The Economic and Social Review, Vol. 36, No. 3, Winter, pp. 189-203

What Can Academics Contribute to the Study of Financial Stability?

CHARLES A. E. GOODHART*
London School of Economics

Abstract: There were hardly any banking crises between 1939 and 1971, so their later re-emergence came as a surprise. Central bank supervisors responded practically by discovering and encouraging the adoption of current best practice in risk management by individual banks, without much theoretical input, whereas economists have mostly focused on models which abstract from default. But default is central to analysis of financial stability. Shubik pioneered introducing default into formal models, and we aim to develop this further. Meanwhile, estimating the probability of default (PD) for individual, or groups of, banks is central to the Basel II process.

I HISTORICAL BACKGROUND

From the late 1930s until about 1970 there was relatively little concern, academic or otherwise, about bank failures and financial instability. This was largely because there were no such failures. Table 1 and Figure 1, taken from Bordo, Eichengreen *et al*. (2001), show the frequency of various kinds of crises from 1880 to 1997. A remarkable feature of this is the virtual absence of either banking crises, or joint banking and currency crises, in any part of the world between 1945 and 1971.

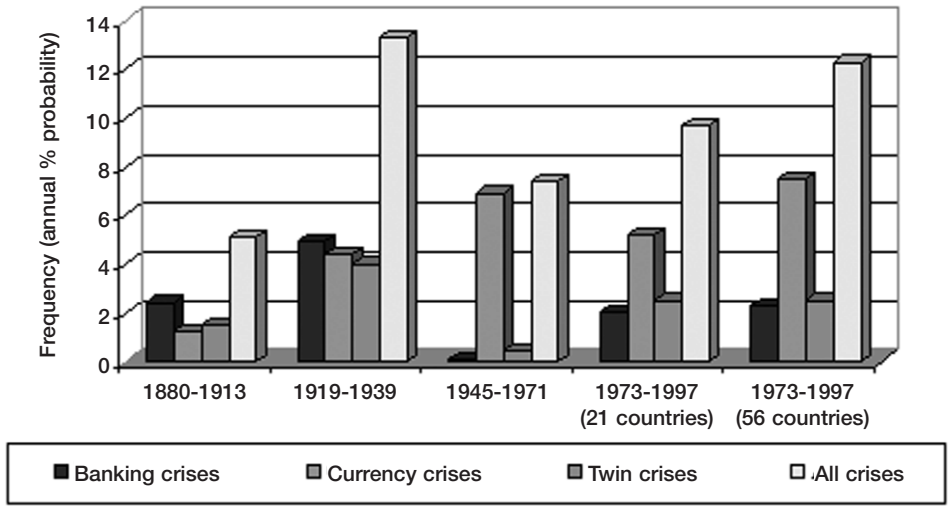
*This paper was delivered as the Central Bank of Ireland Edgeworth Lecture at the Nineteenth Annual Conference of the Irish Economic Association at Kilkenny, May 7, 2005.
e-mail: caegoodhart@aol.com

Table 1: *Crisis Frequency*

Year	*Banking Crises*	*Currency Crises*	*Twin Crises*	*All Crises*
1880-1913	2.30	1.23	1.38	4.90
1919-1939	4.84	4.30	4.03	13.17
1945-1971	0.00	6.85	0.19	7.04
1973-1997 (21 countries)	2.03	5.18	2.48	9.68
1973-1997 (56 countries)	2.29	7.48	2.38	12.15

Source: Eichengreen and Bordo (2003, Table 3.5).

Figure 1: *Crisis Frequency*



Source: Bordo *et al.* (2001, Figure 1, p. 56).

Why was this period unique in this respect? Contemporaries had previously identified excess competition as one of the causes of the inter-war financial crises, and many of their measures to stabilise their banking systems consisted of actions to limit competition, for example the Glass-Steagall Act; to encourage the formation of cartels and oligopolistic structures, such as the London Discount Market Association; and either to fix interest rates by fiat (as in Regulation Q in the USA) or through oligopolistic agreement (as in the UK building societies). The aim was to maintain profitability and a positive franchise value.

Furthermore, the advance of socialism led governments to intervene in the direction, rationing and control of bank credit, a tendency given further impetus by the exigencies of war finance in 1939-45. With many governments leaving the war with massive debts, a continuing defence burden and aspirations to a new Welfare State, it was natural to see their own financial intermediaries as a parking ground for their own debt, partly under the pretext of bank liquidity requirements. Under conditions of reconstruction and a dollar shortage, the authorities directed the constrained amount of bank lending for the private sector towards the large, exporting manufacturing companies. The service sector, importers and persons, apart from the latter's access to specialised mortgage institutions, S&Ls and building societies, were to be denied credit. So commercial bank assets consisted primarily of lending to government or to long-established large manufacturing companies. It was not efficient, but it did have the merit of being a safe system.

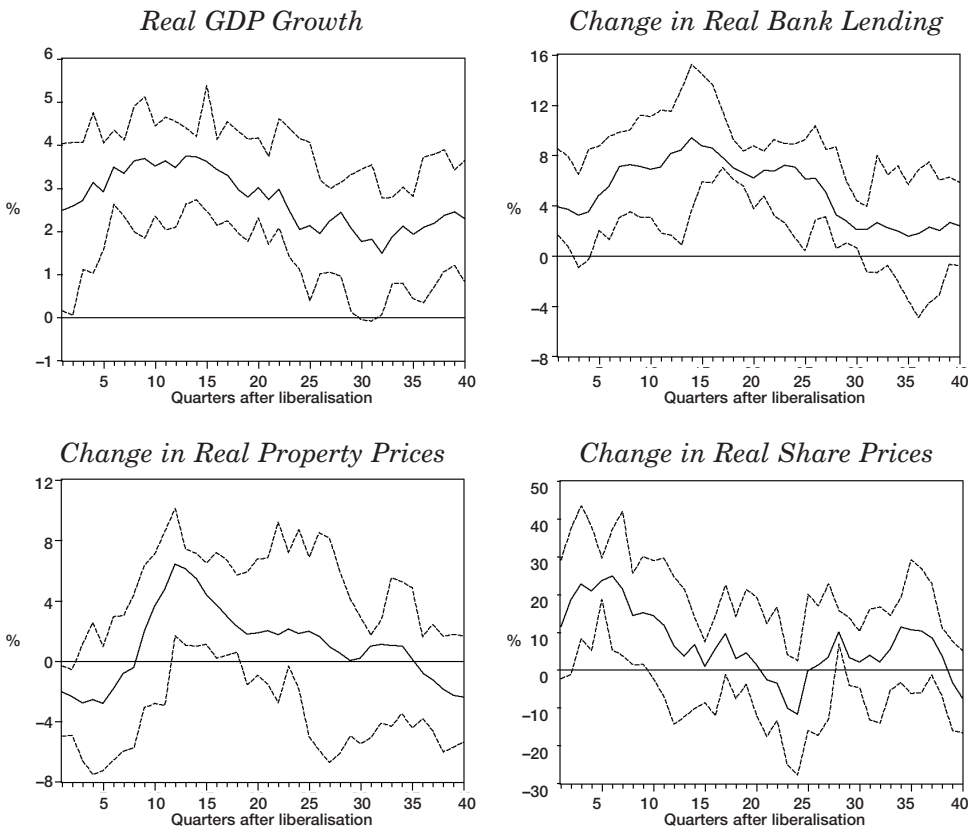
There were a number of reasons for the wave of de-regulation that began in the 1960s, including a restoration of preference for capitalism and free markets over dirigiste socialist planning, and technological developments in IT and travel, which in turn led to greater international competition. Banking systems became progressively liberalised, and liberalisation brought with it financial fragility, as demonstrated by Demirgüc-Kunt and Detriagiache (1998).

This latter was partly because liberalisation enhanced competition, and competition eroded profit margins. A fall in profit margins led in turn to banks economising on capital, in order to maintain target levels of return on equity, and to move into higher risk lending, e.g. on property, to restore their profitability. Moreover, prior to liberalisation, the banks had lived in a constrained, but feather-bedded world in which the professional faculties of risk-assessment and risk-monitoring had largely atrophied. When suddenly given the green light to go out and compete for business, the banks did not know when to stop of their own accord, especially since the post-liberalisation burst of lending usually led to an initial boom. This has been nicely documented by Demirgüc-Kunt and Detriagiache (1998). Figure 2 incorporating some diagrams illustrating this are taken from Goodhart, Hofmann and Segoviano (2004).

II CENTRAL BANK RESPONSE

It was not only the commercial banks who were unprepared for the unfamiliar exercise of risk management and control. So were the authorities, notably the Central Banks. At the onset of the Fringe Bank crisis the Bank of England had a small handful of officials, under the leadership of the Principal

Figure 2: *Post-liberalisation Cycles*



Note: Solid lines are sample averages, dotted lines are upper and lower quartiles.

of the Discount Office, who were involved in monitoring the whole banking system. The Bank neither licensed banks; that was done by the Department of Trade; nor did it really supervise them. Such supervision as was done was in respect of the Discount Market and the Accepting Houses, or merchant banks, since the latter's acceptances might be offered by the former to the Bank for rediscounting.

All that changed in a hurry after the 1973/74 crisis in the UK. That also coincided with other important bank failures, Bankhaus Herstatt in Germany and Franklin National in the United States. Both of these failures had international ramifications, and discussion of their implications led to the establishment of the Basel Committee of Banking Supervision, promoted by Governor Gordon Richardson of the UK, and an offshoot of the BIS Governor's meeting.

Around the world, and internationally at the Basel Committee, Central Banks, (and in those countries which already had specialised banking supervisory authorities), such as Canada, these authorities began to pay more attention to bank supervision. More resources were applied. The initial issues were largely practical, for example to insure that all banks had a supervisor, and that international banks with several subsidiaries were supervised on a consolidated basis.

With different countries having developed differing supervisory structures, the first objective of the Basel Committee was to try to discover what were the various existing supervisory arrangements already in place. The approach of that Committee, and of most national supervisory authorities, was to try to find out what appeared to be existing best practice, and then, subject to national particularities, to build on that. The aim was to see what already worked best, across nations and across banks, and then to seek to adopt those practices that had proven successful amongst those banks and nations whose control mechanisms were most admired. As a procedure it was pragmatic and evolutionary.

It was not, however, academic in character. I very much doubt whether those in charge of the regulatory/supervisory departments ever asked themselves what was the nature of the market failures that justified intervention, and, having identified such failures, what might be the optimal response. Instead the issues seemed practical and obvious; banks with insufficient capital, and often dodgy management, were escaping proper consolidated supervision. Not only were the issues seen as practical, but so were the proposed remedies.

Although the Bank of England had employed economists since the inter-war period, and had an Economic Intelligence Department at this time (1970s), no economist was seconded to, or welcomed into, the new Banking Supervision Department. Economists were in Central Banks to advise internally on money/macro issues, and to justify and to explain such policies externally to commentators and the general public, and they were neither invited, nor encouraged to get involved in supervisory matters. There was even in some quarters a certain satisfaction that bank regulation/supervision was a preserve of practical bankers, free of academic theorising.

While I did have that experience at the Bank – to the best of my now faulty memory – I cannot speak of the ethos at other Central Banks, financial supervisors, or the Basel Committee. Nonetheless, my impression is that economists were rarely used in this field, or asked for their opinions in these early years (1970s and 1980s). The vast majority of economic analysis was applied in the macroeconomic field, not on financial stability issues. Nonetheless, as Central Banks increasingly sought entrants with initial

economic training, some of these eventually were moved, in the regular course of personnel management, into banking supervision. Michael Foot, who transferred from the Bank to the FSA, becoming Head of Banking Supervision there, is a good example. Then gradually, but very gradually, the Financial Stability wing of Central Banks, and specialised supervisory institutions, such as the IMF, began to seek expert economic advice on such issues. But for the first couple of decades, or so, of its re-emergence as a major subject the pursuit of financial stability was not overly encumbered by much internal economic analysis.

III ACADEMIC DEVELOPMENT

On the academic side, however, there was not, in these same early years, in the 1970s and 1980s, much analysis being undertaken that could really help with modelling and resolving financial fragility issues. In earlier years, economic specialists in money and banking had been strong on institutional and historical knowledge; names such as Richard Sayers, J.S.G. Wilson and T.E. Gregory in the UK case spring to mind. They had direct knowledge of the many historical cases of bank failures; what had happened and why. But such (so-called) descriptive studies were being downgraded in professional esteem relative to more mathematically based models.

Then the economics profession took a further step along the road to analytical rigour, via the Lucas revolution whereby such macro-models are required, or supposed, to have optimising micro-theoretic foundations. But such models are quite hard to construct; indeed that in-built complexity is one of the underlying reasons for their academic éclat. To reduce the dimensionality of the macro-monetary economic problem, the standard assumptions are that the agents in each main sector, notably persons and companies, can be modelled via a homogeneous, infinitely-lived representative agent, who *always* manages, in due course, to pay her debts. This latter is known technically as the 'transversality condition'.

Armed with these simplifications, much, perhaps most, macro-monetary analysis in the recent decade has revolved around a three equation model, an IS curve, a Phillips curve and a Taylor-type reaction function. A typical example, taken from McCallum (2004) is:-

$$(1) \quad y_t = b_0 + b_1(R_t - E_t\Delta p_{t+1}) + E_t y_{t+1} + v_t$$

$$(2) \quad \Delta p_t = \beta E_t \Delta p_{t+1} + \alpha_1(y_t - \bar{y}_t) + u_t$$

$$(3) \quad R_t = \mu_0 + \Delta p_t + \mu_1(\Delta p_t - \pi^*) + \mu_2(y_t - \bar{y}_{t-1}) + e_t$$

Symbols: y_t = log of output, $\bar{y}_t$ = log of natural-rate output, p_t = log of price level, R_t = one-period nominal interest rate, v_t, u_t, e_t, = stochastic shocks.

There are numerous problems and shortcomings in this three equation model, some of which I have detailed elsewhere (Goodhart, 2004; 2005), but the main one that I want to revert to now is this transversality condition. What this assumption effectively does is to remove all default risk from private sector borrowing. If so, *every* agent can borrow, or lend, at the safe rate of interest.

Indeed, if there is no default risk, it is not at all clear why there is any need for money! All that is needed is a recording system, showing people's current net debit/credit position. There is no basis for a cash in advance requirement, since any seller knows that she can rely absolutely on any buyer's IOU being fully honoured, and earning the riskless rate until it matures. Moreover any seller may at any time immediately buy anything she wants, since her credit is always impeccable. By the same token and analysis there is no logical basis for money in the utility function. Indeed, to set up a model which simultaneously incorporates transversality conditions and an essential role for money, is tantamount to making a fundamental logical error. This is one reason why a few of us monetary economists have paid some attention to the apparently arcane question of whether, in a world without money, a Central Bank could, and would, still set nominal interest rates, relative to some, perhaps notional numeraire, (see Woodford, 1998; 2000 and 2003; Chapter 2, Goodhart, 2000).

What this implies is that the possibility of default is central to the analysis of any monetary world, (without default we re-enter the Arrow-Debreu-Hahn paradigm). Let me take a brief digression to consider the implications of this for the analysis of the nature, and evolution, of money itself. The main competing theories about the evolution of money are the transaction cost minimisation theory, of Menger, now up-dated by Kiyotaki and Wright, and the Cartalist, or credit theory of money, see for example *Credit and State Theories of Money* (2004, Wray (ed.)). In so far as the possibility of default is the main factor causing us to require immediate payment, in some generally acceptable medium of exchange, rather than accepting an IOU, then the normal means of payment will become the IOU of that agent in society whose probability of default is least; usually low probability of default is correlated with holding considerable power in the socio-economic system, as with temples, and above all with governments.

This is not to claim that commodity moneys do not exist; that all money is credit money. But let us take the two best known types of commodity moneys, the first of these are the precious metals, gold and silver. Let me make several points. First, an unstamped quantity of gold and/or silver, i.e. one not in the

form of a coin, would be a poor transaction medium, because a transactor would need to know its metallic fineness (carats), and that is specialised information. These precious metals need the intervention of a mint to provide reliable information on quality, and that too brings one back to the likelihood of default and debasement. Second, gold and silver were so valuable, relative to normal market transaction needs, that they were rarely used in day-to-day purchases. For the latter, subsidiary coins, (not full-bodied), or tokens, or IOUs were more common, (see on this a forthcoming book by G. Selgin, *Good Money*, about the provision of copper coins to meet transactions needs in the UK's Industrial Revolution (1775-1820)). Third, Gresham's Law meant that if the market value of a metal rose above its prescribed monetary value, then that precious metal would disappear from monetary circulation. So market forces would tend to ensure that precious metal moneys were, on average, overvalued relative to their commodity valuation.

The second point is that, amongst the most common and best known commodity money in Western history, has been cattle; we derive the word pecuniary, which means monetary, from the Latin root of pecus, a cow. If we think of a basis for cost-minimisation in transactions, we tend to require the following qualities in our commodity moneys:- Standardised, Durable, Portable, Divisible. How closely do cattle meet these requirements?

While commodity moneys have surely existed, nevertheless the essence of a monetary system is that this has become based on the IOUs of the most credit-worthy agent in that system, the least likely to default, notably the government. The question of whether money was essentially a commodity, or credit, could be, and was, debated fiercely under the Gold Standard, but the move to fiat money systems should really have resolved that issue by now, in favour of the credit or Cartalist theories of money.

Without default we do not need money; and we do not need financial intermediaries either. If all agents always repay their debts in full, what more information does a creditor need, (and note that that also implies a perfect resale market for all IOUs of any initial maturity)? Without default, all agents can themselves lend, or borrow, at the safe rate. Why is there any need for banks as financial intermediaries; indeed there is none. In my view the foremost text, currently available, on monetary economics is by Woodford, (2003) *Interest & Prices*. Woodford makes absolutely no reference to commercial banks; there is no financial intermediation. In most theoretical macro-monetary models there is only a Central Bank, which supplies base money and sets interest rates on a short-term safe, default-free asset. There are no commercial banks, so obviously no bank failures, no risky assets and no risk premia on defaultable assets. It is remarkable that money/macro analysts have managed to construct such a massive theoretical, and indeed empirical,

edifice on such sanitised and implausible foundations. But whatever one may think about the macroeconomic analysis, it can provide no basis whatsoever for the study of financial fragility.

IV DEFAULT

The implication of the above analysis is straightforward. If academic theorists are going to be able to provide much help in the analysis and modelling of financial fragility, then they are going to have to include default as a central element of their analysis and models.

There has been an understandable reluctance to do so, in some large part because modelling default is difficult. It is difficult because it is not a continuous process, such as is more easily handled mathematically. Instead it is akin to an on/off state switch. Firms, and persons, are either declared bankrupt, or are supposedly solvent. Moreover, the penalties for bankruptcy, to reputation, to self-esteem, to access to future credit, etc., are frequently non-pecuniary, and it can be difficult, or impossible, to give a monetary equivalence to such penalties.

In my view the best, current approach to modelling default has been that developed by Martin Shubik, and his colleagues, (Shubik, 1973; Shubik and Wilson, 1977; Dubey, Geanakopoulos and Shubik, 2000). What they do is to assume that agents assess the likelihood of future (economic) conditions occurring, and then choose a policy which will have state-varying probabilities of default, i.e. bankruptcy. Naturally if a bad state occurs, given the initial policy choice, the likelihood of bankruptcy occurring is much higher than if a good state occurs.

So, given the expectations about the future states of the world, (which expectations are usually assumed to be rational), bankruptcy probabilities will be endogenously chosen, though the number that actually occurs will be contingent on what state of nature arrives. This endogenous choice of bankruptcy obviously depends on the costs of becoming bankrupt compared with the benefits of choosing a higher bankruptcy probability: these benefits are twofold; first a riskier strategy (normally) offers a higher, mean expected return. Second, in the bankrupt state, you get to save on financial out-payments – as the Argentineans were pleased to discover.

Obviously if the penalties on bankruptcy are zero, everyone defaults; but this is socially sub-optional, since equally no one lends and there is no financial intermediation. Equally if the penalty is infinite, (for example, the whole family of a bankrupt is stoned to death), no one defaults, but that is equally sub-optimal, since no one would borrow. There must be an interior

optimum, given bankruptcy costs, future economic expectations, and risk preferences. In the medium term, however, penalties attached to bankruptcy are not fixed, but depend on a variety of factors including social norms and the legal framework. Perhaps because macroeconomists have shied away from dealing with bankruptcy directly, not nearly enough academic economic research has been undertaken on the questions of the socially optimal framework for bankruptcy laws, and on how to set penalties so as to provide the best balance between the interests of creditors and debtors, and between excessive and insufficient risk-taking in the economy.

This approach leads to models of endogenous, state-dependent bankruptcy, with the non-pecuniary costs of default entering into the objective function of agents. The particular way in which default is modelled in the Shubik approach is that an agent will decide a policy which in state of the world j will lead her to default on x per cent of her borrowing. That, however, raises a problem. Because of reputational effects, cross-default clauses, etc., most borrowers, and especially banks *vis à vis* their depositors, either default simultaneously on all their liabilities, or not at all. An agent cannot normally refuse to pay out anything on one tranche of borrowing, and yet go on fully meeting the payment schedules on other tranches.

In what I hope will be an important paper in this field, two of my colleagues (Dimitri Tsomocos and Lea Zicchino) researching in this area, are proposing the argument that there is an equivalence, an isomorphism, between deciding to default in state j on 5 per cent of one's debts for sure, and deciding to have a similar 5 per cent probability of defaulting on *all* one's debts simultaneously. If there are *enough* agents, so that idiosyncratic chance gets removed by the law of large numbers, then the situation in each case is indeed the same. But if there are only a few large banks in a system, then a certain default rate of 5 per cent of assets, is *not* the same as a 5 per cent probability of default on all assets. But large banks are almost never closed and liquidated. Instead they are quickly reconstituted, with depositors taking an, often small, proportional loss; that, in effect, takes us back to a default rate, of say 5 per cent, on all assets.

Particularly if default is modelled in terms of choosing a probability of default, j, in state of the world, x, then it matters precisely who does default. The default of large agents/banks, or more inter-connected agents/banks, has a greater impact on other agents/banks, than in the case of a smaller defaulter. In any case risk-preferences, and expectations of future economic conditions, differ from agent to agent. So this approach naturally leads on to including heterogeneous agents/banks in our models. This again causes another problem, which is that a model containing both (endogenously chosen) default and heterogeneous agents is almost bound to have a much larger

dimensionality, more variables, than the standard macro-models now in common use, and that makes such models less easily tractable, either mathematically or empirically.

After all this it will not be a surprise for you to hear that it is just such models, incorporating default as a central factor, and with heterogeneous agents, banks and households, that I and my colleagues have been working on in the last few years.

V PROBABILITY AND DEFAULT

Now if default is the key feature of financial fragility, then the probability of an agent defaulting becomes the key statistic for risk management. And, of course, PD, or the probability of default lies at the heart of current credit risk assessment, notably in Basel II. How can academic economists help in the estimation of PD? There are several ways. First, a stylised fact is that the occurrence of non-performing loans (n.p.l.s), and of defaults, (both for banks and non-banks), is not constant, but is time and state varying. One relatively straightforward, and essentially empirical, exercise is to explore what variables Granger cause the time series fluctuations in n.p.l.s, or bankruptcies, in some class of borrowers. A particular version of such a method is to identify a set of specific crisis events, and examine what common factors preceded them. There is a vast literature on this.

Such, largely a-theoretical, work may suffer somewhat from the Lucas critique (or indeed Goodhart's Law), that if a stable, strong relationship were to be found between a prior set of variables and some kind of subsequent crisis, then agents and markets would come to anticipate the likelihood of that, and thereby forestall it. The main factor, however, which is currently found to explain n.p.l.s and bank crises is excessively fast prior expansion in bank lending (and in broad money). This empirical work may be rescued from the Lucas critique by a combination of our inability to distinguish between trend and cycle looking forward, and the engagingly optimistic human trait of believing that each cyclical upswing is the start of a new better trend; or in the common parlance a belief that we are entering a 'new economy'.[1]

[1] Ministers of Finance are particularly prone to this delusion since their job, their raison d'etre, is to introduce reforms that will raise average productivity, and lower the natural rate of unemployment. So if productivity rises, or unemployment falls, they see it not as a cyclical phenomenon but as a sign of the success of their policies. So, a fiscal rule that demands surpluses during economic upswings to offset deficits during downturns is almost bound to fail, since Ministers will rarely see themselves as being in an above-trend state. Such above-trend periods are far easier to identify looking backwards than looking forwards. (*contd*.)

Another, a-theoretical way of estimating PDs, which economists have developed, is to try to back out the implied default risk of an agent from market valuations, whether equity or bond prices, using Merton-type models, or from yield spreads. I have certain reservations about the likely accuracy of such implied predictions, both because the market may itself be prone to excessive swings of optimism and pessimism, and have insufficient information, as in the case of Enron, and because the method for backing-out default risk may not be able to distinguish between variations in credit risk and other factors, such as liquidity risk. Nonetheless, empirical models of this kind are now an important part of the armoury of economists and supervisor/regulators.

This approach has normally been used to estimate the PD of a single agent. Now, however, in a valuable and original step forward, some Austrian economists, connected with the Austrian National Bank (Elsinger, Lehar and Summer, 2003) have applied the same Merton-type methodology to seek to assess the PD of a portfolio of banks. Thus can we use such models to predict the likelihood of failure of bank i conditioned on the prior failure of bank j?

As noted earlier, defaults, whether of banks or bank borrowers, are not constant over time but tend to come bunched together. An important question is what causes such temporal bunching. There are two main potential causes. The first is that a common external shock may be responsible; the second is that a failure of one agent may make a second agent more fragile because of positive inter-connections between them. This second source of fragility is normally termed contagion. It is important to be able to distinguish between the two causes of bunched defaults, because the appropriate regulatory remedies will differ. In particular, rescuing individual banks will appear much more sensible if the main cause of financial fragility is contagion, rather than a common shock.

One of the most obvious channels of interconnectedness between banks, and hence of potential contagion, is the interbank market. Recently considerable work has gone into the estimation of whether, conditioned on the failure of one bank, other banks would also fail as a result of interbank defaults. Examples of this genre include Wells (2002), Upper and Worms (2004) and Furfine (2003). In general, the results of this work are quite encouraging, especially when it is assumed that the failing bank can realise its assets *quickly* and thereby pay-off creditors a significant proportion of its indebtedness.

Footnote 1 continued

Complaints about past failures to seize on good economic conditions, for example to reduce debt levels, are easy to make *after the event*, but harder to put forward convincingly at the time, when forward-looking expectations about the future are (excessively) optimistic.

This latter appreciation leads on to three further issues. First, how quickly will reliable and credible information on the extent of the insolvency (of the initial failing bank) be available? Second, and conditional on the first issue, how far will agents withdraw funds from those other banks perceived most at risk, (but not necessarily insolvent), just on the precautionary principle? Third, how far will asset sales, either by the initial insolvent bank, or, much more likely, those that perceive themselves at risk from secondary withdrawals (see issue 2 above), drive down asset prices, and thereby weaken the solvency status of all the remaining banks, (besides adding to general fears).

If these extra conditions are met, so that there is quick, reliable, credible information; few secondary withdrawals from other banks (on the precautionary principle); and not much panic selling of assets, then the generally favourable results from these studies, i.e. that there is little contagious risk from the interbank market, will stand; but if not, not.

What this suggests, in other words, is that there are potentially multiple channels for contagion, and that any one channel may, or may not, become serious depending on prior conditions. This again makes it difficult to undertake any fully satisfactory analysis and modelling. Once again, however, we hope that the modelling strategy which we have been exploring may allow us to examine multiple, simultaneous channels of contagion. But our work is at best only a start.

VI CONCLUSIONS

To conclude and summarise, financial fragility, in the shape of large-scale bank failures, had been quiescent as a major concern between the late 1930s and the early 1970s. When it did then re-emerge as an important issue, the practitioners were not, at any rate initially, keen to seek academic economic advice. They saw the issues as being essentially practical, requiring pragmatic and evolutionary solutions. Moreover, the mainstream theorists in the macro-monetary field were focusing primarily on models which, by assumption, excluded the possibility of default. So that was not much help.

Indeed if economic theory, and formal models, are to provide analytical support and guidance for issues relating to financial fragility, they must make the modelling of default a central feature of their work. This is not an easy exercise, although the treatment of default as an endogenously chosen variable, which was pioneered by Martin Shubik, seems a good starting point.

In the meantime, of course, assessment of the probabilities of default lies at the heart of risk assessment in general, and of Basel II in particular.

Economists have made considerable advances in developing techniques for assessing PDs, notably Merton-type approaches. There does, however, remain a difficulty in distinguishing amongst the causes of bunched defaults, i.e. financial crises, between the common effect of an adverse external shock and contagious inter-actions between banks. In this latter field much remains to be done.

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