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Interactive comment on "Ensemble-based statistical interpolation with Gaussian anamorphosis for the spatial analysis of precipitation" *by* Cristian Lussana et al.

Anonymous Referee #2

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My field of expertise covers data assimilation and statistical analysis, but not meteorology, so my review will not cover the meteorological aspects of the work neither the realism of the fields nor bibliographic references within weather forecast post-processing.

The manuscript adresses the important problem of probabilistic analysis of precipitations and presents a new method (at least new to my knowledge). The study has borrowed methods and terminology from the ensemble data assimilation literature to perform post-processing of EPS outputs. The main originality of the manuscript seems to lie on the use of EPS output as background covariance for the statistical interpolation, which is precisely the modus operandi of the Ensemble Kalman Filter, except

without the need to re-use the analysis as initial condition for the following forecast. Since the ensemble data assimilation framework is also using some concepts from the statistical interpolation literature, the terminology can be confusing at times. I have tried to correct that below. Rather than assimilating data, method is more intended as a "conditional simulation" in the geostatistical literature, it is performing a Kriging of the mean of the Gaussian transformed values and then retrieves the probability distribution of precipitation at every location using a predefined Gamma distribution. I wish such a summary of the method had been spelled out more clearly in the article, however.

The manuscript is otherwise well articulated, moving logically from the algebraic formalism to an idealised case and a real application over Southern Norway. The method seems to work very well according to cross-validation results against independent weather stations data, which is reassuring, but not a proof that the method is flawless.

My main weaknesses of the manuscript are the following (more details below):

- The paper is introducing a new method, but does not give a point of comparison to any pre-existing methods other than references to the data assimilation literature. The literature review of post-processing methods seems to be missing (and I am not an expert of it). As the paper stands, the most natural comparison should be static versus flow-dependent covariances, but that is not done in the paper. One extra experiment with alpha equals to zero would seem necessary to compare the new method to a reference method, albeit very basic.

- The benefits of flow-dependent covariances are not exhibited other than showing their existence. The ensemble does hold information about the physics of precipitation that is absent from the stationary static covariance and I would expect some meteorological interpretation of the ensemble correlation.

- The algorithm description is at times unclear, in particular the adjustment of the Gaussian anamorphosis and the need for the alpha stabilisation parameter. - The testing focuses excessively on two values of the alpha and epsilon parameters but leaves other choices - however critical - under diffuse justifications (the D radius, the dilemma between Gaussian and exponential covariances). The authors have generally a hard time in comparing their results, possibly assuming that excessively solid evidence is needed to choose between two options. This causes an imbalance in the article between the testing of alpha and epsilon versus all of the other choices that felt more interesting to me.

- The choice of the Gamma distribution could have been supported by a histogram of observations.

- The choice of error covariances could have been settled by plotting the experimental variogram of the errors against the densest observation source (the radar). The shape of the variogram at its origin is the critical choice for kriging and should be adjusted manually against whichever of the Gaussian or exponential funcion that fits best. Long ranges of the variogram can be ignored for the sake of this study. The adjustment of the variogram will not only reveal which of the two functions is most representative of precipitation but will also deliver the range parameter which can justify the choice of the reference length scales L and D.

- The Working Assumptions are expressed as vague principles but do not determine the algorithmic choices made thereafter. Reformulating them as statistical hypotheses (temporal independence, spatial scales, monotonicity) would make an easier read.

- The paper can be shortened in several places.

Using the Gaussian covariance function for the unpredicted scales Gamma_u is risky in cases where Pf is equal to zero (no rain the background EPS, but rain in observations). This is not visible in the toy experiment (Figure 2b) because the test is only considering interpolation cases but not extrapolation (observations on one side only). The Gaussian covariance has indefinitely many zero derivatives at the origin, which imposes unrealistic smoothness constraints on the analysis and, as a side effect, causes an "over-confidence" in the analysis: underestimation of the analysis uncertainty and a

tendency to produce high and low values outside the range of observations. Figure 2 (b and d) show that the uncertainties are indeed much smaller with the Gaussian than the exponential covariance (in the "missed" case between points 250 and 300) but is not decisive because both experiments seem to cover well the observations. Looking at these results, I believe that the Gaussian covariance makes less sense qualitatively because the uncertainties are lower when the model predicts no rain at all than only a little rain. As if one should be more confident when the model is completely wrong (no rain) than when it does not predict the right quantity of rain.

If in addition the observation errors R had been zero, the authors would also have experienced problems with the inversion of the (S+R) matrix. The risks related to the use of a Gaussian covariance are described in Diamond and Armstrong (1984).

Overall I have the impression that the method presented in the manuscript is useful and will make an interesting contribution to the field of meteorology and possibly beyond.

Detailed comments:

- The abstract does not mention that the realistic examples are using data over Norway. This could be important to know for the readers.

- l49: The statement that the forecast is better than observations in the absence of observations seems odd to me.

- l81: I would refer to the Ensemble Kalman Filter rather than the Ensemble Optimal Interpolation because the latter uses ensemble members taken at different times, while ensemble members in your method are synchronous.

- Bertino et al. (2003) introduced the concept of Gaussian anamorphosis from geostatistics to data assimilation, but since you are not performing any data assimilation I would think that a general reference on geostatistics is more appropriate: see Chilès and Delfiner (2012), Chapter 6,

- l82: trans-Gaussian would deserve a definition, or to be called explicitly "transformed

Gaussian".

- l85: The authors seem to refer to the "static" covariance matrix rather than "scale". I suggest you call it "static" by opposition to the flow-dependent ensemble covariance term.

- l114: anisotropy and non-stationarity are two different topics. I agree that precipitation is non-stationary but invoking the non-stationarity of weather phenomena or simply the orography would have seemed a better explanation.

- l135: Specify the direction of the function: X is the Gaussian variable, not X tilde.

- l139: The fitting of the Gaussian anamorphosis is unclear: it is adjusted to "each ensemble member" but does not state which time period nor which geographical region. This is made further puzzling in Algorithm 1, as I read that the adjustment was part of the loop "for all grid points", which implies a local adjustment at each analysis time.

- l160: Include a reference to Table 1 somewhere in the first paragraph.

- Section 2.2.2: The working assumption are not mathematically well posed. They read more as general principles than working assumptions. WA3 could be rephrased as "R and Pf are proportional" for example. An assumption of locality is also missing together with WA5 on the temporal independence.

- l204: As above, "static" seems more appropriate than "scale" when you have other meanings for the word "scales" in the same paper.

- l248: The "stabilisation factor" alpha is poorly introduced (what is it suppose to stabilize?) and has no other effect than to increase the variance of the ensemble. I would therefore call it an inflation factor although it does not have the same effect as in an EnKF due to the absence of recursions. If the term "inflation" is also used in the literature related to ensemble post-processing, then I would suggest you call it "inflation factor".

- l258: The reasons for introducing alpha have not been enunciated before.

- Eq (13): please insist that the analysis is applied to the ensemble mean and not each member of the background ensemble. I have lost track of the definitions given a few pages earlier.

- l277: As above, refer to the EnKF rather than the EnOI here.

- l284: The authors should state more explicitly that the method "does nothing" in this case.

- l285: remove "direct" before "inverse" in the whole paragraph.

- Section 2.3 comes back to the anamorphosis and the Gamma distribution and it is not clear whether the optimization of the Gamma parameters are a repetition of what was done in l139 or if the back-transformation is using a different fit than the direct transformation in Eqs (1) and (2). Please rewrite this paragraph.

- Section 3.1.1: The section is very long and it would help if simple concepts as "false positive" and "a miss" were used instead of "an alternative truth" which does not convey appropriately that the authors are dealing with ensemble bias.

- l347: This procedure seems to be a convolution between the signal and a smoother random signal. This does not low-pass the signal as a moving average would do. Please clarify.

- l352: Between "200 u and 300 u": Figure 1b rather indicates 220 than 200.

- l360: Is "the multiplicative model" compatible with the assumption of a Gamma distribution?

- l370: It seems that the length scale of the unresolved covariance is dependent on the measurement network but not the physics of precipitation. This comes as a surprise and should have been clarified upfront as a working assumption in Section 2.2.

- l387: These three references seem to imply that the exponential covariance is more common than the Gaussian for precipitation.

- l394: Variations in the scale matrix -> the choice of the static covariance matrix.

- l401: remove "almost" since Pf is equal to zero.

- l402-404: Keep this remark until after Figure 4 has been introduced.

- l428: Why restraining yourselves with a qualitative judgement when you could have computed a CPRS or a simple ratio of errors versus estimator uncertainty?

- l436-442: I believe this part can be shortened.

- Figures 6 and 7 should include the Sogn og Fjordane box.

- l.495 Where observations are absent (gray shaded areas), the analysis differs from the background. Please comment on whether this change is realistic or not.

- l508: Kriging does induce some systematic smoothing, the authors could have made a note of that when discussing Eq (13). Smoothing can be avoided by other methods in the family of conditional simulations.

- l 522: The extrapolated values are not commented, are these expected or not?

- l531: This is a very long sentence to say "certainly no rain".

- l532-534: This passage does not seem to be connected with the rest of the text, please make a point or remove it.

- l543: Which "EnSI-GAP settings"? There are several of them.

- l. 549: Is it ok that the analysis rules out the precipitation at point B where no observations are present?

- l551-555: As above, the take-home message of these numbers is not obvious, please shorten the passage or remove.

- l560: I have been confused by the contradiction between (i) and (ii). Please rephrase.

- Section 3.2: the case "alpha=0.1" can be called a case of "deflation" by opposition to "inflation".

- Figures 12 to 14 seem to give the same message than Figure 11. I believe that they can be removed together with the related text without loss to the argument.

- l679: The justification of the choice of D value should come upfront in Section 2 rather than in the discussion.

- l685: Why "assuming" that the background ensemble is more likely to overestimate the spread wile you could have verified it with the data at hand?

- Algorithm 1, under "Require": include the calculation of the ensemble mean and the predefined anamorphosis function.

- Figure 1: vertically shade areas of "false alarms" and "miss".

- Figure 6: delineate the boundary of the grey area in panel (c) to highlight what the analysis does in the absence of observations. Do that as well in Figure 8.

- Figure 8: annotate the points "A" and "B" on the figure.

- Figure 10: The flow-dependence of the correlations is visible, but does not speak much for itself. The ensemble may contain some information about the orography for example and it could be interesting to include the elevations with a few isolines.

- Figure 11: An integrated measure of the goodness-of-fit can be included in the legend of each panel.

Typos and minor issues

- l197: tenths -> tens

- l395: remove "the" before "higher uncertainties".

- Use "anamorphosis" instead of "anamorphism". I don't think "anamorphism" is grammatically wrong but it is less usual in the literature.

- l499: "clearly" can be removed. Let the reader judge the quality of the figure.

- l505: "it is evident a sharp gradient..." -> "a sharp gradient [...] is evident".

- l539: "sort of outliers" -> "outliers".

- l617: "much accurate" -> "much more accurate"

- l618: "those" -> "that"

References:

Chilès, J.P. and P. Delfiner (2012) Geostatistics: Modeling Spatial Uncertainty. Wiley and sons. https://onlinelibrary.wiley.com/doi/book/10.1002/9781118136188

Diamond, P., and Armstrong, M. (1984), "Robustness of Variograms and Conditioning of Kriging Matrices," Mathematical Geology, 16, 809–822.

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