Response to the reviewers

The reviewers are sincerely thanked for their contribution to this body of work. Responses to all comments can be found below and those in the annotated documents have also been addressed and highlighted in yellow.

Reviewer 1 (Miriam Coenders-Gerrits)

With pleasure I read the manuscript of Kaptein et al. It deals with an important question whether the newly planted GN use more water then Pinus elliottii. The authors compared two sites and equiped several trees with sapflow sensors to quantify the transpiration. As the HPV-system has many limitations in case one wants to know the stand transpiration, the authors did an attempt to quantify the relation between the sapflow measurements and the transpiration rates determined via a lysimeter. My compliments for doing this!

Overall, I like the study very much. It is relevant, well written and easy to read. I only have the following comments, which can improve the manuscript:

1. you conclude that the transpiration is GN is lower than in Pinus due to the lack of water. Of course this is true: transpiration reduces as water becomes more limited (fig 5). However, I wonder if this very low matrix potential is not caused by the high transpiration rates of GN before the study period? So that the GN depleted already the soil? (feedback mechanism). **Authors agree with this possibility. A statement suggesting this possibility has been added in the manuscript line 277 to 282. The important aspect is perhaps that GN have the potential for a high-water use if the water is available but over the long-term, they depleted the soil water reserves and over the measurement period the water use was reduced as a result.**
2. The contribution of fog. In line 70 you say that fog precipitation is significant. How does this affect your results? How reliable are your precipitation observations? And how would this affect your interception results? **We have modified the statement to read "The area experiences mist which could significantly contribute to overall precipitation through streamflow or canopy interception" (line 66 to 67). The rainfall has been measured accurately with a Texas Instruments research grade raingauge and with a second station nearby to collaborate our results. The accuracy of the rainfall results would impact the interception because the interception equation is based on the rainfall.**
3. Units/dimensions: I think the authors should do a carefull check on the units. In my view transpiration is a flux, thus having a time dimension. Therefore, most of the numbers given in the paper should have the unit mm/day or mm/month or mm/year (instead of mm). Some quick examples: L19, 151 (liter), 183 (mm/year), 213, 214, 215, 242, 265, 285 (mm/year), fig 3d (mm/day) and caption, fig 5 (rainfall mm/day), fig 5 (T=mm/day), fig 6 (rainfall mm/day), fig 8 (rainfall mm/day) and caption, fig 11 (mm/month) plus caption. **Agreed thank you. Completed throughout the manuscript.**

4. Equations 3 +4: I would make this liniear. No need (and reason) for polynomial. **Equations changed from polynomial to linear equation (line 219 to 225).**
5. Section 4.3: I would keep this section for qualitative as you did not measure interception and soil evaporation. Especially, since you have fog the interception could be very high. **Authors have removed this section and focused on the measured tree transpiration data.**
6. L187: unit of Is should be MJ/m2/d. Also check fig 3b. **Amended throughout the manuscript as suggested.**
7. I would recommend to write all parameters in the text in italic and make use of subscript (e.g., Tair => T_{air}): **Completed throughout the manuscript.**

Reviewer 2 (David Scott)

An interesting paper, representing a lot of difficult work, on a topic that receives little detailed investigation, despite its significance. Thus I consider the work worthwhile and worthy of publication, provided that several points are attended to in revision. Whether these represent minor or major revision is a matter of opinion.

1. the comparison between eucalypts and pine is relevant, but there is not enough hard data of make a definitive determination of their relative effects. I think that the title and abstract should soften the emphasis on this comparative water use. **Agreed and thank you for the useful suggestion. The word "comparative" in the article title was removed and the words "comparison" or "comparative" were removed from the abstract.**

2. Interesting data is presented on the comparative transpiration, but the other elements of total water use are poorly quantifiable, so the final comparison of ET by the two species is little better than speculative. **Agreed. Authors have removed the section with the comparison in ET between the two crops and focused on the measured tree transpiration.**

3. Specifically, the crops are very different ages and thus a direct comparison is not convincing (at least not without a lot more convincing information on why a comparison is reasonable. ***Eucalyptus*** **trees in South Africa are grown over 12-year rotation, while Pine trees are grown over a 25-year rotation. So, at 8 years for** ***Eucalyptus grandis*** **x** ***E. nitens*** **and 20 years for** ***P. elliottii*****, they were as similar stages in their growth cycles. This has now been highlighted in the manuscript (line 55 to 57).**

4. Secondly, the soil water component is poorly defined, and the soil & interception figures are estimated from such general sources (of dubious applicability) that the errors on those estimates make the overall estimates speculative. I think these are serious flaws, but I think the information on direct measurements in the paper are still important and useful. But the authors should not attempt to close the water balance except in a broad and speculative way. **The soil water content section has been improved (Line 129 to 142, line 198 to line 216). The sections that estimated ET has been removed and authors focused on direct measurements (transpiration) as suggested.**

2. My annotated version of the manuscript contains several parts where the language or expression is lacking and can be improved. **Thank you for these notes. Language has been improved throughout the manuscript as suggested.**

3. Section 2.5 on the soil water content left me puzzled, and I think that it and the later references to the soil water store, are wrong. Soil wetness is presumably measured with some sort of TDR instrument, that give a volumetric wetness at the 3 depths (this should be stated more clearly in the text - I don't know what a CS616 sensor is). **Clarity on the CS616 sensor has been provided: line 131 to 133 including a description of the profile soil water content calculation (Line 137 to 142 and line 198 to line 216) .**

(a) These volumetric wetness figures could be used to estimate a depth of water in the profile, which is a measure of the absolute amount of water available to the trees (PAW). It doesn't make sense to me to go through some sort of model to estimate the approximate soil water potentials that these wetness figures would relate to (section 3.2). If the objective is to consider plant available water, then it is best to use volumetric wetness, and convert these to an estimate of a depth of water in the soil profile at a each particular point in time. **Soil water potential was calculated through measured retention curves, but we agree that using volumetric wetness is most suitable and the soil water potential results have been removed. The measured soil water content was used to estimate the soil profile water content (line 129 to line 142) and results presented in Figure 4 and line 198 to 216.**

(b) With the deepest probe at 60 cm (properly 0.6 m in SI units), there is a large uncertainty about how much water is still available to the trees at depths below say 0.75 m? The estimates of soil water are therefore incomplete, and only give an indication of the real situation. The information on the response of the trees to increase water availability immediately after rain, or in the dry season, are interesting and useful, but it needs to be acknowledged that the trees may have access to water in parts of the profile that have not been monitored. {Consider Peter Dye's measurements of eucalypts under stress - 1996, Tree Physiology, 16: 233-238, which showed water was being used to 8 m below the surface and beyond}. **Agreed. A statement acknowledging that trees may have accessed soil water beyond 0.6 m was incorporated (line 209 to 216).**

(c) Related to the point in (b), more should be said about the soil profiles, and this should include materials below the conventional soil profile and include the substrate beneath, from which water may be drawn. Is it hard rock, or is it decomposing and permeable (as well as accessible to tree roots?) **Beneath the soil profile, there is weathered, unconsolidated material that is accessible to tree roots. This information has been incorporated (Line 73 to 76 and Table 2).**