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Interactive comment on "Dependence of Predictability of Precipitation in the Northwestern Mediterranean Coastal Region on the Strength of Synoptic Control" *by* Christian Keil et al.

Anonymous Referee #2

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General Comment

The paper discuss the performance of the AROME-EPS ensemble forecast for precipitation during the SOP of the Hymex Project, in dependency of the predictability of the events, as quantified by the convective adjustment timescale. The argument is scientifically very relevant, addressing the convective-scale predictability of the precipitation for an area interested by severe weather events. The work is well structured and meaningful, and clearly presented. However, I am not convinced of some conclusions, due to the verification process. I think there are some weaknesses in the verification interpretation, which hamper the conclusions to be drawn. Therefore, I recommend to

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Discussion paper

address some issues (described below), in particular in Section 5, before publishing the work.

Detailed Comments

Section 2.3 – Please add a reference for the Relative Operating Characteristics ROC and the reliability diagram. Though a description of these well know tool is not needed in the paper, not all the readers may be familiar with their definition. Figure 2: The thin lines are for me unreadable, and particularly their colour. Is it possible to increase the thickness?

Page 8: A small typo: "southeastern foothills if the Massif Central" should be "of"

Section 4 – In figure 6 also the spread of the ensemble is shown, by showing the area average precipitation of the members. There is evident that in the first case the spread is relatively "high", the members being quite different, as also noticed in the discussion of figure 7(b). In the second case, the spread of the precipitation is low, only 2 members having almost no rain, while all the others are close to each other. However, the first case is a predictably one, and the second a less predictable one. The spread, in the case of the precipitation, is not a good indicator, because it depends too much on the amount of precipitation itself: the first case is a predictable one, even if the members are different, because the rain is intense and the differences do not affect the "general performance" of the forecast. I think that, if the ensemble spread is shown, these considerations have to be made explicitly, otherwise the reader may receive a wrong message about the predictability. On top, the spread may be low even when the case is not well predicted, in case the ensemble is overconfident, which seems to be the case of the second case. For this reason, it would be good to have also the average observed precipitation, in figure 6.

Figure 7 and related discussion: an overprediction over Genova is noted in the 6-h period. Is this an overprediction in absolute sense or a timing problem (e.g. heavy precipitation occurred over Genova in the successive 6 hours?) Figure 11: the mean of the RMS error of the ensemble members is shown and compared with the ensemble spread. Why is not shown instead the RMS error of the ensemble mean, which is the quantity which should be matched (statistically) by the spread? The chosen quantity is for sure higher than the other one, since the ensemble mean has (statistically) lower RMSE than all the members. Can you motivate a bit more the sentence (pag. 15): "The larger distance of the ROC curve points during strong control indicates the higher absolute spread when 3-hourly (and daily) precipitation accumulations are averaged over the entire SOP1."? Is this related to the point I raised about Section 4?

Page 15, about the sentence: "the forecast probabilities are consistently too large relative to the conditional observed relative frequencies. This is an indication of overforecasting equivalent to a wet bias.". The overforecasting in probability/frequency does not indicate a bias in the quantity, but in the probability. Therefore it does not indicate a wet bias, but an overconfidence of the ensemble. The members forecasting an event (e.g. 3mm/3h) are "too many" (therefore producing a too high probability of occurrence) with respect to the observed "probability" (which is the frequency) of occurrence of that event in the sample. I believe that the same overconfidence applies also to the dry areas (here you are considering only the wet areas, since you have a threshold >3mm/3h) and it is not related to an overestimation in the quantity itself. Page 17, from line 315 to the end of the Section. I am not convinced by the conclusions drawn here by the authors. "Taking this bias into account by using precipitation percentiles results in a superior spatial forecast quality during weakly forced regimes (Fig. 14b). Thus forecasting the location of heaviest precipitation in the afternoon (expressed by the 95th percentiles) is better during comparably quiescent synoptic-scale atmospheric conditions. This is at first sight an unexpected and surprising result." It is not the scattered nature of the weakly-forced precipitation field, when the isolated intense precipitation spots are selected, which gives an impression of skill by upscaling, just because somewhere a spot of precipitation is always available? I do not think that it is possible to conclude that there is a higher quality in the spatial forecast in case of weakly-forced cases based on this result with the FSS. I agree that orography "keeps" the precipitation in place in case of convection, but I am not sure that with this increase of FSS for the 95th percentile can be a prove of skill.

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