Reply to the Editor, 1st comment

As editor, I however consider one point needs clarification before the paper can be published. It is the experimental set-up of subsection 4.2 (Non-linear example). You write that you have used the shallow-water equations (p. 14, l. 8). What is the domain for the space variable x (from what I understand, it cannot be the set , which is a range of possible values for the initial surface elevation h0(x)) ? And what is the connection between the field h0(x) and the scalar random variable for which you want to determine a probability distribution ? Maybe I have misunderstood something, but that point must be clarified.

This point should be now clear, according to the 2nd comment. At the beginning of Section 4.2 we state that the framework is the same as the first test-case and we refer to 4.1 for details, where the reader can find, e.g., the definition $\Omega = [0, 1]$ and other information about the state variable.

- Both notations $\mathcal{L}^2$ and L^2 seem to be used indifferently (see, e.g., ll. 22 and 27, p. 4). Use consistent notations.

Ok, done.

- Caption of Fig. 3, l. 3, outputs of the model fields at final time.

Ok, done.

Reply to the Editor, 2nd comment

Je viens de t'envoyer mes Editor's comments sur le papier Feyeux et al. Je me doutais bien qu'il y avait quelque chose que j'avais mal compris. Je comprends maintenant que les figures 5 et 6 representent bien des champs h(x), et non des distributions de probabilite. Peut-etre faut-il que ce soit dit un peu plus clairement.

There was a mistake in the figure, we used rho instead of h. We corrected this and named the variable explicitly in the text.

J'ai fondamentalement ete trompe par le fait que vous vous restreignez a des fonctions postives (eq. 4), ce qui est tres limitatif pour un un champ physique. Je suppose d'ailleurs que la distance de Wasserstein n'est definie que pour des champs positifs, ce qui necessite de considerer des champs ayant une borne inferieure stricte. Je suggere que vous le mentionniez.

Ok, done

Enfin, je suggere que vous evitiez l'utilisation du mot 'gaussien', qui ne peut guere dans l'esprit de beaucoup etre associe aux variations spatiales d'un champ physique.

We replaced gaussian by "localised mass function" where we could

Reply to the 2nd Referee

Two minor typos in the introduction that you may pass on to the authors: Page 3, line 11: Bonneel et al. (2011) should use called by a citep. Page 3, line 26: "lays" should be "lies"

Corrected, thank you.