Thanks for sending me this paper, I actually thought it was quite interesting and think it is publishable subject to the revisions below.

The main point to address is that while the paper claims to be about social comparisons, it is in fact about financial dissatisfaction, and financial dissatisfaction is by no means the same as social comparisons. Therefore, the paper needs to be re-written completely to address this.

Related to this, it is not clear that social comparisons are always detrimental for well-being, as the paper claims. It could be that social comparisons actually spur ambition and thus increase well-being.

The second main point has to do with the claims of causality that the paper makes. I think these need to be toned down substantially. The IV is a good attempt, but it is not at all clear that it is appropriate. There are also demand factors for fast phone/internet and, above all, people will sort themselves in places with fast internet (which are also most developed etc) based on unobservable factors such as ability. Less able people will also earn more and will be less financially dissatisfied (and will also have more time to go on Facebook etc). I don't see how the IV specification can address this. There is an interesting correlation between SNS and financial dissatisfaction, but the authors need to be upfront about this and not oversell their IV approach.

Some additional points:

- it's unclear why the survey data used run only until 2012/2013

- are there any specific questions on social comparisons in these surveys? LiTS 2016 for instance includes a question as to who exactly respondents compare themselves to (friends, family etc)
- overidentifying restrictions: I am not sure if anyone believes these, and they cannot prove that the instruments are orthogonal. As Deaton 2009 explains in his JEL article, IV identification is always a substantive, not a technical issue.

- Related to my previous point about overselling the IV, the Hansen J stats are quite borderline in several cases. These are suggestive results at best

- the authors should give some indication of the magnitude of the coefficients on SNS and how they compare to say the impact of being unemployed or unhealthy

- there are some interesting different results with some variables (e.g. GDP per capita has different signs) when the two survey sources are used. Perhaps worth elaborating on why that is the case