Geoscientific Model Development - Discussions
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Review: “Hydrological modelling on atmospheric grids; using graphs of sub-grid elements to transport energy and water”

J. Polcher, A. Schrapffer, E. Dupont, L. Rinchiuso, X. Zhou, O. Boucher, E. Mouche, C. Ottlé, J. Servonnat (2022)

General comments

The authors present and evaluate a methodology that divides grid cells of a land surface model (LSM) into hydrological transfer units (HTUs). This method allows for a finer and more realistic representation of the river discharge and the energy transport (stream temperature) than a calculation of these parameters directly on the coarser LSM grid. The authors show that their method is independent of the original LSM grid and the hydrological digital elevation model used to construct the HTUs.

This approach is very interesting, as it allows for a better representation, at higher resolution, of hydrological extreme events, but it also enables an integration of the influence of human infrastructure and water usage in Earth System Models. However, it is sometimes difficult to follow the explanations of the authors and to relate them to the results shown on the figures. Therefore, I would invite the authors to review in depth their manuscript. I hope that my comments listed below can be helpful.

I have four major comments:

1) The whole text should be carefully reread as there are many errors (see the “technical corrections” below for some examples), such as missing third person “s”, wrong conjugation, etc. The authors are also very parsimonious about the usage of the comma, making some longer or more complex sentences difficult to understand by the reader.

2) The explanations might need to be reformulated or sometimes even restructured, especially in the more technical parts, like section 3, to make it easier to follow for the reader. Some examples are listed in the specific comments below.

3) The authors often use the word *grid*, when they actually mean a single *grid cell* (or *grid point*, or even *grid mesh* or *grid box*), which leads to some confusion for the reader. Especially in the description of the method (section 3), it makes the understanding of the explanations really difficult. This has to be carefully corrected by the authors through the whole manuscript, ideally by using consistently one single denomination. Some examples are:

l. 58: “from grid *cell* to grid *cell”*

l. 179: “atmospheric grid”: Is it the entire grid or one grid cell?

l. 186: “arrows pointing out of the grid *cell*” ?

l. 202: “less than 10% of the grid *cell* area” ?

Figure 2: “atmospheric grid *cell*”, 2 times ?

l. 229: “atmospheric grid *cell*”?

l. 230: “neighbouring grid *cell*”?

l. 232: “flow out of the *mesh*”

l. 234: “atmospheric grid *cell*”?

l. 236: “*grid box*”

l. 440: “per atmospheric grid *cell*”?

4) Section 5

- Concerning the title: as I understand it, this section is more a sensitivity analysis on some HTU and atmospheric grid parameters. I would expect something else under “numerical implementation” (e.g., code performance, scaling tests, etc.).

- To my opinion, this section should be reorganised and better justified. Especially, the explanations why and how this analysis is performed, need to be clarified. For example, the explanation that forcing data at coarser temporal and spatial resolution are used, because high-resolution data are not available yet, might be moved from the end (subsection 5.4) to the introduction of this section. The authors might also add a short discussion on how a sensitivity analysis on the parameters tested here is influenced (or not) by the low-resolution forcing data. One could think that the temporally (from daily to hourly) and spatially interpolated forcing data (l. 384), and the resulting smoothed discharge (e.g., no sub-daily discharge peaks, an underestimated spatial heterogeneity) weakens the analysis presented here. Some examples are:

l. 416-417: Can conclusions from a simulation forced with interpolated daily data be drawn on peak discharges?

l. 430-433: Thus, is this comparison really relevant? The aim is to reach high-resolution at “low costs” for the routing. So, the comparison should be done with high-resolution data to be more robust.

Section 5.2: Are the very good results shown here not due to the interpolation of coarse forcing data? Would a high-resolution forcing to evaluate the information loss when using less HTUs not be more relevant here? From what I understand from this analysis, i.e., that there is almost no performance gain/loss when changing nbmax, I would chose a low nbmax, or even no HTUs at all (to avoid the issue mentioned in l. 446). Thus, the authors might want to clarify this analysis and the conclusions one might deduce from it.

l. 443: In l. 449, it is stated that all stations considered here have a large up-stream area, thus only large catchments are analysed here. So how can the authors conclude that the results do not depend on the catchment size in l. 443?

l. 464: Could the difference not also have a stronger influence on small catchments? Could it not even be (partly) balanced out over large catchments?

Section 5.4: I understand this subsection as a kind of conclusion of section 5, which is useful. However, the authors mainly focus on the time step here, while other parameters were discussed before, too. If this subsection is meant to focus on the time step, it might be more relevant to move it to subsection 5.1.

l. 470-471: The gx parameters are determined for HDEMs, not for different grids, thus the statement saying that they do not need to be adjusted to the atmospheric grid might be true, but it has not been tested here.

Specific comments

l. 22: I would suggest to replace "*Thus"* by "*For Example"*, as this sounds more like an example than a general deduction of the previous sentence.

l. 33-35: While I agree that lateral water movements require a high resolution to be represented in a realistic way, I would say that this is also true for the atmosphere, depending on which processes are of interest. One might think about modelling urban canyons, for example. The authors might clarify that the atmosphere does not need such a high resolution to properly resolve the processes regional atmospheric modelling / land surface modelling usually focuses on.

l. 50: The authors announce two approaches in l. 37. Then, after having introduced these two approaches, they continue with "A complementary methodology…" in l. 50. Would this then be a third approach?

l. 59: "the two linked to the grid": I do not understand what is linked to the grid.

l. 65: Schrapffer et al. (2022) is not listed in the bibliography.

l. 95 equation (1) (and others): What do "j" and "W" stand for?

l. 155-156: This sentence might need to be rewritten.

l. 162: This is also true for the Antarctic region, isn't it? Maybe rephrase to "which do not cover the polar regions"?

l. 169: "with the coarser *atmospheric* mesh" to make it more clear?

l. 181: "The example *over a part of the Rhone valley* in Figure 2c) (*nbmax, which is set to 18 here, will be discussed further below*)" might be clearer.

l. 184-185: This is not clear to me. I understand that the authors still base their explanation on Figure 2c), where there are many HTUs (colours) for the outlet in the SW corner, and not only one as stated here.

l. 189-193: I do not see where the authors consider the two types of confluences presented here in the explanation below (from l. 194 on). Further, it is not clear to me how these two types are differentiated (on the basis of a threshold? If yes, which parameter and which value?).

l. 193: At which threshold is the subdivision too small (< 10% ?) ? And why is there still a need to divide the HTU into two parts if the tributary's confluence is moved downstream?

l. 212-214: This explanation is not clear to me.

l. 213: Should the sums not be computed along all streams *down* to the outflow point?

l. 219: What do the authors mean by "surface groundwater"?

l. 226-227: This sentence should be rephrased to make it easier to understand.

l. 229: It is not clear to me whether the authors want to say that the HTUs *flow* or the atmospheric grid cell *flows* into the ocean.

l. 245, 583, 585: Do the author mean *land surface models* instead of *land system models*?

l. 275: What it the total error? Maybe add something like “from the total error *for each HTU as described above*.”

l. 294: Where on Fig. 4 do the authors see that the dz of HTUs is smaller than the sub-segment by over 15% ?

l. 305: “the same results for *the grid with* the highest resolution”?

l. 314-328: As HydroSHEDS does not provide a hydrologically corrected topography (see l. 320), does this whole comparison make sense? Are these results really comparable? Is this comparison not more an analysis of the differences between a hydrologically corrected DEM and a not-corrected one? If this is the case, this comparison might be out of the scope of this paper, to my opinion.

l. 315: “and are better than 5% for both the elevation change and length of samples”: I do not understand what is better or compared to what they are better. Maybe some words are missing here.

l. 317: The differences when using HydroSHEDS instead of MERIT seem quite large to me, so I would not write that “the behaviour changes slightly”.

l. 330: “are analysed for the Danube *as an example*.” ?

l. 342: “are quite constant except for short segments”: This is not clear to me. Is the reader supposed to be able to come to this conclusion when looking on Figure 5?

l. 344-346: This statement might be rephrased as it can be understood as if the authors determined the optimal truncation on the basis of computational costs, instead of the result of a t-test.

l. 387: “only ~~a few~~ *35 stations* were selected” ? Why these stations?

l. 391: Which reference configuration do the authors mean? The WFDEI-GPCC based simulation from l. 383?

l. 396: Why 225s? Where does this value come from?

l. 399: “only the annual mean is shown”: What do the authors mean? The average over one year (which one), or the total average over 1983-1993?

l. 408: “close to or lower to the recommended value”: It is not clear to me which value the authors refer to.

l. 462: What does “MEDCORDEXHS” stand for?

l. 498-506: I do not understand this analysis, and especially how it relates to and interprets the results shown on Figure 11. For example, l. 502: “Based on the analysis above, we know that if the forcing *is* the same…”: It is not clear to me how one comes to this conclusion.

l. 519: I do not agree that the annual cycle is closer to observations for the Danube. As I see it on Fig. 12, for the Rhine the difference varies between -5 and -2K, while for the Danube it varies between -4 and 0K.

l. 547: “by setting *the scaling parameter* a=10^5 *(eq. 10)*”. Remembering what “a” stands for might be useful here.

l. 555: “for both runoff and drainage *(WFDEI_Top)*”?

l. 566: Is winter really the low flow period for the Rhine, the Elbe, the Loire, etc.?

l. 645: Which HDEMs are the authors talking about? MERIT and HydroSHEDS are already made available by their authors.

Table 1: WFDEI → (Weedon et al., 2014)

Table 3: The caption does not really describe the content of the table.

Figure 1:
- It might be useful, for example for l. 276, to also show the entire grids, e.g., as insets, as well as the main rivers mentioned in this paper.

- “The green colour *indicates*”

- “over the actual land-sea mask shown in yellow/blue.”

Figure 2:
- maybe mark the rivers mentioned in l. 190-191 on Figure 2a?

- limit the scale to 18 colours

- explanation l. 263-267: the blue line does not exactly follow the white arrows. But if I understand it right, the calculation discussed here is based on HDEM data corresponding to the white arrows (see l. 269), thus the blue line should exactly follow them.

- HTU 8 taken as example in l. 269-270 might be coloured/highlighted in a way that makes it easier to identify it on the figure.

- the description in l. 272-274 is difficult to follow on the figure. May it be useful to highlight the elements mentioned here, or maybe to show them on a separate figure?

Figure 3:
- “~~Figure~~ provides”

- I would strongly recommend to present these results as box-whisker plots. One coloured box-whisker plot for each river and for each truncation. This would be much more meaningful. It would also avoid an overlap of the curves and lines as it is the case in the current version of this figure. Further, it would then certainly be possible to show all five rivers (add Rhine and Elbe) without overloading the figure.

Figure 4:
- It might be helpful to add the meaning of the solid and dashed lines in the caption.

- I would only show one legend for all sub-figures, at it is always the same, and increase the font size, as it is barely readable.

- It might also be useful to only list as X axis labels the nbmax values for which there are results.

- There are many points missing on the lines, e.g., for the Rhone dz at 25 and 55.

Figure 5:
- It might be helpful to add the meaning of the solid and dashed lines in the caption.

- I would only show one legend for all sub-figures, at it is always the same, and increase the font size, as it is barely readable.

- What does “Danube10” in the X axis titles mean?

Figure 6:
- Caption: add the meaning of the black horizontal line and information on the simulation shown here (WFDEI-MERIT, period, etc.).

Figure 7:
- Caption: add the meaning of the black horizontal line

Figure 8:
- Caption: add the meaning of the black horizontal line. In addition, I only see three different grids, not four as mentioned here.

Figure 10:
- The Y axis labels are barely readable.

- It might be useful to add in the caption which HDEMs are used. “another HDEM” does not give any useful information.

Figure 11:
- “comparing the ~~observed~~ *simulated* monthly … to observations”?

Figure 12:
- “Mean annual cycle of *monthly mean* river discharge …” ?

Figure 13:
- “stream temperature *is* available”?

Technical corrections

l. 35: “The hydrological community *has* been free”

l. 48: “the horizontal atmospheric grid *is* compatible”

l. 61-62: “… as the hydrological information, which cannot … flow, is treated …”

l. 73: “we will show with *the* ORCHIDEE *LSM* that” as it is the first time it is mentioned in the main text.

l. 144: Should it not be “from 20° *West* to 60° *East*” ?

l. 161: (Nguyen-Quang et al., 2018)

l. 179: “is *built*”

l. 190: “as *illustrated*”

l. 195: “If the subdivision (1) or (2) *is* too small”

l. 223: “to select a *value* of nbmax”

l. 225: “on all grid points of the atmospheric *grid*” ?

l. 234: “HTUs which *flow*”

l. 237-238: “This leads to *situations* like HTU 6 in *Figure* 2d)”

l. 242: “an optimal number of *HTUs*”

l. 245: “a precious *tool*”

l. 250: “each *station*”

l. 261: “represent *such a* segment”?

l. 263: “the *selected* truncation”

l. 264: “the outflow points *belong*”

l. 285: “range *of* +- 10%”

l. 293: “*elevation* changes”?

l. 357: “the reservoir content *is* updated.”

l. 428: “This *result* is”

l. 430: “it has to *be* kept in mind”

l. 488: “atmospheric *forcings*”

l. 569: “*Diepoldsau*”

l. 590: “which *combines*”

l. 600: “if the water continuity equation *could not* be solved”

l. 610: “The simulated discharge *is* not”

l. 618: “much more *elaborated* schemes”

l. 636: “an extremely *powerful* tool”

l. 654: “XZ *contributed*”