This review was prepared as part of graduate program course work at Wageningen University, and has been produced under supervision of Prof Jos van Dam. The review has been posted because of its good quality, and likely usefulness to the authors and editor. This review was not solicited by the journal.

Comment on Egusphere-2022-796

Johan van Snippenberg

Overview & general remarks

The paper under review is a model study, in which a coupled framework of a hydrological model (HEC-HMS) and a water quality model (QUAL2K) is used to investigate the ways that a river section in the upper Ganga basin is affected by various driving factors. These drivers are, in no particular order, climate change, land use land cover change (LULC), population growth, increasing industrialisation, and sewage treatment practices. The effects of the first four 'natural' drivers are isolated, and their contribution to projected changes in water quality is estimated. Moreover, they set up several socio-environmental scenarios, each with different sewage water treatment strategies under the strain of the projected drivers. They concluded which measures will end up providing the best water quality in the river section, and whether the goals outlined by the Ganga Action Plan (GAP) could be met.

The aforementioned coupled framework has been used before by the authors to study the effects of climate change and LULC on water quality in that same stretch of the Ganga (Santy et al., 2020, 2022). It is built upon by integrating projected population and industry sewage production into the water quality model, thereby broadening its scope. To my knowledge, such a comprehensive analysis of the driving factors on the section of the river that is investigated here has not been done before. Furthermore, the societal relevance of such a study is clear, especially given that the GAP has thus far failed to reach its goals. Finally, the objectives of the paper fit the scope and interests of the journal.

The text itself is broadly reasonably well-written. However, figures consistently suffer from problems in legibility and efficiency. The drawn conclusions follow directly from the data presented, but the methodology shows crucial flaws, which leads me to question the validity of the conclusions regardless. The authors identify climate change as the greatest average contributor to the water quality degradation via its negative effects on the incoming upstream streamflow. And although the effects of LULC on the upstream streamflow are considered, the direct effects increased water demand form increasing human activities are not. Furthermore, the interpretation of the absolute change ratio as a contribution of a singular driver to the change in a water quality variable, is highly questionable.

Major comments & concerns

i) Water demand & extraction

In the methodology, there is no mention anywhere of the direct impacts that water usage by people can have on the streamflow, while simultaneously identifying climate change as the most important contributing factor to water quality deterioration via its effects on the streamflow. However, there is ample evidence that streamflow characteristics are substantially affected by water extraction(Anand et al., 2018; Fabre et al., 2015; Liu et al., 2017; Mair & Fares, 2010; Sharma et al., 2019) For instance, one paper found that all anthropogenic activities, including groundwater extraction and land use change, contributed 85%, on average, to declining runoff in the Tapi Basin in West India (Sharma et al., 2019). Lower percentages were also found elsewhere but illustrates that this aspect cannot simply be ignored if the aim is to elucidate the contributions from population growth and increasing industrialisation. What is curious, is that you do cite work that brings up the relation between population and water usage (Khattiyavong & Lee, 2019), irrigation and water extraction is a source of uncertainty (Chawla & Mujumdar, 2018), and the models used in Jin et al. (2015) explicitly take increased water demand into account (Jin et al., 2015).

Based on this, the conclusions may not accurately reflect the reaction of the system to the drivers as presented, and thus all conclusions come under question. The greatest problem I foresee, is that changes of the streamflow are underestimated, and that thus the water quality projections are better than they should be. Therefore, the conclusions on how the investigated mitigation strategies help in solving the deterioration may not be entirely correct, and the water quality may not be projected to be improved as much you now conclude. Because the validity of the conclusions is under question if this issue is not addressed, I suggest the following:

Integrate projected water use increases and the effects of that on the streamflow into the framework and do all analyses again. I am afraid that I personally am no expert in this field, so I am not able to provide much aid in this regard. Furthermore, I am not aware of any way to simulate declines in streamflow due to increased extraction in either HEC-HMS or QUAL2K, not from my cursory reading of the documentation at least. You are likely better informed on the matter than I am. I do believe that simply indicating the limits of the research with respect to this issue is insufficient, since all conclusions are of debatable validity because of it.

ii) Change Ratio

The change ratio is presented as a way of isolating the effects of the investigated drivers, but I am not convinced of this interpretation. It is defined as the ratio of the change in a parameter due one driver, and the sum of the changes to this parameter by all other drivers. Whether this quantity can be interpreted as a contribution of one driver to the projected future changes in water quality depends on whether effects of the drivers are purely additive, with no interactive components. I believe your own results do not show this.

To illustrate: from figure S9b, we can see that at Kanpur, at 174 km, BOD at baseline and in the future C45LP are around 15 and 60 mg/L, respectively. Then from figure 6b we can see that under only RCP4.5, BOD changes with around 175%, so it is a change of around 15*1.75=26.25 mg/L. Figure S6b and 7b we can see that this corresponds to a change ratio of around 70-80%. Compared to the total change of 45 mg/L in CLP45 (in which all drivers were implemented) we see that the fraction of the change brought about by RCP4.5 is around 100*26.25/45~60%. For around 75% contribution to be reached at Kanpur, a change of 33.75 mg/L should be seen, or a total change of over 200% with respect to the baseline. The results in figure 6b do not show this. Furthermore, the changes in 6b that contribute to the changes in CLP45 at Kanpur, add up to about 175+25+50=250%, or to a change or around 38 mg/L. Which is a change that is about 15% less than CLP45. This is to demonstrate that the change ratio cannot simply be interpreted as a contribution of one driver to the total projected change of a water quality parameter. Without considering interactive components, the resulting figures may not reflect their actual contribution to the total. Thus, I would suggest the following:

Calculate the ratio of the change in some water quality variable due to some driver and the total change of this variable in the projected future scenario. For this future scenario, scenario CLP45 may be used. Thus, no new model runs would have to be done. Thus, quantities that can be interpreted as full contributions are found, and the contributions of collective interactive changes can also be quantified. Hereby, any conclusions drawn from these calculations would be in line with the first objective given in lines 105-106.

Furthermore, an indication of the limits of your analysis should be given with respect to the fact that contributions due to interactions between drivers are not quantified. However, an estimate of the total contribution of all interactive effects can be given with the calculations outlined earlier, which may then be presented.

iii) Data presentation

Though the presentation of the data brings across the results, and allows a reader to inspect the data themselves, the figures do not seem to have been designed with legibility as a main consideration. This is a consistent problem across the entire results section, resulting in figures that either lack density, have unnecessary details, or unnecessarily repeat information. I can give several suggestions from my perspective, but on the whole, I urge you to reconsider the figures and your data presentation.

A lot of figures share a tendency to repeat information that need not be repeated, either due to poorly chosen units, labels, and repeated legends. Culprits of the first two problems can be found in figures 3c-d, 4e-f, 5, 6, and 10. The units chosen lead to large numbers being presented, taking up space that would give the graphs some breathing room. Furthermore, the y-axis labels tend to signify the same quantity for differing water quality variables, and thus a great deal of repetition can be seen. Making a single figure with those subfigures that present similar quantities with a clear main title, removes the need to exactly specify every y-axis label. For example, taking figures 5c-g, they could be given a main title 'Point load concentrations in the river', then 'point load' could be removed from every y-axis label. Examples unnecessarily repeated legends are found in figures 5, 7, 8, and 10. Figure 6 does show shared legends, so it is puzzling why this was not employed elsewhere. Moreover, the same codes used there can be used in the preceding figures.

The specific presentation of certain data is done using chart types that either are inefficient or make the figure difficult to read. Figures 7 and 8 take up a lot of space, simply because of the chart that was chosen. Using stacked bar charts there, as well as in figures 4a-d, like in figure S7, would allow a much more concise presentation. Figures 4e-f, 5c-g, and 6 use scatter plots while the x-coordinates for data points are often shared, leading to overlap of the points. Though it is difficult to imagine what might better represent the data here, as a line might not be appropriate and bar plots would make the figure much busier, I would still suggest thinking about how to circumvent this problem. Another, example of chart-choice leading to inefficient presentation, are the limits in figure 9. Simply using a horizontal line indicating the limit would free up space to present the results of many more scenarios.

To summarise, I urge you to consider the following:

- Does the choice of the type of chart inhibit legibility or efficiency of the figure? Can information be conveyed more succinctly with a different presentation of the same data?
- Is data in the entire figure repeated more than once, even though this is unnecessary? Long axis labels and repeated legends is often the main problem in this regard.
- Are the units that the data are presented in inhibiting the legibility of a chart? Removing three extra zeroes along the y-axis by changing the units already improves quite some figures.

Minor comments & errors

Here follows a list where minor comments, errors, and suggestions for improved phrasing are given. They are all given ordered by line number.

Title: The title does not include the investigation of the socio-environmental scenarios. I recommend doing this. You also only investigate a part of the Ganga River. Lastly, you don’t investigate pollution, but water quality in general, let the title reflect that.

Line 31: Add extra keywords: Ganga Action Plan, sewage treatment

Line 43: Provide a citation. It looks like common knowledge, but some support would make the statement much stronger.

Line 60-61: Changing ‘and water pollution’ to ‘and thereby, water quality’ would better represent the contents of the work cited.

Line 71-73: This statement is supported by a rather weak source. Khattiyavong and Lee 2019 does not directly study the effects of population growth, since there never is a comparison to the current situation. So better sources for such a statement should be sought.

lines 77-78: The paper deals with the spatial extent of pollution from a WWTP on a seasonal basis. The statement is not in line with the conclusions drawn by that study. I recommend removing this statement or rewriting it to better represent the results of the paper.

Lines 76-77, 79: Here you use ‘WWTP’, while elsewhere you use ‘STP’. I urge using STP everywhere, since that is used more often in the text already.

Line 99: Add a period after ‘water quality’

Lines 100-103: Provide a citation.

Line 109: Provide meaning of ‘GCM’, like all other abbreviations

Line 161: Put parentheses around ‘2018’ like you did elsewhere for in-text citations.

Line 175, figure 2: Please provide a more detailed caption to this figure. Furthermore, you might want to indicate that the effects of LULC are in fact incorporated in HEC-HMS. Currently this is not the case.

Line 183: R^2 of 0.6 is not too high, maybe indicate how this affects the conclusions of the paper, in what way does the model deviate from the validation period? This is not clear from the text.

Line 190, section 2.2.2: No indication is given of the quality of the validation for the water quality model. Please summarise the results of calibration and validation from the supplements in the text.

Line 278, table 2: The codes devised here, while comprehensive, are rather opaque when they are read in the text. I believe they can be shortened: removing ‘5LPS’ would make them all much easier to read. Of course, if these codes are changed, then lines 247 to 256 must be rewritten accordingly.

Line 327: Replace ‘rise in population’ with ‘increasing population’ and add ‘a’ between ‘to’ and ‘rise in sewage’.

Line 338: Replace ‘for Jajmau’ with ‘at Jajmau’.

Line 349, figure 5: Please indicate where the drain locations are. Figure 6 indicates the names of the drains, do that here as well.

Line 359: Furthermore, the legend for 5g indicates ‘population’ twice, one should be ‘industry’, as I understand it.

Lines 370-371: Idem, as in line 359.

Line 361: Replace ‘having’ with ‘resulting in’.

Line 363-364: The statement seems applicable here, but surely there are also sources that find the same thing for the Ganga Basin? You could use the work by Jin et al. (2015), whom you already cite, to make a more nuanced statement about the Ganga basin, instead of Europe.

Line 372-374: It is unclear whether you cite a finding by Zhou et al. (2013) or corroborate your findings with it. Rewrite the sentence to make that clearer.

Line 389: Replace ‘is’ with ‘in’.

Lines 392-393: Indicate for which climate scenario this holds, if it holds for both, also indicate that.

Line 395: ‘because of the higher denitrification rate’, do you present such a finding anywhere? Please support this statement with some reference to such a finding or source.

Line 411: Add a comma after ‘however’.

Line 416: ‘at Jajmau’ instead of ‘at the Jajmau’.

Line 422: Add comma after ‘4%’.

Line 436: Please indicate what limits you are referring to.

Line 447: ‘population and industrial growth, and land use land cover change’ instead of ‘population, industrial growth and land use land cover growth’.

Line 509: ‘goes below… respectively’, is worded strangely, please rephrase to make clear what you mean.

Line 515: Make sure that the notation of ‘mg/L’ or ‘mg L^{-1}’ is consistent across the paper. Currently you switch here and there.

Line 516: Remove ‘even’.

Line 544: ‘10^8’ rather than ‘10^8’.

Line 561: ‘do not change the nitrate concentration much’, rather than ‘do not change much of the nitrate concentration’.

Line 614: Replace ‘especially in’ with ‘as is demonstrated here for’, as you only investigate the low-flow period.

Line 621: “However, we believe that the qualitative results will not be affected.” There is no elaboration or basis for this statement in the text. I recommend removing it or providing an elaboration or basis.

Line 672: Name misspelt, ‘Bons’, rather than ‘Bonus’

Lines 710-733: These references are difficult to access, maybe you could provide a URL that leads to these sources.

Line 765: Incorrect title, remove ‘670’

References

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