

Interactive comment on “Water-use dynamics of an alien invaded riparian forest within the mediterranean climate zone of the Western Cape, South Africa” by Bruce C. Scott-Shaw et al.

Bruce C. Scott-Shaw et al.

brucecharless@gmail.com

Received and published: 28 March 2017

RC1: 'Water-use Dynamics of an Alien Invaded Riparian Forest within the Mediterranean Climate Zone of the Western Cape, South Africa', Anonymous Referee #1, 20 Feb 2017

HESS-2016-650

Anonymous referee #1 (AR1) is thanked for their thorough review. The comments and suggestions provided were insightful and beneficial to the progress of this paper.

1. AR1 stated that there was a lack of international literature cited in the paper. “Invasive alien plants are a global problem and the authors should present their manuscript

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in a way that appeals to an international audience.” This comment was well received. A further 12 citations from relevant international studies was included in the introduction, and were later compared to the findings discussed in this paper. Although some global problems differ to the South African context, the inclusion of these findings is still relevant as suggested. An example of this is as follows: “invasive plants use up to 136 % more water than the indigenous species at the leaf scale (Baruch and Fernandez, 1993; Dixon et al., 2004; Pratt and Black, 2006). At the plant scale there is a diverse range in water-use ranging from the invasive species using 100 % less to 150 – 300 % more water than the indigenous species (Cleverly et al., 1997; Nagler et al., 2003; Kagawa et al., 2009). At the ecosystem scale studies indicate that invasive species use 189 % more water than indigenous dominates areas, particularly in tropical moist forests (Nosetto et al., 2005; Yopez et al., 2005; Fritzsche et al., 2006).”

2. AR1 suggested that the authors use their data to validate a relevant international model, which would further assist the paper in comparing their findings to findings highlighted in international literature. Although a good suggestion, the authors decided to exclude model results in this paper. This was partly due to a second paper that will be submitted subsequent to the publication of this paper, which uses the data from this study to validate an international model (SWAT). Recommendations were included in the discussion suggesting that this data be used to calibrate and validate hydrological models.

3. AR1 stated that the ‘Discussion and Conclusion’ section was very weak and needed to be revised. The authors agreed with this comment. In order to address this comment, this section was revised. Reference was made to international findings and how these compared to the results provided in the paper. Further information was provided on the importance of the findings and how they could be used for follow-up rehabilitation and research.

4. AR1 queried how the authors obtained total evaporation measurements when only tree transpiration was measured. A closed canopy was assumed as there was

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little to no short understorey vegetation, only litter. This assumption has been included in Section 3.4. Evaporation from the litter and canopy was assumed to be minimal. However, total evaporation was changed to transpiration as the authors agree that this is scientifically correct.

Specific Comments

1. Abstract line 26: add the phrase “of the heat pulse velocity sap flow technique” after the “heat ratio method”. Changed. 2. Abstract general: presenting the water use values in litres only doesn’t mean much to a water resources manager or other decision maker. I would prefer to see area averaged transpiration values (in mm). Both litres and mm were used for the benefit of readers specialized in this field as well as to the more general reader, such as a decision maker. This was updated in the abstract.
3. Pg 3 lines 32-44: How was the LAI measured? A description of the Li-Cor (LAI-2200) plant canopy analyser was provided in the methods.
4. Pg 3 line 48: Indicate that the diameter was measured at breast height (DBH) Changed.
5. The HRM method of the HPV technique is well known in eco-physiological/eco-hydrological cycles. So there is no need for the details on Pg 4 lines 15 to 35. Remove this text. Although the authors considered this, they decided to leave the methods used for the HPV technique as some of the readers are not familiar with the technical component used in this study and it would further benefit students using this technique elsewhere.
6. What would add value is more detail on: probe insertion depths and circumferential arrangements and how the conducting sap wood area was determined. The authors agreed that this would be useful information to include. This has been updated in the methods section.

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7. Pg 4 line 43 – sentence beginning with “ Given the homogenous: : :” Doesn’t make sense. Please revise. Sentence revised: “Due the homogenous composition of the alien stand and the dominance of Vepris and Celtis species within the indigenous stand. . .”
8. Pg 5 line 20: To confirm direct use of ground water, it would have been nice to do this using stable isotopes rather. Include this data if you have it. Unfortunately due to budget constraints, this was not available.
9. Pg 6 first paragraph: Fig 8 is discussed before Figs 6 & 7 which only appear on pg 7. Either revise the text or change the order of the Figs. There is also a mismatch with the units. Both the figure numbering and units were corrected.
10. The text, Pg 7 line 7 presents L/d, but Fig 8 presents water use in mm. Clarify this. Reference to Figure 8 was included in Section 3.4 where upscaled results were discussed. The units of L day⁻¹ were used for single tree results (allowing for tree comparisons) whereas mm was used for upscaled results that could be compared to water balance components.
11. Pg 7, lines 39-40: Not clear if the annual transpiration and ETo are for a single year or averages over 3 years. Please clarify. As the MAP was consistently high over the three years, with little variation between years, monthly averages over the three year monitoring period were used. This was done to simplify the graphs.
12. Pg 8 “Discussion and Conclusion”. As stated earlier, this section is very weak and I would prefer to see; 1) comparisons between the results found here and what other researchers found, and; 2) comparisons with international literature. As discussed in the general comments, this section has been revised.
13. I also have issues with the references: o The following references are missing in the reference list 1. Everson et al., 2007; 2. Richardson et al., 2007; 3 O’Grady et al., 2005; 4. Miller et al., 2007; 5; Everson et al., 2006; 6. Uddin and Smith, 2014. Format

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of the references in the references list is not consistent. Some journals are written in full while others are abbreviated. In places it is not clear to who reports are written for e.g. pg 11 lines 10-11 and other places. For other references, journal names are replaced with address of the institution. For example, pg 11 line 38 is a publication in the Agricultural and Forest Meteorology journal which is bizarrely replaced by the institutional address. The references were corrected

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-650/hess-2016-650-AC2-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-650, 2017.