

## Comments to Referee Report No. 1

First and foremost, I would very much like to thank the first referee of this paper for his/her very thorough, encouraging report. I essentially agree with all the improvements put forward in the report, which I will try to address in a revised version of the paper. In this regard, what follows are merely a few very minor clarification points:

1. I completely agree with the statement that “the largest potential of the paper lies in the stochastic modelling of asset prices and in the analysis of the role of asset prices in the business cycle”, and that this should be more prominent in the paper. The reason I puts so much emphasis on the ‘fallacy of diffusion symmetry’ is that it is such a widespread misconception that it often led readers astray (particularly those more familiar with DGSE models based on deterministic, discrete-time tools). Nevertheless, in the next revision I will try to keep the fallacy discussion down to the strictly necessary to give more relevance to the model itself, which is clearly more important.
2. Regarding the aggregate production function (Section 5), the model in Gracia (2011) aims to show how, in a stochastic world, redistribution flows under the form of economic rents may indeed impact aggregate productivity. The intuitive reason is that redistribution changes the balance of fixed vs. variable costs, and these two elements do not respond in the same way to stochastic shocks. Specifically: the higher the weight of rents over output, the more rigid the production cost structure – and, under uncertainty, rigidity translates into inefficiency. Hence, even if rent-seeking agents’ behaviour does not create any ‘waste’ (i.e. the principals’ loss translates entirely into agents’ gain), their mere success in imposing their rents adds rigidity to the aggregate production function, and results in a more inefficient response to stochastic shocks.

3. In this context, Gracia (2011) did not propose to leave capital out of the production function, but rather to reflect its impact through the economic rents it generates instead of doing so through its value-weighted aggregate. A set of empirical tests of this function against the Cobb-Douglas (which relies on capital aggregates instead) supported that this was a more fitting way to represent aggregate production – and, furthermore, showed that neither current nor lagged capital aggregates correlate to the residuals. This, to be sure, does not imply that capital has no impact on production capacity: only that a value-weighted aggregate is probably not the best to model it.
  
4. More generally, the point of Gracia (2011) is that *any* economic rent (i.e. any form of income redistribution adding to the difference between output value and marginal cost) can lead to rigidity, and therefore inefficiency, in an uncertain world. Hence, the principal-agent rents cycle modelled in the refereed paper is just one possible mechanism relating to a specific type of rent, but other rents (e.g. rents on fixed capital, debt and financial assets, real estate, oil and mining commodities, oligopoly rents, efficiency wages, government taxation, etc.) could also be subject to cycles with different wavelengths. A possible forward research programme might therefore start by re-examining earlier models in search of insights into these rents' behaviour, e.g.:
  - a) Richard Goodwin's (1967) 'class struggle' model (which, I agree, should be given more prominence in my paper) modelled a cycle of what today we might interpret as efficiency wages, but depended on a non-rational-expectations behaviour model and, perhaps more crucially, on the assumption that transfers from capitalists to workers reduced investment volumes to justify a real impact of redistribution on GDP. Yet if, instead, one redefined the model to follow not the expected but the median path, and considered that rents reduce

productivity simply by making the aggregate production function more rigid, the model could probably be made fully compatible with rational expectations.

- b) Perhaps more relevantly in the context of today's global recession, debt deflation models of the cycle such as those proposed by Steve Keen (e.g. most recently Keen 2011) are compelling, but are also subject to the double criticism of (1) assuming irrational expectations (because rational investors would have discounted the risk of default from their initial lending and (2) assuming a mechanism by which the volume of debt has a real impact on aggregate demand (as opposed to merely transferring purchasing power from lenders to borrowers). Yet objection (1) can be made disappear if the model is explicitly designed to follow not the expected path but the median (as the best representation of the observed time series), and objection (2) also disappears if, following Gracia (2011), one models net debt payments as a rent which in good times is very small (because it is offset by further lending flows) and becomes big in recessions as debt repayment flows exceed additional lending.

Once again, I would like to express my appreciation for the referee's very helpful suggestions, which I will try to address in a revised version of the paper.

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