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Interactive comment on "V2Karst V1.0: A parsimonious large-scale integrated vegetation-recharge model to simulate the impact of climate and land cover change in karst regions" *by* Fanny Sarrazin et al.

Anonymous Referee #2

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The paper proposed by Sarrazin et al. aims at adding a new evaporation formulation to the recharge model VarKarst which specialises on the hydrology of karst systems. The aim of this development is to make the model suitable for exploring the impact of climate and land surface changes on these very sensitive hydrological structures. The main themes of these improvements are to be applicable at the large scale and to be parsimonious.

I believe this model fails on both accounts for a simple reason, the authors have neglected the fact that evaporation is strongly controlled by the diurnal cycle of radiation

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and atmospheric processes. One of the main consequences of climate change is to modify the diurnal cycle at the surface and in the atmosphere. Thus the application of V2Karst to climate change is bound to produce unrealistic sensitivities. The model would be more parsimonious and more robust (because based on stronger physical grounds) if it would explicitly represent the diurnal cycle. Furthermore this enhancement of VarKarst neglects 30 years in the developments of land surface models. These models do not represent hydrological processes and even less karst systems, and are rightfully criticized for this. But they have specialized on the surface/atmosphere exchanges and in particular the simulation of evaporation, vegetation processes and infiltration. At no moment do the authors refer to developments in one of the three leading land surface models (JULES, ORCHIDEE and CLM) or their application to the 4 FLUXNET stations used here. A simple Google search would have shown to the authors that these open-access codes (Thanks in great part to GMD !) perform much better on these sites and do not require the tuning of so many parameters. Furthermore they are designed to be applicable at the large scale.

I would recommend to reject the paper and encourage the authors to download one of the above mentioned land surface models and couple it to VarKarst. This would produce a model for these sensitive hydrological regions which is much more robust and produces more credible result for the impact of climate and land-cover changes.

I am sorry to have to make such a harsh recommendation to GMD and in the following I will detail where I believe the basic assumptions of the authors to be wrong and where the usage of developments made for land surface models would help.

Rational to explicitly represent land cover properties :

It is laudable for the authors to use the Penman-Monteith formulation for potential evaporation. But should they have paid attention to its derivation, they would have noted that it only provides potential evaporation over a infinitesimal time intervals as it assumes that atmospheric variables and surface states do not evolve through other processes.

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A constant Rn(t) or ra,can(t) over the course of the day is a very unsatisfactory assumption, especially under a changing climate. Because of very contrasted impact of changing atmospheric composition on long-wave and short-wave radiation, we can encounter the same Rn but with very different radiation balance, turbulent fluxes and surface temperatures. The authors will find in the literature a number of paper which examine the impact of climate change on the different potential evaporation formulation. They all recommend to use sub-diurnal solutions because of the modified diurnal dynamics.

The parsimony of our representation of nature if not for us to choose. We have to prove that certain simplifications in the representation of surface processes are valid for the application we envisage. The authors aim to develop a model valid at the large scale, for climate and land surface change. Is it then reasonable to assume that over the course of a day ra,can does not change ? I think the development of land surface models has shown that one cannot neglect the diurnal dynamic of the opening of the stomata, the soil moisture stress or the dependence of stomatal resistance to atmospheric CO2 concentration. If the authors believe that they have found a way to represent with a single daily value these complex processes and their interaction with the environment they should let the world know as it would allow land surface models to be simplified.

In their rational for their modelling strategy they only mention one land surface model : ISBA in its 1998 version. This is not up to date. Even ISBA has evolved since then and does not use any more a Jarvis type parametrisation. It now also uses a Ball-Berry type formulation which balances carbon uptake and transpiration. Please note that ISBA operates at sub-diurnal time steps.

Soil water balance :

The explanations of the evolution of moisture in the unsaturated zone is not very clear to me. It looks to me like a superposition of buckets with the addition of lateral flows. It

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has been the experience in the land surface model community that this simple representation of soil moisture limits the ability to simulate the impact of stresses on transpiration. This is particularly critical in semi-arid those encountered at 3 of the selected FLUXNET stations. What is the reasoning of the authors behind this simplification in the treatment of the unsaturated zone, apart from “parsimony” ?

Evapotranspiration :

Only one vegetation type seems to be allowed per grid-box, is this correct ? Because of the strong heterogeneity of the distribution of vegetation, it has been the experience of the community that a larger number of plant functional types is needed per grid box. The strict minimum has been found to be a low and a high vegetation. This simplification will be critical for the application to larger domains and in particular in semi-arid regions where the competition of the various vegetation types for water is critical. Please explain here as well why the literature on vegetation modelling is not relevant for this model.

Canopy interception :

This is another topic where the community has acquired a rich experience which could benefit the authors. The representation of canopy interception at different temporal and spatial scales has been fiercely debated in the early 90s. Thus a number of parametrisations were developed to take into account the spatial and temporal variability of interception. This would be relevant here.

Do the authors believe that a rainfall event in the evening or in the morning produces the same interception loss ? Does a rainfall event of 10mm/h and 100mm/h produce the same interception ? So does the assumption of treating these processes averaged over the day have any implication on the sensitivity of V2Karst to climate change ? We know that rainfall intensity and possibly also the time of day at which precipitation will occur will change in a warmer climate.

May I point out at this stage that precipitation intensification has been observed at the sub-diurnal range. Daily mean rainfall has not yet been too much affected by climate change. On the other hand, hourly precipitation rates have been increasing faster than expected from the Clausius-Capleyron relation. Thus, the virtual experiments experiments proposed in section 4.3 are not relevant for climate change. The authors are referred to the wealth of literature published on this topic in the last few years.

Transpiration from vegetated soil :

Transpiration does not occur from the soils (as written in the paper) but from the stomata in the vegetation. This is not a negligible detail. Firstly the stomata only open when daylight is present and thus photosynthesis can occur. During the early afternoon, once the water in contact with the roots and within the plant has been evaporated, transpiration declines. This is caused by the slower diffusion of water within the soil which limits the supply. This is known to be a critical process for transpiration and which will be affected by higher CO2 concentration which will lead plants to reduce the opening of their stomata. I guess these processes are neglected in the proposed model, why ? It would be a very interesting topic to see how this early afternoon depression of transpiration is affected by climate change for plants on karstic soils. It is bound to be different than on loamy soils for instance.

Sorry, the assumption "... evaporation from interception is constant throughout the day ..." is not valid and will change with climate and land surface type.

Parameter estimation :

The proposed parameter estimation is difficult to interpret in view of the strong hypothesis made in the basic equations of the model. The 15 parameters of this model are so conceptual, i.e. far away from first physical principles, that indeed they can all be tuned. But given the large number of "tunable parameters" can it not be expected that the model can be made to match any dataset ? To me hveg, LAI(min,max) or z0 are not "tunable parameters" as they can either be measured or derived from turbulence

theory. Furthermore I find that the range of values explored for these parameters (Table 3 does not provide the limits for all 15 parameters) is much wider than realistic values I have observed.

Land surface models also use the FLUXNET observations to “tune parameters”. But fewer parameters are adjusted and only those where the definition itself includes processes which are not modelled, i.e. are conceptual. Furthermore these parameters are specific to the plant functional type present at the FLUXNET station and then then transferred to the larger scale. This is the value of using vegetation classes in land surface models.

A simple internet search for FLUXNET and the name of one of the leading land surface models, returns a large number of papers. Some where the models are simply validated and others where the observations are used to refine some vegetation parameters. The authors should have done that search during the development of their model.

Conclusion :

I am very sorry to have to write this review about the development of V2Karst. I know what a huge effort it is to develop a complex numerical model. As the authors are working in Britain, I would recommend that they look into the JULES land surface model. It is freely available and could be coupled to VarKarst to produce a very innovative tool which could indeed allow to explore the consequences of climate and land surface changes on water resources of karst aquifers. This need, to initiate a convergence between hydrological and land surface modelling, has been recognized by NERC and lead to the initiation of the HydoJULES program. The authors should contact the leaders of this program to obtain help.

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