Referee 2

Review of 'Reduction of income inequality and subjective well-being in Europe'

Summary

The paper uses four waves of data from the European Social Survey to study the impact of inequality and government-induced reduction of inequality on life satisfaction. It is found that lower inequality and government-induced reduction of inequality increase subjective well-being. This is shown in a multivariate modelling approach, using both using individual level as well as aggregate level data.

Evaluation

This is a generally well-researched and written empirical paper, which tackles a research question that should be of interest to an academic as well as policy-oriented audience.

1. One important reservation I have with the paper is that, at least in my view, the authors are trying to oversell their results. For example, as the estimation results on inequality show in Table 2, significance is clearly reduced to what is reported in Table 1. In any case, I am not sure that it makes a lot of sense to look at significance levels as high as 10%, at least given the sufficiently large number of observations even for the aggregate variables (for the individual-level variables, a 10% significant level is totally inappropriate, even the tiniest violations of the null hypothesis will yield a significant test outcome). Given the number of models estimated, it means that, in expectation, one of the inferences on inequality is statistically wrong. Thus, I would advise a more conservative testing approach, as empirical economics suffers from a strong tendency to over-represent significant results anyway. In my view, Table 3 is even more problematic. The strongest robustness test is implemented in model (2), as it controls for country-specific time trends. And indeed, given my earlier comment about significant levels, there is really not much left in terms of significant results. Finally, I think it is good work that the authors provide tests of differences across coefficients in Table 4. It is less impressive that in the interpretation, they do not take the outcome seriously. Of the presented 10 tests, only two are significant at a 5% level and none is significant at a 1% level. Moreover, none of the tests focussing on inequality reduction is significant even at a 10% level. So my reading of that outcome would be that there is little evidence that results vary across the different groups studied in Table 4 and that, overall, estimates are rather homogenous and not heterogenous, as claimed. In particular, there is no statistical evidence that the effect of inequality and inequality reduction are statistically different in Eastern and Western Europe. So I would advise more caution in terms of the interpretation of the findings.

2. Other reservations I have refer to the mechanism of how inequality is actually transmitted to individuals. Put differently: how do I personally learn about income inequality in the whole society? Given that I cannot observe millions of citizens, I can either rely on official statistics or I can extrapolate local observations to a national level. But if it is local observation, then movements across different neighbourhoods may be registered as changes in the income distribution, although, in fact, the national income distribution remains unchanged.

3. Assuming a convincing answer to this question exists, another issue comes up: if it is perceived inequality that matters, as argued in the paper too, why should that not be influenced by short-term fluctuations? Given the focus on trends, however, this information is no longer part of the analysis.

So perhaps it would be interesting to look at these temporary fluctuations, especially if they represent officially-published values. I would suggest conducting the analysis again using original values as well as using trend and short-term variations as distinct regressors.

4. A related issue refers to gross income inequality. As soon as we deviate from a pure rational agent perspective, and this deviation is quite common in the happiness literature, then it is no longer obvious that people will disregard gross income inequality and will automatically focus on net income inequality. So it could be interesting to look at that as well.

5. Technically, while I can understand the motivation underlying the use of HP-filters, I am not sure that this is necessarily a good idea in practice. The HP filter is symmetric, but the current sample lies at the right end of the observation period for constructing the trend inequality series. This means that the filter is no longer symmetric for the recent values of the inequality variables, which might distort the results.

6. I would like to see more discussion of how we should interpret the degree of inequality reduction. This is obviously not equal to government redistribution per se. But is this indicator really equal to net government redistribution? For a proper interpretation of results, it seems crucial to me to get a better understanding of what exactly this variable measures. Moreover, I am not convinced that it can easily be interpreted as a 'process' variable. Thus, again, more discussion is needed.

7. I am surprised that issues of endogeneity are not seriously discussed. To me, at least in terms of inequality reduction, this would seem to be an obvious potential problem.

8. Given that government redistribution activity is (partially) measured by inequality reduction, I am wondering whether it is still appropriate to use GDP measured at market prices as the relevant control variable. This is also affected by government activity and one might want to control for that to get close to what people actually receive, which would suggest using GDP measured at factor prices. Moreover, a number of economies in the data set are highly open to international capital flows and trade. Thus, rather than using GDP, GNE might be a better income aggregate to capture average income in the economy. In any case, GDP per capita does not equal 'society's average wealth' (p. 5), as GDP measure an income flow and not a stock variable, such as wealth.

9. I take issue with the interpretation of 'percentage of average equivalent household income' as a purely relative income effect. As long as there is no indicator measuring absolute household income, this variable cannot be interpreted as a pure relative income measure but contains a mix of absolute and relative income effects.

Minor issues

1. I do not understand why there should necessarily be a negative connotation to the finding of a positive association between income inequality and work hours, as the interpretation given by Bowles and Park (2005) is at least debatable.

2. For the sake of interpretation, it might be useful to rephrase the inequality reduction variable, so that its interpretation in terms of coefficient signs is symmetrical to the inequality variable itself.

Writing ‘self-reported measure of subjective well-being’ (p. 4) sounds like a tautology to me.

3. Equation (2): Note that the constructed indicator is measured in per cent.