Response to Community Comment

Manuscript: Reviews and syntheses: Bridging Limnology and Satellite Methane Observations for Open Water Surfaces of Small Water Bodies (egusphere-2026-3707)

Page and line numbers refer to the revised manuscript, except where a deletion is described, in which case they refer to the preprint. Because a duplicated table has been removed in revision, the former Tables 7 and 8 are now Tables 6 and 7; our responses use the revised numbering.

We thank the commenter for a careful and constructive reading, and for the assessment that the manuscript is clearly written and that its statements on the validity of satellite data for small water bodies are well reasoned. We are glad, too, that the commenter sees a use for the work in the wider context of the EU Methane Regulation. The criticism concerning plant-mediated transport is well founded, and addressing it has improved the paper. We reply to each point in turn.

1. *"However, I would add, as also suggested by Reviewer #1, the following two articles: Jacob, D. J. et al., 2022 … Dubey et al. 2023 …"*

Both references have been added, and we thank the commenter for directing us to them. They support a new paragraph in Sect. 3.3.2 (p. 14, lines 319–323) establishing that the detection limit of an area flux mapper is not a fixed instrument property but scales with the product of pixel dimension and wind speed, and falls further with repeated observation of the same scene. Dubey et al. (2023) provide the empirical quantification for TROPOMI: 500–8800 kg CH_4 h^{-1} per pixel for a single overpass, and 50–1200 kg CH_4 h^{-1} over a yearlong campaign. This also allowed us to replace an unsupported per-pixel threshold in Table 6 (p. 15) with these published values.

2. *"In their research, the authors completely exclude methane emissions via the aerenchyma of emergent macrophytes (plant-mediated transport). This emission pathway is neither mentioned nor evaluated or analysed in the manuscript. The focus is exclusively on diffusion, ebullition and oxidation; as stated in the discussion: 'All proxy approaches reviewed map variables linked to diffusive methane flux' (line 350)."*

We accept this criticism. The pathway was absent from the manuscript, and we have addressed it in three places rather than in one.

Sect. 1.2 now distinguishes three pathways-diffusive exchange, ebullition, and transport through the aerenchyma of emergent macrophytes, and states which of them the review addresses (p. 2, lines 56–58). Reed (*Phragmites* spp.) and cattail (*Typha* spp.) stands are identified as common in shallow systems, with areal emissions substantially exceeding those from adjacent open water, citing Juutinen et al. (2003) and Bodmer et al. (2024) (p. 2, lines 60–62).

Sect. 3.2.1 now carries a paragraph describing the mechanism (p. 8, lines 218–221): methane conducted through the aerenchyma bypasses both the sediment–water oxidation barrier and the air–water interface, and so avoids the depth-dependent oxidation that consumes 97.9–99.9 %

of sediment-produced methane under stratified conditions. The paragraph states that this pathway was not captured by the search strategy.

The sentence the commenter quotes has been revised. "All proxy approaches reviewed map variables linked to diffusive methane flux" now reads "The proxy approaches synthesised here map variables linked to diffusive methane flux" (p. 18, lines 415–416), since the revision identifies macrophyte extent as a proxy that does not map diffusive flux.

3. *"As the search terms (lines 116–119) do not include any plant-related terms and studies focusing solely on wetlands were excluded (Table 1), the relevant literature was presumably not identified."*

The commenter has identified the cause correctly. The search concepts contained no plant-related terms, and wetland studies without a pond component were excluded by the criteria in Table 1, so the relevant literature was not retrieved. Sect. 2.1 now states this limitation directly rather than leaving it to be inferred (p. 6, lines 146–148). We have not extended the systematic search retrospectively, since doing so after the fact would compromise the reproducibility of the PRISMA process. Instead we have bounded the scope explicitly and treated the pathway where it bears on the review's conclusions.

4. *"Consequently, RQ1 ('What biogeochemical processes drive methane emissions from small water bodies …', line 98) also remains incompletely answered."*

We agree, and RQ1 has been revised to ask about the processes driving methane emissions from the open water surfaces of small water bodies (p. 5, lines 123–124). With the scope now stated and the third pathway described in Sect. 3.2.1, we consider the question answered within its stated bounds.

5. *"Furthermore, in satellite-based methane measurements, emissions from the open water surface, the reed belt and adjacent marshes and wetlands cannot be distinguished from one another, whilst the optical proxies discussed (chlorophyll-a, water temperature) relate exclusively to the open water surface. … I think that this aspect and the specific focus on the open water surface, need to be highlighted clearly by the authors in the title, abstract and introduction."*

We are grateful for this, and have followed it in full. The title now reads "… for Open Water Surfaces of Small Water Bodies". The abstract states that the review addresses the open water surfaces of these systems and that transport through the aerenchyma of emergent macrophytes lies outside its scope (p. 1, lines 6–8), and Sect. 1.2 establishes the same in the introduction (p. 2, lines 56–58). The indistinguishability the commenter describes is now stated in Sect. 3.4.2 (p.16, lines 368-370) as a consequence of pixel size (p. 16, lines 368–370), and taken up again in the Discussion, where we note that a pixel-integrated signal over a vegetated small water body combines pathways with different controls (p. 18, lines 425–427).

6. *"Furthermore, it should be specified whether the area from the GSLPI include reed beds."*

They do not, and we thank the commenter for raising a point that needed stating explicitly. Wachholz et al. (2026) derive the inventory from ATKIS Basis-DLM standing water body polygons. The GSLPI paper does not itself address vegetation, but the ATKIS object catalogue captures reed stands as a vegetation feature (AX_Vegetationsmerkmal, attribute value

Röhricht/Schilf) rather than as part of the standing water body (AX_StehendesGewaesser), so the GSLPI areas delineate open water. This is now stated in Sect. 1.2 together with its consequence: the total methane-emitting area of these systems exceeds the inventoried area, so the size-class analysis in Sect. 3.4.2 describes open water alone.

7. *"In particular, I would also expect a discussion of this point in the discussion section, analogous to the discussion of ebullition (lines 350–355)."*

We thank the commenter for specifying where this discussion belongs. The paragraph now sits in Sect. 4, immediately after the paragraph on ebullition (p. 18, lines 421–429), so that it occupies the position indicated. It states the mechanism, that methane conducted through the aerenchyma escapes the depth-dependent attenuation governing diffusive flux; the magnitude, that vegetated littoral zones emit at substantially higher areal rates than adjacent open water (Juutinen et al., 2003; Bodmer et al., 2024); and the consequence for observation, that emissions from open water, reed belt and adjacent marsh cannot be separated within a single satellite pixel. The paragraph then closes on the observational opportunity this presents, which we address under point 8.

8. *"In this context, it should also be also discussed whether emergent macrophytes, unlike methane itself, can be mapped using optical sensors such as Sentinel-2 or Landsat-8 and used as a proxy."*

They can, and we are grateful for the suggestion. Emergent stands are more tractable to map than the water-column properties this review examined: they produce a strong red-edge and near-infrared signature, and stand extent changes over seasons rather than days, whereas chlorophyll-a is dynamic and requires frequent cloud-free acquisition. Mappability is not the same as predictive value, however. Stand cover indicates conditions favouring methane production rather than flux itself, so its most plausible use is as an explanatory variable alongside the water-column proxies in a model of emission, which remains to be tested.