

Monetary and Social Relationships

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Introduction

The social sciences are much more complex than the physical sciences. Not only are experiments generally easier to undertake in the physical sciences, but also the subject matter of any such studies in the social sciences, we individuals, respond and change our own behaviour in the light of those same economic experiments. Moreover, human behaviour is both variable and reactive, especially in response to major regime changes. So, any attempt to depict the economic macro-system has to involve models which are gross simplifications of underlying reality.

How best then to simplify our macro models? It is such macro-models which will be the main subject of my discussion. When we macro-economists started building solvable models at the outset of the computer age, some 40, or so, years ago, we generally aimed at getting a detailed and comprehensive structure of the economy, on a sector-by-sector, equation-by-equation basis, using national income statistical categories, and this led to large computable macro-models, often with fifty, or more, equations. Amongst the resulting

problems, however, were that the optimising, so called micro-foundations were weak, if not non-existent. Expectations, when considered at all, were often inconsistent with the model's own workings; and some of the implications of such large models were difficult to discern, and when worked out often totally implausible.

All this led to the Lucasian revolution, whereby macro-models had to have 'rigorous', optimising, micro-foundations, often based on so-called rational expectations. This, in turn, led to a degree of mathematical and analytical complexity. In order to continue with these more complex models, the effective solution was to simplify the initial structure of the model to allow it to support the heavier analytical superstructure. Thus, a surprisingly large proportion of the models currently used for policy analysis, (though not, thanks be, for forecasting), in the macro-monetary field have been thinned down to three equations, an IS curve, an AS (Phillips) curve, and a Taylor reaction function; two players, central bank and private sector; and two assets, money and short-dated riskless debt.

I shall spend much of the first half of this chapter arguing that in many respects the Lucasian revolution has transferred the focus of implausibility, often down-right nonsense, from the implications of the solutions of the models to their initial structural assumptions. And one of the key deficiencies of such macro-models, including those used for analysis of longer-term developments, such as growth or optimal currency areas, is that all the action takes place in the private sector, so that the key role of government is frequently unrecognized. This will be the main topic of the second half of my chapter.

The Structural Basis of Macro-models

Let me, however, start with an important sub-field where the analysis has, instead, improved. When I started doing economics some 45 years ago now, one of the key building blocks of macro-economics was the LM curve. The assumed process was that the Central Bank would inject high-powered base money into the system; this would then be translated into growth in the monetary aggregate(s) via a variety of money multipliers, and finally market interest rates would be determined through the equilibration of the demand and supply of money.

Of course, simple observation of the way that central banks and money markets actually behaved would reveal that this was the exact reversal of the truth. The process started with the Central Bank determining its official short-term interest rate, in pursuit of its current policy objectives. Then the demand for bank borrowing, at the chosen policy rate, was the main determinant of the growth of the monetary aggregates. Given M , the money multiplier worked effectively in reverse, to determine the amount of base money that the authorities had to make available to the banking system to sustain their initially chosen interest rate.

Faced with this divergence between reality and economic theorising, the response of the profession was to ignore reality for pedagogic purposes; see any macro-economics or “money and banking” text-book until a few years ago. On the normative front, another reaction was to argue that Central Bank behaviour, in setting interest rates, was a sub-optimal policy, and that Central Banks should adopt a policy of choosing, announcing and sticking to a policy of monetary targetry. Indeed it was argued, notably in the influential Sargent/Wallace 1975 article, that the Central Bank policy of setting interest rates was bound

to lead to Wicksellian instability.

That argument was correct on its own terms, but that assumed that the Central Bank would set its policy variable, (either M or interest rates), exogenously, that is without adjusting that variable endogenously in response to concurrent economic developments. That assumption was unrealistic, indeed somewhat silly. Central Banks invariably set interest rates endogenously in response to perceived economic developments. At times they may have done so in a bad, or destabilising, fashion because of political constraints, inappropriate objectives, misperceptions of reality, or various human frailties. That said, the idea that Central Banks might try to 'fool' the public into working harder by creating 'surprise' inflation was never a realistic description of their activities. The papers by Kydland and Prescott (1977) and Barro and Gordon (1983 a and b) on this issue were academically extremely influential, and indeed contributed to the former authors' Nobel Prize in 2004, but were only tenuously related to the main causes of the inflation of the 1970s.

But the analysis of how Central Banks can, and now generally do, set interest rates so as to achieve price stability, and as a necessary adjunct of that economic stability, has been worked out in practice through the adoption of inflation targetry, where Mervyn King has played a major role, and theoretically through the work of academics such as Bernanke, McCallum, Svensson, John Taylor and Woodford. The prior divorce between theory and practice has gone. This is a considerable and valuable step forward for the profession from its former state.

This reconciliation of theory and practice was marked for me by the publication of Michael Woodford's magisterial book on Interest and Prices (2003). But that is about as far

as I go in applause for recent developments in macro. Let me now take aim at some of the other structural assumptions of modern macro, which mostly are evident in that same book. First, we believe, at least in a rather general sense, that if all prices and wages were infinitely flexible, the real economy would remain in equilibrium at all times, and money would be continuously neutral.

Consequently an understanding of the rationale and workings of wage and price stickiness would, one might have thought, be central to modern macro-economies. Indeed there was an upsurge of interest in this topic, stimulated first by Clower and then by the work of Akerlof and Yellen in the mid 1980s, for example (1985, a and b), but I confess to seeing little real progress on this front since then, either in theory or, perhaps, more important in generally accepted empirical findings. Instead, what one sees much more often is that macro models are based on Calvo pricing mechanisms, whereby a particular (and constant) percentage of firms is allowed in each period to change prices, and the remaining firms are prevented from doing so - though why is never explained. Nobody believes this to be literally true; it is a mathematically convenient fiction; but it forms the pricing basis for much current macro work.

I might add, parenthetically, that quadratic utility functions are just such another convenient mathematical fiction. The likelihood that going from an inflation rate of say 100 per cent per annum to one of 103 per cent p.a., is likely to reduce utility by vastly more than going from 4 per cent to 5 per cent inflation is obvious nonsense, as Margaret Bray and I have recent argued (2003).

Be that as it may, much of the structural foundations, relating to price/wage stickiness in modern macro, rest on a convenient fiction, which has only a distant relationship with

reality. Why such procedures are somehow regarded as professionally acceptable, whereas the assumption of adaptive expectations was not, (especially when empirical studies generally show that purely backwards-looking expectations have a better forecasting record than purely forwards-looking ones), is beyond me.

Perhaps the greatest remaining gap between the work of academic money-macro economists and policy-makers in Central Banks and Ministries of Finance relates to lags, specifically the lag before policy measures affect economic outcomes. In most of the formal models the policy change immediately affects current output, because not all prices and wages are allowed, by a deus ex machina, to adjust, and also affects current and expected future inflation, and usually, depending on the model, future output. Although the lag structure, engendered by price stickiness, can have a long tail, the usual implications are that the largest effects on both output and inflation are immediate.

This is not what policy-makers observe from the data. The standard data-based rules of thumb are that, unless there is an immediate sharp shock to exchange rates, there is a short lag before output reacts at all, and then has a hump-shaped response, and an even longer lag before inflation responds. This latter delay of the inflation response behind the output response is especially problematical for the theoreticians. It can hardly be doubted that monetary policy initiatives, in the sense of interest rate changes, are front-page news, as indeed are continuous 'expert' commentaries on the likely future paths of both nominal interest rates and of inflation. So any suggestion that those making the individual price/wage decisions are slow in learning about changes in current and future patterns of real interest rates is surely far-fetched; there are few islands where media reports of Alan Greenspan's

latest words are not instantly relayed.¹ Yet if there is an observed change to the predicted future time path of real interest rates, then in theory those allowed to change prices/wages should now do so immediately. To summarise, current macro models find it extraordinarily hard to replicate the lag structure which is a key feature of the conjuncture for policy-makers, and for their own forecasters/economists, especially the quite lengthy delay before inflation responds perceptibly to observed changes in monetary conditions. Is it unfair to claim that the reaction of most theoretical model builders is to ignore this discrepancy, and to continue with constructs where both the rationale for, and time profile of, the lag structures has little relationship with reality? To be blunt, I cannot quite see how theoreticians can square the apparent actual lag profiles with the present dominant paradigms about price-setting mechanisms, without substantial modification of their present theories.

The purest theoreticians of the Lucasian age get around the problem of modelling wage/price stickiness and lag profiles for the effects of monetary policy by assuming them away. In such real business cycle models money is neutral, prices are fully flexible and fluctuations come from real productivity shocks. Here we usually have representative agents, a representative consumer and firm, optimising its utility over an indefinite time span and, normally, in a system of complete financial markets. Such an assumption of complete financial markets is, for example, a key element in Woodford's recent book which I have already mentioned. Why is this assumption central? It means that all eventualities can be foreseen and appropriately hedged at the correct insurance/option price.

If all eventualities can be appropriately foreseen, hedged and priced, then there are a

¹ Information was more slowly diffused in the 19th century, yet most studies find that prices/wages were more flexible then than in the 20th century.

number of consequentials. All information problems are effectively solved; rational expectations is a logical implication; there is no residual, unhedgeable risk, financial or otherwise; all agents can borrow/lend at the riskless rate (plus, in a world of heterogeneous agents, an actuarially correct risk premium for the known risk of the agent defaulting, decamping or otherwise failing to meet the transversality condition, (that is that all debts are paid in full at the horizon)). There could, moreover, never be any financial innovations since all the necessary instruments for transferring wealth among future states of nature would already exist.

This assumption, of complete financial markets, lends itself admirably to the construction of soluble models with 'rigorous' micro-foundations of optimisation within a general equilibrium system. The problem, of course, is that the assumption has no connection with the real world. Indeed, the favourite asset of such models, Arrow securities, (which pay out one in the event of some given event and zero otherwise), only occur rarely (for example some insurance products, credit default swaps, and so on).

In particular, the complete financial markets assumption effectively means that all information problems relating to granting credit, expectation of repayment, and so on, have already been resolved. In such a system it is not clear why either money, or banks, or other financial intermediaries should exist. I have never been able to understand how the necessary information is collected and distributed to all participants in such a system in the first place..

An assumption of a system of complete financial markets, in effect, assumes away all Coaseian transactions costs, either because some Divine Auctioneer provides all the necessary information or because there is no time constraint, so we can all spend an infinite

amount of time collecting information and establishing and pricing appropriate hedges.

Unfortunately for us there is no Divine Auctioneer, and the time constraint always binds. Besides the fact that this implies incomplete financial contracts, indeed incomplete contracts more generally, it also implies that I can use my scarce time to work, play, sleep, shop or to gain information and educate myself. Improving my knowledge of the world, and making my expectations more accurate, requires use of my scarce time. It is not rational for agents to have expectations that are consistent with the outcomes of the best, (or even the currently used), model; let alone to have the best possible expectations. What is rational is to use up scarce time until the expected marginal addition to utility from spending more time in learning about aspects of the world equals the marginal opportunity cost in foregone use of time in other pursuits, as Stigler has argued. Applications to business schools rise during periods of downturn and lay-offs in financial markets.

On this basis there is nothing inherently irrational about backwards-looking adaptive expectations, or on relying on the views and forecasts of others; (as a part-time sheep-farmer I am a proponent of rational herding). Yet the Lucas critique, of course, still holds. Faced with a shock, a regime change, one will have to reallocate one's scarce time, and that may well, indeed should, include a decision whether to allocate more time to learning about the new world. In calm conditions simple rules of thumb economise on scarce time; in disturbed conditions it pays to spend more time learning how best to navigate.

What this implies is that the expectations' generating process will itself be time-variant and endogenous, and not constant, as most models assume. That said, there is a heavy initial cost to learning about complex issues, which can be recalled through memory at virtually zero cost, at least until Alzheimer's strikes. So expectation formation processes are

likely to be subject to inertia, hysteresis and initial conditions. To summarise again, the way in which the rational expectations assumption is normally deployed assumes away Coaseian transactions costs, and would itself be irrational in the real world of binding time constraints.

Once we dispense with complete financial markets, default (the failure of transversality conditions to hold) becomes a greater problem because it may, indeed usually is, impossible to hedge, or at least to do so completely, against it. That causes problems for representative agent models. Either the whole system collapses, or no agent does. If the solution chosen is to rule out all possibilities of default, then that is, I believe, equivalent to reverting to an implicit adoption of complete financial markets.

In any case the assumption that everyone, all consumers, all firms, is identical is again a convenient fiction, and, for sure, far further from reality than adaptive expectations or the various other shortcomings in internal logical consistency of the earlier large forecasting macro models. It matters for several reasons. If your model contains a single representative consumer, you more or less have to assume that that consumer meets the transversality condition (that is that in all conditions the consumer chooses a course of action so that her debts are ultimately repaid). That means that it is always perfectly safe to lend to the representative consumer, so that she can borrow or lend at the riskless rate. Consequently the representative consumer is never financially constrained, and can always make a forwards-looking optimising plan, subject to the transversality constraint.

But many of us (perhaps most of us, possibly all of us) choose courses of action which under some circumstances mean that we cannot, and do not expect to be able to, pay all our debts. Some of us, amongst them the criminal fraternity, only expect to pay our debts under unlikely circumstances; fraud is hardly unknown or uncommon. As Martin Shubik has

argued (1973), default is a common chosen facet of optimising behaviour.²

If we now assume the existence of incomplete financial markets and heterogeneous agents, then we start getting back into the real world of financial intermediaries, whose *raison d'être* is largely risk assessment, of risk premia, of financial constraints on behaviour, (because of Stiglitz-Weiss type problems), and so on. In this world money, credit-worthiness, confidence and collateral all play a major role. Some agents find that their optimising plans are constrained, not by some ultimate transversality clause, but by immediate credit constraints; thus the young cannot anticipate future higher incomes to smooth their consumption over time; small firms cannot borrow enough from banks to finance all their perceived investment opportunities, and so on.

This is a much messier, but far more realistic, world than that represented by most macro-models.

The challenge is to meld, and to combine, theory and empirical realism. The Lucasian critique of the early large forecasting models was that they were developed by building up individual equations on an empirical, ad hoc, pragmatic basis; and that the resulting construction had little theoretical basis, and often had internal logical inconsistencies. My own riposte is that the resulting family of macro-models, based on so-called 'rigorous' micro-foundations, are in turn empirically absurd, with complete financial markets, no (credit) risk, no default, transversality conditions always met, representative agents, no necessity for financial intermediaries, hardly any of the attributes of the real world that fill the life and concerns of, for example, a Central Bank official.

² Also see Shubik and Wilson (1977) and Dubey, Geanakoplos and Shubik (2000).

Can theory and practice, in the money/macro field, be reconciled? My own view is that the best attempts to do so, and to give an explicit role for default, credit risk and money, are to be found in the work of Martin Shubik and his followers, amongst them Geanakoplos, Dubey and Polemarchakis. A basic problem is that models which embrace incomplete financial markets, default and heterogeneity are inherently complex and extremely hard to present lucidly and simply. Thus, these real-life features, for example heterogeneity, money, default, incomplete markets make necessary the modelling of default laws, formal treatment of liquidity, regulatory policy, banks, and so on. Moreover, the importance of incomplete markets, heterogeneity (as well as institutions) is underlined by the fact that equilibria are constrained inefficient (Geanakoplos-Polemarchakis, 1986) (that is not achieving the second best). So, government, central bank and regulatory intervention can lead to efficiency gains. Partly in consequence of the inherent complexity of such models they have not made the impact, at least not yet, that I believe that they should.

One field where it is patently impossible to use the complete financial market, no default, representative agent type of model is in studying financial risk and contagion, which is one of the areas in which I have worked as a part-time consultant at the Bank of England. Instead one needs a model in which incomplete financial markets, default and heterogeneous agents, especially banks, are central to the process. In this respect I have been fortunate to find Dimitrios Tsomocos at the Bank. Fortunate for two reasons; first, my own mathematical, model building skills are deficient; second, Dimitri is one of the best students and exponents of the Shubik school. Anyhow, using some of my suggestions about modelling heterogeneous banks and incomplete financial markets, he has developed a model for studying systemic financial stability issues. With the aid of Ton Sunirand, we have

demonstrated that simplified versions can be numerically simulated; and our present, continuing exercise is to calibrate it against the data, see Goodhart, Tsomocos and Sunirand (2003, 2004 a and b).

One problematical deficiency in this field of trying to model and assess financial stability is that virtually all methods of monitoring such financial stability relate to individual banks, and other financial intermediaries, for example VARs, stress tests, solvency and capital requirements, liquidity ratios, and so on. What matters, from society's view-point, is systemic, not individual, stability. The two are obviously related; for example a system where most of the component members are weak is not likely to be systemically strong. Nevertheless it does not take much ingenuity to construct examples where each individual bank appears to be initially strong, but the overall system is nevertheless fragile, for example because of various inter-connections; or indeed vice versa, where the individual participants seem weak, but the overall system is well protected. This has been done ever since Henry Thornton (1802), if not before.

The Holy Grail in this field, which many of us are pursuing, is to be able to complement the known procedures for monitoring individual banks with new methods for assessing systemic strength. We are, I believe, moving forwards, but there is a long way to go.

The Role of Government

Let me turn, however, from modelling to another area where I believe that theory has become divorced from reality. One of the main subjects of topical interest over the last two decades

has been the formation of the euro-system. The main academic tool for assessing whether it was appropriate for countries to share a common currency has been optimal currency area theory, or OCA. This theory described several factors that would make a common currency appropriate, for example openness, wage/price flexibility, trading patterns, size; but it gave very little role to the inter-action of government, and especially of governments' fiscal policies, with the domain of monetary policy. Yet, as Michael Mussa noted, the greatest economic regularity in this field, prior to the euro-system, had been the association between country and currency; whereas the predictive power of OCA theory was almost nil, (apart from correctly predicting that micro-states, such as the Vatican, Andorra, Lichtenstein, would share the currency of their larger neighbours).

Now one might try to dismiss the currency/country link as an inessential symbol of sovereignty, like flags, or national anthems, or made in France labels of origin; but in my view such dismissal would be wrong, [also, see Issing, 1996]. It is, perhaps, easiest to appreciate the necessity of some link between governments and their fiscal policies on the one hand and money on the other in the case of a fiat money system. Why do people accept such unbacked pieces of paper? The answer, of course, is because of the power of the government, and in particular of its power to tax, and to specify what paper currencies it will accept in payment. Of course, such sovereignty can be shared, and the acceptability of the euro is completely assured by the agreement of all member states in the euro-system to do so.

But there are additional key links between fiscal and monetary policies. Within a given monetary domain, one cannot use monetary policy to iron out asymmetric regional imbalances and shocks, but, when the monetary and fiscal domains overlap, one can use fiscal policy to stabilise and to redistribute, as far as political consent allows. Within the

euro-system the monetary system is federal; the main fiscal competences are national.

Earlier in the 1990s I worked on a specialist group commissioned by the European Commission to try to bridge that gap. Our Report ('Stable Money - Sound Finances' (1993), also see 'The Economics of Community Public Finance, Reports and Studies', (1993)) then was pigeon-holed, dismissed by representatives of most member states, and largely ignored. Ever since, I have felt that the failure to deal with that separation of the domains of monetary and fiscal policies was a potential flaw in, the soft-under-belly of, the euro-system. The travails of the Stability and Growth pact, now coming to a head with the possible legal challenge by the Commission against the decisions of national Ministers of Finance, underline both the difficulties and complexity of trying to run a system that divorces the locations of fiscal and monetary competencies.

It is not just in macro-policy, stabilisation issues, that this separation matters. In the field of financial stability, financial crises often require resolution through the use of tax-payers' moneys, frequently a lot of money, as multiple examples around the world attest. There are insufficient funds in the ECB for such a purpose, and none effectively available for such uses from the federal budget in Brussels. If a bail-out in a crisis is undertaken, it will have to come from national budgets and tax-payers in each separate nation state. He who pays the piper calls the tune. One can talk as long as one likes about the tidiness and efficiency of combining the monetary domain with the regulatory domain at the federal level, and of likely overflows between nations in any financial crisis. So long as the actual fiscal payments to resolve a crisis come out of national Treasuries, not from the federal budget, the corollary will be that regulatory and supervisory issues will be decided in conclaves in which national Treasuries take the lead, and the federal institutions become side-lined as observers.

The relationship and, in particular, the separation between a federal monetary system and national fiscal competence, to my mind, was the main problem facing the euro-system. Yet it did not get, and still hardly receives, sufficient attention. It received no mention in the latest discussion on the European Constitution, perhaps because it was perceived as 'too difficult' to resolve. This failure to tackle what is the crucial issue, in my view, arose largely because economic theorists have consistently tended to ignore, or to underestimate, the essential links between government, fiscal policies and money. This goes right back to the theories of the evolution of money itself.

The dominant mainstream theory of the evolution of money is that this developed from indirect barter in a private sector trial and error process with the aim of reducing transactions costs in (market) exchange. It is often emphasized that, whereas governments may have played some subsidiary role in extending and enhancing monetary functions, for example in establishing mints for coinage, they were strictly inessential in its initial evolution.

This was one of the key messages of Carl Menger's seminal work in this field at the end of the 19th century, work that our President this year Erich Streissler has recently commented upon, (see his Chapter on "Carl Menger's Article 'Money': From Barter to Money" in Latzer and Schmitz (2002) book on Carl Menger and the Evolution of Payments Systems). It remains the dominant paradigm today, involving technically fancier papers, such as Kiyotaki and Wright, for example (1989, 1991, 1992), but they maintain the same basic intuition.

Although most economists share this same view, about the evolution of money, most other social scientists working in this field, amongst them numismatists, archaeologists,

anthropologists, and soon, and a minority of economists do not. This latter group, of which I am a member, find the origins of money in the need to resolve social inter-relationships, for example the bride-price, restitution of personal injuries, settling feuds, and so on, rather than, or certainly as well as market relationships.

I have written several papers on this theme recently (2003, 2004), and I do not want to go over the ground again at any length. Let me, however, give a brief quote, (2003, p. 9).

`[The standard mainstream, Mengerian] M-form theory finds it difficult to account for the role, or existence, of money within a general equilibrium model. Money in the utility function, or cash-in-advance models, are proposed, without much conviction. This difficulty is not surprising given that such models also abstract from the existence and role of government. While it is, of course, the relationship between taxation and the demand for money that the [Cartalist, Keynesian] C-form theory emphasizes, it should also be remembered that it is the maintenance of law and order, the form and enforcement of contracts, and the whole infrastructure of regulation within society, that allows the epiphenomena of (organized) (private sector) markets to occur at all.

A disclaimer may, however, also be needed. The purpose of this [passage] was to argue, first, that money frequently played an initial means-of-payment role in inter-personal social and governmental roles before it played a major role as a medium-of-exchange in market transactions, and second that the relationship of the State, the governing body, to currency in all its roles has almost always been close and direct.

But I do not claim that the private sector cannot, and has not, ever been able to

develop monetary systems without the involvement of state authorities.’

Let me just give you one more telling example. A common word for a monetary recompense is that it is pecuniary. The word pecuniary derives from the Latin word pecus which means cow, or cattle. Cattle were commonly used in large deals in early Roman history before the introduction of coinage, see Burns (1927), and are commonly used in societies studied by anthropologists for such socially-related exchanges. But does a cow satisfy the standard characteristics required for market exchange? Is a cow standardised, durable, divisible? Could you go out shopping with a couple of cows in your purse? On all this, also consult the excellent paper by Hudson (2004).

Economics is a very new social science. Recently both the London School of Economics and the Faculty at Cambridge University celebrated their centenaries. Few statistical series go back before about 1870. Perhaps in consequence, economists focus mostly on current and near-future prospective events. For such purposes the underlying institutional framework of law and governance tends to be taken largely for granted, as a given. Yet the successful functioning of markets depends on that same legal and governance structure.

In the field in which I have been working recently, concerned with the prevention of financial crises, linkages between the infrastructure of markets and the maintenance of financial stability are clear and important. The development of globally accepted accounting standards, the use of standardised legal proforma for financial contracts, netting arrangements, the role of centralised counter-parties, clearing and settlement systems, to name but a few examples are crucial to the health and continuing development of financial

markets and the financial system.

It is not possible to build a superstructure of (financial) markets, or of theories relating to the workings of such markets, unless the underlying infrastructural plumbing is sound. That plumbing, which relates to the organisation, administration and resolution of disputes within such markets, is a social construct and relies on law and governance. It may often be boringly detailed, but it is essential. Our monetary and financial systems will not work without it.

Normally this governance infrastructure, of which I suggest that the institution of money is itself a part, is taken largely for granted, as a constant in the background. Recently, however, there have been shocks that have underlined its importance. Foremost amongst these has been the collapse of communism. Trying to shift from a directly-controlled command system to a market economy has been made much more difficult by the need to build-up a governance system that extended beyond the passage of laws onto the record book also to a general understanding and acceptance of the underlying governance principles by the wider population. It is particularly hard to do so in countries with no prior history of the rule of law, outside the State's will. Not that such principles are always followed in our own economies, as various scandals at home and abroad have testified.

In a series of justly celebrated papers (amongst them 1997, 1998), Shleifer and various colleagues have attributed the relative economic success of countries to their adoption of differing legal systems. I wonder whether their focus was perhaps too narrow, so that possibly the successful working of a particular legal system reflected a more broadly well-functioning governance system.

When I consider cross-country and regional differences in income and wealth, my

own view is that governance is hugely important. If you could offer a be-nighted country, say in Africa or the Middle East, extra resources, for example access to oil, or extra capital, or extra foreign aid, or better governance, which would you think most important? Yet, with the exception of (applied, empirical) studies, such as those by Barro (1991, 1995), most of the formal models of growth omit any role for government and associated legal and governance structures. Indeed almost all the studies examining the influence of government and governance on growth have been empirically based.

Once again, the simplifications which macro-theory have employed seem to me to have excluded many of the key features of the real economy. But how could one best incorporate governance issues within a long-run growth model? Alas, an answer to that question is well beyond my own abilities or competence.

Conclusion

You all know that joke about the parade of armed forces in Moscow where the procession of weapons of increasing destruction, tanks, planes, rockets, was concluded with a truck carrying a few grey-suited men. But “Who are these?” asked an onlooker. “Economic theorists,” was the reply – “But why?” “You should see the devastation that they can achieve.”

As an epitaph for the economic desolation caused by command and control centralised systems, it is perhaps reasonable to allocate a great deal of blame to bad theory, as Keynes emphasized more widely. Moreover, I have recently been doing a review of Allan Meltzer’s first volume, monumental History of the Fed, in which, of course, the Great

Depression of the 1930s takes central place. In this sad story, it seemed to me that reliance on prior theories, particularly on the Classical Dichotomy, whereby price/wage flexibility will soon allow the real economy to be equilibrated by real determinants, but also on the Real Bills Theory, in the face of overwhelming evidence to the contrary, played a major role in encouraging the Fed to remain passive.

Fortunately no disasters on that scale are currently affecting the developed world, though conditions in parts of Africa seem still terrible enough. Even so, the divorce between much of modern macro theorising and what I take to be empirical reality strikes me as being greater today than in earlier decades. Back in the 1960s we tried to build up our models, equation-by-equation, on a largely pragmatic basis, to fit the empirical facts as we perceived them.

The criticisms of this earlier approach, its lack of theoretical micro-foundations, its internal inconsistencies, its ill-considered treatment of expectations, were all fully justified. But has the Lucasian counter-revolution tossed out plausible empirical foundations, thrown out the baby along with the bath-water? When I try to read much of modern macro, I generally find that the initial structural assumptions are chosen largely to facilitate the subsequent technical/mathematical analysis; the fact that their relationship with reality might be tenuous, or even non-existent, seems to be of little, or no, concern.

I have tried to give examples along the way, amongst them Calvo pricing, complete financial markets, no default, representative agents, rational expectations (as usually defined), and so on. This covers so much of the guts of modern macro that you may think that I am simply against all macro-theory. Not so. There are a few theorists, notably Martin Shubik and his followers, who have tried to build up theory on the basis of real world

conditions, such as incomplete financial markets, default, credit and confidence, rather than try to cram down an overly simplified, but internally rigorous, theoretical system upon reluctant economic data.

Perhaps the greatest such gap in money/macro, between theory and reality, has been the general exclusion of the role of government, especially in setting the underlying infrastructure of governance and law, from our analysis. Again I have tried to give examples, relating to the evolution of money itself, to the centrality of the links between fiscal and monetary policies, including effects on the structure of financial regulation and its importance for the working of the euro-system, and, indeed, to economic growth and welfare more widely.

The 1960s were a great decade of hope for macro-economists, that we could steer the economy in a successful way. Those hopes were punctured by the bad decade of the 1970s, and the theoretical explanations of the inherent failings of the 1960-type macro-models. In the last decade, or so, that hope about steering the economy, through the monetary policies of independent central banks, seems to me to have been seeping back, but, in my view, as much despite, rather than because of, mainstream macro-theorising.

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