

Interactive comment on “Comparisons of stemflow yield and efficiency between two xerophytic shrubs: the effects of leaves and implications in drought tolerance” by C. Yuan et al.

D. Dunkerley (Referee)

david.dunkerley@monash.edu

Received and published: 9 September 2016

This paper reports field data on stemflow volumes from a dryland field site in China, collected over two successive annual wet seasons. The paper is systematically presented, though rather too long in light of the scope and volume of the primary data that are presented. The field data are of interest because they include stemflow measurements at the scale of individual branches.

I felt that the authors needed some evidence to support their repeated claims (e.g. line 58-59) that stemflow exerts a high influence on the survival of dryland shrubs, especially under drought conditions (e.g. line 107 refers to ‘... a novel characterization of plant drought tolerance...’ as one of the outcomes proposed for the present study).

[Printer-friendly version](#)

[Discussion paper](#)



The authors collected only data on rainfall and on stemflow volumes. They did not record soil moisture near the plant stems, or observe the fate of stemflow near the soil surface – where, for instance, it might be involved in lateral flow through organic litter materials, or indeed trickle away as overland flow. Instead, they were content to assume tacitly that all of the stemflow was plant-available. Soils are only briefly described, but the authors do note in passing that the surface textures differed between the two shrub species examined (refer to lines 136-137), one being loess and the other, sand.

Field experiments were conducted only during the rainy season (line 143) but about a quarter of the annual rainfall comes in the drier season, and I think that conditions then needed to be considered also, as the longer, 8 month dry season is possibly the time when plant available moisture is more critical.

Only four individuals of each species were instrumented to collect stemflow data. This is not a large sample, though I appreciate the tedium of instrumenting multi-stemmed plants. Furthermore, of the four plants, only about one third of the branches were instrumented for *C. korshinskii*, and less than half for *S. psammophila*. This reduces the effective sample size still further.

Given that it has often been reported that stemflow may fall from branches when rain becomes intense (and overtaxes the ability of stems to conduct all of the incident water), I wondered about the possible effects of trapping and diverting stemflow from so many branches into collecting vessels. This presumably reduced branch drip and so, perhaps, the branch flow carried by branches lying beneath higher ones from which the stemflow had been diverted. I think that the authors need to consider and discuss this possibility, in relation to the possible path of rainfall and throughfall (both free and released) through the canopy of these shrubs.

Relevant field data that I would have liked to see included in the paper are on air temperature, humidity, and windspeed. Solar radiation data would also be informative,

[Printer-friendly version](#)

[Discussion paper](#)



together with data on whether the rainfall was recorded primarily during daylight hours or at night, since this is relevant to evaporative losses and to the efficiency with which stemflow can be conveyed across the plant surfaces. The authors can hopefully shed light on at least some of these issues.

The authors are imprecise when reporting their results. For instance, line 287 reports average branch stemflow volumes in mL, but the authors do not state whether this is across all rainfall, or averaged per rainfall event, or processed in some other way. For reported stemflow volumes, the associated time period must be stated. Likewise, in line 297, 298, etc., are the volumes reported the sum of stemflow for all branches or the mean per branch or something else? The reporting needs to be much clearer. It is the same when the authors discuss funneling ratios in line 342 and following. Are the figures in this section ratios for individual rainfall events, or averaged over all events? As mentioned earlier, the authors also need to consider how the complete trapping of stemflow from upper branches might have affected the stemflow on lower branches, that might have received less drip from above.

I felt that the authors were vague in their discussion of other results. For instance, lines 366-367 state that precipitation amount was the most important rainfall characteristic that affected stemflow in the studied shrub species. Here I presume they mean that precipitation amount had affected aggregate stemflow volume (and presumably measured at rainfall event scale). Other aspects of stemflow, for instance the peak flux or rate of delivery of stemflow to the base of the plant, are much more likely to have been affected by rainfall intensity. I am not sure why the authors only consider overall stemflow volume, and they should make a case for neglecting other ways to characterize stemflow, including the timing of its delivery from the plant. Stemflow volume alone does not provide a complete exploration of the origin and fate of stemflow.

The fundamental argument of the paper is again in need of supporting evidence from the beginning of the Discussion at line 393. The authors discuss 'effective utilization' of precipitation but as pointed out above, have no data relating to this. Their data only

[Printer-friendly version](#)

[Discussion paper](#)



estimate stemflow volumes on above-ground parts of the plants. How this translates to soil moisture in the root zone (allowing for evaporation and interception on litter) is not clear. The authors should not make claims that are not supported (or supportable) using their available data. They argue in lines 404-405 about the 'effective utilization of precipitation' by the two shrub species in rainfalls of < 2 mm. However, any stemflow delivered to the base of the shrubs in what are likely to be short showers, might be largely lost to evaporation once the short event ended. This should illustrate how spurious it might be to infer utilization from stemflow data not supported by soil moisture data, or indeed by measures of transpiration by the plants. The authors proceed (e.g. line 420) to argue about energy conservation, again speculating about the utilization of stemflow from rainfall events of < 2 mm. All of this is completely unsupported by the data, and should be eliminated from the paper, or at least highlighted as completely speculative. Again, in line 430-431 the authors speculate about drought tolerance; not only do they have no supporting data, but the data that they do have were derived during the rainy season, and not in drought conditions at all. How the shrub foliage etc. might change during drought years remains unknown and the authors should eliminate all of their speculation about drought tolerance. Their data relate to stemflow alone, and they should restrict themselves primarily to discussing and interpreting those data. Lines such as 476-478 inclusive are completely speculative, though the authors write as though they are presenting a result from their work. They refer to stemflow production under 'water stress conditions' though they did not observe this; they refer to their estimated stemflow being 'of significant importance for the survival of the xerophytic shrubs, particularly during long intervals with no rainfall' though they present absolutely no evidence to support this claim, having no data from long periods with no rainfall. All of this speculation should be eliminated from the paper, or at the very least identified as speculation not supported by any data. Overall, the focus of the paper needs to shift from speculation to the discussion of what can validly be determined from the field evidence available, namely, the estimated stemflow volumes.

Minor errors:

Line 41: what are 'stemflow channels'? Does this imply fixed pathways? Line 41: 'pointedly' should be 'directly' or similar. Line 44: what is meant by 'biogeochemical reactivity at the terrestrial-aquatic interface'? Line 58: please cite references to support the claim about 'disproportionately high influence [of stemflow] on survival and competitiveness of xerophytic shrub species'. Line 81: insert missing space before 'Murakami' Line 155: how do branches exist 'as independent individuals'? Line 214: 'at the' should be 'in a' Line 238: should '4080-mm' be '40-80 mm'? Line 268 and many other instances: do not write '18-mm'; the hyphen is not allowed in the SI metric system. There must be a space between the numerical quantity and the symbol for the unit of measurement (e.g. '18 mm' is correct). Line 280: do the authors data justify 4 decimal places of precision? This requires fixing in many places, such as line 475. Line 475: should 'events of 12-mm' read 'events of 1-2 mm'? Line 492: 'had not determined yet' should read 'have not yet been determined'.

David Dunkerley Monash University

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-420, 2016.

[Printer-friendly version](#)

[Discussion paper](#)

