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Hydrology and Earth System Sciences Discussions

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.

T. Marke et al.

thomas.marke@uni-graz.at

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The authors would like to thank reviewer 3 for contributing to the improvement of our manuscript.

——————— Comment 1: ———————

One prinicpal 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 RCM) and the scale difference between original grid (ERA and RCMs) and PROMET model grid relative downscaling resolution should have an effect, too.

——————— Response: ———————

The present study represents research that has been carried out in the framework of the GLOWA-Danube project (www.glowa-danube.de), where the impacts of climate change on the water resources of the Upper Danube Watershed are investigated. The hydrological simulations carried out in GLOWA-Danube aim at the investigation of changes in mean flow as well as in discharge extremes at the outlet of the catchment in Achleiten, in order to analyze potential changes in river discharge and water availability that downstream countries might have to deal with in the future. To provide results that are comparable to and consistent with other hydrological studies carried out in the GLOWA-Danube project (e.g. hydrological simulations based on station observations (Mauser and Bach, 2009), downscaled RCM data (Marke, 2008; Marke et al. 2011) or a stochastic weather generator (Mauser and Marke, 2009), the coupled model system applied in this study (GCM/ERA40-RCM-PROMET) has been evaluated with focus on the simulation of discharge conditions at Achleiten as well. As indicated by reviewer#3, the scale of investigation (here: the catchment size) has to be chosen accounting for the spatial resolution of the meteorological simulations. Considering the large grid spacing of the applied RCMs (REMO 50 km, MM5 45 km) as well as the fact, that the effective resolution of RCMs is at least twice as coarse as the grid spacing (Pielke, 2002; Grasso, 2000), the integrated hydrological response of the Upper Danube Watershed (76653 km2) as reflected by the discharge at the outlet in Achleiten is considered to provide an appropriate basis for the analyses in this study. The authors fully agree that the evaluation for different subcatchments would be very intersting and should definitely be the subject of future research (as pointed out in the conclusions). As such evaluation of the applied hydrolometeorological model chain for further discharge gauges (=subcatchments) requires i) a thourough analysis of the meteoro-

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logical input for all model setups (applying meteorological observations and different combinations of global boundaries and RCMs as input for the hydrological model) and subcatchments (as done for the Upper Danube Watershed in Fig. 3) combined with ii) a detailed hydrological evaluation for the different meteorological drivers (meteorological observations and different combinations of global boundaries, RCMs and statistical downscaling, see Figs. 7-13 in the new version of the manuscript), including the analysis of the model performance for further subcatchments goes beyond the scope of the present publication. Reviewer#3 is totally right noting that part of the discussion above should be added to the manuscript. The respective sections (introduction and conlusions) have been updated accordingly.

——————— 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 sigificantly 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.

——————— Response: ———————

Reviewer#3 is right when pointing out that considering the first version of the manuscript (HESS discussion paper) it could be made more clear what has already been investigated in previous studies (e.g. Marke et al. 2011) and what the differences between the previous studies and the present study are. Concerning a comparison to Marke et al. (2011), Yarnal et al. (2000) and Wood et al. (2004), a major difference is found in the application of different global models and different RCMs. Hence, the

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present study goes beyond a single combination of global boundary conditions and dynamical regionalization and gives a comprehensive overview of the dependence of the hydrologcial model results on global boundaries, dynamical regionalization (RCMs), statistical downscaling and bias correction. Mauser and Bach (2009) give an introduction to the hydrological model PROMET. These authors have further carried out thourough validation of the model, however, only using meteorological observations and no RCM data as meteorological input. Their analyses include a detailed comparison of simulations and observations on an hourly and daily time basis and even consider the model's capabilities in the reproduction of extreme discharge and return periods. The indroduction has been modified in the updated version of the manuscript to emphasize all these differences between the individual studies and publications.

——————— 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.

——————— Response: ———————

Reviewer#3 is right, the respective section in the manuscript has been updated.

——————— 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.

——————— Response: ———————

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We aggree with reviewer#3, "de facto“ is confusing in this context. We have updated the manuscript to make clear that this is a proposed minumum standard.

——————— 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.

——————— Response: ———————

The authors understand that it is necessary to be more specific, the mansucript has been updated accordingly.

——————— 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 problably are also used (remains unclear) should have an influence.

——————— Response: ———————

The authors meant to point out that no observation-based data is used as input for global climate models. We understand that the quoted sentence is rather unprecise and have therefore modified the manuscript to avoid any misundertsandings.

——————— 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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——————— Response: ———————

We meant the "...lower limit of the atmospheric boundary layer". The manuscript has been corrected.

——————— 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.

——————— Response: ———————

As noted by reviewer#3, there are different ways to describe the degree to which two models are coupled. The one-way coupled setup (often also refered to as a linked model system) passes data only in one direction, whereas two-way coupled models (often refered to as fully or bilaterally coupled model systems) exchange data in both directions. The coupler SCALMET can be applied in both modes: i) a one-way coupled setup, where RCM data is merely downscaled and provided as input for the models operating at the land surface (e.g. Marke et al. 2011) and in ii) a two-way coupled setup, where the energy and mass fluxes calculated at the land surface on the basis of the RCM data are returned to serve as input for the RCM (e.g. Zabel et al. 2011). In the

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present study, SCALMET is applied in a one-way coupled setup and is only providing the hydrological model with downscaled RCM simulations without returning mass and energy fluxes from the land surface to the atmosphere. This is why the upscaling of land surface calculations is not explained in more detail. In the uncoupled model setup, the hydrological model PROMET uses spatially interpolated station observations as meteorological input. The authors have updated the manuscript and have added additional information to make clear what is meant with "uncoupled“ and to emphasize that the hydrometeorological model chain is used in a one-way coupled mode whenever RCM data is applied in the present study. The authors never indended to make the reader believe the model system would be operating in a two-way coupled setup. Information on the internal time step of the RCMs has been added to the manuscript.

——————— 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.

——————— Response: ———————

To follow reviewer#3's suggestion and include a discussion of differences in simulated snow cover, we have analyzed the hydrological simulations achieved with MM5 and REMO data. We have added an illustration that compares the results achieved with the two RCMs and clearly shows much higher values of areal mean snow water equivalent in winter for the REMO-driven model run. Similar to differences in evapotranspiration, these differences in snow water equivalent result from differences in RCM-simulated global radiation (as also discussed in the paper) leading to less energy available for snow melt in case of REMO. As proposed by reviewer#3, these findings are a major improvement for our manuscript as they help to explain differences in simulated dis-

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charge in May. We thank reviewer#3 for this valuable suggestion!

——————— Comment 10: ———————

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

——————— Response: ———————

The authors consider the need to keep in mind and discuss the setup of a coupled hydroclimatological model whenever results are analyzed. Although that might sound trivial, this is often not sufficiently done in climate change research (see comment of reviewer#2, Hydrol. Earth Syst. Sci. Discuss., 8, C3886–C3888, p. 3886: "The great number of papers dealing with the impact of a possible climate change on the water cycle are of very different quality and their conclusions are unfortunately often more the consequence of some subjective methodological choices than that of a climatic signal. From this perspective the paper intends to fill a gap."). The authors understand that the word "urgent" is confusing in this context, we have corrected the manuscript accordingly.

——————— Comment 11: ———————

p. 6354, line 4: "spatial resolution of 1 x 1 km2" 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):

——————— Response: ———————

We have added information of the spatial resolution of the RCMs to the conclusions as suggested by reviewer#3. Information on the temporal resolution of the RCMs has

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been added to the model description section.

——————— 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?

——————— Response: ———————

The authors meant differences in the extent. We have updated the respective section in the manuscript to more clearly specify what is meant by "differences in the RCM domains“.

——————— 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.

——————— Response: ———————

As the effect of downscaling is tracked through the whole model chain (from global models (≈200 km) over different RCMs with different grid spacings (45 km for MM5 and 50 km for REMO) to the grid resolution of a hydrological model (1 x 1 km) the authors are not sure if including these different model resolutions in the title is an improvement. Of course the authors follow this suggestion if this is still requested. We have replaced the word "runoff“ with "discharge“ in the title to more appropriately refer to stream flow – this change is in line with comment#16 of reviewer#3.

——————— Comment 14: ———————

p. 6332, line 3: "are coupled", line 8: "12 coupled model runs." To my view, like in comment # 8, the information whether one-way or two-way coupling is meant would

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help to better understand the scope and content.

——————— Response: ———————

The reviewer is totally right, we have added "one-way coupled“ to make things clear.

——————— Comment 15: ———————

p. 6332, line 17: "simulation of discharge volume." To my view, this wording is incorrect, as discharge is always given in m3/s and not as a volume per period of time, e.g. per month.

——————— Response: ———————

The authors chose that wording to distinguish between the models capability in the reproduction of exact volumes and in the reproduction of the temporal course. We understand that discharge is defined as a volume and have modified the manuscript accordingly.

——————— Comment 16: ———————

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

——————— Response: ———————

The authors have followed the suggestion of reviewer#3.

——————— 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?

——————— Response: ———————

We have put the reviewer's suggestion into practice and are no longer introducing/using the acronym "HM“ in the manuscript.

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———————— 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.

———————— Response: ————————

The observation-based meteorological data applied in our study is provided by the meteorological preprocessor in PROMET. This information, together with information on where to find more information on this preprocessing, is given the sentence before the one cited by the reviewer. The wordings " meteorology“, "aggregations“ and "realtive comparison“ have been replaced by more appropriate expressions as proposed by the reviewer.

———————— Comment 19: ————————

p. 6348, line 2: limits 1000 and 2000 m3/s are used, but not justified.

———————— Response: ————————

We understand reviewer#3 and have corrected the wording to: âĂŽ... above ∼1000 m3/s’. This discharge value is based on the data displayed in the Figure and makes it easier for the reader to follow the line of argumentation in the subsequent discussion.

———————— 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.

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I strongly recommend to copy-edit the manuscript carefully to erradicate 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"

——————— Response: ———————

The reviewer is suggesting sevaral language improvements that have all been incorporated in the new version of the manuscript. Beyond this, various other formulations have been changed in the text to improve the quality of the manuscript.

——————— Comment 21: ———————

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

——————— Response: ———————

The authors consider it usefull to show examples for the applied downscaling functions as done in Fig. 4. Also this figure is similar to that published in Marke et al. 2011, the illustrated downscaling function is based on different global boundary conditions (ECHAM5) which are not considered in Marke et al. 2011. Hence, to show the correction of the biases in RCM simulations that are induced by the global boundaries of the ECHAM5 model, it is not possible to make reference to other publications. Concerning the period 1971-2000, reviewer#3 is right suggesting that it should be included in the caption which has been done in the updated version of the manuscript. Fig. 7a) is

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indeed also part of Marke et al. 2011. As it is very important to show the performance of the hydrological model PROMET driven with spatially interpolated observations in order to evaluate and discuss the model results achieved using RCM data to drive the model, the authors think it is appropriate to include this illustration in the present publication. We do, of course, agree to exclude this illsutration and make reference to the one in Marke et al. 2011 if this is requested.

——————— 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.

——————— Response: ———————

We have followed these valuable suggestions and have modified the illustration.

——————— Comment 23: ———————

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

——————— Response: ———————

The observation-based data is provided by the meteorological preprocessor in PROMET, we have updated the caption accordingly.

——————— 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.

——————— Response: ———————

This is absolutely right, we have modified Fig. 5 according to the suggestions made.

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

——————— Response: ———————

By not forcing the regression line through the point of origin, it is possible to also interpret the intercept of the regression line, which is considered to provide valuable information. The "a)“ has obviously gone lost in the process of generating a pdf from the word document as it is included in the manuscript that has been uploaded in form of a word document. The text size of legend and axes has been increased in Fig.7, and also in all subsequent figures according to the suggestion of reviewer#3. We made Fig7b) a separate figure in order to be able to increase size of Fig 7a) (now Fig. 7).

——————— Comment 26: ———————

Fig. 7, Fig. 8: The source of data for uncoupled simulation is not mentioned in the figure caption.

——————— Response: ———————

Information on the source of the meteorological data has been added to the figure captions.

——————— Comment 27: ———————

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

——————— Response: ———————

The figure shows actual evapotranspiration as simulated by the hydrological model PROMET, we have added information to the cation to make this clear.

——————— Comment 28: ———————

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

——————— Response: ———————

Global radiation represents the sum of direct and indirect shortwave radiation. It is provided by the RCMs and is used as input for the hydrological model. We have updated the caption to empasize that the illustrated radiation is provided by the RCMs and does not represent hydrological model results. The definition of global radiation has further been added to the manuscript.

——————— Comment 29: ———————

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

——————— Response: ———————

We have updated the captions accordingly.

——————— 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. Climatic Change)

——————— Response: ———————

We have updated the references section according to the suggestions and have added online sources and DOI wherever possible. Corncerning the abbreviation of the journal "Climatic Change“ the authors have followed the ISI webofknowledge guidelines for journal abbreviations (as proposed in the HESS author guidelines). Here, no abbreviation is suggested for the journal "Climatic Change“ (see also: http://images.webofknowledge.com/WOK46/help/WOS/C_abrvjt.html).

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——————— Comment 31: ———————

References of Mauser should be placed after those of Marke.

——————— Response: ———————

The reviewer ist totally right, we have corrected the manuscript accordingly.

——————— Comment 32: ———————

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

——————— Response: ———————

We have updated the references section with the final version of the cited paper.

——————— 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.

——————— Response: ———————

We have added a title to the reference "MPI (2011)“ and have updated the last access date.

——————— Comment 34: ———————

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

——————— Response: ———————

The references section has been updated according to the suggestion of reviewer#3.

——————— Comment: ———————

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

——————— Response: ———————

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We have added the final paper to the references list.

References:

Grasso L. D.: The differentiation between grid spacing and resolution and their application to numerical modeling, Bull. Amer. Meteorol. Soc., 81.3, 579-580, 2000.

Marke, T.: Development and Application of a Model Interface to couple Regional Climate Models with Land Surface Models for Climate Change Risk Assessment in the Upper Danube Watershed, Ph.D. thesis, Department of Geography, Ludwig-Maximilians University, Munich, Germany, 2008.

Marke, T., Mauser, W., Pfeiffer, A., and Zängl, G.: A pragmatic approach for the downscaling and bias correction of regional climate simulations: evaluation in hydrological modeling, Geosci. Model Dev., 4, 759–770, doi:10.5194/gmd-4-759-2011, 2011.

Mauser, W. and Marke, T.: Climate Change and the Water Resources: Scenarios of low-flow conditions in the Upper Danube Watershed, IAHS Publications, 327, 225-236, 2009.

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Pielke Sr. R. A.: Mesoscale meteorological modeling. Second. International Geophysics Series, Vol. 78, San Diego, California, USA, 2002.

Thank you very much for your investment of time and effort! We hope to have adressed your comments adequately and would like to thank you again for your valuable suggestions! Your endeavors are highly appreciated!

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