Ref: **Egusphere-2026-2159 (BG)**

Title: Temperate marine macrophytes are highly variable sources of Biogenic Volatile Organic Compounds - a comparative study from the Baltic Sea and NE Atlantic

Main comment: This study presents the BVOC composition and emission rates of three marine macrophytes (*Fucus vesiculosus*, *Ulva intestinalis*, and *Zostera marina*) across two regions (Ireland and Finland) and in two seasons (spring and summer). This manuscript is overall well written and the discussion is solid. I suggest providing greater methodological detail (see specific comments below). Otherwise, I believe this manuscript provides valuable information on BVOC composition and emission rates from macrophytes, whose cover has declined over the past century. There is therefore a growing need to accumulate data from similar studies for inclusion in marine BVOC budgets.

Methods:

Line 192: I disagree with the term "treatment". This is a species-region combination whereas a treatment implies active application or manipulation of the samples. Instead, I suggest using the term group/category/sample type throughout the manuscript.

Line 214: "kept submerged in mesh bags". In in situ water? At in situ seawater temperature?

Line 219: "4 m depth" for consistency with previous sentence.

I suggest changing the PTR-TOF acronym to PTR-TOF-MS throughout the manuscript. This is a time-of-flight mass spectrometer, not just a "time of flight".

What was the incubation flask made of?

Line 266: Vocus Zero-air Generator?

In Figure 1, I also suggest changing the terminology to "VOC-free air generator" or "zero-air generator" as "clean air generator" doesn't mean much.

Line 277: "the flask was filled with 4.5L of **pre-bubbled** filtered seawater**, leaving 500 mL of headspace for BVOC measurements by PTR-TOF-MS."**

Line 282: how different was the incubation temperature (20°C) from in situ temperature at collection site?

Lines 282-283: does it mean that BVOC emissions were only measured once for 30min for each macrophyte replicate? Were replicates measured sequentially? Was it in replicate incubation flasks or in the same flask? If the latter, was the water changed in between incubations? – All these details need to be provided

Lines 332-341: Since emission rates are expressed in ng BVOC/g DW/hr, it seems like a factor of 10^9 is missing in this formula to go from g to ng. This is probably implied, but it could be said explicitly.

Also, the Nppb of 2.5x10^10 molecules/cm3 assumes conditions of 25°C. This needs to be adjusted for the incubation temperature of 20°C (line 282).

Results:

See previous comment about the "treatment" terminology.

I suggest referring to the figure in order. Therefore, Fig 2c should be swapped with Fig2a (and therefore Fig2a & b become b & c).

Line 372: "Only 32 compounds were measured in every treatment…" is confusing. Do you mean "32 compounds were consistently detected across all treatments"?

Line 377: It is an assumption that these compounds were only consumed. It could also be that consumption was greater than production in this particular context (i.e. one-off 1h measurements under light exposure). Also, is it clear that the presence of macrophytes led to a loss of these BVOCs or were these particular compounds also degrading in the background? If so, their decay could be a consequence of photochemical or chemical processes more than "consumption". In the same way as you are talking about "emission" (and not "production"), I would suggest talking about "uptake" or "loss/decay/degradation" rather than "consumption".

Lines 412-413. Please refer to Fig. 3b and 3c when mentioning DMS and sesquiterpenes. I also suggest adding the molecules' names (DMS and sesquiterpenes) on the y-axes of these 2 figures.

Lines 420-424: I suggest removing parenthesis after "were" and "by".

Lines 424-426: Same here, I suggest replacing the parentheses after "compound" and "halocarbon" by a coma.

Discussion:

General comment on VOC profile (interpreting the first paragraph of the Results section): there is a small, robust common core of VOCs (32), but each species has a much wider potential emissions profile (72+) that varies with regional context, suggesting strong environmental or biogeographic modulation of VOC emissions.

Lines 463-465: It is important to precise that DMS is only one of the degradation products of DMSP as it is produced through the breakdown of DMSP by enzymatic cleavage (the breakdown of DMSP via the demethylation pathway leads to MeSH, which is in fact the dominant degradation pathway of DMSP). Isn't it possible that in this particular context (heat wave or geo-temporal context), the metabolic switch in DMSP degradation led to more DMS than MeSH (see Wang et al 2022 - *Oxidative Stress Regulates a Pivotal Metabolic Switch in Dimethylsulfoniopropionate Degradation by the Marine Bacterium Ruegeria pomeroyi*).

Lines 541-542: Do the authors mean that they should have tried to mimic in situ temperature at collection sites instead of maintaining temperature the same in all incubations? It seems like maximum temperature was 22°C on the Finnish coast and 20°C at Mace Head, which doesn't seem like a massive difference for incubations conducted at 20°C. What was the in-situ temperature during collection though? If not that different, this comment could be removed and instead discussion could focus on temperature conditions prior to collection?

Lines 546-548: the comment on whether BVOCs from macrophytes' flowers could attract pollinators sounds like a stretch when you are already trying to understand whether the

intraspecific variation of macrophyte BVOC emissions is not related to seasonality or this specific heat wave. I understand mentioning growth processes, which are linked with seasonality, but I suggest removing this comment.

4.3.1. Methodological insights and issues: I think it is important here to remind the reader that this study report one-off measurements that were only conducted over an hour period and under artificial light conditions. These measurements thus don't capture circadian variations in BVOC emissions from these organisms.

Line 585: I suggest replacing "online measurements" by "real-time continuous measurements".

Lines 594-596: here you could replace "online" and "offline" with "continuous" and "time-integrated" measurements, respectively.

Lines 601-602: The Vocus ToF MS allows for compound identification with high mass resolution. This is obviously not perfect, and for some isomeric compounds, you would benefit from coupling PTR-ToF-MS and GC measurements, but I see this argument as a positive point rather than a drawback. I suggest rewording as follows: "Although the PTR-TOF-MS allows for (list advantages)…this study could benefit from combining instrumental approaches…(develop)."

Line 606: "it **is** impossible to capture…"

This comment applies to PTR-MS measurements in general. The principle of Proton Transfer reaction remains the same for PTR-TOF-MS and PTR-quadrupole-MS.

Lines 680-682: "…showcasing a chemical diversity that has rarely been reported in the marine realm." This sentence is to be taken with a grain of salt as only few studies have looked at the full BVOC signature of marine species.

Supplementary Material

Table S1 & S2: it is essential to add the 'm/z' to these tables

Excel "supporting information": check this spreadsheet for typos (e.g. "potential", "unidentified")

Table S3: "protonated mass" is m/z. You could write it as "protonated mass (m/z)"