Referee report on:

"Family reunification or point based systems? The case of the US and Mexico" by Lopez Real.

The paper tackles an interesting question, namely what would be the effect for the US of switching from the current family reunification based immigration policy to a point based system. The author develops and calibrates a dynamic general equilibrium model to answer it, focusing on a two country setting in which the only source is Mexico (whereas the US are the destination). Many of the results obtained appear rather surprising and unrealistic. Furthermore, the paper is very poorly and not precisely written. It is full of small mistakes, and at least half of the references quoted in the manuscript are missing. Many of those references are the sources of the parameters used in the calibration exercise, and the lack of precise information makes it rather cumbersome to assess the quality of the analysis.

Overall, I do not think that the manuscript should be published, and I recommend the author to spend much more time in cleaning up his work in the future before submitting it to a journal.

Major comments

Is the contribution of the paper significant?
- Potentially the paper could be of interest. But to assess the effects of introducing a point based system in the US, looking only at migration from Mexico is rather restrictive. Indeed I would expect substantial inflows of skilled migrants under this system from other developing countries, like China and India (just to name a few examples).

Is the analysis correct?
- I could not find any obvious mistake in the setup of the problem, but I have many doubts concerning the parametrization of the model, and some of the results obtained in the paper appear quite counterintuitive. For example, one important assumption is that the real rate of interest is at 5% in the years after 2005. This sounds way too high. A more realistic figure would probably be 0.5-1% over this period. The estimates of the *pecuniary* component of the migration costs also appear rather unplausible, as they are set at the one year average earnings in the US. I am not sure how these figures are calculated, but they really do appear very high. Also, I wonder whether the author did not use some measure from the literature on labor market discrimination to estimate the non-pecuniary cost of immigration (i.e. the loss in productivity).

Main strengths and weaknesses.
- I have already discussed some of the strength and weaknesses of the paper above. An important point which should be addressed is that point based immigration systems can vary substantially in their emphasis on different desirable attributes of the migrant, and in the paper the analysis of this aspect is surprisingly crude, given how central this is to the model. In particular, having a point based migration system translates in admitting only individuals with at least a high school degree. At a minimum the author should better defend this assumption. More appropriately, I would also like to see a series of robustness checks carried out to assess the effects of imposing tougher skill requirements.
- Some of the results of the analysis are rather counterintuitive. For instance, the author shows that a point based immigration system will initially increase the skill level of the migrants compared to a family reunification based system, but that over time this effect would vanish, i.e. both types of policy will lead to similar long run skill profiles for the migrants.

Somehow this result looks rather unrealistic, and I wonder about the role played by the adjustment in the interest rate in generating it. An alternative strategy could be to assume that both countries (the US and Mexico) are "small" when it turns to capital markets, so that they take the interest rate as given, and show what would be the dynamics in this case.

- Another set of results that should be clarified concerns the effects of the two policy regimes on income inequality. The author numerically shows that migration brings about a reduction in inequality in both countries, and as this result is somewhat counterintuitive, I would like to see it better explained.

- As mentioned before, the paper is rather poorly and non-precisely written. Often quantitative statements are made, without citing the source. There are numerous typos. For instance, the 1924 legislation cited on page 3 referred to the composition of the US population in 1890, not in 1980 as mentioned in the paper. Also, the author refers on page 4 to US Census data for 2009/10, but it is not very clear what exactly is the source etc. etc.