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Atmospheric Chemistry and Physics Discussions

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 #1

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This is an interesting paper. The paper aims to use the convective adjustment timescale to examine the synopic control on convection in the Meditteranean region of France and Italy over a 2-month autumn period using the AROME convection-permitting ensemble, and hence determine the nature of the ensemble forecast predictability and performance in the different regimes.

The manuscript is largely clear and produces worthwhile results, but there are some aspects that need some further attention before it should be published. I'll first outline my main concerns before going into some other less crucial detail.

Main points:

1. You say that the domain should not have an area larger than 500x500km as recommended by Wernli and you do meet that criterion, as you say, but by having a domain 800km in the west-east direction are you not in danger of incorporating more than one larger-scale wave and therefore more than one regime (which you are trying to avoid)? Your third case (13th Oct) appears to have two areas of precipitation. One in the west that looks predictable and one further east that looks much less predictable. I wonder if you did your statistics for two overlapping domains each 500x300km whether you would get different results for that type of case and a better partitioning. Would it be possible to try that for at least that case? One of your main conclusions is that the strong forcing cases dominate, but is that partly because the domain is too extended and is always likely to capture a strongly forced event which will contribute the most to the timescale calculation?

2. Many times you say that the ensemble overpredicts the rain in the non-equilibrium cases, but I wonder how much of that is actually an artifact of the gauge interpolation missing rain than an over-prediction by the model. In a showery situation it is very likely the gauges will miss the heaviest rain cores, especially if locally focussed on hills where there are fewer gauges. A good test would be to take a model field, extract the values at the gauge locations and then do the interpolation. I would suspect you will get a lower domain-average value than the original field. Even if you don't try that out, it would still be worth mentioning in the article that a guage interpolation can miss rain when the rain coverage is low and has local spikes.

3. Some aspects of the methodolgy need a bit more clarity. You don't say why you choose a minimum rain amount of 3mm in 3h, which seems somewhat arbitary. A sentence or two about that would be helpful. I know you have a reference, but a few words would still be helpful. Linked to that, do you try to determine whether rain is convective or stratiform in nature? I assume you don't, but then there will sometimes be frontal rainbands that act to lower the timescale because rain occurs along with zero CAPE. You should at least mention this potential difficulty or explain why you

think it isn't a problem. I can see it making the convective timescale appear smaller especially going later into the year. Why do you choose 1mm/3h for the standard deviation, but 3mm/3h for the convective timescale calculation? What horizontal scale of Gaussian smoothing do you use? That could potentially have a singificant effect. If the smoothing is done over too large an area the precipitation threshold may not get exceeded anywhere in the scattered convection cases. On the other hand, some degree of smoothing will bring in more locations that may have high CAPE and that will affect (lengthen) the convective timescale. I take it, just for absolute clarity, you do not include any points with rain < 3mm/3h even if CAPE in non-zero?

4. To be honest I'm not sure about the value of some of the discussion of the individual cases It is useful to see the figures but there is a lot of descriptive text around them that isn't really adding much to the purpose of the paper, it's just describing where rain occurs in that event. A lot of readers will not know the location of regions in France, so it would be better to have some annotation on the figures to point to features instead. The key thing it seems is whether the rain is more or less widespread, and whether the members agree in say region "A" and region "B". Again, I'm not so convinced about the overestimation argument in weak control.

5. You should explain what you mean by the "ensemble mean FSS". Do you generate an ensemble mean precipitation field and then threshold for the FSS? That wouldn't be a good thing to do because you filter the true ensemble spread and change the frequency biases. Do you calculate the FSS for each member based on those thresholds and then take the average? Again that would not be the best thing to do because you might penalise ensemble spread as much as ensemble error. Do you threshold each member then take the ensemble mean of the binary probabilities and then apply the FSS? That would be the most sensible of those three options because it is evaluating the final probability field without clipping the distribution. Maybe you do something else? Definately it was good to choose the 95th percentile. Have you looked at other percentiles?

Other points:

1. You choose a threshold timescale of 3h, but then say it is different for summer and autumn (understandably). If you are most interested in partitioning out the days with the strongest and weakest synoptic control for evaluation could you just take the highest 30% and lowest 30% and then not have to worry about a timescale threshold. I'm not suggesting you do that here (as the results would be very similar), but it may help further studies of this sort.

2. When you talk about a "barrier" I assume you mean a stable layer? Sometimes storms form over mountains because of elevated heating and there isn't a clear inversion or organised storms form where there is an inversion but lifting mechanisms reduce it.

3. In some ways this seems to come down to wther the precipitation is contigouous or broken. I wonder if you were to classify that way whether you'd get something similar?

4. What would a graph of rain against Tau look like?

5. Figure 2 caption is hard to follow.

6. Might be interesting to know something about the spread of Tau and CAPE. Not suggesting you have to include that though.

7. You specify rank correlation values, but not actual correlation values. That leaves me a bit suspicious that they are not as good (closer to zero). Is that the case?

8. In line 163 you say "provides a better suitable measure" - but better than what?

9. For the indivuidual cases a few pressure contours would be informative to help set the context for the precipitation.

10. Presumably the skill-spread scores are picking up the bias, but also indicating potentially that there are too few members when evaluated at the grid scale, as well as saying the spread isn't sufficient.

11. I'm not sure it is a huge surprise that convection linked to mountains is more spatially predictable than convection that is mobile. I wonder what would happen if you just fixed a domain over the Alps and compared that with a flatter region for the convective timescale partitioning?

12. It might be worth also mentioning the paper by Flack et al Flack, D.L.A., Plant, R.S., Gray, S.L., Lean, H.W., Keil, C. and Craig, G.C. (2016), Characterisation of convective regimes over the British Isles. Q.J.R. Meteorol. Soc., 142: 1541-1553. doi:10.1002/qj.2758 This also references the papers you reference prior to 2016 along with Keil and Craig 2011, which you don't reference - which is a surprise.

There are some places where the text could be clearer (although in general it is well structured and readable), but it would probably be better to address these after dealing with the points above, which are going to involve changes in the text.

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