Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/hess-2021-240-RC1, 2021

Comment on hess-2021-240

Anonymous Referee #1

Referee comment on "Vegetation greening weakened the capacity of water supply to China's South-to-North Water Diversion Project" by Jiehao Zhang et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-240-RC1, 2021

In this manuscript the authors aim to isolate the effects of vegetation greening on water yield in a large basin that serves as donor basin for a major water diversion project. To do so the authors designed a modelled-based scenario analysis. The overall finding of their analyses suggests that greening has the potential to considerably reduce basin water yield and thus supply for the water diversion project. While the experimental set-up is systematic and, in principle, logical and the manuscript is well written, I nevertheless have a number of serious concerns that need to be addressed and resolved in detail before this manuscript could be considered for publication.

(1) The experiment is designed, the results are interpreted and, as a consequence the manuscript is framed from a purely engineering perspective with focus on water yield available for the diversion project. As a consequence, the non-explicit message that is delivered between the lines here is the following: to secure water supply for the diversion project greening needs to be reduced. Or in other, more explicit words: stop afforestation - chop down the forest! I am not sure that this can and should be the message to be conveyed here for the simple reason that this analysis is not comprehensive enough to draw this conclusion. Little explicit consideration is given to potentially relevant feedback effects of greening the water cycle. This includes the potential for increased local precipitation recycling (P), reduced vapour pressure deficits (VPD), temperatures (T) reduced through increased latent heat flux, which in turn affect water partitioning and thus water yield. These points need at least to be discussed in substantial detail and the conclusion needs to explicitly state these limitations.

(2) Linked to (1) is the design of scenarios S2 and S3, which may indeed contain a fallacy. By fixing NDVI and land cover to the values of 2001 (S1) and 2018 (S2), respectively, the authors aim to isolate the "real" and potential effects of greening. This is in principle ok. BUT: it is not clear from the description of the experiment how the related feedbacks in P, T, VPD and even radiation (changes in albedo!) were accounted for. As far as I understood, the assumption for the 2 scenarios was that *only* NDVI and land cover is

fixed to the values of the 2 individual years. If this is so, the authors overlook that the observed past P, T, VPD and radiation data already account for changes in NDVI and land cover. What are the effects of that? Do the results then really allow to isolate effects of greening? If I am mistaken here, then I would nevertheless ask the authors to makes this much clearer in the description of their experiment.

(3) Linked to (2), the description of the models and their actual implementation does not provide sufficient detail to allow the reader to meaningfully assess the results and interpretations. For example, it is completely unclear how NDVI, land cover but also soil data were used. The reader can only assume that these data are somehow used to estimate the variables PAR and FPAR (Eq.1). Even if this is described elsewhere in detail, it will be necessary to provide this crucial information here as well. In addition, it remains completely opaque, which parameters the two models required and which of those had to be calibrated and which were a priori fixed (e.g. from look-up tables). How were the models calibrated? Was the calibrated model tested on independent data (which should be a rather standard procedure in the year 2021)?

(4) Linked to (3), no attempt, whatsoever is made, to estimate the uncertainty around the models, their parameters and the associated results. Not even confidence intervals around the regressions (and the underlying parameters) are given. Quite frankly, I find this very surprising, as this should be part of any meaningful and serious scientific protocol.

Additional specific comments:

- 2, l.42: this is a completely non-sensical use of the term "drought". The term "drought" is always refers to a negative anomaly with respect to a specific local reference value, defining a "normal", typically a median. By convention, conditions below this normal are then defined as "drought". By extension, there can then be no location with more "frequent" droughts than other locations, as drought is always the deviation from the local/regional normal.
- 2, l.50-51: if afforestation was meant to safeguard water availability, then this is in contradiction with l.55-56. Please rephrase.
- 2, l.56: greater leaf area in itself does of course not increase transpiration. Vegetation metabolic activity increases transpiration. Leaf area is merely an indicator for increased metabolic activity and thus transpiration.
- 2, l.61: should read as "…are not feasible…"
- 3, l.69: what are "hydrological entities"?
- 3, l.76: droughts are low frequency phenomena that require considerable time to develop and to recede. The 18 years of this study are thus likely not enough to make a meaningful statement about changes in drought regimes.
- 4, Figure 1: please also show the location of the reservoirs and hydroelectric facilities.
- 6, l.130-131: irrelevant, can be omitted
- 6, l.132-133: not clear what is meant here. Which other models?

- 6, l.138: more detail is needed for this choice here. Why 45%? How sensitive is the model to this choice?
- 6, l.139-140: how were PAR and FPAR determined?
- 6, l.139-150: Much more detailed is needed on which parameters these models feature and how the parameters were determined, including their prior distributions and the calibration strategy applied.
- 7, l.163: R2 and NSE have a very similar information content: NSE collapses to R2 in the absence of a bias. Thus, I am not sure of the added value of using R2 as performance metric here.
- 7, l.164: reliable? Many would argue otherwise (e.g. Schaefli and Gupta, 2007, HP). In addition, what does "reliable" actually mean here?
- 10, Figure 4: given that the model only provides monthly estimates of water yield, the model does not do a particularly good job in reproducing the observed water yield, in particular for the 2012-2014 period. What is the implication of this? What are the uncertainties around that? How does it affect the results and interpretation?
- 11, l.225-226: I am concerned that the change point analysis here is really very sensitive to the rather short time period considered and that the points identified here may be mere artefacts (e.g. Zhou et al., 2019; HSJ). I strongly suggest to omit this from the analysis.
- 11, l.220ff: am I right to assume that scenarios S1 and S2 are shown and discussed in this section? Please clarify and make this explicit.
- 12, 237ff: not clear what is considered here. Is it the difference between S1 and S2? If yes, I wonder how much of the correlation is spurious, as NDVI is kept constant, while still using observed T and VPD that are the result of a variable vegetation cover. This needs to be made much clearer.