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Title: Nonequilibrium steady state and induced currents of a mesoscopically glassy system: Interplay of resistor-network theory and Sinai physics

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I am one of the same referees who reviewed the author's previous submission LJ13309 entitled *Nonequilibrium steady state of sparse systems with nontrivial topology*, which was rejected by PRL in 2011. After two years we have a variation on the same theme, as the authors resubmitted a revamped version of their original ms. Some similarities and differences are as follows:

- The model is the same.
- The authors tried to take my previous criticism on board that the ms. 'completely lacks a general introduction ... that motivates this work, and makes it accessible, for a more general audience.' Unfortunately, in my view this attempt goes very wrong as explained in detail below.
- Their previous Fig.1 has been replaced by a much more 'appealing' one; unfortunately, I do not see the science in it; details see below.
- The detailed calculations of the SMF have been largely moved into the Supplement; quantities like driving intensity have been relabeled; plots like Fig.2 have been re-done in semi-log plot.
- The old ms. focused more on estimating the EAR; here the details are more on the statistics of the model.
- The same formula for the current is derived by using, as far as I can see, the same method, just rewritten in a different way.

In summary, a very analogous ms. about the same model with the same methods, and to quite some extent the same results was rejected quite some time ago by PRL. I acknowledge that this version contains in addition some new results. The authors have also made an attempt to rewrite this ms. such that it better addresses a more general audience. They also made an attempt to relate their results more closely to experiments. Unfortunately, in my opinion they do not succeed neither with the former nor with the latter, as explained in detail below. I thus come to the same conclusion as two years ago: I consider this to be an interesting paper which, however, is clearly important for a more specialized audience being familiar with modeling and theoretical analysis of random walks in random environments. Or, as previous reviewers put it, this ms. still 'lacks impact'. As last time, I strongly recommend to put main text and supplement together for a long paper. Along these lines, the authors could

have informed their scientific community two years ago already about their findings. Such a long paper should form a very nice contribution to Physical Review.

1. p.1, 1st paragraph: This part provides in a way the 'general motivation'. Within a few sentences the authors cite 24 references. The first 8 are ok. I then took the effort of checking quite some of [9-24] to see whether they are really closely related to their model: Refs. [9-12] are about Langevin dynamics for ratchets with purely periodic non-random potentials. I am sorry, but where is the cross-link to the author's model here? The ratchet phenomenon is not due to random walks in random environments. I also checked Refs. [14,18,19,20] (with [20]=[22]). They all treat somehow one-dimensional chain models, but I failed to detect any close resemblence to the model studied by the authors. After this I gave up checking further citations. It seems to me that the authors are rather setting up a smoke screen here than being scientifically sound. Like two years ago, I thus completely lack a general motivation of their model.

2. p.2, 2nd paragraph: In my view, Sinai diffusion is a very special case due to asymmetric transition rates. In the author's paper it looks as if this case is extremely common. If so, they should cite more literature about it, in particular experiments where Sinai diffusion is observed thus replacing the many superfluous citations in paragraph one.

3. p.2, 3rd paragraph: To which extent does the 'glassiness' that the authors describe in this part match to, e.g., typical glassy systems as described in Ritort, Sollich, Adv. Phys. 52, 219 (2003)? To me the author's 'glassiness' appears to be a very atypical case that is not physical.

4. p.2, 4th paragraph: I am sorry, but the authors do not choose a model that 'has all the essential ingredients of the problem'. Rather, they try to find a physical problem that matches to their model. Nothing wrong with that as long as one does not create cross-links that are not there; unfortunately, cf. remarks 1-3 above. This were not necessary if the authors were satisfied with a publication in Phys. Rev.

5. p.1, Fig.1: May I conclude from this figure that if I put my ring into a water glass, and on a hot Summer day expose it to the sun, there is a current induced in my ring? I appreciate that the authors try to attract the attention of a wide audience with this nice-looking figure, but in my view this goes a bit too far.

6. p.1, 5th paragraph: How typical are log-wide disorder distributions in physical reality? Can the authors give examples that have been studied in experiments?

7. Fig.2: The color code for this figure seems to be missing.