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Interactive comment on "Use an idealized protocol to assess the nesting procedure in regional climate modelling" *by* Shan Li et al.

Anonymous Referee #2

Received and published: 15 January 2019

The manuscript presents an original approach in the evaluation of the so-called nesting technique used in regional climate modelling. The perfect model framework allows to perfectly isolate the detrimental impact of imposing lateral conditions to a limited area model. However the framework is somewhat different from the traditional RCMs: the forcing area is not a narrow band, but the rest of the globe. The paper is short and avoids non-essential descriptions, because some details might depend on the model and on the approach. This study deserves publication after a few clarifications and minor corrections listed below:

1. page 1 line 34: do not forget to mention here statistical downscaling

2. page 1 line 35: "impact" an impact model and an RCM are two different things

C1

3. page 2 line 2: The climate of an RCM is generally better because of the higher horizontal resolution, but also because the empirical adjustments of the parameterizations are specific to its domain

4. page 2 line 4: "boundary", the relaxation area is not a boundary here, as the RCM has a global integration domain, and thus no boundaries

5. page 4 line 17: how do 90 min compare with the model time step and the frequency of updating the relaxation conditions ? The authors should mention that in an actual RCM the relaxation time generally varies between these two times from the inner to the outer relaxation zone.

6. page 5 line 38: what is the difference between "idealized" and "ideal" ? Do the authors oppose "simplified" to "accurate" ?

7. page 6 line 2 and further in the text: "autonomy" does not fit in the case of DS300-to-300 (it might be more suitable in the case of DS300-to-100). Indeed the day-by-day solutions of the RCM and GCM should be identical. The difference appears because of the numerical inadequacy of the driving, amplified by the non-linearity of the equations (similar to the butterfly effect in GCMs). If the forcing were perfectly adequate, the differences between the two models should be minimal, irrespective of the "autonomy" of the RCM.

8. page 7 lines 1-5: please discuss further this feature wrt lateral forcing (e.g. is T2M subject to horizontal advection ?) 9. page 7 line 20: "first ten"; this error is found further in several instances.

10. page 8 lines 29-34: the decreasing trend in the correlation is certainly significant. This is not necessarily the case of the fluctuations (e.g. EOF3 vs EOF4). To make the assessment clearer, the authors should calculate the correlation at each grid point. After a proper spatial filtering, they could observe a correlation minimum in the centre of the domain. Then, an EOF with its maximum weights in the centre is expected to

C2

have a lower correlation.

11. page 9 line 16: reference ? (e.g. Michelangeli and Vautard)

12. page 12 line 3: stronger

13. page 12 line 11: comes

C3