

Dear Drs Feng Liu and Xin Li,

I have received two reviews of the new version of your paper. The referees are the same as those of the previous version. In particular, referee 2, who has again let his name known, is Prof. P. J. van Leeuwen.

Both referees consider that your paper has been improved, and that the mathematical derivations are now clear. At the same time, both write that they still do not fully understand the logic and the significance of your work. Referee 1 writes *While the new version is much improved, I still can't say that I understand it completely* and adds *I think that, at this point, it's just a matter of wording*. Referee 2 is in a sense more critical and writes *I don't understand the stochastic equation in scale space, neither where it comes from nor how it helps solve the representation error problem*. Both of them ask for a major revision.

Referee 1's most important comments bear on the use you make of the Lebesgue measure. He/she mentions that the equality on l. 10, p. 9, is in contradiction with the definition of the scale you give on ll. 11-12 of p. 8. This may be only a question of notation or of misunderstanding from the part of the referee, but it has to be clarified.

Referee 2's comments are at a more fundamental level. The most important ones are those relative to pp. 11-13 of the paper. They develop his general comment above on the significance of the stochastic equation in scale space.

Thinking of the general significance of your paper, I actually think as Editor that it would be useful to add a simple illustrative example, which would show explicitly how considering a stochastic scale transformation can impact the assimilation and the probability distribution that it produces. You have removed the section dealing with the Radiative Transfer Equation (RTE) which was included in the previous version of your paper, considering it was not closely tied to the other sections of the paper. I do not think it was explicit enough to show the impact on assimilation of a stochastic scale transformation, but it could possibly be used, with appropriate modifications, for that purpose. Without making it a condition for acceptance of the paper, I think an appropriate illustrative example, whether based on the RTE or not, would make the paper more understandable.

In addition to their basic comments, both referees also make a number of suggestions for editing (the first five comments of referee 1, and all comments of referee 2 down to p. 11, plus a few others). I myself add below a number of suggestions for corrections.

1. P. 5, l. 14, and p. 8, l. 5. I suggest to add indices i as follows

$$A_i = [x: a_{k,i} \leq x_k \leq b_{k,i}, k = \dots]$$

2. P. 6, l. 17. I presume you mean

$$\forall t_1 > s_1 \geq t_2 > s_2, \text{ the increments } W(t_1) - W(s_1) \text{ and } W(t_2) - W(s_2) \text{ are independent.}$$

3. P. 8, l. 15, ... *a unit interval* ... $\rightarrow$... *the unit square* ...

4. Probabilities are denoted $p()$ in some places (eq. 8, p. 10 for instance), and $P()$ in other places (p. 11, ll. 2-4). Please use consistent notations.

Please revise your paper according to the comments and suggestions of the referees, as well as to my own ones. And please give a point-to-point response to all of these comments and suggestions. Should you disagree with one particular comment, or decide not to follow a particular suggestion, please state precisely your reasons for that. The revised manuscript will be submitted to two referees, who may, or may not, be those of your former versions. Submission of a revised manuscript does not necessarily ensure publication in *NPG*.

I thank you again for having submitted your paper to *Nonlinear Processes in Geophysics*, and look forward to receiving a new version.

With regards,

Olivier Talagrand

Editor

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