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Interactive comment on "A novel method for cold region streamflow hydrograph separation using GRACE satellite observations" *by* Shusen Wang et al.

Anonymous Referee #2

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General comments: The research on baseflow separation has a long history and there have been a variety of methods as summarized by Pelletier and Andreassian (2020). This study proposed a new method for snow-dominated regions that bases on watershed-scale water balance approach. Apart from streamflow observations, it relies on the water storage change observations from GRACE, air temperature and the watershed snow water equivalent observation from assimilated dataset. Through comparing with the other six hydrograph separation methods, the proposed method provides the most conservative estimate of baseflow in light of consideration of snowmelt runoff. However, the method is built on some strong assumptions, which must be consolidated by further evidence. Overall, the manuscript is well written. But the following issues

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Discussion paper

should be addressed properly before the paper can be considered for publication in the HESS. Thus, the paper needs a major revision.

Major comments: 1. As one strong assumption of the physically-based method, the watershed water balance closure can be captured by the suggested observations. First of all, the surface water budget could be closed with detailed observations, but the groundwater water system may not be closed in the watershed. Secondly, the authors did not provide the uncertainties of all components, particularly the non-dischargeable water change (Sn). Thus, more discussion is needed for this assumption. 2. Another implicit assumption is the invariant model parameters (k0, Tc, and a). If my understanding is correct, the authors only derived the three values from the model calibration shown in Table 1. I am not sure if they use the 15-year data for the calibration. If so, this means the baseflow mechanism is stable over the years. In fact, the baseflow mechanism could be influenced by yearly weather changes. If not, please provide the variation of the yearly calibrated parameters. Furthermore, in light of the contrast hydrological mechanisms between the winter season and the summer season, the assumption of the invariant model parameters within a hydrological year might be also problematic. More evidence is needed for this assumption. 3. The authors declare the proposed method yields the most conservative estimates of baseflow in comparison with other methods. However, since the model parameters are derived from the winter season, systematic bias might come from here when using them for the snowmelt season and the summer season. 4. This novel method was only applied in one watershed, so the applicability for other large size watershed was not examined. And the conclusion that filtering out snowmelt runoff bias in baseflow estimates was not strictly tested and quite occasional, it could be the possibility that this model underestimated baseflow. One possible reason is that using the single winter data to calibrate parameters, in fact, the runoff components in winter were quite different from those in spring/summer.

Minor comments: 1. Line 54-55: Is baseflow primarily driven by the subsurface drainable water storage in all situation watersheds? Please be more specific. 2. Line 86 and

Line 97: Is surface water which is under the surface water holding capacity contribute to baseflow, in some cases it could turn into non-dischargeable water. 3. Line 126: The nested numerical iteration scheme was not clarified. 4. Line 130: The dimensions were inconsistent in Eq. 4, where Stot [mm] vs Qobs [mm/day]. Please clarify this. 5. Line 211: high evapotranspiration, please quantifying it. 6. Line 246: Why did you artificially set and thought that BFI is 1.0 in the winter season, while all other models estimated that surface flow also existed in winter. Please find these paper: Streletskiy, D.A., Tananaev, N.I., Opel, T., Shiklomanov, N.I., Nyland, K.E., Streletskaya, I.D., Tokarev, I., Shiklomanov, A.I., 2015. Permafrost hydrology in changing climatic conditions: Seasonal variability of stable isotope composition in rivers in discontinuous permafrost. Environ. Res. Lett. 10, 95003. https://doi.org/10.1088/1748-9326/10/9/095003. 7. Please discuss more quantitively the advantages and disadvantages of your model compared with others. 8. Line 330-335: Lack of logic. Not convinced. 9. Line 351: any progress in recent hydrograph separation applied in large size watersheds? 10. Line 355-356: No support information.

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