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Interactive comment on "Large-scale atmospheric circulation biases and changes in global climate model simulations and their importance for regional climate scenarios: a case study for West-Central Europe" *by* A. P. van Ulden and G. J. van Oldenborgh

Anonymous Referee #1

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General comments

This paper analyzes the suite of global climate simulations that has become available for the 4th assessment report of the IPCC. I find the paper interesting and the science behind it clearly merits publication. Indeed, the type of validation performed could be taken as a zero-order procedure to weight the members of multi-model ensemble simulations (e.g. when constructing probabilistic climate scenarios). However, some of the conclusions of the paper appear somewhat biased. In particular, I believe that the paper overstates the role of the large-scale circulation, and the conclusions regarding the overall performance and suitability of the models are too critical. Further details regarding these points are given below.

Below I have chosen to categorize my scientific comments into “major” and “minor” (in distinction to the ACP recommendations), as I find it useful to earmark those comments that have affected my overall assessment.

Major comments

(1) One of the main innovations of this paper is the use of simple one-parameter measures to qualify the performance of the simulations. This is a very useful proposal. However, the authors do little about convincing the readers that their approach indeed serves this purpose. More specifically, does the (objective or subjective) consideration of the full SLP fields lead to the same conclusions regarding the performance of the models? I propose that, in order to address this question, two additional figures are added, showing the SLP circulations for DJF and JJA, along with the corresponding analyses (e.g. for the 8 models considered in sections 3-4). It is possible that such material has or will appear in other publications. In this case, a discussion of the relevant literature would suffice.

(2) Throughout the paper, the authors emphasize the role of the large-scale circulation. However, some of the results do not support these claim. For the control simulations, Fig.10 (see revised version that has been posted as a discussion item) shows that the circulation-related and residual temperature biases have roughly the same magnitude. For the scenario simulations, Fig.14 shows that the residual temperature changes (panel c) are substantially larger than those due to circulation changes (panel b). Thus, at best half of the error is circulation induced. This fact should obtain more space in the text (for instance when discussing Fig.14 on p.7427, l.6-9). Indeed, this result is at

odds with the mere focus on circulation changes expressed in the abstract and conclusions. It is also of critical relevance to downscaling studies, and suggests that statistical downscaling based on circulation patterns alone will miss the majority of the warming. A more careful discussion of the main results is thus needed.

(3) In my view, some of the conclusions are overly critical. For instance, the discussion section (p.7430) starts with the following sentence: "Many coupled climate models are able to simulate the long-term mean of monthly global patterns of the sea-level pressure. Unfortunately, this is not the case for northern midlatitudes and for Europe." This conclusion is not warranted by the current study and the text (including abstract) should correspondingly be revised. I agree with the authors that many models do a poor job, but the texts sounds as if all models were bad. Figs.3, 5 and 6 should be interpreted more carefully. While the figures show the standard deviations of the respective fields, the argumentation regarding quality does not appear to take these into consideration. To be more specific: The bias of most or all of the 8 better models is within one half standard deviation from the mean! I think this is quite a successful result. Also, in terms of the wind field, the models have monthly biases of around 1 and 2 m/s for the southerly and westerly components, respectively. This roughly corresponds to the observational uncertainty of single radiosonde observations.

(4) Figs.4 and 7 are useful, but they should be discussed more carefully, as the variability of the frequency distribution is not considered. Thus, the diagrams cannot be used to qualify the simulations.

(5) In citing literature, the authors reference a few recent studies (many of which have not yet appeared) but miss many of the standard papers in the respective areas. There is also some Dutch bias with the literature list. Some (but not all) specific locations that should be given consideration to are: P.7423, l12-13; p.7427, l.4; p.7428, l.11-12; p.7429, l.25-29. The authors should make an effort to present a more balanced list of references.

(6) I am not sure whether all the information in Figs.16-19 is needed. I believe this could be cut down to half or even less of its current length.

Minor comments

(7) Analysis shown in Fig.1 and Tab.1: Are the standard deviations from the different simulations computed after the mean bias (in SLP pressure) has been removed from the data in the respective domains? Mean biases might affect the error measures shown, while a model with a substantial mean bias (but the proper circulation) might still be useful. This comment applies in particular to the small European domain. Please explain.

(8) p.7419, discussion of Fig.1: The special curves in Fig.1 (labeled as "std Obs", "Nat Var", "ERA-40") should be explained in more detail. The text should allow other scientists to reproduce this figure. Thus, for all data sets used, their length should explicitly be listed (if the simulations differ in this regard, detailed data should be provided in Tab.1).

(9) p.7420, l.14-15: "... it is likely that the regional model will inherit much of this bias". Regarding circulation, the statement should be much stronger, e.g.: "... it is obvious that the regional model will inherit all of this bias". It is widely accepted (and actually a desired property of limited-area modeling) that the large-scale circulation is almost completely inherited from the driving model.

(10) p.7427, l.4: the most appropriate reference here is probably Pal et al (2004): Pal, J. S., F. Giorgi and X. Bi, 2004: Consistency of recent European summer precipitation trends and extremes with future regional climate projections. Geophys. Res. Letters, 31, L13202

(11) p.7428, l.2-6. Here the authors link local changes in precipitation and evapotranspiration. This link is sometimes referred to as "evaporation-precipitation fallacy", as it is not physically relevant on a regional scale. Indeed, atmospheric water vapor is

in the average transported over many thousand kilometers before getting involved in another precipitation event. If there is any direct link between evapotranspiration and precipitation, then it must be due to the soil-moisture precipitation feedback. The latter does not directly invoke the amount of water vapor that is being evaporated, but rather the associated modification of the low-level atmospheric profile.

(12) p.7429, l.25-29: please cite a broader set of references (those cited have not yet appeared). Also, the Lenderink et at. and Van Ulden et al. studies are missing from the list of references.

(13) p.7430, l11-12: see comment (9) above.

(14) It would be useful to discuss the performance of the 4AR models of the IPCC process in comparison with the TAR models. Have the models improved in the last 5-10 years? I would not expect the authors to repeat the analysis for the TAR models, but maybe some inference can be drawn from the published literature, for instance from the model intercomparisons conducted in the framework of AMIP and CMIP.

Technical and editorial comments

(15) p.7415: The title of the paper is very long. Also, is the term “west-central Europe” properly used? It appears to me that much of Western Europe is not included in the domain, may be the authors should rather use “Central Europe”

(16) P.7416-17: The abstract is too long and overlaps with the introduction.

(17) p.7419, l.27: this should be “6 models explain more than 50% of the spatial variance”, not “5 models ...”

(18) Tab.1 and 2: The order of simulations in the two tables is not identical (see FGOALS), please reorder.

(19) The legend of Tab.2 should be extended such as to define the quality measures used.

(20) P.7422, l.6: “The majority of the models have a mean westerly bias ...” rather than “Most models have ...”.

(21) P.7422, l.9 and 14; page 7423, l.16-20: the definition and use of symbols and abbreviations should be improved. The westerly component of the geostrophic flow is referred to as G-west (line 9) and G_w (line 14). Please use the same symbol throughout, and preferably the standard symbol from dynamic meteorology u_g (u subscript g). Similarly, I recommend using v_g and zeta_g for southerly wind and geostrophic vorticity.

(22) p.7424, l.20, Fig.8: Why did you use a poorly performing model for this plot?

(23) p.7440, Fig.4, applies also to Fig.7: I am unable to distinguish the different symbols. May be the figures should be redrawn using larger symbols, or be reproduced with larger size.

(24) p.7444, Fig.8, first line of legend: this should probably read “CCSM3” rather than “CCSM3.2”

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 7415, 2005.