Comments to:
‘Stylized facts on the interaction between income distribution and the great recession’

The paper investigates whether the great recession was linked to the pre-crisis income distribution. It uses four indicators for the size of the recession (GDP change in 2009, cumulated GDP change in 2008-10, peak-to-trough GDP fall, GDP trend change between 2008-10 and 2000-07) and five indicators for the pre-recession distribution (Gini coefficient in 2005, poverty rate in mid 2000s, inter-quintile ratio in mid 2000s, wage share in total incomes in 2007, adjusted wage share in 2007). Both sets of indicators are also collapsed into two summary indicators by means of principal component (PC) analysis. The empirical evidence is based on the correlation between these indicators across 37 mainly industrialized countries. Results do not show any strong link between income distribution and recession.

The paper provides interesting empirical evidence on the raw correlation between pre-crisis income distribution and the extent of the great recession. The use of multiple indicators for both phenomena is also an interesting aspect of the analysis. These results would be a good starting point for an in-depth analysis of an issue that is receiving growing attention in the literature. Yet, it cannot constitute a paper by itself. Measured correlations may have little meaning if they are not integrated with a careful analysis of other potentially confounding factors – something that is explicitly recognised by the authors in writing that ‘the existence of more compelling evidence … may have been diluted by the global character of economies’ and in their discussion in section 6. Apart from this fundamental problem, the paper has in my view other drawbacks.

First, the overview of the literature on the link between the pre-crisis distribution and the recession could be considerably sharpened. This section should go beyond a plain summary of the links suggested in the literature and show how these links could be empirically tested. For instance, some authors stress that income inequality affected recession through consumption patterns (namely, the different propensities to consume at different income levels). It would have been interesting to show how this channel could be tested by also considering consumption. Finding an effect of the distribution on the recession is much less interesting than providing some evidence on the underlying channel. Moreover, I have the impression that in some cases the description of the various mechanisms can be improved: with regards to Fitoussi and Saraceno (2010), for instance, it is unclear to me who are the ‘people profiting from redistribution’ and whether ‘high return on investment’ does mean ‘high capital gains’ or something different. Lastly, a short paragraph is devoted to the reverse effect, i.e. the impact of the crisis on the distribution of income. This issue is not central to the paper, but the literature is richer than the single reference in the paper. See, for instance, the volumes ‘The Great Recession’ (edited by Grusky, Western and Wimer, 2011), ‘Who Loses in the

Downturn? Economic Crisis, Employment and Income Distribution' (edited by Immervoll, Peichl, Tatsiramos, 2011), and 'The Great Recession and the Distribution of Household Income' (edited by Jenkins, Brandolini, Micklewright and Nolan, 2013), and the references therein.

Second, in a purely empirical paper like the one under examination it is crucial to provide a careful documentation and discussion of the variables used in the analysis. This is almost entirely missing. For instance, it is possible to deduce that the Gini coefficients are drawn from the OECD's Standardized World Income Inequality Database only looking at table sources. No further information is provided about these data, or why they have been preferred to other sources used in the literature. GDP series appear to have been drawn from Eurostat: is it also the case for the many non-European countries considered in the analysis? How are the cyclical 'peaks' and 'troughs' identified? Is it the OECD's business cycle dating? What is the exact definition of 'wage shares adjusted by employment changes'?

Third, while I appreciate the use of multiple indicators, it is not clear to me the gain from focusing on the overall PC indicators. I understand that the aim is to reduce the dimensionality of the problem and to eliminate redundancies, but what is the economic meaning of these summary indicators? Sub-indicators provide different information on the great recession. One might argue that their link to income distribution indicators is different exactly because of their specificities: mixing them into a single indicator might obscure the true links.

The paper needs some checks, including references. Here are some examples.

• Throughout the paper the term 'polarization' is used as equivalent to 'inequality'. However, polarization is currently used to indicate something different from inequality, such as the spreading out of the distribution from the median (Foster, Wolfson) or clustering phenomena (Esteban, Raj). I would suggest not using the term polarization, in order to avoid misunderstandings.
• Page 5 – Atkinson et al.: should it not be 'Atkinson and Morelli'?
• Page 5 – '… he found that inequality …' should be '… they found that inequality …'.
• Page 5 – Bordo and Messner (2011) are missing from references.
• Page 7 – What is the exact meaning of 'change towards the 'mid nineties''?
• Page 7 – Drop 'the' before 'lie between zero …'.