Discussion of “Capital Injection, Monetary Policy, and Financial Accelerators”

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1. Background

This paper is part of the large literature that takes as its starting point the recent financial crisis. A key aspect of the crisis was that it did not start in the non-financial corporate sector, and that instead banks’ liquidity and solvency appears to have been the core issue. For this reason, and similarly to many other papers written since the onset of the crisis, the authors have extended a model originally built for firms’ financial problems so as to also address the financing of banks. The model thereby gives a role to banks’ balance sheets and lets them affect the interest rate at which banks fund themselves. Contrary to most of the literature (e.g., Gertler and Karadi 2010), the present paper retains the firms’ financing frictions. The special aspect of the model is this twofold financial friction—for both firms and banks, linked together in a “credit chain” consisting of two principal-agent problems.[1]

An important lesson from the crisis is that business-cycle dynamics are characterized by more than one interest rate. This seemingly obvious fact had been abstracted from in most macro models in the past decade or two. The existence of multiple interest rates allows the computation of spreads, i.e., the difference between two given rates. An important contribution putting multiple interest rates, for a given maturity, into standard New Keynesian analysis is Cúrdia and Woodford (2009, 2010), who examine how monetary policy optimally should respond to one general stylized spread. The present

[1] In terms of using a double moral hazard framework, the paper is similar to Aikman and Paustian (2006) and Meh and Moran (2010), although those papers have a more elaborated link between the bank’s and the firm’s problem.

paper takes a step further by modeling multiple spreads, but limits the financial frictions to the financing of firms' capital stock. In principle, one can also allow for a separate spread for household borrowing. I think the authors are on the right track in this endeavor, and at the same time there are still good opportunities for both empirical and theoretical future research regarding implications for business cycles and monetary policy of spreads faced by households, firms, and banks. I will now briefly summarize the paper, imposing my own subjective emphasis, and then move on to my comments.

2. Summary of Paper

The main exercise in the paper is to compare macroeconomic dynamics and evaluate welfare implications from using (i) spread-adjusted Taylor rules and (ii) capital injection policies. This exercise, and the computation of the optimal size of the Taylor-rule spread coefficient/capital injection response, is performed separately for each type of shock, similarly to Cúrdia and Woodford (2010).

The richness of the model means that there are multiple spreads that the policy rate can respond to, and capital injections can also be made either to firms or banks. The authors make use of this richness and compare the welfare implications of letting the policy rate respond to three different spreads, one in each exercise: the bank borrowing spread, $(Z^{FI} - R)$; the bank lending spread, $(Z^e - Z^{FI})$; and the entrepreneurial spread, $(Z^e - R)$. Note that the two bank spreads add up to the entrepreneurial spread.

In terms of mechanics, the model is essentially a double version of Bernanke, Gertler, and Gilchrist (1999): both firms' and banks' profitability contains an idiosyncratic privately observed component which makes debt financing optimal and default costly. When profits are too low to repay the debt, default occurs and a fraction of the value of the firm's capital is lost. The same applies to banks. The costly default is the fundamental reason for credit spreads. Note that the linear solution method implies that risk is not priced. In other words, credit spreads are not affected by variations in stochastic discount factors.

To obtain this result, it has to be assumed that banks cannot completely diversify away the idiosyncratic risk they face. This is one of the debatable parts of the model.

One nice feature compared with the original model of Bernanke, Gertler, and Gilchrist (1999) is that in this paper banks actually bear aggregate risk, and their balance sheets are accordingly directly affected by aggregate shocks. This property is indeed required to make the model relevant for the questions at hand.

2.1 Main Results

In my reading there are five main results:

(i) It is generally welfare improving to let the policy rate offset, to varying degrees, the changes in some spread.

(ii) A striking result is that for several shocks, the optimal coefficient on the spread (whether the entrepreneurial spread or the bank lending spread) is reported to be around 2. This implies that the policy rate more than offsets the movement in the spread.

(iii) Shocks directly affecting banks, as opposed to those affecting firms, have the greatest impact. This result is somewhat mechanical, as it follows from the fact that banks have higher leverage than firms. For the same reason, capital injections should target banks (lenders), not firms (borrowers). This result provides a clear answer to the question asked by Governor Vergara during the conference regarding who should be supported during a crisis, although in this context it should be mentioned that the model abstracts from the moral hazard problem related to bank bailouts.

(iv) The spread-adjusted Taylor rule dominates capital injections from a welfare perspective. This is not very surprising, but it is nice to confirm standard practice: Use the policy interest rate as the stabilization instrument when possible, i.e., when monetary policy is not constrained by the zero lower bound.

(v) Finally, the optimal unconditional simple rule is computed. The optimal coefficients on the spreads confirm the shock-specific ones mentioned above: they take values between 2 and 3.

3. My Comments and Views

3.1 The Approach to Policy Analysis

The paper mainly evaluates simple ad hoc interest rate rules separately for each shock. This is an implicit way to allow policy to be conditional on the shock. It is fine for some exercises—e.g., it is reasonable to think of bank capital injections as a way to specifically counter the effects of a financial shock to bank balance sheets. Nevertheless, it is a valuable complement to this analysis that the authors also report the unconditional optimal coefficient on the spreads. One reason that this is useful is that it results in an explicit policy prescription. In future work it might also be valuable to consider fully optimal policy, i.e., Ramsey policy.

In my opinion, the main missing ingredient in the paper is a clear theoretical argument that sets up the stage. In particular, before the results for any quantitative policy exercise are described, the analysis would benefit greatly from first documenting which externalities are targeted by the policy intervention. Why and in what settings are fluctuations in spreads welfare reducing? What are the tradeoffs for the various instruments considered? These questions have already been discussed in the literature, but it would be useful to have the answers spelled out for this particular model in order to fully understand the exercises performed.

A final point regarding the policy analysis is that when adding a variable to a simple interest rate rule, we need to start from a benchmark where the coefficients on the pre-existing variables of the rule have been optimized (or, even better, optimize over all coefficients simultaneously). If we do not do it this way, we might have an unintended mechanism, similar to a "bias," affecting the coefficient on the added variable. For example, if the interest rate rule has a suboptimally low coefficient on the output gap, there will be a tendency for optimization to yield a positive coefficient on any variable that co-moves positively with output.[2] The authors do perform this type of optimization of the "traditional" Taylor-rule coefficients. Unfortunately, as in most of the literature on such coefficients (see, e.g.,

[2]This applies to many papers, e.g., Gelain, Lansing, and Mendicino (2013) in this conference volume.

Schmitt-Grohé and Uribe 2007), they find the optimal coefficient on inflation at the upper bound of the interval explored. This leaves a slight doubt regarding what drives the high optimal coefficients they generally obtain for the spreads. As mentioned previously, the reported optimal spread coefficients are around 2 for several of the shocks explored. At an intuitive level it might be surprising that the optimal coefficient on the spread would be greater than unity. Nevertheless, Cúrdia and Woodford (2010) report that also in their model, values above unity are optimal for some shocks, although in their case this only occurs for quite transitory shocks (they provide examples for when the autoregressive coefficient $\rho \leq 0.5$), while in the present paper all shocks are highly persistent with ρ around 0.9.

3.2 Empirical Validation

Since the onset of the financial crisis, a large amount of papers with various financial friction models have been written. Unfortunately, the fact that the empirical relevance of some type of financial friction became obvious seems to have led to the questionable assumption that there is no need to provide empirical validation for the particular model used.

A more specific problem for the empirical relevance of the present paper is that it does not imply positive co-movement between private consumption and business investment for any of the six shocks documented, with the exception of the neutral technology shock. Instead, a negative co-movement is very clear in the impulse responses for both net worth shocks, n^{FI} and n^{E}, in figure 1. These might be the true impulse responses for these shocks, but in that case they are not the main drivers of the recent crisis, nor important for business cycles in general, as we observe substantial positive co-movement between private consumption and business investment in the data. An indication that the empirically important financial shocks induce co-movement is the structural VAR results in Gilchrist and Zakrajšek (2012). In that paper a shock to corporate spreads—the "excess bond premium" shock—is shown to induce negative responses of both investment and consumption, as well as generate a substantial part—25 percent and 10 percent, respectively—of their business-cycle variation.

The reason for the lack of co-movement is the following: The main effect of a financial shock in the model is to yield a change in the spread and thereby affect the interest rate at which firms finance their investment. The increased spread causes a decrease in investment that is less than fully accounted for by a decrease in output, and consumption accordingly increases. Whether output falls as much as investment depends on details of the model, including the labor-supply elasticity, the degree of habit formation in consumption, and the degree of wage stickiness and price stickiness. Opposite results—implying co-movement between consumption and investment—are obtained, e.g., in Christiano, Trabandt, and Walentin (2011) in response to net worth shocks to firms.

One attractive and natural extension of the financial part of the model that would generate co-movement is to assume that banks lend to both households and firms. Anything that hurts banks' funding opportunities and thereby increases the interest rates at which they lend to both of these agents would then tend to decrease both consumption and investment. Surprisingly, business-cycle analysis of lending spreads faced specifically by households is highly unusual in the literature. Aoki, Proudman, and Vlieghe (2004) and Guerrieri and Lorenzoni (2011) are two very different theoretical modeling approaches to this issue, while Walentin (2012) uses a structural VAR to estimate the business-cycle effects of shocks to mortgage spreads.

3.3 Concluding Remarks

To conclude, the paper treats important issues that are very relevant for policymakers at central banks. Its contribution to the literature is to compute optimal simple rules for the policy rate response to different interest rate spreads, and compare welfare between responses to the different spreads. Most of the analysis is conducted separately for each shock in a model with financial constraints for both firms and banks in one credit chain. The main result is that it is welfare improving to let the policy rate offset, to varying degrees, the changes in one of the spreads, most often the total entrepreneurial spread.

I believe that in future work along the lines of the present paper, one might extract additional insights from the chained financial friction. In particular, it would be interesting to understand in more detail why the total entrepreneurial spread is not sufficient for fully characterizing the macroeconomic dynamics, as well as the optimal simple interest rate rule. Some insights might also be obtained from complementing the simple rule analysis by fully optimal policy.

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