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Not easy to extrapolate general results

Anonymous Referee #1

Referee comment on "Simulation of long-term spatiotemporal variations in regional-scale groundwater recharge: contributions of a water budget approach in cold and humid climates" by Emmanuel Dubois et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-71-RC1, 2021

The manuscript "Simulation of long-term spatiotemporal variations in regional scale groudwater recharge: contributions of a water budget approach is southern Quebec" by Dubois et al. submitted to HESS-discussion presents a physically based model aiming at evaluating the groundwater recharge on height watersheds in southern Quebec. The model, based on some simplifications of the atmosphere-soil processe interactions is calibrated on flow data gathered over the period 1961-2017 at stations located on the surface water network draining the groundwater regional flow.

Here below my general comments and afterwards some minor remarks.

- Lines 172-175. The Authors mention three basic hypothesis. I have serious concerns on the third one: the watershed response time is shorter than one month, thus compensating for the absence of water routing. This is a very strong hypothesis, as it allows neglecting transient dynamics of the aquifer. Before proceeding with any computation, Author should demonstrate and convince the reader that this a reliable assumption.
- The model accounts for 8 parameters to be calibrated. It seems to me that some of them are set of parameters. For example the Runoff factor (as explained by the Authors "Partitioning between runoff computed with the RCN method and infiltration into the soil reservoir") do depend also on the land cover or not? in the first case, you are calibrating each runoff factor for each soil. Am I wrong? Maybe I do not understand correctly
- The coupling of time steps (daily time step for soil modelling and monthly time step for GWR) is not clear to me. Please, give more details on that
- The targets for calibration are both the total surface flow and the baseflow. However, the first is observed, whereas the second is estimated (through the Lyne and Hollick filter). In my opinion this is a weak point of the whole calibration procedure: generally speaking I do not like to calibrate a model using output of another model. Even if they match each others, what can I say on the reliability of both? The Authors should at

least convince the reader on the reliability of the baseflow estimates.

- The previous one is a very important point, also because the objective function (eq. 1) is a linear combination of two different metrics, one referring to the total flows, the second one only to the baseflows. In my opinion, dependence of the calibration on the weights adopted to define the objective function should be explored much more in details. The explanation you gave for your choice (lines 203-205) is too simplistic.
- I found the sensitivity analysis performed on the W6 group of gauging stations very interesting and potentially the core business of the paper (which otherwise it is only a modelling study, important for Quebec, but not interesting for the international reader of HESS). Why the Authors decided to carry out the sensitivity analysis only on one group of gauging stations. In my opinion, it could be much more interesting to perform the same analysis to several groups of gauging stations to explore the possible differences among watershed and the dependences on climate forcing, soil, land cover etc In other words, has the ranking proposed for W6 a general validity? why?
- Presentations of the results are sometimes not easily readable. For example, in section 4.3 one can find several information already shown in figure 6 and table 4). I think that the description of the results (sections 4.2, 4.3, 4.4) can be much simplified. On the contrary, results on the temporal evolution (analysis of trends) deserves for much more attention and a dedicated picture to presents results.
- The entire discussion on the temporal patterns of groundwater recharge relates the time variation of meteorological forcing to discharge, neglecting possible changes in the land cover. I do not know if such an assumption is correct, however it should be explicitly stated
- Results of figure 6 are surprising: proportion between runoff, AET, GWR does not depend on the watershed (differences among watersheds are in the order of 1-2%). Figure 5(b) shows different patterns: for example over W2 GWR/P spans between 0.1 and 0.15 for most of the grids; on the contrary, over W4, it seems that GWR/P is >0.3 for half of the cells. Maybe I missed something, but it does not seem to me that Fig 5 and Fig 6 are coherent.

Minor remarks:

- Line 102-109 All the information given here are summarized in Table 1. This lines appear to me not useful
- Line 112. In my opinion it would be better to add also the mean bias as metrics of the goodness of interpolation, to avoid systematic under or overestimation
- Figure 1. In my opinion the map in the middle is not useful: I suggest to eliminate it, retaining the location map and the watersheds map.
- Figure 4. As GWR is your main output, I would present several graphs as the panel (b) for several stations (one as an example is not useful)

As a general comment, I found this manuscript too much focussed on the specific study area: no substantial new concepts, ideas, or methods. Moreover, I have some concerns on the way to present the results: I suggest the Authors, in case of resubmission to be much more concise.

Based on the remarks presented above, I suggest the editor to reconsider the manuscript after major revisions. I warmly suggest the Authors to “fly higher”: the work done is a good basis for a scientific paper to be published on HESS, but some hints are not adequately developed to be interesting for a wider audience.