Response to Referee 2

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.

1. *The scope is very narrow to justify a dedicated review. Restricting the search to the intersection of "small water body CH_4 emissions" and "satellite remote sensing" inevitably yields a small corpus (30 papers). I question whether a topic this narrowly defined warrants a standalone systematic review.*

We thank the referee for raising this, as it obliged us to state the review's rationale more clearly than we had. The search spans two large and separately developed literatures, freshwater methane biogeochemistry and satellite remote sensing, rather than one narrow topic, and the sparseness of their intersection is an empirical result rather than a limitation of the search. Identifying that fewer than 30 papers exist at this intersection after 32 years of parallel development in two mature fields is itself a finding. This review follows the Reviews and syntheses scope as defined by Biogeosciences, which explicitly accommodates papers characterising emerging research frontiers. We have made this positioning explicit in Sect. 2.2 (p. 6, lines 153–157), which now attributes the small corpus to the technological infeasibility of direct satellite detection rather than to limited research activity (p. 6, line 154). We accept, however, that this argument alone does not make a synthesis, and have addressed that under point 2.

2. *The central finding is close to a foregone conclusion. The bulk of the manuscript is devoted to establishing that satellites cannot directly detect CH_4 enhancements from small water bodies. Given that typical emission rates (0.01–1 kg CH_4 h^{-1}) are 3–4 orders of magnitude below point-source detection thresholds (50–300 kg CH_4 h^{-1}), this outcome is obvious from the sensor specifications alone and does not require an extensive literature synthesis to demonstrate.*

We thank the referee for this assessment and accept its premise: the detection gap follows from sensor specifications and does not require a literature synthesis to establish. The referee has identified a real problem with how the manuscript was weighted. Three contributions now carry the analytical weight. First, Sect. 3.4.2 and Table 7 report what fraction of the GSLPI inventory each sensor class can resolve (p. 15–16, lines 359–366, and Table 7, p. 16), which cannot be obtained from specifications alone. Second, Sect. 4.1 states for each method class what funding would buy and what the binding constraint is (p. 18-19, lines 431–465). Third, Sect. 3.3.2 derives the area flux mapper detection limit from pixel dimension, wind speed and revisit frequency following Jacob et al. (2022) and Dubey et al. (2023) (p. 14, lines 319–323), and identifies a consequence available only by reading those studies against the limnological literature: the temporal averaging that lowers the detection limit removes the resolution needed for episodic ebullition.

The direct-detection material is condensed accordingly, and now serves as framing rather than as the primary analytical contribution. Three passages of restatement have been removed: the emission-value list at preprint p. 14, lines 286–289; the paragraph restating pixel dimensions, thresholds, emission rates and the blind-test result at preprint p. 14, lines 289–298, together

with a duplicated statement of the Sherwin et al. (2024) result in the same section; and a redundant passage at preprint line 272.

3. *The proxy-based approach of inferring emissions from optically retrievable variables such as chlorophyll-a, turbidity, and surface temperature is, by the authors' own admission, the more realistic pathway. Yet this is treated comparatively briefly (Sect. 3.5), especially relative to the extensive space given to direct-detection infeasibility. If going for a revision, I would encourage the authors to invert the emphasis of the review.*

We thank the referee for this recommendation, which both referees make and which we have followed. We would add that the referee has identified a misalignment between the manuscript's emphasis and its own research questions: RQ3 asks how satellite-derived proxies can be combined with in-situ flux measurements and modelling, and in the submitted version that question received considerably less space than RQ2, which established what satellites cannot do. Inverting the emphasis brings the paper into line with the questions it set itself.

The proxy pathway is not an alternative introduced for want of a better option; it follows directly from the biogeochemistry synthesised under RQ1. West et al. (2016) established an empirical link between epilimnetic chlorophyll-a and sediment methanogenesis potential ($R^2 = 0.32$, $p = 0.02$), and showed that whether the methane produced reaches the atmosphere depends on water depth, with a threshold near 6 m below which ebullition dominates and above which most methane is oxidised (p. 2, lines 49–55). Temperature governs production rates. The three primary controls identified in Table 2 (p. 9) are therefore depth, productivity and temperature, and these are precisely the variables accessible to remote sensing through bathymetric mapping, chlorophyll-a retrieval and land surface temperature products. The proxy approach works by mapping the drivers rather than the gas, which is why it is not bound by the sensitivity barrier established under RQ2.

Two quantitative findings support this in the revision. Sect. 3.4.2 shows that Sentinel-2's visible and near-infrared bands, from which chlorophyll-a and turbidity are retrieved, resolve 70.4 % of the GSLPI inventory, against 0 % for the methane-band satellites and 24.1 % for Sentinel-2's own shortwave-infrared bands (Table 7, p. 16). On the same platform, the proxy bands resolve almost three times as many water bodies as the methane bands (p. 15, lines 363–364).

What limits the pathway is calibration rather than sensor capability. The chlorophyll-a–methane relationship shifts with trophic state, season and catchment land use, so a relationship fitted at one site does not transfer to another (p. 19, lines 445–447). Ebullition compounds this, since it is episodic and spatially heterogeneous and largely decoupled from surface optical properties (p. 18, lines 415–419). Sect. 4.1 therefore identifies the required work concretely: a multi-site campaign pairing water-column CH_4 measurements with concurrent Sentinel-2 acquisitions across the trophic gradient, using the GSLPI as the sampling frame, with eddy covariance validation at representative sites and pathway-specific rather than total-flux proxy relationships (p. 19, lines 445–449).

The proxy material is expanded accordingly in three places: the quantitative coverage analysis in Sect. 3.4.2 (p. 15–16), the research priorities in Sect. 4.1 (p. 18), and the macrophyte pathway raised by the community commenter, which we identify as a second and possibly more tractable proxy because emergent stand extent has a stable spectral signature and changes over seasons rather than days (p. 18, lines 426–429).

4. *The manuscript is imprecise in places, which undermines confidence in its quantitative rigor. As an example, the "10^3–10^4" mismatch between detection limits and actual emission rates is stated correctly in several places (Sect. 3.4, Sect. 3.4.1, Table 7) but appears truncated to "10^3–10" in Sect. 3.2.2 (line 215) and again in the conclusion (line 359). Given that this number is the paper's central quantitative result, such an inconsistency is not a trivial typo and raises*

questions about how carefully the quantitative synthesis was checked. Similarly, "CO" appears in place of "CO_2" twice in the introduction. These are simple fixes individually, but their presence in key statements suggests the manuscript needs a more careful revision pass. On a similar note, I found that statements were repetitive (e.g. line 262 and line 272).

We thank the referee for identifying these errors. Both were rendering faults in which a superscript was lost, and both have been corrected. In checking them we found an arithmetic error the referee had not raised: the range stated in Sect. 3.2.2 did not span the values in Table 3, where Crampton Lake at 8.67×10^{-5} kg CH_4 h^{-1} falls 5.8×10^5 to 3.5×10^6 below the threshold band. That figure now reads 10^3–10^6. The conclusion retains 10^3–10^4, which is correct for the 0.01–0.016 kg CH_4 h^{-1} range it refers to. (p. 19, line 468).

The "CO" for "CO_2" occurrences were a subscript split across text runs in the PDF rendering and have been corrected at both (p. 2, lines 29–31). The repetition between preprint lines 262 and 272 has been resolved by removing the redundant passage at preprint line 272. We have conducted a full consistency check of all quantitative values in the manuscript, which also identified and corrected the TROPOMI pixel dimension, addressed in our response to Referee 1.

5. *The review of satellite technology is scattered, and the coverage is not current. Section 1.3 introduces the satellite remote sensing landscape but omits PRISMA, EnMAP, EMIT, and GaoFen-5 entirely; these instruments are only introduced later (Sect. 3.3.1). Plus, one would expect a review of detection limits here. Table 5 is internally inconsistent with the text: it lists GaoFen-5 (not previously introduced at that point) but omits EnMAP, which is discussed in the surrounding text. More importantly, several of the most relevant current missions for point-source methane detection such as MethaneSAT, GHGSat, and Carbon Mapper are not mentioned anywhere in the manuscript. For a review whose central question is precisely whether current satellite capabilities can close the detection gap, omitting these platforms is a significant gap. Restricting the technology discussion to pre-2025 literature is not adequate for a review intended to be topical as of 2026.*

We thank the referee for this detailed critique and accept all three sub-points. Sect. 1.3 now introduces every instrument discussed later in the manuscript, including PRISMA and EnMAP at 30 m, EMIT at 60 m and GaoFen-5 at 30 m, with an overview of detection thresholds by mission class, so that no instrument first appears in Sect. 3.3 (p. 4, lines 97–106). The closing paragraph of the section now lists both sensor categories with all member instruments (p. 4, lines 107–111). Table 5 now lists EnMAP, which was discussed in the surrounding text but absent from the table, and GHGSat, Carbon Mapper and MethaneSAT have been added to both Sect. 1.3 and Table 5 (p. 13).

MethaneSAT is included as launched in March 2024 with 130 × 400 m pixel resolution targeting 200 × 200 km² regional domains, citing Jacob et al. (2022) (p. 4, lines 102–104, and Table 5 footnote, p. 13). We note explicitly that contact with MethaneSAT was lost later in 2025 and the mission is no longer returning data, so it is presented as a design case rather than as a currently available capability. Carbon Mapper, also launched in 2024, operates at 30 m nadir resolution with predicted minimum detection limits of 66–144 kg CH_4 h^{-1} (Duren et al., 2025), and this is now the figure against which we assess what instrumental improvement could realistically achieve.

References added in response to this review

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