

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-118

Anonymous Referee #1

Referee comment on "Optimization of weather forecasting for cloud cover over the European domain using the meteorological component of the Ensemble for Stochastic Integration of Atmospheric Simulations version 1.0" by Yen-Sen Lu et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-118-RC1>, 2022

General Comments:

This manuscript describes a very large ensemble simulation to evaluate the ability of numerous physics parameterization combinations in WRF to simulated cloud cover over a European domain. The overarching experiment evaluation appears to be valid and may have scientific merit; however, the manuscript is very difficult to follow, lacks many details, and limits the ability of the reader to completely understand certain information. Therefore, numerous questions arise about the experiment design, conclusions, and the applicable nature of the recommendations that the authors provide. A complete overhaul of the full manuscript is required to fix these problems. Principal criteria scores are as follows:

Scientific significance: 3
Scientific quality: 3
Scientific reproducibility: 2
Presentation quality: 4

Major Comments:

- The overall manuscript lacks clarity, making it very difficult for the reader to understand the experiment design, analysis, and results. At times, the text is also missing critical justification for decisions made by the authors.
- It is not very clear why the authors chose to separate the experiments into the three sets described. To help clarify, it would be useful if each set of experiments listed all physics parameterizations used, with the "cluster" name referenced, so they can be compared more easily. Also, the ensemble members that use stochastic physics need

to be clearly identified. The authors mention using SPPT as well as SKEBS, but only SKEBS appears to be described in 2.2.

- It is not clear if the authors considered the appropriateness of each physics parameterization for the resolution used in the simulations. Certain physics parameterizations are targeted at specific resolutions, unless they are truly scale-aware. Some parameterizations used in WRF are specifically targeted toward convection-allowing scales (~ 3 km resolution). Therefore, the authors need to thoroughly explain which schemes may not be appropriate for the 20-km resolution they are running, if any, and exclude those from their runs.
- Additional details about the ESIAS and general experiment design would be helpful. For example, why does ESIAS use ~ 1000 members? Does it always run with that many? If it is specifically being run with ~ 1000 members only in this case to sample as many WRF physics parametrizations as possible, please mention that. Which of the ~ 1000 members employ stochastic physics? Is the ESIAS always run at 20 km resolution using the same 180 x 180 grid point domain? Can it be configured to run differently? Is it run operationally?
- Section 3.1 is unclear. GEFS data are apparently used, but is it for both ICs and LBCs? What is the frequency at which LBCs are applied? What is the forecast length (48 hours)? What is the frequency of the output (every three hours)?
- The work described in this manuscript may represent one of the largest physics sensitivity studies using the WRF to date (certainly this is true for the evaluation of cloud cover?). It may be worthwhile for the authors to highlight this fact in the abstract/conclusions.
- Grammar should be double-checked throughout the manuscript. Some explanations by the authors are very difficult to understand. There are also numerous typos found throughout the manuscript.
- The authors state in the conclusions that they "... offer a recommendation on the choice of physics configurations for studying the European domain and for weather forecasting purposes." The manuscript only focused on the evaluation of cloud cover, which is just one of tens to hundreds of variables that are important for NWP. If the authors wish to provide physics recommendation for general NWP over Europe, many more variables need to be evaluated.

Minor Comments:

- Lines 41-43: Are the authors saying that most physics combinations will exhibit a bias when compared to surface-based observations? If so, sure, and that's inevitable as model physics will never be completely bias-free. Also, why only surface-based observations? Upper-air observations can be used equally to verify model simulations, with model physics having the potential to impact upper-air variables just as much. Model simulations also include more than just physics, so it's not possible to say that all bias is due to just the model physics. In addition, having some kind of bias doesn't necessarily make a physics parameterization or suite "unsuitable for deterministic forecasts". All operational models have some kind of physics bias, and work is always ongoing to minimize the error.
- Line 49 - What is "the scientific challenge of proper scoring rules"? Please clarify.
- Lines 50-51 - Is the "technical challenge" creating "large supercomputing facilities", finding the resources to run on large supercomputing facilities, the ability of an ensemble to forecast extreme and damaging events, or all of the above? Please clarify.
- Line 52 and elsewhere - A simulation isn't probabilistic by itself, but probabilistic forecasts for a given event can be created from an ensemble forecast. I would replace

- “probabilistic simulations” with “ensemble-based probabilistic forecasts”.
- Line 54-55 – It isn’t clear whether stochastic physics is used in all ~1000 ensemble members described in the ESIAS, or whether there is a subset of additional members that employ stochastic physics. Please clarify.
 - Line 56 – “cope with” or “meet”?
 - Line 59 – “ESIAS-met” has not been defined yet. It appears to be defined in line 63, but it’s not clear what the difference is between ESIAS and ESIAS-met until the next section.
 - Lines 59-62 – Please double check grammar. Also, are multi-physics simulations combined with stochastic simulations?
 - Line 78 – “to better fit the system” – I’m not sure what the authors are trying to say here.
 - Any specific reason why only SPPT and SKEBS are used? Did the authors also look at using SHUM or SPP?
 - Line 91 – Can the authors briefly describe the “different approach” used in the other study?
 - Line 93 – “to investigate the optimal physics configuration for the simulation output” – it might be a bit clearer to say that the optimal physics configuration is for the accurate representation of cloud cover.
 - Line 110 – What does “perform” signify here?
 - Table 4: Why wouldn’t “over-predict” in Table 4 also be a “miss”? The difference between “over” and “over-predict” isn’t very clear.
 - Line 131 – Can the authors define what a “rater” is here?
 - If CFC data aren’t available over Northern Europe, why wasn’t cloud cover verified over the western and southern portion of the simulation domain?
 - Lines 169-171 – The explanation of how/if the CFC data are upscaled for verification is unclear.
 - Figures 6 and 7, 9 and 10 – Why were the specific dates chosen for these figures?
 - Lines 247-250 – This text is unclear. Please clarify.
 - Lines 329-332 – It is unclear how many ensemble members exist in this study.
 - Line 344 – A deterministic simulation is never going to be unbiased.
 - Lines 349-350 – Note that Jankov et al. (2019) don’t necessarily advocate for multi-physics over stochastic-based ensembles. The authors describe the practical and theoretical deficiencies of multi-physics ensembles as well.
 - Line 352 – Spread produced by a multi-physics-based ensemble is mostly due to physics biases, not physics uncertainty.
 - Lines 354-355 – I wouldn’t call this random. It’s a specific result of the different physics parameterizations. It’s also possible that probability matched mean could be calculated for the cloud field instead of just the standard mean to alleviate some of these problems.
 - Line 356 – Jankov et al. (2019) used an eight-member ensemble, not four.

Abstract – the following two sentences aren’t clear: “We then selectively conduct stochastic simulations to assess the best choice for ensemble forecasts. The results indicate a high variability in terms of physics and parameterization.”

Line 23 – “negative wind energy prices” – This topic needs to be briefly explained

Line 23 - to study to the -> “to study the”

Line 26-29 - The introduction to deterministic models should be followed by a reference to the WRF model as being deterministic. Something like "Various global and regional **deterministic** weather models"

Line 31 - "optimal meteorological models," - I would say "optimal model configuration" instead

Lines 39-41 - Double check for typos and correct comma placement.

Table 4 - Typo in description -> "Indaddition"

Line 206 - Typo - "most" -> "the most"