
Research and policy at the Bank of England: the things we'd like to know but never dare ask

The Deputy Governor⁽¹⁾ considers the link between research and policy in three areas of particular interest to the Bank: money, EMU, and regulation of the financial system. In each case, he outlines a range of questions that have preoccupied the Bank and the consequent research conducted by its staff. He argues that research can help to find answers to the questions, even if, in the end, there will still be political judgments to be made.

I intend to focus today on the link between research and policy. How can research feed into the policy-making process? And, equally importantly, how best can policy-makers identify the areas in which they should be encouraging and supporting research? I have become closely involved with this interrelationship since joining the Bank, partly because I chair the editorial board of the *Quarterly Bulletin*.

I am reminded here of that famous quote from Keynes: 'Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist'. Since we know that economists these days do all their creative thinking before the age of 30, all our senior economists are defunct by definition, so we need to plug into outsiders. We are not, of course, primarily a research institute, though we do undertake a lot of research work as you know—most of it, though, what one might describe as 'near-market' work, in other words work which relates reasonably closely to practical policy questions.

I shall comment on the link between research and policy in three areas which are of considerable interest to the Bank and, I hope, to many of you too. These are *money*; *European Monetary Union*; and *regulation* of the financial system. There are some very clear policy questions here, on which research work can provide vital insights. I might mention three in particular: How concerned should we be by the currently strong growth of both narrow and broad money? What are the pros and cons of UK membership of EMU? And what should banking supervision be trying to achieve—indeed should it exist at all? Research can help us to find answers to these questions, even if, in the end, there will still be political judgments to make.

We are interested in money because inflation is essentially a monetary phenomenon. Such a statement can easily end up being repeated like some central banking mantra, to the point where it becomes vacuous. What I think we mean is simply that the price level, by definition, is the price of goods and services relative to money. Inflation, then, is a change in this relative price and can thus have one of two sources: the goods market or the money market. Over the

long run, we think of the supply of *goods* as being fixed by factors beyond the central bank's control—by natural endowments, technology and such like. That then leaves the supply of money—which *is* within the central bank's control (at least central bank money is)—as the crucial determinant of sustained inflationary surges. Now that may sound like a very classical—not to say convenient—conclusion. But over long runs of data—I am thinking of decades and centuries here rather than months and years—the very close correlation between money and prices remains a striking monetary fact. Indeed, it is one of the few monetary facts we have available to us.

I am aware, however, that this simple conclusion conceals a multitude of complexities. The relationship between central bank money, which the central bank can control, and what counts as money in the economy, change over the long run as techniques of money transmission change. And the central bank's control over wider versions of money usually involves influencing the demand for them. So even in the long run it is hard to tell a simple Quantity Theory story about any money stock which is directly under the central bank's control.

And over the shorter term, the relationship between monetary aggregates and other variables, such as activity and prices, is often far from predictable and robust. This has been particularly evident from attempts to model money in this country, where the half-life of a money demand equation has typically been no longer than a year or two.

Nevertheless, at least some money and credit aggregates—and some sectoral components of them—do have reasonably good *leading indicator* properties over future activity and prices; or are *corroborative*, in the sense of confirming the evidence we have from the real economy. Indeed, some of the money-income correlations we have unearthed appear genuinely *causal* in that a clear story can be told about the relationship between money and credit on the one hand, and activity and prices on the other. This is important if we are to improve our understanding of the transmission mechanism of monetary policy. How do changes in monetary policy feed through to the economy? And how,

(1) In a speech given at the Money, Macro and Finance Conference at the London Business School on Thursday, 5 September.

within this process, do money and credit interact with other variables?

A considerable amount of research on these subjects has been undertaken within the Bank over many years. And in doing so we have drawn heavily on the ideas and techniques developed by academics outside the Bank, including many of those closely connected with the Money, Macro and Finance Group.

As an example, consider the article we published on broad money in our *May Quarterly Bulletin*.⁽¹⁾ Indeed, Ryland Thomas, the author of that article, presented some of the more detailed results from our own research at an earlier session of this Conference. In particular, we have recognised the importance of using wealth, as well as income, as a scale variable in broad money equations; and the need to model rates of return on alternative assets, as deposits with banks and building societies have become increasingly close substitutes for holdings of other financial assets. It is also valuable to look separately at the demand for money in different sectors of the economy (in particular individuals, corporates and other financial institutions) and to model sectoral balances in conjunction with real variables (for example personal sector deposits together with consumption, and corporate sector deposits together with investment and stock building). These are theoretical advances which we have made in step with outside academic work. They have helped us to capture important interrelationships and thereby to understand better the transmission mechanism of monetary policy. But we recognise only too clearly that this is an evolutionary process and that we still have a long way to go in understanding broad money.

What use do we make of all this in thinking about policy? Over the last year or so we have attempted to explain the relatively rapid growth of broad money and to examine the implications of this for future activity and prices. Rather than expressing concern merely because broad money is growing at around the top of its 3%–9% monitoring range, we are concerned because we cannot explain adequately the growth of personal sector deposits over the last 18 months in terms of what has been happening to personal sector income, wealth and consumption, and to relative interest rates. We therefore regard the apparent overhang of personal sector liquidity—perhaps around £10 billion, or 2% of total M4—as generating an upside risk to future activity and inflation which is additional to the information available from looking at variables other than broad money. And the recent strength of corporate sector deposits could be a harbinger of future investment growth, again contributing to a pick up in activity.

Of course, given my earlier comments on the robustness of money demand equations, we have to ask how much weight we should place on these empirical results. But this should

be a reason for undertaking more research rather than less and for being honest about what we do and do not know. And we have to remind ourselves that although there are considerable uncertainties surrounding the interrelationships between money and other variables, this is true of all of the other information we look at in forming our judgment about the appropriate stance of monetary policy. The difficulty in measuring the output gap is but one example. Indeed, we have made these many uncertainties explicit by presenting the projections of inflation which we publish in our *Inflation Reports* as probability distributions rather than point estimates.

We also spend a considerable amount of time looking at narrow money. Apart from its importance for our own balance sheet, and schedule planning at the Printing Works, one good reason for doing so is its long-standing statistical property as one of the best single leading indicators of inflation in the United Kingdom. This is a result which extends back at least as far as some pioneering work undertaken by Andrew Crockett⁽²⁾—now the Managing Director of the Bank for International Settlements—when he was a young economist at the Bank. It has always been difficult to provide an entirely convincing theoretical explanation of this statistical result, not least because it is difficult to explain why there is £400 in cash for each man, woman and child in the United Kingdom. But that shouldn't stop us—or you for that matter—continuing to look for one.

More recently, the research task has been to explain why narrow money velocity, which had been growing consistently since 1945, stabilised in the early 1990s and has now begun to fall. That is, why narrow money has been growing in excess of nominal spending. We offered some possible explanations for this in an article in the February *Quarterly Bulletin*.⁽³⁾ These included a slowdown in financial innovation—which might prove to be only temporary, though electronic money still seems difficult to establish with the public—and the move of the United Kingdom to a low-inflation environment. A number of other countries, notably Canada, Ireland, Italy, the Netherlands and Spain, have all experienced a reasonably strong correlation between lower inflation and negative velocity growth in the 1980s.

But the difficulty in explaining the path of narrow money does not mean that we should exclude it when forming our views on monetary policy. Indeed, as discussed in the article on simple policy rules by Alison Stuart in August's *Quarterly Bulletin*,⁽⁴⁾ we include, within the wide range of information variables which we look at, the results of the McCallum policy rule. This is based on the relationship between narrow money—adjusted for medium-term shifts in velocity growth—and nominal income. We find this—and the perhaps more familiar Taylor rule—useful reference points for reflecting upon the appropriate level of nominal interest rates, even if we do not regard either rule as

(1) See Thomas (1996).

(2) See Crockett (1970).

(3) See Janssen (1996).

(4) See Stuart (1996).

providing a monetary policy equivalent of autopilot. My suggestion that they might allow significant person-power reductions in our Monetary Analysis divisions has not been taken up with notable enthusiasm so far.

For what it is worth, neither the McCallum nor the Taylor rules imply paths of money and interest rates which are much off-track at the moment, certainly by comparison with the 1970s or late 1980s. The McCallum rule hints that, most recently, the monetary stance has been a little on the loose side. That squares with the message in our August *Inflation Report*.⁽¹⁾

I should mention for the sake of completeness that we also pay close attention to other monetary aggregates, including Divisia money. And in playing an active part in the preparations for European Monetary Union we have also undertaken some research on EU-wide monetary aggregates, although it has to be said that as yet we do not find any of the statistical results in this area particularly encouraging. The history of EU-wide money demand estimation seems to me like a classic case of measurement and method being put ahead of theory and common sense. That is never the sort of platform upon which effective policy-making is likely to be made or based.

That said, not least because we are a central bank, we have developed close links between research and policy-making in the area of money and credit. I have been unable, however, to find quite such close links in the area I turn to next, namely economic and monetary union. What does research tell us about whether EMU is a good idea and, if it is, whether the United Kingdom should join?

I appreciate, of course, that there has been a lot of academic work on issues such as whether the European Union—or some subset of it—is an optimal currency area; whether individual EU countries might be subject to large asymmetric shocks; and whether fiscal transfers could play a role in smoothing out differences across countries or across regions when nominal exchange rate adjustment is no longer possible. But this research has not completely answered the key questions, partly because the results have been inconclusive, and partly because any move to monetary union must ultimately be a step into an unknown policy regime which may, indeed is likely to, alter prior relationships.

I do not wish to sound negative here. Instead, I think it would be useful to explore the apparent gap between research and policy-making in the EMU context and to ask whether the underlying problem is that the policy-makers' questions are unanswerable or simply that insufficient work has been done to find the answers.

It is worth asking first what we can learn from previous monetary unions. Two of our economists⁽²⁾—Jag Chadha and Suzanne Hudson—have been looking at thirteen

previous monetary unions spanning the last three centuries, ranging from the England-Scotland union of 1707 to the most recent German reunification of 1989. They found it useful to categorise these former monetary unions into three broad types. First, nation-building unions, as for example in the cases of England and Scotland, the United States, Italy and, most recently, Germany. In some cases these involved relatively long transition periods, and they were all categorised by a strong underlying political impetus, including the political will to overcome various economic problems which arose during the process of political and monetary union.

Second, there have been monetary unions based on a leader-follower relationship—such as the union between Belgium and Luxembourg—the former Soviet Union and the CFA franc zone. In these cases the follower nations accepted—albeit with more or less tension and unwillingness—the leadership of a larger country which was also able to support the smaller countries in various ways. Third, some monetary unions have been between more or less equal countries which retained a large degree of national autonomy. These unions, such as the Latin Monetary Union, arose primarily from attempts to exploit the potential benefits of free trade, freedom of capital movements and other aspects of economic interdependence.

One interesting aspect of this historical overview is that it illustrates the extent to which some of the earlier monetary unions were held together for lengthy periods by forces which may be less strong today. These include greater labour mobility, including large-scale migration within Europe and from Europe to the United States and greater (but by no means perfect) price and wage flexibility, again enabling countries within the monetary union to respond more effectively to shocks. But there may also have been a sense in which the sovereignty of national monetary policy was felt less keenly by the public at large—certainly governments in the 19th century had significantly less democratic incentive to concern themselves with the employment and output consequences of monetary policy—and correspondingly less pressure for fiscal transfers between regions and countries as a means of responding to asymmetric shocks.

Many monetary unions did, however, eventually break down as economic conditions across the member countries became less uniform. It seems that the most enduring monetary unions are generally characterised by a clear form of political integration, or a particularly strong and clear leader-follower relationship. In other cases, an unwillingness to give up monetary sovereignty—or a desire to restore it—has tended to lead to a break-up of monetary unions lacking a strong element of political union. This does not necessarily imply that these unions should not have been formed in the first place—just that they may have outlived their usefulness.

(1) See the *Inflation Report*, August 1996, page 3.

(2) See Chadha and Hudson.

Almost all the monetary unions we studied were formed well before economists had developed the theory of ‘optimal currency areas’. But it is not clear that this would have made any difference to the outcome, since the theory of optimum currency areas has proved more useful as a framework for organising how we think about monetary unions than in providing an operational guide for policy-makers. You are probably more familiar than I am with the theory of optimum currency areas and with various attempts to test whether the European Union—or some subset of it—constitutes an optimum currency area. But I would like to share some thoughts with you about some of the puzzles which remain with me as a policy-maker.

I can understand the keen interest in the extent to which EU countries are likely to be subject to asymmetric shocks, or whether, even if they are subject to the same shocks, this could have different effects because of diverse production structures or different levels of wage flexibility across these countries. But the results reported in the academic literature—and some research undertaken at the Bank—do not provide a particularly clear picture. Some of the results suggest that it is possible to identify a core of EU countries—usually including Germany, France, the Netherlands, Belgium and Denmark—which are subject to quite closely correlated shocks. Others, however—including ourselves—have found it much more difficult to identify any clear core of countries which have been subject to reasonably symmetric shocks. Even where a core set of countries can be identified, the constituents of this core can vary quite sharply depending on the choice of methodology and sample period.

Similar difficulties arise in assessing the evidence on the diversity of production structures across EU countries. Most studies have found that production structures tend to be more diverse across US regions than across EU countries. But some have argued that this greater specialisation is, in part, the result of the United States being a monetary union and that greater specialisation will also result within a European Monetary Union. At the same time, of course, we need to remind ourselves that the regions within an individual EU country may not themselves emerge as an optimum currency area when subjected to these sorts of tests. An article in our August *Quarterly Bulletin*⁽¹⁾ shows that there are much more diverse responses to a monetary policy shock across industries within the United Kingdom than are found when analysing cross-country responses to similar monetary policy shocks.

Let us proceed for the moment by assuming the worst, which is that EU countries are likely to be subject to significantly asymmetric shocks, or exhibit significant differences in industrial structure which generate different responses to identical shocks. Does this necessarily imply that it would be inappropriate to move to a monetary union? The key issue now becomes how these countries would adjust to these shocks.

In the early literature on optimal currency areas it was usually assumed that wages and prices were sticky and that the boundaries of an optimum currency area would therefore depend on the area across where there was sufficient mobility of factors of production—labour and capital—to offset the effects of asymmetric shocks. Studies of the European Union have typically found that, although financial capital has become highly mobile as capital controls have been dismantled, imperfections in the European-wide labour market—not least language and cultural barriers—have placed considerable constraints on labour mobility. There certainly appears to be far less labour mobility across Europe than there is, for example, within the United States. And some argue that elements of what is known as ‘social Europe’ may even tend to reduce mobility further. The Posted Workers Directive, for example, requires workers from another member state to be subject to collective wage agreements in the host country, which reduces the scope for workers from high unemployment areas to bid for work elsewhere. Indeed, the whole question of the relationship between social and monetary Europe seems to us to have been insufficiently considered by policy-makers and economists. Otmar Issing, the Bundesbank’s Chief Economist, has made a similar point.

In the absence of sufficient labour and capital mobility, adjustment to asymmetric shocks is likely to require a movement in the real exchange rate. But in a single currency area this can be achieved only through wage and price flexibility. It is generally assumed that wages and prices are not perfectly flexible, especially over the short to medium term. So adjustment to shocks has to take the form of changes in quantities—including employment—rather than changes in prices alone.

But there do not appear to be any accurate, let alone user-friendly, measures of price and wage flexibility. For example, is the degree of wage/price rigidity any greater looking at the same product in different countries than it is looking at different products in the same country? I cannot claim to have any answers here, but I suspect strongly that we are not always sufficiently precise about the nature of price and wage flexibility which is most important when considering adjustments within a single currency area. Moreover, a further important unknown is the extent to which wage and price rigidities in individual countries reflect expectations about future monetary policy, which could change significantly as these countries entered European Monetary Union. Might wage/price flexibility be given a fillip by the act of entering a monetary union?

Yet another possibility would be to make more active use of fiscal policy as a means of offsetting asymmetric shocks. This might take the form of a combination of fiscal transfers *across* countries/regions—that is, an EU-wide fiscal policy—and transfers *within* a single country/region across generations—that is, domestic fiscal policy. But which of

(1) See Ganley and Salmon (1996).

these is the better option? Perhaps neither? Our understanding of the costs and benefits of European Monetary Union would be enhanced by further research on the relative merits of different fiscal policies and the costs these impose.

Another important issue is the extent to which different European economies react differently to interest rate changes. It is sometimes argued by critics of EMU that the United Kingdom, with its preponderance of variable rate housing finance, reacts more sharply to short-term interest rate changes than, for example, does Germany. Some recent work in the Bank casts doubt on that view, and suggests that the impact of interest rate changes on inflation and output is broadly similar in the United Kingdom and Germany, but it is another area in which we would be most interested in the views of other economists.

All of these uncertainties do suggest that there are certainly risks involved in entering a monetary union prematurely. In an ideal world it would be desirable to eliminate as many rigidities in the labour and capital markets as possible before entering monetary union, just as it is desirable to achieve as much nominal convergence as possible. Implementing a common monetary policy across the single currency area would then be a much smaller step to take—at least in terms of economics—since the incentives to retain national sovereignty over monetary policy would be correspondingly smaller.

Nor should we forget that, for a great many countries, national monetary sovereignty has in the past been a poisoned chalice. The freedom to inflate at a different rate than elsewhere has for many countries meant inflating more rapidly. Discretion has led to misbehaviour. So the advantages of locking-in a credible monetary policy should not be overlooked. But at the same time there is a risk that a prospective monetary union may converge not on the *best* inflation performance—read Germany—but on the *average* of all countries in the union. The latter is a much less appetising prospect. But is it likely? Again, answers on a postcard please.

Finally on EMU, I might mention the EMI work underway on the choice of *strategy* for monetary policy—for example, should the future ECB operate money or inflation targets, or some combination of the two? And on the choice of *instruments* of monetary policy—for example, do reserve requirements have a role? The Bank of England clearly has firm views on these issues, as evidenced by our existing approach to the target and instrument problem. But others, judging by *their* existing monetary infrastructure, clearly have different ideas. It would be useful to have some academic arbitration service come in and settle these disputes once and for all—a kind of Eco-Acas. Any volunteers?

Before I conclude, I would like to say a few words about the work of the Bank's Financial Stability Wing and look at the

links between research and policy there. We have established several units within that wing to carry out policy-related research. These span questions of market micro-structure, such as how bond and equity market structures affect the price formation process; research into banking as an industry; as well as research into the economics of regulation.

Today I intend to focus particularly on regulation, where we are currently intensifying our research efforts and building closer links with academics. Economics and finance research has an important role to play in the design and assessment of supervisory methods, in particular whether risks are being adequately captured and whether the regime gives appropriate incentives both to the firms and to the wider public. There are also important wider questions related to the appropriate scope of regulation and the costs and benefits.

As far as the structure of capital requirements for market risk is concerned, we have carried out our own research and also collaborated with academics in order to move towards more risk-based requirements. The first truly risk-based requirements for market exposures in the United Kingdom were those put in place for securities houses by the TSA, the forerunner of the Securities and Futures Authority. These requirements were developed in the mid-1980s and a good deal of the research into the design of the bond requirements was carried out in the Bank. The Bank was also involved in the SIB/LBS work to develop a simplified Sharpe model on which to base the requirements for a portfolio of equity positions. As a Stanford man, once taught by Sharpe and his disciples, I was pleased to learn of that. This early work—plus, of course, an enormous contribution from the Fed—was very influential in the later discussions in Basle on a standard to apply to the market risk of banks and in Europe on the Capital Adequacy Directive which applies to banks and non-banks—although agreement could not be reached on the use of the simplified Sharpe methodology for equity positions. An approach may be right in economic terms but may not always be possible to negotiate internationally. Likewise, the Bank carried out much of the original research behind the Basle requirements to capture counterparty exposures on swap positions.

The debate has now moved on from the setting of risk-based requirements for market exposures by the regulators to reliance on the firms' own in-house value-at-risk models. These use past data on returns to establish the level of capital required to protect against losses on a particular portfolio, subject to parameters (such as the confidence interval) and various safeguards, set by the supervisors. Use of these models within firms is relatively new and it was clearly essential that the supervisors understood the reliability of the model results and their degree of bias before using them to set capital requirements. Joint research⁽¹⁾ was carried out by Bank staff and William Perraudin at Birkbeck to look at this issue and the results were presented at a research conference on risk

(1) See Jackson, Maude and Perraudin (1995).

measurement and systemic risk which the Bank was involved in organising in conjunction with the Federal Reserve, Bank of Japan and BIS. In fact, the work showed that the different approaches to building the value-at-risk models did have a significant effect on their accuracy. The only type of model which delivered the 1% tail probability it was built to achieve was a non-parametric (simulation) approach using long runs of returns data.

Research now needs to move on into new areas such as how models could be built to encompass *credit* risk as well as market risk. We are currently carrying out some work in that area.

As I mentioned earlier, an important question relates to regulatory incentives—are firms given the right regulatory incentives to meet best practice? Clearly moral hazard is an important issue. But a parallel question is whether the incentives for individuals within firms exacerbate risks. We are looking at that question now. Different methods of calculating bonuses can create different incentives with regard to individual traders. In particular, poorly designed schemes could create incentives for individuals to take exposures which are not in the interests of the firm. Our study is looking at how firms actually determine their compensation packages and considers the implications.

Research is also needed into the development of new tools for supervisors. Last year we developed a model to rank banks according to their riskiness. Earlier work had looked at whether options pricing models using data on the equity value and deposits of eight large UK banks would provide a way of looking at the exposure of the deposit protection arrangements to a particular bank.⁽¹⁾ We are also considering whether to finance a study to look at whether neural networks applied to the banking data would help the supervisors to spot anomalies and act as an early-warning system. One of the conclusions of the post-Barings Arthur Andersen Review of Banking Supervision was that even more effort should be placed on building such tools. The French have already pioneered some work on bringing Artificial Intelligence to bear on supervisory issues.

These are all questions about how supervision should be carried out. There are also very important strategic questions which need to be addressed about the boundaries of supervision, and about the costs and benefits of particular approaches. Heroic attempts have been made by various academics to measure the costs of regulation; quantifying the benefits is even harder. It may be more fruitful to focus on the issues which regulation is attempting to address and to consider whether there are other lower cost ways of addressing them.

Perhaps the fundamental question is why financial services firms, and banks in particular, should be regulated by a public agency at all. Not all industries are subject to the kind of regulation we impose. One objective is to deal with externalities—the divergence between private and social

costs. For banks the externalities could be very large because of the vulnerability of the whole financial system—and the real economy—to shocks which undermine confidence in the banking sector. Another objective is to protect investors/depositors. Indeed, this objective is the one stressed in the Banking Act. Again one could ask why depositors need to be protected by regulation of banking firms, whereas consumers in general must exercise buyer beware?

Partly, this is because of the nature of the transactions, where funds are invested for lengthy periods, thereby making investors susceptible to fraud. Asymmetries of information between the bank and its customers are in general greater than is the case with other consumer transactions, which leaves bank customers peculiarly exposed. Depositors cannot easily judge for themselves the strength of a particular bank and its fitness and properness—although one of the issues here is whether more could be done to encourage investors to exercise greater judgment on the basis of more information. We have always favoured a deposit protection scheme which insures only part of any deposit, thereby helping guard against moral hazard problems.

Here the New Zealand approach to banking supervision is interesting, although not easily transferable to a much larger more complex and more international banking market like the United Kingdom's—almost all banks active in New Zealand are parented elsewhere, which is of course not the case here. They are placing much more emphasis on disclosure of information and credit ratings by the banks to the public and on public awareness of the risks in banking. They have also increased the incentives on directors in banks to ensure themselves that systems and controls are adequate.

Another issue is whether it would be possible to test the price which investors would be prepared to pay for regulation. Ideas are put forward from time to time about mechanisms which would do this—for example narrow banking. Would it be possible to establish tightly regulated narrow banks which would invest in low risk assets and which would offer depositors much more security, albeit at a price (a lower interest rate than that offered by non-regulated complex banks)? As only the narrow banks would be members of the payments system, would this protect against systemic problems? Or would this simply engender more instability elsewhere in the banking system? And how could the transition to such a new structure be managed?

I should say that, at present, we are generally satisfied that the regime we impose is not excessively costly, or excessively restrictive, or indeed unjustifiably lax. But a learning organisation, such as the Bank seeks to be, must always consider whether there are other ways of achieving the objectives of regulation with lower cost, particularly because of the danger that every time there is what is

(1) See Maude and Perraudin (1995).

perceived to be a regulatory failure there is pressure to tighten the regulatory screw another notch.

We are currently increasing our research into these wider regulatory questions and into the changing nature of systemic risk. Systemic risk has traditionally been viewed in terms of problems in the banking sector, with the failure of one or more banks leading to a collapse in confidence in other banks with implications for the real economy. One question is whether developments in the industry have substantially changed this risk or the risk of other types of contagion (for example through direct exposures between firms).

The question falls into three parts. The first is whether the risk of a bank failing is now substantially changed. Developments such as greater diversification across product ranges, availability of better hedging and risk control techniques have helped to reduce elements of risk. Against this, internal control issues are raised by the complexity of firms and the speed with which new instruments enable exposures to be built up. Overall, it is not clear that the banks are carrying smaller exposures than was the case hitherto, indeed risks have to be taken in order to make profit.

The second part of the question is whether the risks of contagion between banks have changed. The development of a real time gross settlement system (RTGS) has reduced the extent of exposures between the small group of settlement banks. But substantial (interbank, OTC and FX settlement) exposures between the settlement banks remain and RTGS has not altered the exposures of the very large numbers of other players (both bank and non-bank) to the

settlement banks. Most importantly the risks of confidence contagion remain.

The third issue is whether the links between the banks and the real economy are now looser, which would reduce the systemic impact of any problems. Here our current view is that banks' role in the payments system and as a prime home for transactions balances continues to make them a fundamental part of the functioning of the real economy. They also continue to have a central role in the provision of finance to the personal and small company sectors. Much of the research carried out in the past by the Bank on regulation has not been published, which with hindsight has not helped academic debate. We now intend to rectify this and take a more open approach to the publication of research and thinking in this area. One of the objectives will be to make more material available to researchers working outside the Bank on these issues.

That covers the three areas—money, monetary union and regulation—on which I wanted to touch. The Bank is also undertaking research in a great many other areas—on the real economy and on financial markets to name but two—which I have not had chance to mention here. And I recognise that I have raised more questions than I have provided answers in those areas I have covered. But that is the nature of research. And asking the right questions is at least a good starting point towards providing meaningful policy answers. To end with Keynes again, he observed that: 'Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back'. Whilst I would object to the 'madman' sobriquet, I certainly hope to be distilling some of the ideas from this and future MMF conferences in years to come.

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