Response to the comments by Reviewer #1

The following revisions have been made based on the Reviewer #1's comments to manuscript "Distributed modeling of land surface water and energy budgets in the inland Heihe river basin of China" authored by Y. Jia, X. Ding, C. Qin, and H. Wang. The manuscript number is HESS-PUC-09-M106.

The paper presents the application of a land surface model to a river basin in China. I appreciate the effort the authors have made in the preparation of this paper. However, there are a number of problems with the paper.

Comments

The first problem is that it provides no advancement to our current knowledge about land surface modeling. This type of models has been applied to basins worldwide.

Response

The paper really needs a revision to make its advancement points easily understood, but it is quite difficult for us to agree with the reviewer on this comment. We hope the reviewer may pay attention to the following points at least:

(1) Most land surface models consider the water and energy processes only in the vertical direction, whereas many researchers (Koster et al 2000, Walko et al 2000) have noticed that it is very important to reflect the effect of horizontal redistribution of moisture and water on the partition of land surface water and energy fluxes. Therefore, the combination of land surface models and physically-based distributed hydrological models is a research front in the field, just as the VIC model does. The WEP model applied in the paper is just one of these kinds of models which combine the land surface modeling of vertical water and energy fluxes, and that of the horizontal hydrological processes. Moreover, the modeling of hydrological processes in WEP is physically-based, and it includes not only the overland flow and river flow routings but also the multi-layered groundwater simulation module and water utilization to fully reflect the effect of horizontal redistribution of moisture and water on the partition of land surface water and energy fluxes, which makes it different from VIC model.

(2) Confronting the complexity of land surface hydrology and the existing key issues like heterogeneity/scale, our knowledge of land surface processes is still very poor, and the case study of a specific basin is still meaningful and helpful at present. This paper showing the efforts of a detailed case study in an inland arid and hilly basin (the Heihe river basin) is surely of referential values. In addition, as described in the third paragraph, the study adopted a methodology different from all of existed studies in the basin.

(3) In high-elevation basins like the Heihe river basin, the consideration of frozen soil on land surface hydrology is quite important, especially on river base flow. Experiencing the difficulty in reproducing the base flows in the hydrographs of Fig.6 to Fig.8, we suggested the equation (31) to reflect the temperature influence to the saturated hydraulic conductivity in the frozen soil, and the issue is successfully solved. Although it is a simple approach, it works well, thus it is believed to be of referential values.

The first chapter (INTRODUCTION) is revised to clearly describe the main advancements in this study.

Comments

The second problem with the paper is that it has been calibrated and validated with discharge data alone. This means that the internal states and fluxes of the model can be quite wrong, but that the model still provides reasonable discharge results. This relates to the well-known equifinality problem, about which enough papers have been published. This has not been discussed at all in the paper.

- Page 2213, line 24-26. If the physical processes in the model are wrong, they are wrong as well on a daily as on a monthly time scale.
- Page 2216, line 10-15: indeed, these results may be totally wrong, since they have not been validated!
- Page 2216 and the page after: As stated above, good discharge simulations may have been obtained with totally erroneous internal states and fluxes. This has not been discussed at all.

Response

As commented by the reviewer #1, the issue of equifinality is important for the hydrologic-response simulations. Equifinality problem refers to more tan one parameter combination providing an equally good (or poor) representation of the integrated hydrologic-response (Brian, 2006). Besides runoff, it would be better to validate the distributed variables (e.g. ET, Soil moisture, Groundwater table, etc.). It is necessary to further discuss these problems. However, with the development of spatial technologies (e.g. RS, GIS, etc.), the use of distributed observations for WEP parameter estimation would partially reduce the errors of input/boundary data and reasonableness of parameter set (within reasonable bounds) in our rainfall-runoff modeling efforts. We have added more detailed description for parameter estimation in the section 3.2 of the manuscript.

WEP model is based on water balance, energy balance and hydro-physical process and parameters are physically-based. Although it is needed to verify the reasonableness of parameter estimation, it's not "totally wrong". 1) Based on water balance equation: $P = R + E + \Delta S$. At an annual scale, $\Delta S = 0$ (see Fig.10), so $E = P - R$. Therefore, if precipitation (P) is based on observations and runoff (R) is verified, evapotranspiration (ET) would be at an acceptable level. 2) In the manuscript we have verified monthly and daily runoff hydrograph. These indicate that it is acceptable for the calculation of surface runoff, subsurface runoff and groundwater flow. Moreover, at each time step and each layer they are constrained by water balance equation, which make sure that it is not "totally wrong" for ET, soil moisture, groundwater table and other variables at each time interval and each layer. And 3) Different from empirical model and conceptual model, parameters of WEP model are physically-based. The domains of parameter values and their combinations would be constrained with the physics. Without the physics, there would no such constraints, and any combinations can lead to similar effects (Savenije, 2001; Loague & VanderKwaak, 2004). We have added more detailed description for the parameter values in the section 3.2 of the manuscript.

There are still some issues, even though the distributed variables can be verified based on observations. As stated by Beven (A manifesto for the equifinality thesis, 2006): 1) Many hydrological variables are of obvious spatial heterogeneity and are different from lumped variables like runoff. ET, soil moisture, groundwater table and energy fluxes, for example, might be predicted as an average over a model grid element and over a certain time step; the same variable might measured at a point and a limit scale in space. Therefore, there are also some sources of error for these variables because of heterogeneity and scale effects, non-linearities of measurement technique problems (the incommensurability problem of Beven, 1989). 2) Equifinality also relates to errors of input data and bound conditions. Therefore, as commented by the reviewer #1, it is necessary to discuss and research equifinality issue. However, it is difficult to solve this problem at present and even several researchers have found that the use of distributed observations did not appreciably reduce equifinality in their rainfall-runoff modeling efforts (e.g. Lamb et., 1998; Vertessy and Elsenbeer, 1999; Blazkova and Beven, 2002; Blazkova et al., 2002.). It can't be concluded for any modeling study that simulation results have already eliminated equifinality.

Comments

A third problem with the paper is that no explanation at all has been provided regarding the parameter calibration. This is going to have a strong impact on the results. In the same context, it is unclear whether a distinction is made between a calibration and validation period.

- Page 2210, line 3-10: We really need more explanation on how the parameters are tuned. Are they tuned manually, or is some automatic calibration algorithm used? The authors perform a scenario analysis with their model, so this is going to be a very important issue!
- Page 2212, line 1-10. Again, it is unclear how the parameters were adjusted. Also, it is not clear what exactly the calibration and validation periods were.

Response

We agree with the reviewer. The revision has been made.

As commented by the reviewer #1, we explain the comments from the following two aspects: model calibration which will point out the method how the parameters were adjusted as well as what the calibration period was, and model validation which will point out what the validation periods were.

For model calibration, the calibration period is 1996 to 2000. The model is calibrated using daily and monthly discharge processes observed at Yingluoxia hydrological station, and the main calibration parameters include saturated soil hydraulic conductivity, groundwater aquifer hydraulic conductivity and specific yield, infiltration coefficient of riverbed material as well as the Manning roughness. There are three calibration rules: (1) minimizing the average annual discharge error during the simulation period; (2) maximizing the Nash-Sutcliffe efficiency coefficient; (3) maximizing the correlation coefficient between simulated discharge and observed values. The parameters were adjusted using the “trial and error” method according to the above three rules: firstly, determine the initial value according to physical properties, experimental data and reference data; then adjust the parameters. Specifically, saturated soil hydraulic conductivity, groundwater aquifer hydraulic conductivity and specific yield as well as riverbed infiltration coefficient were adjusted according to soil types; the Manning roughness was adjusted according to different land use types; snow melting coefficient, melting critical temperature and frozen soil hydraulic conductivity were adjusted according to discharge hydrograph.

After model calibration, keeping all model parameters unchanged, model validation for monthly discharge process was conducted in two periods, Period 1: 1982 to 1995, and Period 2: 2001 to 2002. In other words, there are two validation periods due to the lack of observed monthly data between them. Similarly, due to the lack of observed daily data during 1982 to 1989, the two validation periods for daily discharges process were, Period 1: 1990 to 1995, which is different from that of monthly discharge process, and Period 2: 2001 to 2002.

Comments

- Abstract, line 6: hydrological and energy cycle. This should be referred to either as the hydrologic cycle, or the water and energy cycle. The hydrologic cycle encompasses both the water and energy cycle.

Response

As commented by the reviewer #1, we have made the correction in the manuscript.

(i.e., “hydrological cycle” is changed to “water cycle”)

Comments

- Page 2191, line 5-8. There are statements that really should be substantiated by references.
- Same page, line 16-18: same remark, references are needed.

Response

As commented by the reviewer #1, some references were missed, and we have added them in the manuscript.

(i.e.,

Wang, Z., Liu, C., and Wu, X.: A review of the studies on distributed hydrological model based on DEM. Journal of Natural Resources, 18(2),168-173,2003

Wang, Q., and Luo, G.: The main reasons for ecological deterioration in the Heihe River Basin and control countermeasure. Gansu Agr. Sci. and Techn., No.12, 3-5,2002)

Comments

- Page 2192, line 3: Please spell HBV in full the first time it is used and provide a reference.

Response

As commented by the reviewer #1, the full name of HBV is given the first time it is used and a reference is provided in the manuscript.

(HBV: Hydrologiska Byråns Vattenbalansavdelning (Hydrological Bureau Waterbalance-section)

Reference:SMHI. The HBV-96 Model [R] .1996)

Comments

- This is a remark that can be made regarding almost all equations: when the variables are explained, units or dimensions should ALWAYS be provided. For most equations this is not the case.

Response

As commented by the reviewer #1, we have provided units or dimensions of the variables in the manuscript.

Comments

- Page 2196, line 12-14: "The net radiation and soil heat flux corresponding to the saturated vapor pressure are used in the Penman equation while the actual soil may be unsaturated". This statement is not true. The Penman equation uses measured values of the net radiation and soil heat flux, which are consistent with reality. Maybe I understand it wrong, but then more explanation is needed.

Response

The Penman equation was proposed to compute the evaporation from an open water surface or the potential evaporation from a saturated soil layer. Our detailed explanation is referred to in Appendix A.

Comments

- Page 2197, line 7-8: "In the water body group, surface runoff is estimated as precipitation minus evaporation". This means that, if it rains on a lake, and if the rain rate is higher than the evaporation rate, there is no increase in storage in the lake, but the excess rainfall is removed as surface runoff. This makes no sense. Again, maybe I am understanding it wrong, but in that case, please provide a better explanation.

Response

We mean "In the water body group, surface runoff is estimated as precipitation minus evaporation, and the estimated surface runoff is added to the water body storage".

The revision has been made in the manuscript.

Comments

- Same page, line 24: what is a "left period"?

Response

As commented by the reviewer #1, “left periods” is changed to “the remaining periods”.

Comments

- Page 2200, equation 23. Where does this equation come from? What is "slope"? This does not look like the Darcy equation, which is the basic equation for groundwater flow. Please provide more explanation.

Response

As commented by the reviewer #1, this is not the Darcy equation, but a formula put forward in our study to approximately calculate the subsurface runoff into river from unsaturated soil layer in mountainous and hilly areas. Slope means the land surface obliquity from a horizontal line. We have provided more explanation in the manuscript.

Comments

- Page 2202, line 6: What is the amount of water "transferred" from the snow?

Response

As commented by the reviewer #1, "transferred" is revised as "transformed".It means the amount of water that would be obtained if the snow should be completely melted.

Comments

- Same page, line 8: What is a model "debugging" parameter? Does this mean a calibration parameter? This also comes back on page 2211, line 15.

Response

Yes. It means a model calibration parameter, and the revision has been made in the manuscript.

Comments

- Page 2209, line 6-8: Please provide the equation for rainfall.

Response

As commented by the reviewer #1, we have provided the equation in the manuscript.
(y=0.0042x-2.9922, where x is the elevation (m), and y is the daily average rainfall (0.1mm))

Comments

- Page 2210, section 3.2.1. More explanation is needed on the calculation of the soil moisture characteristics, and on the development of the empirical formula.

Response

As commented by the reviewer #1, we have provided some relevant research results in the Heihe River Basin adopted in the study as follows, and the corresponding revision has been made in the manuscript.

In this study, soil moisture characteristics parameters are calculated based on following soil moisture movement experimental research results in the Heihe river basin and adjacent regions: soil moisture movement in the irrigated areas has been studied in the middle reaches of the Heihe river basin (Cao et al., 2000); the soil moisture parameters have been studied in the diversifolia area in the lower reaches of the Heihe river basin (Zhu et al., 2002); the soil moisture movement parameters have been studied in the Qinwangchuan irrigated area (Wang et al., 2002).

The relation of soil moisture content θ and suction S (negative pressure head) is expressed as (Haverkamp et al.,1977):

$$\frac{\theta - \theta_r}{\theta_s - \theta_r} = \frac{\alpha}{\alpha + (\ln S)^{\beta}} \tag{55}$$

where θs = saturated moisture content; θr = residual moisture content; α, β = constants.

The relation of unsaturated hydraulic conductivity k(θ) and soil moisture content θ is expressed as (Mualem,1978):

$$k(\theta) = k_s\left(\frac{\theta - \theta_r}{\theta_s - \theta_r}\right)^n \tag{56}$$

where ks = saturated hydraulic conductivity.

In addition, according to the soil composition and soil moisture content, soil thermodynamic properties parameters under different soil moisture conditions are established based on empirical

formula as shown in E.q. (55, 56, 57, 58) and Table2.
The soil heat capacity c_h (106Jm-3K-1) (Kondo 1994) and the soil heat conductivity k_h (Wm-1K-1) (Chung & Horton 1987) can be expressed as :

$$c_h = c_{hm}(1-\theta_s) + c_{hw}\theta \qquad (57)$$

$$k_h = 0.243 + 0.393\theta + 1.534\theta^{0.5} \qquad (58)$$

In the above two equations, chm is the heat capacity of mineral composites, chw the water heat capacity, θ the soil moisture content and θs the saturated moisture content.

Table 2 Thermodynamic properties parameters of soil and other mediums

type	heat capacity ch (Jm-3K-1)	heat conductivity kh (Wm-1K-1)	damping depth d0 (m)
water	4.18E6	0.57	0.061
dry soil	1.3E6	0.3	0.08
wet soil	3.0E6	2.0	0.135
asphalt	1.4E6	0.7	0.117
concrete	2.1E6	1.7	0.149
air	0.0012E6	0.025	0.756

Comments
-Sections 3.2.2. and 3.2.3. also need more explanation, they are unclear.
Response

For Section 3.2.2 Groundwater aquifer, hydraulic conductivity and specific yield in the Zhangye Basin are deduced from groundwater simulation and geological exploration data. The saturated soil layer parameters in mountainous areas are deduced from runoff simulation process, hydraulic conductivity is set as 65.5m/month multiply the thickness of soil layer, and specific yield is set as 0.05.

For Section 3.2.3, for four vegetation types, main parameters including vegetation coverage rate(veg), leaf area index(LAI), vegetation height(hc), root depth(lr), and minium lobular impedance(r_{smin}) are shown in Table 3. Aerodynamic parameters for vegetation and other surface covers are shown in Table 4.

Table 3 Vegetation parameters

		Month											
Type	Parameter	1	2	3	4	5	6	7	8	9	10	11	12
Wood	Veg	0.2	0.2	0.3	0.4	0.6	0.7	0.8	0.8	0.7	0.5	0.3	0.2
	LAI	2.0	2.0	2.5	3.5	5.0	5.5	6.0	6.0	5.5	4.5	3.5	2.0
	hc(m)	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0
	lr(m)	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0
	rsmin(s/m)	250											
Grass	Veg	0.1	0.1	0.2	0.3	0.5	0.7	0.8	0.8	0.6	0.4	0.2	0.1
	LAI	0.5	0.5	0.6	1.0	1.5	1.8	2.0	2.0	1.6	1.2	0.6	0.5
	hc(m)	0.1	0.1	0.1	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.1	0.1
	lr(m)	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5
	rsmin(s/m)	250											
Shrub	Veg	0.2	0.2	0.3	0.4	0.6	0.7	0.8	0.8	0.7	0.5	0.3	0.2
	LAI	2.0	2.0	2.5	3.5	5.0	5.5	6.0	6.0	5.5	4.5	3.5	2.0
	hc(m)	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0	5.0
	lr(m)	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.5	1.5
	rsmin(s/m)	250											
Crop	Veg	0	0	0.1	0.6	0.7	0.8	0.9	0.8	0.6	0.1	0	0
	LAI	0	0	0.1	0.5	2.0	4.0	6.0	6.0	5.0	1.0	0	0
	hc(m)	0	0	0.1	0.5	0.5	1.0	1.0	1.0	1.0	1.0	0	0
	lr(m)	0	0	0.1	0.5	0.5	1.0	1.0	1.0	1.0	1.0	0	0
	rsmin(s/m)	150											

Table 4 Aerodynamic parameters for surface covers

Type	zom (m)	zov=zoh (m)	d (m)
Wood	0.123hc	0.1zom	0.67hc
Grass	0.123hc	0.1zom	0.67hc
Shrub	0.123hc	0.1zom	0.67hc
Crop	0.123hc	0.1zom	0.67hc
Water body	0.001	0.001	0
Soil	0.005	0.005	0
Urban surface	0.1	0.1	0
Building	0.30huc	0.1zom	0.30hc

The corresponding revision has been made in the manuscript.

Comments
- Page 2213, line 15. Figure 5 does not show a discharge comparison, it shows a land use map.
Response
As commented by the reviewer #1, this is a mistake, it should be “Figure 8” here, and we have made the correction in the manuscript.

Comments
- Page 2215, line 5: is water "assumption" the same as water "consumption"?
Response
As commented by the reviewer #1, this is a mistake, it should be “consumption” here, and we have made correction in the manuscript.

Comments
- Page 2218, line 13-15: I have a big problem with the statement that this type of model can be applied to an ungauged basin. If one looks at the amount data needed to run this model, it is fair to say that it cannot be applied to an ungauged basin at all!
Response
Since the model is based on physical processes, and all the parameters have physical meaning, combining with GIS data and remote sensing data, we think the model has the potential to be applied in poorly-gauged basins.

Comments
- Are all the figures from figure 11 on needed? They have not been validated at all.
Response
As commented by the reviewer #1, we have deleted the figures except figure 11.

Comments
- Page 2194, line 13: "heat conduction" should be "heat flux".
- Same page, line 15: lambda is the latent heat of vaporization
- Page 2202, line 20: Hydraulic "conduction" should be hydraulic "conductivity".
Response
According to the suggestion of Reviewer #1, all of above mistakes have been corrected in the text.

APPENDIX A A MODIFIED PENMAN EQUATION FOR ACTUAL EVAPORATION FROM SOIL

A.1 TRADITIONAL PENMAN EQUATION

According to the theory of turbulent diffusion, sensible heat flux H and latent heat flux lE (from land surface to air) can be expressed as follows:

$$H = \rho_a C_p \frac{T_s - T_a}{r_a} \tag{A.1}$$

$$lE = \frac{\lambda \rho_a \varepsilon}{p_a} \cdot \frac{e(T_s) - e(T_a)}{r_a} \tag{A.2}$$

And the energy balance equation on land surface is:

$$R_n = H + lE + G \tag{A.3}$$

By assuming that the vapor is saturated at evaporation surface (e.g. water surface), namely $e(T_s)= (e_s(T_s)$, the difference of T_s and T_a is not too big and combing the above three equations, Penman suggested the following equation to compute potential evaporation:

$$E_p = \frac{(R_n - G)\Delta + \rho_a C_p \delta e / r_a}{\lambda(\Delta + \gamma)} \tag{A.4}$$

In the above equations, R_n is the net radiation, G the heat flux conducted into the soil of land surface, T_s the land surface temperature, T_a the air temperature, $e(T_s)$ the land surface vapor pressure and $e(T_a)$ the air vapor pressure, δe the air vapor pressure deficit $(e_s(T_a)-e(T_a)$, p_a the atmosphere pressure, Δ the gradient of saturated vapor pressure vs. temperature, ρ_a the air density, C_p the air specific heat, λ the latent heat of vaporization, r_a the aerodynamic resistance, and ε the ratio of the vapor molecular weight to the dry air molecular weight (0.622), $\gamma = C_p p_a / \varepsilon\lambda$ the psychrometric constant.

To estimate actual evaporation E_s from a soil layer, an evaporation coefficient k is usually used, i.e.:

$$E_s = kE_p \tag{A.5}$$

A.2 A MODIFIED PENMAN EQUATION

If the purpose is just to estimate actual evaporation, the above method may be enough. However, if the heat flux partition at soil surface needs to be conducted like the case of land surface parameterization, the above method has a defect from theoretic aspects. Because R_n-G required by A.4 should be correspondent to the saturated vapor pressure of land surface soil or water surface, however, the soil is usually unsaturated in most cases. If we use Equation A4 to compute the potential evaporation, we need compute R_n and G correspondent to the saturated vapor pressure which are unknown if an assumed scenario is not simulated. If we use Equation A5 to compute the actual evaporation, let alone ambiguity and uncertainty of the evaporation coefficient k, we need compute R_n and G correspondent to the actual unsaturated soil again to keep a correct flux partition at unsaturated soil surface.

In the existed studies, measured values of the net radiation and soil heat flux are used when applying the Penman equation, which does not satisfy the assumptions of the Penman equation or implicitly think the difference between the net radiation and soil heat flux of saturated soil and the measured ones are not obvious. To make a theoretic consistence, the Penman equation is modified here to compute directly actual evaporation from unsaturated soil.

There are two methods commonly used in land surface parameterization schemes to consider the vapor deficit or the specific humidity of soil moisture (to consider actually the soil resistance to evaporation), namely the α method and the β method (Lee and Pielke, 1992). Here α and β are called as the wet functions.

According to the α method, $e(T_s) = \alpha\, e_s(T_s)$ (A.6)

According to the β method, $e(T_s)- e(T_a) = \beta\,(e_s(T_s)- e(T_a))$ (A.7)

Here the β method is adopted to derive a modified Penman equation to compute actual evaporation E_s from soil directly. The derivation is as follows:

Substitute A.7 into A.2 , we have

$$lE = \frac{\lambda\rho\varepsilon}{p_a}\cdot\frac{e(T_s)-e(T_a)}{r_a} = \frac{\beta\rho C_p}{\gamma}\cdot\frac{e_s(T_s)-e(T_a)}{r_a}$$
$$= \frac{\beta\rho C_p}{\gamma r_a}\left[e_s(T_s)-e_s(T_a)+e_s(T_a)-e(T_a)\right]$$

$$= \frac{\beta\rho C_p}{\gamma r_a}\left[\Delta \cdot (T_s - T_a) + e_s(T_a) - e(T_a)\right] = \frac{\beta\Delta}{\gamma} H + \frac{\beta\rho C_p}{\gamma} \cdot \frac{e_s(T_a) - e(T_a)}{r_a}$$

Therefore we get

$$H = \frac{\gamma}{\beta\Delta} lE - \frac{\rho C_p}{\Delta} \cdot \frac{\delta e}{r_a} \tag{A.8}$$

Substitute A.8 into A.3, we have

$$R_n - G = (1 + \frac{\gamma}{\beta\Delta}) lE - \frac{\rho C_p}{\Delta} \cdot \frac{\delta e}{r_a}$$

At last we obtain

$$E_s = \frac{(R_n - G)\Delta + \rho_a C_p \delta e / r_a}{\lambda(\Delta + \gamma / \beta)} \tag{A.9}$$