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Discussions

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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Rossi and Ares report on a series of overland flow experiments on ten small areas of desert where surface microtopography had previously been estimated using "close range stereophotogrammetry". The flow experiments appear to consist of a single nozzle supplying water at a fairly high flow rate close to the surface. A camera mounted overhead records the evolution of the 'plume' of water as it accumulates and flows overland for anything from 4 to 18 minutes. The authors then use a model (of apparently their own devising) to invert these observations (along with a range of soil moisture observations made at the same time) for the friction and infiltration properties of the soil.

It is difficult to know where to begin with this paper. There are some unique aspects, and the subject matter is worthy. However there are some serious issues with the central issues of depression storage, infiltration and modeling. I can only provide a discussion of a few of the issues here, and recommend rejection from HESS.

1. 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?

2. 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

3. 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.

4. Their description of the model also seems to contain some errors (see below also). Equation 1, the mass balance for the overland flow plume includes a term "overland flow" O(t) with units of mm3/s. What is this? It is never explained and its presence is mysterious given that the model is lumped for the whole plume. 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).

5. 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!). 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.

6. 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 signiïňĄcantly 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: “ConïňĄdence 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 ïňĄve 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!

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?). 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 water, minus twice the infiltrated amount, minus O(t), which as I said is not defined). Equation 15 is the “average overland for velocity” but there is no justification for this definition in terms of the model previously presented.

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. 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.

9. 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.

Parsons, A. J., A. D. Abrahams, and J. Wainwright (1994), On determining resistance to interrill overland flow, Water Resour. Res., 30(12), 3515–3521, doi:10.1029/94WR02176.

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