

Economics eJournal / Referee Report on

The “Credit-Cost Channel” of Monetary Policy. A Theoretical Assessment by Roberto Tamborini

Following the guidelines for the review process, referees should set the focus on the following two questions:

- (i) Is the contribution of the paper potentially significant?
- (ii) Is the analysis correct?

Regarding this paper, my answers to both questions are firmly positive, with only minor quarrels regarding the exposition of the core arguments.

The contribution of the paper is significant, because the author manages to combine the credit and cost channels of monetary policy transmission in a simple stylized model with asymmetric information in the capital market. The combination of the two transmission channels helps to show that, with minimal deviations from the standard perfect competition model and under plausible parameter constellations, monetary policy will not only affect aggregate demand, but also aggregate supply. At first sight, one might think that combining the credit and cost channels amounts to deviating from the standard competitive model by more than one friction. This may nowadays be a conventional procedure (see, e.g., Mankiw and Reis on “Pervasive stickiness”, 2006), but – plausible, as it may seem – it invites the usual charges of ad-hocery. In the model of this paper, however, the combination of the credit and the cost channel is based on the one and only friction of an *ex post*-verification problem of lenders about the true state of firms. Delegating lending and monitoring to specialized intermediaries, such as banks, may be efficient, but introduces a credit risk premium as a cost factor that makes aggregate supply sensitive to the interest rates set by the central bank. The model employed in the paper shows this conclusively for parameter constellations that find general support from empirical studies.

Moreover, apart from drawing attention to these supply-side effects of monetary policy, the analytical framework of the paper appears to be able to explain the empirical regularities of negative correlations between short-term interest rates and inflation, output and real wages, as well as the stylized fact that output reacts to interest-rate shifts prior to inflation. The paper may thus be seen as an interesting challenge to the standard DSGE literature that, in various alterations of New Keynesian or other models, resorts to goods and labour market imperfections (mostly nominal rigidities), but is not able to explain (all of) these phenomena.

My remaining quarrels with the paper are the following:

- (1) The paper uses the Christiano, Eichenbaum & Evans (CEE 1997) comparison of sticky price and limited participation approaches as benchmark. However, Christiano (with co-authors Motto, Rostagno, Trabandt and Walentin) and

Eichenbaum (with co-authors Altig, Linde et al.) have written a host of papers in recent years, in which they have varied their frameworks considerably (including various credit frictions) and in which they attack the mainstream reliance on nominal rigidities. One would like to see a more systematic discussion of what sets the paper under review apart from the contributions of Christiano and Eichenbaum, in particular. The CEE 1997 paper may still be the most suitable benchmark, but one would like to have this stated more clearly.

- (2) The paper treats the cash-in-advance constraint of the households rather casually as “non-essential” (p. 7, fn. 8). However, this requires further explanation, since the crucial sequence of finance and payments in the model (as presented in figure 1) might break down, if households were allowed to borrow against future income. A simple argument might run as follows: Once loans to firms present a credit default risk and household (i.e., worker) incomes depend on the loans extended to firms, the cash-in-advance constraint of households is just derived from the credit constraint of the firms. Even so, however, there is some extra default risk of households left to be explained? (By the way, footnote 8 is misplaced; it should be connected with the first sentence of the paragraph)
- (3) The discussion of the combination of parameter values in section 3 is essential for the model’s capacity to provide theoretical support for the stylized facts stated above. Yet the discussion is not very transparent and based on only few sources. In order to avoid the impression of ad-hocery, it might be useful to provide a more systematic indication (e.g., by way of a table) of the range of threshold values of the parameters, and to make sure and explicit that the references to standard empirical results are representative (in terms of recent key contributions or surveys).
- (4) Section 4 on the policy implications is unsatisfactorily brief and imprecise. Instead of discussing the results from sections 2 and 3 systematically in terms of the analytical framework itself (which could easily be done), the section raises various new points and refers to other literature in a very general way.
- (5) There are numerous typos, both in the text and in the footnotes.