Response to Referee 2

Overall, this manuscript addresses an important and timely topic. Methane cycling beneath ice sheets remains poorly constrained because the subglacial environment is extremely difficult to access and observe directly. In this context, the use of a reaction–transport model to explore methane production, oxidation, and export beneath the Greenland Ice Sheet is meaningful and potentially valuable. I therefore think the study is relevant and could make a useful contribution to the field. At the same time, the manuscript in its current form still has several important issues that need to be addressed before it can be considered further.

1. abstract: The authors should review how papers are written in The Cryosphere, as the current abstract is divided into three paragraphs, which I do not consider appropriate. In particular, the sentence in lines 28–30 is too long and difficult to read.

Revised as suggested

2. Regarding Figure 1, I do not think the current layout is clear. Either it should be integrated into a single figure, or the two panels should be presented at the same size. As it stands, the right panel appears to be the overview figure and the left panel a zoomed-in detail, but the current arrangement does not clearly convey this information. In addition, several symbols appear in lines 71–72 of the caption that have not been defined earlier in the text, which makes it difficult to understand what is being expressed. Also, the authors do not cite Figure 1 in the main text.

Revised as suggested

3. Line 74 What does "BRNS" mean? What is its full name? The full term should be given when it first appears.

Revised as suggested

4. Line 87, what is ϕ?

Definition added and revised as suggested

5. The authors devote 11 pages to a detailed description of the methods they used. Although this information is important, I do not think this is an appropriate way to present a scientific paper. A broader issue is that the Methods section does not clearly separate model formulation from background justification. In particular, large parts of Section 3.3 read more like an extended discussion of why certain pathways were excluded (e.g. nitrate, iron, sulfate-related processes) rather than a concise description of what was actually implemented. I suggest moving much of this contextual material to the Introduction or to a short model assumptions and scope subsection, so that the Methods focus more directly on the equations, boundary conditions, parameterization, and workflow used in this study. At present, this very long Methods section mixes parameter sources and applications with figure and table explanations, which, in my view, does not make for a logically clear or well-structured Methods section, and it is quite tiring to read. And Line 168, there are two verbs in this sentence. Line 319 there are two range of.

Here is our response to Reviewer 1, which addresses the reviewer 2 correct concerns:

We agree that Section 3 previously mixed methodological description with elements of model justification, which made it harder to read as a clear methods section. We have therefore extensively streamlined and restructured the section to improve the separation between the conceptual basis for the reaction network and its mathematical implementation. We also have emphasized the use of the model results as a diagnostic tool to derive inverse constrains on inaccessible environmental information based on meltwater outflow concentrations.

Importantly, however, we retain the conceptual justification as a separate sub-section within the method section. It is not background or introductory material in the conventional sense, but an integral part of the model design. In subglacial environments there is no universally agreed set of dominant biogeochemical pathways, and the relative importance of intermediate terminal electron acceptor processes, methane oxidation pathways, and external methane sources remains actively debated and strongly system dependent. For this reason, the selection and exclusion of specific reactions is itself a key modelling decision guided by our current understanding, but aspects of identifiability and predictive skill.

We therefore consider it necessary to explicitly document and justify these choices within the modelling framework. To address this while improving readability, we have consolidated all process justification into a dedicated sub-section (3.3.1 Selection of biogeochemical processes) placed prior to the mathematical formulation. This section clearly outlines the evidence base and modelling rationale for each included and excluded pathway, using a consistent structure, while the remainder of Section 3 now focuses on the reaction network formulation and implementation.

Additionally, we addressed the comments on lines 169 and 319.

6. Please check the caption of Figure 5 carefully. The figure does not contain the left and right panels mentioned in the caption. If the authors provided the comparison details in lines 391-395, why they did not show the exact observed data in Fig 5.

The caption has now been revised. Adding the exact observed data is a valid suggestion; however, it would lead to more data points on an already overcrowded panel, adding only limited information, as the focus is the comparison to the observed range. Furthermore, comparing to the exact observed data would give the wrong impression that the model is trying to replicate these observations, which is not the aim.

7. The manuscript relies on a very strong structural assumption that O2 is effectively the only terminal electron acceptor relevant for constraining methane consumption in the model domain, while other redox pathways are excluded. I think this simplification

may be acceptable, but it needs to be framed much more explicitly as a limitation on model scope rather than as a sufficiently justified representation of the full subglacial system. At present, the manuscript sometimes reads as if this simplified network is broadly representative, whereas the authors themselves later acknowledge that iron, nitrate, and sulfate related processes may be important.

We agree with this point and have revised the relevant section to make this distinction more explicit. Specifically, we now state directly that the exclusion of intermediate TEA pathways (denitrification, iron reduction, sulfate reduction) reflects a deliberate simplification of model scope, adopted because these pathways cannot currently be parameterised as spatially explicit reaction terms at the catchment scale given existing constraints on oxidant availability, reactivity, and transport and not because we consider them absent or negligible in the subglacial system in general.

In addition, we have also revised this section to justify the assumed dominance of AeOM over AOM in the fundamentally oxidative character of the subglacial environment beneath the GrIS.
That said, we would like to highlight that field and experimental evidence consistently shows that aerobic methanotrophy is the primary biological CH_4 sink at glacier margins (e.g. Dieser et al., 2014; Adnew et al., 2025, Lamarche-Gagnon et al., 2019), and that where O_2 is depleted, CH_4 tends to accumulate rather than being consumed by alternative pathways.

While molecular evidence from GrIS-adjacent sediments confirms the presence of ANME-affiliated organisms capable of anaerobic CH_4 oxidation, aerobic methanotrophs emerges as the numerically and functionally dominant CH_4-oxidising community (Ardoin et al. 2026).

8. I am also concerned that several key arguments rely heavily on unpublished, in-preparation sources. This is especially important where these sources are used to support major modelling assumptions (e.g. the negligible role of AOM) or to constrain/compare the case-study calculations. The authors should either replace these with published evidence where possible, or more clearly state that these assumptions and comparisons remain provisional.

Two of the cited studies have been published in the meantime: Arndt, in review, 2025 is now Arndt et al. 2026 Global Biogeochemical Cycles, while Hatton et al. in review 2025 is now Hatton et al. 2026 Nature Geoscience. The negligible role of AOM, at least at our current understanding, is based on Dieser et al., 2014 and Adnew et al., 2025. Both studies highlight the dominance of aerobic oxidation of methane.

9. The sensitivity analysis is useful, but it mainly explores parametric uncertainty within a fixed model structure. It did not address several structural assumptions that may be equally important, including steady-state behaviour, diffusion-dominated transport, negligible advection across boundaries, and the exclusion of deeper geological CH4 inputs. I think the manuscript should make this distinction much clearer, because at

present the global sensitivity analysis may give the impression that the main uncertainty has been comprehensively explored, whereas several conceptual uncertainties remain outside the tested ensemble.

We have clarified these points in the revised manuscript:

Diffusive vs advective transport: *We note that methane production itself, as quantified by the model, is not mechanistically dependent on the mode of porewater transport (diffusive vs. advective): production is controlled by organic matter availability and degradation kinetics within the anoxic zone, independent of how the resulting methane is subsequently transported. The choice of transport regime instead affects the fate of that methane after production, specifically, the extent to which it is oxidized in the shallow oxic surface layer. We clarified this point in section 3.2 Transport Model*

The primary effect of advection would be to allow a fraction of produced methane to bypass the oxic surface layer. Under this scenario, our diffusion-only representation would tend to overestimate the degree of methane oxidation and correspondingly underestimate net methane export. We consider this a conservative bias with respect to our reported export estimates.

Steady State: *We acknowledge that subglacial water O_2 concentration may vary across melt-season and seasonal timescales. However, resolving this transient forcing explicitly would require time-resolved boundary condition data (e.g., continuous subglacial O_2 or meltwater flux records) that are not currently available or easily deducible at both local and catchment scale. A steady-state treatment is the standard approach adopted in comparable modelling studies under this same data limitation. This does not mean that our study assumes that subglacial O_2 concentrations are constant. It explores how subglacial sediment CH_4 cycling responds across the plausible envelope of oxygen concentrations (i.e. from anoxic to fully oxygenated subglacial waters). The applied ensemble approach is useful for bounding the plausible range of subglacial sediment methane fluxes, in the hope that future field work studies will shed light on subglacial oxygen concentrations.*

In addition, our sensitivity analysis shows that CH_4 production is only weakly sensitive to O_2 concentration itself, meaning that uncertainty introduced by unresolved temporal variability in O_2 supply is unlikely to propagate strongly into the model's primary output. We therefore consider the steady-state treatment a pragmatic simplification necessitated by current data constraints, rather than a claim that O_2 dynamics are genuinely static in the subglacial system, and we have revised the manuscript to state this explicitly. We clarified this point in section ***3.6 Environmental Ensemble for global sensitivity and diagnostic framework.***

Exclusion of deeper geological CH_4 inputs: *As discussed in the manuscript, stable isotope signatures and microbial indicators consistently indicate that methane exported from southwest Greenland catchments is predominantly biogenic, and methane hydrates are unlikely to constitute a significant or sustained source beneath ice-sheet margins (Wadham et al., 2012; Lamarche-Gagnon et al., 2019; Adnew et al., 2025). We have further highlighted throughout the manuscript that this study focuses on biogenic methane production and export.*

10. Section 4.5 currently feels under constrained. The case study combined modelled sediment fluxes with assumptions about hydrologically active area, soft-sediment cover, residence time, and in-transit oxidation/off-gassing, many of which are

themselves poorly known. As a result, the inferred concentration and flux ranges become extremely broad, and the analysis reads more like scenario matching than a strongly constrained inference of subglacial conditions. I think the authors should either tighten this section substantially or present it more explicitly as an exploratory exercise rather than an inference of likely catchment properties. I also find the interpretation too strong where the authors state that cases in which simulated CH4 falls within the observed range imply little or no oxidation/off-gassing during transport. Matching the observed outlet concentration does not uniquely support that conclusion; it could equally arise from larger subglacial production combined with stronger in-transit removal. This part should be rephrased more cautiously.

This is indeed one of the main intentions of this section, and it does work backwards from observations, identifying which combinations of OM content, reactivity, sediment cover, and oxidation reproduce measured CH_4 concentrations at both glaciers. But we agree that the framing is a bit obscured from how the section is currently structured.

To clarify our intent, we use the model to simulate a large environmental ensemble with two aims: 1) to quantify potential CH_4 production rates and fluxes in subglacial sediments and identify their main environmental controls, and 2) to use the ensemble as a diagnostic tool, combined with observed meltwater CH_4 concentrations, to infer plausible ranges of environmental conditions and assess the possible extent of in-transit CH_4 loss (i.e. in an exploratory way).
Section 4.1-4.4 address the first aim and results are discussed in the context of available observational data. Section 4.5 addresses aim 2), and we agree this was not made explicit enough, contributing to the section reading as scenario matching. As detailed in our response to Reviewer 1, we have restructured this section, as well as the abstract and methods section to state this more clearly upfront and present the analysis explicitly as an inversion that helps identify which environmental conditions are consistent with the available observations at Isunnguata Sermia and Leverett Glacier rather than a uniquely constrained inference. Broad flux range are retained as motivation and seasonal variability is discussed explicitly.

The two main conclusions of this diagnostic framework are that 1) we do not need to infer exceptional sediment cover or OM content to explain observed methane fluxes and that 2) across a wide range of plausible subglacial conditions, predicted CH_4 concentrations are consistently higher than those measured at the glacier outlet, pointing to oxidation, off-gassing, or both as near-ubiquitous features of lateral CH_4 transport. Matching the observed outlet concentration does not necessarily mean that there is no removal, it could indeed equally arise from larger subglacial production combined with stronger in-transit removal. We clarified this in the revised version.

We also explain this in the response to Reviewer 1, who raised a closely related concern about this section.

11. The manuscript does not clearly distinguish between generic model behaviour and site-constrained interpretation. Before Section 4.5, the Results mainly describe model outcomes across a broad parameter space rather than a case-specific application. However, these generic simulations are already discussed in close connection with field observations, which blurs the distinction between exploratory model behavior and site-based inference. I suggest that the authors make this transition much clearer, and reserve the interpretation against real catchment observations for the dedicated case-study section.

We agree that the distinction between exploratory ensemble behaviour and site-constrained interpretation could be made clearer, and that Sections 4.1–4.4 currently blur this distinction by discussing generic simulation results in close connection with field observations. As detailed in our response above (and to Reviewer 1), the ensemble serves two distinct purposes and we have now revised the abstract, methods and result sections to make this distinction clearer.

We do, however, consider it important to discuss simulated rates and fluxes in the context of the available literature within Sections 4.1–4.4, even though this is not a site-constrained inference in the sense of Section 4.5. Comparing simulated values against the broader range of published subglacial and proglacial CH_4 observations provides a plausibility check on whether the ensemble produces geochemically realistic rates and fluxes and helps identify which parts of the explored parameter space correspond to conditions already known to occur in nature, versus regions of parameter space that, while mathematically permitted by the model, may be less environmentally realistic. We see this as distinct from the site-specific inversion performed in Section 4.5, which uses two individual, well-characterised observational records to constrain plausible local conditions. We have carefully revised sections 4.1-4.4 in this spirit.

12. Another general issue is the very heavy use of abbreviations throughout the manuscript. While I understand that some abbreviations are unavoidable in a modelling paper, in the current version there are so many that they interrupt the reading flow and make the manuscript unnecessarily difficult to follow. I frequently had to go back and check what a term meant

Some less common abbreviations (RTM, LG, IS, RU, TEA, GSA, kAeOM, OM,) are now spelled out to improve the reading experience. While others are kept as they are common in biogeochemical modelling studies (O2, CH4, GrIS, SW). AeOM is kept abbreviated, but its meaning is now often spelled out to improve reading flow.

13. Some statements in the Conclusion appear to go beyond what the current model can firmly support. In particular, the recommendation that drilling strategies should prioritize areas with deep sediment seems somewhat premature, given that the model

does not include several potentially important processes, such as additional redox pathways, transient hydrology, erosion, and seasonal changes in O2 supply. I suggest either softening these applied implications or linking them more explicitly to the stated limitations of the model.

We have toned down our implications and highlighted the model's limitations to a certain extent but would like to point out that despite the simplicity of the used model the reviewer's suggested potentially important processes (additional redox pathways, transient hydrology, erosion, and seasonal changes in O2 supply) would not affect the stated recommendation for deep drilling sites. Biogenic methane production requires stable anoxic conditions, which are found in deeper sediments, and even deeper when additional redox pathways and varying O2 supply are considered.

13. The Conclusion section would also benefit from substantial tightening. At present, it reads partly as an extension of the Discussion rather than as a concise synthesis of the main findings. Several points appear repetitive, and the section would be stronger if it focused more clearly on the key model insights, what can and cannot be inferred from the current framework, and the main priorities for future work.

We agree on the benefit of substantial tightening the conclusion and revised as suggested.

References

Ardoin, L., Larose, C., Tison, J.-L., Keuschnig, C., Gkinis, V., El Amri, S., Blard, P.-H., Bierman, P., Blunier, T., Dahl-Jensen, D., Maistriau, C., Steffensen, J.-P., Röckmann, T., and Fripiat, F.: Methane and carbon dioxide dynamics beneath the Greenland Ice Sheet: Insights from ice core basal materials, https://doi.org/10.5194/egusphere-2025-6204, 13 January 2026.