Discussion of "Procyclicality of Capital Requirements in a General Equilibrium Model of Liquidity Dependence"

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1. Introduction

The paper that motivates this discussion belongs to a literature tradition (centered on capturing the effects of financial frictions in otherwise "standard" dynamic macroeconomic models) whose evolution and interest has been boosted by the current crisis. The crisis has made suddenly evident that conventional macroeconomic models offered an insufficient understanding, if at all, of banking, liquidity, and credit market phenomena whose development have been of great importance to the world economy during the last three years.

I will take the discussion of the paper as an excuse to give some general thoughts on the literature of reference. I will first frame the discussed paper in one particular branch of the tradition, then comment on alternative avenues and developments explored by the wider literature of reference, and finally come back to the discussed paper, with a brief summary of its modeling strategy and my critical reading of its main results. In that last part I will elaborate on why, in this type of model, the assessment of the likely effects of a significant rise in capital requirements (say, due to Basel III) may significantly differ from those obtained in models that give explicit consideration to the frictions that affect the dynamics of bank capital accumulation.

2. Placing the Current Paper in the Literature

The analysis provided in the article by Francisco Covas and Shigeru Fujita is based on a model in the tradition of Carlstrom and Fuerst

(1997). It explores the implications of incorporating ingredients taken from a specific well-known microeconomic model of financial imperfections into an otherwise standard real business cycle model. In Carlstrom and Fuerst (1997), the microeconomic setup was taken from Bernanke and Gertler (1989), while in the paper discussed here, the core microeconomic setup is taken from Holmstrom and Tirole (1998), henceforth HT. One might say that the authors of this paper fit the micro model of HT in a macro setup using essentially the same modeling "tricks" as Carlstrom and Fuerst (1997).

HT develops a three-date model that emphasizes the liquidity provision role of banks for financially constrained firms that, after obtaining some initial funding for an investment project, may suffer shocks that imply some additional liquidity need prior to the terminal maturity of the project. HT characterize the optimal contract that may govern the relationship between the firms and one intermediary that finances it. The contract can be naturally interpreted as a combination of an initial loan and a credit line that can be used or not along the remaining life of the lending relationship. HT is a good reference model if one wants to emphasize the liquidity provision role of credit lines—which the authors of the discussed paper do in the introduction and, paradoxically, not so much when discussing their results.

In fact, as recognized by Covas and Fujita, the idea of exploring the implications of the HT model into a real business cycle framework was already undertaken by Kato (2006), which is then the true closest reference for this paper. The distinctive and novel incremental contribution with respect to Kato (2006) yields at the explicit consideration of bank capital and its excess cost.

3. Incorporating Financial Frictions in Dynamic Macroeconomics

Many papers in the wider literature of reference can be described as attempts to explore dynamic macroeconomics mechanisms whose importance and microeconomic details had so far only been explored in rather static (and frequently also partial equilibrium) corporate finance and banking models.

Introducing the financial imperfections that affect firms’ or households’ investment decisions into the real business cycle or, more recently, into dynamic stochastic general equilibrium (DSGE) models is not immediate. It requires adopting what I have provocatively denoted as “tricks”: whatever more or less convincing assumptions that one adopts with the main purpose of embedding the microfinance model into the macroeconomic model without altering “too much” the structure of any of them.

One first set of tricks is required in order to insert the typical risk-neutral, static (or at most two- or three-period) optimizing agents of the micro models into the dynamic structure of the macro models, where typically there is a representative household that is risk averse and optimizes over an infinite horizon.

The big alternative to this approach would be to try to derive the details of the micro part in the macroeconomic structure directly and in a fully consistent way. So far this avenue is not the most prevalent, especially among central bank researchers, but it has been explored already. Albuquerque and Hopenhayn (2004) and Clementi and Hopenhayn (2006), for instance, study infinite-horizon optimal financial contracting into particular general equilibrium setups with fully optimizing agents using a reasonably tractable “recursive utility” formulation. The problem with this approach (more satisfactory in terms of microfoundations and overall consistency) is that the techniques required to determine the equilibrium dynamic contracts are at odds with the current standards of application of DSGE models, especially at central banks.

Specifically, the models with complex dynamic financing problems typically do not yield closed-form solutions for the equilibrium contracts and tend to require considering additional state variables for the recursive representation of equilibrium. For example, decision rules may be the outcomes of Bellman equations whose solution one must determine, with numerical methods, at the same time as the whole dynamic general equilibrium is solved. Additionally, these models easily produce heterogeneity among the agents subject to financial constraints and to possibly idiosyncratic non-perfectly diversifiable or non-insurable shocks. Frequently, the full distribution of wealth among the wealth-constrained agents will have to be treated as an additional state variable.

Hence the models with fully fledged dynamic contracting problems stemming from financial frictions force the analyst to move away from solution techniques based on log-linearizing a set of manageable equilibrium conditions around some non-stochastic steady state. And this poses a problem from the perspective of the current standards of application of DSGE models.

Not surprisingly, most of the tricks found in the papers developed by central bank researchers and their academic periphery adopt the strategy of just "fitting" some essentially static problem with financial constraints into the dynamic macro model. And there are various available sub-strategies for doing this. One possibility is to simply add successive generations of short-lived agents that are around for at least (but not much more than) two dates, i.e., that operate for one period and then die or exit the economy. For example, these agents may be entrepreneurs who are born penniless or with limited wealth, get funding from the representative household (or from a bank that collects savings from such household), develop some specific one-period investment project, and die after its completion. At that point they are replaced by a new generation of entrepreneurs who enter the economy.

Many papers in this literature (starting with the seminal contribution of Bernanke and Gertler 1989) rely on simply incorporating some entrepreneurial sector whose essentially static funding is subject to imperfections described by a well-known micro model (costly state verification, moral hazard, adverse selection, etc.). Such entrepreneurial sector is typically assumed to be funded by a competitive financial market or banking sector that "lends" to the entrepreneurs the funds saved by more conventional infinitely lived households that truly optimize over time.

The models resulting from adopting this very trick are a good start but produce too little history dependence. In particular, they do not capture the dynamics of accumulation of wealth or net worth by the financially constrained entrepreneurial sector. To get more history dependence at a small modeling cost, one possibility is to introduce warm-glow bequests so as to make some entrepreneurial wealth pass from each generation of entrepreneurs to the next (see, for instance, Aghion and Bolton 1997). The trick here is to allow for intergenerational wealth transmission without making the entrepreneurs of each generation fully internalize the utility of the next generation. If they internalize this, say under standard altruistic preferences, then they will have to solve a fuller dynamic optimization problem, and the resulting model will have the same tractability problems as the models associated with the full dynamic-contracting approach.

Another possible approach is to think of infinitely lived risk-neutral entrepreneurs that somehow along the equilibrium path (and insofar as they remain active entrepreneurs) are always financially constrained and, consequently, are always saving as much as possible (an example along these lines is Kiyotaki and Moore 1997). In such an environment, entrepreneurs will tend to find it optimal to keep accumulating any earnings resulting from prior investments as net worth used for the self-financing (or as collateral in the external financing) of their subsequent investments. In this approach, entrepreneurs are (at least implicitly) assumed to solve a dynamic problem but one whose solution is (or is assumed to be) trivial in the part that refers to their wealth accumulation decisions.

In fact, in these models one typically needs to adopt some additional trick to prevent the internally accumulated net worth of the entrepreneurial sector from growing unboundedly. The typical trick here is to add some assumption (perhaps an exogenous shock) whose effect is to force (some) entrepreneurs to decumulate wealth. For instance, one can assume that some shocks arrive (say, following a Poisson process) that make entrepreneurs die or retire (or lose access to their investment projects), in which case they lose or decide to consume their accumulated wealth. One can alternatively assume that, for some reason (perhaps obtaining output from their projects which cannot be sold in the market), entrepreneurs are forced into something equivalent to paying out (or consuming) a fraction of their earnings.

In some other models, the process of net worth accumulation by entrepreneurs gets endogenously bounded by assuming that entrepreneurs are more impatient than the typical saving household (perhaps, but not necessarily, in a setup where households are assumed to differ in their discount factors so that, in equilibrium, some of them act as lenders while others act as borrowers).

A more sophisticated construction along these lines is the one developed in Gertler and Karadi (2009) and Gertler and Kiyotaki (2010), where the representative household has the special class of financially constrained entrepreneurs (and also some financially constrained "bankers") as some of its members. The representative household is assumed to provide consumption insurance to these agents which, effectively, then behave as risk neutral with respect to the supposedly diversifiable idiosyncratic risk of their investment activities. In this setup, agents are somewhat "schizophrenic" since for the purposes of consumption smoothing they are part of the representative household, while as entrepreneurs, they obtain some wealth from the representative household when starting up and, then, keep managing that wealth separately, on their own, until they fail or exit (in which case any residual wealth reverts back to the representative household). So the household insures entrepreneurs' consumption needs but does not insure or back their businesses (i.e., the financial needs related to their investment projects). However, if entrepreneurs have too little (or run out of) wealth in their business activity, the household to which they belong does not "recapitalize" them. Hence, this sophisticated structure is still one full of tricks.

Papers in this literature need further tricks in connection with the final goal of exploring the quantitative implications of the resulting models in a way that facilitates comparison with mainstream quantitative macro models. This may require describing the production process of, say, the final consumption good using a Cobb-Douglas production function, so that labor shares and capital-to-output ratios can be matched to their empirical counterparts according to standard practice.

To make such a feature compatible with the producing role assigned to entrepreneurs, many papers introduce (following Bernanke and Gertler 1989) an explicit capital-producing sector in which the entrepreneurs act as the producers. Hence, instead of assuming, like in the canonical neoclassical growth model, that the consumption good can be transformed into the capital good using a frictionless, linear, reversible technology, models with financial frictions typically assign the role of transforming the consumption good into the capital good to the wealth-constrained entrepreneurs. The entrepreneurs produce capital out of projects subject to moral hazard problems or some other type of agency, contract, or informational imperfection that justifies the frictions that affect their financing.

4. Banks, Bank Capital, and the Discussed Paper

Prior to the current crisis, the macro literature had formally accepted these and similar tricks as part of their stock of knowledge, but I think that many macroeconomists felt uncomfortable about the practice of continually adding tricks to the basic frameworks. Perhaps this explains the little progress made after the synthesis provided by Bernanke, Gertler, and Gilchrist (1999), henceforth BGG, which incorporated a canonical financially constrained entrepreneurial sector model (based on costly state verification frictions) into a New Keynesian model with nominal frictions.

The arrival of the financial crisis led to the sudden discovery that the mainstream dynamic model and even the BGG model did not have banks. The latter, in particular, had financially constrained entrepreneurs and nice credit spreads that could move with the business cycle but no specific bank or banks, and hence no specific role for bank capital.

Recent research efforts have led to the emergence of a new generation of models that attempt to incorporate banks in the analysis. These new models include Meh and Moran (2010) and the previously mentioned Gertler and Karadi (2009) and Gertler and Kiyotaki (2010). Their strategy can be summarized as consistent on adding a second layer of financially constrained agents which are the banks or their owner-managers (the "bankers").

Among the references in the microeconomic literature relevant for this task, Holmstrom and Tirole (1997) occupies a prominent position. In that paper, moral hazard problems affect the incentives of both the entrepreneurs and the banks that monitor some of them. Specifically, the banks can ameliorate the incentive problems of the entrepreneurs in some intermediate net worth range. However, in order to undertake their own costly monitoring activity, bankers need to have the right incentives, and this essentially requires them to own a stake in the success returns of the funded projects. Such a stake justifies the incentive role of bank capital: bankers contribute their own wealth to the funding of the projects in exchange for a share in the success returns. So in this model bank capital plays for banks' funding essentially the same role as entrepreneurial net worth for entrepreneurial funding: it reduces the deadweight costs of external financing. This model is also nice in that it provides an intuitive rationale for (market-based) bank capital requirements.

Meh and Moran (2010) use Holmstrom and Tirole (1997) as the microeconomic model of reference and formalize the dynamics of bank capital in essentially the same way that the dynamics of entrepreneurial net worth had been modeled by the inherited literature tradition. In Gertler and Karadi (2009) and Gertler and Kiyotaki (2010), the rationale for bank capital comes from the existence of an enforceability problem (bankers might run away with a fraction of the resources under their management) that can be ameliorated by making bankers contribute their wealth to the funding of the bank. Bank capital also accumulates as the result of earnings retention by bankers.

The approach taken by Covas and Fujita in the discussed paper departs from this line of research in that it does not incorporate bankers who accumulate wealth so as to contribute it as equity. In fact, in the model, banks operate repeatedly for just a period and there are no meaningful bank capital dynamics. Regulation imposes that banks must finance a fraction θ of their lending with equity capital. Equity capital is provided to the bank by the representative household out of its savings essentially in the same way as it also provides deposit funding. However, equity funding is assumed to involve some "issuing cost": a resource cost equal to a proportion γ of the equity used in each period. Hence, the regulatory capital requirement is to all effects equivalent to a proportional tax on bank lending with a tax rate equal to $\gamma\theta$.

The paper explores the steady-state and cyclical implications of Basel I and Basel II capital requirements, taking into account the possibility that the excess cost of equity γ is sensitive to the cyclical position of the economy. Under Basel I, θ is assumed to be constant while under Basel II θ is described as a smooth increasing function of a total factor productivity parameter A, whose assumed random evolution is the final source of (cyclical) fluctuations in the model economy. The cyclical variability of γ is also captured in reduced form by making γ a smooth function of A.

The model is then carefully calibrated and its steady state and cyclical properties are analyzed with state-of-the-art techniques. A few tables and graphs containing numerous impulse response functions summarize the main results. One section is devoted to analyzing the effects of permanent and transitory increases in a (Basel I type) cyclically invariant capital requirement. Another section examines the cyclicality added by a (Basel II type) cyclically variant requirement.

The results are nicely explained in the paper and yield a picture of qualitative effects that go well in line with intuition. Quantitatively, however, the effects of both increasing capital requirements and making them more cyclical seem to me quite tiny. For instance, in the baseline parameterization, a permanent unanticipated increase in capital requirements from 8 percent to 12 percent produces a deviation in total bank lending and investment of less than -0.7 percent in the short run and about -0.3 percent in the long run. The deviation in total output is of less than -0.1 percent.

One possible reading of the results is that capital requirements do not matter much after all. However, I think a fairer assessment of the results is that capital requirements cannot have important effects in this very type of model unless its calibration were stretched too much. Specifically, it turns out that if the excess cost of equity funding is calibrated to have a mean value of 5 percent (like in table 2 in the paper), moving the capital requirement from 8 percent to 12 percent essentially moves the implicit "tax" on bank lending from 0.4 percent to 0.6 percent and, eventually, it is the incidence of this "tax" the force that moves everything else in the exercise. Are these numbers reasonable?

On the one hand, an excess cost of equity funding of 5 percent is really a big number, since the model is calibrated on a quarterly basis and the model implies that the bank reissues its entire equity capital base at the beginning of every period, which in practice means that one unit of equity funding has an attributed yearly excess cost of 20 percent! On the other hand, when this number is combined with capital requirements of 8 percent and 12 percent, it implies a yearly tax on bank lending of 1.6 percent and 2.4 percent, respectively. And these numbers are important but arguably not very large.

In fact, I think that the numbers are possibly too large as an estimate of the long-run implications of having capital requirements in place, since a typical industry estimate of the required rate of return on bank equity is 10 percent per year (not 20 percent!). However, I also think that the results in the paper are likely to underestimate the (shadow) cost of equity capital and its contractive impact on bank lending during the transition from the steady state with $\theta = 8\%$ to the steady state with $\theta = 12\%$. Let me explain why.

Accommodating such a change in capital requirements with just a tiny decline in bank lending (as in the results presented in the paper) means that banks should be able to increase their equity funding by about 50 percent in a single quarter. Fifty percent is a huge increase by historical standards and one that, possibly, has never before been accommodated, at an industry-wide level, by issuing all the required extra equity in the market. I suspect that raising all that capital in a single quarter might not be feasible or only at a very high, perhaps prohibitive cost.

I would expect that, in practice, banks in the real world would start accumulating earnings at perhaps a higher speed than normal (say, by sacrificing payouts to their shareholders) so as to make their equity funding gradually converge to some new desired long-term level of capitalization. So in the transition, the internally accumulated equity funding of the banks (the wealth of the bankers in some of the models commented above) would be scarcer than usual and its shadow value would then be higher than usual. This will have two main effects: (i) it will make bank lending more expensive and smaller in quantity than in the new steady state, and (ii) it will make bank earnings per unit of equity capital temporarily higher than in the new steady state. The first effect points to a temporary credit crunch effect that the current model cannot capture (see Repullo and Suarez 2009 for a model with its own tricks that captures temporary credit crunches and in which banks hold buffers of excess capital to partially prevent the crunches from happening). The second points to a force that will tend to endogenously accelerate the process of convergence to the new steady state (since it should allow speeding up the process of internal accumulation of equity funding) and is also missed in the current analysis.

So the main deficit of the model proposed by Covas and Fujita is the explicit treatment of bank equity capital as a stock variable that can be increased via earnings retention and, perhaps, also by issuing equity but definitely not at the same cost. The perfect model in this field is still to be produced. Ideally, we would like to have endogenously determined earnings retention, equity issuance, and payout policies. This is a challenge not only because of the difficulties involved in finding "tricks" with which to add them to a macro model but also in terms of the available microfoundations. Indeed the understanding of the distinction between inside and outside equity funding in corporate finance and banking literatures is still far from perfect, but that would be a story for another discussion.

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