Reviewer #1 reject

Suggestions for revision or reasons for rejection

I cannot see any progress in the revised version. The Darcy-Weisbach parameter f is still used as an empirical parameter with no link to hydrodynamics.

The author state: "Darcy-Weisbach roughness coefficient f values (dimensionless) were calculated from the study velocities. Comparing the study f values to published empirical values derived from research plots allows determination of the ability of the Darcy-Weisbach equation to be used as a robust routing equation in hydrological models in gently sloping agricultural basins."

However, the log-log plot (figure 10) of f against v shows enormous ranges of f (or v). A log-log plot of n versus v would have given the same results. Why didn't the authors plot n versus v? f can be expressed by n (and vice versa) - see Equations 8 and 12. The argument that the values of n are not in the range given by literature is not convincing since the values of f are not in the literature range as well (Moody diagram for turbulent and laminar flow).

In conclusion, a reliable estimation of flow velocities and roughness coefficients (as stated in the title) is not possible in the Canadian Prairie basins.

Response:

We have modified the title in response to this comment. We appreciate the perspective this reviewer has provided in the past. We had addressed each of their comments, but we felt that the including the hydrodynamic approach suggested was outside the scope of our intended communication. Note that this paper does not deal with any form of modelling, so the hydrodynamic approach has nothing to do with our work. We are attempting to evaluate why the response times of the research basins were so large, much greater than would be expected from the majority of empirical equations.

The reason for plotting f vs v was to demonstrate that the values of f computed for the research basins fall within values measured for small research plots, for the small basins. Therefore, it is believed that the reason for the slow responses in these basins is in part related to the surface roughness dominating overland flow velocities. This is explained in lines 470-477 of the revised document:

> "Many of the observed Prairie basin flood wave celerities and velocities are very small. It is apparent that the smallest values of the study celerities and the ratios of basin velocities to stream velocities, and the largest estimated values for Manning's n and the Darcy-Weisbach f, were obtained from the smallest basins. This indicates that the cause(s) of the exceptionally small basin velocities are related to the presence of overland and/or shallow

> subsurface flows, as channel flows will dominate at large scales. This finding also agrees with the work of Brannen et al. (2015) and Costa et al. (2020) which were carried out on very small basins. The effects of scale appear to hold true even in the badlands-containing basins (05CE002, 05CE020, and 05FC004) and the montane basin (05CC001), despite the possibility of different predominant flow pathways in these basins from those in the plains."

The very large Manning's n values estimated for the small basins are also evidence that the cause of the slow basin response is due to overland flow. They are also evidence that the overland flows may not be fully turbulent.

However, because reviewers #1 and #3 did not fully understand our objectives and scope, we have endeavoured to make this clearer.

We now state more explicitly that our objective was never to produce "a reliable estimation of flow velocities and roughness coefficients". Note our objectives, which are listed on lines 95-99 of the revised document:

> "The objectives of this research are to determine a) if small runoff flow velocities are a general feature of the study area and therefore of the Canadian Prairies, b) if the velocities can be related to any obvious basin-scale parameters, and c) the effects of the flow velocities on basin-scale roughness parameters. 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"

Note that we specifically advise against the use of the roughness parameters estimated herein.

We have now stated the difficulties in using hydrodynamic modelling for prairie basins, which currently require the use of hydrological, rather than hydrodynamic, modelling in lines 71-94 of the revised document, as listed here:

> Hydraulic modelling programs have been used widely within the Canadian Prairies to model flows within river channels, but they have not been widely used to model overland flows. Costa et al. (2020) demonstrated the use of a hydraulic model to simulate overland flows within a very small (~2.1 km^2) basins. Because of the region's complex hydrology, hydraulic models having simplistic representations of cold-regions hydrological processes (including snowfall sublimation and redistribution by the wind, and infiltration to deeply-frozen soils) invariably fail in this region. Costa et al. (2020) avoided this problem by forcing their hydraulic model with streamflow data.
>
> Many of the basins in the Canadian Prairies are very large; the basins within this study are up to 1150 times as large as that modelled by Costa et al. (2020). Hydraulic models have the disadvantages of being very computationally-intensive as they need to simulate

at very small space-time scales. Because of the region's complex hydrography, any hydraulic model will need to operate in at least 2 dimensions, further increasing its computational costs. High-resolution hydraulic models require high-resolution soil, vegetation and digital elevation model (DEM) data, which may not be available or may be expensive to obtain.

In contrast, the hydrological modelling programs such as the Cold Regions Hydrological Modelling platform (CRHM) (Pomeroy et al., 2022), and Modélisation Environmentale Communautaire—Surface and Hydrology (MESH) (Wheater et al., 2022), represent all the relevant cold-region processes. CRHM and MESH are semi-distributed models based on hydrological response units (HRUs) and grouped response units (GRUs), respectively. The programs can also represent the varying contributing fractions of prairie basins. Importantly, the data and computational requirements of models created using these programs are relatively modest.

Given the challenges of using hydraulic models, it is likely that hydrological models will be important within the region for the foreseeable future. However, determining the surface roughnesses at HRU/GRU scales is not easy, particularly for those HRUs/GRUs which model overland flows, rather than flows in channels. Successful parameterisation of hydological models remains difficult without an understanding of the reason(s) for the apparent slow responses of prairie streams. As an example, Annand (2022) described the tendency of a CRHM hydrological model of a prairie basin to be too "flashy", which required modification of the model structure.

We hope that this will help to explain the reasons for our interest in the response times of prairie basins, and their possible causes.

<u>Reviewer #2 technical corrections</u>

I have reviewed the manuscript "Estimating response times, flow velocities and roughness coefficients of Canadian Prairie basins" for the second time. It is a valuable contribution to the field of hydrology, as it questions assumptions which are usually taken without much attention (e.g., estimating response times or Manning's n coefficients for hydrological modeling). They reported that, for the Canadian praries, these assumptions are usually not valid. The text was well written from the beginning, but now the authors have improved the formatting, making it clearer. They have made the codes and spreadsheets used for calculations avaialable for download. I would consider the manuscript ready for publication, except for the fact that I was not able to retrieve the values that they reported. I apologize if this is my mistake. I tried to retrieve the values of the following equations: Kirpich, Watt & Chow, Capece and Sheridan. None of them resulted in the same values reported in "table2" (zenodo -> output -> table2.csv) nor in "basin_responses" (zenodo -> output -> basin_responses.csv).

Response:

Unfortunately, some files were included in the zenodo repository, which should not have been. The file “basin_responses.csv” was one of them. This was an old output where the empirical equations were evaluated using the Gray relationship for the main stem length or had erroneous units. The calculations were later redone using the main stem values estimated from GIS. Unfortunately, because the DOI for the repository was assigned by the journal, we are currently unable to make any changes in the files. We are hoping to be able to get permission to do so.

Comment

The values for Kirpich, Capece and Sheridan are at least in the same order or magnitude. However, the values for Watt & Chow are in a completely different order of magnitude.

For instance, for catchment 05CC001, here is the comparison between the values reported by the authors, and the values I retrieved, respectively:

Kirpich - 13.9335; 34.3315

Watt & Chow - 18.4379; 0.19856

Capece - 7.36392; 9.0854

Sheridan - 91.6754; 169.4879 (the value of 169.4879 is actually reported in "basin_responses" under "Sheridan_area_tc" column, but it is not the value reported in "table2 > sheridan_tc".

These values are not reported as numbers in the main manuscript. They are only plotted in Fig 6. This is why the values in the same order of magnitude are not a big problem, however the fact that I was not able to retrieve the same values raised questions as to whether the calculations are correct, particularly for the Watt & Chow equation, which do differ in orders of magnitude. I applied the equations following the values provided in Table 1 (for instance, for catchment 05CC001, L = 125.9km and S = 0.0014 (-)). I tried to be careful about the units, following how the units are described in the tables and in the text (for instance, L in km and S dimensionless).

Here's one of my attempts to calculate the Watt & Chow equation, which resulted in 0.19 instead of the reported 18.43:

https://www.wolframalpha.com/input?i2d=true&i=0.000326*Power%5B%5C%2840%29Divide%5B+125.9%2CSqrt%5B0.0014%5D%5D%5C%2841%29%2C0.79%5D

My suggestion is that the authors review these calculations. If they are correct and this was my mistake, I apologize for the inconvenience. If these are not correct, I suggest that the authors review all the calculations in the manuscript.

Response:

This error has two causes.

1. Some of the empirical equations were calculated (by us) using incorrect values of some of the basin parameters, due to using an incorrect file. Fortunately, none of the other calculations

in the paper (celerities, velocities, n and f values) used the incorrect parameter values. Very few of the values changed greatly. The only result was to increase the scatter in some of the plots in Figure 6 and to slightly change the mean values of the ratios of the empirical to experimental response times. None of the results changed enough to change the findings in this document.

2. The reviewer used Lc in km, rather than in m, as is required by the original equation. We apologize for not having mentioned that this equation used a different length unit and have added this to the manuscript.

The actual calculation for the Watt and Chow equation for basin 05CC001 is therefore:

$$t_{IW} = 0.000326\left(L_c/\sqrt{S_c}\right)^{0.79}$$

Lc = 125.915 km = 125915 m

Sc = 0.0014136

tlw = 0.000326 (125915/(0.0014136^0.5))^0.79 = 46.55 hours

Comment

Moreover, there is one sentence that can be improved in the manuscript: "Artificial drainage channels are much narrower for a given depth, and also be straighter and shorter, than natural channels which will result in deeper and faster flows than in natural channels." - I think the correct way would be to say that the "Artificial drainage channels are much narrower for a given depth, and are also straighter and shorter, than (...)"

Response: Corrected.

Comment

I hope my comments are useful. Thank you for improving the manuscript and for taking my comments into consideration.

Response: We appreciate the comments and suggestions that the reviewer has provided.

<u>Reviewer #3 major revisions</u>

I have read the manuscript „Estimating response times, flow velocities and roughness coefficients of Canadian Prairie basins“ by Shook et al. with great interest. The authors analyzed a big dataset of Canadian prairie basins and show that their observed tp does not fit good to most of the compared empirical equations. Furthermore, they calculate basin wide velocities and resistance coefficients. They also show in the chapter discussion that they have a great understanding of the hydrology and the hydraulic processes in the surveyed catchments and conclude that more research needs to be done.

Response:

This is consistent with the intended messages of this paper.

Major Comments:

The authors explain the meaning and differences between Time of Concentration, lag time and time to peak very well. Nevertheless, they state that "The definitions of these response times, as described above, they are similar enough that they can be compared with the observed tp values, despite the differences described above." and eventually compare them to their observed tp. I get that in this case there is no rainfall data available to specify tc and tl. But by comparing them there should be given an idea about the relationship and sensitivity of these values in contrast to tp and also followed up on that in chapt. 4.2.

Response:

Thanks you for this comment. We believe that we have discussed the many contradictory definitions of tp, tc and tl. We have added a statement to answer the possible effects of the centroid of effective precipitation on tl., in revised paper (lines 245-251) :

> "As discussed above, because t_l begins at the centroid of effective rainfall, rather than the point of rise of the stream, there would be an expected difference between its value and that of t_p. However, in the example plotted in Fig. 4, it is evident that the centroid of the daily precipitation before the peak greatly preceded the initial rise of the hydrograph, because of the very slow response of the hydrograph. Accordingly, it is unlikely that the centroid of the *effective* precipitation would have occurred after the initial rise, meaning that using t_p to test computed t_c values will be conservative in that the value of t_c for the stream would likely be slightly greater than t_p. Therefore, the definitions of the empirical response times are deemed to be similar enough that they can be compared with the observed t_p values, despite the differences in their definitions."

Comment:

As I understood there were velocity measurements carried out for different discharges at the streams. Based on these there was an extrapolation of a velocity-discharge-curve to determine flow velocities for the observed peak flows. Based on the known cross-sections at the gauging stations you could calculate the mean velocity for the streamflow directly from the discharge which seems to be more accurate?

Usually you should have also Q-h curves or similar for all the gauging stations which are used in the first place to calculate the discharges? Streamflow velocities for peak flow velocity could also be given in Tab. 3 and also be compared to the cited values of Bjerklie (2007). Also, here manning/DW coefficients for the channel could be calculated for comparison reasons to the basin wide values and a discussion of the differences could follow as another point why a more detailed analyses (differentiation between overland flow, channel flow, retentional areas …) is needed.

Response:

We do not have "known cross-sections at the gauging stations" or Q-h curves for the gauging stations. All that we have is flow rates and mean velocity values for some summer events.

The reviewer assumes that stage-discharge relationships were used to estimate basin velocities. This is incorrect. The basin velocities were based on basin calculations and not on observations of stage and discharge used in a rating curve.

The suggestions of including peak stream velocities is intriguing, but it too relies on the rating curve information and not the response time calculations we used.

Comment:

One could learn a lot from a deeper analysis of individual basins. "Hydrographs demonstrate that the observed times to peak varied widely amongst, and within, basins" E.g. Basin 05CG006 looks like it has a very big spread in observed tp. This shows that there can be already a natural spread of tp for one basin which seems to be independent from the parameters used in the tested equations. This could also lead to a better understanding of the processes and shows also the uncertainties using approaches for calculating tp (even when the observed data is already so diverse) based on these really simple eq. How should a simple approach fit to those data if the data itself has already such a big spread?

Response:

We agree that much could be learned from more detailed studies that would follow from ours. However, as we stated in the paper, we are using the maximum time to peak from the experimental basin hydrographs as our tp value. As stated in the paper, we believe that the variations in the hydrograph times to peak are largely due to the variation in the area responding to rainfall (which we did not assess, as this is *not* a modelling paper).

Comment:

"The Darcy-Weisbach equation, (…), has the advantage of being applicable across all flow regimes, from laminar to fully turbulent." This sentence is correct in relation to hydrodynamics and has the (physic based) advantages to calculate friction factors for pipes and channels based on the Reynolds number and potentially superposition them with drag coefficients. The authors however are looking at the entire basin and by doing so they mix up all the (hydraulic) processes (fill and spill, routing, vegetation, overland flow, channel flow …) and calculate a synthetic basin velocity leading to an abstract empirical friction factor. This should be properly discussed in the manuscript and also kept in mind by comparing the basin wide values to values based on experiments. Also, one could think about using mbasin und fbasin for these empirical values.

Response:

This is a useful point. However, it is very clearly addressed in the paper:

"It is important to note that these values are basin-scale averages; they do not represent the velocity of flow at the outlet, or at any other point."

The objective of the paper is not to recommend any coefficient. We are attempting to understand the cause(s) of the apparent slow responses of the experimental basin.

Comment:

Therefore, there should be no advantage about using DW over Manning besides that the coefficient is dimensionless. The comparison between DW and Manning coefficients with literature is also lacking some depth. Please keep in mind that the calculated abstract manning and DW coefficients by the authors are for the whole basin therefore it´s highly depended on the diversity of flow processes. It´s not suprising that some of the values are fitting better to mannings for channel hydraulics (Schneider and Arcement), some are better fitting for overland (Weltz et al.) and some are even more rough and could be affected by other effects which could be also of geometrical nature (routing, retention areas …).

Response:

Yes, that is actually the point of the comparison. Again, we are not attempting to determine "better" values of parameters. We are attempting to determine why the basins respond so slowly. The fact that the basin-scale Manning n values are so much greater than those published for channels are evidence that

A) The cause of the slow responses is probably due to overland flow
B) Modellers should not be using table channel values for Manning overland flow calculations, and
C) The use of Manning for overland flows may not be justified, because of concerns that the overland flows may not be fully turbulent

Comment:

DW values look better in this case due to the used log-scale in contrast to Manning values but a lot of them are much bigger than the comparative values.

Response:

The point of the figure is to show that the DW values of some of the smallest basins plot *within* the values for some small-scale plots. Therefore, we believe that it is likely that the cause of the slow responses of the small basins is their overland flow.

Comment:

The DW-values which are fitting to the experiments from very small experimental plots are also partly fitting to those manning coefficents of Weltz.

Response:

That is correct. Note that the coefficients of Weltz were also determined from small experimental plots. We have expanded this section.

Comment

If approaches are needed to estimate tp in Canadian prairie basin, an approach that includes the retention/depressions in the effective area might be useful? On the other hand one can also ask oneself whether such a blanket approach is still appropriate today. In times where we have access to a lot of remote sensing data / land use data and high computational power. Which could lead to the use of hydrodynamic models - or simplified models of the basins based on sub-basins and channel flow – or the use of machine learning approaches.

Response:

We agree that some of those methods might complement a physical understanding of the actual processes which was our interest. However, addressing variable contributing area due to depressional storage, and improved modelling, are not part of the purpose of the paper.

We are not suggesting approaches to estimate tp for prairie basins. That is not our concern. We are simply demonstrating that the response times of many of the basins are much greater than would have been expected, compared to empirical equations in general use.

Nor are we suggesting using simplified models as the reviewer suggests. We call the reviewer's attention to the stated objectives of the paper:

> "The objectives of this research are to determine a) if small runoff flow velocities are a general feature of the study area and therefore of the Canadian Prairies, b) if the velocities can be related to any obvious basin-scale parameters, and c) the effects of the flow velocities on basin-scale roughness parameters used in hydrological modelling."

Obviously, complex modelling strategies are required in this region. We do not suggest that modellers use these parameters for basin scale modelling. As set out in the discussion, we are attempting to understand why the basins are responding slowly.

To address the reviewers' confusion, we have added a discussion about modelling in Canadian Prairie basins. We now discuss how modelling is actually done in the region, using semi-distributed, physically based hydrological models. See lines 71-94 in the revised document, as mentioned in the response to the first reviewer.