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Comment on hess-2020-599

Anonymous Referee #1

Referee comment on "Spatially distributed impacts of climate change and groundwater demand on the water resources in a wadi system" by Nariman Mahmoodi et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-599-RC1, 2021

GENERAL COMMENTS:

The present paper is on “Sustainable use of water resources…”. The topic/paper is interesting but needs a number of modifications before it can be accepted.

“Sustainable/ sustainability” is mentioned several times in the text (and the title). In my understanding sustainable water resources management is not simply supplying an ever growing demand (that’s a really old, outdated approach from the last century) but an important part is water demand management. In the paper there is nothing about water demand management, but in one of their scenarios the authors simply interpolate past/observed water use/demand into the future.

Overall, the wording (or definitions) needs to be reconsidered. The authors define “sustainable/ sustainability” as state where "GWR>GWW"; i.e. "groundwater recharge" is higher than "groundwater withdrawal". A state where GWW/GWR ratio is higher than "1" is considered “unsustainable”. The authors consider GWR only; there is no information (to my understanding) on the groundwater level or volume. The authors assume that the whole water demand can be withdrawn, e.g. stating that "The rate of GWW to GWR is greater than 2..." (p. 7, lines 195/196). Without information on the groundwater level or volume, how can they say that the water volume demanded can be withdrawn? In my opinion the whole text needs to be changed (I will define demand on groundwater here as “GWD”): the authors can only calculate what volume can be “sustainably” withdrawn from groundwater. In cases where “GWD>GWR” water withdrawals will become unsustainable, i.e. water demand on groundwater resources is larger than groundwater recharge, meaning the groundwater level or volume will decline. The (or one version for) correct wording would be that “Only 50% of the water demand can be sustainably withdrawn…” instead of "The rate of GWW to GWR is greater than 2..." (p. 7, lines 195/196). I think the whole text (and Tables/Figures) needs to be changed in this direction.

It is not clear if the results (e.g. “GWW/GWR”) represented (e.g. Fig. 2) a calculated on an annual basis and then averaged over the entire (30 years) period or if it is calculated for the entire (30 years) period. If it is calculated on an annual basis and averaged afterwards, how is groundwater storage, e.g. in a very wet year GWR may be (much) higher than GWD and increase availability (increased groundwater level or volume) in the next year, considered? Also, to my knowledge, there is no groundwater flow between sub-basins in SWAT. In reality a sub-basin (region) with high groundwater withdrawals, resulting in declining groundwater levels, may receive groundwater inflow from neighbouring sub-basins (regions) without groundwater withdrawals, i.e. high(er) groundwater levels.

The paper builds on work of Mahmoodi et al. (2020a) (referenced as Mahmoodi, N., Kiesel, J., Wagner, D.P., and Fohrer, N.: Water use systems and soil and water conservation methods in a hydrological model of an Iranian Wadi system. J. Arid Land, 12, 545–560, https://doi.org/10.1007/s40333-020-0125-3, 2020a.), describing the set-up and calibration/validation of a SWAT model to the Halilrood river basin.

In a simplified way the water balance can be given as:

$$SW(t) = SW(t-1) + PRECIP(t) - SR(t) - ET(t) - PERC(t) – SSF(t)$$

where SW(t) is the soil water content at time step t, PRECIP is precipitation, SR is surface runoff, ET is evapotranspiration, PERC is percolation, and SSF is subsurface flow (with the last two variables describing groundwater-/subsurface part of the system, this part may be differentiated differently depending on the complexity of the approach/model). The important point is that there are some variables that are observed usually, i.e. PRECIP and river flow (combining SR and SSF), while for other variables seldom or only for very few locations observed values are available, e.g. ET and PERC. Assuming (for simplicity) constant SW and known PRECIP and river flow, potential and actual evapo(-transpi-)ration and PERC are unknown. Depending on the parametrisation of the model potential and actual evapo(-transpi-)ration and PERC can differ strongly. In a model calibrated/validated on stream flow only there is high uncertainty if potential and actual evapo(-transpi-)ration and PERC are simulated correctly - here either observed values, e.g. lysimeter measurements (point information), or from other sources, e.g. LAI or actual evapotranspiration from GLEAMS or MODIS (areal information) should be used to validate the model results. In the paper of Mahmoodi et al. (2020a) such a validation is not carried out and therefore I wonder how the authors can prove the reliability of their SWAT parametrisation. For instance in Table 4 (Selected parameters for calibration in the SWAT model) Mahmoodi et al. (2020a) give "EVRCH Reach evaporation adjustment factor from 0.5 to 0.8", to my understanding this means that the potential evaporation is reduce strongly by 20 to 50% (i.e. more water may percolate, become groundwater recharge)!

As given in Mahmoodi et al. (2020a, p. 551):

"WYLD=SURFQ+LATQ+GWQ–TLOSS, (1)

where WYLD is the water yield (mm); SURFQ is the surface runoff (mm); LATQ is the lateral flow contribution to stream (mm); GWQ is the groundwater contribution to stream flow (mm); and TLOSS is the transmission losses (mm), i.e., water loss via transmission through the bed of the channels." Transmission losses in semi-arid environments/rivers can be very high, introducing another uncertain variable in the simulations.

Furthermore, the paper builds on work of Mahmoodi et al. (2020b) (referenced as "Mahmoodi, N., Wagner, D.P., Kiesel, J., and Fohrer, N.: modeling the impact of climate change on streamflow and major hydrological components of an Iranian Wadi system. Water Clim. Change, https://doi.org/10.2166/wcc.2020.098, 2020b."): "The G-RCM CSIRO-SMHI was chosen since it represented the median model of the major hydrological components (Mahmoodi et al., 2020b)." The authors write that they follow the argumentation of Tebaldi and Knutti (2007) and Thober and Samaniego (2014). However, reading the paper of Tebaldi and Knutti (2007) I only find that they discuss using (weighted) averages of a number of climate models, based on the idea that the performance can be improved by averaging or combining results from multiple models. But this is that first a number of climate models are run and then the results are averaged (in the case of impact models the input from a number of climate models are used and then results of the impact models are averaged). In the study of Thober and Samaniego (2014) a number of (meteorological) indices are used to select regional climate models, i.e. reduce the number of impact model runs. In case such an analysis (to select G-RCM CSIRO-SMHI) was carried out in the present study this analysis and its results need to be described (e.g. in the Suppl. Material), otherwise the results are not replicable (I also need to mention that Thober and Samaniego (2014) reduce the number but they do not propose to use results of only ONE climate model). In the paper of Mahmoodi et al. (2020b) I don*t find any information that "G-RCM CSIRO-SMHI … represented the median model of the major hydrological components".

The use of only one climate scenario restricts the value of the paper – for a more wet or dry (climate) future results could be very different. Such results would be needed to enable more robust decisions on future water resources management, especially as the authors state that effects of climate change on surface/groundwater resources are much more significant than future water demand/use.

When reading the paper Mahmoodi et al. (2020b) I also found that minimum and maximum elevation for the river basin shown in Figures 1 (Mahmoodi et al. (2020b) and the present study) are different - please explain.

In the whole text please use "streamflow" instead of "flow" when referring to river flow, otherwise readers could be confused as the paper is on surface water and groundwater. The results given for streamflow (IHA, VRA) are all for the outlet of the basin?

SPECIFIC COMMENTS:

- Introduction:

Page 1; Lines 23: "…alteration caused by natural or anthropogenic activities…" I am not sure what is "natural … activities"; rethink formulation.

Page 2; Lines 40/41: "…precipitation... rainfall..."; please use either "precipitation" or "rainfall" in the whole text

Page 3; Line 68: "Further aggravation will put increasing pressure on the..."; "aggravation" of what (rethink formulation)?

- Materials and methods:

Page 3; Lines 80/81: "…increased over the last years at the outlet of the basin during the past 33 years..."; reformulate "…increased over the last years … during the past 33 years..."

Page 4; Lines 95/96: "Good performance for modeling daily streamflow values was achieved judged by a multi-metric approach including NSE (0.76 and 0.54)..."; according to Moriasi et al. (2007: Model evaluation guidelines for systematic quantification of accuracy in watershed simulations. Transactions of the ASABE, 50(3): 885–900) an NSE of 0.54 is considered as "satisfactory".

Page 4; Lines 114 and 116 (also line 459 "Statistics Center of Iran"): on page 114 it is "Statistics Cerner of Iran"; on page 116 it is "Statistical Center of Iran" - The homepage (https://www.amar.org.ir/english) gives the translation/name "Statistical Centre of Iran", please use the official translation/name given on the homepage.

Page 4; Lines 123/124: "To meet the future domestic, agricultural and industrial water demand, increases in the number of wells and qanats are linearly extrapolated...", so you assume that all wells and qanats have the same (water) yield? Please justify this assumption.

- Results:

Page 7; Line 199: "...unsustainable subbasins (GWW<GWR)...", unsustainable is "(GWW>GWR)"; however, I suggest to change the wording (see "GENERAL COMMENTS") and this part needs to be rewritten anyway…

Page 7; Lines 207/208: "... Among these 56 unsustainable sub-basins, GWW/GWR ratio is higher than 5 in 42 sub-basins....", this means "only 20% of the water demand can be supplied sustainably"?; change the wording (see "GENERAL COMMENTS") - all the results presented need to be changed accordingly.

Page 8; Lines 211/212: "...it drops from 385 (106 m3 yr-1) in model setup period to 172 (106 m3 yr-1)...", what is the unit of the first numbers (385; 172)?

Page 8; Lines 217: "3.1" is "Groundwater sustainability"; therefore I suggest to call "3.2" "Streamflow alteration",… give a subtitle that's clearly points to Streamflow/Surface water…

- Discussion:

Page 10; Line 282: "…substantial deficits in discharge during..."; is this "groundwater recharge" or "river discharge"?

Page 11; Line 285: "…predicted unsustainability of groundwater could..."; correct "…predicted unsustainability of groundwater use could..."?

Page 11; Line 287: "…could lead to a higher groundwater withdrawal in summer season..."; correct "…could lead to a higher demand on groundwater in summer season..."?

- Conclusion:

Needs to be adapted according to "GENERAL COMMENTS".

Supplementary material

- in Figure S1 units (e.g. for “Group 1” and “Group 2” the for x-axis "year" and for the y-axis "m^3 s^{-1}") should be given

- Figure S2 needs a better description to understand what is shown (e.g. the title states “Distribution of annual values…” but shown are monthly values; or is it - the single dots - the 30 monthly values of the 30-years per period, e.g. for January?); also here units are missing. The results shown are scattered (not ordered) – is this because the results are given according to their temporal occurrence (i.e. the year)? If yes, why a different x-axis when compared to Fig. S1?