Conversations with …

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Wynne Godley

Early in my career, in the early 1970s in the Department of Applied Economics at the University of Cambridge, I had the privilege of working for Wynne Godley, the then Director. He is an extraordinary man.

In his twenties Wynne Godley was a professional oboe player. Then, in 1956, when he was in his thirties, he joined H. M. Treasury, rising to Under-Secretary, and deputy director of the Economic Section. In 1970 he was appointed director of the Department of Applied Economics at the University of Cambridge, where he was to remain until 1985, and a fellow of King's College. In 1980 he was appointed Professor of Applied Economics. From 1994 to 2001 he was a Distinguished Scholar at the Levy Economics Institute in New York, and since the end of 2001 he has been a Senior Visiting Research Scholar at the Judge Institute of Management in Cambridge.

He divides his time between surveying the US, UK, and global economic situation – of which he is a brilliant observer – and developing an alternative macroeconomic theory of how monetary economies function. Much of his analysis is based on stock/flow models of the US and UK economies. These are not forecasting models in the customary sense: rather, they are ways of tracking economies through the sectoral financial balance identities. He uses these models to simulate a range of alternative futures, and then considers policies that might be appropriate over a medium term (5 - 7 year) horizon. He has recently published, with Marc Lavoie of the University of Ottawa, a book on macroeconomics[1] that aims to revive the tradition practised by the original Cambridge Keynesians, notably Nicholas Kaldor, combined with the theory of asset allocation pioneered by James Tobin.

Godley has an extraordinary mind, and a powerful one, trained by the philosopher Isaiah Berlin. However, he did not have a strong formal training in economics, and this has bedevilled him throughout his professional life. Godley once described himself as having a sort of 'verbal dyslexia' which causes him great difficulty in explaining to colleagues exactly what he thinks, and is trying to do. Yet he knows exactly. He can take a vast spreadsheet of numbers, study them for minutes, sometimes hours, at a time, and then pronounce "That figure is wrong," stabbing at it with an elegant oboist's finger. He is invariably found right. How does he know? The explanation – and it took his econometrician colleague Hashem Pesaran to recognise this – is that he has what amounts to a full macroeconomic model in his head, which, by some sort of subconscious process, he computes. Pesaran indeed helped Godley, and himself, profit from that: some of their joint papers were the product of Godley's intuition, if that is how it should be described, and Pesaran's formalism.

Godley was in his early years a deeply troubled man. In 2001 he published, in the London Review of Books, an astonishing article, *Record of a nightmare*, in which he described what he called 'a disastrous encounter with psychoanalysis'. He explained how, after an unconventional and damaging childhood – the details were extraordinary – he found himself, in his thirties, in a 'state of terrible distress', caused by living through what he termed 'an artificial self'. The condition, he explained, is difficult to recognise because it is concealed from the world, and not least from the subject, with ruthless ingenuity.

Godley explained that it was a 'paralysis of his will, rather than the pain itself', that enabled him to infer that he needed help 'different in kind from the support of friends'. He consulted D.W. Winnicott, then President of

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[1] Godley, Wynne and Lavoie, M., *Monetary Economics: An Integrated Approach to Credit, Money, Income, Production and Wealth*(Palgrave MacMillan, 2006)

the British Psychoanalytic Society, who in turn recommended psychoanalysis by Mohammed Masud Raza Khan who – it was subsequently revealed – was himself undergoing psychoanalysis, with Winnicott, slept with and abused many of his patients, including Godley, and was an alcoholic. Eventually he was disbarred, but not before, in Godley's telling, he had essentially tortured Godley in a 'long and fruitless battle culminating in a spiral of degradation'.

It is a testimony to Godley's intellectual strength that he came through all this, to produce some of the more novel and insightful analysis of economies in his generation.■

Gunnar Myrdal

Shortly after I had taken up my first academic appointment, at the University of Cambridge, in 1970, I was asked by Robert Neild, then a newly-appointed Professor Economics, if I would be interested in meeting the famous Swedish economist, sociologist, and politician Gunnar Myrdal. Myrdal was in Oxford at the time, and if I would care to drive over there and bring him to Cambridge, I would have the opportunity of spending a couple of hours alone with the great man. I jumped at the chance.

I read up on Myrdal before I went. Born in 1898, he had been a student of Knut Wicksell and Gustav Cassel. He made his international reputation as an economist and sociologist with his 1944 book, *An American Dilemma*, a now-classic piece of research that played a major role in the US Supreme Court's 1954 ruling on *Brown* v. *Board of Education of Topeka*, which outlawed racial segregation in public schools. His reputation grew yet further with his *Asian Drama: An Enquiry into the Poverty of Nations*, which, building on his foundation work on population theory, argued that the only way to bring about rapid development in Southeast Asia was to control population, widen the distribution of agricultural land, and invest in healthcare and education.

Besides being an economist and a sociologist, Myrdal was also twice elected to Sweden's Parliament as senator, was minister for trade and commerce, and served as the executive secretary for the United Nations Economic Commission for Europe.

Later, together with Friedrich Hayek, Myrdal was to go on to be awarded the Nobel Prize in economics for "pioneering work in the theory of money and economic fluctuations and for [their] penetrating analysis of the interdependence of economic, social, and institutional phenomena."

I duly picked up the great man, and we talked about many things. The striking thing to me, beyond his intelligence and the kind way in which he talked to a young and doubtless naive economist, was the breadth of his interests. No narrow-minded theorist he, for whom the real world was a largely irrelevant special case. On the contrary, he was not only a powerful economic analyst, but he also understood, and was able to bring out clearly, the importance of sociological and political considerations in the determination of events and outcomes.

Of the many things about which we talked, one in particular remains in my mind to this day. I had been lectured a lot about "the market" by my economics teachers over the years. However, their talk had been intrinsically abstract, and I often wondered whether those who had lectured me had had any real experience of their much-vaunted market. In particular, I wondered whether the fluctuations in financial markets – stock prices, bond yields, and all that – were determined by economic events and, if so, to what extent. So I put the question to Myrdal. "Were such markets determined, or even influenced much, by economic factors?"

Myrdal considered his answer for quite a time before he answered. "For long periods," he replied, "market movements can have surprisingly little to do with economic fundamentals." "But in the end," he said, "the facts kick."

I have since learned that this slightly odd phrase is a direct translation of a common Swedish saying. But the point is key, and went on to affect my professional life fundamentally. Having spent nearly thirty years of my life undertaking economic research and giving policy advice, I finally decided to work firsthand in "the markets," as Global Chief Economist at Lehman Brothers. There, at long last, I saw for myself that Myrdal was indeed right. Markets can move for surprisingly long periods, and a surprisingly long way, on whatever fad, theory, or preoccupation takes their current fancy. And much money can be lost by the person who fails to recognise that. But in the end, as Myrdal said, "The facts kick."

The trick, of course, is to have the position on when they finally do – but not too much before!■

Brian Reddaway

Almost everyone in the Cambridge Faulty of Economics and Politics during the 25 years from 1955 to 1980, and the many generations of Cambridge economics students over that period, were influenced, often fundamentally, by Brian Reddaway: although, curiously, few beyond Cambridge, and very few outside Britain, were anything like so strongly influenced. Certainly he had a strong effect on me, though to this day I am not sure why it was quite so strong.

Reddaway was an applied economist, with not all that much time – some would say too little time – for theory. Certainly, as he once explained to me, he had long discovered that fact is stranger than fiction: or, in his case as a professional applied economist, that data were stranger than theory. That is not to say that Reddaway did not appreciate the role of theory: but he was more inclined to move from a study of facts to the construction of a theory, than to start with a theory and search for facts to test it.

This led Reddaway to devise his own, unique, way of reviewing articles, which he employed when he was co-editor, with David Champernowne, of the *Economic Journal* – Reddaway reviewed the applied articles, Champernowne the theoretical ones. Reddaway's practice was first to read the tables and charts, and make up his own mind about what they showed. Then he would read the author's own conclusions. If, and only if, these tallied with his own would he then read the article proper. If they did not, he would write back to the author and ask how the conclusions had been arrived at. This happened surprisingly often, he once explained to me: plainly, many authors started with a theory, sought to verify it, and forced the data and the conclusions together, even though they did not fit.

A central feature of Reddaway's own applied work was that, whenever he was considering the effect that some policy or variable had had, he was scrupulously careful to make his evaluation with reference to what might have been expected to have happened otherwise. While the importance of the 'counterfactual' is perhaps more widely appreciated today, at least in economics, than it was in Reddaway's day, he undoubtedly did his students and his colleagues great service by always placing so much emphasis on this simple, but basic and all-important, issue.

So influential was Reddaway's approach to applied economic questions that it even acquired a name: "The Reddaway method", which he himself explained thus:

> "I have attempted to tackle *practical* problems, whether on full employment, growth, underdeveloped economies, inflation, the effects of direct investment overseas, the selective employment tax, or the investment of portfolios. To do so, I have sought to combine theory with realistic data and look for the factors which are quantitatively important, rather than those which are intellectually stimulating. I have tried to be pragmatic in my choice of methods for tackling problems and to be clear about the alternative position with which comparisons are effectively being made (and to be sure that it is a meaningful and consistent one). Favourite slogan for pupils and research colleagues 'It is better to be roughly right than to be precisely wrong (or irrelevant).'" (Reddaway, *Who's Who in Economics, p. 932* (1999)

Not surprisingly, perhaps, Reddaway was somewhat critical of econometrics, or at least of the use to which that technique was often put. He considered that there were often too many relevant variables for econometrics to be able to cope with; that practitioners all too frequently confused correlation with causation; and that too little attention was paid to limitations imposed by structural change. While theory, and to some extent practice, has in subsequent years come to address these problems, even today's practitioners are often less careful than they would have been had they been taught by Reddaway.

I really came to know Reddaway over two episodes. The first concerned his evaluation of the Selective Employment Tax (SET), which had been introduced by the Labour government in 1966 on the advice of Nicholas Kaldor, for whom I had just gone to work. A clear theory lay behind Kaldor's tax: that manufacturing output was constrained by the level of manufacturing employment; that services, however, could expand output without employing more labour – for example by moving from small shops to supermarkets; and that labour would not flow from services to manufacturing because, in the United Kingdom at least, wages had approximately (and, in Kaldor's view, prematurely) equalised between the two sectors. Given that services output was largely (in Kaldor's view) linked to, and driven by, manufacturing output, it followed that aggregate output growth was constrained by the supply of labour: and this constraint could be eased by taxing employment in the services sector, so as to make it more expensive to hire, and thereby encouraging employers to be more economical in its use.

Reddaway was asked by H.M. Treasury to prepare an independent and impartial assessment of the effects of the Selective Employment Tax, and this he duly did (*Effects of the Selective Employment tax (*HMSO, 1970). Because Kaldor and Reddaway were not only colleagues but also near neighbours, and because Kaldor was a very insistent man, Reddaway showed his draft chapters to Kaldor shortly before the report was published. And Kaldor hated them. Hated the approach. Hated the conclusions. He wrote Reddaway four long letters, totalling over fifty foolscap pages – copies of which I have, and treasure – complaining about Reddaway's method which, Kaldor felt, paid virtually no attention to the way in which the tax was supposed to work, and hence the counterfactual against which it ought to be judged.

The second episode involved a piece of research that I had undertaken for Wynne Godley on the determinants of the United Kingdom's import prices. Wynne liked the piece, and suggested to Reddaway that he publish it in the *Economic Journal*. Reddaway read my piece, and basically approved of it. But he then sat me down and told me all of the things that in his judgement needed to be done to it. I duly did them: and then he sat me down again and repeated the process, with a whole new list of 'requests'. Believe it or not, that paper went through twenty one revisions after Reddaway had accepted it in principle. I was not particularly thrilled with the process, by the end, but I have to admit that it would not have been half as good had it not been for Reddaway. He could be incredibly kind, especially to the young.

He could however also be tough, bordering on the harsh, with older people. I started to feel this towards the end of my time at Cambridge, when one of my responsibilities was secretary of the Degree Committee of the Faculty Board, of which Reddaway was the chairman. Whenever I was even slightly late with some piece of administration – which I often was – Reddaway, whose room was opposite mine, would invariably descend on me and make my life a misery. I began to dread bumping into the old buzzard, as I had begun to see him. What kept me going, more than anything else, was a print, given to me by my good friend Nicholas von Tunzelman, of one of Daumier's *Types et physonomie, les moments difficiles de la vie.* It portrays a desiccated lawyer, the spitting image of Reddaway, pleading for leniency for a miserable sinner (clearly me). I hung it on the wall in my room in the Faculty, where it gave me much solace. Reddaway saw it once, and studied it for what felt like an eternity. But he made no comment. I have no idea whether he recognised the likeness. I have it still, on the wall of my study.

Reddaway died in 2002. There were many obituaries, and I read most of them; and more recently, in 2006, my friend Ajit Singh has written a thorough, perceptive, and appreciative Biographical Memoir for the British Academy.

So, why did Reddaway have such an influence? I am still not sure. But he did.

Joan Robinson

Some 30 years ago, just after I had taken up my first academic appointment, at Cambridge, I first met the redoubtable Joan Robinson. Always one to take an interest in "the young", she zeroed in on me and asked what I was working on. I explained that I was trying to model consumer confidence, so as to improve the forecasting ability of the consumption equation in Wynne Godley's model of the UK economy.

Joan was clearly torn by the wish to be kind to Cambridge's newest recruit, and the intellectual need to demolish what she considered to be a misguided exercise. Intellectual honesty triumphed. What I was trying to do, she declaimed, was "dotty." Retreating into technique as a defence, I explained that, while aggregate income was, as Keynes had asserted, *fairly* good at explaining (in a statistical sense) the level of consumer expenditure, there was nevertheless quite a bit of unexplained variation. And much of this could be "explained" by fluctuations in consumer confidence. Hence all that one had to do, I concluded, was to forecast consumer confidence, and one would thereby get a better forecast of consumption.

Joan accepted the first point, but disagreed forcefully with the second. It was "impossible", she asserted, to forecast consumer confidence: Maynard had taught us that "animal spirits" were, by their very nature, incapable of being predicted:

> "Most, probably, of our decisions to do something positive, the consequences of which will be drawn out over many days to come, can only be taken as a result of animal spirits – of a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities."[2]

And that, as far as she was concerned, was that. Suffice it so say that that particular attempt to model consumer confidence failed.

Thirty years on and with the issue of consumer confidence as uppermost as ever, the evidence is not particularly encouraging for the forecaster. As I read it, the current evidence concerning consumer confidence in OECD countries is the following:

- While the level of consumer confidence can be measured by survey, its determinants seem to change over time.
- Survey measures of the level of consumer confidence correlate well with the (year-on-year) growth in a range of expenditure-related measures
- Hence, given that the confidence indexes are published well before the corresponding income and expenditure data, they give quite a good indication of the *current* level of expenditure, and thereby activity
- However, consumer confidence has only weak predictive power at best. And in Europe, it would appear, its predictive power is nil.

On this evidence, there can be little doubt that fluctuations in the major consumer confidence indicators are responsible for a substantial proportion of the variance in the economy. And weaker investment expenditure by companies in the light of that weaker consumption compounds the situation. Much less clear, however, is what confidence will be in the future. Of course, consumer confidence may respond to the blandishments and actions of the central bank, which may well have the *will* and, so long as inflation remains within bounds, the *scope* to cut rates by as much as it needs to get the job done. But the truth is that all of that remains largely a guess. Joan still looks to have a lot of right on her side.■

Robert Solow

I first met Professor Solow – or God, as I thought of him – in Oxford in 1968. I was a doctoral student at the time, researching the sources and causes of the economic growth of the New Zealand economy after the Second World War. I was following in the footsteps of one Edward F. Denison, who had pioneered such work at the empirical level, on the basis of a theoretical framework developed by Robert Solow.

[2] Keynes, J.M., The General Theory of Employment, Interest and Money, (London 1936) Bk. IV, Ch. 12.

My research was not going well. Having read everything – which was not much – that had been produced by New Zealand economists, I was starting to test their teachings against the data. I was finding most of them to be unsubstantiated by the data, and some flatly refuted.

My supervisor, Professor Corden, was as helpful as he could be, and more so than most other supervisors at the time. But the task of producing “a significant and substantial piece of research” is daunting. And my state of mind was not being helped by periodic little visits from a slightly older research student who would look over my shoulder and mutter things like “You’ll never get a doctorate at Oxford fitting Cobb-Douglas production functions. Only a CES production function is sophisticated enough for an Oxford doctorate.” He was on a research scholarship from a central bank, and ran an E-type Jaguar, so clearly he knew what he was talking about.

And then Robert Solow came to town. He had just delivered a series of lectures, “Growth Theory: an exposition”, at the University of Warwick, and was now re-delivering them at Oxford, as the Radcliffe lectures. I attended every one, and was spellbound. Each was intellectually brilliant, yet delivered in a simple, clear style, with a mixture of humility and wit the like of which I had never encountered. Moreover, at the end of each, Solow would announce that he would be taking coffee in the combination room, and that anyone who wished to come and chat was welcome. This was unheard of in Oxford at that time, when lecturers flapped around in black gowns, looked like eagles, and were about as approachable.

Miserable that my research was going nowhere, I finally plucked up the courage to take up the coffee offer. I introduced myself, received a warm, unaffected greeting from Solow and then, accepting his invitation, proceeded to pour my heart out.

Solow listened carefully, thought for a while, and then started to talk. I did not write down until afterwards what he said, but my recollection includes the following snatches:

> “You are trying to be much too sophisticated ... the different theories that I teach are all very well for thinking about how economies grow, but in practice the data simply don’t exist to distinguish between them … I don’t know of anyone who has successfully fitted a CES production function: the data demands are much too great ... actually, you are doing quite well if you can get even a Cobb-Douglas to fit …. let’s talk about what it is practical to do … I don’t know anything about New Zealand, but here are a few basic thoughts … it seems to me that there are some ideas that could be worth exploring…. Have you considered …”

The long and the short of it is that this great man, who later, in 1987, was to be awarded the Nobel prize for his contributions to the theory of economic growth, spun out in the course of half an hour no fewer than nine ideas that he thought it might be interesting to investigate, given the data I had described and the little that he knew about New Zealand.

I left the coffee room that morning a changed man. If a simple, commonsense, straightforward approach was good enough for Robert Solow, then it was going to be good enough for me. So I took his nine suggestions, followed my nose, did whatever seemed sensible, and in due course was awarded my DPhil.

Years later, I wrote to Robert Solow, to ask him if he remembered the episode, and to thank him for all his help. To my delight he remembered our several meetings; and over the years that followed we have met one another a number of times, in various parts of the world. Thank you, Bob.■

Post script. After Oxford, I went to Cambridge, and got to know Joan Robinson. Though her fights with the American neoclassicals were legendary, I got on as well with her as I had with Robert Solow – presumably because I was too young to matter. I therefore particularly appreciate the story, told by Sir Alan Budd, that Joan Robinson once said to Bob Solow "Mathematics is just a substitute for thought", to which Bob replied, “Since thought is such a scarce commodity it is very sensible to have a substitute for it.”