Responses to reviewer #3

Comment

Thanks for your last reply, it gave me some more insight. I would like to follow up on the DW roughness coefficient:

In the abstract you still have the sentence: “It is likely that the Darcy-Weisbach equation, which is applicable to all flow regimes, may perform better for hydrological prediction in this region.” (similar statement in conclusion). You stated in your last response that you are working with basin-scale averages (velocities). Thus, the friction factors are also empirically basin-scale averages. Therefore, there should be no advantage of using the DW over Manning.

Response

We are not referring here to using either equation at basin scales. We are attempting to infer the causes of the slow responses. We use the basin scale data, as this is all the data which currently exists. However, as stated in lines 122-124, modelling is invariably done at much smaller scales, which is where the parameters would be used:

“The intent is not to determine basin-scale roughness parameters be used by hydrological models, as these are distributed or semi-distributed and so invariably operate at much smaller (HRU/GRU/grid) scales”

To clarify, the abstract has been changed to “It is likely that the Darcy-Weisbach equation, which is applicable to all flow regimes, may perform better in high resolution hydrological models of this region.”

Comment

You can also use Manning eq. for non-primarily turbulent flow (just not the recommended values for river hydraulics, but Manning values especially for this condition). For overland flow there is eg. a table from Engman 1986: ROUGHNESS COEFFICIENTS FOR ROUTINGSURFACE RUNOFF. Or you could also use water-depth dependent manning values… Or in your case you could also convert the DW friction factors which fitted to your data to manning values and you would get the same result like with using DW. So, it´s from my point of view not likely that DW eq. will perform better for hydrological predictions in this region. It just depends on the understanding of the system and choosing (or empirically calculating) “correct” roughness values and either way DW or Manning should perform equally.

Response

Thank you for the reference, we were not familiar with it. The text has been updated to refer to it. Note that the largest values found by Engman are much smaller than the largest basin-scale values that we obtained. Also, at the high resolutions used by hydrological modellers, it is quite likely that Manning’s n values would be even greater.

Comment

Following on that I think the conclusion regarding this topic should be more into the direction that the processes are very complex (sometimes dominant channel hydraulics, sometimes dominant overland flow and sometimes other effects like routing or depressions which influence DW and Manning roughness on a basin scale immensely) as you already stated.

And thus, by comparing basin-scale-roughness-values with empirical roughness values it shows: 1. It´s not useful to use roughness values (DW and Manning) from channel hydraulics as basin-scale-roughness-values. 2. for some catchment's roughness values empirically determined for overland flow (DW and Manning) have been somehow correct and 3. for most of the catchments the roughness values from the examined literature examined weren´t satisfactory at all. So, it might be questionable if one can find a correct parameter set for the Canadian prairie which is also an open question in the end? (some of this you have already written but it still is not completely clear to the reader).

Response

We are mostly in agreement on this. As you said, we do refer to the possible cause(s) of the slow responses at the basin scales. We also state that further research will be required to understand the cause(s) of the very slow flows, and therefore to come up with methodology for estimating roughness parameters at small scales.

Comment

Please have also again a look into my comments (in pdf file) from my last review. There were also some technical aspect which haven´t been addressed. hess-2023-51-referee-report.pdf Most are just suggestions or questions. But some should be fixed eg. Fig.9 minus is missing in manning unit

Response

Thank you. Actually, the minus was present, but it was almost impossible to see as it was joined to the bar dividing 1 by 3. The spacing has been increased so that the minus is now evident.

We have addressed the comments that were in the referee report pdf in the paper, except for the comments addressed here:

Comment

P 6, L 109

Maybe switch chapter 2.2 and 2.4 for better chronology? Then hydrometric-data is directly after presentation of basins.

Response

We disagree as the most important data set is that of the streamflows.

Comment

P 13, L 231

Maybe it would be helpful to distinguish between the parameters by also adding an additional letter designated to the researcher to the parameters Lc and A etc.

Response

We could do this but decided that it would make things too confusing as the same variables are used in many equations

Comment

P 14, L 246

Isn´t this method sensible to geometrical errors in the DEM?

Response

Yes, but not to a great extent. The differences between the divide and outlet are many metres.

Comment

P 15, L 296

could be divided into calculation of basin velocity and stream velocity for better understanding

Response

This is a very short section. It doesn’t seem useful to further subdivide it

Coment

P 15, L 300

What about meandering rivers and longer routing than the direct distance due to obstacles?

Response

The meandering nature of the rivers were taken into account when calculating the stream lengths. The statement that “the use of Lc derived from the gross drainage area is conservative” refers to the use of the gross area rather than the effective area. We have added “in the main channel” to further clarify that we are talking about the channel length.

Comment

P 16, L 313

Why is it a useful comparison – please clarify? Isn´t the mesaurement or assumption of a streamflow velocity (for a natural stream) highly depending on the cross-section of the river where you are measuring?

Response

That is quite true. However, we use the ratio of basin velocity to stream velocity to indicate the effects of scale on the basin velocity, and note that all ratios are smaller than 1, indicating that the basin velocities, despite their many simplifying assumptions, are not grossly in error.

Comment

P 20 Figure 6

Think about reducing the x-y-scale to max 200 for better visibility. Only 1 point not shown in diagram from Langridge which can be mentioned in text.

Response

We left the scale as it is as a) we didn't want to omit the point and b) the objective of the graph is to show how 4 of the 6 methods resulted in empirical values << observed values, rather than to show the scatter in each empirical method.

Comment

Table 3

you could also add stream velocity and stream manning

Response

The table is getting to be a bit wide. The stream velocities are given in the repository data set. Manning's n was not calculated from the streamflow velocities

Comment

P 27, L 482

Could it be the possibility that the „ineffective area" was effective during the choosen scenarios as they had a high precipitation (possible greater than every 2 years)? Also, there are still depressions in the effective area, which should slow down the flow even when they are already filled.

Response

As discussed in the text, the contributing area in basins dominated by depressional storage, changes dynamically from event to event, and even within a given event. The point of doing the regressions was to use the effective area fraction and wetland percentage as indices of the maximum effect of the depressional storage.