

## ***Interactive comment on* “Can the *Pinus sylvestris* var. *mongolica* sand-fixing forest develop sustainably in a semi-arid region?” by Yiben Cheng et al.**

### **Anonymous Referee #1**

Received and published: 18 April 2019

Using a newly developed DSR lysimeter, the authors monitored the soil moisture dynamic and the DSR of the *Pinus sylvestris* in Mu Us Sandy land for 3 years (2016–2018). They found that there were two soil water recharge source with spring snow melting and summer precipitation. Also they concluded that the proportional precipitation intensities varied across recharge depth. This paper addresses a timely issue for sand-fixing forest, given expected changes deep soil recharge. In general terms, the manuscript has a clear focus and in situ measurements (although the methods are simple) adequately tested the research questions herein. Sections of the manuscript, however, take away from the strengths of this manuscript, particularly the Results and Discussion. Thus, I proposed a few comments as the following. General Comments.

[Printer-friendly version](#)

[Discussion paper](#)



1. The Title: “Can the *Pinus sylvestris* var. *mongolica* sand-fixing forest develop sustainably in a semi-arid region”. The topic was “too large”, and, the paper seemed like to study the soil moisture dynamics and recharge source, not relevant the subject. I really think that the study is interesting, but the title might reflect better the performed research. 2. It was not appropriate to rely on three years (2016-2018) of soil moisture measurements to determine whether the sand-fixing tree species survives. First, the experiment time was too short, and the artificial trees with life cycles over decades. The adaptability of long-lived woody species cannot be based solely on water, temperature, light, soil texture, etc. 3. Logically describe the work you do in the Introduction. For example, the description of the semi-arid regions and the drought situation in China should be merged into other paragraphs without the need for separate sections; and these statements were not relevant to the subject of this study. 4. The Result and Discussion should be separated. I saw more results but no discussion. 5. For the Summary and Conclusions, it reads too much like the Abstract and simply restates the main results, instead of leaving the reader with a “take-home message” and “fruit for thought”.

Special comments. 1. Need to mark the line number. 2. The main part of the Abstract focused on describing the research background (almost 1/3). I would expect some results and discussion or implications of the main findings. 3. L30. What do you mean the current precipitation conditions? Was it the annual precipitation (2016, 2017, and 2018) mentioned later? If so, what was the relationship between the evaporation and precipitation? 4. In the Keyword, I would recommend adding soil moisture and DSR, which were the two main monitoring indicators of this study. 5. L95. Replace with “over  $3 \times 10^5 \text{ hm}^2$ ” 6. L155. “In another word” is not common and can be replaced with “In other word”. 7. L170. The recharge depth of spring snow melting in Abstract was 160 cm (L25), why was here it 140 cm? Another problem was that I did not see the recharge depth in 2017 and 2018, and only in 2016. Can I think that the recharge depth in the Abstract was the 2016? 8. L 175. Why was there no change in precipitation in Figure 3? 9. L265. Move to the Methods. 10. In the Conclusions (No. 3), what was the

start of the year? 2016? why was there a negative value (-16 mm)?

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-110>, 2019.

**HESD**

---

Interactive  
comment

Printer-friendly version

Discussion paper

