Responses to Editor’s comments

Comment #1
I absolutely agree with you, that your data do not allow distinguishing between overland flow and stream flow. Yet the option that shallow overland flow is a limiting /explaining factor for the slow catchment response is worth to be discussed, also in the light of choosing 5/3 for beta.

Response
Thank you.
In fact the data does not allow us to distinguish directly among overland flow, stream flow or subsurface flow! Therefore we are being very cautious when describing the flow mechanism(s) as we cannot eliminate the possibility that some of the slow responses may be due to shallow subsurface flow.

In the abstract (line 15) we state:
“slow basin response at small scales could be related to predominance of overland and/or shallow subsurface flow over the very level topography”

We mention the possibility of shallow subsurface flows in several other locations.

However, we do discuss the role of overland flow, which is the primary runoff mechanism for Canadian Prairie hydrology.

We also emphasize that the most likely cause of the slow basin responses is the effects of overland flow, rather than channel flows. We do this in two ways:

1. We demonstrate that the flows are slowest where the basins are small and therefore the response times are dominated by overland/suburface flows. In larger basins, which are dominated by channel flows, the computed values of Manning’s n are similar to published values for channels.

2. We demonstrate that the computed Darcy-Weisbach f values for the small basins are similar to those found experimentally on vegetated plots.

Comment #2
Manning’s equation assumes fully turbulent flow and a flow depth that is much larger than the lengths scale of roughness element. This won’t be the case for overland flow on agricultural land, where roughness depends also on the growth status of the crops and surface preparation. While, I think that to using depth dependent Mannings coefficients is a quick fix, it clearly indicates the limitations of the Gauckler-Manning-Strickler equation ...

Response
We are not attempting to determine useful values of Manning’s n in this paper. The reason for estimating the roughness coefficients is given in line 330 of the revised paper:

“The roughness coefficients can be compared to other study values to evaluate the suitability of commonly used equations for modelling streamflows in these basins”

We are attempting to determine the validity of the equations at basin scales. As you note, and was demonstrated by the values plotted in Figure 9, Manning's equation is not useful for the basins which are (presumably) dominated by shallow overland flows. We state this very clearly in the paper:

Line 417
"The large values of n calculated here imply that Manning's equation does not adequately describe the flows in many of the small basins, as the values are far too large to be plausible."

Thank you for pointing out the Gauckler–Manning–Strickler equation which is how Manning's equation is known in Europe. We have added a comment regarding this to the main text at line 334.

Comment #3
By choosing beta equal to 5/3 you assume fully turbulent conditions for the catchment average. Recent work from my own group (Schroers 2022 HESS) revealed that Reynolds numbers of shallow overland flow were not necessary in the turbulent range. I think this choice needs to be discussed, even if there is no better alternative at hand.

Response
Thank you for the reference. We have now cited it at line 311 in the revised paper:

"... as Reynolds numbers for shallow overland flow are not necessarily in the turbulent range (Schroers et al. 2022)."

The reason for using 5/3 for beta is related to this statement on line 160:

"The premise of this research is that the hydrological responses to rainfall and underlying runoff velocities in Prairie basins are much slower than in many other regions. To avoid false confirmation of the premise, all assumptions herein are made to be as conservative as is possible, i.e., acting to maximize the estimated basin velocities."

We used 5/3 for beta because it is conservative, in the context of this paper, in that it will yield larger estimated values for the velocities than if we used beta = 3.

To clarify the reasoning, we have added a sentence to emphasize our reason for selecting beta = 5/3 on line 311:

"Estimating the velocities from the celerities by assuming β=5/3 is conservative for this study in that it gives larger estimated velocity values."

We repeatedly stress that the flows may not be fully turbulent. This was stated to be one of the reasons for using Darcy-Weisbach, on line 357:

"The Darcy-Weisbach equation, although less widely used than Manning's, has the advantage of being applicable across all flow regimes, from laminar to fully turbulent"

We also acknowledge that other researchers have found flows well within the transition from laminar to turbulent.

Finally, we state in the Conclusions (line 531) that "the assumption of turbulence is highly uncertain."

Comment #4
You calculated the Darcy-Weisbach roughness coefficient f, by solving the Darcy Weisbach equation for f and using the estimated empirical velocities. However, in open channel hydraulics the f coefficient is determined by the Moody diagram, as function of the Reynolds number and the relative roughness (Nikuradse roughness/ hydraulic radius Rh). Wouldn't it be interesting to either compare both values?

Response
As with the Manning's n calculations, the objective is to determine how reasonable our basin-scale values are for f. We compare the estimated values of f to published empirical values calculated from research plots, rather than comparing them with theoretical values from pipes or open channels. Note that we are validating our results against values for shallow overland flows, as discussed in comment #1.

With respect to comparing Darcy Weisbach with f from Nikuradse roughness/ hydraulic radius Rh, these were developed from pipes which cannot be compared to overland flow and were not pursued. Our experimental values include shallow overland flows and flows through depressions and culverts, none of which can be related to simple channel flows.

Comment #5
Is there any data evidence about the areas contribution to rainfall - runoff generation, beyond the assumption that it is the entire catchment?

Response
The only way to determine the fraction of the basin responding to the rainfall would be by modelling which, given the role of depressional storage in mediating Prairie runoff, would be very difficult. Assuming that entire basins are responding is as conservative as possible. All of the empirical equations are solved using the parameters for whole basins as these result in the largest calculated times to peak. If the whole basin is not responding then empirical equations would be under-estimating the response time much worse than they did.

When examining the basin responses, we use the maximum observed time to peak as the assumed basin response time. However, we do note in line 200 that:

"Precipitation events large enough to cause runoff over a whole basin may have durations of runoff smaller than the time of concentration of the basin, causing the basin responses to be asynchronous, and resulting in reduced peak times."

Thus, the maximum observed peak time may underestimate the true response time of a basin. So the assumption that the entire basin is contributing flow to the events is conservative

Comment #6
Maybe I missed it, but it seems that there is a mismatch in the temporal resolution of discharge (hours) and rainfall (days). How does this mismatch affect the calculation of centroids and the related ratios?

Response
While a mismatch might be perceived, there is no problem because the rainfall values are not used in any of the calculations. We do not compute the centroid of precipitation or any rainfall/runoff ratios.

The reason for including the rainfall data was to illustrate the precipitation driving the streamflows and to aid in the determination of simple runoff events. For example, Camrose Creek near Camrose (gauge 05FA025) responded very slowly as plotted in Figure 5. We divided the response time of the stream into 2 periods, based in part on a gap in rainfall from July 23-24. Thus, we computed the response time of the basin as being 190 hours, rather than 403 hours, to be conservative.

We have re-emphasized this by adding to line 129: “The intent in determining the mean daily rainfalls was only to confirm the existence of rainfall events which occurred before the streamflow peaks. Daily rainfall data was sufficient for this purpose.”

Responses to Reviewer #1

Comment
I have reviewed the manuscript by Shook et al., in which they estimated response times, flow velocities and roughness coefficients for basins in the Canadian prairies. They claim that the velocities found are much smaller than previously thought, and that Manning's n values were higher than expected, and this has relevant implications in the parametrization of models for this region. If the paper only provided estimation of response times, flow velocities and roughness coefficients for a given region, this would have been a very simplistic manuscript. However, I believe this is a relevant contribution because it highlights previous misconceptions about the Canadian prairies. The authors try to relate the velocities or Manning's n values to basin's characteristics, but no strong correlations were found. However, the discussion is rich and it provides hypothesis on why the correlations with basin's characteristics was not significant. From reading the discussion, it's clear that the authors have good knowledge about the hydrology of the study area (the Canadian prairies), and this paper provides important insights about the hydrological processes of the region. I therefore recommend that the paper should be accepted, but some concerns are presented as follows.

The manuscript is overall straight-forward, but I believe that there are some missing references. I think there could be more references about other studies that show the variability in estimating Manning's n values or the time of concentration, for instance (e.g., Grimaldi et al., 2010; https://doi.org/10.1080/02626667.2011.644244).

Response
Thanks. The reference has been added.

Comment
I think the structure of the paper could be revised. For instance, there is a sub-section "darcy-weisbach roughness coefficient f", and it looks like the remaining of the text is within this sub-section, because the next sub-section is already "summary and conclusions". Moreover, the "Summary and conclusions" section should be a wrap-up about the manuscript and not cite new references (L516-517).

Response
Unfortunately, the heading “Discussion” was accidentally deleted. It has been restored.
The section from lines 515-524 has been moved to the discussion.

Comment
Other specific major concerns are:
L 89: "the basins are dominated by agriculture" - are there any effects of water use for irrigation then? Because if yes, then this should be discussed. Moreover, are there any controls by dams in the region? If not, then please state this. This is relevant because later in the manuscript, you discuss about the effects of roads, so other factors such as water use for irrigation or impact by dams should be presented.

Response
None of the streams are affected by damming or irrigation diversions. It is stated on line 78 in the original paper (line 79 in the revised document) that all the streams examined are unregulated, meaning that no dams or irrigation diversions exist.
To make this clearer, we have added the sentence:
“This region is under dryland farming, i.e. without irrigation.”

Comment
L 294-295, 298: Isn’t the celerity calculated as Lc divided by tp? In the manuscript, it is stated that celerity is calculated by dividing tp by Lc.

Response
Oops! You are quite right. It has been fixed. The error is only in the text; the calculations are correct.

Comment
L 544 "All data, R code, and calculation results used in this research will be published online at zenodo.org": ideally as a reviewer I would like to have access to these calculations, so I would be able to double-check some results. The link to the data should have been submitted in conjunction with the manuscript.

Response
Agreed. It has now been added.

Comment
Minor concerns:
L 25-26: "region's cold region"

Response
Thank you. We have fixed this rather awkward phrasing.

Comment
L 70: "general feature of the region" -> please specify which region, Canadian prairies?

Response
Yes. This has been clarified. The sentence now reads:
“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

Comment
L 74: "the the response times"

Response
Thanks. Fixed.

Comment
L 77 and 78: why is "study area" within "data" ?

Response
Thanks, the heading has been changed to “Study Area and Data”

Comment
L 82: "other factors believed to influence" - too vague. please cite these factors

Response
Agreed.

Changed to
“factors (stream lengths,surface geologies, and depressional storages) believed
to influence”

Comment
L 87-88: "If small velocities are documented in the study basins.." - I am sorry, I didn't really understand this sentence. Is this a hypothesis?

Response
This statement is related to the objective stated in line 70:
“The objectives of this research are to determine a) if small runoff flow velocities are a general feature of the region,”
Changed to
“If small velocities are documented in the study basins, then in concert with the data for Steppler and St. Denis, it may be concluded that they are a feature of the Canadian Prairie landscape.”

Comment
L 92: no comma between subject and verb ("equations for basin response times are listed in Table 1")

Response
Thank you. It’s been removed.

Comment
L 110: why May 24?

Response
On the Canadian Prairies, the spring melt of the accumulated snow pack generally occurs in the months of March and April, although it can occasionally extend into early May. Using the end of May as the beginning for rainfall runoff is a very conservative assumption. May 24, which happens to coincide with a national holiday, is generally considered to be the beginning of summer in the Canadian Prairies. It is traditionally the date when people plant their gardens as the soils are warmed and the risk of frost is low.

The text now reads:
Manual gauging values obtained between May 24 and September 1 (which very conservatively approximate the frost-free period) in the Canadian Prairies)

Comment
L 116: unfortunately, an unpublished work is a weak reference. is it under review?

Response
No. The text is changed to
“Many of the selected basins responded to large-scale rain events in the summer of 2011 (not shown here).”

Comment

Figure 3: the colors of blue and green are very similar. maybe for the badland and montane graphs the gauging station code could be written on top of the graph?

Response
The figures have been re-plotted, separately listing the gauging stations directly for the badland and montane graphs. The colours are now selected from a palette which is more friendly for colour-blind readers.

Comment
For equations, please make sure that units are presented (e.g., unit of tcK in equation 1, is it hours?)

Response
Thank you. The units have been added.

Comment
L 240, 241, 242, 270, 271 (...): please adjust the formatting so that the variables are defined within the text (as they were defined in the previous equations), and not as items.

Response
Done

Comment
Equation 3: not ideal to have a variable with two letters (HT). It could be interpreted as H times T.

Response
Agreed. However, as was stated on line 204 in the original paper (line 214 in the revised paper): "Note that the equations given below are as they are taken from the literature, so the symbols used, and their units, vary."

We have kept the original nomenclature because:
1. It is consistent with the original work,
2. As was stated, the units may vary among the authors. Note that the area is sometimes in km^2 and sometimes in ha. Changing to a common symbol might imply that the units are also common.
3. The methods by which the values were obtained may also vary, and in many cases are not known by us. Therefore, we are not exactly sure of their actual meaning.

We have also made sure each was in italics and that there are spaces between elements so HT differs from H T

Equation 5: same problem of a variable defined by two letters. Also, previously, the authors used A for drainage area. Please be consistent with variables and abbreviations.

Response
See above response.

Comment
L 362: the section should be called "Results and Discussion", because the results are presented and already discussed.

Response
We have now separated section 4 (Results) from section 5 (Discussion)

Comment
L 375: why is table 3 presented before table 2?

Response
It has been moved.

Comment
L 383-384: "the points are coloured according to the basin topographic type" - this is more appropriate to be written in the figure's caption

Response
Agreed. Done.

Comment
L 410: use comma instead of point

Response
Thank you. It has been fixed.

Comment
L 424-425: "The values of f for basins ... in Figure 10" could be described in the figure's caption instead.

Response
The reason for including the description within the text, rather than the caption, is that it gives the context needed for the next sentence: “The agreement between the observed points and the published values is remarkable ...”. This applies particularly for the specified stations.

Responses to Reviewer #2

Comment
Please see detailed comments in the attached pdf.
The paper generally gives an important contribution about the usefulness empirical formulas about time of concentration, time to peak and lag time in different sized catchments in Canadian Prairie basins. The evaluation of these formulas matters because many hydrologists use them without a critical understanding of their premises. Wrong estimation of response times leads to wrong shapes and peaks of the resulting hydrographs.

Response
We thank the reviewer for this comment as it aligns completely with the intention of our paper.

Comment
However, the analysis is not performed very well.
My main concerns are (please consider also the comments in the attached pdf):
• Time of concentration, lag time and time to peak have different meanings and physical backgrounds. Please clarify the differences between them and how it effects rainfall-runoff-simulations.

Response
We provided this in detail in the original manuscript, and emphasized it in in the section "Observed basin response times". It was also addressed in "Response times from existing empirical equations". It is our opinion that the reviewer's point regarding clarifying the differences between these terms was fully covered. The second part "how it effects [sic "affects"] rainfall-runoff-simulations [sic]" is outside the scope of our paper. We noted the importance to simulations in lines 25-34. Our paper does not include rainfall-runoff simulations so this part of the comment seems to fall outside of the scope of our paper.

Comment
• The distinction between stream flow velocities and overland flow velocities is important and not sufficiently discussed.

Response
Note that we do not use the term overland flow velocities to describe the basin responses, as we cannot distinguish between overland flow and sub-surface flows. However, the difference between basin velocity and stream velocity was addressed in several places. For example:
Line 398 (revised 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."
Line 435 (revised paper)
"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."
The word "basin" has been inserted before "velocities" in Line 435 to make this clearer.

Comment
• Empirical equations are only valid within the limits of the based experiments. This should be discussed in order to evaluate the empirical equations.

Response

Agreed. This is addressed in the section 3.3. "Response times from existing empirical equations" where the limitations of each empirical equation, i.e. how its originating data set differs from the conditions within the research basins, are discussed.
Unfortunately, many empirical equations in the literature either do not specify the experimental limits of the source, or the original limits have become lost over time. In a more perfect world empirical equations would have meta information about original limits, and limit extensions that have been determined in wider application.

Comment

- The discussion shows some of the shortcuts in the analysis very clearly. Why didn't the authors try to divide the catchments into more homogenous subcatchments?

Response
The study basins are defined by the gauges which exist within the region, and that streamflow data used to test/validate the empirical equations. There are no further gauges available by which to divide the basins into more homogeneous sub-basins. Note that the study basins were classified by their topographic type in order to see if this consistently affected the basin responses (it did not).
It may be possible for a modelling study to sub-divide the catchments, but modeling approaches remain outside the scope of our paper.

Comment

- The paper should be improved by discussing (and applying) hydrodynamic approaches (Only Costa et al. is mentioned as an 2D hydrodynamic application).

Response
Our paper presents an investigation into the observed characteristics of some basins to determine how they can affect modelling.

The objectives of the paper are stated in line 70 (revised 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."

The reviewer's comment seems to be based on an expectation that rainfall-runoff models are included in the study. They are not.

Comment

- It makes no sense to calibrate the Darcy-Weisbach f roughness parameter. Whereas Manning's n could be treated as an empirical parameter (However, it has also physical constraints), the parameter f is determined by the Moody diagram. It has a pure physical meaning and is dependent on the Reynolds number, the hydraulic radius and the absolute (Nikuradse) roughness. Therefore, we have an ideal basis to calibrate hydrodynamic models with this approach. However, the authors treat f as an empirical parameter like Mannig's n. I do not see any advantage when using f in this way.

Response
We disagree. The Darcy-Weisbach f roughness parameter values were estimated from the actual flow data. The values of f were not calibrated as might be done in a modelling exercise.

The Manning equation obviously did not work at basin scales in many locations. This indicates that it is not suitable for modelling flows which may have an overland component. Could the Darcy-Weisbach equation be a more practical method of modelling roughness in the Canadian Prairies? This was stated clearly in the revised paper in lines 358-361:

“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.”

Comment

- A scientific benefit of the paper would be in the comparison / connection of hydrological and hydrodynamic approaches to estimate the time of concentration

Response

Thank you. This is an interesting suggestion, but outside the scope of the paper. Such as study might be a valid follow-on. Our intention was to document the very low flow rates and relate them to the physics and characteristics of these basins and compare to the existing literature upon which modelling in the future might be based.

Detailed comments from the PDF

Line 40

Comment

More of hydrodynamic is missing in the paper!

Response

We do not understand this statement. Our paper is unrelated to hydrodynamic modelling so we are unable to determine what the reviewer expects to be added.

Line 50

Comment

Please provide more details of this catchment.

Response

We do not agree that more detail is required. We have already stated the location, the area, and the topography, and that the basin is described in detail in Brannen et al. (2015). Lines 35-38

“An example of a very slow Prairie event is shown in Figure 1, where a flood wave took about 39 hours to travel approximately 1.8 km from the inlet to the outlet of a small (gross area ≈ 1.2 km2) hummocky sub-basin near St. Denis, Saskatchewan, Canada, within the St. Denis Research Basin (SDRB). SDRB is a small (22.1 km2), relatively hummocky, endorheic basin which has been studied for more than 50 years. The basin is described in detail in Brannen et al. (2015).”

Figure 2

Comment

Where is the catchment of figure 1 located?

Response

Thank you. We have added the locations of the St. Denis and Stettler catchments in the overview map and include details in the caption.

Line 93
Comment
How do you get this data? Not from SRTM, correct?

Response
Yes, the main channel slope is derived from SRTM. The resolution of SRTM is adequate for determining the basin attributes used in this study. See the following references for confirmation on this point:

Annand, H. J., H. S. Wheater and J. W. Pomeroy (2023). "The influence of roads on depressional storage capacity estimates from high-resolution LiDAR DEMs in a Canadian Prairie agricultural basin." Canadian Water Resources Journal/Revue canadienne des ressources hydriques: 1-20

Datta, S., S. Karmakar, S. Mezbahuddin, M. M. Hossain, B. S. Chaudhary, M. Hoque, M. M. Abdullah-Al-Mamun and T. K. Baul (2022). "The limits of watershed delineation: implications of different DEMs, DEM resolutions, and area threshold values." Hydrology Research

Nazari-Sharabian, M., M. Karakouzian and S. Ahmad (2019). "Effect of DEM Resolution on Runoff Yield, and Sensitivity of Parameters Contributing to Runoff in a Watershed." Preprint http://dx.doi.org/10.20944/preprints201901.0192.v1

Yang, J. and X. Chu (2013). "Effects of DEM resolution on surface depression properties and hydrologic connectivity." Journal of Hydrologic Engineering 18: 1157-1169

Line 95
Comment
Why 2 years return period?

Response
The 2-year return period was used by Godwin and Martin (1975), who did the delineation for the Prairie Farm Rehabilitation Administration. The return-period length was presumably used as it gives the median peak flow, which is useful in characterizing the highly skewed discharges in the region.

Line 133
Comment
The accuracy of SRTM is not sufficient for basin cannel estimation.

Response
We agree that the SRTM data are coarse and this limits their use. However, we used SRTM to estimate the length of the main channel, and the elevation difference between the divide and the outlet. We did not do any form of drainage network delineation, or channel width delineation, using it. As noted above, a higher resolution dem would not affect the area, the length of the main channel nor the highest and lowest elevations on this nearly flat landscape.

Line 155
Comment
definition of response time (see below) is missing.

Response
Altered the sentence at line 151:
“As is described in the next section, there are many existing empirical equations for basin response times.”

Line 164
Comment
Please analyse the differences and the consequences!

Response
The differences among the varying definitions are detailed and discussed. Again, modelled consequences are outside the scope of the paper.

Line 170
Comment
That is fine. However, describe the implications for the further analysis.

Response
We do not quite understand what the reviewer is requesting. We state the meaning of each method of defining the response time, including the fact that the time of concentration cannot be measured empirically.
To address this, we have added a summary paragraph at the beginning of this section “Modelling based on these empirical relationships requires caution and understanding of the assumptions of any chosen for use. The empirical relationships are based on quite limited sets for observations and extrapolation to different landscapes can be challenging. As will be seen, most available empirical equations fail on the landscapes in this study so modelling based on those relationships is likely to be unsuccessful.”

Line 203
Comment
What means "some differences"? Be precise.

Response
The differences were explained above. However, the sentence will be clarified. Line 203 now reads:
“The definitions of these response times are similar enough that they can be compared with the observed t_p values, despite the differences described above.”

Line 295
Comment
The other way round!

Response
Thank you. As we said in response to reviewer #1, the typo in lines 295 and 298 has been fixed.

Line 335
Comment
That is not so important (common sense).

Response

The reviewer’s comment may be true. But the intent here is to clearly document the simple way in which we estimated the hydraulic radius at the scale of these basins and not make the reader guess.

Line 357
Comment
f is a function of Reynolds number and Nicuradse roughness. There is physic behind it! You treat it like an emperical equation..

Response
That is not what we are doing. We are trying to see if Darcy-Weisbach could be used at large (i.e. basin) scales, where the Manning equation evidently cannot. As stated in the text, Manning requires fully turbulent flow, while Darcy-Weisbach works in all flow regimes. If Darcy-Weisbach works, then some of the issues with using Manning in these basins may be related to the flow regime, as is discussed.

Line 383
Comment
tp and tc and tl are different parameters!

Response
These are different ways of conceptualizing the response of a basin as described in section 3.1. The values $t_{p,}$ t_c and t_l are not parameters.

Line 388
Comment
All equations cannot be applied in a meaningful way (= are useless). The mean ratio says nothing.

Response
We are not sure we entirely understand this statement. The mean ratio is a simple test to demonstrate approximately how well each empirical equation did in representing the response times of the basins. Equations where the mean ratio is near 1 did a better job than the those equations where the ratio is very small as they grossly underestimate the basin response times.

Table 3
Comment
Add Lc

Response
The value of Lc is given in Table 1, which lists the basin parameters. Table 3 lists the responses of the basins, so putting a basin parameter in Table 3 would be redundant.

Comment
Why do you apply the ratio 3/5 here? This is not valid here.

Response
We assume that the reviewer is referring to the 5/3 ratio between celerity and velocity as in equation 7. This is how we are estimating basin-scale velocity from the celerity. We are not aware of any invalidity in its use here.

If the reviewer is referring to the use of beta=5/3 being valid for turbulent flows, we agree. As is stated in the response to the editor above, the reason for using this value is to be as conservative as possible, i.e. to ensure that the computed velocities err on the side of being too large, rather than too small.

Line 417
Comment
and too small!!

Response
Possibly, however the smallest computed basin-scale Manning’s n values are greater than that for a clear straight channel.

Figure 9
Comment
This is the n for channel and surface. You should introduce two Manning's n seperately for surface and channel.

Response
The reviewer has misunderstood the figure. All the points are for basin-scale estimates of n. The dashed line is simply to put the points into context, by allowing the user to compare the values to a value that they might be familiar with, i.e. that of a straight stream, as taken from the literature. This is made clear in the figure caption.

Line 429
Comment
Therefore, f is also not plausible. n and f are closely related (see eq. 8 and 12).

Response
We show that f values that we have estimated are quite plausible for the smallest basins, unlike the n values. Furthermore, as we have stated, n and f are only related when the flow is fully turbulent, which may not be the case.

Figure 10
Comment
These Darcy Weisbach values do not match with the Moody diagram. Which sense do they have?

Response
We do not understand what the reviewer means by this. We are not talking about flows through pipes or in channels, but very complex flows through vegetation, through culverts and depressions. The Darcy-Weisbach values are not expected to match with values from a Moody diagram. We do not mention Moody diagrams in the manuscript, and we do not make this comparison.

Line 431
Comment
Therefore, we have to distinguish between channel flow and overland flow!

Response
Yes, that is one of the points of this paper.

Line 444
Comment
That is okay because you compare tp with tp within the same slope range. All empirical formulas are only valid within their experimental boundaries.

Response
That is correct for some, but not all of the empirical equations. That of Watt and Chow did not work well:
Line 442 (revised paper)
“ The slopes of the study basins lie within the range of those used by Watt and Chow (1985) to derive their relationship and at least three of the Alberta basins lie within the range of the areas of the basins that they used. However the values of t_{cK} and t_{lW} were much smaller than the observed tp value.”

So, slope alone cannot explain the results.

Line 471
Comment
Therefoe, you cannot model it in a lumped version. It must be divided into subbasins.

Response
We are not modelling or including rainfall-runoff models in this study.

Line 480
Check the effective area calculation. Is this equivalent to depression storage?

Response
The effective area fraction is the fraction of the basin which responds to flow at least 1 year in 2. It is the fraction of the basin that is not much affected by depressional storage. It was calculated by dividing the effective area (which was determined by Godwin and Martin (1975)) by the basin area. There is no error in this calculation.
However, the effective area fraction is related to the depressional storage, in that basins with very large effective fractions will tend to have small depths of depressional storage and vice-versa.

Line 487
Comment
Correct!!

Response
Thank you.

Line 491
Comment
This is a further argument for hydrdynamic approaches.

Response
Yes, but that is not what we are trying to do with this paper. Rainfall-runoff models are not included in the study.

Line 497
Comment
good discussion.
Response
Thank you.

Line 518
Comment
good!
Response
Thank you.

Line 522
Comment
Exactly, therefore hydrodynamic modelling is needed!

Response
Possibly true. However, it’s not what we are doing. The reviewer’s comment seems to be based on an expectation that rainfall-runoff models are included in the study. They are not.

Line 532
Comment
Good!

Response
Thank you