Comment on Goodhart and Huang

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I congratulate Goodhart and Huang on their choice of topic. Financial crises have become more frequent than at any time in the postwar period, and the scale of losses has increased, beyond any previous experience we know, when measured relative to the total deposits or GDP. Since most countries have deposit insurance, either de facto or de jure, most of the losses are paid in one way or another by taxpayers. Further, the social loss is almost always greater than the losses on the failed projects that the system financed. Failures often occur around the time that economic policy changes from promoting excessive expansion to forcing contraction. Disruption of the payments system or banking relationships increase the underutilization of labor and capital resulting from policy change.

The authors state that a main purpose of their paper is to correct the classical liberal bias in favor of bank failures. Goodhart and Huang claim (p. 5) that when banks fail, panic sets in, and policy mistakes become more likely. Their model formalizes this result. Allowing bank failures to occur increases the economy's excess burden by generating avoidable uncertainty.

Before I read their paper, I believed that policymakers had an interventionist bias to save banks, even insolvent banks. This bias arises from "not on my watch", the very human tendency of policymakers to use society's resources to avoid a crisis even if the risk of a crisis has low probability of occurrence. Their paper has not changed my mind. I do not find the liberal bias in the banking literature. That literature has many articles on forbearance and moral hazard. The most common proposal for reform calls for the use of subordinated debentures to avoid
socializing private losses. This is not a classical liberal argument for failure or laissez faire. It is a recommendation for institutional reform to reduce moral hazard. The reformed system might still require a lender of last resort in a liquidity crisis.

The contribution of the paper lies elsewhere. There is very little research showing formally how lender of last resort activities affect financial performance. At different points in their paper, the authors use their model to analyze: (1) a "too big to fail" policy of saving large banks and letting small banks fail, (2) the risk of contagion resulting from failures, and (3) the risk of moral hazard, resulting from avoidance of failures.

**Bagehotian Principles**

To put the authors' contribution into context, I will briefly set out what I think is the received doctrine on the lender of last resort. The best parts of that doctrine go back to Walter Bagehot (1873) or Henry Thornton (1807). I summarize Bagehot in four propositions: Meltzer (1968)

1. The central bank is the only lender of last resort in a monetary system like our own.
2. To prevent solvent but illiquid banks from closing, the central bank should lend on any collateral that is marketable in the ordinary course of business when there is no panic. It should not restrict lending to paper eligible for discount at the central bank in normal periods. By requiring collateral, Bagehot separates illiquid from insolvent banks.
3. Central bank loans, or advances, should be made on demand at a rate of interest above the market rate. This discourages borrowing by those who can obtain accommodation in the market.
4. The above three principles of central bank behavior should be announced publicly, in advance of a crisis, and followed in a crisis.

Nowhere does Bagehot suggest that large banks should be treated differently. On the contrary, he describes in some detail the failures of some leading financial institutions of his day. He does not suggest that the failures should have been prevented. A main point of his book is to show that large failures do not have large systemic effects if pre-announced rules to prevent a market panic are followed. He cites several examples to show that when the Bank of England followed correct principles—i.e., Bagehot's rules—without precommitment, financial panics ended within a few days. He argued that precommitment to the lender of last resort rules would prevent the panic or reduce its length and severity.

For decades central banks have not allowed large banks to fail, so we have few observations on Bagehot's rule under present-day arrangements. The closest experiences are events like the failure of a large issuer of commercial paper, the Penn Central Railroad, or the failure of Drexel, Burnham, Lambert, a well-known investment bank. Following both events, the Federal Reserve offered to lend and made open market purchases. There was no panic, as in the cases Bagehot discusses. In other cases, such as Continental Illinois, First Pennsylvania, Franklin National, or Long-Term Capital Management, the Federal Reserve tried to save the problem bank. One does not find any principles, Bagehotian or other, at work.

**Differences Between Bagehot and Goodhart and Huang**

Goodhart and Huang use their static model to show the benefits of too big to fail. They assume (p. 12) that the loss to society increases as the square of the
bank run, while the cost of the bailout is proportional to its size (p. 15). Hence they are able to find that it is socially more beneficial to rescue large banks. They recognize that their argument about costs cannot be defended (p. 16). Since taxes must be levied (or expenditures reduced) to pay for the bailout, making costs increase directly with size implies that the marginal disutility per dollar of tax is constant.

Let me grant for the moment, what is really their assumption, that a run on a large bank is worse than a small run to ask four questions of the authors:

First, suppose as in the 1930s many small, agricultural banks fail under a common shock? Doesn't the social loss depend on the size of the run and not on the size of the bank?

Second, since the U.S. adopted deposit insurance, depositors either do not run, as in Ohio and Maryland in the 1980s, or they run from bank to bank and not from deposits to currency or gold. A run from bank to bank does not pose a threat to the payments system whether the failure is large or small. Doesn't the neglect of deposit insurance remove the practical content of their model for domestic bank failures and an internal drain?

Third, in many developing countries with de facto or de jure deposit insurance, the serious threat is an external drain. An external drain has, at times, forced devaluation in developed countries; France, Sweden, and Italy are examples in this decade. Most notably in Sweden, many banks failed. Is it desirable or feasible to shield the stockholders and management of large banks while allowing small banks to fail?

Fourth, if "too big to fail" is the rule or practice, why would small banks remain? Why not merge to reach protected status? With protested status, the banker earns higher returns but does not bear the relevant risk. Why would a bank owner choose to earn lower returns while bearing the full, higher risk? (p. 17)
Goodhart and Huang respond to the last question by introducing "constructive ambiguity". In contrast, Bagehot advocates a pre-announced rule, a position consistent with rational expectations. If the LOLR announces a rule and follows it, banks will conduct their affairs on the assumption that the LOLR will prevent panic but bankers must bear the risks they assume. At the level of the individual bank, the proper strategy is to hold sufficient collateral to minimize the risk of a costly failure. Owners of the bank's subordinated debentures or equity can be relied upon to enforce the rule. This pre-commitment and pre-announcement reduces private and societal risk and reduces moral hazard.

The authors claim that constructive ambiguity is a rational policy because uncertainty prevents banks from choosing their risk position as a function of size (p. 17). Banks know that the policy rule is "too big to fail." They are uncertain about when a bank is "too big", so they all adopt the same attitude toward risk (p. 17).

This last claim seems to me to apply only at the margin. The largest banks know that the policy applies to them; the smallest banks know that they can fail. All banks can observe who lives and who dies. Ambiguity will be limited to a size range that rational bankers would avoid.

"Too big to fail" is only one of several major differences between Goodhart and Huang and Bagehot. They differ also on the choice facing the LOLR. In their dynamic model, the LOLR must choose between contagion and moral hazard. If the LOLR fails to lend, the panic spreads; hence there is contagion. If he lends, banks take on additional risk, so there is moral hazard, a sustained difference between private and social costs of risk taking.

Goodhart and Huang make this a relevant choice in their model by assuming that the solvency of the failing banks is unknown until after the failure occurs. This assumption is commonly made when modern central bankers discuss failures.
They have not understood Bagehot. The very purpose of lending only against collateral and only at a penalty rate is to avoid both contagion and moral hazard. By lending only at a penalty rate, the LOLR assures that a rational banker will borrow from the LOLR only when the market will not lend at a lower rate; that is, at the market rate. (This makes the point that there really is no market rate in an illiquid market. The penalty rate must be set as a premium over the rate on the specific collateral before the panic began.)

By requiring collateral, Bagehot separates illiquid from insolvent banks. An insolvent bank does not hold marketable assets. It can not pay off its liabilities by selling its assets in the market. An illiquid bank has collateral to sell, but the market fails to function in a panic. It pays a penalty to borrow from the LOLR.

The Bagehotian LOLR avoids contagion by lending on demand to all eligible borrowers. By charging a penalty rate and requiring collateral, Bagehot's lender of last resort does not create moral hazard.

Goodhart and Huang recognize (p. 18) that if the LOLR lends to solvent banks, there is no reason for moral hazard. They dismiss this outcome, however, but they dismiss it because, they say, the public will not be able to observe whether the borrower is illiquid or insolvent. They can not get this result from their model, so they assume it is true. They also are unable to show whether the loss from contagion is larger or smaller than the loss from moral hazard (p. 18). They assume contagion causes the greater loss (actually increases the probability of loss by a larger amount).

The authors recognize that their problem requires a dynamic model in which the probability of loss from intervention and the bankers' risk position depend on the LOLR's past behavior. Unfortunately, the equations become uninformative. To draw implications, they must assume (p. 19); (1) that failure of the lender to
intervene increases both current and future failures, and (2) that this effect is stronger than the effect of intervention on moral hazard.

The assumption is problematic at best. Contagion should generate two effects: more banks fail currently, but surviving banks become more prudent about risk positions in the future. This seems to be true if we compare U.S. banks in the 1980s and 1990s. Judging from their small share of loans to Asia, they seem to have learned something about risk from the experience of the 1980s.

Admitting this type of learning is costly for the authors. They lose two main implications. They can not establish the social benefit of "too big to fail" and they can not justify intervention on grounds that losses from contagion exceed losses from moral hazard.

To sum up. The authors have chosen an interesting problem. Modeling the lender of last resort function shows the conditions under which Bagehot's rule, or another rule, works the way we expect. I commend them for beginning their work. I think they will agree that their model needs some improvement. I hope those improvements will include more attention to expectations and to Bagehot's rule.