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Interactive comment on "Modelling the water budget and the riverflows of the Maritsa basin in Bulgaria" *by* E. Artinyan et al.

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Interactive comment on "Modelling the water budget and the river64258;ows of the Maritsa basin in Bulgaria" by E. Artinyan , F. Habets , J. Noilhan , E. Ledoux , D. Dimitrov , E. Martin , and P. Le Moigne

E. Artinyan

[Reviewer comments in italics, authors replies in bold]

The authors would like to thank all of the reviewers for precise and thoughtful comments and constructive criticism which has led to a better manuscript. Below we respond to each referee comments individually. Reviewers please note that figure, table and page numbers refer to figures, table and pages in the uncorrected manuscript.

Response to Anonymous Referee #2:

Major comments: (1) The paper is rather long and can be shortened especially in the descriptive sections. Although the acquisition of the necessary land surface and meteorological data must have been formidable challenges, its description in 4.3 and 4.4 does not add much to the present paper. The link between soil texture and vegetation (through "tables of correspondence"; see 4.3) is unclear and Figure 5 is not very informative. Similarly Tables 1 and 2 are not absolutely essential.

We agree with this comment. The paper length was reduced and more emphasis has been put on scientific aspects. Section 4.3 and 4.4 were partially rewritten and subsequently shortened. The number of tables was reduced: tables 1 and 2 were removed. Figures were also reduced: figures 1, 5, 6 and 7 were removed from the paper.

(2) Re. Equation (3), why is the shape parameter dependent on altitude?

Linking the shape parameter to the elevation results in better statistical results of the streamflow simulation. Dependency of the shape parameter on altitude empirically account for the higher relative contribution of the surface runoff at high elevation in mountains due to steeper slopes and shallower soils.

(3) The comparison of two years of computed Penman and observed pan evaporation (see 4.4.4) can hardly be described as a 8220;validation8221; of the atmospheric forcing data.

The referee's observation is correct. It was found interesting to compare the measured pan evaporation to the computed one using the model forcing data, however it isn't enough to validate the forcing data. The entire paragraph 4.4.4 was removed as well as the linked figure 7.

(4) Section 4.1 refers to the calibration of the groundwater module for storage coefficients and transmissivity for eight sub-regions of the unconfined underground layer. It

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does not describe how this was done nor does it indicate over which period and for which parts of the basin.

The text about parameter calibration is now added to Section 5.2.

(5) Data for 12 dams are available for the first year of simulation: October 95-September 96 but not available for 96-97). Table 4 indicates that the modified model (with the two additional soil reservoirs) is calibrated twice over October 95-September 96: first, with 56 stream gauges and second, using 68 stations (56 stream gauges + 12 dams).

Data about dam inflow and release was available only for the first year. Calibration of the modeling parameters was achieved for the first year only once having already "imposed" the streamflow after each dam. The second set of statistical parameters in Table 4 shows the statistics computed for the same year without "imposed" streamflow to show the impact on the results. The third set in the table shows validation over the year 96-97 (for which no data from dams was available).

It would have been nice to have had more detail on these calibrations and especially on the "imposition" of the dam data.

Calibration of the hydrological model parameters: of the unsaturated zone reservoirs and of the unconfined underground layer is now better described in Section 5.2. "Imposition" of the dam data is described in Section 4.2. The method is considering that the first upstream reservoir inflow is the "natural" flow, therefore the computed by the model "natural" flow is subtracted from each simulated downstream station riverflow with respect to the time lag between the dam's river cell and the corresponding station cell. Secondly the dam's outflow given by the dam company is added to each downstream station riverflow again with respect to the time lag. After the same procedure is conducted with the next downstream dam, etc.

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These calibrations yielded values for C1, C2, α, h1max and h2max for each of 68 sub-catchments. Does Table 3 refer to the first or the second calibration? Why not validate for 1996/97 (in Table 4) with the result of the second calibration as well?

Table 3 refers to the single calibration with use of station's measured discharges and "imposed" after-dam streamflow.

Can the spread in parameter values referred to in 5.2 and Table 3 be related to topography, soil texture, land use, vegetation etc.?

In Section 5.2. are explained the relations found between the geographical spread of those values and the corresponding geomorphology. The Section 5.2. was edited in order to clarify those relations.

(6) The validation of the modeling results for October 96-September 97 [for river flow; snow depth; snow water equivalent; soil moisture content] are encouraging. In order to appreciate Figure 12(b), it would be necessary to give some background information on the climatological methods used by Vekilska (1982) a publication which is not easily accessible to HESS readers.

Some more details on the climatological methods were added in the new version of the text.

(7) The paper needs a substantial amount of editorial work. For example Lines 22-25 on p. 487 requires rephrasing. Similarly, the use of some terminology needs to be tightened up and lines 4-8 on p. 498 rephrased. The reference to the water stress (line 8, p. 499) is also not clear. Careful editing of the headings to Tables 1, 2 and 6 is needed. Clarify basin-range and country-range in Figure 12. Also note the use of gages and gauges; precipitations vs. precipitation; the valley vs. the valleys or the low lying areas; (mis)use of the term "aquifer" and "water table"; the studied area vs. the study region.

Last paragraph of p. 487 and the first paragraph of p. 498 were edited and the

terminology corrected. The entire line containing the term "water stress" was removed from the text as it doesn't add more information. Tables 1 and 2 were removed. Heading of Table 6 was edited. In Figure 12 it is now explicitly mentioned that it compares whole country climatological values with values resulting from the 2 years simulation of Maritsa basin energy budget. Use of terms "aquifer" and "water table" was checked and corrected.

All minor comments made by the Referee #2 were taken into account in the final version of the paper.

Response to Anonymous Referee #1:

This paper is written as a detailed report on the application of a coupled large-scale hydrological and SVAT model (ISBA-Modcou); it therefore is very complete in terms of input/output data analysis, but lacks a scientific discussion thread; the paper should be rewritten so that one scientific issue is tackled: the authors should clearly state what is the specific scientific question posed behind the sole ISBA-Modcou implementation.
If not, it is not clear in what aspect this application is different/innovative compared to previous applications of the ISBA-Modcou framework.

The Referee observation is highly appreciated. This first application of a distributed hydrological model for Bulgaria incorporates some new aspects may be not enough "visible" throughout the paper. However considerable part of the paper was used to describe them. The need of new formulation of the unsaturated underground flow is explained and its introduction is described in Section 3.2. Its calibration is depicted in Section 5.2. An empirical link was established between the shape coefficient (sub-grid runoff coefficient) and the cell elevation, which enhances the quality of hight flow simulations of mountain catchments(Sec. 4.3). Artificial reservoir's operation is taken into account for the hydrological modeling (Sec. 4.2.), which results in more precise estimation of model parameters during the calibration. Lacking data of global solar radiation was replaced with

new empirical formulation making use of available data of cloudiness and bright sunshine duration (Sec. 4.4.3).

I suggest to split the paper in two parts: one should be converted into a technical memorandum of the NIMH detailing the gathering of all input data and presenting the description of the outputs that are not directly relevant to the chosen scientific objective, and the second, referring to the memorandum, should be the HESS paper itself with a specific scientific approach, incl. a statement of the scientific objectives that the authors want to achieve (assumptions/materials and methods/results).

In the revised paper detailed description of data gathering and treatment is removed. Sections 4.3. and 4.4. now contain only new methods concerning shape parameter estimation and the preparation of the global solar radiation model input.

It seems to me that the most interesting development of this study is the modification of the slow flow component and the spatialized calibration of this add-up. In that case the paragraph concerning flow delay should be largely expanded: what are the processes involved ? How does one know the extent and intensity of each process ? How does it translate into the modelling choice that has been made ? Why using 2 reservoirs for the slow component ? How does it interact with the VIC model ? etc In that respect the conclusion that the inclusion of this 2-compartements scheme improves the simulation is not supported by the available information. I suggest this improvement could be used as a guideline for the paper.

It is mentioned in Section 3.2 that the newly added two-reservoir drainage flow formulation is a conceptual scheme assuming there is at least two parts in the underground unsaturated flow. One that comes from deeper layer below the root-zone and flows longer in time but resulting in relatively low discharge values. This reservoir maintains the summer flow (till few months) in dry conditions, which is by far not possible with the coupled ISBA-Modcou system at the time

when this study was started. The second reservoir stands for an upper layer of the unsaturated soil, again below the root-zone. It has a higher depletion coefficient and is emptied within few weeks after the rain event. Concerning the depletion speed it plays an intermediate role between root-zone reservoir (as part of ISBA) and the lower unsaturated zone reservoir. Figure 11 shows the impact of the added reservoirs on the basin monthly water budget. Figure 11a shows that during four consecutive months of 96 and one month in 97 there is practically no water available for the runoff (sum of both surface runoff and root-zone drainage). Figure 11c shows that "unsaturated zone reservoirs" during those months are not empty, thus supplying flow to the rivers.

Another concern is the lack of evaluation data; although validation at such a large scale is difficult to achieve, local measurements have little representative value (e.g. soil moisture) and in this study streamflow is the only integrating/large scale "validation" variable. The discussion section should mention this, and at least propose alternative methods to check the relevance of the complex model used in the study. For instance, one could imagine an evaluation of the evolution of snow covered areas or surface temperature patterns with low resolution remote sensing to better constraint the realism of the modelling framework. Low resolution remote sensing data is free of charge and easily accessible nowadays. The paragraph on energy balance could be shortened, because it is purely descriptive in absence of evaluation methods.

For the modeled period (95/97) no data is available from remote sensing about surface temperature.

Details:

Page 482 line 2: the 2 layer scheme is already describing an unsaturated zone; specify that you want to describe "an unsaturated zone below roots". What I do not understand is that it seemed to me that this unsaturated zone below the root zone is the third layer of ISBA-3C. please comment on that.

At the time of the application set-up ISBA didn't include a third layer. Nowadays it is possible to test either ISBA-3C or "Nash's cascade" to simulate the "unsaturated zone below roots".

Page 484 line 2: how is Tc calculated ? Be more specific

As Tc, the maximum concentration time for the basin [day], is an integer value it was calibrated empirically by running the hydrological model with the more realistic values (between 4 and 7 days). Then the value of Tc giving the higher statistical scores when comparing simulated to measured riverflow was chosen. This is now explained in the revised Section 5.2.

Page 485 line 12: provide reference for albedo/min. Stomatal resistance table

In the revised version the reference was added: Champeaux and Legléau (1995)

Page 486 line 1: provide reason for using eq. 3; impact of such a choice ?

Dependency of the shape parameter on altitude account for the higher relative contribution of the surface runoff at high elevation in mountains due to steeper slopes and shallower soils. When comparing the simulated against the measured riverflow higher efficiency is achieved linking the shape parameter to the elevation.

Page 486: provide reference on software Bluepack;

In the revised version the reference was added: Delfiner, P., Renard, D., and Chiles, J. P., 1978, BLUEPACK-3D Manual: Centre de Géostatistique, Fontainebleau, France.

page 490: "validation": I8217;d rather use the term "evaluation"

The Referee #1 suggestion is accepted and the Section 4.4.4 title was correspondingly modified.

Page 491 par 5.2: this paragraph is not clear and should be rewritten (both the parameter specification procedure and the statistical calibration procedure are difficult to understand); you calibrate the slow flow model, not the reservoirs; how do you define the dry period of the year ?

In the final version Section 5.2 was partially rewritten and the calibration procedure explained in details. For the calibration year the dry period is easy to estimate from the measured riverflow (see Fig.8). It starts in July and ends in October. As the calibration has been made for each sub-basin with available measured data, Qdry was estimated for each one of the 56 hydrological stations and 12 dams.

Page 492 first par.: expand the description

In the revised paper the description is expanded with explanations about the possible link of the two reservoir's maximum levels and the geomorphological characteristics of the corresponding area.

page 521: what happens to the energy balance in April 96 ? It sounds that Rn-G<H+LE

The figure was unclear and now is corrected. The lines concerning Rn and H should have marks at each month between the columns of LE and G.

Table 5: what is "W&B" ?

This is now explained in the table heading. W&B stands for Wheat and Barley cultures.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 475, 2007.