Reply to referee 2

Thank you for your comments.

Comment:
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.

Answer:
Regarding Table 2 and Table 3 we think that our interpretation is appropriate and careful enough.
In Table 2, coefficients of four models are directly comparable with the main result (Model 1, 2, 5, 6). In these models, coefficients on inequality reduction remain roughly the same (between 0.030 and 0.045 vs. 0.036 in the main model). As we noted, most of the Eastern countries are excluded from Model 4, and in Model 7 Western countries are also overrepresented due to the population weights, which might be the main reason of the lower point estimates. Despite the change in the sample, even in these models the coefficient on inequality reduction is always significant at the 1 percent level. We also explicitly noted in the paper that the estimated coefficient on income inequality is insignificant in one case and only marginally significant in another one.

In Column 2 of Table 3, the estimated coefficients lost their significance, but the magnitude of the point estimates are noticeably higher than in Column 1. They are insignificant due to the imprecise estimation (higher standard errors), but it is not a surprise as the number of the estimated parameters are much higher than in Column 1 (because of the included country dummies). In this model, we have 65 observations and 34 independent variables. We interpret the result of Model 2 of Table 3 as suggesting that income inequality reduction has a positive and income inequality has a negative but imprecisely estimated effect on life satisfaction. We changed the paper to be clearer regarding the results in Table 3 (page 10, paragraph 3).

Regarding Table 4 we agree with the critics and changed the interpretations to be more careful.

Comment:
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.

Answer:
We agree that local income inequality might be more important, but we think that the change in national inequality and the change in local inequalities are positively correlated. Unfortunately, we have no data on local inequalities.
Nevertheless, it seems that using nationwide inequality is not completely wrong: In the ESS dataset there is a significant negative association between our inequality reduction variable and preferences for redistribution (the share of those who strongly agree with the statement that the government should reduce income differences) on the country-wave level, even controlling for unobserved time-invariant country characteristics and time trends that are common to all countries.

Comment:
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.

Answer:
Model 3 in Table A5 of the Appendix includes not only the trends of income inequality and income inequality reduction but the cyclical components as well. The results clearly show that the effects of the cyclical components are zero. The effects of the trend variables remain unchanged. This might mean that the cyclical components indeed contain the noise caused by the survey measurement of Gini indices, as we argue in the paper. It might also mean that temporary fluctuations of inequality and inequality reduction do not matter, but we think that using survey data these short-term fluctuations cannot be measured with the necessary precision, thus, the first interpretation of the result is more reasonable.

Comment:
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.

Answer:

Model 5 in Table A5 of the Appendix checks this suggestion. The coefficient of pre-government Gini index is negative and significant. It means that higher pre-government inequality is associated with lower well-being, since, for a given level of inequality reduction, higher pre-government inequality indicates higher post-government inequality. If we run a model where only pre-government Gini index is included (inequality reduction is excluded), then the effect of pre-government inequality is insignificant.

Comment:
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.

Answer:

Gini indices are based on surveys, thus, they are inevitably measured with noise. Therefor our estimates, based on the "raw" data, might be biased (toward zero). We think that using trend values could mitigate this problem.

We checked whether calculating trend values using data for the years 2001-2009 change the results. These coefficients are shown in Table A5 of the Appendix (Model 4). We can see that the estimated effects are slightly lower, but the main conclusion is not altered.

Comment:
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.

Answer:

We added some more explanation regarding the income inequality reduction variable (page 4, the paragraph after Eq. 2).

Comment:
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.

Answer:

We included country dummies in the models to control for unobserved time-invariant country characteristics, and wave dummies that accounts for time trends that are common to all countries. Of course we cannot rule out the problem of endogeneity entirely, but this is the best solution that is widely used in the literature.

Comment:
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.

Answer:
We corrected the mistake on page 5.

We also checked the results using expenditure-side real GDP from Penn World Tables (which is recommend to compare relative living standards across countries and over time) and the results remained the same.

Comment:
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.

Answer:
We think GDP is a proper proxy for average income, thus 'percentage of average equivalent household income' can be interpreted as a relative income effect. This is supported by the fact that in a model that includes absolute income (equivalent monthly household income) as well, coefficient on GDP is significantly smaller. Coefficients of income inequality and inequality reduction remain unchanged in this case.
Furthermore, these variables serves mainly as control variables, thus they are not in the focus of our analysis.

Comment:
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.

Answer:
According to the empirical literature the effect of working hours on subjective well-being is negative or insignificant. It seems that at least overtime has a negative effect.

Booth, Alison L. – van Ours, Jan C. (2008): Job Satisfaction and Family Happiness:The Part-Time Work Puzzle. *Economic Journal.* Vol. 118, 77-99.
Pouwels, Babette – Siegers, Jacques – Vlasblom, Jan Dirk (2008): Income, working hours, and happiness. *Economics Letters.* Vol. 99, No. 1, 72-74.
Knabe, Andreas – Rätzel, Steffen (2010): Income, happiness, and the disutility of labour. *Economics Letters.* Vol. 107, No. 1, 77-79.
Harter, James K. – Arora, Raksha (2010): The Impact of Time Spent Working and Job Fit on Well-Being Around the World. In.: E. Diener, J. F. Helliwell and D. Kahneman: *International Differences in Well-Being.* Oxford University Press, Oxford, 398-435.

Comment:
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.

Answer:
We think that the positive sing of the inequality reduction variable is intuitively correct, since it means that the larger the extent of inequality reduction, the more satisfied the people are. We corrected the 'self-reported measure of subjective well-being' expression.

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

Answer:
Although below equation (2) we explained that „the inequality reduction index shows the percentage reduction in inequality by government tax and transfer policies", we highlighted it in the explanation of the notation as well. (page 4).