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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Why does a conceptual hydrological model fail to predict discharge changes in response to climate change?" *by* Doris Duethmann et al.

Anonymous Referee #2

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In their manuscript "Why does a conceptual hydrological model fail to predict discharge changes in response to climate change?", D. Duethmann et al. investigate possible reasons for the deficiencies of a conceptual hydrological model (HBV model type) in reproducing observed changes in discharge as a response to changing hydrometeorological conditions in 156 catchments in Austria. The authors set up hypotheses that belong to three groups of possible causes: (i) data problems, (ii) problems related to model calibration, and (iii) problems related to model structure. They test these hypotheses by comparing simulations generated by modified versions of the model according to the hypotheses against a baseline model. Data problems and model structural problems with respect to vegetation dynamics have been identified as the most

relevant causes for the model deficiencies.

General comments:

The paper is well written and well structured. It addresses a relevant scientific question and provides valuable insights for hydrological modelling under changing climate conditions which surely is of broad interest. Still, I have a few comments and suggestions that may further improve the manuscript:

The results are mostly presented as averages over the investigated 156 catchments. I wonder if we could not learn even more if also the statistical and/or spatial distributions will be presented. As stated in the discussion, reasons for hydrological model deficiencies can be very site specific. By including more of the variability between the catchments, prominent cases could be identified which do not (or particularly do) support the conclusions which are based on the mean of all 156 catchments. This may also feed the discussion on possible further causes for model deficiencies which have not been tested in this study.

The modified model versions V2, V7, and V8 have led to the best improvements. Maybe it is worth showing another figure on these results in the same manner as Fig. 3 (or the modified version of Fig. 3). This could be a nice illustration of the key results of this study.

Specific comments:

Title: The title is catchy but also provocative since it suggests that conceptual hydrological models in general are not suited/justified for climate change impact studies, which is not correct.

P2, ll9-11: what is meant by "minimum requirement". Passing or failing the test? How is this determined?

P3, l25: Please provide references.

P4, l14: The numbers show comparatively large differences in elevation ranges. I wonder if this has any influence on the testing result. Are there any altitude-dependent differences in the results of testing the hypothesis? This partly corresponds to my general comment.

P4, Fig.1: When I look at this map, I am reminded to a paper that has identified (homogenous) hydrological regions in Austria (though it was probably with reference to flood types). Anyway, do the presented testing results show any systematic spatial differences regarding the major reasons for model performance losses or improvements? For the baseline model, Fig. 3 (c) presents a map in this regard. For the tested hypotheses, however, spatial information is not presented. I think, though, that this could be interesting. This also corresponds to my general comment.

P5, ll9-13: I remember from other regions and countries that their official meteorological data products are already corrected for potential undercatch. I am not familiar with the SPARTACUS data; I just want to be sure that no "double-correction" is performed here.

P6, Section 2.3.1 could also make a reference to Table 1.

P7, l19, and P8, ll9-10: "(E3)" confuses me. Did I miss E2? On P8, E3 is compared to E2. Later, only results for E0-E2 are reported (e.g. Table 3). I assume that E3 is E2. Please check. Also, "than" instead of "tha" (P8, l9).

P8, Eq.8: Is fbeta the same as fp? Otherwise, fbeta is not explained. Is the same objective function applied in Merz2011?

P9, l4: One more sentence on how the shuffled complex evolution algorithm works would be nice.

P9, l9: It could be highlight that the seven 5-year calibration periods have no temporal overlap.

P10, ll13-16ff: I agree that such problems will probably not affect many catchments.

For selected catchments, particularly in mountainous areas, it still might be a cause for problems in calibrating and evaluation the hydrological model. Does the HZB provide information in this regard?

P11, Figure 2: You may add to the figure caption to which number of stations P1 (P2) and T1 refer.

P14, l18: Does Esim refer to the model estimation based in Eq.2? Or does it refer to the difference between P-Qsim? Would it make any difference (also regarding the consideration of the same uncertainties that refer to the estimation of Ewb)?

P15, ll3-6: How has this been done?

P16, Figure 3 (and others): I see that these figures are designed to match the presentation by Merz2011. However, I think that by presenting only the mean a lot of information is hidden. Boxplots or additional maps (as in Fig. 3) would be more appropriate. This also refers to my general comment.

P17, Figure 4: Do the seven 5-year calibration- and evaluation periods show any marked differences in terms of hydro-meteorological conditions?

P18, Figure 5 (also Figure 7): You could add to the figure caption that the impacts of altering these variants in the hydrological model are summarized in Table 4.

P19, Figure 6: You may indicate that Fig. 6 (a) is the same as Fig. 4 (a).

P20, Table 5: This table (in combination with Table 2) is really nice since it provides a good summary of the tested hypotheses. Maybe the result of V8 can also be summarized here.

P21, ll25-27ff: It could be emphasized more clearly why you choose to combine V2 with V7 to V8.

P22, Discussion: The discussion reads nicely, and I agree with the main conclusion that the consideration of interrelations between climate, vegetation, and hydrology is an

important further step for hydrological modelling in transient climate. Still, I have a few remarks and thoughts regarding the discussion. a) The discussion in its current form gives the impression that model structure deficiencies regarding vegetation dynamics is the most important reason for model performance deficiencies in transient climate, although fixing problems in the precipitation data have led to improvements of similar magnitude. Finally, it could be highlighted that the combination of both approaches has led to the largest improvement (reduction in mismatch by about 95%). b) For good reasons, model structure improvements are restricted to incorporating vegetation dynamics only. Still, what could be further model structural issues that cause model performance losses in this particular study region? Maybe it is worth highlighting that glaciated catchments have not been considered here. Have they been considered by Merz2011?

P23, ll1-3: Considering my complaint regarding the title: This is a good example for the benefit of a conceptual hydrological model. By applying a rather simple approach, vegetation dynamics can be considered to some degree for hydrological simulations in changing climates.

P24, l1: I think this refers to V2 which indeed had a considerable effect.

P24, l4: One ")" is missing.

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