

***Interactive comment on “Methods of investigating forecast error sensitivity to ensemble size in a limited-area convection-permitting ensemble” by Ross Noel Bannister et al.***

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# Responses to reviewers for, “Methods of investigating forecast error sensitivity to ensemble size in a limited-area convection-permitting ensemble” (gmd-2017-260)

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This document is our general (undetailed) response to the main criticisms of reviewer 1. A second (more detailed) part will follow should the editor allow the paper to be revised with a reasonable chance of acceptance in GMD.

## Response to reviewer 1

We would like to thank reviewer 1 for his/her comments and criticism of our manuscript sent to GMD.

We are disappointed and puzzled to see the reviewer’s opinion on this work, especially his/her belief there is not enough original work in the paper (para. 4 of reviewer 1’s report), and their suspicion that some of the analysis is flawed (para. 3). We hope to make a case that the paper does actually have significant and interesting new results. Of course virtually all scientific work has limitations, and we acknowledge ours, but we strongly believe that the work presented is useful and has enough new content, as

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explained below. Unfortunately the reviewer's comments appear to give only a one-sided view.

1. Single case study. One of the comments concerns the presence of a only single case study, and this limitation is indeed highlighted by the authors (para. 2 of reviewer's report). It does mean that the specific results do not necessarily represent firm conclusions, but it certainly does not mean that the results are not useful, e.g. they are probably representative of the particular weather regime studied (especially as the case comprised several days' data). Menetrier et al. (2014), which the reviewer cites (para. 3), is also based on a single case, demonstrating that this need not be a show-stopper. The 'single case' limitation (which was outside of our control) is the reason why we have emphasised the methodology, rather than the specific results, and to seek publication in this particular journal. Indeed, parts of the methodology could be adopted by others studying other systems.
2. Originality. We believe that the reviewer's opinion on originality is not a fair judgment. The new aspects of the paper are not highlighted in the reviewer's report, even though they are present in the paper. A version of this manuscript was originally sent to another leading journal, and the only reason why it was not accepted there was because of the single case study limitation, which should not be a problem given the scope of GMD. Importantly in that submission, both reviewers' comments were otherwise very positive about the work (it was described as "state of the art", and a "significant contribution to the field"). Of course, that was the outcome of a separate editorial process, but it does serve to highlight that it is possible, as in the present case, to get a distorted view of a piece of work from a small number of reviewers. It is not clear to us why the present reviewer should hold their opinion, but it might be the case that he/she was expecting that every figure should represent a brand new diagnostic. It is usual for a study to use a range of new *and* standard diagnostics as part of an overall picture. Many of the

figures in the paper are in fact, we believe, based on new developments in the field of ensemble forecasting and/or data assimilation, which the reviewer has not discussed in his/her report, and this may wrongly lead the editor to believe that the paper contains no or little original content. To emphasise our point, these are areas of the manuscript that we think especially have not been explored before in the context of ensemble forecasting/DA:

- (a) The way that a large ensemble can be generated from an existing smaller ensemble (Fig. 2, Sect. 3.2). This technique will almost certainly be of interest to other research groups who would like to extend their ensemble systems.
- (b) The linear independence tests to show that the members do develop linear independence (Fig. 3, Sect. 3.3). This is a simple but essential diagnostic to confirm the usefulness of the above method.
- (c) Study of the kinetic spectrum in an ensemble context (Figs. 9 and 10, Sect. 6). This suggests how errors in kinetic energy of a finite ensemble change as a function of scale – very important information to have when designing and interpreting ensemble data assimilation systems.
- (d) Study of the form of the correlation functions of variance errors, in particular finding an excellent fit to an exponential form (Figs. 13 and 14, Sect. 7.3), and how this could be used to generate variance fields that have a prescribed form of sampling error characteristic of a finite ensemble (Eq. 7). The exponential fit makes the length-scale analysis different from that covered previously, e.g. Menetrier et al. (2014), Pannekoucke et al. (2008), and Raynaud and Pannekoucke (2012/3), which looked at parabolic or Gaussian forms (7th minor comment or reviewer's report).
- (e) A potential new test of whether an ensemble is large enough to meaningfully neglect sampling error (Sect. 7.2, and Fig. 13).

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- (f) Analysis of the errors in the sub-sampling for many diagnostics (e.g. the fit to the exponential, Fig. 14).
  - (g) The application of the above and standard diagnostics to the high-resolution Met Office Unified Model.
3. **Validity.** The reviewer has questioned the validity of the assumption made in the paper that the large (93 member) ensemble has negligible sampling error (main comment 2). This is indeed an important issue, and is part of what makes this manuscript interesting scientifically. We have done what is usual in science: we have made an assumption, used it in an attempt to learn about a system, and then discussed how valid the assumption is. In our particular application there is evidence that this assumption may not be met (see discussion after Eq. 6), but that is still very useful information. This would be an unfair basis on which to reject the manuscript.

The reviewer's report contains some minor and detailed aspects which we can address in a way to improve the paper, should the manuscript be taken further.

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