts of sale and purchase would be executed sequentially rather than simultaneously, and hence would be conditioned by expectations about what was coming next. Crucially, they would also be facilitated by the use of some universally acceptable intermediate item.

Money or, more precisely, the institution of monetary exchange, was thus understood to be as essential to this Keynesian economy as were the activities of the auctioneer to its Walrasian counterpart. Indeed the two performed essentially the same tasks in the competing systems. The auctioneer was much better at them, though, being able to ensure that all trades took place at market clearing prices in a system of frictionless multilateral barter. On the other hand, in the non-Walrasian system, trades of goods and services against money might be executed at non-market-clearing - “false” - prices, rendering the execution of previously formulated plans impossible. Agents would thus need to devise rules to cope with such eventualities, apply them, and try again . . . and again . . . The presence of quantities in those Keynesian equations that had attracted Clower’s attention turned out to follow from this last insight, as did the likelihood, so thoroughly laid out by Leijonhufvud (1968), that their presence could create dynamic adjustment processes that would amplify, rather than damp down, the consequences of any initial shocks or errors.

By 1972, explorations of the consequences of these insights, notably by Barro and Grossman (1971), and various contributors to Edmund Phelps (ed.) (1970), were in full swing, though the super-structure of this style of Keynesian economics was by no means as complete as that of the IS-LM centered NCS which it was now challenging. And to complicate the intellectual environment further, in (1970) Milton Friedman had at last explicitly declared his Monetarist “Counter-revolution in Macroeconomics” against those same mainstream ideas. This insurgency had been in progress for some years and was mainly based on empirical and policy related, rather

John Hicks, author (with constant encouragement from Axel Leijonhufvud) of *A Market theory of Money* (1989) was an important part of the story of the evolution of Keynesian macroeconomics.

than micro-theoretic analysis. Friedman himself had earlier suggested that the intellectual space in which it was taking place had previously been occupied by a pre-*General Theory* version of the Quantity Theory of Money, though some were suggesting that, particularly in his treatment of the demand for money, Friedman was in fact encroaching on Keynesian territory.⁷

V The Appealing New Classical Simplification

Be that as it may, when Peter took up his first academic appointment in 1972, no school of thought dominated his chosen field of study, and no-one was quite sure if or how all the above-mentioned pieces fitted together. But in that same year the *Journal of Economic Theory* published a brief and technically difficult paper on “Expectations and the Neutrality of Money” by Robert E. Lucas Jr.

This paper’s contents are too well known to require repetition. What matters here is that, at first, almost no-one noticed its particularly destructive significance for the still incomplete Keynesian agenda, on which work therefore continued.⁸ Two of its features in particular were attracting critical attention at the time: namely, its apparent implication that a market economy would be extremely unlikely ever to achieve and maintain full employment without the continuous help of activist policy aimed at that outcome; and its relative neglect of price level fluctuations.⁹ The need to attend to these was pressing because, in the real world, high employment had in fact persisted everywhere for almost a quarter century and was now being accompanied by rising and increasingly conspicuous inflation.

Peter had already worked on the price level in the chapter in his thesis that became his 1974 *JPE* paper “Stability and The Quantity Theory”. There he had shown how the awkward and totally contrived separation of the determination by an auctioneer of the price level from the processes of market exchange that had marked Patinkin’s exposition of the Walrasian microeconomics of the real balance effect could be eliminated by considering a Keynesian set up in which money prices were set and varied by “shop-keepers” in response to sales of their wares.¹⁰ This paper’s

⁷ See Patinkin (1974). As to monetarism’s more general Keynesian connection, the largely independent work of Karl Brunner and Allan Meltzer, both of whom had strong UCLA connections, on the place of money in the economics of information, surely confirms it. See Brunner (1971), Brunner and Meltzer (1971) and Pierrick Clerc (2019, 2022)

⁸ And of course its significance extended far beyond this particular matter. See Laidler (2022). Furthermore, “almost no-one” is not everyone. Crucially for the future development of New Classical Economics, Thomas J. Sargent and Neil Wallace grasped the significance of Lucas’s paper even before it was published. See Sargent (2021)

⁹ Because this analysis mainly focussed on the forces producing the variations in real variables that had so occupied Keynes in (1936), and because these stemmed from trading at non-market clearing prices, it was all too convenient to simplify matters by assuming that prices were constant, as did, for example Barro and Grossman (1971). This author can attest personally to the increasing difficulty of getting some of his Manchester students to take such models seriously as inflation rose to well over 20 per cent per annum.

¹⁰ Patinkin’s analysis in effect left the auctioneer to set prices but deprived him of the task of then bringing trading partners together. This completely arbitrary adjustment to the usual Walrasian arrangements left room for the emergence of a precautionary demand for money. Kiyotaki and Wright (1989) would later utilize the same analytic contrivance in the development of their search theoretic analysis of the emergence of monetary exchange, as Peter long ago pointed out to me.

very title, never mind its content, made it clear that Keynesian theory could support an explanation of price level behaviour in terms of variations in the money supply just as easily as could Monetarism, no minor point at a time when most exponents of NCS Keynesianism were favouring cost-push explanations of inflation and denying the relevance of money to its progress.

As to the task of reconciling analysis initially intended to explain chronic unemployment to the fact of persistently high employment, Leijonhufvud's (1973) "Effective Demand Failures" suggested that the non-Walrasian monetary economy might well have an equilibrium solution in the vicinity of full employment which lay within a *corridor*. Here deviation-damping mechanisms would dominate output's response to disturbances, but outside of this zone, these might be overwhelmed by deviation-amplifying processes. Once within this corridor, whether as the result of chance or sound policies, the economy would tend to stay put, as it in fact had in the post-war years, unless and until it was hit by a large enough shock to push it into unstable territory. Typical of Axel's style, this paper was longer on theoretical vision than on the hard analysis needed to convince skeptical readers. Peter provided the latter in (1978), along with references to the work of other scholars who had explored similar ideas (these included Irving Fisher 1933, and Hyman Minsky 1964).

The Leijonhufvud-Howitt "corridor" papers between them had the potential to provide a jumping-off point for the exploration of the inflationary consequences (whose destructive nature Axel himself laid out with great originality in 1977) of positive destabilising shocks, and hence to lay the foundations for a Keynesian alternative (or supplement - which was not clear) to contemporary Monetarist analysis of inflation. But they didn't. Between their publication dates, Keynesian macroeconomics had lost most of its readership as macroeconomists realised that Lucas had shown that the (admittedly complicated) non-Walrasian micro-foundations that Axel and Peter were taking the lead in developing were unnecessary for the explanation of fluctuations in real variables. This understanding took hold, moreover, just as it was also becoming apparent that the best known Keynesian macro-model of the time, that of Barro and Grossman (1971, extended in 1976), was leading not forward into new territory, but back to a structure not much different from the traditional fixed price-level IS-LM set up, while also yielding the grossly false prediction that, if a condition of excess aggregate demand ever developed, output and employment would fall.

Barro and Grossman themselves would be among the first of many to conclude that Keynesian macroeconomics was not only more complex than was required, but also less productive of useful results than was desirable, and to abandon it for NCE, which also attracted many recruits from the Monetarist camp. NCE's Walrasian nature made it incapable of incorporating the institution of monetary exchange, so that it could treat "money" only as some sort of store of value.¹¹ Despite this awkward fact, however, it also seemed to many to provide a coherent micro-

¹¹ Hence the importance of overlapping generations models of money in the NCE literature. It is true that some of its exponents, notably Lucas himself in (1984), sometimes incorporated Clower's (1967) "cash in advance" constraint

theoretic basis for the increasingly persuasive claims that Monetarism was making about the empirical importance of monetary shocks in driving price level behaviour, the essentially transitory nature of their effects on real variables, and (particularly as further elaborated by Lucas himself (1976) and Sargent and Wallace (1976) the futility of discretionary stabilisation policies, particularly those guided by econometric models. In 1981, James Tobin would rename NCE “Monetarism Mark 2”, and frame the macroeconomic debate as one between NCS, which he still defended, and NCE. Many people (including Peter 1986, but not this author 1981) found this renaming appropriate enough, but few shared Peter’s continuing belief that his brand of Keynesianism also had something to contribute to the debate in question.

VI *Empirical Complications*

NCE, in the form that Lucas and Sargent (1978) promoted it, quickly came under pressure, both theoretical and empirical. To begin with, though it certainly provided a rigorous theoretical basis for Friedman’s (1968) claims that systematic variations in the behaviour of the money supply could have no effects on real variables, its manner of doing so seemed a bit too simple. Specifically, it was quickly noted that it would not hold up if some feature or features of the economy led to agents collecting, processing, and/or acting on less information than NCE claimed would inform their expectations.

One obvious barrier here was money wage stickiness, a prominent feature of real-world labour markets and a phenomenon that had long been utilized by NCS to explain the occurrence of unemployment. Papers by Stanley Fischer (1977) and Phelps and John Taylor (1977) duly deployed it to show that, thus modified, otherwise NCE models yielded predictable results about the potential effectiveness of activist monetary policy, thus giving what evolved into NKE its start, not to mention its problematic label.¹² In addition, costs of gathering and processing information, staple ingredients of Keynesianism, were also quickly invoked to explain why agents might not make use of all that was available when it came to forming what Edgar Feige and James Pearce (1976) called “economically rational expectations.” Exponents of NCE, initially remained unimpressed by all of these criticisms, and some of them, (e.g., Barro 1979) even refused to discuss any results based on such phenomena unless their patently obvious

into their models – a notable act of intellectual larceny – but this procedure completely violated their own analytic principle that everything in a model should follow from its own economic fundamentals, so the practice did not catch on. To the best of my knowledge, the extensive literature that followed on from Kiyotaki and Wright (1989) never took the all-important step of involving the institution of monetary exchange in the price formation process.¹² The problem being that, whatever the textbooks of the time, based on NCS, might have said, Keynes himself had denied that what he called involuntary unemployment was the consequence of price stickiness. It might also be noted that, with the arrival of NCE, the phrase “price flexibility” had changed its meaning, and so therefore had its opposite “price stickiness”. Before 1972 the first phrase referred to prices that would move quickly to eliminate a disequilibrium between supply and demand and the second to prices that took longer to adjust. After 1972, the “price flexibility” that NCE assumed involved prices that moved so quickly as to endure that a disequilibrium never arose in the first place, and anything short of that became “price stickiness.” See Laidler (1996).

empirical existence had been explicitly deduced from “first principles” that met New Classical criteria of soundness.

Peter’s (1980) paper “Activist Monetary Policy under Rational Expectations” can be read as a response to this challenge. He analysed explicitly maximising individual behaviour in a many agent economy where, as in Howitt (1974), money prices were set at the beginning of a trading period by shop-keepers.¹³ He then formally showed that if it was at all costly to extract information from publicly available money-supply data that might in principle help in individual pricing decisions, one agent’s choice of whether actually to do so would depend upon, among other things, an assessment of how other agents would tackle the same decision. If others seemed unlikely to go to the trouble of absorbing and acting on such information, then going it alone would yield no benefits, and, though available, it would be ignored by everyone. More generally, Peter argued, whether and/or how the collection and utilisation of information was co-ordinated across agents would affect how they utilised it; so indeed would the state of their information about other aspects of the economy’s structure, which NCE models usually assumed, with no justification beyond analytic convenience, to be complete.

By the time this paper appeared, more than just NCE’s policy ineffectiveness propositions were coming under critical scrutiny. Basic inconsistencies between its predictions and certain facts of economic life that had long been recognised, but initially brushed off as likely to be resolved with further technical work, were beginning to be taken seriously. Notably, it had long been recognised that fluctuations in real variables systematically precede those in the price level and that they also persist over time. The often informal analysis deployed by Monetarism Mark 1 had been tailored to fit these properties of the economy (see, e.g., Friedman and Schwartz 1963), but the Lucas model had real variables responding along an aggregate supply curve to variations in the price level, and only if these were unanticipated as well, while its configuration also ensured that the price-level/relative-price-confusions that underlay such responses could only last for one period.

The first of these awkward issues was inherent in the very properties of a perfectly competitive economy in which an auctioneer sets prices before economic activity begins, and though Lucas (1975) himself tried to address the second by stretching out the dissemination of price information across the islands of his model economy to multiple periods, this solution turned out to work only in the absence of economy wide asset markets.¹⁴ The econometric troubles implicit here duly became explicit, first with Lucas’s own early (1973) resort to *ad hoc* adjustment lags in

¹³ In (1988) Peter would stray from this approach to the analysis of price level determination and consider a system in which a Lucasian aggregate supply function played a central role. This paper was concerned with investigating the consequences of explicitly incorporating a labour market characterised by search behaviour, as developed in Howitt and McAfee (1987), into a macro model that otherwise followed new classical precepts, in order to show how such considerations could lead to persistent employment fluctuations without resort to arbitrary wage and price stickiness assumptions. This effort was successful in its own terms but proved to be a detour in the development of his Keynesian approach to short-run macro modelling. See also footnote 1, above.

¹⁴ See Edi Karni (1980) for a formal presentation of the argument.

the explanation of output fluctuations, more conspicuously with the tortured compromises between theory and evidence that marked Robert J. Barro's (1978) efforts to fit a small econometric version of it to post-war US data, and then, decisively, with John Boschen and Grossman's (1981) demonstration (which confirmed but did not cite Peter's (1980) predictions) that variations in readily and immediately available data on the US money supply were not used to form inflation expectations, and were in fact systematically and strongly related to subsequent, highly serially correlated, variations in real output. Initial reporting errors in these series, moreover, which measured variations in the money supply that could not have been anticipated, had no discernable influence at all on anything.

VII *More Disarray Followed by Simplification Resurgent*

By the early '80s, then, it was clear that Lucas's (1972) money-supply-surprise model had failed in the face of empirical evidence. And fairly or not, its reputation had also been damaged by a bad dose of guilt by association with the Volcker disinflation, which had been widely touted as a failure of Monetarism of both Marks.¹⁵ NCS too, and particularly the big econometric models associated with it, had already sustained severe damage from earlier theoretical attacks on their foundations, as well as from their perceived association with the macroeconomic policies that had brought on and then failed to curb the inflation that Volcker's policies had brought to a painful end. Thus, it suddenly seemed as if the Keynesian approach to macroeconomics was the only one not in serious trouble, that its recent eclipse might have been temporary, and that the field was wide-open for that Keynesian Recovery. In the circumstances, the timing of the publication of Axel's *Information and Co-ordination* in (1981) and Peter's review article discussing it in *Economic Inquiry* (1984) seemed perfect. So, what happened?

To begin with, Real Business Cycle Theory (RBC) arrived with the 1982 publication in *Econometrica* of Finn Kydland and Edward Prescott's "Time to Build and Aggregate Fluctuations" and proved immediately attractive to those many exponents of NCE for whom its main virtue had been the equilibrium modelling idea and the techniques that went with it. RBC provided a new and extensive agenda where they could continue to exercise their methodological preferences. Perhaps, had it not turned up, some of them might eventually have been tempted to look at a Keynesian menu for new research topics. But it did, so the barriers to such a temptation were never seriously tested.

In the meanwhile, NKE had also continued to develop and attract the attention of those more interested in empirical content and policy implications than theory for its own sake. Its early pragmatic adoption of *ad hoc* price stickiness was in due course replaced by the incorporation of monopolistically competitive pricing, while its relatively unfastidious attitude towards other *ad hoc* fixes that might increase its empirical content permitted it to deal with persistence by unashamedly resorting to adjustment lags in the style of Lucas (1973). At a time when the

¹⁵ To be clear, this author, like Karl Brunner (1983), is firmly on the "not" side in this debate, but there is no space here to discuss the reasons why.

search was on for a new “place to stand” for monetary policy in the wake of money growth targeting’s failures, NKE found a ready audience, particularly among those who had never lost their suspicions of Monetarism during its short period of ascendancy.¹⁶ And NKE’s ability to accommodate the ancient argument that the money supply’s movements represent passive endogenous responses to factors affecting the economy’s demand for it, rather than playing any causative role in their determination, only served to make it even more attractive in those quarters.

Even so, Peter made a considerable effort to promote a more thoroughgoing Keynesian foundation for monetary policy analysis in his *Monetary Policy in Transition: a Study of Bank of Canada Policy 1982-85* (1986a). Here, his practical recommendation was that the Bank should develop and then adopt a system that he called “adaptive monetary control”. Its goal would be to attain and then maintain price stability by putting and then keeping the growth of money (a broader aggregate rather than the narrow one that had caused so much trouble in the ‘70s) within a limited range by systematically varying the Bank’s policy interest rate in response to any incipient tendency for it to go off track.

This focus on interest rate control differentiated Peter’s proposal quite sharply from contemporary monetarist work, where the monetary base remained the preferred policy instrument. Indeed, if we were to substitute the inflation rate for the money growth rate here, it would look very much like a quantitatively less precise anticipation of the quintessentially New Keynesian Taylor Rule. But Peter also explicitly argued, in this monograph and elsewhere, that claims about the passive endogeneity of money that were becoming a prominent feature of NKE were wrong.¹⁷ More generally, his Keynesian convictions about the vital importance of the monetary and financial system for the successful co-ordination of economic activity were on full display in this monograph as well, as they would be later in his essay on “Zero Inflation as a Long Term Target for Monetary Policy” (Howitt 1990b).¹⁸ It was NKE’s failure to pay serious attention to just these matters that would eventually render it so helpless in the face of the financial turbulence that began in 2008 and of today’s inflation as well.

Peter’s policy work was both visible and influential in the local Canadian debate, but this success did not extend to creating a significant readership for the theoretical work that lay behind it. In Canada as elsewhere, monetary policy would in due course find its theoretical place to stand, not in Keynesian economics, but in NNCS, a merger between NKE and RBC. The former, much tightened up from its 1970s formulations by application of “representative agent” modelling techniques, focussed on the influence of variations in the real value of a policy interest rate on

¹⁶ *Monetary Policy: Finding A Place to Stand* was the title that Governor Gerald K. Bouey chose for his 1982 Per Jacobsson Lecture on central banks’ then ongoing search for new principles on which to base their policies in the wake of Volcker disinflation and the alleged failure of monetarism that it had revealed.

¹⁷ This topic was discussed in our only joint paper, Howitt and Laidler (1979)

¹⁸ Keynesianism also marked this essay’s extensive dynamically framed cost-benefit analysis of inflation reduction policies in the style of Phelps (1967, 1972)

aggregate demand, while the latter accounted for the evolution, as determined by resource endowments and productivity, of the economy's "natural" output level. The resulting "output gap" between these two variables, both of which were subject to random shocks, then affected the time path of the inflation rate relative to its rationally expected value. The Dynamic Stochastic General Equilibrium (DSGE) analysis that was the product of this synthesis, having adopted the NKE label, in due course came to dominate academic macroeconomics. As inflation targeting programs proliferated in the 1990s, it also came to dominate the formulation of monetary policy, as the all-embracing sub-title of Michael Woodford's (2003) *Interest and Prices: Foundations of a Theory of Monetary Policy* attests.

But representative agent economies in perpetual equilibrium cannot suffer from information and co-ordination problems and they have no room for monetary exchange either, so Keynesian economics was pushed yet again, and even further, towards the intellectual fringes by these developments.

VIII *Style Outweighs Substance*

This is *how* things happened, but *why* did they develop in this way? Like Peter, I believe that the answer here lies more in matters of style than of substance, in the persuasive rhetoric with which equilibrium modelling was marketed from the very outset, rather than in the specifics, let alone the accuracy and usefulness, of its empirical predictions. Peter paid particular attention to these matters in his *JEL* review of Arjo Klamer's (1984) *Conversations with Economists* (Howitt 1986c) and treated the weight that Klamer attached to rhetoric for its own sake with suspicion. He agreed that *NCE*'s strictly deductivist methodology and equilibrium modelling techniques were novel, attractive, and applicable to a wide range of questions, and that these stylistic characteristics had done much to promote its popularity. But he also criticised Klamer for paying insufficient attention to *NCE*'s shaky substantive content.

Now, an analytic method is separable from any particular model that conforms to it, so the rules of inference require only that the latter be discarded in the face of contradictory evidence. From the 1970s onwards, this consideration had underlain, as it still does, the specific claims made on behalf of equilibrium modelling in macroeconomics that its analytic method, being just that, is without specific empirical content, and that any criticisms of it based on the poor empirical performance of particular models are therefore beside the point. If there were no more to the matter than this, then Peter's (1986c) criticism of Klamer would have to be dismissed as irrelevant. But in Howitt (1990c) he would demonstrate that there was indeed more to it, *that equilibrium modeling actually prevented discussion of certain empirical issues, a factor even more important than the veracity of the predictions that emerged from the discussions that it did allow.*

His working paper of that year, "Wicksell's Cumulative Process as Non-Convergence to Rational Expectations Equilibrium" developed an example of how this could happen. The

conceptual experiment he considered was one where, in a conventional NCE macro model, the monetary authorities peg the nominal rate of interest, with the real rate of interest then being subjected to a positive productivity shock. For the system to remain in equilibrium at a constant nominal rate of interest, the expected rate of inflation had to fall to offset this positive shock. Furthermore, if agents were forming their expectations rationally and considering only equilibrium outcomes in doing so, they would read the maintenance of a constant nominal interest rate as a statement of the monetary authorities' intention to maintain this lower inflation rate. So, the economy's equilibrium response to the initial productivity shock would be an immediate fall in this variable to its new equilibrium value.¹⁹ But, crucially, Peter then showed that, if the requirement that equilibrium be constantly maintained was dropped from this experiment, a positive productivity shock in the presence of a constant nominal interest rate could instead open up a gap between the rate of interest that borrowers were willing to pay and the one that lenders were charging. The resulting disequilibrium would then set in motion an explosive Wicksellian cumulative process of inflation that would persist for as long as the nominal interest rate remained pegged. This specific example had a general implication: namely, that *an insistence on continuously clearing markets can eliminate discussion of empirically interesting, not to mention potentially accurate, predictions, and hence is not a scientifically neutral method of analysis.*

Peter's paper was later published under a revised title in the highly visible *JPE* (Howitt 1992), but equilibrium modelling went on its way, impervious to its implications. Perhaps this was not surprising. After all, it had been well known long before 1992 that: the aggregate production function that lay at the heart of the RBC component of DSGE analysis was non-existent; that the cross equation constraints derived from fundamentals on which its computational exercises relied held only for the behaviour of individuals and vanished upon aggregation; and, indeed, that monetary exchange itself had no inherent rationale in an economy always in equilibrium. None of these considerations had been seen by its many exponents as reasons to question it.

Could there be stronger evidence than this that the failure of a Keynesian Recovery to take hold in the 1990s was prompted not by the logical and empirical merits of the matter, but by a rhetorical case for equilibrium modelling that was persuasive enough to blind the majority of macroeconomists to its substantive limitations? The presence of all those Ns in the acronyms used in this essay to label schools of thought should perhaps have given the game away already: they all stand for "*NEW*", whether in English or Greek, a favorite adjective among marketers. The only cause for wonder, perhaps, is that NKE took as long as it did to inflict real damage on our economies by blinding policy makers to the significance of two of their most important features, both already known to be sources of trouble if they misbehaved: namely the financial

¹⁹ These same mechanics also imply that, if the real interest rate remains constant, and the central bank wishes to lower inflation, then the appropriate policy is to lower the nominal interest rate, a proposition nowadays associated with the policy program known as Neo-Fisherianism. Beyond remarking that its exponents seem to have ignored the implications of Howitt (1990c, 1992), further discussion of this matter is best left for another time and place.

system, where instability brought on the crisis of 2008-9, and the money supply, whose sudden explosive growth began in 2020 and triggered the inflation of 2021-2.

IX *The Keynesian Recovery Redux?*

Peter's standing in our profession today rests overwhelmingly on his contributions to the theory of Economic Growth, mainly made in collaboration with Philippe Aghion, and I am not qualified to discuss these. Philippe and Peter's first, seminal, paper (Aghion and Howitt 1992) appeared just as NNCS's long period of domination over macroeconomic theory and monetary policy was becoming well entrenched, but this timing did not reflect an acceptance on Peter's part that the short-run Keynesian macroeconomics to which he had by then contributed so much was at a dead end, let alone a loss of confidence in its basic soundness. Peter started collaborating with Phillippe in the early '90s, but he also continued to collaborate with Robert Clower.

In his 1986 review of Klamer (1984), Peter had suggested that part of NCE's popularity could be attributed to the fact that it met at least one of Harry Johnson's (1971) criteria for a successful intellectual revolution: namely, that it provided a new, but not too new, agenda for younger researchers to work on, and novel techniques as well, just difficult enough to deter their older competitors from joining in.²⁰ Peter perhaps noted the lesson implicit here for his own work, because, not too long after that Keynesian Recovery had failed to materialise in the '90s, his collaboration with Clower would result in a pioneering application to macroeconomics of Agent-Based Modelling, a then new approach, but one whose reliance on advanced computational techniques and deployment of the idea of adaptive learning nevertheless provided it with clear linkages to certain strands in the mainstream literature.

It is now almost a quarter century since "The Emergence of Economic Organization" (published in *JEBO* 2000) got its first conference airing in Ottawa. In this paper, Bob and Peter showed how the activities of initially independent agents, as they followed rules of thumb in their interactions, and systematically adapted these in the light of evolving experience, could lead to the evolution of such entities as specialised shop-keepers, and institutions such as monetary exchange, indeed of an economic system not unlike the one which Peter had analyzed in his Ph.D. thesis at the very beginning of his life as a Keynesian.

Since then, further applications of these methods have expanded explosively along paths which, having retired in 2004, I am not licensed to guide readers. Let me be content, then, with noting that Peter has contributed to these, helping us to understand, for example, how banks might function as co-ordinating agents in a monetary economy (see Ashraf, Gerschman and Howitt,

²⁰ In Laidler (2015) I made much of the applicability of Johnson's analysis to the New Classical Revolution without citing Howitt (1996). Peter recently and cheerfully accepted my apology for this oversight, even suggesting that it might have been me who had drawn his attention to Johnson (1971) in the first place. (Personal communication 14/06/2022).

2017), and, more generally let me affirm that, even as an intellectual day-tripper in this territory, I can see that an analytic method which puts co-ordination issues at its very center is once again finding an established place in macroeconomics. Closely related, the last two decades have also seen Behavioural Economics pile up a mass of empirical evidence showing that, although economic agents do behave consistently enough to be roughly predictable, the rules they apply to guide their activities frequently depart from the economically rational.

In the light of these interlinked developments, it can no longer be claimed that forward-looking maximising behaviour fully co-ordinated in markets that clear is the only academically respectable basis for macroeconomic theorizing. Models based on this principle are still defensible as tractable metaphors for the much messier and complex processes and situations that we all know actually govern real world behaviour. But, but whatever our past views, we must surely agree that nowadays, Agent Based Modelling and Behavioural Economics, not to mention the technology that makes them possible, provide a capacity that did not exist three decades ago for researchers to do things in other ways. And the troubles caused for mainstream macroeconomics by the recent empirical record of financial crises, recession and now inflation to be followed by who really knows what, suggests that they might also be able to find a market among policy makers.

X Conclusion

As his contribution to these proceedings makes clear, Peter Howitt still believes, as he always has, that though “We do have small models, based on a priori notions of rationality and equilibrium, that seem to be relevant in some circumstances. . . they are of limited usefulness in dealing with coordination problems. Equilibrium should be a possible emergent property of a macro model focussed on coordination problems, not an a priori assumption” (personal communication 14/06/22) His work in macroeconomics has always been based on this quintessentially Keynesian view, and his efforts have been vital to keeping it alive through some very difficult times. If macroeconomics is indeed finally set for a Keynesian Recovery after a fifty-year hiatus, then this transformation will owe a huge debt to his scientific imagination and intellectual perseverance.

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