

COMMENTARY

Skander Van den Heuvel’s paper is a very well-done and thought-provoking essay. The paper summarizes and discusses the policy implications of his research on what he calls the “bank capital channel” of monetary transmission, which he develops in detail in a separate paper (Van den Heuvel 2001).

The analysis in that paper is quite sophisticated in places, but the basic story is easy to summarize. The model begins with three key assumptions. First, banks face regulatory capital requirements. Second, they are unable to raise new capital in the outside equity market, presumably because of the adverse-selection problems that plague new equity issues. And third, banks’ balance sheets are mismatched in such a way that their profits are exposed to interest rate movements. In particular, banks have a tendency to lend long-term and borrow short-term, so that when interest rates go up, their profits fall.

Given these assumptions, the model then considers what happens when there is an exogenous increase in the short rate. Because of the balance sheets’ asset-liability mismatch, this rate increase has a negative impact on bank profits, and hence on banks’ retained earnings. The decline in the capital position of banks cannot be offset by an infusion of outside equity, so the regulatory capital requirement binds more tightly. The end result is a decline in lending. This is the bank capital channel in action.

Is this a plausible account of how monetary policy works? The answer probably depends on just how literally the model—which focuses *exclusively* on the link between interest rates and bank capital—is to be taken, that is, on how much one wants to suppress the role played by other unmodeled frictions. At one end of the spectrum, it seems hard to argue that bank capital does not have the potential to play an important supporting role in the monetary transmission process. But at the other end, it may be more of a stretch to claim that the specific interest-rates-to-bank-capital link outlined in Van den Heuvel’s paper is the single dominant piece of the transmission mechanism.

Suppose, for example, that one poses the question, can movements in bank capital have an important effect on loan supply? There is a large body of empirical work that, taken as a whole, offers a strongly convincing “yes” answer.¹ It then follows as a corollary to this line of work that movements in bank capital may serve to amplify other, more direct effects of monetary policy. Thus, if a contractionary monetary shock—working, say, through the traditional IS-LM channel—leads to a first-round decline in output, and hence to loan losses for banks, it seems eminently plausible that these loan losses and the accompanying hit to bank capital might in turn cause a significant second-round cutback in lending that exacerbates the initial downturn.

But note that the model developed by Van den Heuvel has a more front-and-center role for bank capital in the monetary

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transmission process. That is, bank capital does more than just amplify a disturbance that has its roots elsewhere. Rather, the link between interest rates and bank capital is what gets the monetary ball rolling in the first place. This is a more novel and as-yet untested theory. In what follows, I try to raise a few issues—both theoretical and empirical—that this more aggressive version of the theory will have to confront if it is to ultimately be convincing.

On the theoretical side, a first observation is that the model takes as exogenous the Fed's ability to move short-term interest rates. In other words, a more complete model would have to spell out another friction that enables the Fed to have this leverage over interest rates. Moreover, this friction is likely to be wholly distinct from anything having to do with bank capital, which underscores my question about whether bank capital can really be thought of as being at the center of whatever it is that gets the ball rolling in the first place. In contrast, most other models of the monetary transmission process are concerned with identifying the specific friction that enables the Fed to move interest rates. In the textbook IS-LM model, this friction is something like a cash-in-advance constraint, which prevents households from being indifferent between holding money and bonds. In my rendition of the bank lending channel (Stein 1998), this friction is banks' demand for monetary base in the form of non-interest-bearing reserves—a demand that is induced by the fact that reserves make it possible for banks to issue riskless insured deposits, and thereby get around problems of asymmetric information in the market for their liabilities.

A second theoretical issue is that the model rests entirely on the premise that banks' profits are exposed to interest-rate movements, but it does not explain why banks do not take any steps to hedge this exposure. One would not want to make the facile argument that all types of risks to bank profits can be easily hedged; for example, it seems likely that it would be difficult for a bank to measure, much less hedge, the effects of movements in real estate values on its loan losses. But estimating the pure interest-rate exposure of bank assets and liabilities is a relatively straightforward task, and plenty of derivative instruments exist that can be used to close such an interest-rate gap.

A more data-oriented way to bring out this point is to ask what fraction of the total variation in banks' post-hedged profits is explained by interest-rate movements. The model's implicit premise is that this fraction is large. But if in fact the fraction is only modest, then movements in aggregate bank capital over the business cycle may still have important economic effects, but these are likely to be second-round effects, with the direct first-round effect from interest rates to capital being a relatively minor part of the story.

On the empirical side, my sense is that it will be a challenging task—though a potentially very interesting and illuminating one—to design a set of tests that can sharply differentiate the bank capital story told in the paper from competing alternatives. As an example of the ambiguities that arise, consider the finding mentioned by Van den Heuvel, that output growth is more sensitive to monetary policy in states with low levels of bank capital. While this finding does fit nicely with the bank capital channel, it can also be seen as consistent with standard accounts of the lending channel, which focus on the frictions that banks face in the market for their uninsured debt. In particular, when a bank's capital is low, its debt is riskier and lower rated, and hence it will be less able to issue liabilities such as uninsured certificates of deposit to offset a Fed-induced contraction in its deposit base.

Instead of sorting on the basis of bank capital, an alternative approach—and one that may be more tightly connected to the logic of the model—would be to sort individual banks on the basis of their degree of interest-rate mismatch. (However, getting the appropriate data here may be difficult.) One could then ask whether, controlling for other factors, those banks with the greatest mismatches have lending behavior that is more responsive to changes in the stance of monetary policy.

In Kashyap and Stein (2000), we made a crude attempt to discriminate between the bank capital and lending channel hypotheses. To do so, we observed that, according to the bank capital story, a monetary shock works just like any other shock to bank profits—for example, just like a supply-side shock to output or a shock to real estate prices. Thus, a decline in GDP unrelated to monetary policy should produce effects very similar to a contraction in monetary policy. But in our analysis of bank-level data, we found the reverse: while a contraction in monetary policy leads to a tightening of banks' liquidity constraints, a reduction in GDP (controlling for monetary policy) has the opposite effect, leading to an *easing* of banks' liquidity constraints. Although this is just one small bit of indirect evidence, it reinforces the general observation that it may be tricky to make the more aggressive version of the empirical argument—that is, to demonstrate that the bank capital channel is the dominant piece of the transmission mechanism.

Nevertheless, even if one remains skeptical on this point, Van den Heuvel's research contains an important and highly policy-relevant message: the idea that bank capital requirements can play a significant role in amplifying the effects of monetary policy, or of business cycles more generally, deserves to be taken very seriously.

For a concrete illustration, consider the ongoing debate over how risk-based capital standards should be refined and updated. On the one hand, common sense suggests that it

would be a good idea for ex-ante capital requirements to depend on the riskiness of a bank's loan portfolio. For example, a bank that habitually lends to BB-rated borrowers should face a higher steady-state capital requirement than a bank that lends to AA-rated borrowers.

On the other hand, Van den Heuvel's research indicates that great care is required in the dynamic implementation of such risk-based standards, especially with regard to how rapidly one adjusts the capital requirement over the course of a business cycle. In particular, if a given bank's existing AA-rated loan customers are downgraded to BB as the economy enters a

recession, it may be a mistake to force the bank to conform immediately to a higher capital standard, as this will tend to amplify the capital-crunch effect and potentially deepen the recession.

The model developed by Van den Heuvel provides a natural framework for thinking more rigorously and quantitatively about these types of dynamic issues. To the extent that it does, the paper makes a very valuable contribution—one that transcends many of the specific questions and caveats raised here.

ENDNOTES

1. I will not attempt to cite the many excellent papers on this topic. However, Peek and Rosengren (1997) do an especially good job of solving the difficult identification problems that arise in this work, thereby isolating a causal role for bank capital.

REFERENCES

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