A Conference on Federal Credit Allocation

by Joseph G. Haubrich and James B. Thomson

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Introduction

It is 10:00 a.m., and Paul Davidson is telling the assembled economists how to pull rabbits out of a hat. Metaphorically, of course: Professor Davidson was discussing the assumptions behind a thought-provoking paper presented at the Conference on Credit Allocation: Theory, Evidence, and History, held last October 17–19 in Cleveland.

The Federal Reserve Bank of Cleveland and the *Journal of Money, Credit, and Banking* sponsored this conference to support research into the costs, causes, and consequences of credit allocation by the federal government.[1] It is one of those peculiar paradoxes that federal credit allocation remains an esoteric topic despite the general familiarity with student loans, deposit insurance, and the Federal Reserve's influence on interest rates. The conference aimed to bring together an emerging body of work that looks at these issues from the standpoint of modern economics, emphasizing both common concerns and methodological differences to highlight an area that deserves greater attention.

In a world of scarce resources, *something* must allocate credit, be it the marketplace or the government. In a perfect world, prices are the most efficient method of accomplishing this. In a world with market imperfections, including significant information costs, the government might improve upon the market allocation system. However, public choice theory reminds us that government imperfections can lead to credit market intervention that reduces society's welfare. Therefore, it is important that we understand the nature of the market imperfections and the alternative solutions.

Understanding the actual effects — intended or otherwise — of particular programs is perhaps the most important immediate goal. But in thinking about the future, and in removing the prejudice about what seems "natural" or "politically feasible," both critical theory and historical studies have a place.

The papers reviewed here were grouped into three sessions at the conference. The first presented a general overview of the problem by examining the broad rationales for credit allocation: abstract market defects and the concrete historical record. The second session took a closer look at specific programs and regulations. Housing, bank capital requirements, and

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community reinvestment were examined analytically, empirically, and as the outcome of a political process. The third session focused on a number of issues related to pensions and federal pension guarantees. The sheer amount of money tied up in pensions makes the consequences of inept policy particularly severe — and the need for research correspondingly great.

I. Session 1: What Can Be, Might Be

Stephen Williamson first examines whether information problems justify government credit allocation. Ronnie Phillips then looks at the debate over credit allocation during the New Deal, when many current programs were first introduced and many more radical proposals were seriously considered. Finally, Marvin Goodfriend stresses the credit allocation inherent in current Federal Reserve and Treasury practices. Taken together, the papers produce a strong sense of "what can be, might be" — for better or worse.

Williamson

In "Do Informational Frictions Justify Federal Credit Programs?" Stephen Williamson evaluates the effectiveness of government credit programs using two models with imperfect information. In these models, informational frictions lead to a credit market with many realistic features, such as bonds, banks, and bad borrowers, and with flaws, such as credit rationing.

The first model looks at an economy where people must bear a cost to learn the true state of the world. (These models are hence known as "costly state verification" models.) This provides a motivation for debt and for credit rationing. Increasing the interest rate on a loan, for example, reduces the chance that the borrower will make those higher payments and thus boosts the expected verification costs. This rations some borrowers out of the market, because offering to pay a higher interest rate will not get banks to lend to them. The market treats identical borrowers differently; some get credit and some do not, even though the overall return on their investment would exceed the market interest rate.

The apparent market flaw, however, does not immediately imply that a governmental solution exists. Williamson shows that if the government credit program breaks even, neither lenders nor borrowers profit. Consider a credit guarantee

program that the government funds by charging lenders an insurance premium. With a hike in the interest rate, the bank does not bear the full increased cost of default directly, so banks overall charge a higher rate. They do bear the higher cost in the form of heftier insurance premiums, however, so their expected return can fall. Borrowers then face a steeper interest rate, while lenders get a lower expected return. This lower return means that lenders supply less capital and credit rationing gets worse. In other words, everybody loses.

The second model looks at a market where lenders must screen out bad borrowers. As lenders deny some borrowers credit, the government has a potential concern. As in the first model, however, subtle perverse effects arise from government credit allocation. Without government intervention, lower-quality borrowers, who face higher interest rates, never try to pass themselves off as high-quality borrowers because, if discovered, they get no loan at all. If the government offers loans to people who have been denied credit, it decreases the penalty for those who misrepresent their type. This in turn raises lenders' screening costs, exacerbating the credit problem as more resources get used up in overhead and fewer are available for borrowing.

On one level, Williamson's results may seem obvious: An unfettered market provides the best possible contracts for borrowing and lending. More important, the paper rebuts the oft-heard charge that market imperfections create a need for government intervention. Williamson goes well beyond such general issues, however, and shows that government intervention is not only unnecessary but also may prove harmful.

Paul Davidson criticizes the entire tradition behind the Williamson paper, that of classical theoretical economics. For example, he points out and questions the statistical assumptions regarding risk that allow inferences about future default rates.[2] Davidson also questions the model's informational assumptions: Could private markets efficiently uncover the information and replace the implicit insurance of banks with an explicit form? Is it true that individuals know their default risk better than lenders do?

■ **2** Technically speaking, the stochastic process must be ergodic, with the time average of past values converging to the phase average across states (see Breiman [1968], chapter 6). Strictly speaking, Williamson avoids this problem because agents in this model have direct knowledge of the relevant probabilities. In actual practice, however, people must learn this from experience, which again raises the question of ergodicity.

The Davidson critique boils down to two central concerns: 1) Which assumptions best capture the real world (that is, do credit market imperfections arise from imperfect information [Williamson] or non-ergodicity [Davidson]), and 2) What vital elements has Williamson left out (competing lenders? multiple loans?). These are hard questions that are well worth thinking about, but the Williamson paper, by clearly and cleverly drawing conclusions from a well-specified set of assumptions, survives the criticism as one important way to proceed.

Pure logic cannot settle such disputes. Empirical evidence can't either, but it can help. Recent work by Berger and Udell (1992) finds little evidence of credit rationing. The classic work of Ellsberg (1961) indicates that people's perception of risk may be based more on the fear of a vague "uncertainty" than on a statistical calculation of probabilities.

Phillips

In "An End to Private Banking: Early New Deal Proposals to Alter the Role of the Federal Government in Credit Allocation," Ronnie Phillips documents that the financial reformers of the early New Deal had a list of concerns that are still voiced in policy circles today. Banks taking on riskier loans to increase profits, excessively harsh bank exams, small businesses starved for loans, Federal Reserve accountability, and the incentive effects of deposit insurance were only some of the topics the reformers considered. Not surprisingly, many policy prescriptions of that era also look familiar, encompassing narrow banks, a Reconstruction Finance Corporation, and changes in the Fed's discount window policy.

Phillips describes how the politicians and economists of the time clearly understood the important distinction between money and credit. Credit is the transfer of real resources from lenders to borrowers; money is the means of payment, or the medium of exchange. The proposals seriously considered 100 percent reserve banking, establishing small mutual savings associations for local lending, and extensions of the Reconstruction Finance Corporation to allocate credit globally among local associations and between larger corporations. One set of lessons from the New Deal debates that deserves more emphasis is the objections people had to the proposals — the fear of political control, evidence of ineffectiveness, and lack of specific objectives.

A less direct lesson concerns the importance of history and the path-dependent nature of economic experience. Phillips strongly suggests that a series of historical accidents lay behind the New Deal's rejection of the proposals to separate money and credit. Roosevelt's failure to consult Senator Carter Glass before appointing Marriner Eccles to the Federal Reserve chairmanship led to Glass' obstructing key reforms. A prominent senator supporting the reforms, Bronson Cutting of Nevada, died in a plane crash before the measures could be put to a vote. These facts strongly suggest that our current system has arbitrary components and deserves a fresh look.

Walker Todd's discussion heightens the relevance of the historical perspective provided by Phillips. He brings out the larger political economy issues framing the 1930s' debate, showing how the early New Deal reforms were opposed both by those advocating increased government involvement and by traditional fiscal conservatives. The continuing importance of these strands of thought means, paradoxically, that the historical record can provide a useful commentary on current proposals.

Goodfriend

Marvin Goodfriend provides a natural follow-up to the history lesson, arguing persuasively that we live with the adverse consequences of not facing up to the distinction between money and credit. In "Why We Need an 'Accord' for Federal Reserve Credit Policy," he suggests extending the 1951 Fed–Treasury accord, which eliminated the central bank's commitment to support government bond prices. Goodfriend similarly wants to free current Fed credit operations from potential abuse because of concerns over fiscal policy.

The author defines monetary policy as a change in the stock of high-powered money, while credit policy is a change in the central bank's assets that keeps the stock of high-powered money fixed. In his view, an effective central bank should not be distracted by entanglements peripheral to its mission. The accord of 1951 effectively freed Federal Reserve monetary policy from entanglement with fiscal problems, but the necessary credit actions of the central bank (especially as the lender of last resort) currently have little or no protection. Applying three basic principles would provide such protection: Limit assistance to illiquid but solvent institutions, do not

use credit policy to avoid congressional authorization of funding, and do not directly transfer Fed surplus to the Treasury.

Applying these rules would have some nontrivial implications. Quite clearly, it would prevent discount window lending from delaying the closure of insolvent banks (see Todd [1988, 1993]). Less obviously, it would also affect foreign exchange intervention. When the Federal Reserve buys German marks, for example, it acquires international reserves that increase the money supply; this constitutes monetary policy. But usually, the Fed acts to "sterilize" the intervention, offsetting the increase in reserves by selling domestic securities: Sterilized intervention thus constitutes credit policy. Goodfriend wants this type of intervention to be subject to explicit congressional authorization. Similarly, he believes that Congress should authorize "warehousing," wherein the Fed buys foreign currency from the Treasury and agrees to sell it back at some point in the future. In both cases, managing the balance-of-payment deficits constitutes proper fiscal policy actions of the U.S. government. Central bank activity obscures the funding process and would fall under the proposed accord.

The "credit accord" would also stop the transfer of Federal Reserve surplus to the Treasury (authorized by the Deficit Reduction Act of 1993). The Fed surplus is part of its capital account and represents retained earnings. Traditionally, the central bank has maintained a surplus account equal to paid-in capital. Goodfriend explains how the transfer, which the Fed finances by selling Treasury securities in its portfolio, results in no actual deficit reduction in the long run. That's because once the securities are sold to the public, the Fed no longer remits to the Treasury any interest earned on Treasury securities. Over time, the loss of this revenue offsets the surplus transferred.

E.J. Stevens' discussion traces the problems that Goodfriend seeks to resolve to an even deeper source: the lack of clear objectives for Federal Reserve policy. Given a pessimism about any near-term change in this situation, an accord may serve as a second-best way to extricate the central bank from inappropriate transactions. This immediately raises two questions: Which transactions are inappropriate, and who should be party to the accord?

Stevens argues that in most cases, the Fed is not the most appropriate party to the accord. Under the Federal Deposit Insurance Corporation Improvement Act of 1991 (FDICIA), an accord already exists limiting Fed lending to insolvent

institutions; thus, a tune-up may be desired, but is a major reform in order? Likewise, the Treasury and Congress can *by themselves* refrain from using the Fed for underhanded financing, be it by foreign exchange warehousing or the transfer of surplus funds.

Ultimately, the issue is that while Congress and the Treasury may not want a commitment to avoid entangling the Fed, the Fed may desire such a commitment. It really does dieters no good to know that if all ice cream manufacturers voluntarily ceased production, they could stay thin. The more difficult issue that Goodfriend and Stevens wrestle with is the actual importance of the particular problems Goodfriend cites.

II. Session 2:
Specific Programs

The second session of the conference looks at specific government programs in more detail. Allen Berger and Greg Udell evaluate risk-based capital requirements for banks. Since such requirements alter the relative cost of funding for different types of assets, the policy may have credit allocation consequences. Charles Calomiris, Charles Kahn, and Stanley Longhofer then model the credit imperfections in housing markets and assess the possibility of beneficial government intervention. Finally, Anjan Thakor and Jess Beltz look at the political economy behind rules that target bank credit toward specific groups.

Berger and Udell

In "Did Risk-Based Capital Allocate Bank Credit and Cause a 'Credit Crunch' in the United States?" Allen Berger and Gregory Udell investigate the reallocation of bank credit from loans to securities in the early 1990s. In searching for the cause of this portfolio shift, they test a variety of possibilities, including the imposition of risk-based capital requirements, tougher loan examinations, increased leverage requirements, and several nonregulation-based reasons. Berger and Udell are the first to examine all of these competing theories simultaneously.

The study utilizes an extensive data set covering quarterly numbers on almost all U.S. commercial banks between 1979 and 1992. It is also distinguished by the use of a control period that lets the authors determine if the early 1990s look different enough to merit designation as a "credit crunch." To do this, Berger and Udell estimate a series of supply equations

for bank credit and then test for differences in credit behavior between the crunch period and the control period.

They also compare the portfolio allocation decisions of well-capitalized banks with those of undercapitalized banks, since a number of the hypotheses predict differences in portfolio allocation effects across these subsamples. *How* the supply equations shift allows Berger and Udell to distinguish between the competing theories.

The results provide little support for most of the supply-side theories of bank portfolio shifts in the early 1990s. For example, the estimated supply equations show that banks with low risk-based capital ratios did not reduce their lending or increase their securities between the control period and credit crunch more than did banks with higher risk-based capital ratios. The other supply-side stories provide a better explanation than risk-based capital, but still seem quantitatively unimportant. The demand-side theories fare better, though perhaps only because their effects are harder to pin down with banking data.

Berger and Udell's conclusions differ from previous work showing that risk-based capital requirements significantly alter banks' portfolio behavior, such as Haubrich and Wachtel (1993). Much of the discrepancy probably can be traced to different empirical methods. Berger and Udell stress the importance of the control period and of assessing a credit crunch only in relation to the control. By claiming that a credit crunch occurs *only* if the loan supply function differs between the crunch and the control, the paper drives this point too far. The imposition of risk-based capital requirements has increased the number of capital-constrained banks; these institutions then reacted to the constraints. The behavior of a capital-constrained bank during the control period, however, need not differ from the actions of one facing risk-based standards. Tough regulators could have caused the problem either because they got tougher or because banking conditions exposed their inherent toughness. This is really the old economic distinction between movements *along* a supply curve versus movements *of* a supply curve. A decrease in demand can cause a reduction in equilibrium quantity even if the supply curve does not shift.

Merwan Engineer provides a thoughtful commentary on the Berger and Udell paper. He correctly points out that since risk-based capital guidelines were imposed internationally, a comparison with other countries may help to resolve the issue in dispute. He also points out that a common problem is estimating supply equations for heavily regulated industries such as banking: Heavy regulation generally means that supply relations change frequently, making it difficult to get a fix on them. In the case at hand, variables measuring bank risk influence bank portfolio choice via regulatory behavior, so when regulations change, the estimated relation should change. Finally, he argues that the issues considered fit into a broader context that was not raised in the paper: Were the new capital standards wise, and if so, were they adopted at the right time?

Calomiris, Kahn, and Longhofer

In "Housing-Finance Intervention and Private Incentives: Helping Minorities and the Poor," Charles Calomiris, Charles Kahn, and Stanley Longhofer look at the goals behind government housing programs, the possible market imperfections that may justify such intervention, and the costs and benefits of the intervention. Economists typically fall back onto equity and efficiency issues when undertaking such an analysis of government programs. Unfortunately, while equity is the main motivator of government housing intervention, equity (unlike economic efficiency) is a slippery concept with different meanings for different individuals. Consequently, even though efficiency is the main yardstick for examining federal housing intervention, Calomiris et al. provide the reader with a framework for understanding the equity issues. They define three types of equity — equitable procedures, equal outcomes, and retributive justice — and illustrate how each may imply a different form of intervention or program design.

After providing an overview of the different types of housing interventions, the authors outline four different classes of housing market problems that government interventions could be designed to solve: wealth inequality and poverty, informational externalities, bigotry, and rational discrimination. Although the issues and relevant literature on each class of problem are reviewed, the presentation deals mainly with rational discrimination.

Rational discrimination in housing finance arises from informational asymmetries associated with evaluating mortgage applications. At each stage of the credit evaluation of an applicant, the lender must determine whether to

continue collecting information or to stop (that is, to deny the loan). Calomiris et al. argue that if nonminority loan officers find it harder to interpret signals from minorities, then they must make the loan relying on fewer informative signals, and the bank both denies a larger share of minority loan applications and faces a higher degree of minority default. As a result, the lender's costs of processing minority applications are higher and its expected return is lower. Therefore, lenders will employ more conservative stopping rules to minority applications.

This is obviously discrimination, since otherwise identical minority and nonminority applicants face different probabilities of receiving credit. It is rational because the higher denial rate is not a function of bigotry, but rather is based on the lender's private benefits and costs associated with information collection.

The rational discrimination model is particularly interesting because it resolves the "Becker paradox." If minorities face discrimination, the paradox runs, they should have lower default rates than whites, since banks lend only to ultra-safe minority borrowers. This contradicts the evidence, which shows that minorities have higher default rates than do whites, even after controlling for all of the relevant economic variables. The empirical evidence, however, is consistent with Calomiris et al.'s rational discrimination model.

In a review of some recent evidence on discrimination in mortgage markets, the authors find that most of the formal and informal evidence is consistent with rational discrimination. Moreover, they argue that explanations such as cultural affinity or bigotry cannot explain the poor Community Reinvestment Act (CRA) ratings of minority-owned banks. However, to the extent that minorities face educational disadvantages and have lower average wealth than society in general, the poor CRA ratings of minority-owned firms are not inconsistent with rational discrimination.

If what we observe in housing finance markets is indeed rational discrimination, then what is the appropriate policy response by government? Calomiris et al. conclude that subsidized community development banks appear to be the most efficient solution.

Robert Van Order's critique of the Calomiris paper provides some useful insights on both the issue of discrimination and the analysis of discrimination. Van Order questions the usefulness of empirical studies of mortgage discrimination because of the omitted-variables problem. If explanatory variables omitted from the regression experiment are correlated with race, biases are introduced in the race coefficient of the logit

regression. Therefore, considerably more work is needed before strong conclusions can be drawn from these studies. Van Order also questions conclusions about the "Becker paradox" drawn from mortgage default rates. He points out that the proposition is based on the default rate of the marginal borrower, not the average one, and that the "econometric problems of isolating what is marginal are formidable."

Van Order suggests that the model of rational discrimination is consistent with commission-based compensation for lending officers. When commissions are based on the number of applications processed that meet underwriting standards, lending officers find efficient ways to allocate their time. These include stopping rules based on variables that have been found to be correlated with the creditworthiness of the borrower, including race. Van Order argues that rational discrimination can be dealt with through the same testing and enforcement mechanisms used to counteract bigotry.

He also makes a useful distinction between community lending issues and issues of discrimination. Distinguishing between these two is important because the solutions may be quite different. On one hand, community lending issues are about channeling funds into depressed areas. Van Order notes that the problem here is not necessarily one of race but rather of neighborhood externalities. On the other hand, he suggests that discrimination is most serious for middle-class blacks, most of whom do not live in depressed areas. In his view, one of the problems with CRA is that it does not distinguish between these two sometimes conflicting issues and therefore does a poor job of solving either one.

Thakor and Beltz

In "A 'Barter' Theory of Bank Regulation and Credit Allocation," Anjan Thakor and Jess Beltz advance the discussion of government involvement in credit markets beyond market failure and instead attempt to understand the self-interested motives behind these interventions. They posit that the existing complex web of regulatory subsidies and taxes is the outcome of what starts out as a mutually beneficial barter arrangement.

In their model, government subsidies benefit banks more than they cost taxpayers. In return, banks allow the government to dictate some aspects of their credit allocation. For example, they may accept the CRA in exchange

for deposit insurance and access to the discount window. This barter arrangement is a dynamic one, however, with the costs of government intervention increasing as new regulations are needed to counteract banks' circumvention of the original statutes.

Such a regulatory exchange can be mutually beneficial as long as the subsidy exceeds the cost of regulation. If a subset of banks finds this barter arrangement unprofitable, then a bad Nash equilibrium arises, which Thakor and Beltz call a regulatory trap. A bank opting out of the system would not attract any depositors, who prefer institutions covered by deposit insurance. This holds true as long as some banks find the regulatory barter process to be profitable. The banking system as a whole may prefer to give up deposit insurance when credit allocation regulations become too imposing, but a coordination problem prevents this.

Thakor and Beltz's basic model is a two-period one in which all agents are risk averse. There are three types of borrowers: the good (G), the bad (B), and the underprivileged (U). Only G borrowers have positive net-present-value projects in which to invest. The bank cannot distinguish between G and B borrowers, but it can readily identify the U's. Without government intervention, banks would never lend to U borrowers.

All projects last one period, and banks can infer the borrower type by the realized return on the first-period project. Only G borrowers with successful first-period projects obtain financing in the second period. Finally, successful G borrowers in the second period may have only a risky project to invest in or a choice between a risky project and a higher-valued safe one. However, only the lending bank in the first period can distinguish between borrowers with and without a choice of second-period projects. This is an important assumption because depositors will price their deposits according to the perceived attributes of the bank's portfolio. The inability of depositors to see the banker's private information and the incentives banks have to misrepresent their information on borrowers lead to a higher deposit rate than would be obtained in the full information case. Unfortunately, at this higher deposit rate, the banker cannot offer borrowers with a project choice a lending rate that will make the safe project profitable. Hence, the agency conflict both prevents the bank from exploiting its proprietary information and distorts investment.

In this model, government provision of a financial safety net through deposit insurance is Pareto improving. Deposit insurance solves the agency conflict between depositors and the banks arising from informational asymmetries by making deposits riskless. As a result, deposit insurance allows a bank to earn rents on its monopoly information in the second period by allowing it to price its loans according to borrower characteristics. Lower interest rates can be offered to borrowers along with an option to invest in the safe project, thus making the choice of the more highly valued safe project optimal. Consequently, deposit insurance removes the second-period investment distortion arising from informational asymmetries. Finally, if deposit insurance is underpriced, then banks and borrowers share in the surplus that results. However, given that the deposit insurance subsidy is available to banks that invest their deposits in marketable securities, a barter agreement between banks and the government becomes feasible.

The final element in Thakor and Beltz's analysis is the introduction of lending to U borrowers as a political good. Then, as a condition for insurance, the government mandates that banks lend a fixed portion of their deposits to U borrowers. Banks are willing to enter into this contract as long as the increase in profits from access to deposit insurance exceeds the cost of complying with the lending regulation.

Thakor and Beltz then show that if this barter arrangement becomes unprofitable over time for a subset of banks, these institutions will be trapped into maintaining the arrangement as long as other banks find it profitable to continue. This regulatory trap arises because the profits of the trapped banks are conditional on the actions of the nontrapped institutions. Unilaterally dropping deposit insurance would lower a bank's profits if other banks do not follow and instead retain their insurance. This occurs because the bank dropping its insurance will have higher funding costs than the insured institution and therefore will be unable to compete in the lending market. However, all banks could profit if the barter arrangement were dropped (that is, if they all canceled their insurance) and side payments were made from the trapped banks to the untrapped ones. This solution is precluded by coordination problems.

The essence of the Thakor/Beltz analysis is that social regulation and financial safety net subsidies go hand in hand. It is therefore unrealistic to argue for a reduction in the regulatory burden without a reduction in the subsidies. It is also impractical to think that one can extend social regulation to nonbank financial firms without also providing them access to deposit insurance and the discount window.

In her comments on Thakor and Beltz's paper, Deborah Lucas raises a number of valid concerns about the analysis. First, she correctly notes that the government could unilaterally impose regulations on the banking industry without offering special subsidies. Therefore, the authors need to explain why banks are different from industries such as automobile manufacturers, who face costly regulations (fuel efficiency standards, for instance) but do not appear to be compensated. In the absence of such an explanation, the barter theory seems less justified than a straight regulatory tax story.

Lucas also raises questions about the robustness of the Thakor/Beltz results to different modeling assumptions. As she notes, by introducing subsidized deposit insurance as a means of solving the monitoring problem, the authors produce an outcome in which deposit insurance lowers bank risk. This, of course, is at odds with the option-pricing approach to valuing deposit insurance and the attendant moral hazard problem, which leads to increased bank risk (Merton [1977]).

Finally, Lucas agrees with the authors' conclusions regarding the implications of the bad Nash equilibrium and that banks as a whole could benefit by opting out of the unprofitable barter arrangement. However, she points out that if the story is not one of barter but rather a simpler one of regulatory taxes, then the same policy conclusions may not apply.

III. Session 3: Pensions

The third and final session takes a more in-depth look at a particular area — pensions — where federal programs may have a huge impact on both individual fortunes and economywide variables. Indeed, the parallels between the Pension Benefit Guaranty Corporation (PBGC) and the Federal Savings and Loan Insurance Corporation (FSLIC) are at times uncanny. Like the savings and loan debacle, the large and growing contingent liabilities of the PBGC have the potential to strike a public nerve — as does the cost of any bailout. The two papers in this session offer a somewhat different perspective on an aspect of federal credit allocation: that of ascertaining the facts. The answers are not always easy to obtain, even to straightforward questions such as "How much do people contribute to their pensions?" or "How valuable is PBGC insurance?"

Gale

William Gale offers a new look at the determinants of pension contributions in "Public Policies and Private Pension Contributions," which provides the reader with a synthesis of the literature on pensions and pension contributions. Many aspects of the contribution decision — tax deductibility, benefit guarantees, and vesting rules — depend on government regulations, especially the Employee Retirement Income Security Act of 1974 (ERISA). Uncovering the smoking gun that links shifts in policy with shifts in contributions has been difficult, in part because the data on private pension contributions are sparse. Moreover, shifts in demographics during the 1970s and 1980s further cloud the issue.

Gale provides the reader with some insights into the issues surrounding pensions by examining changes in the legal and regulatory environment as well as trends in pension coverage, pension plan choice (defined benefit versus defined contribution), and funding status over time. He then sorts out what the literature has to say about these recent trends and the role that government intervention into the nation's pension markets has played. For example, ERISA was a major force in the shift by employers from defined benefit to defined contribution plans. However, the pension literature points to changes in industrial composition and employment as two other important factors explaining the shift in plan choice.

In seeking to understand the determinants of private pension contributions, Gale is faced with isolating the effects of government intervention into the nation's pension markets using data that are fragmented and inconsistent over time. To deal with this issue, he estimates his empirical model using two different sets of data. First, he uses standard data from the National Income and Product Accounts (NIPA). Unfortunately, the NIPA data are inadequate because they omit employee contributions, which are a growing and important share of total contributions. To control for this deficiency, Gale constructs a second measure of pension contributions by piecing together the standard NIPA figures both with IRS Form 5500 reports from private pension plans and with a new Brookings Institution series. With this improved, comprehensive series in hand, real contributions per worker are correlated with earnings, per capita asset holdings, the previous year's contributions, and dummy variables for various regulatory episodes.

Gale's empirical results highlight the inadequacy of standard data sets such as the NIPA. Time-series regressions using the dependent variable constructed from NIPA data reveal no evidence that ERISA or the Omnibus Budget Reconciliation Act of 1987 (OBRA87) had any effect on real pension contributions. Yet, when the dependent variable is taken from Gale's "constructed measure," both are shown to be significant factors affecting private pension contributions. The study finds that ERISA increased annual real contributions by an average of $213 per person and that OBRA87 reduced contributions by $154.

One caveat on the regressions using the constructed measure of real contributions per worker is that both ERISA and OBRA87 occur close to the sample dates at which data from different sources were spliced together. Therefore, the ERISA and OBRA87 dummy variables could be proxying for rules that Gale used to arrive at his constructed measure.

In his review of Gale's work, Joseph Ritter notes that private pensions are part of the compensation packages offered by some firms and as such may be an important part of the structure of incentives used to motivate workers. In other words, there may be much more to pensions than their impact on the structure of compensation and the composition of private savings. Consequently, government policies affecting private pensions may have important spillover effects on labor and capital markets.

Ritter ultimately finds the paper to be interesting, well motivated, and a useful survey of data sources, legal and regulatory changes, and empirical evidence. Moreover, he finds the empirical tests to be appropriate and well executed. He does question the robustness of the results, however, because the dependent variable (employee composition) is constructed four different ways across time. Unfortunately, the shifts in how employee composition is constructed tend to coincide with the events Gale is studying.

Pennacchi and Lewis

George Pennacchi and Christopher Lewis seek to determine "The Value of Pension Benefit Guaranty Corporation Insurance" by modeling PBCG guarantees as a put option with a stochastic exercise date. At first sight, this may seem like a lot of machinery for one number, but the number lies at the heart of the PBGC problem. Evaluating the PBGC's assets and liabilities lets us know if the insurance fund is healthy, tottering, or another FSLIC waiting to explode.

Merton (1977) shows that financial guarantees like PBGC insurance can be modeled as a put option (that is, the right but not the obligation to sell a stock at a predetermined price). To value a standard put option, however, one must know the exercise price and the exercise date, which for pension guarantees are unknown. Unfortunately, while reliable estimates of the exercise price can be obtained, the exercise date cannot be predicted accurately. To resolve this problem, Marcus (1987) values PBGC liabilities as a futures contract with a maturity equal to the time of the sponsoring firm's (pension plan's) bankruptcy. This futures contract model for valuing PBGC guarantees links the value of the guarantee to both the financial condition of the pension fund and the likelihood that the sponsoring firm will become bankrupt.

Pennacchi and Lewis take a different tack. Using a continuous-time options-valuation approach, they value PBGC guarantees as a put option with an uncertain exercise date. They thus extend Marcus' model in an important way. Futures contracts are different from options because they represent an *obligation* to buy or sell an underlying asset at a future date, as opposed to the right to carry out the transaction. Consequently, Marcus' formula for valuing pension guarantees implicitly assumes that the PBGC would experience a gain whenever a bankrupt firm's pension plan was overfunded. Pennacchi and Lewis' put-option formula explicitly recognizes that the PBGC's guarantee is contingent on both a firm's bankruptcy and its pension plan being underfunded, or insolvent.

Pennacchi and Lewis add another important wrinkle to valuing PBGC guarantees. They attempt to control for the firm's ability to increase its pension liabilities in the period just preceding bankruptcy.[3] To do this, they gross up the firm's pension liabilities by a factor λ.

The study shows that the value, at time zero, of the PBGC guarantee on one dollar of accrued pension liability is a positive function of λ and of the ratio of grossed-up pension liabilities to pension assets. The value of the PBGC guarantee is a negative function of the time remaining until the firm goes bankrupt.

■ **3** A firm's ability to adjust its balance sheet dynamically in response to external events is what Ritchken et al. (1993) call the *flexibility option.*

After solving for the value of a put option with a random exercise date contingent on firm bankruptcy, Pennacchi and Lewis use some representative parameter values to calculate the PBGC's liabilities and to conduct some comparative statics exercises. The exercises show that the put option model always yields higher pension costs than does Marcus' futures contract model.

For firms with low pension funding ratios and low net worth, Marcus' model appears to be a good approximation of the put. This is simply because a put option that is "deep in the money" (one that is almost certain to be exercised) is very similar to a futures contract. The bias in the Marcus model increases along with a firm's net worth. Interestingly, this is because a high level of net worth gives a firm with an overfunded pension plan time to underfund it. For similar reasons, the bias also rises with the pension-funding-to-liability ratio.

Andrew Chen's follow-up discussion points to the contingent put option model of PBGC insurance as an important contribution to the pension literature. He notes that the comparative statics performed by Pennacchi and Lewis provide useful insights into the properties of the PBGC and produce results consistent with economic intuition.

Overall, Chen finds the Pennacchi/Lewis paper to be an important contribution to the literature, but suggests that the analysis is incomplete. While Pennacchi and Lewis' model is a clear improvement of Marcus' futures contract model, Chen raises five questions about the model and its assumptions. His strongest criticisms are that Pennacchi and Lewis ignore taxes in their analysis and do not look at the volatility of pension assets. Chen argues that a complete analysis of PBGC guarantees must account for the tax factor, which is a major determinant of corporate pension asset and funding decisions. Furthermore, he suggests that the comparative statics for the volatility of pension assets must be explored.

Chen also offers some other less serious criticisms of the analysis. First, he finds the authors' use of market-value insolvency as a proxy for bankruptcy to be inconsistent with the legal definition of bankruptcy (a firm's inability to meet its contractual payments obligation). Second, he argues that the assumption underlying scaling up pension liabilities at termination by a factor λ is inconsistent with the empirical evidence found in Bodie et al. (1987), which suggests that eleventh-hour increases in pension liabilities are uncommon. Finally, he questions whether modeling PBGC guarantees as an infinite-maturity option is

superior to a one-period option with an uncertain exercise price.

IV. Conclusion

Each paper presented at the conference illuminates some important aspect of federal credit allocation. Taken together, they illustrate the range and significance of the government's intervention into the broad credit market. Some of the work has practical applications already, such as valuing and quantifying the effects of regulations. Collectively, the papers might best be thought of as a series of warnings: Some simple insights and obvious stories turn out to be untrue; some easy solutions don't work. Taken as a body, the papers also point to three unresolved issues that demand attention:

1) How important are racism, credit rationing, and other imperfections in the credit market?

2) Given credit market imperfections, what is the best means of resolving the problems — regulations, taxes and subsidies, or government organizations?

3) How will actual, as opposed to ideal, solutions work in the real-world political and economic environment?

The overriding normative question about the desired extent of government intervention remains open. We believe that the papers presented here provide both a direction and a springboard for needed future research.

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