Review of: "Time evolution and convergence of simple migration models"

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Potential competing interests: No potential competing interests to declare.

There are several issues with this paper:

1. I don't think peer review is supposed to replace careful proof-reading. For instance: there is leftover material at the end of the Introduction, at the end of page 1 there is a reference to Fig 2. that should be to Fig 1, the legend in Fig 1 refers to population ratios as p_i and p_j when earlier those ratios where called m_i and m_j, etc. I stopped taking notes after this.

2. In both models, a cell that has no population will never have a population since the inflow into it is proportional to the population. Hence we have at least as many steady states as we have cells.

3. The radiation model introduces heterogeneity - it is trivial that you will not get a homogeneous steady state for a heterogeneous geography. However, what might be interesting is whether you get a steady state that reflects the heterogeneity. For instance, if you have a linear gradient in the density of the cells, do you get a steady state distribution that has a similar linear gradient.

4. There are important scales in these models that are not related to each other. The relevant scales are distance, time and populations. For instance, the flow models are essentially treated as continuous which is the tacit justification for ignoring zero population cells. Cells are discrete and there are small number of them while there is an interest for long times, i.e. time is considered continuous. It is very unclear to me how these various implicit assumptions about scales relate to the supposed interest about population migration on a scale of the earth.

5. The globe is not a model for periodic boundary conditions - a sphere is not a torus topologically. This is not nitpicking: a torus allows a stationary nonzero flow whereas a sphere does not.

6. The fact that the gravity model has a unique singular steady state is noted as a deficiency. However, the deficiency lies in the homogeneity assumption and the assumption of deterministic flows. Giving the gravity model a range or a random component will easily allow for multiple metropolities.

7. The major problem with the paper is that it does not ask questions related to population flows but ask modeling or mathematical questions and creates population flow answers. A better approach would be to discuss open issues in population flows and then discuss which model can answer them or which type of model, with some additional assumptions will be able to say something relevant to the population flow issue.