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**Three Revolutions in Macroeconomics:
Their Nature and Influence**

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David Laidler

Abstract. Harry Johnson's 1971 ideas about the factors affecting the success of the Keynesian Revolution and the Monetarist Counter-revolution are summarised and extended to the analysis of the Rational Expectations - New Classical (RE-NC) Revolution. It is then argued that, whereas Monetarism brought about a revival of the quantity theory of money from the limbo into which Keynesianism had pushed it, RE-NC modelling was responsible for that theory's most recent disappearance. This happened despite the fact that, initially, RE-NC economics appeared to be a mainly technical extension and refinement of Monetarism, rather than a radically new economic doctrine. Some implications of this story for today's macroeconomics are briefly discussed.

Key Words: Keynesianism, Monetarism, Rational expectations, New Classical economics, Quantity Theory, Money, Velocity, Monetary policy, Inflation, Unemployment, Business cycle, Phillips curve.

JEL Classifications. B21, E31, E41, E52.

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Macroeconomics is prone to “revolutions” - episodes of rapid change in majority opinion about how the economy functions, what policy can do about it, and, more fundamentally, about what analytic and empirical methods should be used to address these questions.¹ As such episodes unfold, certain ideas, sometimes groups of ideas, disappear for a while, only to reappear later. The quantity theory of money – the proposition that variations in the general level of prices or, a little more generally, in the nominal value of national income, are the consequence of variation in the quantity of money in circulation - and associated analyses of the mechanisms through which such effects are transmitted have been a case in point since the 16th century. Most recently, having faded from view in the late ‘30s and reappeared in the ‘50s, these ideas moved back into the shadows in the ‘90s, a cycle associated with three successive revolutions, Keynesian, Monetarist, and Rational Expectations - also often labelled New Classical. This essay discusses certain common characteristics of these revolutions and their influence, not least on the quantity theory’s career. It is not the work of a dispassionate historian. As an anything but disinterested macroeconomist, its author was a participant in some of the episodes discussed and an attentive spectator at others, and has argued on many occasions that macroeconomics took a wrong turn in the 1970s.² Even so, the purpose here is to investigate why particular tracks were followed at particular times, and only secondarily to evaluate them.

Harry Johnson on Revolution and Counter-Revolution

Harry Johnson’s Richard T. Ely Lecture on “The Keynesian Revolution and the Monetarist Counter-Revolution” (Johnson 1971) is nowadays best remembered for an unfortunate *ad hominem* element. There is no doubt that, in writing it, Johnson wanted, among other things, to irritate his colleague Milton Friedman, or that he succeeded in doing so, particularly with his suggestion, extrapolated from, but also beyond, Don Patinkin’s (1969) study of the issue, that there was an element of “scholarly chicanery” (p.11) to the way in which Friedman had tried to link his ideas to an earlier Chicago tradition. But this lecture’s combative tone should not obscure its scholarly purpose³. It explored the “reasons for the speed of propagation of the monetarist counter-revolution... [and] . . . of the Keynesian revolution” (p.3), reasons which Johnson believed to be so similar that the latter could usefully be deployed as a template for analysing the former, especially when “the ‘as if’ approach of positive economics” was adopted (p. 3). This provocative allusion to Friedman’s (1953) essay notwithstanding, the parallels which Johnson noted between the two episodes were real and, crucially, much more likely reflected some rather general features of the intellectual dynamics of macroeconomics than simply an opportunistic choice of debating tactics on Friedman’s part. Johnson’s lecture thus dealt with serious questions and offered equally serious answers to them, answers that, as we shall see below, also seem to apply to the later Rational Expectations-New Classical (henceforth RE-NC) revolution.

Johnson summarised the reasons for the success of the Keynesian revolution – five of them by his own count, but they overlap -- as follows:

¹ The final, and important, phrase of this sentence is there at the urging of Peter Howitt.

² For example, earlier in David Laidler (1982), particularly essays 1 and 3, and most recently in Laidler (2011)

³ Bradford DeLong, (2005), who refers to Johnson’s “superb but not entirely fair critique of Milton Friedman” (p.128) is a rare recent commentator who has recognised this essay’s quality, as Roger Sandilands has pointed out to me. Johnson, now almost unknown among younger economists, in the 1960s and early ‘70s exerted a degree of professional dominance over macroeconomics and international economics seen neither before nor since. Donald Moggridge’s (2008) biography of this complex man is required reading for anyone seeking to understand the evolution of these areas, and indeed of economics as a profession, in the third quarter of the 20th century.

on the one hand, the existence of an important social and economic problem with which the prevailing orthodoxy was unable to cope; on the other hand a variety of characteristics that appealed to the younger generation... - notably the claim of the new theory to superior social relevance and intellectual distinction, its incorporation in a novel and confusing fashion of the valid elements of traditional theory, the opportunity it offered to bypass the system of academic seniority by challenging senior colleagues with a new and self announcedly superior scientific approach, the presentation of a new methodology that made general equilibrium theory both manageable and socially relevant, and the advancement of a new empirical relationship challenging for econometricians to estimate (pp. 6-7).

He also showed how little here had to be altered in order to account for the then recent success of Monetarism.⁴

For the Keynesian problem of mass unemployment, and the associated theoretical attack on a prevailing orthodoxy's (alleged) claim that the economy was inherently prone to return to full employment, Monetarism substituted inflation and a theoretical attack on (alleged) Keynesian claims about the irrelevance of money for macroeconomic behaviour. For Keynesianism's superior scientific approach, which starting from the primitive general equilibrium IS-LM framework had come to involve "the proliferation of larger and yet larger [econometric] models of the economic system" (p. 9), Monetarism substituted the methodology of positive economics, whose aim was to "select the crucial relationships that permit one to predict something large from something small" (p. 9). As its "new empirical relationship" Monetarism deployed the stable demand for money function in place of the Keynesian consumption function (and hence the multiplier), though this function was also one of those valid elements of existing (Keynesian) theory taken over in a "new and confusing way" In both cases, moreover, and of crucial importance, an impatient younger generation stood ready to deploy the new doctrine as a means of circumventing the authority of their older colleagues who found the analytic and quantitative techniques associated with it just a little too difficult for comfort. Closely related, their adoption of new ideas also excused them from mastering a complicated but potentially obsolete older literature.

Not that Johnson got everything right. To begin with, in addition to Friedman (1970) on "the Monetarist Counter-revolution", Johnson also cited Karl Brunner (1970) on "The 'Monetarist Revolution' in monetary theory", but he seems to have preferred Friedman's term because it made it easier for him to launch that above-mentioned *ad hominem* attack on the latter's treatment of the "Chicago tradition". Specifically, Johnson prefaced this attack by suggesting that "a revolution by definition can ignore . . . the problem of establishing some sort of continuity with the orthodoxy of the past" (p. 10), whereas a counter-revolution cannot. Perhaps so, but this was to ignore two salient facts: namely, that in (1956) Friedman did not claim that the Chicago economists of the 1930s represented any widely prevailing orthodoxy of the time, and that Keynes had also taken considerable pains to link the *General Theory* to older, albeit heretical, ideas in ways that proved controversial. Johnson thus missed an opportunity to follow up yet another element common to his two revolutions, namely a selective, even

⁴ Much has been written about the nature of the Keynesian revolution and of Monetarism since 1971, and in recounting Johnson's interpretations of them, I do not wish to commit myself to supporting them on all matters. Question of space, not to mention the importance of sticking to the point, preclude discussion of what for this essay are side-issues.

questionable, deployment of the sub-discipline's history as a means of highlighting their own importance.

Johnson's treatment of the immediate future was also unsteady. His lecture ended with a conjecture that Monetarism's success would be transitory because its central problem, inflation, was inherently less socially important than unemployment. There were two problems here. First in the course of the '70s, inflation would lead to many more political and social difficulties than Johnson had expected, and created an entrée into practical policy making for Monetarist ideas. Second, quite inexplicably, Johnson on this occasion overlooked the facts that, in Chapter 7 of their (1963) *Monetary History of the United States* Friedman and Schwartz's treatment of the Great Contraction had already staked Monetarism's claim to be able to deal with mass unemployment in a more satisfactory way than Keynesian economics, and that this book was already well on its way to becoming the most enduringly influential work in Monetarism's quest to re-establish the importance of money in general, and of the quantity theory in particular.

But Monetarism's success was short-lived nevertheless, not because of a Keynesian revival, the admirable efforts of Axel Leijonhufvud (1968) - acknowledged by Johnson - to set one in motion notwithstanding. What did the trick were, first of all the slow but steady erosion of its core claims about the stability of the demand for money - and hence of the velocity of circulation to use the traditional language of the quantity theory -- in the face of accumulating empirical evidence, and second, more decisively, the success of the third of the revolutions mentioned above, namely the RE-NC revolution. This was already breaking out as Johnson wrote, and would fit his template very nearly as closely as did its two predecessors.

The Rise and Decline of Monetarism

New economic ideas, once they catch on, are usually found to have been anticipated long before they came to general attention, but subject to this qualification, it is safe to identify the beginning of the Monetarist counter-revolution with the 1956 publication of *Studies in the Quantity Theory of Money* - a mere three years, be it noted, after Alvin Hansen (1953) had finally established the hegemony of the IS-LM version of Keynesian theory in the undergraduate curriculum. This volume's opening "Restatement" of the quantity theory by Friedman himself (Friedman 1956) laid out the core claims of the new doctrine.

For Friedman, the quantity theory was not a theory of the price level, or of nominal income, but of the demand for money. Crucially, he suggested that this demand, which could most conveniently be modelled as one for real balances, was a stable function of a few arguments. The quantity theory, as a theory of the price level or nominal income, had often been formulated explicitly in terms of the supply and demand for money in earlier years, and there were also partial precedents, some of them awkwardly in the writings of Keynes (1930, 1936), for analyzing the demand side of the relationship in portfolio choice terms, as Friedman did. Nevertheless, it was Johnson's judgement in (1962) that "Friedman's application to monetary theory of the basic principle of capital theory - that income is the yield on capital and capital the present value of income - is probably the most important development in monetary theory since Keynes' *General Theory*" (p.33).

As Johnson explained, this application led in turn to the insight that "the concept of 'income' relevant to monetary analysis should correspond to the expected yield on wealth rather than the conventions of national income accounting. This is a concept Friedman has elaborated under the name of 'permanent income'" (p. 33). And in (1959) Friedman deployed precisely this concept in support of claims about the empirical stability of an extremely parsimoniously

formulated version of his (1956) demand function, arguing that, once due allowance was made for variations in the relationship between permanent income and the measures of current national income used in conventional measures of the income velocity of circulation, he had been unable to find any systematic influence of the level of interest rates on velocity at annual frequencies. He thus flatly contradicted the standard textbook treatments of the period which routinely postulated that the demand for money was highly sensitive to the rate of interest and perhaps unstable as well.⁵ Friedman's (1959) results suggested that independently determined variations in the quantity of money alone were responsible for essentially all systematic variations in nominal national income. Such a traditional formulation of the quantity theory as a theory of nominal income – which Friedman left implicit – would have been instantly recognisable to any of that theory's exponents in earlier periods, though his claims about the empirical stability of the relationships lying behind it would surely have struck them as, at the very least, exceptionally bold. Nor did Friedman and Schwartz make any explicit claims about the place of this version of the quantity theory in their (1963) narrative history, but at least two reviewers – Johnson (1965) and James Tobin (1965) – not only thought that they discerned its systematic deployment there, but also conceded, somewhat reluctantly, that it seemed to match a great deal of evidence surprisingly well.

Friedman's (1956) theoretical formulation of the demand for money function had nevertheless paid careful attention to a potential role for the opportunity cost of holding money, as measured by a representative nominal interest rate as well as the expected inflation rate. The 1960s' history of econometric work on the relationship, which as Johnson noted in (1971), provided a professionally fruitful research agenda for many ambitious young economists, is of a partial but distinct retreat from Friedman's (1959) empirical formulation. On the one hand, it soon became clear that Friedman had been unable to find interest rate effects on the demand for money because he had not looked hard enough. On the other, in the 1960s, the demand for money did indeed seem to be a stable function of only a few arguments, including the interest rate, and there was no support for Keynesian conjectures about extreme sensitivity in the latter relationship. Those who were still unwilling to take the quantity theory seriously as representation of the determination of money income thus had to resort to an argument that had been also been advanced by sceptics during earlier episodes in its long life – notably by 19th century exponents of so-called “Banking School” ideas. This had it that the quantity theory read causality in the wrong direction, that monetary institutions worked so that variations in nominal income (or prices) caused variations in money and not *vice versa*. Nicholas Kaldor, Joan Robinson, and James Tobin would all deploy versions of this argument in papers published in 1970.

Be that as it may, in the '70s the inflation that had begun in the mid-'60s gathered momentum across advanced countries and policy makers started to pay attention to controlling money growth. At precisely this time, however, the apparently well-determined demand for money functions upon which they were now relying began to show signs of empirical instability. Three interdependent forces seem to have been at work here.

First there was what would soon be called “the Lucas critique”, which argued quite generally that, as any policy regime changed, private sector agents would take note and adapt

⁵ The British Radcliffe Committee's (1959) striking characterization of velocity as a “mere statistic”, the result of dividing one number, nominal national income, by another, the quantity of money to which it was completely unrelated in any behavioural sense, was a limiting case of this point of view.

their behaviour to the new environment, thus causing apparently stable empirical relationships among economic variables to shift. Second there was the closely related Goodhart's law which, as first formulated, emphasised that the reactions of policy makers to increased public focus on the behaviour of the monetary variables under their control would change the policy-induced behaviour of those variables.⁶ And finally there was the more general tendency for monetary systems to evolve over time in ways that alter the role played in the economic system by the particular financial instruments that are included in (or excluded from) variously specified monetary aggregates.⁷ Those who had constructed the historical time series for various measures of the money supply used in all those successful 1960s studies of the demand for money had looked backwards to ensure that aggregates constructed from *earlier* data conformed to *then current* notions of what constituted money. To the extent that they had done their work well, and apparently they had, they produced conceptually consistent series well calculated to impart stability to demand functions estimated using them. But, when, with the passage of time, monetary institutions continued to evolve but *no longer current* notions of what was and was not money continued to be used to *extend forward* the relevant time series, empirical relationships were likely to begin to drift. *Ad hoc* adjustments began to be continually required to allow for new developments as they were updated. And of course, the evolution of monetary institutions in the 1960s and 1970s was hardly independent of the ever changing incentives to financial innovation that inflation and efforts to deal with it, not to mention a new politically-driven fashion for deregulation, were creating.

Monetarists had been aware from the outset that defining money could be problematic, but they had paid insufficient attention to the fact that the problems in question were likely always to be evolving. The full significance of a comment by Kenneth Boulding (1969, p.55) on Laidler (1969) would only be fully grasped as the 1970s progressed. As Boulding all too accurately put it

We must have a good definition of money
For if we do not then what have we got
But a quantity theory of no-one knows what
And this would be almost too true to be funny

By the early '80s, it was indeed no longer clear to anyone just what we had got, so the stable demand for money function was in a great deal of empirical trouble, and Monetarism along with it. It was no accident that the word *Problems* was added to the subtitle of the third edition of a book originally published in 1969 as *The Demand for Money: Theories and Evidence* – See Laidler (1985).

Nevertheless, the end of the quantity theory's monetarist career was not an inevitable result of these problems. Perhaps it might even have survived Friedman's celebrated false predictions about the imminent return of double digit inflation in the light of money growth data in the early '80s (see. e.g. Friedman 1984), though, given Friedman's extremely visible association with the agenda of that well-known composite politician "Ronald Thatcher", these did the theory disproportionate amount of harm among politically aware spectators. Even so, earlier formulations of the quantity theory had always been qualified by an "other things equal" clause and a warning that these, not least velocity, were likely to be anything but equal in practice. A reversion to this admittedly *ad hoc* way of arguing might have protected the theory

⁶ The early history of Goodhart's law is documented by Alec Chrystal and Paul Mizen (2003)

⁷ Some of these factors received a preliminary examination from Michael Bordo and Lars Jonung (1981), and a more thorough one from the same authors in (1987).

from the worst consequences of its apparent empirical failings at this time.⁸ After all, as Friedman himself often argued, adverse empirical evidence alone is not enough reason for a theory to be given up; there must also be another theory to turn to. In this case, it was not just another theory, but another revolution in macroeconomic thinking that decisively disposed of Monetarism and eventually of the quantity theory too.

The Rational Expectations - New Classical Revolution

The RE-NC revolution had specific roots in two papers on expectations by John Muth (1960, 1961), and more general roots in the energetic search for “micro-foundations” for Macroeconomics that had begun to engage so many economists in the 1960s.⁹ Nevertheless it is usual to cite Sargent (1971) - “A note on the ‘accelerationist’ controversy” and Lucas (1972) - “Expectations and the neutrality of money” as marking the revolution’s beginning. Similarly, the (1981) publication of Lucas and Sargent (eds.) *Rational Expectations and Econometric Practice* by common consent marked its clear triumph. As we shall now argue, the extraordinary change in macroeconomics that took place in this brief interval fits Johnson’s (1971) template closely.

First of all, this third revolution had its own distinctive and socially important policy problem. Where Keynesianism had unemployment and Monetarism inflation, RE-NC economics confronted the co-existence and interaction of these two variables. In the 1970s “stagflation” as it came to be called not only needed explaining, but also posed serious policy questions about what the consequences for unemployment of trying to restrain inflation by monetary means would be. RE-NC economics addressed these issues with the help of a central idea of undoubted distinction and novelty: namely, that forward looking maximising agents would form the expectations upon which their choices depended by applying to the problem an understanding, based on economic theory, of how the relevant variables would indeed behave. Expectations deployed by the economist in modelling the economy would henceforth “depend, in a proper way, on the same things that economic theory says actually determine that variable” (Sargent and Wallace 1973) The Keynesian revolution had treated expectations as exogenous to its models – a step forward in its day, given the analytic techniques available to most economists, - and Monetarist analysis had typically extrapolated them from the past behaviour of the relevant variables – another technique driven advance - albeit often subject to specific adjustments that seemed relevant to particular circumstances. To treat their formation as an integral component of a broader maximising decision-making process was a powerfully simplifying development, again made possible by advances in technique.

The rational expectations hypothesis also had a wide range of applications, as was evident from the beginning. Sargent’s (1971) note was concerned with its implications for the interpretation of the econometric results involving distributed lags, when these were being

⁸ Theoretically based Divisia aggregation, whereby the growth rates of individual components of a monetary aggregate are weighted by their user cost (the difference between their own rate of return and a representative rate on a non-monetary asset) offered another possible way forward here. See William Barnett (1980) for an early exposition, and Michael Belongia (1996) for a statement of the case that some attention to the behaviour of Divisia aggregates in the early 1980s could have saved Friedman from considerable embarrassment at that time. The approach remained a minority taste, partly as a by-product of the general neglect of money that followed from developments to be discussed in the next two sections of this essay.

⁹ This search, which focussed on the interactions of inflation, output and employment, engaged the attention of Keynesians such as Leijonhufvud (1968) as well as monetarists. The still well-known “Phelps volume” (Edmund Phelps (ed.) 1970) is the *locus classicus* for the results it had achieved on the eve of the Rational Expectations Revolution.

applied to giving empirical content to the concept of expected inflation. Lucas's (1972) paper used abstract general equilibrium analysis – a variation on the real Walrasian thing, not the simplified IS-LM version that, according to Johnson, had represented this approach in the Keynesian revolution – to pose a fundamental theoretical question: namely, how could an economy made up of competitive markets that always cleared nevertheless generate fluctuations in income and employment? It showed that this could happen because, in the absence of full information about all individual prices, rationally formed expectations about the overall price level could still be subject to errors that would then lead agents into voluntary and mutually consistent decisions about quantities, different from those that would be made and executed under full information.

The third element in Johnson's template was the incorporation of valid ideas from the previous orthodoxy into the new framework, albeit in a "new and confusing way". If we treat Monetarism as the previous orthodoxy, the RE–NC revolution did this as well. It insisted, as had Monetarism, that any apparent inflation-unemployment trade-off was a transient phenomenon, and that it was impossible to use monetary policy to bring about systematic changes in real variables. As we shall see below, however, its derivation of these older propositions was modified in a way which, if it not inherently confusing, certainly confused many people at the time.

Johnson also argued that the success of a new idea in creating a revolution would depend on its providing younger economists with an opportunity to escape from the intellectual authority of past work by deploying novel analytic and empirical techniques that were just a little too difficult for most of their senior colleagues to master. RE-NC analysis provided such opportunities in abundance. Lucas (1972) was a seminal development in the process whereby models based on the idea of maximising behaviour co-ordinated by clearing markets that had long since come to dominate microeconomics also became the basis of macroeconomics, thus rendering two or three generations of literature based on variants of IS-LM obsolete and seemingly redundant.

Also, it was implicit in Sargent (1971), and soon to be made both explicit and general by Lucas (1976) that much earlier econometric work could also safely be neglected if expectations were indeed endogenous with respect to policy impulses, and if the same expectations at work in one sector of the economy were simultaneously at work in all the others. Large Keynesian policy-oriented econometric models had routinely neglected these matters, and hence were not to be trusted to reveal anything about the economy's structure that could be used as basis for decision-making. Results obtained by deploying the simpler single equation techniques favoured by monetarists committed to the "much from little" methodology of positive economics were equally problematic. What was needed in empirical work was the deployment of full-information techniques capable of teasing out information about the "deep parameters" of utility and production functions from general equilibrium systems, because only these were invariant to changes in policy regime. Given that such parameters characterise individual agents and production processes, the search for them would always be a dubious enterprise, despite the deployment of that methodological fiction, the "representative agent". As it happened, work on what would come to be called the "Sonnenschein-Mantel-Debreu theorem" about aggregation also began in the early 1970s, and thirty years later, S Abu Turab Riztvi (2006) would summarize its outcome as follows: "If the only data we have access to are at the aggregate level, general equilibrium theory does not generate refutable restrictions" (p.247)

These problems notwithstanding, and macroeconomics by and large ignored them, to embrace the RE-NC revolution was not only to be relieved of the need to read virtually anything written about macroeconomics before the early 1970s, but also and more importantly, to be handed a large menu of new research topics as the whole sub-discipline was reconstructed from the foundations up, using new techniques.¹⁰ The majority of an older generation of macroeconomists, whose education in modern microeconomics had often been perfunctory, and whose intellectual capital in econometrics was concentrated on applications to large models or particular single equations, rather than theoretical issues in their estimation, were at a disadvantage when it came to contributing to such an enterprise. Small wonder that younger economists – and an unusually large number of them too, thanks to the demographics of the baby-boom – were soon dominating the journals. In this respect RE-NC economics met this criterion of Johnson’s for revolutionary success far more comprehensively than Keynesianism and Monetarism ever had.

Only Johnson’s final requirement, that a revolution should provide younger members of the profession with a new and simple empirical relationship for econometric investigation, does not fit this story. To begin on a superficial level, who needed a new equation when all the old equations in the economic system were now up for re-examination? But a second deeper factor was also at work here. The RE-NC revolution’s two predecessors had used new analytic techniques to develop and test novel empirical generalisations about how the economy worked, and then deployed these in pragmatic policy commentary, never straying too far from constraints imposed by observed behaviour in the process. This third revolution, in contrast, was more about understanding this behaviour’s roots in a microeconomic theory based upon the postulates that “markets clear and agents act in their own self-interest” (Lucas and Sargent 1978), and about applying new purpose-built analytic techniques in the process. Its exponents were therefore willing to stray further from the data than their predecessors. Indeed, in their emphasis on theoretical methods they were “counter-revolutionaries” in exactly the sense that Johnson had applied the phrase to Friedman, believing as they did that, before being interrupted in the 1930s by the Great Depression and Keynes, economics had always been about working out with slowly improving techniques the implications of rational choice and clearing markets. This view is surely mistaken (see, e.g., Laidler (2011) for a statement of this case), but it is still widely held.

Monetarist Ideas, RE-NC Economics, and Awkward Facts

If Monetarism began in earnest in 1956, it was only in 1968, with the publication of Friedman’s AEA Presidential address on “The role of monetary policy” that its intellectual structure became more or less complete. Here Friedman reformulated the Phillips curve, at that time a widely accepted inverse relationship between the inflation rate and the level of unemployment (or the deviation of output from a long-run norm at which the price level would be stable) by adding the expected rate of inflation to its right hand side. He argued that the latter variable’s coefficient should be unity and that its value would be determined by agents capable of learning from experience. Thus, he concluded, monetary policy systematically aimed at keeping output above the abovementioned norm (and hence unemployment below the level associated with it) would lead to ever accelerating and hence unsustainable inflation.

¹⁰ The microeconomic literature on aggregation was initially technically difficult and hence inaccessible to non-specialists, but this excuse for ignoring it soon wore thin. Allan Kirman’s (1992) “Whom or what does the representative agent represent” is a model of non-technical clarity, and appeared in a journal with unimpeachable claims to a wide readership. .

These ideas made the explanation of “stagflation” possible as the outcome of monetary forces alone, without recourse to then popular ideas about “cost-push” inflation and the role of commodity price “shocks”, and they supported the proposition that monetary policy might as well be deployed to providing price (or perhaps inflation) stability, since it was incapable of delivering anything else in the long run. It was precisely these already existing ideas, mentioned earlier, that the RE-NC revolution incorporated in a “new and confusing way”. Indeed they figured prominently enough in its earlier manifestations that many observers treated it as a simple extension of Monetarism – “Monetarism Mark 2” as Tobin would term it as late as (1981). Lucas, Sargent et al. perhaps themselves thought at the outset that they were merely clarifying Friedman’s work by reformulating it using more rigorous techniques and a new way of modelling expectations, and the fact that the latter also suggested an optimistic answer to the immediate policy dilemma posed by stagflation, namely that pre-announced contractionary monetary policy would reduce inflation with essentially no adverse side effects on unemployment, only strengthened the widespread impression that this was so. Nevertheless, the way in which RE-NC economics set about refining monetarist analysis ended up thoroughly detaching it from this starting point.

The factor that initially caused so much confusion about these matters was that two very different interpretations of the expectations-augmented Phillips curve had co-existed uncomfortably in the literature, since the appearance Friedman’s (1968) paper, without being clearly recognised as such¹¹. The first had it that this curve was an equation describing the disequilibrium adjustment of money wages (or prices) to excess market demand – Richard Lipsey (1960) had most fully expounded this approach – rendered more sophisticated by the addition of the expected inflation rate to its right hand side. The second interpreted it as a particular way of writing down the economy’s aggregate supply curve. The RE-NC revolutionaries were concerned to restore the links between macroeconomics and the fundamental micro-postulates of maximising behaviour and clearing markets, so they naturally opted for this second interpretation, as, belatedly, would Friedman in (1975).

Furthermore Lucas’s so-called “money-supply surprise” model of cyclical fluctuations and inflation, which dominated so much of their early work, combined this aggregate supply curve with a quantity theory based demand side – also taken over from Monetarism of course – to produce a system that looked very Friedmanesque indeed. Only the incorporation of the rational expectations idea in the form of a postulate that expected inflation would depend upon agents’ ideas about the future time path of the money supply, and its corollary that only surprising, or unexpected (and hence unsystematic) fluctuations in money growth could create fluctuations in real variables, a point of which Sargent and Wallace (1976) would make so much, set it apart from earlier Monetarist orthodoxy. Most observers welcomed these changes as adding clarity to the analysis, apparently without at first noticing that they were also changing its fundamentals. It was only over the next few years that it became apparent that this system in fact incorporated two violent breaks with preceding orthodoxy when it came to the relationship of macroeconomic theory to the facts of economic life.

First, a key foundation of Monetarism was the empirical observation that monetary policy seemed to affect real income and employment, and eventually inflation, with a “long and

¹¹ Once again, Phelps (ed.) (1970), already cited above (fn. 9), contains a representative sample of this literature in which both interpretations of the curve are to be found.. Laidler and Parkin’s (1975) “Inflation a Survey” documents the presence of these two approaches, but shows little awareness of the tensions between them , or of the fundamental changes in macroeconomic thought that RE-NC economics adoption of one of them would bring about.

variable lag” – anything from one to two years, and sometimes even longer. Second and closely related, a central element in the Monetarist research agenda, as with any variant of quantity theoretic analysis, was the elaboration and investigation of the transmission mechanism through which these effects were propagated. More specifically, this mechanism was analysed as running from central bank policy actions through the banking system to changes in the growth rate of the money supply, and thence into markets for financial and also real assets such as consumer durables, business inventories and fixed investment, with the resulting changes in aggregate demand, output and employment eventually affecting the inflation rate. Monetarist work on these matters, including be it explicitly noted on the role that reverse causation might play in the story, was already well under-way in the early 1960s. Johnson’s (1962 pp. 44-51) account of it ends with a lengthy and still worth-reading passage from Karl Brunner (1961), and this line of enquiry would persist well into the 1980s, albeit with much diminished intensity. The details of all this work are not important in the current context. What matters is the simple fact that, as we shall now see, the RE-NC revolution was inattentive to the time patterns in the data when it constructed its early theoretical models, and hence squeezed questions about the transmission mechanism out of macroeconomic discourse. In due course these characteristics would drive out the quantity theory too.

More specifically the revolution’s NC component – flexible-price market-clearing models of the interaction of money, inflation and the cycle - had quantities responding only to price level surprises, and hence required quantity fluctuations to occur simultaneously with, or even lag behind price fluctuations. Their logical structure, therefore, could neither accommodate the data generated by those long and variable time lags between monetary impulses and inflationary responses, nor provide any analytic capacity to investigate the logic of Monetarist questions about the nature of the linkages at work here, many of which lay in the responses of asset markets to portfolio disequilibria.¹² Furthermore, then as now, reasonably accurate money supply data were available on an essentially contemporaneous basis in the US economy, and the revolution’s RE component required that this simple fact be recognised and its implications, for the behaviour of agents who believed in a model in which prices would respond immediately to monetary shocks that they knew about, be taken seriously when data were confronted.

By the end of the 1970’s, systematic empirical work had begun to confirm the seriousness of these tensions. It will suffice here to cite two notable studies. Robert J. Barro (1979b) made heroic efforts to estimate a plausible empirical version of the money-supply surprise model using U.S. data, but ended up with a system in which prices seemed to react at once to anticipated changes in money, but took two years to respond unanticipated ones, despite the almost immediate availability of after-the-fact information about the latter. And John Boschen and Herschel Grossman (1982) tested more directly another implication of the availability of such information: namely, that the only surprises with potential real effects should have been those created by reporting errors in initially published data that were corrected

¹² In the 1970s, macroeconomics had begun to break out of the IS-LM straightjacket and pay attention to modelling the explicit dynamics of the transmission mechanism, following leads given by Alan Walters (1967) George Akerlof (1973) and Rex Bergstrom and Clifford Wymer (1974). This work, much of it concerned with what came to be called “buffer stock” effects, had the potential to bridge the gap between complete macro econometric models with Keynesian roots and single equation estimation in the monetarist tradition. See, for example, Peter Jonson, Eleanor Moses and Wymer (1976) on a Reserve Bank of Australia model and James Davidson and Jonathan Ireland (1987) on the NIESR model of the UK. Such work would run into difficulties with instability issues associated with the demand for money function, but its problems were empirical, not conceptual, unless seen from the standpoint of the equilibrium modelling associated with the RE-NC Revolution.

subsequently. Their work clearly revealed, however, that reporting errors explained nothing, but that variations in the money supply that everyone ought to have known about and to which prices alone should have responded at once, remained stubbornly related to subsequent changes in output and to even later ones in the price level, just as they had long before anyone had heard of rational expectations.

The Demise of the Quantity Theory

It is clear that these empirical problems were the result of applying RE–NC principles to a model incorporating the quantity theory of money, and need not have damaged the quantity theory itself. Even so, they laid the groundwork for the next and final stage of that theory’s disappearance from macroeconomics.

At first this outcome seemed anything but inevitable. Laidler (1982) for example argued that, in the light of the RE-NC revolution’s emerging difficulties, Monetarism remained useful, while Peter Howitt (1986) suggested that a “Keynesian recovery” in macroeconomics was called for. The former would have seen macroeconomics move back into closer contact with empirical evidence and immediate policy-relevance. The latter would have offered an alternative set of microeconomic foundations for macroeconomics, which would have given a central role to the monetary system rather than to Walrasian markets, in the dissemination of information among agents and the co-ordination of their decisions. These had already been sketched out by Leijonhufvud in (1968), and developed further, even after the early successes of the RE-NC revolution had rendered such work unfashionable.¹³ Moreover, they were potentially complementary to the Monetarist analysis of the transmission mechanism for which RE-NC economics had provided no space, and hence might have provided micro-foundations for a macroeconomics in which the quantity theory still had a place.

But none of this would transpire, because two other lines of escape from the empirical impasse that RE-NC economics faced in the early 1980s were also available. These required neither a retreat from, nor even a major revision of, by then deeply entrenched ideas about how to conduct policy-relevant macroeconomic research, so they proved a great deal more popular than any alternatives. Their success would ensure the quantity theory’s final demise and would also confirm the exclusion from macroeconomics of the detailed analysis of the monetary system which an understanding of that theory’s associated transmission mechanism had required.

First of all, Stanley Fischer (1977) along with Edmund Phelps and John Taylor (1977) had already shown that the introduction of nominal wage and/or price contracts into an otherwise RE–NC economic system could delay inflation’s reaction to monetary impulses and force some of the initial adjustment onto real variables, hence bringing models so constructed back into contact with the facts. Even though some continued to be convinced by Robert J. Barro’s (1979a) view that to postulate anything other than perfect price flexibility was to resort to “*ad hoc non theory*” and hence not a path to be followed, those with a particular concern to produce practical policy relevant analysis nevertheless did so in increasing numbers.¹⁴ The resulting literature was one key strand in the story that would ultimately lead, through the work of, among

¹³ Leijonhufvud (1981) and Howitt (1990) are representative collections of these authors’ work on such questions, dating back to the late 1960s in the first case and the early 1970s in the second..

¹⁴ This development is often, following Michael Parkin, (1982), referred to as *New Keynesian theory*. This label refers to the role played by wage-price stickiness in the relevant analysis. Whether Keynes (1936) himself relied on this postulate is debatable, but it was certainly a central feature of what had come to be called Keynesian economics by the 1950s.

many others, Guillermo Calvo (1983), Taylor (e.g. 1993), and Bennett McCallum (e.g. 1999), to Michael Woodford's (2003) masterly analysis of what by then had become the dominant monetary policy model. In this model, like all its predecessors, and Barro style purist objections notwithstanding, "the delays involved before the next time prices are reconsidered (or perhaps, before a newly chosen price takes effect) are taken to be an institutional fact, just like available production technology" (Woodford, 2003, p.7). But Woodford's model nevertheless shunned those ideas, long associated with price stickiness, about market disequilibrium which had dominated not only the-old Keynesian macroeconomics in the 1960s, as Woodford noted, but also Leijonhufvud's "economics of Keynes", not to mention 1970s Monetarist analyses of monetary policy's transmission mechanism, as he did not.

This characteristic reflected the influence of the second escape route taken by many devotees of RE-NC economics in the early 1980s, namely that offered by Finn Kydland and Edward Prescott's (1982) "Real Business Cycle" (RBC) theory, which offered an explanation of fluctuations in real variables that precluded the need for any compromise with price stickiness, let alone with ideas about markets that failed to clear. Their extension of neoclassical growth theory into an integrated model of growth and the cycle provided new research topics for those already skilled in the relevant analytic techniques. It also brought with it into macroeconomics from microeconomics, a new empirical technique – numerical calibration of general equilibrium systems – just at the time when the preferred older one – full information econometric estimation – was proving itself overly demanding by rejecting too many theoretically promising models. More important to the current story, however, so long as the dubious microeconomic basis of highly aggregated production functions was overlooked, RBC theory enabled all the basic theoretical principles that had made RE-NC modelling so attractive in the first place to be maintained, while allowing irritating evidence about the cyclical behaviour of money and the price level to be circumvented.¹⁵

Initially this theory treated such evidence as irrelevant to the problem of explaining cycles in real variables and hence ignored it, but very soon Robert King and Charles Plosser (1984) dealt the potential significance of this evidence an even more telling blow. They integrated a monetary system into an RBC model in such a way as to ensure that fluctuations in the money supply – predominantly in their scheme, a matter of variations in the quantity of "inside money" created by banks – a response to, rather than a determinant of, cyclical fluctuations in the real economy. As they summarised the matter "This paper integrates money and banking with real business cycle theory. The result is a class of models that can account for the correlation between money and business cycles in terms that most economists would label 'reverse causation'" (p. 363). Thus, an alternative to Monetarist analysis of the transmission mechanism that had recently been the sole property of Keynesians and, in particular, post-Keynesians, none of whom were particularly fond of an economics based on the ideas of rational self-interest and clearing markets, was claimed by a group whose devotion to those very same principles was as unquestioning as the history of economic thought has ever seen,

To those not overly concerned with policy relevant modelling, and unwilling to rescue the money-supply surprise model by resorting to *ad hoc* fixes involving arbitrary assumptions about wage and price stickiness, the appeal of RBC theory was compelling, and it enabled the RE-NC revolution to take on a new lease on life vigorous enough to persuade some

¹⁵ There is a perfect parallel between the proclivity of macroeconomics to ignore aggregation difficulties in this context and with respect to idea of the representative agent. See fn 10, above. For a retrospective discussion of the so-called Cambridge controversies on this issue, see Avi Cohen and Geoffrey Harcourt (2003)

commentators (e.g. Brian Snowden, Howard Vane and Peter Wynarczyk 1994) to treat it as a separate and distinct stage in the development of macroeconomics. But RE-NC economics' incorporation at this stage in its development of the reverse causation view of the linkage between the macro-economy and the monetary sector also conveyed an all too persuasive message about the value of further research on money in particular and the workings of financial institutions in general. If the role of these institutions was simply to adapt the quantity of the money to whatever variations in the economy's demand for it might emerge, with no subsequent feedbacks to be considered, why bother to try to incorporate their workings into macroeconomic models?

In the 1980s, moreover this same message also began to resonate among practical policy analysts and makers, who were on the lookout for a regime to replace money growth targeting, which was by then judged to have failed in place after place in the face of the institutional instabilities discussed earlier.¹⁶ Central banks had in any case never given up conducting monetary policy by manipulating short run interest rates even when they were targeting money growth, so an approach that took pragmatic account of wage and price stickiness, deployed the latest in dynamic stochastic modelling techniques taken over from RBC theory, but treated the quantity of money as a passively determined residual variable of no causative significance and hence of no further interest, was just what they were looking for. This, then, was another strand, parallel to that started by Fischer (1977) and Phelps and Taylor (1977), in the story that ultimately led to "Monetarism without money", inflation targeting supported by essentially the analytic framework that would be systematised and expounded by Woodford (2003). The aptness of this label will not be discussed here, other than to note that its deployment of the words "without money" all too clearly marked the final return of the quantity theory to the intellectual limbo from which it had emerged in the 1950s.¹⁷

Macroeconomics without Money

True to the traditions of the RE-NC revolution, of whose maturity they were a product, the dynamic stochastic general equilibrium (DSGE) models that underpinned Monetarism without money paid no attention to those questions about monetary policy's transmission mechanism that had figured so prominently in monetarist analysis proper. The generic form of such models had variations in a policy determined interest rate directly influence expenditure, and thence, by way of an "output gap" (the outcome of voluntary and fully co-ordinated behaviour) move the inflation rate relative to its rationally-expected level. Intervening behaviour in asset markets was simply ignored. It was not, of course, that economics more broadly neglected the study of financial institutions and markets. Much attention was paid to the so-called "equity premium puzzle", and a whole literature on bank runs developed as well. So did "new" theories of money, one of which sought the micro-foundations of its existence in the "backing" offered to it by its status as a claim on income generated by real assets, or by the tax revenues available to the

¹⁶ Interestingly though, the two countries that persevered hardest and longest with money growth targets, Switzerland and Germany, also experienced the least serious, albeit still significant, inflations of the 1970s and 1980s. Once again, there is more to be said about a question than space permits.

¹⁷ The phrase seems to have originated in the 1980s. The earliest use of it that I have been able to find was by Frank E. Morris (1983), then President of the Federal Reserve Bank of Boston, as the title of a paper delivered at one of the many conferences of that time dealing with the confused and confusing state of monetary policy in the United States.

authorities who issued it, and another which sought them in search theory.¹⁸ More generally the field of “finance” flourished as never before as a sub-discipline in its own right. But all these developments took place in isolation from mainstream macroeconomics, which continued on its way without help from the analysis of monetary and financial institutions and asset markets.

For a while, such help did not seem to be needed. Monetarism without money delivered an influential way of thinking about and executing monetary policy whose close temporal association with the so-called “Great Moderation” of the decade or so before 2007 was probably no coincidence. Thus, the stability of this period seemed to confirm the first part of a famous dictum of John Stuart Mill, quoted by Friedman in (1968): namely that “There cannot. . . be intrinsically a more insignificant thing, in the economy of society, than money. It is a machine for doing quickly and commodiously, what would be done, though less quickly and commodiously, without it . . .”; but Mill had had more than this to say on the matter, which Monetarism without money overlooked. Specifically: “and like many other kinds of machinery, it only exerts a direct and independent influence of its own when it gets out of order”, a qualification on which Friedman (1968) had elaborated as follows

True, money is only a machine, but it is an extraordinarily efficient machine. Without it we could not have begun to attain the astonishing growth in output and level of living we have experienced in the last two centuries – any more than we could have done without those other marvellous machines that dot our countryside and enable us, for the most part, simply to do more efficiently what could be done without them at much greater cost in labour

But money has one feature that these other machines do not share. Because it is so pervasive, when it gets out of order, it throws a monkey wrench into the operation of all the other machines. The Great Contraction is the most dramatic example, but not the only one. (1968, p. 105)

In short, the macroeconomics that dominated discourse as the Great Moderation was approaching its end, useful though it was, had limitations that turned out to be crucial. The DSGE models that lay at its theoretical heart had nothing at all to say about “a residue of things” (the phrase comes from Lucas 2003, p. 23) including financial crises and their real consequences. But in 2007, an example of this residue, potentially as serious as the Great Contraction that had helped provoke the Keynesian Revolution, became the central problem that macroeconomics needed to deal with, thus creating, following Johnson (1971), the first pre-condition for yet another revolution in macroeconomic thought: namely “the existence of an established orthodoxy which is clearly inconsistent with the most salient facts of reality” (p.3).

Final Reflections

So is the sub-discipline once more on the verge of revolution? Will the quantity theory of money and its associated analysis of the monetary transmission mechanism come back to life yet again in some new form? Or, is a long-delayed Keynesian recovery at hand at last? Or, is some amalgam of these two older approaches feasible? Current discourse is not short of discussions of these and a host of other possibilities. My own views on what is desirable have been expressed elsewhere (see e.g. Laidler 2011), but what might be desirable and what is likely to happen are sometimes very different matters, as I discovered in (1982). If there is any lesson to be drawn from the quantity theory of money’s career during the three revolutions in macroeconomics

¹⁸ Mehra and Prescott (1983), Diamond and Dybvig (1983), Sargent and Wallace (1982), and Kiyotaki and Wright (1983) respectively started the analysis of these topics.

discussed in this paper, it is, once more following Johnson, that serious empirical and policy problems are not enough to overthrow a well-entrenched orthodoxy. Indeed, more than the revival of an old theory, or even the discovery of a new one, might be required to bring about another successful revolution. The theory in question must also be attractive to impatient younger economists who are on the look-out for a central idea with some links to prevailing orthodoxy, but few enough of them to enable it to pose an array of new questions. And it would also help were this theory to arrive in the company of novel analytic and/or empirical methods. In due course we shall see whether such developments are already in progress even as this essay is being written, just as they turned out to have been when Johnson was writing his (1971) essay. But his failure then as a prophet of subsequent developments in macroeconomics warns against further forecasts at this juncture.

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