Response to Reviewer 3

The authors thank the reviewer for a thorough evaluation of our manuscript. Below we provide a point-by-point response.

Reviewer comment (Line 128):
"The redistribution is weighted according to the inverse distance from the dying particle within a user defined distance limit dmax,: How does the diffusive transport of particles that is adjusted by this re-distribution of mass and affect the model results?

Response:
We agree that this should be discussed, and the text has been revised and now reads: *"This solution changes the problem of non-physical loss of dissolved gas to one of non-physical redistribution. This can affect model results by shifting particle mass toward the particle origin(s), since the density of particles is generally higher closer to the release point. However, we consider this artifact less problematic than mass simply disappearing."*

Reviewer comment (Page 5, last line):
"This solution changes the problem of non-physical loss of dissolved gas to one of non-physical redistribution, which is generally considerably less problematic."

How do you say this is less problematic? Based on any evidence or proof?

Response:
While it is difficult to "prove" this formally in general, our reasoning is as follows: if a particle with mass *M* is completely removed, this results in a direct error of *M* in the concentration field. By contrast, redistributing the mass preserves the total amount, though at the expense of potentially shifting its spatial distribution. We therefore expect the numerical error in the field to be smaller.

We acknowledge that in very specific, artificial cases - such as a constant, noise-free current where particles are repeatedly removed at the same point - redistribution could theoretically cause severe local artifacts. However, we consider this scenario very unlikely in the applications discussed in our paper.

Reviewer comment (Lines 35–40):
"Our aim is to provide a framework which can integrate all key processes governing free and dissolved transport and transformation of seeped gas to provide a full 3-D concentration field in the water column and total atmospheric release estimates." There have been previous modeling studies in literature focusing on these aspects. See the

studies: https://pubs.acs.org/doi/full/10.1021/acs.estlett.3c00493, https://www.nature.com/articles/s41467-024-53780-7, https://sintef.brage.unit.no/sintef-xmlui/handle/11250/2730544 , https://pubs.acs.org/doi/full/10.1021/acs.est.5c03297

Response:
We thank the reviewer for these valuable references. The sentence has been revised to: *"Even though previous modelling efforts have mainly focused on modelling individual pathways \citep[e.g.][]{McGinnis2006, Graves2015, Silyakova2020}, key steps toward modelling the complete system have been made in \cite{Dissanyake2023} and \cite{Nordam2025}. We aim to expand on these studies from a methodological perspective, to provide a pilot framework that integrates all key processes governing free and dissolved transport and transformation of seeped gas, yielding a full 3-D concentration field in the water column and estimates of total atmospheric release."*

To further emphasize the contribution of Dissanyake et al. (2023), we added a reference to their study in the following paragraph.

Regarding the SINTEF report, we note that Nordam et al. (2025) builds extensively on this work and has undergone peer review. For this reason, we have chosen to cite the readily available paper by Nordam et al. (2025) rather than the SINTEF report.

Reviewer comment (Line 345):
"Modeling step, were calculated using the dissolved gas profiles" Are these modeled or measured?

Response:
They are modelled. The sentence has been revised for clarity:
*"Dissolved gas injection rates, which ar*e needed as input in the particle dispersion modelling step, were calculated using modelled (by M2PG1) dissolved gas profiles (not shown) and Eq. (...)."

Reviewer comment (Line 505):
In the model presented, the vertical binning size is dependent on the bubble rising speeds wo. The bubble size which controls the rising speed changes with pressure changes and the mass transfer at different depths. Hence the bubble size distribution (BSD) changes at different depths. BSD at what depth level was used when you decided the vertical bin size and why?

Response:
This is correct. This is true and the note about the assumed unchanged BSD should have been included. We included the following in the revised manuscript:

"Note that the BSD is expected to change with height above the seafloor (which also changes $P[w_o]$). For the purpose of this calculation, however, we assume the BSD remains constant"

Reviewer comment (Line 550):
"OpenDrift by seeding N = 1900000 particles". This is a very large number of particles that were used in the simulations. How did you decide (what is the basis) the number of particles to be used? and how much computer resources needed for these simulations and time taken. It would be good to give an idea of this to the readers.

Response:
The number just has to be sufficiently large to ensure that the "ground truth" histogram estimator manages to obtain a reasonable density estimate over the chosen grid (the non-normalized error is proportional to 1/N_{a,b}^{0.5} where N_{a,b} is the number of particles in the cell). To make it sound less arbitrary we changed the particle number to 2000000 (400 timesteps and 5000 at each step) in the revised manuscript. However, it has only negligible effects on the end result.

We also added some performance metrics to the manuscript and referred to the public GitHub repository, where simple performance tests are available for both the synthetic PDM toy model and different KDEs.

Reviewer comment (Line 620):
In this section where you describe the oxidation rates you are presenting several ranges. For example rates of CH4 oxidation

Varying from 10−8 to 10−2 nM s−1,) and ((kox) range from 0.02·10−6 to 1.74·10−6 s−1,. You should state which numbers were used in your study in the Norwegian waters and justify the reasons for choosing these numbers as there is a large variation.

Response:
The values applied in our case study are stated in Section 3.3 and also later in Section 3. To improve clarity, we added the sentence:
"For the application offshore northwestern Norway, we used the average of all compiled values."

We acknowledge the lack of local measurements and therefore have no stronger justification beyond using an average. This is also discussed in our reply to General Comment 1 from Reviewer 2, which partly addresses this limitation.

Reviewer comment (Section 4, Conclusions):
I expect large variability in the biodegradation rates will affect the results presented to a large extent. I believe should be discussed in the manuscript and included in the conclusion. Please see the recent studies presented in https://sintef.brage.unit.no/sintef-xmlui/handle/11250/2730544 and https://pubs.acs.org/doi/full/10.1021/acs.est.5c03297

Response:
We agree. The "Interpretation of the results" section now includes an expanded discussion on the importance and challenges of incorporating microbial oxidation into the modelling framework.

We also performed a sweep of rate coefficients and examined the resulting sensitivity of atmospheric fluxes and concentrations. This analysis is now included in the revised manuscript, and in the conclusions we added the following:

"*In particular, mass loss due to microbial oxidation poses a significant challenge since rate coefficients are shown to exhibit large variability which cause considerable differences in the modelled atmospheric fluxes and concentrations. Current parameterizations of mass loss due to atmospheric ventilation are also simplistic, and they were originally developed for large scale ocean regions, not coastal areas.*"