Interactive comment on **"BVOCs emission in a semi-arid grassland under climate warming and nitrogen deposition" by H.J. Wang et al.**

Title: BVOCs emission in a semi-arid grassland under climate warming and nitrogen deposition
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MS No.: acp-2011-977
Special Issue: Integrated Land Ecosystem-Atmosphere Processes Study (iLEAPS) (ACP/BG Inter-Journal SI)

We would like to thank the reviewer for the detailed comments on our manuscript. Below we reply in detail to all comments.

1. In P5 L1: what is ANOVA? It should be introduced.

Response: ANOVA here is abbreviation of "analysis of variance", we just showed the statistical method we used. In fact, the statistical methods also were described in the followed section. Then we only keep *p* value and deleted ANOVA in the revised MS in P6 L6.

2. In P5 L5: "BVOCs emission was measured by static chamber technique for 11 times ……and for 13 times ….". Please explain the meaning of 11 times and 13 times, and introduce how many samplings were collected for different situations in 2007 and 2008 measurements.

Response: Generally, we collected samples on sunny day only every week. However, sometime there was no one sunny day during a week, which is the reason why different sampling times between 2007 and 2008. In each time, we collected all samples in all sites.

In the revised MS, we changed it. "We took samples for BVOCs analysis between 10 am and 2 pm on sunny day only (PAR>1000 μmol m^{-2} s^{-1}) every week. However, sometime there were no samples in one week due to no one sunny day, then totally we took samples 11 times from 20 June to 30 September 2007 and 13 times from 25 June to 25 September 2008."

3. In P5 L10: "the transmission of photosynthetic active radiation (PAR) through the chamber was more than 95%", how does it obtain? It's measured in the chamber in 2007 and 2008 experiments or estimated by a fixed transmission rate?

Response: we got this ratio based on the difference of PAR between in chamber and out of chamber.

4. In P5 L18-19:"We found that BVOCs concentration linearly increased with time". It should be given the time period for the linear response with the time.

Response: Every time, we collected four samples at 0, 3, 6 and 9 min, or 0, 4, 8 and 12 min, BVOCs concentration linearly increased with time. We did not try a longer interval time or more than 4 samples in one chamber.

5. In P5 L25: please introduce the parameters such as precision and accuracy of GC.

Response: The precision and accuracy of GC was introduced in next paragraph in MS. The accuracy of the GC-PID for analyzing α-pinene was about 2-3% (standard deviation of the mean; n=10) and the time resolution of the analytical cycle was 10 min.

6. In P6 L30-P7 L2: "cut fresh individual green plant, put it into glass syringe with 100 ml VOCs-free air, and incubated it for 3 min under sunshine, then measured the concentration of monoterpene in it to calculate SEF" The SEF measured and calculated in this situation is not the SEF in natural condition, it should be explained clearly in the text. Then, the SEF values obtained in different situations should be given clearly in the manuscript, so as to tell the difference for different emissions.

Response: The SEF, which was obtained by incubating leaves in syringe, was not natural SEF. It was only used to compare the difference of SEF within different species in Figure 4. In the end of the third paragraph in page 6, I clearly introduced what it is SEF. SEF was normalized NER by biomass, which represents the standard emission potential. In the original MS, I used "fresh individual green plant" which might be confused. I changed it to "fresh leaves of each species" in the revised version.

7. In P9 L9-L12: "Warming did not change A. frigida biomass over the two growing seasons…… warming marginally increased the biomass of A. frigida by 56% (p=0.087) in 2007", here is a conflict for biomass change?

Response: Because the effects of warming on *A. frigid* biomass were different each year. If we analyzed all two-year's data, warming did not change it. However, we separately analyzed them in 2007 and 2008, the effects appeared.

8. In P9 L20-L24 (and other parts in this manuscript): "The mean NER was 266± 53 μg m-2 h-1 in 2008, which was significantly higher than that in 2007 (107±16 μg m-2 h-1). However, the mean SEF (0.96±0.12 μg g-1 dw h-1) in 2008 was substantially lower than that in 2007 (1.87±0.33 μg g-1 dw h-1)." What's the reason for this conflict between NER and SEF for 2007 and 2008? It may come from the SEF calculated from syringe sampling, how many syringe samplings were collected? and

how many syringe SEF values were used to get total SEF?

Response: Here all SEF were normalized from NER by biomass, not from syringe sample. NER is the natural monoterpene emission rate based on ground area, and its unit is μg m^{-2} h^{-1}. However, SEF is normalized NER based on plant biomass, its unit is μg g^{-1} dw h^{-1}(dw is dry biomass weight), which represents the emission potential. Because the emission potential was stimulated by drought in 2007, the SEF in 2007 was higher than that in 2008. But because the biomass in 2008 was much higher than that in 2007 and NER equals SEF * biomass, the effect of biomass exceeds the effects of SEF on NER. Therefore, they were different between NER and SEF for 2007 and 2008.

9. In P13 L10-L15: "In addition, the temperature in 2007 was higher than in 2008, and the SEF in 2007 was also higher, while the natural emission rate in 2007 was lower for drought-depressed A. frigida. The different emission rates between 2007 and 2008 further highlighted that the frequently used algorithms of emission response to temperature could not be simply applied in semiarid and arid area, where NPP was more sensitive to drought". The authors should consider and explain the influence of SEF from syringe measurements.

Response: We only used syringe measurement to compare the difference of SEF within species in Figure 4, all other SEF were normalized from NER by biomass.