We thank Reviewer 1 for the careful and constructive evaluation of the manuscript. Several of the comments concern places where the original text claimed more than the analysis established, and we have revised the manuscript accordingly. Below, each comment is reproduced in italics, followed by our response and the corresponding revised text.

Beyond the point-by-point edits, the revised manuscript incorporates a document-wide editorial pass. We also changed the title to a high-dimensional framing; it now reads “High-dimensional parameter optimization of a biogeochemical model: a multi-variable BGC-Argo data assimilation approach.” Together with the specific changes below, this pass aligns the manuscript’s tone with the reviewer’s concern that the results should not be overstated.

Minor comments

> ***Ln 1060.*** *I did not find a test that the estimate is ‘robust’ in a statistical sense – on the contrary, several parameter settings lead to a similarly good fit (e.g. ln 891 ‘all three optimization strategies showed comparable skill against assimilated data’). Please rephrase. I agree that Argo-float measurements are very useful – still robustness of their estimate was apparently not tested (e.g. it is likely that the estimated optimal parameter set changes when another data set is used for fitting).*

Response. We agree the original wording claimed a statistical robustness we did not establish, given that several parameter sets reach a comparable fit. We removed the phrase “robustly constrain” from the parameter-uncertainty results, which now describe the constraint as uneven across the 95 parameters and concentrated in the most sensitive ones, and the Discussion notes that most parameters remain weakly constrained. To address the specific point that robustness against alternative fitting data was not tested, the Conclusion now states: *“Whether the optimized parameters stay stable and transferable across other observational datasets — different temporal windows or subsets from the same float, and floats from other biogeochemical regions — was not assessed here and remains to be tested.”*

> ***Ln 1085ff:*** *Here it is implied that the presented results could solve the problem of the bg-models being ‘under-constrained’. I would suggest to tone down a bit. It has not been shown in the presented work that respective issues are solved and, on the contrary, the authors state that several parameter settings lead to a similarly good fit (e.g. ln 891). Respective model sensitivities to changes in the external conditions (e.g. global warming) were not tested and compared. The authors might consider to mention this omission (or to perform respective experiments).*

Response. The reviewer is right that the original passage implied the under-constrained problem had been solved. We reframed the equifinality discussion so that it presents the result as a change in the character of the problem rather than its resolution: the abstract and Discussion now describe the work as shifting correlated equifinality to uncorrelated equifinality, in which multiple optimal parameter sets still exist. On the second point, we added a limitation paragraph noting that sensitivity to conditions outside the present-day range was not tested; it concludes: *“a parameter set tuned to present-day conditions may lose validity under future regimes, ultimately requiring periodic re-calibration or adaptive parameterizations that respond to the ambient state.”*

> ***Ln 1122:*** *At this point I don't agree. In my view biogeochemistry inescapably depends on ocean mixing – also in 1D (the authors might convince me by presenting a test experiment where they still obtain the same parameter set while changing the mixing significantly in 1D).*

Response. We take the point that biogeochemistry depends on ocean mixing, including in a 1D configuration. We rewrote the discussion of the 1D approach: rather than presenting it as separating the biogeochemical response from ocean-dynamics uncertainty, the revised text states that vertical mixing is prescribed by the CMEMS-PHY analysis rather than constrained by the float, so that errors in that mixing may be partly compensated by the calibrated parameters. We did not carry out the mixing-perturbation experiment the reviewer describes, and the manuscript now says so directly: *"We did not test how the optimized parameters would change under substantially different mixing conditions."* As partial evidence on the question, we note that the parameters nonetheless improve the 3D regional model, which uses its own dynamically simulated physics rather than the offline forcing of the 1D experiments.

> ***Ln 1140:*** *better? 'reasonable' instead of 'robust'*

Response. We removed "robust" here. In place of the suggested "reasonable," we settled on "fuller accounting," which we found more precise about what the All-parameters strategy provides; the Conclusion now reads: *"this approach provides a fuller accounting of parametric uncertainty for unassimilated variables than optimizing smaller, GSA-informed subsets."*

> ***Ln 1148:*** *I would suggest to rephrase 'forecasting systems' because no forecasts were presented in this study.*

Response. We agree "forecasting systems" was imprecise, since the study performs no forecasts. The manuscript no longer uses that phrasing; the Conclusion now describes what was actually validated: *"This demonstrates that a 1D optimisation trained on a single North Atlantic BGC-Argo float can yield parameter sets that improve an operational 3D biogeochemical model simulation in the IBI domain."*

> ***Ln 1126:*** *The authors nicely illustrate that their parameter estimates can also improve the model performance elsewhere (with exceptions). That said, I find the statement 'confirms the transferability of the optimized parameters' a bit too strong since it is not clear if the optimized parameters from the 1D experiments are also optimal in a 3D context. They are 'just' better than the original parameters also at another location. Please don't get me wrong – I regard this as a large step. There is no need to exaggerate.*

Response. We agree the original wording was too strong. The manuscript no longer states that the 3D validation confirms transferability; it now presents the result as the reviewer characterizes it — the optimized parameters improve performance relative to the reference set at locations away from the training site, without being optimal there: *"although they may not represent the optimal parameter set for this 3D simulation."* The Discussion now also describes the transfer to the 3D model as only partial.