

The "credit-cost channel" of monetary policy. A theoretical assessment"

Reply to Referees #1 (November 18), #2 (December 22), #3 (December 23).

All comments are useful and constructive, and I wish to thank the referees for their contributions. Several suggestions can be used and integrated into a new version of the paper. The three reports share some remarks, and in the following I will reply on these.

1) All reports are concerned with the reference literature of the paper not being updated to the most recent developments in the field. It is, correctly, suggested that the benchmark work of the paper (Christiano, Eichenbaum, Evans (CEE, 1997)) is no longer fully representative of the state of the art. It is stressed that since then a number of DSGE models have been developed which include financial market frictions.

I cannot but agree with the remark that the paper should make reference to these developments, but with some qualifications that may clarify my point of view. The motivation of the paper is to contribute to knowledge about the transmission channels linking monetary policy with macroeconomic variables, in particular with reference to the well-known stylized facts considered in the paper. When I started working on this, more than one year ago, the "current state of knowledge" was largely dominated by the standard New Keynesian model, with no banking sector, no capital market imperfections, no bankruptcy risk, neither outside nor inside money. As is well known, the whole positive and normative theory of monetary policy in that model hinges on sticky goods prices *à la* Calvo. Furthermore, a vast majority of the profession thought that the model was substantially sound, and that it would not need extensions towards capital markets.

It is true that *at the origins* of the New Keynesian school there was the idea that capital market imperfections are a key issue in business cycle theory (e.g. Mankiw and Romer (eds., 1991, vol. 2), Stiglitz (1992), Greenwald and Stiglitz (1993a)), and this idea gave rise to a prominent research strand culminated in the celebrated works by Bernanke and co-authors on "financial fragility" and the "financial accelerator". But it is a matter of fact that what is now universally known as "the" New Keynesian model dispenses with capital market failures altogether (of the two New Keynesian sub-schools gathered in the Mankiw-Romer volumes, only the one in the first volume on "Imperfect competition and sticky prices" has seized the name's copyright). It is also a fact that the works by Bernanke and co-authors have had little impact on the New Keynesian framework of monetary policy (there is no trace of them in the most authoritative book in the field, Woodford (2003), and even two of the authors themselves ignored their previous findings in Bernanke and Gertler (2001)).

There has certainly been a sudden revival of this other New Keynesian literature in the last couple of years due to the "theoretical shock" created by the eruption of the financial crisis. Complaints about the limitations of the standard New Keynesian model, that I mention in the paper, have gained momentum. One reason is that many observers have attributed distinct responsibilities for the crisis to monetary policy (especially in the United States), not so much as the result of misbehaviour by monetary authorities as of unforeseen consequences of the prescriptions distilled from the dominant model. The search for where the fault lies and how to fix it is now afoot. It is telling that almost all papers that the referees indicate to me, and those of which I have knowledge, are "mimeos" dating back no earlier than 2006. They certainly deserve more consideration in the paper. Yet, on balance, I do not think that this still largely "grey material" can be regarded as a newly established view that may displace the motivation and focus of my paper on some deficiencies of the *standard* New Keynesian model. On the contrary, these materials testify that much work is still to be done to (re)establish the view that capital market imperfections are a key feature for the theory and practice of monetary policy (for instance, Woodford (2008), and Canzoneri, Cumby, Diba, Lopez-Salido (2008) have expressed contrary opinions)

2) As to the relationship between my CCC proposed model and the current revival of models with capital market imperfections, a further qualification concerns the aim of analysis. Generally, these models are mostly concerned with the design of optimal monetary policy, namely whether inclusion of some "financial frictions", and possibly of the banking sector, calls for substantial change in the conventional Taylor rule. Yet this is not the aim of my paper, which is more preliminary, namely how to obtain a monetary transmission mechanism that may account for the whole set of facts of interest.

In this perspective, I see common grounds with Christiano, Motto and Rostagno (2007a, b). These works can indeed be viewed as developments of CEE (1997), and they should certainly be included in the CCC reference literature. However, let me point out at once, that the modeling strategy of CMR differs from CEE (and myself) in one crucial point: they do not introduce real "financial frictions" as an *alternative* to nominal rigidities. They simply add up them all, with a key role assigned to *sticky nominal wages* and *non-indexed nominal financial contracts*. As a result, they maximize their model's fit to the data, but they lose theoretical comparative power with respect to CEE. The world is full of imperfections, and fitting the real world data is an important task for applied and policy purposes. Yet this work of mine is in the ordinary New Keynesian line of research that sought to investigate the explanatory power of capital market imperfections alone, in a system of competitive, flex-price, labour and goods markets.

This is of course primarily a (long-standing) theoretical problem, not one of (dis)preference ordering on "frictions". As regards research strategy, I do believe that a model suited for dissecting theoretical issues may not be *ipso facto* suited for empirical analysis. Hence, I actually agree with Referee #1 that "one drawback of stylized models is that it is not easy to take them to the data". That is why I have chosen the kind of quantitative assessment of the model exemplified by CEE rather than taking it to the real world data abruptly. Since I think the model has passed this pre-empirical assessment, next step, on which I am working, is econometric analysis with all the empirical qualifications that are necessary, but without losing the focus on the qualifying theoretical hallmark of the model (more on this methodology in e.g. Hoover, Johansen and Juselius (2008)).

Having mentioned nominal rigidities, I wish to reply specifically to the observation of Referee #1 (p. 1) who finds that my claim that sticky prices are a *sine qua non* condition to obtain real effects of monetary policy in "these models" is imprecise. First, "these models" in my paper is the standard New Keynesian model, for which there is no doubt that my claim applies. In the Referee's mind "these models" are those of CMR, which can indeed dispense with sticky prices only because they introduce sticky wages and non-indexed financial contracts. Hence I think we can agree on the more general statement that in these models (the standard New Keynesian model and the CMR type) *nominal rigidities* are the *sine qua non* condition to have real effects of monetary policy.

3) The referee reports also contain remarks and questions about specific points of the model.

A key feature of the CCC model is firms' price uncertainty. This point of the model is borrowed from Greenwald and Stiglitz (1993b) assuming that each firm's *unit revenue* (my paper, p. 8), P_{jt+1} , is a random variable of which the firm at time t knows the correct probability distribution, with expected value P_{t+1} . This is plain rational expectation hypothesis. The difference $P_{jt+1} < P_{t+1}$ is the basis for losses that give rise to bankruptcy. What may appear peculiar, in relation with the perfect competition hypothesis, is that all firms face the same expected price, P_{t+1} , but may obtain a different unit revenue. However, this does not necessarily imply imperfect arbitrage by consumers, though I agree that this passage may need better clarification. "Unit revenue" may be interpreted not as the take-home price of consumers, who may well pay a single price P_{t+1} all over the market, but as the take-home revenue of the firm. The latter may differ from P_{t+1} owing to a variety of reasons embedded in the internal organization of the firm, such as unexpected events in the retail branches, etc.

Another point concerns the assumption that households are not allowed to borrow against future income and hence face a cash(deposit)-in-advance constraint on current consumption. To Referee #1 this appears quite a "dramatic friction", which may impair my claim that the CCC model makes no recourse to non-competitive hypotheses and frictions. My claim only refers to the labour and goods market. Then, how "dramatic" the situation is in the capital market is a matter of modeling choices and aims. Overall, I believe that my CCC model may be appreciated on the grounds of parsimony (especially with respect to the recent works mentioned above). I have sought for a simple, classical, general-equilibrium setup, with one single "friction" (costly state verification of firms under uncertainty and asymmetric information) to which all other features are related.

a) The CIA constraint is a standard tool to deal with money (questionable as it is, I personally join the party of those who prefer this tool than money in the utility function). Here, however, it is defined on *bank credits and deposits* (not cash), and *it operates symmetrically* on firms (credit in advance in order to pay for working capital) and households (deposit in advance in order to pay for goods). The credit-in-advance constraint on firms is typical of the cost-channel models, yet the fact that it exists, and that it is fulfilled by banks, requires an explanation, that is provided by the originary "friction". Then, introducing symmetry of households and firms is not only a matter of consistency (why should the two be treated differently?) It also performs the task to close the accounting circuit among firms, households and banks without leakages (e.g. cash or firm-household direct arrangements). These would unduly complicate analysis. In fact, cash is traditionally unimportant in the New-Keynesian, credit-channel world. No non-bank connections between firms and households fit the assumption that the two have no direct financial relationships. Note, also, that households' deposit-in-advance constraint is less dramatic than it seems the case with pure cash. Deposits (not cash) are generally the means to access payment facilities granted by banks. If, say, also households are allowed to borrow from banks, it is typical that the latter wish the borrower to keep a deposit.

b) The deposit constraint, in conjunction with the assumption that households cannot borrow, specializes them as *the* lenders in the economy *vis-à-vis* firms that are *the* borrowers. It is obvious that in reality anyone can be a lender or a borrower in different times and circumstances. However this clear-cut specialization, in connection with informational problems, is typical of New Keynesian models from the origins (e.g. Kiyotaki and Moore (1997)), as it offers an obvious advantage when it comes to trace out the spillover effects of changes in borrowing and lending. Let me also add that households are net lenders and firms are net borrowers according to the national financial accounts all over the world (except, recently, in the United States).

c) What is the importance of the households' no-borrowing constraint? This question can be indirectly addressed in the quantitative assessment part of the paper. The only difference that households' borrowing would

make in the model is that they might anticipate consumption beyond available deposits. This fact would affect one of the parameters of the model, namely the marginal propensity to consume out of real deposits, C_D . The more consumption is independent of current deposits, the smaller this parameter should be. Small values of C_D improve the model's assessment (see p. 16).

As to banks, I wash out recovery of bankrupt firms' outstanding revenues with monitoring costs. Hence, the bank's net expected return in case of bankruptcy is zero. If this value were positive, the credit risk premium would be lower, as suggested by Referee #1. This, however, has no significant impact on the core results (qualitative and quantitative) of the model. The reason is that the bank lending rate would remain approximated by a log-linear relationship with the central bank rate and the risk premium (see eqs. 10 and 11). Of course, the spread would be smaller, and changes in credit risk would have smaller effect, but nothing would change as far as shifts in the central bank rate are concerned.

Finally, it is true, as Referee #2 notes, that I include profits in the set of variables to explain, but I do not trace them out in the model. The reason is that profits' behaviour depends on technology. If it is assumed a Cobb-Douglas technology as in the quantitative assessment of the model, it is easy to see that total profits (on average) are a constant share of output, and hence fall with output and total wages after a monetary contraction.

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