

## ***Interactive comment on “The effect of downscaling on river runoff modeling: a hydrological case study in the Upper Danube Watershed” by T. Marke et al.***

**Anonymous Referee #3**

Received and published: 13 October 2011

————— General comments —————

As the authors state on pages 6335-6336, the scope of the manuscript is to "investigate the potential and limitations of an application of RCM simulations as input for the hydrological model component in DANUBIA." Different combinations of model runs and techniques should separate the impact of global boundary conditions, dynamic regionalization (Regional Climate Models, RCMs) and subsequent statistical downscaling on the results of the physically based, uncalibrated hydrological process model PROMET. The RCMs in this study are run with comparable spatial resolutions to exclude system-

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atic biases induced by different spatial RCM resolutions. The hydrological component is run at a time step of 1 h and a spatial resolution of 1 km × 1 km.

The scope of the manuscript has been mainly achieved, using a new combination of models, RCM data, and methods.

Even if the manuscript is of overall good quality, the individual flow of arguments is not always well presented (also because of major flaws in English language) and needs to be improved at some specific points.

On a technical level, the manuscript should be improved. This concerns the figures as well as the use of the English language.

In the review, the following available comments and responses of the author have been evaluated: Reviewer # 1: Hydrol. Earth Syst. Sci. Discuss., 8, C4162-C4165, 2011 Reviewer # 2: Hydrol. Earth Syst. Sci. Discuss., 8, C4166-C4177, 2011 The responses of the author are considered to be in generally valid. Only when an issue was raised by these reviewers that was deemed to be of special importance, this issue was re-emphasized in the following comments.

————— Specific comments: Major issues (general) —————

Comment #1

On p. 6336, lines 9-11: "Analogously to studies by Yarnal et al. (2000) and Wood et al. (2004) the results of the presented model runs are evaluated by comparing the discharge simulated for the out let of the Upper Danube Watershed at Achleiten to observations."

Actually, Wood et al. 2004 did use four streamflow stations, rather than only one, although the hydrological model resolution was coarser (1/8 degree) than in the manuscript under review. Yarnal et al. 2000 used the Upper West Branch (ca. 60 km x 120 km) of the Susquehanna River Basin, which has a much smaller size than the Upper Danube Basin (250 km x 300 km, 77,000 km<sup>2</sup>).

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One principal flaw of the paper is to concentrate only on one outlet of the relatively large basin. Although it is correct as the authors state that the gauge at the outlet represents an integrated response (p. 6336, line 12), the consequence of this reduction to only one reference variable should be more clearly stated in the following text, as the spatial resolution of the climate input (ERA or RCMA) and the scale difference between original grid (ERA and RCMs) and PROMET model grid relative downscaling resolution should have an effect, too.

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Comment #2

Similar studies have been made by the author and its co-authors. (see Hydrol. Earth Syst. Sci. Discuss., 8, C4166, 2011: Marke et al. 2011) This concerns especially Marke et al. 2011, where e.g. also the RCM MM5 has been used, and obviously the same methods have been applied. It seems that one major difference to the manuscript under evaluation is the inclusion of another RCM, i.e. REMO.

It would significantly improve the manuscript to state more clearly which are the differences to previous studies of the authors or studies cited in the references, especially Marke et al. 2011, Mauser & Bach 2009, Wood et al. 2004, and Yarnal et al. 2000, as also similar graphs are used for evaluations.

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Specific comments: Major issues (specific)

Comment #3

p. 6334, line 9: "While reanalysis data can be considered to supply (almost) perfect boundary conditions ..." Actually Jacob et al. 2007 on p. 35 only state that reanalysis data from NCEP and ECWMF (i.e. ERA40 and ERA15) are best available climatology. I strongly suggest that the authors follow that wording, because also reanalysis data are subject to errors and biases in the model and data assimilation scheme used for the reanalysis.

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Comment #4

p. 6335, line 21: "GLOWA-Danube strictly follows what has been formulated by Wood et al. (2004) as a de facto minimum standard" To my view, either a standard is defined/formulated through specific reasoning or it is a de facto standard through manifold use. It is not clear what the authors wanted to state here.

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Comment #5

p. 6336, line 1: (effects) "on the results of..." (the model) This formulation is too unspecific, as e.g. discharge at the basin outlet has been selected, but I wonder what the authors had in mind besides that.

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Comment #6

p. 6338, line 11: "For projections into the future, global climate models like ECHAM5 have to be operated running largely free, i.e. without incorporating observational input." Once again, it is not clear what the authors mean by that. I imagine that the prescription of the CO2 level, even if it is not observed, has a strong effect. Also other predefined or projected boundary conditions which probably are also used (remains unclear) should have an influence.

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Comment #7

p. 6339, line 17: "providing a realistic annual cycle of lower boundary conditions" What is meant here? The bottom/lowest limit of the atmospheric boundary layer, or the level of minimum influence of boundary conditions, or what else?

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Comment #8

p. 6340, line 21: "DANUBIA, SCALMET performs a synchronized exchange of energy and water fluxes between the models for the land surface and the atmosphere." p. 6341, line 2: "One of the main technical principles in SCALMET is that the down-and upscaling is carried out at runtime of the coupled model system." Later, only the downscaling is addressed more in detail. I wonder whether the coupling is one-way or two-way (which has been mentioned in other publications of the author). If it is two-way, then which upscaling takes place or was used. Also it is not fully clear which was the original temporal resolution of the RCM data.

p. 6346, line 16: "In the framework of this paper, only a brief overview of the model performance in the uncoupled model setup is given in order to provide a basis for comparison to the results of the coupled model runs presented subsequently." Again here, it remains unclear what is meant with the "uncoupled model setup". The reader is intended to believe that SCALMET works always in two directions.

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Comment #9

p. 6342, line 14: "snowpack ... controls the discharge at the outlet" I wonder why this was not covered in the discussion of the methodology or the results more in detail, (e.g. in a separate figure, rather than showing figures of evapotranspiration and global radiation) as this is a probable reason for the large deviation of discharge in the month of May as cited by the author.

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Comment #10

p. 6354, line 13: "The authors therefore emphasize the urgent need to carefully con-

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sider the setup of any coupled model system before interpreting the model results achieved for past as well as potential future climate conditions." This sounds somewhat trivial, but as it is important why no re-emphasize it here. But why is it considered "urgent"?

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Comment #11

p. 6354, line 4: "spatial resolution of 1 x 1 km<sup>2</sup>" Following line 13, it should be made more clearly in this paragraph, which were the initial spatial (and temporal) resolutions of the RCMs (0.4 degree), as later follow-up studies are announced with a better spatial resolution (0.088 degree) of a RCM (p. 6356, line 10):

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Comment #12

p. 6355, line 5: "to differences in the RCM domains." It remains unclear which differences of the RCM domains are meant here - WITHIN, extent, location, or what else?

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Comment #13

p. 6331, title: "The effect of downscaling on river runoff modeling: a hydrological case study in the Upper Danube Watershed." As the authors plan to switch from the current RCM resolution 0.4 degree to higher resolution 0.088 degree in a future study, and also to be more specific about the resolution used, I would suggest to include the RCM resolution in the title.

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Comment #14

p. 6332, line 3: "are coupled", line 8: "12 coupled model runs." To my view, like in

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comment # 8, the information whether one-way or two-way coupling is meant would help to better understand the scope and content.

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Comment #15

p. 6332, line 17: "simulation of discharge volume." To my view, this wording is incorrect, as discharge is always given in m<sup>3</sup>/s and not as a volume per period of time, e.g. per month.

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Comment #16

p. 6337, line 8: "discharge from 150 to 1750 mm per year" - please change to "runoff ..."

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Comment #17

p. 6336, line 8: "hydrological model (HM)" This should be PROMET, why not mention PROMET later, too, rather than to introduce a new unspecific acronym?

————— Specific comments: Minor issues (specific) —————

Comment #18

p. 6343, line 1: (relative) "comparison" ... "observation-based meteorology", line 2 "aggregations" First, here and at several other locations in the text, it is not clear where the observations are coming from (ERA40?) Second, "meteorology" is a science, so better use another term, also perhaps for "aggregations" the meaning of which is not fully clear, also what is meant by "relative comparison" (also later in the text), e.g. a ratio.

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Comment #19

p. 6348, line 2: limits 1000 and 2000 m<sup>3</sup>/s are used, but not justified.

————— Technical issues —————

Comment #20

The use of the English language has to be improved.

To a great extent terms and idiomatic expressions obviously derived from German are used. At many locations in the text, also some unspecific formulations or even wrong words are used. In some cases this even introduces errors or (at best) possible misunderstanding by the reader. I strongly recommend to copy-edit the manuscript carefully to eradicate this.

Examples are e.g.

- p. 6336 line 5 "input parameters" better: "input variables" (as conflicting with "parametrization")
- p. 6336 line 7 "temporal resolution" better: "time step"
- p. 6340 line 9 "after Morcette et al." (and others), better: "following ...", or "by ..."
- p. 6343 line 10 "comparatively small deviations" - compared to what?
- p. 6345 line 29 (to partition) "amount \_or\_ the hours before the recording" - meaning "to"?
- p. 6346 line 5 "luff-lee" better: "windward vs. lee side"
- the usage of "with" or "into", e.g. p. 6350, line 5, "To further investigate into this assumption"

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Comment #21

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Concerning the figures, some of them have already been published elsewhere, perhaps in some different versions (see examples). Why not make reference to this?

Fig. 4 (also missing mentioning of period 1971-2000) = identical content to Fig. 5 in Marke et al. 2011

Fig. 7 (a) (also missing "a") = Fig. 6 (a) in Marke et al. 2011

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Comment #22

Fig. 1: In this b/w figure, lakes and urban areas are hardly distinguishable, unless the reader recognizes the German names of the lakes. Perhaps put the legend entry title "Lakes" in italics as done with the names in the map.

The first legend entry of the watershed boundary is obsolete, as the insular-type map only depicts the selected basin.

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Comment #23

Fig. 3: "observation-based data" source unclear (ERA40?)

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Comment #24

Fig. 5: The graphical elements (squares etc.) are used in a different way than those in Fig. 2, and both figures should be designed consistently.

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Comment #25

Fig. 7: Why is the regression of the left sub-figure not made through the point of origin? "a" is missing for the left sub-figure.

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For the right sub-figure as in following similar plots, the text of legend and axes is very small and hardly readable.

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Comment #26

Fig. 7, Fig. 8:

The source of data for uncoupled simulation is not mentioned in the figure caption.

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Comment #27

Fig. 13: Is it actual or potential evapotranspiration? Is it an output of PROMET?

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Comment #28

Fig. 14: Is global radiation the sum of shortwave and longwave incoming radiation? Is it an output of PROMET?

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Comment #29

Fig. 13, Fig. 14: In the captions, selection "vari & bias" is not (fully) mentioned

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Comment #30

The reference section (starting at p. 6538) should be more strictly formatted according to the format guidelines.

- journal articles should have a DOI (or any other electronic resource, e.g. for the many Ph.D. theses cited) whenever possible - journal names should be abbreviated (e.g.

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Climatic Change)

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Comment #31

References of Mauser should be placed after those of Marke.

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Comment #32

Reference of Marke et al. 2011 should be updated with the final paper.

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Comment #33

Reference of MPI 2010 should hold a title rather than only the URL address and its outdated URL should be updated, perhaps changing the year to 2011.

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Comment #34

Reference of "Mürth" 2008 is incorrect, should read "Muerth".

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Comment #35

Reference of Pfeiffer and Zängl 2011 should be updated with the final paper.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6331, 2011.

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