Reviewer #2 (Julien Jouanno)

This paper presents an open-source, customizable Lagrangian framework for Sargassum transport and growth, implemented within the Parcels model. It couples physical transport by currents, windage and a depth-integrated Stokes drift with a biological growth model limited by temperature, nitrate and salinity, and it can be initialised from SaWS satellite detections. The development of an open, community-usable tool of this kind is a useful contribution, and the implementation of a depth-integrated Stokes drift is of interest. I have a number of comments that I believe would strengthen the manuscript.

We thank Dr Jouanno for these very kind words and his enthusiasm for our manuscript!

Major comments

1. Positioning relative to previous studies. The authors do not clearly delineate what is new. Several components already exist in the literature: the temperature/nitrate/salinity growth limitation is largely taken from previous work, and the Lagrangian growth–decay approach is close in spirit to Podlejski et al. (2024), which reaches a similar conclusion on the role of temperature (by construction of the temperature dependence of the growth function I guess). It would help the reader to state more explicitly what the genuinely novel contributions are. I take these to be the implementation within Parcels and the depth-integrated Stokes drift.

This is a good point by the reviewer. In the revised manuscript, we are now much more explicit about these two lines of novelty (lines 35-38 of the track-change pdf): "*We take a Lagrangian approach, where we explicitly model the kinematics of individual Sargassum rafts. Advantages of a Lagrangian approach are that full trajectory information is available and that stranding can be incorporated by a simple velocity-based criterion. A drawback is that spatial concentrations of Sargassum are less easily computed as in a tracer-based Eulerian framework.*"

2. Stokes drift and its interaction with windage. The depth-integrated Stokes drift is an important aspect of the transport that, to my knowledge, has been overlooked by the Sargassum community. I think this aspect is currently under-exploited in the manuscript, and developing it further could substantially increase the interest of the study. The windage coefficient (1%) has traditionally been used to account for unresolved Stokes drift, so resolving Stokes explicitly while retaining a 1% windage may double-count the wave contribution. Since the fields and machinery are already in place, a sensitivity analysis, running with and without the explicit Stokes term, and with windage across a 0–3% range, evaluated against the observed distribution, would justify the parameter choices and

quantify what the explicit Stokes term adds beyond windage. This would also help constrain the raft depth extent, currently fixed at 1 m without a demonstrated sensitivity.

This is a good point by the reviewer. While we primarily envisioned our notebook to be used by researcher who would want to explore these sensitivities themselves, the reviewer is right that we can already provide a sensitivity analysis ourselves. We have therefore now added an analysis of the effect of windage and Stokes drift to the manuscript (section 3.1.2 and Figure 3 on lines 250-254 of the track-change pdf).

3.Model validation. The transport validation rests on a single visual comparison (Fig. 2) between the July 2024 SaWS distribution after 31 days and the 1 August detection. Over a one-month integration the two distributions are very similar, which limits what can be concluded from this figure. Since basin-wide monthly runs for all of 2024 are already produced (Section 3.2) and SaWS imagery is available year-round, a seasonally-resolved and quantitative skill assessment would be more convincing. Metrics such as those used in Berline et al. (2019) or Bonner et al. (2024) would allow an objective evaluation and would help refine the windage and Stokes-drift choices raised above. In short, my main recommendation would be to make the validation more quantitative.

This is also a good point by the reviewer. We have now added an analysis of the True Skill Statistics in Figure A3 and lines 215-228, 240-243 and Figure 3 of the track-changed pdf.

4. Nitrogen limitation. Growth is limited by surface nitrate alone, which is at odds with conclusions reached by several authors indicating that both nitrogen and phosphorus limit growth (Lapointe et al. 1986, McGillicuddy et al., 2023, among others). There is also growing evidence for the role of diazotrophy, which can locally relieve nitrogen limitation when temperature, iron and phosphorus conditions are favourable. I therefore read the argument at L283–290 differently from the authors: Jung et al. (2025) attribute Sargassum growth to diazotrophy (N_2 fixation) driven by excess phosphorus from equatorial upwelling, which implies that the relevant nitrogen source is not only the ambient nitrate. Using this study to justify neglecting phosphorus and modelling only nitrate therefore appears to run counter to its conclusion. I would suggest revisiting this reasoning, and possibly discussing whether a phosphorus- or diazotrophy-informed nitrogen source would better represent Sargassum growth patterns.

This is a good point by the reviewer. Indeed, our assumption that growth is limited by surface nitrate alone was too simplistic. We have now extended our model to also support phosphorus growth limitation in a similar style as nitrate growth limitation.

However, because we couldn't find any reliable estimates of the magnitude of the phosphorus half saturation constant k_P, we have set it to 0 for our analyses, so that $f_P = 1$.

However, other users of our framework will be able to easily adapt the value of k_P to explore growth sensitivity for themselves.

We have now added a discussion of phosphorus growth limitation to the revised manuscript and removed the statement that the Jung et al (2023) result suggest that phosphorus is not relevant (lines 170-174, 275-279, 320-322 and 370-371 of the track-change pdf).

5. Relevance of the Lagrangian approach. A discussion of the strengths and limitations of representing Sargassum with Lagrangian particles, relative to Eulerian alternatives, would be a useful addition. Points worth addressing include the treatment of raft dispersion and fragmentation, and the behaviour of the simple velocity-based stranding criterion at the coast.

We have now added a few sentences on our motivation for choosing a Lagrangian approach in the introduction (lines 35-38 of the track-change pdf), as well as notes on further development in the discussion (lines 381-387 of the track-change pdf).

Minor comments

L21: Wang et al. (2019) would be a more appropriate citation.

This is a good point by the reviewer; we have changed the references to Gower etl al (2013) and Wang et al (2019) in this sentence (lines 22-23 of the track-change pdf).

L36: Maybe cite Jouanno et al. 2023, in which a seasonal forecast was developed with skill assessment.

We have now added Jouanno et al (2023) to the list of references (lines 40-42 of the track-change pdf).

L40–42: It is not clear what "biophysical feedback" means here, please clarify. Regarding the potential influence of size, mass, and density, I agree in principle, but I am not sure any of the cited references have actually quantified or demonstrated such an impact.

We have now shortened this sentence by removing the term biophysical feedbacks and the references (line 46 of the track-change pdf).

L47–49: In Jouanno et al. (2025) it was shown that the GASB offers more favourable conditions for Sargassum growth, in terms of temperature, light, and nutrients, than the Subtropical Gyre.

We have now added a reference to Jouanno et al (2025) to this sentence (lines 5 of the track-change pdf).

L114: I find this formulation somewhat unusual. What is the rationale for the exponent of 2? I understand that Hanisak and Samuel use this form because it is the most appropriate way to approximate a growth rate from two measurements distant in time, but for numerical treatment I would find it more accurate to write B = B(t) × (f − G)·Δt.

The factor of 2 arises directly from the definition of the specific growth rate used in our model. Following Hanisak and Samuel (1987), μ is expressed in units of doublings per day and is calculated as $\mu = log_2\left(\frac{W_t/W_i}{t}\right)$. Consequently, biomass after a time interval (Δt) is obtained as $B(t + \Delta\, t) = B(t)\; 2^{\mu\,\Delta t}$.

Thus, the base 2 is not introduced as a numerical approximation but follows directly from expressing the empirical growth rate in doublings per day. We also note that the suggested linear formulation would represent a different numerical treatment of biomass change and would not directly preserve the experimentally defined doubling rate. As our objective is to retain consistency with the empirical growth-rate formulation, we keep the original formulation.

L144: I suspect this is largely dependent on the forcing used. Surface NO_3 can differ by more than an order of magnitude between biogeochemical models.

Indeed, that is also what we expect; which is why we mention that their value is optimized for their specific model configuration.

L157: Please specify the basic characteristics here, in particular, whether assimilation is included or not.

We have now clarified that all data sources we use are indeed data-assimilated runs (line 74 of the track-change pdf). The full details are in Table B2.

L189–L190: I do not see a comparison with the information obtained from this cruise.

That is correct; the data from this expedition was not directly useful for this manuscript. Instead, our model is useful to help further interpretation of the expedition data.

L193–198: For a one-month integration, the distributions are very similar, so it is difficult to assess the model's efficiency. Could you provide a more quantitative metric? See, for example, Berline et al. (2019). This would allow you to refine the choices of windage and of the vertical integration of Stokes drift.

As we also mentioned in our response to point 3, we have now quantified the agreement between model and satellite distributions using the True Skill Statistics of Allouche et al (2006).