Author's response to referee review

The manuscript presents a novel deterministic ensemble data assimilation method, the GHOSH
filter, together with a corresponding high-order sampling strategy. The central idea is to achieve
approximation order higher than 2 along the leading uncertainty directions while retaining the same ensemble size and the same asymptotic computational complexity as standard second-
order deterministic filters. The method is formulated in detail and evaluated against SEIK in an extensive set of Lorenz–96 twin experiments. Overall, I find the manuscript technically interesting and substantially improved, and I support publication after minor revision.
My remaining comments are mainly aimed at improving clarity and avoiding possible over-interpretation.

We thank the reviewer for the positive general comment about the manuscript. All the specific comments have been addressed and our answers are listed hereafter.

1. Please clarify more explicitly that the validation in this Part 1 paper is limited to the Lorenz–
96 setting. The manuscript already states that a companion paper addresses a more realistic marine biogeochemical application; this is mainly a framing issue, not a weakness in the present study.

To address this comment, we changed line 657 from:
"The feasibility of the GHOSH filter in a realistic geophysical application has also been assessed in a companion paper (Spada et al., 2024) by testing a 3D parallel implementation of a Mediterranean physical-biogeochemical model."
to:
"In this work the validation and the implementation of the GHOSH filter is limited to the Lorenz96 idealized setting, while the feasibility in a realistic geophysical application has been assessed in a companion paper (Spada et al., 2024) by testing a 3D parallel implementation of a Mediterranean physical-biogeochemical model."

2. It would help to add a bit more information on what "order" means in this context. The manuscript defines order carefully, but this terminology may not be immediately transparent to all readers. A brief sentence early in the introduction noting that this refers to polynomial exactness or moment matching, rather than time-integration order or convergence order in ensemble size, would improve readability.

We clarified the concept of order in the introduction by changing the text at line 31 from:
"Common examples of deterministic EnKF are SEIK (Pham, 2001) and ETKF (Bishop et al., 2001). Extending the definition of second-order exact methods in \citet{pham2001}, we want to introduce here the concept of *polynomial order of approximation* of the filter…"
to:
"Common examples of deterministic EnKF are SEIK (Pham, 2001) and ETKF (Bishop et al., 2001). According to Pham (2001), these are second-order exact methods, where the term

order refers to polynomial exactness (in contrast with the order convergence in ensemble size in Monte Carlo stochastic methods or the time-integration order in numerical methods). We want to extend this concept by introducing here the notion of *polynomial order of approximation* of the filter…"

3. It would be helpful to state more explicitly that, although GHOSH is formulated as a hybrid method through the inclusion of the parametric covariance term Qi , the Lorenz–96 experiments are performed with Qi = 0 in order to keep the comparison with SEIK as direct as possible. This is a reasonable design choice, but a brief clarification in the abstract or conclusions would help readers understand that Part 1 mainly validates the high-order sampling strategy rather than the full hybrid formulation.

We improved our conclusions to avoid possible over-interpretation by changing line 659 from:
"The improvements of the GHOSH filter with respect to the SEIK filter are demonstrated in an extensive set of twin experiments based on the Lorenz96 model."
to:
"The improvements of the GHOSH filter (in its non-hybrid version) with respect to the SEIK filter are demonstrated in an extensive set of twin experiments based on the Lorenz96 model."

4. A discussion of the scaling matrix Λi is useful and could perhaps be brought slightly earlier in the manuscript. Since Λi determines the principal directions along which the higher-order treatment is applied, it is an important practical ingredient of the method. While Λi = I is natural in the Lorenz–96 setting, the manuscript rightly notes that its choice may require more careful design in realistic applications.

An introductory discussion on Λi has been added to line 203, changing the text from:
"In relatively simple applications, Λi can be the identity matrix, but in most scenarios, Λi needs to be properly designed (see Section 6 and Spada et al., 2024)."
to:
"In relatively simple applications, Λi can be the identity matrix, but in most scenarios, Λi needs to be properly designed. For example, if different variables represent non-comparable quantities, it is common to standardize such variables by dividing by their standard deviation. In the formalism of equation (7), this is equivalent to defining Λi as the diagonal matrix of the uncertainty variance (i.e., the square norm of the rows of Li). Further discussion on Λi can be found in Section 6 and Part 2 (Spada et al., 2024)."

5. A slightly clearer positioning relative to related deterministic nonlinear ensemble methods would help readability. The paper already cites relevant literature, including ETKF-type methods and related nonlinear ensemble approaches. A short early paragraph clarifying how GHOSH differs conceptually from these nearby approaches would help readers. I do not think additional experiments are necessary for this point.

To help clarifying how GHOSH differs from other approaches, we added after line 60:
"In this sense, our approach presents the same computational complexity and number of ensemble members as existing deterministic ensemble approaches (e.g., SEIK or ETKF) but exploits the advantage of a higher order only where the approximation errors are larger."

6. The manuscript is generally readable, but some sections are rather dense, especially Sections
2 and 3. A compact notation table would help readers keep track of r, s, h, Li , Λi, Ci , W , T , Qi, and the various forecast and analysis quantities.

We added the suggested table to help readability.

7. Please use standard mathematical notation for Gaussian random variables and densities, for
example in Eqs. (1), (A6), and (A7). As written, N (z; 0, Is) is used inside integrals as a density, while the text refers to it as a distribution. It would improve clarity to distinguish explicitly between Z ~ N (0, Is) and the corresponding density, e.g. φs(z; 0, Is) or f (z).

We thank the reviewer for pointing out this misleading notation. We changed equations (1), (A6) and (A7) accordingly.

8. In l. 122 the authors state that “this approach is beneficial independently of the uncertainty pdf.” This is reasonable at the level of the general moment-matching argument, but the specific Gauss–Hermite construction is tied to the Gaussian case. The manuscript should be a bit more careful here. In particular, Appendix A would benefit from a clearer separation between:
(i) the general statement that exactness for polynomial forecasts follows from moment matching up to order h;
(ii) the fact that this statement is not restricted to Gaussian priors; and
(iii) the more specific point that Gauss–Hermite nodes and weights arise when the chosen reference prior is Gaussian.
A short clarification would likely suffice.

We slightly modified text in Appendix A in order to clarify that the Gaussian prior is not a primary requirement in the high-order sampling. For example, after line 698, we added: “However, it is worth noting that the following is independent of the chosen pdf (as long as it is rotational invariant after normalization and de-correlation, which is later required to avoid solving the moment-matching system at every resampling).”

9. Equation (A1) should be written more carefully. Since j1, . . . , jξ are summed independently,
the same monomial is counted multiple times, so the coefficients are not uniquely defined unless symmetry under index permutations is imposed. I suggest either stating this symmetry
explicitly or rewriting the sum in a non-redundant form. Also, the ξ = 0 term should be written explicitly as the constant term.

We included the constant term in (A1) and (A2). Moreover, we totally agree with the reviewer that coefficients are not uniquely defined. However, the bijection between coefficients and polynomial functions is not needed here, since (A2) holds even if the polynomial function in (A1) is not uniquely determined. Thus, we prefer to keep the current expressions in order to lighten an already overloaded notation.

10. Please correct the typo “Hybryd” to “Hybrid.”

The typo has been corrected.

Additional detailed comments are included in the annotated manuscript.

All the suggested comments have been included in the revised manuscript. We thank the reviewer for the effort and the very valuable help.