Symposium on Macroeconomics

1 Fresh Water, Salt Water, and Other Macroeconomic Elixirs*

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Viewed from a distance of 10,000 miles, the dominant features of American academic macroeconomics are the world-wide prominence of its leading creative thinkers, and the surprising insularity of its scholastic debates. Nothing seems further from the main macroeconomic concerns of the United States, not to mention of the 23 other industrialized nations that constitute the OECD, than the 'fresh water, salt water' dichotomy between new-classical and new-Keynesian traditions which has dominated the coffee-break oral tradition of American macroeconomic conferences for the past decade. I suggest here that the 'salinity criterion' no longer serves its original purpose of describing the central disputes in American macroeconomics, not only because it is no longer geographically accurate but, much more important, because (1) it leads commentators greatly to exaggerate the influence that new-classical economics ever had on the main battlefield of any academic debate—the campaign for the minds of the young, and (2) because it is no longer relevant to the central unsolved puzzles concerning macroeconomic behaviour.

Background

More than a decade has now passed since Robert Hall (1976) brilliantly christened the central schism in macroeconomics as a debate between the 'fresh water' and 'salt water' schools of thought. His nomenclature was based on the geographical location at that time of the three major developers of new-classical macroeconomics (Robert Lucas, Thomas Sargent, and Robert Barro) in four universities placed on or near bodies of fresh water, Carnegie-Mellon, Chicago, Minnesota, and Rochester, and of the major defenders of Keynesian economics at universities on either coast.1 Hall's classification scheme came at or near the high water mark of new-classical economics, which in the late 1970s was creating a near-sensation in macroeconomic conferences and classrooms, with its dramatic 'policy ineffectiveness proposition' claiming that countercyclical monetary policy rules which could be anticipated by the public could have no effect on real output. Since Hall's unpublished paper which introduced the salinity criterion is not well known, it is worth repeating here his evocative characterization (1976, p.1) of the leading figures of the day: 'To take a few examples, Sargent corresponds to distilled water, Lucas to Lake Michigan, Feldstein to the Charles River above the dam, Modigliani to the Charles below the dam, and Okun to the Salton Sea.'

The Rise

The reasons for the rise and subsequent fall of new-classical macroeconomics are well known and need be summarized here only briefly. Cycles in the acceptance of doctrines often follow the timing of economic events. Just as the undeniable facts of the Great Depression help explain the rapid spread of the original Keynesian doctrine, so the undeniable acceleration of US inflation between 1965 and 1970 quickly undermined the mid-1960s

1 Four universities get credit for three individuals, since Robert Lucas moved from Carnegie-Mellon to Chicago in 1974, immediately after writing three of the five most influential papers in the development of the new-classical economics, and immediately before the rise of the new-classical school of thought to the peak of its influence in 1976-78 (see the next footnote). My choice of the five most influential papers consists of Lucas (1972, 1973, and 1976), Sargent-Wallace (1975), and Barro (1977). I include Sargent, but not Wallace, among the founders of new-classical macroeconomics, because Sargent wrote a number of other influential pieces by himself, while Wallace did not (see also the discussion below of the citation counts).
The anti-Keynesian intellectual counter-revolution was particularly swift and powerful because of timing: within two years of Milton Friedman's 1967 Presidential Address to the American Economics Association (1968), US inflation had accelerated sufficiently to destroy the credibility of the long-run Phillips trade-off and reinstate the classical notion of long-run monetary neutrality. Soon events delivered a second blow to the tattered orthodoxy. Supply shocks, including in the US not just the first OPEC oil-price hike but also the effects of the imposition and unwinding of price controls, created a positive correlation between inflation and unemployment throughout most of the 1970s that seemed further to sabotage the negative Phillips trade-off idea. In flowery language that amounted to a simultaneous declaration of war and announcement of victory, Lucas and Sargent (1978, pp.49-50) described 'the task which faces contemporary students of the business cycle [as] that of sorting through the wreckage ... of that remarkable intellectual event called the Keynesian Revolution ...'

Since Friedman had reasoned from theory, while the Keynesians had been destroyed by facts, another implication of the late-1960s debacle was to shift the critical standards of the profession. Supporting facts were no longer enough to validate a paradigm: facts were now expected to be accompanied by a structural model of maximizing behaviour, Lucas' famous econometric 'critique' (1976), a formal demonstration that empirically estimated slope parameters were not invariant to policy interventions, both rationalized the demise of the long-run Phillips trade-off and established an 'archeological criterion' for future econometric research, which would dig below shallow policy-sensitive relations to uncover 'deep parameters' of taste and technology. This meant in practice that new-classical macroeconomics consisted of a priori theorizing in the analytically convenient setting of 'representative agent models', where one could move back and forth between the individual agent and the aggregate economy simply by adding or removing 7 subscripts, without having to consider such analytically inconvenient issues as coordination failures or the speed of price adjustment.

The original Lucas version of the new-classical macroeconomics combined the undeniable appeal of rational expectations with two more dubious assumptions inherited from Friedman (1968), that is, continuous market clearing and imperfect information, to form the foundation of the famous 'Lucas supply function' (more justly, the Friedman-Lucas supply function). Soon Sargent and Wallace (1975) extracted from Lucas' model its implication for monetary policy, the famous 'policy-ineffectiveness proposition'. The demonstration by Barro (1977) that one could interpret historical US data to be consistent with the proposition and the theory brought new-classical economics to its short-lived period of peak influence.

The Fall

Part of the downfall came early and on theoretical grounds, with the realization that real-world information lags for aggregate variables like the price level and money supply were much too short to rationalize the persistent multi-year deviations from equilibrium that seemed to characterize business cycles in most industrialized countries. The second dubious assumption, continuous market clearing, was viewed more critically once it was recognized that it was not an inextricable concomitant of rational expectations, especially when Stanley Fischer (1977) and Edmund Phelps and John Taylor (1977) showed that rational expectations could be embedded in a model containing real-world institutional features like multi-period wage and price controls to generate non-market-clearing behaviour. Once Fischer and Phelps-Taylor had shown that rational expectations by itself was a proposition, the explicit statement of the proposition was worked out by Sargent and Wallace (1975). As another difference, of the three Sargent was the most interested in developing theories and methodologies for uncovering deep parameters, and Barro much less so.

The high-water mark can be placed fairly precisely at 8:59 am EDT on Friday, 13 October 1978, at Bald Peak, New Hampshire, just before Robert Barro and Mark Rush began their presentation of an empirical test of the policy-ineffectiveness proposition on quarterly US post-war data that was not only severely criticized by three discussants, but also contained dubious results that seemed questionable even to the authors. Never again after that occasion did any prominent proponent of the central proposition of new-classical macroeconomics even attempt to present empirical evidence in its support, and soon thereafter strong evidence against the proposition was presented by Mishkin (1982) and Gordon (1982).
necessary but not a sufficient condition to validate new-classical policy conclusions, the race was on to develop the new-Keynesian theory based on rational expectations and one or another institutional impediment to continuous market clearing. The new-Keynesian theory had the double appeal not only that it seemed better to incorporate whatever adjustment costs led real-world economic agents to constrain their own price-setting behaviour by entering into explicit or implicit contractual agreements, but also that it did not require any arbitrary assumptions about information lags.

It is less widely understood that the downfall of new-classical economics was reinforced by its own empirical failure and the simultaneous empirical revival of Keynesian economics. The short-lived attempt to develop an econometric validation of the policy-ineffectiveness proposition by Barro and others was, simply, a research failure. It floundered on their inability to develop a symmetric explanation of output and price behaviour. Barro (1977) showed that output was not related to anticipated monetary changes but could not demonstrate the required corollary—the full and prompt responsiveness of prices to anticipated nominal disturbances. This failure was not a matter of arcane methodological debates: it was evident in the gross inconsistency of the new-classical bedrock assumption of a perfectly flexible aggregate price level, mandatory in a world of continuous market clearing, with the empirical reality in the postwar US of a time series for the inflation rate that was much more sticky and inertia-prone than the corresponding time series for changes in nominal aggregate demand.

Meanwhile, the effects of supply shocks were absorbed into the empirical Phillips curve literature in the late 1970s (see Gordon, 1977) through the development of a dynamic econometric analogue to the static aggregate demand and supply curves that swept the textbook market at the same time. Since supply and demand shocks entered symmetrically into the determination of the inflation rate, unemployment and inflation could be either positively or negatively correlated in the short run, depending on the dominant source of shocks. The evolution of real world events, which had undermined Keynesian economics in the late 1960s and 1970s, now came to its rescue in the form of a monetary disinflation that had been predicted to be painless by prominent new-classical advocates but quite evidently was not. The precise path of the inflation rate after 1980 depended on a particular combination of monetary disinflation, falling oil prices, and an appreciation of the dollar that no one had predicted in advance. Nevertheless, a crucial implication of the resuscitated 1980-vintage empirical Phillips curves, the value of the "sacrifice ratio" of lost output required to achieve a permanent deceleration of inflation under conditions of variable oil prices and exchange rates, turned out to be surprisingly close to predictions made in advance (Gordon-King, 1982).

New-classical economics has been undeniably influential, but not in the way that its three prominent creators originally imagined. Its most important contribution to macroeconomics, the assumption of rational expectations, was stolen almost immediately, and applied more fruitfully, by the new Keynesians. As individuals, the three primary creators of new-classical macroeconomics have long since departed for greener research pastures and have left their child, the policy-ineffectiveness proposition, to die neglected and unmourned. Two of the three, Thomas Sargent and Robert Barro, have also physically departed from the nation's fresh-water heartland, the first to a university located a few miles from one of the nation's leading salt farms, and the other to the hotbed of both the old and new versions of Keynesian economics—Cambridge, Mass. Hence my introductory comment that the fresh water, salt water distinction is no longer geographically accurate, at least as a description of the location of the main figures who created new-classical macroeconomics.

Real Business Cycle Theory

Into this vacuum has stepped Edward Prescott, from fresh-water Minnesota, who has picked up the frayed New-classical banner with his "real business-cycle theory". This is based on the core new-classical element of continuous market-clearing but generates a business cycle not from imperfect information, as in the original Lucas-Sargent version, but rather from an autoregressive technology shock process. Believe it or not, the entire explanation for recessions and even the Great Depression in this new-classical macro 'Mark II"
boils down to technological retrogression, that is, a sustained negative realization of the technology shock. Thus far Prescott and his small band of followers have understandably shielded away from any of the empirical research needed to provide support for their theory, in particular, studies of individual industries to identify the sectoral locus of technological retardation in recessions, and studies of both industry and individual price behaviour to isolate the inverse correlation between prices and output that every textbook since Marshall's *Principles* would predict to occur as the counterpart of a technological shock.

Some real-business-cycle proponents defend themselves by claiming that the correlation between prices and output is not foreordained to be negative but rather depends on how the monetary authority responds to real events. I would respond that this 'way out' for the real business cycle theorists, to attribute the positive correlation of output and prices that we observe in most historical episodes to procyclical fluctuations of the money supply, just underscores the inadequacy of the empirical research program, which thus far shows no signs of even attempting to estimate models in which output, prices, and money are jointly determined subject to cross-equation restrictions. Even a moment's consideration suggests that such a research program for new-classical macro 'Mark II' will fail, as did the attempt to validate the new-classical macro 'Mark I'.

Clearly, price behaviour has turned out to be the Achilles heel of new-classical economics, both in its policy-ineffectiveness and in its real-business-cycle incarnations. In fact, to date real business cycle theory has been 'price free', which for an economic theory is about as appealing an attribute as 'one-armed' would be for a concert pianist.

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*Fresh-Water Economics: Leaders without Followers*

While the basic elements in the rise and fall of new-classical economics are widely understood and have become part of the Conventional Wisdom, even the most perceptive commentators seem to have overstated the influence of fresh-water economics at its peak. The most persistent and articulate chronicler of the revival of Keynesian economics is Alan Blinder, who in the following passage shares the widespread misconception that around 1980 new-classical macroeconomics had swept the younger half of the profession:

By about 1980, it was hard to find an American academic macroeconomist under the age of 40 who professed to be a Keynesian. That was an astonishing intellectual turnabout in less than a decade—an intellectual revolution for sure . . . Thus freed of any need to absorb the knowledge of the past, newly minted Ph.D. economists could concentrate on what they saw as the wave of the future . . . the young were recruited disproportionately into the new-classical ranks . . . By 1980 or so, the adage 'there are no Keynesians under the age of 40' was part of the folklore of the [American] economics profession [1988, pp. 1, 14].

Was the new-classical 'intellectual revolution' really this influential in 1980? First, we need to clarify the use of words. Even those young American economists who believed firmly that markets regularly failed to clear were reluctant in the era 1975-85 to identify themselves as 'Keynesians'—that word was always offensive to me, and to other people, because of the intellectual baggage it carried, and indeed only Alan among then-young Americans carried it high. But this was just a name—on any issue of substance there was a large group which considered sticky prices and failures of market clearing to be an essential part of any satisfactory explanation of business cycles. A big breakthrough came in 1976-78, when Modigliani's Presidential Address (1977) and new macroeconomics textbooks published by Dornbusch-Fischer and myself recast the central policy debate in terms of 'monetarists' battling 'activists' or 'non-monetarists', rarely if ever mentioning the word 'Keynesian'.

It is a truism that the influence of an intellectual

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6 The movement away from the word 'Keynesian' started earlier with Okun's classic (1972) analysis of the conditions necessary for what he called 'activist' policy prescriptions.
revolution hinges on its success in the battle to capture the minds of the young. The original Keynesian revolution of 1936 was a near-total success by this criterion. The issue, then, is how influential was new-classical macroeconomics in its heyday of the late 1970s? This turns not on whether young people called themselves 'Keynesian', but on what they thought and wrote.

The original Keynesian revolution of 1936 was not carried on the citations to individual (the) Keynesians, but on what they thought and wrote. Hence economists over virtually the whole period of existence of the Social Science Citation Index (1971-85) which also, luckily, is close to the right period for analyzing the influence of new-classical economics (although the cut-off is too early to evaluate the influence of the 'Mark II' version).

Citation Counts

Nothing is more fun for US economists than to sit in their faculty clubs and chart the fall and rise of doctrines and, even better, individuals. Hence the excitement caused by the recent underground circulation of an unpublished paper by Medoff (1988) that contains by far the most complete count ever carried out on the citations to individual economists over virtually the whole period of existence of the Social Science Citation Index (1971-85) which also, luckily, is close to the right period for analyzing the influence of new-classical economics (although the cut-off is too early to evaluate the influence of the 'Mark II' version).

The Medoff evidence consists of total citation counts, with all self-citations deducted, over 1971-85 for the 150 top-ranked economists (excluding those who had won the Nobel Prize by 1985), a second ranking of the same group by total average citations divided by years since Ph.D., and a third ranking of economists who were 40 or under in 1985. One can extract several striking conclusions from these lists, which are all subject to the defect that the source (SSCI) counts citations only for the first-listed co-author and thus it significantly undercounts individuals with names late in the alphabet who have done their most significant work with co-authors. All of the following findings lead me to infer that the three founders of new-classical economics failed to attract a set of influential followers.

1 Looking at the 100 most-cited economists on the more meaningful second (age-adjusted) ranking, surprisingly few macroeconomists make the list at all, just 13 out of 100. To reach even one-fifth of the names, one would have to add five international economists and two public finance specialists who occasionally dabbled in macro.

2 The founders of new-classical economics obviously stirred up a ferment with their own work, but that of their followers did not. Barro, Lucas, and Sargent, are respectively ranked 2, 4, and 5, but there is no other name among the remaining 10 macroeconomists on the list of the top 100 who could be labelled as a new-classical proponent.

3 Only six of the 50 most-cited younger economists practise macroeconomics, and none of them could remotely be described as belonging to the new-classical group.

Conference Participants

To provide further documentary evidence on the failure of the new-classical economists to develop influential disciples, much less dominate the profession as Blinder’s interpretation would imply, I’ve collected names of authors from the three major conference volume series through which US macroeconomics is purveyed. On the left, we have the Brookings Papers on Economic Activity (BPEA), founded in 1970 by Arthur Okun and George Perry. This group is and always was Keynesian, so much so that Barro, Lucas, Prescott, and Wallace have never been invited to a single meeting of the 47 which have been held over the last 19 years.8 On the right, we have the Carnegie-Rochester conference series (CRCS), run biennially since 1973 by Karl Brunner and Alan Meltzer, which has cast a wider net than BPEA and has been particularly hospitable to the research of the fresh water macroeconomists. In the middle we have the NBER research group on macroeconomics, admirably run by Robert Hall to include new classicalists and Keynesians alike, which holds about four annual meetings.9 To decide who was who in US macroeconomics among young academics who received their Ph.D. degree in the period 1975-85, I counted from the following

7 The closest is B. McCallum, ranked 91. The others in order by the second criterion (and their ranks) are A. Blinder (20), R. Hall (22), R. Solow (26), R. Gordon (32), W. Nordhaus (36), S. Turnovsky (38), S. Fischer (50), R. Fair (66), and B. Friedman (68).

8 The names and age-adjusted ranks are A. Blinder (3), M. Darby (9), W. Buitel (14), J. Taylor (18), F. Mishkin (25), and L. Summers (35). Excluded from consideration is L. Hansen (7), whom I classify as an econometrician making primarily methodological contributions, just as I have excluded C. Sims (25) from the count of established macroeconomists.

9 Sargent invited to participate only in a single year, 1973.

10 The NBER EF group as such does not have a regular publication outlet for its numerous annual meetings and conferences. Recently a separate annual conference series under NBER auspices, run by Stanley Fischer, has been initiated with its own publication outlet, the NBER Macroeconomics Annual.
documentary material—the tables of contents of all BPEA and CRCS conference volumes published since 1981, and the programs of all NBER economic fluctuations and macroeconomics conferences held in the last four years.\(^{11}\)

Perhaps the most surprising result of trying to carry out this exercise is the same as that I reach from the topic count in the next section—far from falling neatly into two lists of ‘new classicals’ and ‘new Keynesians’, most people defy that dichotomy either because they play both sides of the street or, more importantly, the classical/Keynesian distinction is just not relevant for the type of economic fluctuations and macroeconomic conferences held in the American Economic Association (AEA) and the Conference Research Center of the National Bureau of Economic Research (CRCS) in the last four years.

In fact, I conjecture that only about half of the people on the new-classical list would identify their primary field as macroeconomics as opposed to econometrics, international economics, public finance, or microeconomics theory.

**Which are the Topics of Central Concern?**

**Topics of Recent US Research**

The attention still given in coffee breaks and written commentaries to the fresh-water, salt-water dichotomy seems to imply that the core of US macroeconomics today concerns a debate over the central assumptions of new-classical macroeconomics, that is, rational expectations, market clearing, and imperfect information, and their implications for the real effects of nominal disturbances. To evaluate this implicit proposition, I went to the place where new-classical macro should have been most dominant, the tables of contents of 15 volumes of the CRCS published since 1980, containing 67 articles that I could classify. My conclusion is that the central concerns of US macroeconomists in their actual research have not related to new-classical economics, either pro or con, over this period. Here is my count of the topics, with the number of articles in parentheses:

1. Traditional monetary economics (18): institutional aspects of monetary policy, monetary instruments, term structure, money demand, banking deregulation.

2. Real and monetary international economics (10).

3. Evaluations of economic policy in other countries (8), of which five concerned developed countries and three less developed.

4. Public finance, supply-side economics (7).

5. Inflation (7): hyperinflation, costs of inflation, inflation variability, disinflation strategy.

6. Technical issues with no policy content (7): VAR models, overlapping-generation models, pure theory of intermediation, temporal aggregation, optimal prediction.

7. Labour market and productivity (4).

This totals 61 of the 67 articles, leaving only six related to the fresh water, salt water dichotomy, and only a few of these pursue the new-classical approach. Most notably Prescott (1986).

**An Obsolete American Preoccupation?**

The fresh water, salt water dichotomy is no longer relevant to the advance of knowledge...
One response to this accusation of irrelevance is to protest that 'policy is not the only thing that macroeconomics is about; macroeconomics is also a science devoted to understanding basic economic phenomena like business cycles'. But even by this criterion, when judged by the normal standards for evaluating science, new-classical macro is a scientific failure. We have already seen that the 'Mark I' version failed empirically to explain aggregate price behaviour, a fatal flaw for a theory with pretensions to science, while the 'Mark II' version evasively has nothing to say about price behaviour. 'Yes, true', the defenders may retort, 'but a theory is valuable even if it is wrong, since thinking about why it is wrong may teach us something'. By this lower standard we finally arrive at the true contribution of new-classical macro: it has given us all something to argue about.

Redirecting Future Research

Macroeconomics needs to be redirected away from demand and toward supply, but not in the direction suggested by Prescott's attempt to build a model of real business cycles based on wholly exogenous and unexplained technology shocks. Instead, the leading supply-side puzzle concerns the world-wide slowdown in the growth rate of multi-factor productivity (MFP), sometimes called 'Solow's residual'. We need to determine what accounts for the slowdown, and for differences among the major countries. Paul Romer's 'Crazy Explanation' and its sequels have shown that research on long-run growth and productivity change can deal with these basic issues and create plenty of controversy along the way. However, the fact that people have been pondering the productivity slowdown for more than a decade without breakthrough helps to explain why so few young people are drawn to this set of research questions: it is hard, and it intrinsically involves messy empirical work that deals with differences among countries and industries.

Work also needs to be directed away from the salinity debate toward the long-run implications of the twin deficits. Ultimately we care about the deficits because the accumulation of debt has long-run consequences, and in this sense the deficits fall under the general topic heading of long-run growth and productivity. Both raise awkward questions for economists who would prefer to be tillling in the fields of homogeneous representative agent models. The US fiscal deficit is uniquely persistent for peacetime and appears to represent a genuine political innovation, raising questions about the relevance for this episode of recent theorizing on the political determinants of deficits.
The trade deficit raises a host of questions about how it will all end in view of the resistance of the deficit to a fall by half in the yen-dollar rate. Why do American firms fail to make well, or make at all, so many of the things that we import from Asia? And how does Japan manage to avoid raising its dollar prices in anything like the proportion by which the dollar has fallen? Indeed, rated by its achievement in achieving low unemployment, low inflation, and rapid productivity growth, Japan scores first by all three criteria in comparison with the US and any other of the large European nations (albeit Japan ties with West Germany on inflation). How can systematic differences in performance be incorporated in models while retaining rational behaviour? Why don't utility-maximizing agents copy whatever elements of policy management or group behaviour that underlie the Japanese success?

Extreme cases often provide an essential service in sorting among alternative theories. One such extreme case is the Great Depression, which creates suspicion of any theory like new-classical Mark II which suggests that factories and workers were idle as the result of a massive supply bottleneck. Rapid economic growth in East Asia is another extreme case that also helps to sort some elements of the productivity story that look more plausible than others. One doubts that high energy prices are a major explanation of the productivity slowdown, given the dependence of these successful countries on imported energy which should have derailed their progress but did not. Rising in plausibility is P. Romer's argument that the return to high rates of capital accumulation is greater than in standard competitive models, and the same may go for accumulation of human and managerial capital (in the pure version of Romer's 'crazy' model the elasticity of output with respect to capital input is unity and to labour input is zero). Macroeconomists still have a way to go in building and testing models of growth, debt, and accumulation before we throw in the towel and appeal to the help of other disciplines, particularly comparative sociology and religion, to explain the outstanding economic performance of East Asia.

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