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Interactive comment on "Large impact of tiny model domain shifts for the Pentecost 2014 MCS over Germany" *by* Christian Barthlott and Andrew I. Barrett

Anonymous Referee #1

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This study examines simulations of a rather impactful MCS over Germany in 2014. The COSMO model at convection-allowing resolutions is used to simulate the MCS in a control run, and additional simulations move the model domain one grid point in eight cardinal directions. The predictability and forecast errors of the MCS are largely dependent on convection initiation in western France and subsequent propagation of the system over land, as well as the environment the system encounters during the day before impacting Germany. Substantial variability exists amongst the simulations, whereby some instances of the MCS-induced rainfall is forecast reasonably over Germany and other instances there is no precipitation in this region at all. It doesn't appear that systematic movement of the domain resulted in clustering of forecast errors either.

I do have some concerns regarding the motivation and methodology for this work. While the authors motivate the work as a worthwhile approach to improve convection-allowing model (CAM) forecasts, effectively creating sufficient spread across the forecast distribution, I struggle to see the benefit of this domain-shifting approach over other well-documented lateral boundary perturbation techniques (e.g., Torn et al. 2016), and I don't feel the author's sufficiently demonstrated this benefit in the introduction. Additionally, I believe the authors try to attribute the forecast variability to their methodology (i.e., domain shifting), but I do not feel there is substantial corroborating evidence to support this conclusion. For instance, the forecast variability and predictability could simply be a function of the dynamic sensitivity of the evolving forecast: MCSs developing along the coast of France are inherently less predictable. I have elaborated on these concerns below that should guide the authors in their manuscript revisions, outlined below in the "Major Comments" section. Other minor comments and technical suggestions follow the major comments.

Major Comments

1. My first and foremost major concern is the contribution of this work to the atmospheric science literature on MCSs, predictability, and forecast generation. I believe what the authors have described is merely a technique to perturb lateral boundary conditions, thereby producing spread in the initial states of the model simulations which filters into the forecasts of the MCS. If we assume the environment at the model boundaries is relatively homogeneous, at least within a few grid points which only amounts to < 10km, then what the authors have described here is essentially a 10-member ensemble forecast system with perturbed boundary conditions. This methodology is not in itself flawed by any means, but I do not believe it is innovative or new. See Torn et al. (2006), Gebhardt et al. (2011), and Romine et al. (2014) for other examples of boundary condition perturbation studies. Given this statement, however, I think the authors could easily address my concern by a number of avenues:

a. If the authors believe this truly is an innovative technique to generate CAM ensembles, they should either more succinctly clarify this in the introduction with references supporting this claim or demonstrate the methodology alongside some of the more traditional techniques (e.g., covariance perturbations) for this case study.

b. If the authors would still like to use the domain-shifting methodology to investigate the predictability of the MCS, I would caution attributing the methodology to why the MCS is inherently unpredictable. In order to scientifically attribute the poor forecast predictability to the domain-shifting methodology, substantially more analysis and simulations would need to be conducted. For instance, do you see the same poor predictability if the domain is moved 5, 10, or 20 grid points? What about if another perturbation technique is used? Can you reproduce the poor forecasts?

c. If the authors would rather focus on the predictability aspect of this event, I believe the authors could implement some other analysis techniques to derive some of the dynamic aspects for this case to complement what has been presented. Sensitivity approaches such as those demonstrated by Schumacher and Davis (2010) and Ancell and Hakim (2007) could be valuable additions to the analysis. I invite the authors to consult a number of papers that apply sensitivity analyses to convection-resolving forecasts as well: Bednarczyk and Ancell (2015), Hill et al. (2016), Limpert and Houston (2018), and Torn et al. (2016). Additionally, other aspects of predictability could be garnered through initializing ensemble forecasts at later times, which may answer the particular question of whether CI is the limiting factor of predictability.

2. A second concern I have is in the presentation of the forecast itself. There is no mention of the upper-level dynamics that could be supporting MCS development, particularly since the orientation of development and distribution of environmental parameters conducive to MCS propagation are misaligned from traditional understanding. For instance, the MCS propagation within a region of predominantly northwesterly or westerly surface winds, which would not advect the CAPE-rich air from the southeast. Typically, we would expect a convergence of moisture and higher theta-e air just ahead of the MCS, but this is not the case. Also, there is no mention as to what causes the

MCS to initiate so early in the day. My inclination from reading the forecast description is that the orientation of the longwave mid-tropospheric trough is supporting the traversal of short-wave troughs through western Europe. I suggest the authors add supporting evidence for how the MCS initiates, which could elucidate some other predictability elements that have not been considered, e.g. the position and placement of upper-level vorticity maxima.

3. Why is accumulated rainfall used as the sole metric of forecast evaluation? I would think observed radar reflectivity compared to simulated reflectivity would be a better metric for comparing model runs. Comparing reflectivity would better illuminate the intensity and structure of the MCS between observations and simulations; accumulated rainfall doesn't discriminate these differences well.

Minor Comments

Abstract

1. Why was the predictability low for the operational prediction systems? Do those systems parameterize convection or is it explicitly solved?

2. "However, the low predictability of the event was evident by the surprisingly large impact of tiny changes to the model domain": Is the argument there is low predictability because of dynamics or because of the model configuration? There appears to be two separate statements of predictability related to this event, but it is unclear what statements the authors really want to make. I'm assuming the main predictability element comes through the numerical (domain) aspect. Introduction

3. Lines 28-30: Were the German Weather Service operational models convection resolving? Is there any indication as to "why" the deterministic and ensemble systems failed to produce convection over Germany? This piece of discussion would be a good addition to the manuscript to help explain "why" the model forecasts failed and potentially motivate the use of convection-allowing models.

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4. Line 35: First bullet point: what operational model is being discussed here? Second bullet point: Is the COSMO model being discussed here? Please be explicit about what model and associated configuration is being altered.

5. Line 49: What is COSMO-DE? While the COSMO acronym has been properly described, I don't know what "DE" references.

6. Lines 85-91: What benefit does "domain shifting" have over other traditional lateral boundary perturbation techniques (e.g., Torn et al. 2016)? I have not been convinced in the introduction that there is significant benefit in developing a new technique to perturb boundary conditions. Would it be appropriate to compare the described "domain shifting" technique with other perturbation techniques? Including this type of analysis would presumably shift the focus of your manuscript to an evaluation of ensemble-generation techniques for a specific MCS case study. Alternatively, the authors could instead focus on the true predictability of the event (rather than the domain shifting idea) and include some additional predictability analysis (e.g., ensemble sensitivity). See Schumacher and Davis (2010), Ancell and Hakim (2007), Bednarczyk and Ancell (2015), Hill et al. (2016), and Torn et al. (2016) for some examples of sensitivity analysis for precipitation and high-impact weather forecasts. (Major comment above)

7. I would suggest leaving the descriptive nouns out of the manuscript, and let the reader decide what is "surprising" or not (e.g., Line 92).

Section 4

1. What is the source of radar observations? Would be appropriate to add this into the manuscript for reproducibility.

2. Line 144: should be (Fig. 4a)

3. Lines 158-159: I actually do not agree with this statement. I think the reference forecast has some glaring errors that do not make this a particular good forecast. Consider revising or removing this statement.

4. Lines 179-181: All the eastward shift simulations have poorer prediction though.

5. Lines 270-272: I think a reasonable counter argument could be that the W run initiated the convection well to the east (east of the red circle) and therefore had an earlier impact over Germany than the reference run, making it a “poor” forecast. Additionally, this forward storm system appeared to greatly impact the CAPE field in Figure 9, which seemingly had an impact on the development of upstream convection in the red circle. Furthermore, there is clearly a neutral to slightly negatively-tilted mid-tropospheric trough to aid in the propagation of shortwaves (hard to tell where these might exist in the coarse resolution of Figure 1): what role did mid-tropospheric dynamics play in this system? I suggest a more thorough evaluation of the simulations and discussing all aspects of the environment more thoroughly, including any convection that might have influenced convection initiation (CI) in the focus area.

6. Line 284: The sea surface temperatures have not been described in detail yet. How do we know these SSTs are the limiting factor? We do not know what the SSTs from each simulation are or how they dynamically are impacting the simulation convection. Seems like a reaching statement without any evidence and I would suggest revising or providing more concrete, quantitative support.

References:

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