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Earth System Dynamics Discussions

Interactive comment on "Comparing internal variabilities in three regional single model initial-condition large ensembles (SMILE) over Europe" *by* Fabian von Trentini et al.

Anonymous Referee #1

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I have difficulties in giving a condense summary of the study by von Trentini and colleagues. As I understand the present ms, the authors intend to investigate the effect of internal variability on projected changes in inter-annual variability of key atmospheric variables over Europe by means of 3 high-resolution SMILEs. This would be scientifically interesting and would provide new and important insights into the uncertainty of simulated future climate change signals. However, the concept of inter-annual variability is treated here as a concept of internal variability (often synonymously), thus causing a lot of confusion of concepts and distraction from a clear line of investigation and argumentation. I don't see an advantage in doing this. Why not investigating projected changes in inter-annual variability the same way as projected changes in any

other variable, using i) the SMILEs to provide a sound estimate of the associated uncertainty due to internal variability (i.e. sensitivity to initial conditions), and ii) the three models to provide an estimate of the associated uncertainty due to the choice of the climate model? The problem becomes apparent already in the introduction (L34ff) when uncertainty in projected climate change signals due to internal variability is confused with "future changes in uncertainty due to internal variability". The mixture of internal and inter-annual variability sometimes leads to unsound comparison, e.g. L34 and 37 (see below), and even to unsound conclusions, e.g. that an increase in inter-annual variability implies an increase in the uncertainty of climate projections due to internal variability (L338).

Moreover, the presented analyses are sometimes questionable. For instance, the significance test for a linear trend in IMV (Fig.9) is based on time series which have been smoothed by a 20-yr running mean. A running mean can heavily reduce the variance of a time series and thus increase the significance of its linear trend artificially. Anyway, the significance of climate change signals is more meaningfully tested against the variance of an unforced control simulation (constant atmospheric greenhouse gas concentrations). Such a control simulation is not provided for any of the RCMs but would be essential for each to substantiate the results.

Further, I miss some important analyses. The results of the investigated RCMs are not compared with their parent driving GCMs (the authors are aware of this, L328). Such a comparison, however, is of high interest and would increase the impact of the study significantly since it allows to assess the error in GCM-based estimates of inter-annual variability. I expect the signal over the British Isles for example to be strongly influenced by the temperature of the ocean, which is prescribed by the GCM in two out of the three RCM ensembles. A RCM-GCM comparison might also provide important information about the influence of the RCM domain size on the projected change signals. The RACMO domain for example is rather small and accordingly I expect a strong influence of the boundary conditions here. Also the imipact of different ensemble sizes is not discussed but of high interest. The ensemble sizes range from 16 to 50 members. Do the results suggest that 16 ensemble members are enough to study internal and/or inter-annual variability in the atmosphere?

Finally, a great advantage of having 3 ensembles at hand is that we can learn a lot about the driving mechanisms of the simulated future changes and their representations in different models. What are the physical driving mechanisms of the changes that agree in sign and what could be the reasons for the disagreements? The results should be put closer into context e.g. of the studies cited in the introduction (L59-74). Some suggestions of driving mechanisms are already given but should be strengthened by analysing and explaining more details. E.g. L310, arctic amplification and sea ice loss as a driver for decreasing winter temperature variability in Europe is not obvious.

Because of these major concerns, I suggest to reject the ms in its present form. Nevertheless, because of the great potential that I see in the comparison of 3 GCM/RCM SMILEs I like to encourage the authors to revise/extend their study thoroughly and resubmit a new ms.

Other general comments:

It is worth to add to the discussion or conclusions section that the ensemble means of the projected changes can be interpreted as the future changes associated with highest probability (under the considered emission scenario and the individual model constraints) but which specific change would in fact become realized depends on internal variability.

Please also add that by evaluating simulated inter-annual variability with E-OBS you also assume that this single realization (and period) of nature is not an outlier in terms of inter-annual variability under the prevalent climatic conditions.

In many paragraphs, the distinction between historical conditions and projected future

changes is not clear. E.g. L59.

No information about the variations in the initial conditions of both the GCMs and RCMs is provided.

Some specific comments:

14: Suggest: "Simulated inter-annual variability is evaluated against the observational dataset E-OBS and potential future changes under increasing atmospheric greenhouse gas concentrations are compared across the ensembles."

15: Delete sentence "To the knowledge of ..."

34: "Uncertainty of future climate projections can stem from at least three sources ..."

37: In L34, you mention the uncertainty in projected changes due to internal variability. Here you refer to "projected changes in uncertainty" understood as "projected changes in inter-annual variability" which addresses a different aspect of internal variability. These latter projected changes are subject to uncertainty due to internal variability as any other considered variable.

55: Using IMV to quantify IAV should not be motivated by "convenience" but by an advantage. What is the advantage here? Disturbing low-frequency variations are said to be small for seasonal mean and heavy precipitation. What about temperature? Using e.g. a running standard deviation over detrended 30-yr periods would not be sensitive to low-frequency variations. Further, it would be calculated over the same period (30 years) instead of over 16-50 years. IMV is similarly prone to biases due to events in the external forcing.

63: "However" doesn′t make sense here.

72: I guess you mean they found significant changes in inter-annual variability only in a small number of CMIP5 models.

118: I assume "surface temperature" refers to 2-m air temperature and "precipitation sums" to accumulated precipitation. Pleas clarify.

121: Analysis is not limited to summer. A heat wave in winter, though, does not have obvious societal impacts.

140ff: A reference to Fig.5 is missing.

145: "normally" distributed might be more appropriate than "randomly" distributed. The latter more suggests an equal distribution.

159: Why detrended by the ensemble mean rather than by each member individually? The trends are subject to internal variability at lower frequencies and can influence the calculated inter-annual variability.

164: IMV is only insensitive to trends if the trends are the same among the ensemble members. And it is not insensitive to external forcing effects. E.g. if the variability of a specific variable is significantly lower after a volcanic eruption, the IMV would decrease as well. In fact, I would expect the IAV to be generally larger than the IMV (Fig.2). Any idea why IAV < IMV?

218: The E-OBS time series might also be too short to infer a representative pdf, in particular for extremes.

229: Accronyms such as IMV are not used consistently.

234ff: Are these changes significant? For green and blue, the end of the tas-DJF time series shown in Fig.8, for example, seem to be close to or even within the historical ranges shown in Fig.2. This means that the future ranges clearly overlap with the historical ranges. Resting a significance test of a linear trend on smoothed time series, as done in Fig.9, is not valid.

254: Ensemble means are not shown in Fig.5.

258: The correlations shown in Fig.S10/S11 only reflect the signs of the respective changes shown in Fig.5/S2/S4 and do not add any information. In fact, a correlation

analysis between time series subject to trends is heavily influenced by the trends and thus not quite meaningful.

272: Scientific discussions are always critical.

285: Many biases might be inherited from the driving GCMs. A comparison is highly recommended.

290: What is the "coefficient of variation" applied by Giorgi et al.? Why not using it?

294: "Agreement and dissent" evaluates the results as kind of ambivalent. This does not fit with "even better agreement" at the beginning of the next sentence.

304: If I understand the approach correctly, from a future increase in IMV one cannot infer whether this increase is due to an increase in inter-annual variability or due to an increase in the spread of the mean states caused by internal variability. In L55 it is said, that it is valid to use IMV as an approximation for IAV if long-term variations are small compared to IAV. However, long-term variations (including the inter-member spread in the projected change signals) need to be compared with the projected changes in IAV, not only with absolute IAV.

319: Why is it plausible that the statistics of the length of dry periods increase for RCP8.5? In northern Europe, precipitation is projected to increase due to the enhanced moisture transport from low to high latitudes.

329: I highly recommend to include the RCM-GCM comparison in the present study. Whether downscaling with respect to inter-annual variability is important or not can only be demonstrated by such a comparison.

338: I disagree. An increase in inter-annual variability does not imply an increase in the uncertainty of climate projections due to internal variability. Climate change signals are typically based on climatological means. The spread of these is referred to as uncertainty due to internal variability and this metric does not necessarily depend on inter-annual variability.

340: The mean is not shown but required to assess this statement.

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