

# ***Interactive comment on “Parameter space Kalman smoothers for multi-decadal climate analysis in high resolution coupled Global Circulation Models” by Javier García-Pintado and André Paul***

**Anonymous Referee #2**

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# Review of “Parameter space Kalman smoothers for multi-decadal climate analysis in high resolution coupled Global Circulation Models”

by Javier García-Pintado and André Paul

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This is a very interesting study, supported by a substantial amount of work, both theoretical and numerical. The theoretical and experimental parts are well balanced; the experiments are well chosen and interesting. The manuscript certainly should be considered for publication. Nevertheless, I believe it has a few flaws and requires significant improvements before being acceptable for publication.

In my opinion, the main issues are:

1. The manuscript is too long. Several discussions are unnecessary and frankly a bit wordy, especially in the introductory parts.
2. There are quite a few typos that need to be corrected.
3. I believe that there is no need to give these methods new names just because they are applied to parameters. But this is certainly up to the authors.

4. Please avoid the use of the term "adjoint method" which is – as of today – rarely used, ill-defined or at best does not precisely correspond to 4D-Var. See comments below.
5. The synthetic experiments with the CESM is a great piece of work, but unfortunately quite inconclusive. This is unexpected since we could have hoped for clear and neat results with such experiments. Tables 4 and 5 point to uncleared problems in the experiments. It is nice to use an ETKF in conjunction with a Gaussian anamorphosis. However, if the outcome is inconclusive (is it?), then it casts doubts on the interest of such test, or more likely on the implementation of the method (bugs?). See additional comments below (sorry for the redundancy).
6. It would be worth introducing in the CESM synthetic experiment some additional model error that you do not control in order to check how the methods are compensating for this error. This would be realistic and convincing.
7. Why is the data assimilation code not available? I thought it would be mandatory to do so for GMD, is it?

List of remarks and suggestions, some pertaining to the main criticisms:

1. page 1, l.4-5: "In a model framework where we assume that model dynamic parameters account for (nearly) all forecast errors at observation times,": Right, but is this framework usually met?
2. page 1, 12: "are evaluated in numerical experiments": Are these twin/synthetic experiments? In other words do you use real observations or synthetic ones? It is necessary to mention it here in the abstract.
3. Page 1, l.14: "the pFKS obtains a cost function": This expression seems meaningless. Please rephrase.

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4. Page 1, l.14: the expression "adjoint method" should be avoided as it is not well defined.
5. Page 1, l.14-15: Frankly, the whole sentence "Firstly, with Ebm1D the pFKS...behaves slightly worse." is difficult to understand, especially in an abstract. (For me, the technical terms are not the problem, since I am fluent in them.)
6. Page 1, l.17: You have to explain in the abstract why you would use an ETKF with a Gaussian anamorphosis or not mention it at all.
7. Page 1, l.18: Having the lowest cost function value is rarely a criterion as it depends much on the prior used in the cost function.
8. Page 1, l.21: "The issue of fusing data into models arises in all scientific areas that enjoy a profusion of data.": Not really. This is specific to areas where costly models are used!
9. Page 1, l.23-24: "Such methods can be considered as an approach for interpolating or smoothing a data set in space and time where a model acts as a dynamical constraint (Evensen, 1994a)": I don't believe you should use such outdated comment, all the more since nowadays there is a general consensus on a Bayesian view on data assimilation/inverse problems.
10. Page 2, l.16-19: "Other geophysical applications share this relevance of model parameters on the assimilation problem, as the estimation of distributed parameters and state for multiphase flow in petroleum reservoirs (e.g.; Gu and Oliver, 2007; Oliver et al., 2011), or hydraulic tomography for groundwater applications (e.g.; Schöniger et al., 2012).": You should mention atmospheric chemistry first, all the more since it quite close to climate (e.g., Bocquet et al., 2015).

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11. Page 2, l.20: "A related issue is the enforcement of physically based conservation laws, which by default is not taken into account by (ensemble) Kalman filters." No! You are right in general, but all linear constraints are properly enforced. (Which is why the use of the EnKF is widely spread!)
12. Page 2, l.23: ";
13. Page 2, l.23: "confirming re-integration": This is unclear to me. Please clarify.
14. Page 2, l.30: "under the assumption the errors" → "under the assumption that the errors"
15. Page 3, l.3: "conduct" → "conducted"
16. Page 3, l.6: "in section ,": Section number is missing.
17. Page 3, l.9: "adjoint method": please avoid this expression. It does not correspond to anything rigorous.
18. Page 3, l.17: "opposed" → "as opposed"
19. Page 3, l.17-18: This is an outdated view. Today, it is considered a doable task to estimate uncertainty within a variational framework (this is actually operational at the ECMWF). Read for instance Bousserez et al. (2015).
20. Page 3, l.24-25: "Other than that the formulation is identical than it would be for the corresponding filtering versions.": unclear or awkward.
21. Page 3, l.31: Twin experiments? This should be mentioned here as well.
22. Page 4, l.2: "the not only" → "not only"

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23. Page 4, l.18-19: "We also assume that the model is weakly nonlinear, such that it can be linearized.": This is not a clear statement. Any smooth model (even very nonlinear ones!) can be linearised.
24. Page 4, l.20: "small": do you mean low-dimensional?
25. Page 4, l.28: "The problem is to fit three spatial dimensions in time.": the sentence is unclear. "in" → "and"?
26. Page 5, l.1: Assuming time-invariant system is very restrictive in climate models where most forcings are time-dependent. Please justify.
27. Page 5, l.6: "That is, that the system..." → "That is, the system..."
28. Page 5, line 23: "in 4D-Var then" → "in 4D-Var is then"?
29. Page 5, line 23: "non-linear" → "non-quadratic"
30. Page 7, line 6: "is the same that" → "is the same as"?
31. Page 7, line 7: "4D-Var, IKS" → "4D-Var, the IKS"
32. Page 7, Eq.(16) and around: Such an operator exists only if the observations are time-averaged values, right? In general observations will depend on the initial condition. This must be discussed (this is actually better discussed in the introduction!).
33. Page 7, line 8: What is a "quasi-equilibrium"?
34. Page 7, line 14: In my opinion, there is no need to introduce a new term. This is just an IKS in parameter space.

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35. Page 8, Eq.(17): This type of formulation is frequent in many areas of geosciences; there is no need to look as far as history matching in oil reservoir modelling. For instance, this is very often met in source/fluxes inverse problems in atmospheric chemistry.
36. Page 8, line 13: "While it": A typo?
37. Page 9: In my opinion the discussion on the computation of the sensitivities is not only convoluted but also not very useful. It is obvious to the reader (to me at least), that you will use finite-differences in the end. Essentially only the last paragraph of section 3.2 is needed.
38. Page 9, Eq.(22): The linearisation in parameter space should be carried out at the  $j$ -th estimate of the parameters, not the background parameters (except for  $j=1$ ). What you wrote is just an approximation, which would make the iterative approach not as accurate as expected. Please clarify.
39. Page 10, line 20: "Iterative linear methods" is awkward, even though I guess I understand what you mean.
40. Page 10, line 24: Parentheses are needed around Bell and Cathey (1993).
41. Page 10, beginning of section 3.3: I don't see the point in the discussion with the EnRML. You can probably do without it.
42. Page 11, Eq.(25): The notation is unclear (I understand but many colleagues would not) and should be made consistent with Eq.(22).
43. Page 11, line 22: Actually the use of the MDA trick is slightly different in Bocquet and Sakov (2014) than in Emerick and Reynolds (2013), because the weights are adjusted over several data assimilation cycles.

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44. Another reference relevant to your manuscript is a study of the iterative ensemble Kalman smoother applied to a joint state and parameters estimation problem (Bocquet and Sakov, 2013).
45. Page 12, line 21: "opposite to" → "as opposed to"
46. Page 13, line 21: The sentence is a bit ambiguous since the model integration is part of the analysis (and so-to-speak a part of the analysis!). Please reformulate.
47. Page 13, line 26: What is a "temporal solution"?
48. Page 13, line 27: "detect linearity assumption": This expression is unclear. Please rephrase.
49. Page 13, line 3: I am familiar with the Levenberg-Marquardt scheme(s) and I do not understand your sentence!
50. Page 13, line 12-14: You have to give more details of your implementation. First, I do not see why you would need localisation for the state variables, since you are not updating them. Second, it is well known that, without a few tweaks, one cannot update global parameters in a LETKF.
51. Page 14, line 21: "It is not standard, however, how the GA should be applied in the context of DA.": There have been reviews and papers about that; for instance Bertino et al. (2003), as you rightfully mentioned, but also Bocquet et al. (2010); and above all Simon and Bertino (2009) and Béal et al. (2010) who set the standard on this topic. As far as I can understand, you are using their method. Please amend.
52. Page 15, l.9: "adjoint method (4D-Var)" → "4D-Var (based on the adjoint)"

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53. Page 15, l.11, L.13, l.27: Please avoid the "adjoint method" expression which is really outdated, and not use in data assimilation study. Refer instead to 4D-Var or variational method, possibly mentioning the use of the adjoint model.
54. Page 15, l.24, "standard 4D-Var applications" → "standard in 4D-Var applications"?
55. Page 15, l.29-30: I do not understand the last sentence.
56. Page 15-16, section 4.1: Where did you describe the parameters and how many are they? This is absolutely key to the feasibility of the problem. There are tables; but the parameter should be more clearly discussed in the text.
57. Page 17, line 4: "Note the original" → "Note that the original"
58. Page 17-18, section 4.3: In this section, you keep referring to the "ajoint method". Please do not use this term. This is a loose term, used in a loose way which generates confusion. At best, it refers to the computation of the gradient via the adjoint model, and not to the optimisation method you actually imply. That is why it is not used in written texts of the data assimilation community. You even refers on page 18 to the "adjoint", a short-cut which definitely lacks rigour.
59. Page 18, lines 1-14: It seems that it all boils down to the presence or absence of a prior for the parameters. Isn't it? If this is so, then this discussion is not really focused on what it should be.
60. Page 20, line 9: "multi-component data assimilation": To the best of my knowledge/understanding, this is rather called "strongly coupled data assimilation".
61. Page 20-21: I would more precisely enumerate/list/discuss the control variables. For instance, at some point, clearly mention: "Hence, our first control variable is..." etc.

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62. Page 23, line 26: "that is could be" → "that it could be"
63. Page 22-24, section 5.5: The results are not very enlightening. This is frustrating since we were expecting clear and neat results in such a controlled synthetic experiment. It might point to problems with these experiments (bugs?, too weak sensitivities, hence bad conditioning?). The reduction of uncertainty, as seen in table 5, does not seem consistent (too high) with the estimates reported in table 4 compared to the truth. This is worrisome.
64. Page 25, line 16: "and the trust one" → "and the true one"?
65. Page 25, line 25-26: "The estimation of the flux correction in our example has not succeed for the pFKS.": I do not understand the sentence. Please rephrase.
66. Page 26, line 9: "(or adjoint method)": no, rigorously, 4D-Var is not and should not be called the "adjoint method".
67. Page 29, line 7-8: I am surprised that you do not make your data assimilation codes available. I thought this was a mandatory rule for a potential GMD publication.

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