Submission to EGU—Biogeosciences: Answer to reviewers (round 1)

The manuscript entitled *"Terrestrial browning from colored dissolved organic matter (CDOM) changes the seasonal phenology of the coastal Arctic carbon cycle"* explores the influence of CDOM on both biogeochemical and physical processes in the Mackenzie River region. The subject is both relevant and timely, addressing key concerns about the role of land-ocean interactions in a rapidly changing Arctic environment. The study provides promising insights that could significantly advance our understanding of how CDOM modulates seasonal carbon fluxes and ecosystem functioning in coastal regions.

However, a few issues require clarification and refinement before the manuscript can be considered for publication.

Major comments:

1. Clarification needed on the consistency of the validation datasets and years used

The authors state that they focus their analysis on the year 2012, and most of the figures throughout the manuscript appear to reflect results from that year. However, it remains unclear whether the model-data comparisons presented are also based solely on 2012 data. For instance, in Appendix B (Figure B1), the terrestrial CDOM ratio is compared to observations from 2009. The manuscript would benefit from a clear statement on whether these validation figures are meant to support the 2012 model run specifically, or whether they are included to assess model performance more generally.

According to the reviewer comments, we clarified that the year chosen for model study and model-data evaluation is 2012 (see later answers) and we added a sentence in the Methods L181 — "*We took the advantage of an extensive in-situ carbon dataset collected in 2009 to update the parameterization of CDOM mass fluxes …*". We also included in the sentence L191-192 a note emphasizing that our model parameterization for CDOM (terrestrial CDOM ratio and k_{CDOM}/CDOM relationship) was estimated in 2009, the year of the Malina field campaign: "*… we focus our analysis on the 2012 – different from parameterization year (2009) – for two reasons: …*".

In addition, a major part of the validation appears to rely on the study by Lewis and Arrigo (2020), yet the exact years used in that comparison are not clearly stated in the present manuscript. Upon reviewing Lewis and Arrigo (2020), it is also not obvious which datasets were used by the authors of the current manuscript to reproduce the validation figures, particularly Figure D1.

Greater clarity is needed on:

- which years of observational data were used in each validation figure,
- whether these years correspond to the 2012 simulation,

- and which specific datasets were extracted from Lewis and Arrigo (2020) or other sources.

According to the reviewer comments, we clarified the year of the model-data comparison in the core of the manuscript at L230 as follows: "*A more detailed and comprehensive model-data evaluation for 2012 is provided in Appendix D*". We also clarified both year and Lewis and Arrigo (2020) datasets in Appendix D, Figure D1 (now Figure 9), and L499–502 as follows: "*We compared the 2012 weekly surface-ocean Chl-a (mg Chl-a m^{-3}) and daily primary production (mg C m^{-2} d^{-1}) simulated by ED-SBS (Run_{ctrl} and Run_{full}) with satellite observations from Lewis et al. (2020) data estimated by the AOReg.emp algorithm. We also compared simulated daily-mean SST (°C) for 2012 in both models with in-situ/satellite observations (OSTIA Good et al., 2020).*".

2. Terminology in the manuscript may overstate the scope of the analysis

In a few places, the manuscript uses terminology that suggests a broader ecosystem analysis than is actually conducted. For example, in the introduction, the authors state that they explore the effect of CDOM on the "seasonal cycle of plankton biomass, productivity, and carbon cycling," which implies a study of both autotrophic and heterotrophic plankton communities. Similarly, in Section 3.4, the title "CDOM effect on marine productivity" gives the impression that both primary and secondary production are addressed, whereas the analysis focuses solely on phytoplankton primary production.

Another example appears in the sentence: "We explore the specific effects of CDOM light attenuation and ocean heating on the coastal ecosystems and the carbon cycle." Again, the wording implies a broader analysis than what is presented in the paper.

To improve clarity and avoid overstatement, I recommend that the authors revise general terms such as plankton productivity, ecosystem dynamics, and carbon cycle to more specific phrases such as phytoplankton production, chlorophyll concentration, or primary production. This is especially important given the study's focus on a single trophic level (phytoplankton only), while terms such as "ecosystem" may imply a broader biological or biogeochemical system.

According to the reviewer comments, we clarified the focus of the study by correcting the terminology where we identified a clarification was needed, replacing "*plankton biomass*" by "*phytoplankton biomass*" (see L62), "*productivity*" by "*primary productivity*" (see L62, subtitle 3.4), "*ecosystems*" by "*primary producers*" (L276), "*low nitrogen ecosystems*" by "*low-nitrogen adapted phytoplankton*" (L410-411), and specifying what aspect of the carbon cycle we focus on (L275–276) as follows: "*In the remainder of the study, we explore the specific effects of CDOM light attenuation and ocean heating on coastal primary producers and the carbon cycle, focusing on the biological and solubility pump*."

3. Potential confounding effects due to differing nutrient concentrations in simulations

In Figure 7a, it appears that the initial nutrient concentrations are higher in the CDOM ON simulation than in the control simulation (CDOM OFF). However, the manuscript does not mention any differences in nutrient forcing or initial conditions between the two simulations.

Given that nutrient availability is a key limiting factor for phytoplankton growth, such differences could strongly influence the interpretation of CDOM-related effects.

If nutrient fields or boundary fluxes differ between the simulations, the observed variations in phytoplankton biomass and productivity may not be attributable solely to the effects of CDOM on light attenuation or temperature. I recommend that the authors clarify whether nutrient initial conditions and boundary forcings were kept identical between simulations. If they differ, a justification should be provided, and the interpretation of the results should be revisited accordingly.

We thank the reviewer for pointing out this difference, which might confuse the readers. We confirm that we used the same forcing for every simulation and clarified this in L193–196 as follows: "*However, all simulations were performed with the same forcings over 5 years (2008–2012) to mitigate spin-up effects in processes directly affected by the inclusion of CDOM, such as dissolved carbon (DOC and DIC) concentration, or indirectly affected such as nutrient stock through changes in primary productivity.*" The changes in nitrate concentration from CDOM OFF to CDOM ON in Figure 7a are then solely related to difference in phytoplankton consumption resulting from CDOM dynamics in the 4 years of simulation preceding 2012. We clarified this L293–294: "… – *Note that the differences in May surface nitrate concentrations observed in Figure 7a are related to changes in stock replenishment over the spin up period due to CDOM inclusion*"

4. Clarification needed on bloom timing and comparison to observations

In the discussion (line 370), the authors state that the model "successfully simulates the timing of the bloom peak compared to Lewis et al. (2020)," while also noting that improvements to the sea ice model are needed to simulate bloom initiation more accurately. However, it is not clearly stated which simulation is being referenced (CDOM ON or CDOM OFF).

Based on the figures comparing model results to observations (e.g., Figure D1), it appears that the CDOM OFF simulation initiates the bloom closer to the observed timing, while the CDOM ON simulation results in a delayed bloom. This seems to contradict the statement that the model accurately reproduces bloom timing — unless the authors are referring specifically to the amplitude of the bloom rather than its onset.

I recommend that the authors clarify:

- which simulation is being evaluated when claiming agreement with observations,
- whether CDOM improves or delays bloom timing relative to the data,

- and how this affects the interpretation of CDOM's role in modulating bloom phenology.

A more nuanced discussion would be helpful here, especially if CDOM appears to delay the bloom in a way that is less consistent with observed phenology.

We thank the reviewer for his comment and strengthened our discussion to better emphasize how CDOM improves the model ability to simulate physical and biogeochemical seasonality with regard to sea-ice melt. Below are the following aspects of the paper we changed to strengthen the discussion:

- We moved Figure D1 in the main text (Now Figure 9). Note that changes in time series appear due to the adjustment of an artifact observed in the Lewis et al. (2020) product. This point is now further explained in Appendix D (L507–513) and new Figure S3.
- We added a paragraph (L380–396) discussing the model-observation comparison with regard to SST, phytoplankton, and sea-ice phenology and further discussed model improvements needed to enhance bloom phenology (L397–414).
- We modified Appendix D to further detail the comparison method and analyze SST comparison and added a new Figure D1 showing the model-observation spatial comparison for SST and surface-ocean Chl.

5. Apparent inconsistency between NPP and chlorophyll concentration.

In line 365, the authors state that the simulated phytoplankton amplitude is 85% higher in the CDOM ON simulation, which is consistent with the values shown in Figure 7a (NPP ≈ 4.6 Gg C d^{-1} without CDOM vs. ≈ 8.1 Gg C d^{-1} with CDOM). However, in Figure D1 — which compares surface chlorophyll concentrations from both simulations to observations — the amplitude of the two simulations appears quite similar.

Since chlorophyll is commonly used as a proxy for phytoplankton biomass, one would expect a notable difference in amplitude if NPP nearly doubles. The manuscript does not provide an explanation for this apparent discrepancy. Could the authors clarify whether this discrepancy reflects a decoupling between biomass and productivity in the model (e.g., due to C:Chl ratio dynamics or other processes such as loss terms or export)? Additional clarification would help interpret how CDOM influences not only the rate of primary production but also the standing stock and its comparison to observations.

We thank the reviewer for pointing out this difference between Figure 7a and Figure D1 (now Figure 9), which could cause misunderstanding. We argue that the differences in the time series are related to the difference in the calculation of this time series (see further explanation below), which makes any further assumption on the change in C:Chl ratio dynamics or other processes complicated.

Indeed, surface NPP in Figure 7a is calculated daily using all model grid cells located within the river plume region (n=281), while surface-ocean Chl-a and primary production in Figure D1 are calculated on a monthly (for Chl-a) and weekly (for NPP) basis using the model grid cells located within the river plume region (defined by isohaline 27 threshold) and for which a satellite observation from Lewis et al. (2020) is available (7<n<281). Our statement is supported by the difference in primary production amplitudes between Figures 7a and D1b, similarly as observed by the reviewer for surface Chl.

To avoid any confusion and better guide the reader, we further detailed the model-data post-processing method in Appendix D (L502–505): "*As both observational products have a finer horizontal grid spacing compared to ED-SBS, we bin averaged the observations within each model grid cell. We then calculated the spatially-averaged value within the Mackenzie River plume region where satellite data were available to assess the model's ability to represent these observations (Figure D1). Note that the number of observations (n) available within the river plume area (n_{max}=281 for the entire area) varies in time.*".