Response to Anonymous Referee # 2

We would like to thank Referee #2 for his/her comments. We have done our best to address each of the points as detailed below. Reviewer comments are in italics and authors responses are in standard font.

#1. Repeatability of experiments:
The authors carefully addressed the determined data by calculating the uncertainties of the analysis, and I agree that the presented data was analyzed precisely. But, I'm worrying about repeatability of the experiment. Generally, this kind of experiment repeats more than three times, and the universality should be argued. Did the authors check the universality of this experiment? I recommend that the authors check the repeatability by carrying out the extra experiments or add some comments regarding the repeatability of their experiments at least.

We agree with the reviewer. Repeatability is extremely important for such types of experiments. As pointed by the reviewer we tried to be very careful when carrying out experiment/analysis. However, repeating the entire experiment is currently not feasible. As suggested by the reviewer we have included the following text in the conclusions section "The experiments should also be rerun using the same phytoplankton species and experimental conditions to check the repeatability of the results."

#2. Page 13544, Lines 19-20:
How are the concrete values of purging efficiencies for all BVOCs? While the authors carried out the experiments of temperature dependence for BVOC emission, is there no temperature dependence of purging efficiencies? As solubility varies depending on the temperature, purging efficiency may vary as well. I recommend that the authors add a table including the values purging efficiency as a supplemental information.

The text now reads: "The analysis was based on the principle of liberating BVOC from the water samples into the gas stream. To quantify the amount of BVOC recovered by the purging analysis, purging efficiencies were calculated separately for each compound and are reported in Table 1. The purging efficiencies were calculated by spiking seawater with a known concentration of the standards. Successive purging steps from the same sample vial were performed until the compound concentrations were below the detection limit. The purging efficiency was calculated by taking the ratio of the initial purge BVOC concentrations divided by the sum of the BVOC concentrations over all the purging steps. The purging procedure was optimized for >90% purging efficiency for isoprene and monoterpene species. Calculated purging efficiencies were comparable to values of >90% and >95% obtained by a similar analysis from Broadgate et al. (1997) and Shaw et al. (2003), respectively."

#3. Page 13546, Lines 5-6 and Table 1:
What is "analyte i" meaning here? In Table 1, "RSDi" is not listed. Please revise the sentence and maybe Table 1 to make it easier to understand for readers.

The text now reads: "where RSD_{Total} is the total uncertainty for each BVOC, *i* stand for different BVOCs (i.e., isoprene and monoterpene compounds), RSD_i is the uncertainty for each compound (listed in Table 1), $RSD_{Chl\text{-}a/CC}$ is the 19.3% uncertainty for Chl-*a* measurements or the 15.6% uncertainty for cell count measurements."

#4. Page 13548, Lines 13-15:
As far as I understand, α-pinene production rate markedly increased for T. pseudonana especially for higher light condition (>150 mmol m-1 s-1) from Figure 3. The pattern of increases are similar to P. carterae rather than T. weissflogii. Is it my misunderstanding?

The text now reads "Other strains (i.e., prymnesiophyte, dinoflagellate and cryptophyte) do not exhibit marked changes in α-pinene production rates with time on day 1 (see Fig. 3c-d). During a second 12-hour exposure on day 2, there was no increase in α-pinene production rates for *T. Weiss*, while the rest of the algae species either showed considerable increases (i.e., *T. pseud.* and *P. carter.*) or exhibited little discernible differences."

#5. Page 13551, Lines 6-18:
I suppose that these arguments are written in conclusion section rather than discussion section.

We made changes as suggested.

#6. Page 13553, Lines 20-23:
I agree that the mechanical stress from purging may be one of the factors of making difference in BVOC emission for the two dinoflagellates. On the other hand, how is the influence of the stress on other group such as diatom, prymnesiophyte and cryptophyte? The authors should describe the evidence which the mechanical stress was not responsible to BVOC emission for other group.

The text now reads: "Previous work has shown that mechanical stress, by sparging or shaking dinoflagellate cells, can cause an increase in BVOC emissions (Wolfe et al., 2002). However, similar problems have not been reported for cyanobacteria, diatoms, coccolithophorides, and chlorophytes (Bonsang et al., 2010). Different methods for the partitioning of BVOCs into the headspace…"

#7. Page 13554, Lines 12-15:
I agree the authors' argument that the experiment periods are short to fully evaluate the light and temperature dependences of BVOC emission from phytoplankton species. Ideally, additional experiments over the full photoacclimation period should be performed while I understand it is hard to re-setup the experimental instrumentation. Otherwise, I recommend that the authors describe their surmise or speculation on BVOC emission pattern from the phytoplankton species after day 3. I also recommend that the authors add discussion referring to Kameyama et al. (2011) in which the variation of isoprene emission from the same diatom T. pseudonana was investigated by using continuous monitoring with PTR-MS system over 2 weeks.

Although our study and the work by Kameyama and co-authors used the same diatom species, the experimental conditions were very different: high vs. low light regimes, discrete purging vs. continuous flow, and actual production vs. apparent production rate calculations. Therefore, it is hard for us to speculate on BVOC emission pattern from the phytoplankton species after the day

2. Nevertheless, the following sentence (and the reference) is added to the conclusions section: “Future studies should also assess the effect of growth phase (Kameyama et al., 2011) on BVOC production rates from various phytoplankton species.”

#8. Figure 2:
Readers probably misunderstand that the second light cycle starts after 2 h from the end of the first cycle. I recommend that the authors add a shaded area (maybe note “12-h dark period” in the area) between the cycles and renumber the incubation times of the second cycle from 0 h to 12 h.

Figure 2 is changed as suggested by the reviewer.

#9. Page 13547, Line 28:
I suppose that the authors would write it as not monoterpene but isoprene here.

Changed as suggested by the reviewer.