

***Interactive comment on* “Estimation of overland flow metrics at semiarid condition: Patagonian Monte” by M. J. Rossi and J. O. Ares**

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Reply to Mr. Harman

RC: Reviewer comment; AR: Authors reply. References to Discussion Paper: P5837-5869; References to Revised ms.: P1-35

Dear Editor,

please find below our reaction to Mr. Harman comments. The reviewer has apparently performed some rough calculations on our data mixing flows at some plots with non corresponding areas and came up with grossly wrong estimates of water flow and slopes. These errors further plagued his analysis of the rest of the paper. In this reply

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note, we follow your indications to address in detail the reviewer points concerned with the validity and representativeness of the flow rates used experimentally - the defensibility of using the kinematic wave approximation in the context of steep slopes - and the potential for spurious correlation. We show the errors of Mr. Harman flow and slope estimates, and the consequent irrelevance of his comments on the representativeness of the kinematic wave approximation to our case. We also include all the data (in Table or graph forms) used in statistical analyses in order to address eventual spurious interpretations of the correlations involved. Detailed comments to these and the rest of the reviewer comments follow below:

RC1. “The approach to overland flow experimentation seems deeply problematic: a single nozzle delivering inputs of up to 5mm per second (based on my estimate of the 30x30mm delivery area in figure 3). This is far higher than anything that would be observed in even the most extreme storms. What is the point of this?”

AR: Mr. Harman made some rough estimates of flows where he mixed up flows with un-corresponding plot areas as estimated from Figure 3. This is incorrect because plots with higher flow inputs (not represented in this Figure) also corresponded to wider wet areas. Should one follow the reviewer procedure using corresponding values, the correct values to be obtained were: P1: 2.7 mm/s, P2: 0.32 mm/s, P3: 0.25 mm/s, P4: 0.15 mm/s, P5: 0.34 mm/s, P6: 0.49 mm/s, P7: 0.71, P8:0.45 mm/s, P9: 0.29, P10: 0.25 mm/s. These are in average one order of magnitude lower than Mr. Harman estimate. The point is that it is inadequate to force the analysis of water inflows in our study into one of a rainfall-runoff experimental setup, as implied in the estimations by the reviewer. In our case, as in many other plot studies on overland flows, water input and overland flow occurred in different areas. Areas directly receiving the water inflow were excluded from further analysis of water movement. Moreover, a rainfall-runoff comparison (if at all pertinent) should take into account the area occupied by the overland plume that does not receive water other than that coming from the inflow area. This can be estimated from data in our Table 2, lines 7 and 11. Taking half of

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the final plume areas as a conservative (low) estimate of the average areas involved in overland flow, the resulting numbers would be: P1: 0.06 mm/s, P2: 0.24 mm/s, P3: 0.17 mm/s, P4: 0.06 mm/s, P5: 0.01 mm/s, P6: 0.016 mm/s, P7: 0.008 mm/s, P8: 0.011 mm/s, P9: 0.014, P10: 0.014 mm/s. These are in average (0.061 mm/2))two orders of magnitude lower than the reviewer estimate. The relevant point is however that it is meaningless to compare the amount of water participating in overland flow at any area of the soil surface with a nominal rainfall input. Sources of overland flows may exceed rainfall rates by orders of magnitude because of flow routing, stem flows, etc. In their pioneering work on flow resistance in semi-arid environments, Abrahams-Parsons-Luk (IAHS Publ. no. 189,1990) used trickles to apply as much as 140000 mm³/s to 10 m² plots. Dunkerley (Earth Surf. Process. Landforms 28, 475–491, 2003) in studies on friction of shallow overland flow used input flows in the range 1×10³ to 9×10³ mm³/s in plots of 1.4 m². On an area basis, these antecedents exceed our flows by more than 3-4 times. We conclude that the flows we used are adequate for the kind of experiment we performed.

RC2. “The authors discuss “depression storage” (DS) at great length, despite the fact that A) their sites appear to be on slopes of around 10-20.”

AR: The reviewer estimate of slopes is also wrong. Plume start-to-end slopes in our study (as estimated from Figure 3, P5866) are: P5: 11.7%, P6: 29%, P8: 5.3%, P10: 1.7%. Average slope: 12%, Median: 8.5%. Slope estimates based on Figure 3 are however deceiving because the scale of the images is not sufficient to represent slopes changes occurring along the main direction of the plume flow. At a microtopography scale, overland flows advance over variously sloping ground in all directions occupying small depressions, sorting small mounds or over nearly flat spots. This is the spatial scale at which our study was performed. A more accurate estimate of average slopes involved at this scale can be obtained from data in Table 2, Line 13, (the average height drop S (mm) along the plume edge at each time interval). Slopes lengths (mm) can be estimated as:

$$L = (A^*)^{0.5} / n$$

where A^* is the total plume area (Table 2, Line 11), and $n = 14$ is the average number of time intervals (see P9 L10). Slope values ($S/L \times 100$) are: P1: 1.1%, P2: 14%, P3: 2.9%, P4: 7.2%, P5: 7.8%, P6: 1.7%, P7: 2.2%, P8: 5.5%, P9: 11%, P10: 3.0%. Average slope: 9.9%, Median: 7.5%. These values are included in the revised ms. (P7, L27-29). The slope of the field transect along which all the plots were selected is 7.2% (see P7 L18). These are about 50% of Mr. Harman rough estimate. To our knowledge, there is no reason why DS might not occur under these conditions. In fact, we did observe DS areas in our plots, as described in our ms (see for example Figure 2b, Figure 3).

RC3. “Their model is a spatially lumped model based on the kinematic wave equation that seems totally inappropriate for the application. Given the extremely high input rates of water and the slope of the ground, the counter-slope inundation referred to as “depression storage” seems to be FAR more likely to be due to inertial and hydrodynamic forces than to depression storage. Moreover where the intent is to estimate the effects of depression storage, surely it is not appropriate to assume (as the kinematic wave approximation does) that ground and friction slopes are equal? For this reasons it seems very unlikely that the inertial and hydrodynamic terms of the St Venant equation can be neglected in this case.”

AR: This comment is plagued with Mr. Harman previous erroneous estimate of flows and slopes in our study. As shown above, water input rates were not “extremely high” but just adequate to the experimental setup. DS implies counter-slope flow advances (but also see Figure 2). Mr. Harman speculation about the relevance of inertial and hydrodynamic components is not supported considering his errors in the estimates of flows and slopes. Froude numbers corresponding to our plots confirm that inertial forces were small in all cases (see Table 3).

RC4. “Their description of the model also seems to contain some errors (see below

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also). Equation 1, the mass balance for the overland flow plume includes a term “overland flow” $O(t)$ with units of mm^3/s . What is this? It is never explained and its presence is mysterious given that the model is lumped for the whole plume.”

AR: Please clarify this question. Equation (1) (P5846) is a standard formulation of a differential equation describing the change of the state variable Q_0 . Units of all terms are correct.

RC4. (cont.) “Equation 9 seems to be the Chezy equation (incorrectly referred to as the “Darcy” equation) rearranged to give the flow depth, and incorrectly using the water inflow rate (W) rather than the velocity (V). Consequently the reported values of the friction factor C implicitly incorporate the wetted width and depth of the overland flow plume. This appears to be an error of method, rather than just a typo, given that the values of C and Darcy-Weisbach friction factor presented in table 3 do not follow the standard relationship $C = \sqrt{8g/f}$. Moreover the reported values of friction factor in table 3 are on average two orders of magnitude higher than those of more careful studies (e.g. Parsons et al 1994).”

AR: Equation 9 is not the Chezy equation because W is not a speed but a water inflow rate (P5848L17). Accordingly, C is not a friction factor (in Chezy’s sense), and the reference to Darcy friction law is proper. There is no ground to expect that C would follow $C = \sqrt{8g/f}$. Darcy-Weisbach friction factors have been shown to vary by orders of magnitude depending on the experimental setups used to generate overland flows. (Parsons et al., 1994). Their results of these authors in studies of Dunne overland flow differ by one order of magnitude from similar studies of their same research group (Abrahams et al., 1987). It is not surprising that some friction factors in our study where Horton overland flow predominates are considerably higher than those reported in the studies above. Under Hortonian conditions, the flow speeds are considerably lower than those over saturated soil, because infiltration extracts water from overland flow. However, we agree that a detailed discussion of Darcy-Weisbach f values based on observed Hortonian flows is probably inadequate for most purposes, and is certainly

beyond the scope of our study. Accordingly, we decided to drop the subject in the revised ms.

RC5. “Two forms of the Green-Ampt infiltration equation are used, one with saturated and one with unsaturated hydraulic parameters. The saturated parameters are used at the edge of the plume where the up-gradient flow is occurring (which is weirdly assumed to be the depression storage, thus justifying the saturated conditions!).”

AR: The fraction of DS over the whole area is estimated by sampling at the edge of the plume at each time interval. It is not “weird” to relate upslope advances with the occurrence of DS. (See revised ms., Figure 2, P9L20 and ss.)

RC5 (cont.) “The unsaturated parameters are used for the remainder of the plume, using the antecedent moisture to get values of K from assumed characteristic curves. This is an unusual use of the model, and contradicts the standard assumption for Green-Ampt that infiltration under ponding is always saturated. Given that (as a result of the way depression storage is defined) areas initially defined as “depression storage” will later switch to “non-depression storage” (as shown in figure 5) infiltration in these areas will switch from saturated to unsaturated conditions while being ponded the whole time. This is all deeply un-physical.”

AR: There is no contradiction to the Green-Ampt model. Under un-saturated conditions, our formulation might be unusual but is nevertheless correct. See Appendix I of the revised ms. for a full description of the rationale applied. Areas that are ponded at the beginning of the experiment might not remain in this stage during the overland flow expansion, depending on the local balance of water inflow and infiltration rates. Overland flow is variously re-routed during its expansion and changes in local infiltration as the soil becomes saturated may deplete pond water in areas previously ponded. Additionally, our procedure to estimate DS is based on a moving cumulative average algorithm (see AR to RC5 above). Interim estimates might over-sub-estimate DS. This does not have a physical meaning, but is due to the sampling procedure.

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RC6. “The methods used to perform model inversion are never specified, and amongst a lot of important-sounding discussion about the “convergence criteria”, it is almost impossible to determine which parameters were actually calibrated. For example on page 5848 it is stated that Ksat was estimated from an ANN pedotransfer function, but later on p 5851 it is stated that the “model estimates of Ks at the upper vadose zone were significantly correlated to the ANN estimate based on textural data”. As far as I can tell one spatially and temporally uniform value of Ksat and C (the pseudo-Chezy- Darcy friction coefficient) was obtained for each site, which implies an independence of the estimates between sites. But then there is this statement: “Confidence intervals ($P < 0.05$) of the correlation coefficient r of measured-modelled values 9–16 were built by bootstrapping paired comparisons such that randomly selected 9 plots were used for model calibration and the rest for model validation.” This is confusing both because it seems to imply that model estimates are not independent between sites, and because Ksat is one of the values being referred to (value 9 in table 1) and C is not!”

AR: The description of the calibration procedure was reformulated. See Section 3.1 of the revised ms.

RC 7. “Equations 11 to 15 present a number of composite variables that are later regressed against each other and other model parameters. These variables seem to have no purpose apart from padding the results with meaningless discursion.” For instance, equation 12 defines a variable that is an unexplained transformation of the ratio of the final wetted area and the total applied volume (where did a and b come from?).

AR: The statistical analyses of model variables is dropped in the revised ms. of this Discussion paper.

RC 7 (cont.)“...Equation 13 is defined as the “run-off coefficient, dimensionless” but is neither equal to the runoff, nor dimensionless (it is actually equal to the total applied

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water, minus twice the infiltrated amount, minus $O(t)$, which as I said is not defined”).

AR: Run-off coefficient defined in terms of the model variables (see Eq. 13, revised ms.).

RC 7 (cont.) “...Equation 15 is the “average overland for velocity” but there is no justification for this definition in terms of the model previously presented.”

AR: The model calculates expanding flow areas. A^* is previously defined in Table I, P5861. The sqr of these divided by the time is an estimate of overland flow (1-D) velocity. This simple relation does not seem to require an additional explanation after careful reading.

RC 8. The discussion and conclusions bear little relationship to the presented results. The authors argue that they are offering some sort of alternative hypothesis to the analysis of depression in Antoine et al 2012 that has something to do with unsaturated infiltration. The justification has something to do with the “time-serial correlation” of the DS and “run-off coefficient, dimensionless”, but given that the meaning of both of these variables is somewhat obscure (see above), it is hard to evaluate what claims are being made.

AR: The discussion is ordered in relation to the paper objectives declared in the Introduction. Alleged obscurity on DS and run-off has been addressed in the replies above (see AR replies to RC5-7 above).

RC 8. (cont.) The paragraph on p 5854 starting line 9 suggests that the model would not generate any runoff unless it was assumed that infiltration was unsaturated in the areas where overland flow was occurring (thus reducing the infiltration rates) but that the observed soil moisture profiles (obtained from a few soil cores) could not be reproduced without assuming that the infiltration was saturated in the “depression storage” areas. There is no actual support for this statement in the results.”

AR: Statement based on data not shown in Results was dropped in the revised ms..

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RC9. “Figure 6 shows an extremely good correlation between the fiction factor C and the Froude number and a textbook example of a spurious correlation between Reynolds number and mean flow depth. In the first case, it is almost impossible to determine whether this relationship has any meaning given the issues with C described above and the possible presence of compensatory artifacts introduced by the calibration process. In the second case the entire relationship depends on the inclusion of a single datapoint. Exclude that point and there is no relationship. This does not prevent the authors from claiming that this result shows that the effects of temperature on the kinematic viscosity of water (and hence the Reynolds number) should be considered in the estimation of overland flow depth”.

AR: The issues with C described above by the reviewer have been rebutted (see AC to RC4. above). “Compensatory artifacts” invoked by the reviewer (but not specified) are unlikely given the simultaneous calibration of the model to fit parameters (9-16) in Table 1. Note that calibration on $A(t)$ is time-serial (see Figure 5), which further restricts the possibility of compensatory artifacts during the calibration process. Our comment on the significance of temperature in calculating Re (see P5855, L14 and ss.) does not imply assigning any spurious meaning to Figure 6. Since this is secondary to our main objectives, this issue was dropped in the revised ms.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/9/C3557/2012/hessd-9-C3557-2012-supplement.pdf>

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