Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "The sensitivity of Alpine summer convection to surrogate climate change: An intercomparison between convection-parameterizing and convection-resolving models" *by* Michael Keller et al.

Anonymous Referee #1

Received and published: 16 August 2017

General

This is a very nice process study concerning summer convection in the Alps under climate change. The authors use the surrogate climate change approach and compare intermediate resolution (convection-parameterizing) and high-resolution (convection-resolving) simulations for an 11-day period in summer 2017. The paper is well written and clear, the employed methodology well established and sound, the presentation is

straight forward and the presented material (figures, tables) appropriate and (in most cases) clear. The only drawback of the paper is that it essentially produces already known results (even in the abstract the authors write, e.g., '. . .exhibit substantial differences, which are well known from the literature') - but with a not yet used approach. I therefore have only one 'major comment', which invites the authors to better work out what actually is the benefit from this study (it should be noted that also replicating a result with another approach has a value in itself). Apart from this I have a number of minor comments – and overall I think the paper can be published subject to 'minor revisions'.

Major comment

In the introduction, the authors give a very nice (and quite comprehensive) overview on the state the art in regional climate modeling with respect to summertime convection in the alps. Essentially, CPM and CRM resolutions had been compared (e.g., Ban et al. 2015), the surrogate climate approach (SCA) has been used before (Kroener et al. 2017), the two microphysics schemes had been compared (Keller et al. 2016). What is new in the present study according to the authors (p16, l.11) is the use of the SCA for CRM. So, one would expect to learn a bit more on the advantages of the SCA in the first place (what is it that we can learn from it that we cannot obtain from the decade-long CRM simulations?) – and why? Second, one would expect the approach to be put into perspective: open questions according to the introduction are i) the limitation of precipitation extremes due to the Clausius-Clapeyron equation (p2, l.25), ii) the role of stratification [changes] for the precipitation in CRM (p3, l.2) and iii) the impact of the microphysics scheme on the cloud top [not so much the precipitation], (p3, l.11). So, which of these topics can be better addressed with the SCA than with decade-long simulations (and why)? What can we learn from a 11-days process study that cannot be obtained from the full decade-long simulations? At the end of the introduction, the authors then formulate three questions they want to address (p3, l.14). These questions, however, do not correspond (one-to-one) to those open questions – and to

some degree do also not reflect what the authors summarize in their conclusions. I think therefore, the authors could make a much stronger case for their simulations if they would thoroughly work out what the potential of their simulations (approach) is (more than 'it has not been done') and by discussing their results in the light of earlier findings (and whether a disagreement could be resolved or explained) and their own questions in the beginning.

Minor comments

P2, l.27 I think the authors should expand a little on the advantages of the surrogate warming (and also maybe on the disadvantages). While in 1996 this approach was certainly mainly advantageous with respect to computing time, this has changed a little in recent times.

P6, l.25 yielded a bimodal. . ..

P7, l.14 Indeed, the 11-day period is quite limiting. Maybe the authors can expand a little on what processes they want to explore in more detail than possible with the decade-long CRM simulations. And especially, how this is related to the potential of the SCA.

P7, l.28 we assume. . .: of course, because this is a paper about convection - but couldn't this hypothesis be checked more thoroughly (using all the available data)?

P9, l.22 here we also have observations - so it would be interesting to see the mean diurnal cycle of the observations, too (this could be realized by also in the model only considering the 'observed part' of the domain - assuming [but the authors can of course judge, based on the results], that the mean daily cycles in the model runs will not largely change if only a subdomain is used).

P9, l.26 first of all: the responses to HW and VW are... . More important: I am not sure what the authors want to point out here. Under the 'response to HW and VW' I would intuitively understand the difference between the blue curves on the one hand, and the

red/orange lines on the other hand. If we take the peak, the dashed lines in Fig. 4a are rather closer to each other (i.e., the red/orange closer to the blue) than the full lines. So, is it something else that the authors want to point out? Apparently, when reading on, the authors refer to mean, temporally averaged precipitation (as given in Tab. 2). In a paper that deals with convection, however, I would find it extremely noteworthy, that HW is only larger during the night (and the microphysics scheme doesn't change this). So, what I really find striking is the large difference between HW and VW during the night - irrespective of all other differences. Can the authors comment on this?

P9, l.28 'these' are the present simulations, right?

P9, l.29 we attribute 'this' reduction': which now? the CPM, or the CRM? those of Ban et al. or the present? Please specify (Note that if the present simulations were referred to, an appropriate comment would have been that this could be more than a hypothesis - because the authors have all the simulation data so they could identify the circulation changes over the 11 days...).

P9, l.30 interestingly, in my paper collection this is Kroener et al 2017.Âă (Clim Dyn (2017) 48:3425–3440, DOI 10.1007/s00382-016-3276-3).

P11, l.2 what is the âĂŽconvection setup'?

P11, l.9 see above: most findings are 'similar to those of Ban et al'. → by elaborating a little bit more on why to use this surrogate climate approach (even if for only 11 days) this would help to better motivate the present study.

P13, l.4 couldn't those 'discrimination lines be shown in the figure?Âă

Fig. 7 the smallest letters and numbers are definitely too small.

P14, l.6 'below 310 hPa' seems to be misleading (I intuitively first checked p<310 hPa...). Better would be 'for heights below 310 hPa'.

P14, l.15 . . .indicates a similar timing. . .: Still it is interesting to note that precipitation

peaks around 1500 UTC (Fig. 4) for the CRM simulations and 1200 UTC for the CPM simulations. Combining earlier statements in the paper (p2, l.14, and p9, l.19, i.e. the reference to the Langhans et al. paper where it had been shown that the precipitation maximum is better simulated in higher resolution, and therefore should occur closer to 1500 UTC), this would imply that - at least for the chosen 11-day period - precipitation peaks almost [1-2 hr difference] when cloudiness has its minimum (Fig. 8). This is why (among others) I have suggested to add observed precipitation to Fig. 4. Maybe it can also trigger some additional analysis concerning which type of clouds actually contribute to OLR and to what degree OLR is determined by clouds.

Fig. 8, caption: '(a, b) outgoing longwave radiation at the top of the atmosphere (obs) and the model domain (mod), respectively (I presume).

P15, l. 4 'All the control simulations show a too early peak': isn't this a contradiction to the finding in OLR? If the peak is too early, then the cloud cover peak is too early (but the OLR peak is too late). Can the authors comment on that?

P16, l. 24-26 The results of the CPM. . ..: I think these three lines contradict themselves. First, they can only refer to the VW/HW discussion, and second, if the two largely coincide why then do we have significant differences?