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Comment on egusphere-2022-865

Anonymous Referee #2

Referee comment on "Rain process models and convergence to point processes" by Scott Hottovy and Samuel N. Stechmann, EGUsphere, https://doi.org/10.5194/egusphere-2022-865-RC2, 2022

The authors propose a pathway on how to link between a somewhat physically based diffusion processes and more common empirical/statistical pointwise processes. Even though the diffusion processes suggested are not necessarily the most widely accepted models for rainfall. The idea is very good as it may help in guiding future modeling strategies in terms of being able to combine physical intuition with data. The main results consists of a a) rigorous proof in the L2 norm, and b) and not so rigorous and rather very shaky asymptotic expansion, of the former type of processes to the latter. I think the paper can potentially become a very good publication worthy for the readers of NPG-EGUSphere but I conquer that the paper maybe better read if it was submitted to a more math/stats oriented paper as statisticians/mathematicians from other disciplines my appreciate as well as. Nonetheless, the following points may need to be addressed before the paper can be published.

The most serious suggestion I can make is that the authors could simply forgo the section of the asymptotic expansion because it is a mere distraction that doesn't add anything to the main result. I have a hard time making sense of the asymptotic expansion work which seems to have very serious flaws (see specific comments below). However, I trust that the convergence results in Section 3 are correct although I haven't gone through all the details, especially I haven't checked all the references to make sure that results reported in the literature have been applied correctly, namely because it is my specific area. The revised paper may benefit from being reviewed by a theoretician working in the area of statistics/stochastic processes.

Some of them as mis-comprehension/bad notations or typos and some are more serious by they are provided in the order as they appear in the paper.

1. Line 30 - 4: (which is the mixing ratio of water vapor in the air), delete "mass, or mass"

2. Line 45+3: Change "when q(t) reaches a lower threshold" by "when all the moisture is depleted".

3. Line 55+2: Onset of moist convective instability has no meaning. Do you mean to say the onset or convection or the threshold for the release of convective instability --- an example is "sampling" parcels of air that have enough energy (buoyancy) to overcome the CIN energy barrier, e.g. see

Mapes, B.E.: Convective inhibition, subgridscale triggering energy, and “stratiform instability” in a toy tropical wave model. J. Atmos. Sci. 57, 1515–1535 (2000)

Majda, A., and B. Khouider, 2002: Stochastic and mesoscopic models for tropical convection. Proc *Natl. Acad. Sci. USA*, **99**, 1123–1128.

3. Line 60: The main purpose instead of the main result. A definition is not a result.

4. Line 60: convergence to what?

5. Line 61: Spikes at infinity means here: do you mean that sigma(t) become a Dirac delta distribution, i.e, the spikes in sigma are infinite? Also, this view is not consistent with Eq 1 where the the values of sigma are either 0 or 1 not zero or infinity. This is very confusing as to what exactly all this means!

6. Line 60+2: whereas is one word.

7. Equation 2: Eq 2: should the second value of sigma^epsilon be r over epsilon?

8. Line 105-1: These are the duration times for dry and rain events, respectively. Add respectively.

9. Line 110: Should the math expression at the end be t = T_1 instead of t > T_1

10. Line 125-1: to zero instead of at zero.

11. Figure 2: Some labeling of some sort should be added to panel d to illustrate the fact sigma is singular, that the spikes are infinite.

12. Equation 8: sigma^epsilon = 1 —-> sigma^epsilon = r/epsilon; Should one of the D_0's be D_1 instead?

13. Line 165+1: Having both the subsrcipt/assignment notations q=0 and q=b and the Dirac deltas (delta(q) and delta(q-b) at the end of the rho_0 and rho_1 equations in (10) and (11) is an overkill. One of the two should suffice.

14. Line 175, end of paragraph: I agree with the authors that the equations in (10), (11) and (12) are both unusual and interesting. However, providing a simple reference for their justification is suboptimal. It will be helpful for the readers if some more discussion is provided, especially in terms of why and/or how the singular terms are obtained. Readers who do not have access or do have the time to read that reference should be given enough information to be able accept/trust these equations. Also there are a lot of similarities and also discrepancies between (10) versus (11) and (12a) versus (12b). If I am reading the equations correctly, the singular terms introduce coupling between the two distribution at q=0 and at q=b. However, at rho_1 =0 at q=0 and rho_0=0 at q=b may render these coupling terms obsolete, especially if their 1st and second derivatives follow suit, which is likely the cases if the distributions are smooth enough!

15. Eqn 15-16: It is a bit weird that the finite epsilon Fokker-Plank equations in (10) and (11) are defined for q in (-infinity, + infinity) but the limiting equations in (15) are restricted to (-infinity, b)!!? Some explanation/reconsideration is warranted.

16. Eqns 19-20: I am not sure I understand the goal nor the effectiveness of asymptotic expansion ansatz. Obviously the nondim parameter in Eq. 19 is epsilon^2 but the ansatz stops short at epsilon level. I am not an expert in asymptotic expansion and I give the authors the benefit of the doubt that what they are doing is most likely correct but I think they owe the reader some motivation about this ansatz. Maybe a better expansion should be in terms of even powers of epsilon only. That way the repetition in (20a) and (20b) wont happen! Without mentioning anything about the higher order terms, O(epsilon^2) there is no guarantee that there isn't secular growth.

17. Line 195+1: Why O(epsilon) and not simply epsilon as it is initially set in the definition of the two regions above?

18: Lemma 38 (by the way why this is called lemma 38, this is the only lemma every written in the paper, did I miss something? Same applies for the two theorems.)

This lemma should be reworded so that the main result is the inequality-upper bound of the probability distribution as provided. The fact that it decays exponentially as N tends to infinity follows immediately --as a consequence.

19. Line 270: Should the equation be sigma^epsilon = r/epsilon?