COMMENT TO "THE IMPACT OF INCOME INEQUALITY AND FISCAL STIMULI ON POLITICAL (IN)STABILITY" BY LUCA AGNELLO, VÍTOR CASTRO, JOÃO TOVAR JALLES AND RICARDO SOUSA

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In the past several years the distribution of income and income inequality have experienced a remarkable comeback in macroeconomics. Largely ignored before, the post-2007 financial and economic crisis has made the economic profession more sensitive to a trend that actually had been going on for years, if not decades. More and more questions are being raised about the macroeconomic and political impact of inequality as in some advanced economies indicators of income distribution returned to levels observed at the beginning of the 20th century.

The paper by Agnello, Castro, Tovar Jalles and Sousa is part of this new wave of interest. It sets out to explore the empirical nexus between income inequality and political (in)stability. The underlying, and a priori perfectly sound assumption is that unequal societies are more prone to political instability and that fiscal expansions can quell political discontent. The results of the empirical analysis carried out by authors – a panel regressions covering 128 OECD and non-OECD countries – broadly confirm these priors. In fact, all of their findings look very sensible from a macro perspective and seem to confirm popular priors as well as existing findings in the literature. I do not review them in detail here. The presentation in the paper is very clear and comprehensive.

There is only one specific finding, which in my view stands out (at least in the version of the paper that I had the pleasure to review for the 2015 edition of the Banca d'Italia workshop on public finances) and which I personally find rather delicate. Notably, too much democracy is reported to be bad for political stability, especially in non-OECD countries. While I am not a political scientist and hence may not be familiar with the relevant literature, I find such statements too sweeping especially when based on results from reduced form regressions. The sign and statistical significance of a coefficient in a panel regression do probably not represent ultimate and compelling pieces of evidence to conclude on the role of democracy for political stability. Are we sure of the causality? Are the proxies for democracy really robust? Do we really understand how the alleged transmission between democracy and political (in)stability works? These are only some of the question that came to my mind when I stumbled across this particular finding and which, after all, is not crucial to the main focus of the paper and could possibly be toned down.

At the same time, by taking issue with the paper's conclusion on democracy I implicitly open up to a more general difficulty of empirical papers that address very weighty macro questions via reduced-form relationships. The number of possible interactions is large, the direction of causality very complex and the existing empirical literature correspondingly rich. Figure 1 tries to illustrate in a very simplistic manner the space of macro factors at play.

The parts in colour highlight the specific angle taken by the authors whereby political instability is expected to be influenced by income inequality and fiscal policy making while at the same time controlling for the possible role played by the macroeconomic situation and a number of institutional features. Of course, as becomes clear from the illustration, there are many other possible angles from which one can look at the space of macro relations. In fact, the literature

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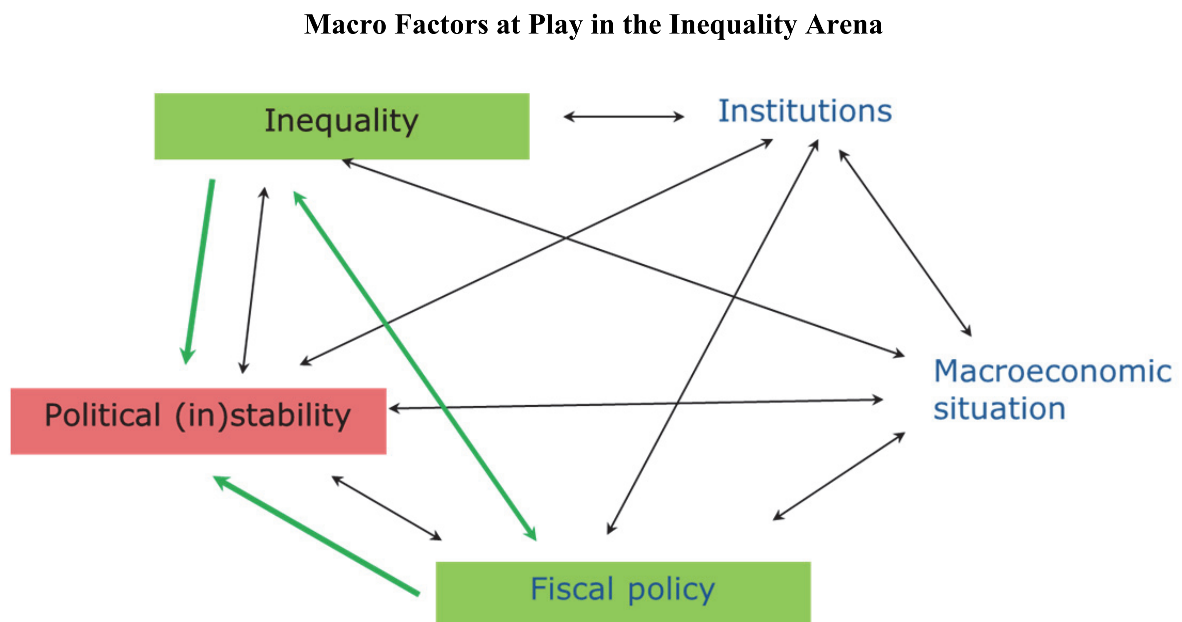
Figure 1

Macro Factors at Play in the Inequality Arena



review included in the paper is very extensive covering almost all possible combinations such as the impact of institutions on macroeconomic performance or political stability, the impact of fiscal policy on economic performance, the link between institutions and fiscal performance to mention a few.

I am not making this point to put into question the paper by Angello *et al.* In particular, I am not trying to argue that the empirical strategy chosen by Angello et al. is inappropriate, inadequate or not granular enough. There are far too many papers in the empirical literature following the same or a similar path, including one of my own papers.[1] In fact, the econometrics used in the paper is fairly solid.

What is lacking for my taste is a more detailed description or a theory of how the postulated nexus is expected to operate. As indicated earlier, the presumption that income inequality may nurture political instability is intuitive. However, the microeconomics of the link is not spelled out and not that obvious. Since the space of possible linkages is so complex and allows for many different narratives it would be very useful to refer to relevant political economy models. Without such a theoretical reference or compass that would help navigate the data many questions concerning the results spring to mind and remain unanswered.

Firstly, and going beyond the apparent intuition behind the presumption of the paper, the political science literature known to me does not establish unconditional links between income distribution and political (in)stability. To be more concrete, political instability is rarely driven by the 'bottom 5 or 10%' of society. Most of the times political discontent, which eventually translates into instability, originates in fears of the politically more involved middle or upper middle class. Keeping this in mind, how would changes at the upper or lower end of the income distribution

[1] Larch, M. (2012), Fiscal Performance and Income Inequality: Are Unequal Societies More Deficit-Prone?, *Kyklos*, No. 65, pp. 53-80. doi: 10.1111/j.1467-6435.2011.00527. The paper looks at whether income inequality has a direct and indirect effect (via political instability) effect on fiscal policy. The findings are consistent with those of Angelo *et al.* in the sense that I find a significant link between inequality and fiscal performance. The crucial difference is that I postulate a different causality.

effectively translate into political (in)stability? Can we assume a linear relationship, or are we more likely to deal with non-linear relationships where thresholds may play an important role?

Also, since the early 1980s income distribution has become increasingly more unequal in OECD countries, in some cases reaching all-time highs. Has political instability increased? On the face of it, no. What are then the other factors at play? Better political institutions? While I am quite sure there is no single theoretical framework capable of providing an answer to all these questions, finding and describing relevant models would further improve an already interesting paper.

Another key aspect of the paper that deserves a moment of reflection is the assumed link between income inequality and fiscal expansion. Again, the intuition underlying the paper is straightforward: To the extent that fiscal policy can influence the distribution of income, specific fiscal episodes should help mitigate political instability. In practical terms, and assuming that fiscal consolidation is detrimental for income distribution, Agnello *et al.* proxy the relevant fiscal policy episodes with indicators of fiscal stimuli where the indicator looks at a given change in the cyclically-adjusted primary budget balance.

As before, this approach looks perfectly reasonable from a certain distance but triggers a number of important questions. For instance, is a fiscal stimulus necessarily beneficial for the distribution of income? Is fiscal consolidation always detrimental? One can easily think of tax cuts that favour individuals with above median income. While I am not an expert of recent or less recent tax reforms that lead to a reduction in the tax burden, I am sure there are quite a few reforms that increased the inequality of income rather than reducing it. By the same token, it is not at all obvious that fiscal consolidation will always increase the gap between the rich and the less well off. A recent and quite prominent example is the EU-IMF financial assistance programme for Ireland which encompassed a very significant fiscal retrenchment. However, the expenditure cuts and tax increases were implemented in such a way as to minimise their social impact. A quick look at relevant data shows that the consolidation episode was successful in two ways. It brought public finances back to a sustainable path while keeping indicators of the distribution of disposable income, that is, after taxes and transfers, essentially unchanged throughout the adjustment period.

To use fiscal stimuli as proxy was most likely dictated by the availability of relevant indicators. As far as I am aware, there are no datasets providing cross-country comparable indicators measuring the distributional impact of fiscal policy measures. However, even leaving aside the issue of data availability the question of how fiscal policy actually affects income distribution remains. Does the effect work directly and predominantly through taxes and transfers or rather indirectly through the effect on aggregate demand and unemployment? The paper could provide some more insights in this regard. Unemployment is one of the most incisive events that lead to a loss of personal income. Hence, the rate of unemployment would be a natural candidate to be added to the list or variables controlling for economic conditions in the panel regressions. Apart from affecting the distribution of income, unemployment is arguably also a factor that should directly impact on people's sentiment *vis-à-vis* government even if social protection is effective. I may be wrong, but my prior is that once the rate of unemployment is included, inequality may lose some of its statistical significance. This would not invalidate or undermine the findings of the paper. It may clarify the actual channel through which fiscal policy and inequality affect the political stability of a country.