Response to referee comments

We are grateful to the reviewers for the detailed and constructive comments that helped us to improve our manuscript. The manuscript has been revised to comply with the referee comments and suggestions. Please find below the point-by-point responses to the reviewers. In the following, the referee comments are in black and our answers tare in blue.

First referee comments

The study is novel and could be worth publication after minor-to-major revisions.

General comments:

The presentation of the major findings and results as associated with the key figures should be more adequately outlined and addressed. Now it often appears too brief as it rushes from one figure and relatively short explanation and discussion to the next. This should be improved.

We thank the reviewer for raising this aspect. First, we want to stress that the paper has been written in the aim of being as concise and compact as possible. Indeed, the BFN-QG method was already described in a recent work (Le Guillou et al., 2021) published by some of the authors of this paper and tested using an Observing System Simulation Experiment (OSSE). Similarly, the validation strategy involving an Observing System Experiment (OSE) was already presented in a previous work (Ballarotta et al 2020) also published by some of the authors of this paper. That is why the figures are used in a way to support the main messages more than providing deep analysis. Still, we agree that the figure explanations and discussions could be more extended and better organized. We made consistent changes to do so:

- Figure 1 is removed, as it was not discussed in the manuscript. **From now, figure numbers in our answer refer to the new ones of the revised manuscript.**

- The paragraph starting at line 117 has been modified to better discuss Figure 1.

- We add some text to better present Figure 2 in the paragraph starting at line 138:

  "*Figure \ref{fig:03} shows maps from BFN-QG and DUACS, for one single day, with a SARAL/AltiKa altimeter track superimposed. SARAL/AltiKa SLA observations and SLA interpolated from both maps onto the satellite track are shown in the middle panel. In the case presented here, the BFN-QG result fits the independent observations SLA^{ind} better than the DUACS product.*"

- Section 4 has been reformulated to allow a deeper analysis of Figures 2, 3 and 4. Figure 3 is discussed in a first paragraph to assess the performances qualitatively. Then, two paragraphs are added to present quantitative diagnostics, each specific to one metric (RMSE and PSD). After, we present Figures 3 and 4 by adding this sentence at line 162:

  "*The results of the quantitative evaluations are reported on Figures \ref{fig:03} and \ref{fig:04}. Figure \ref{fig:03} shows the number of observations per box, the spatial distribution of $RMSE_{BFN-QG}$ and R for all scales and for the mesoscales. Figure \ref{fig:04} the PSDs and the PSD scores.*"

- Section 5.1 has been modified to better discuss Figures 5, 6 and 7.

Does the choice of the 1.5 layer QG model impact the results in any way? In other words, are there other modes than the first baroclinic mode that might be influecing the dynamics and variability in the greater Agulhas Current region? Is it possible to find discussion of this in the literature? Need some more consideration.

Yes, higher baroclinic modes influence the dynamic in the Agulhas Current region, although the QG dynamics —and therefore SSH dynamics— is principally captured by the first baroclinic mode (Fu and Flierl 1980; Shafer Smith and Vallis 2001). We deeply think that the performances of the assimilation procedure rely on the balance between the density of observations and the complexity of the dynamical model. Simulating the dynamics of higher baroclinic modes would greatly decrease the controllability of the dynamical model with only sparse along-track altimetric data and hence reduce the performances of the reconstruction. However, as the just-launched SWOT mission will considerably increase the density of SSH observations, we plan to assimilate these future data in multiple layers QG models to improve the reconstruction of the SSH dynamics. This perspective has been added in the last section of the manuscript in a dedicated paragraph starting at line 270, following the Reviewer's last general comment.

Line 210 comments on the Agulhas Current area and claim that it is characterized by weak advection of the frontal structures. However, in view of the area (Figure 9) the presence of Natal pulses can propagate southwestward on the nearshore side of the Agulhas Current core while highly rich and complicated dynamics occur in the retroflection area where eddies are often shed and also sometimes reintegrated into the retroflection. These areas are characterized by advection and sometimes rapid

occurrances and closing of frontal structures. So the statement must be refined to better balance these conditions.

We thank the reviewer for this very relevant remark. The manuscript has been modified accordingly (lines 217-219).

In Figure 10: The black line in the 3 panels seems to be contours of the land-ocean boundary. Must be explained. A bit strange to see values within boxes that are completely inland.

Indeed the black lines refer to the coastline. The colored pixels "inland" are due to different definitions of the coastline in the diagnostic algorithm and the plotting function. This mismatch has been solved for all figures in the manuscript, see hereafter an example for Figure 9.
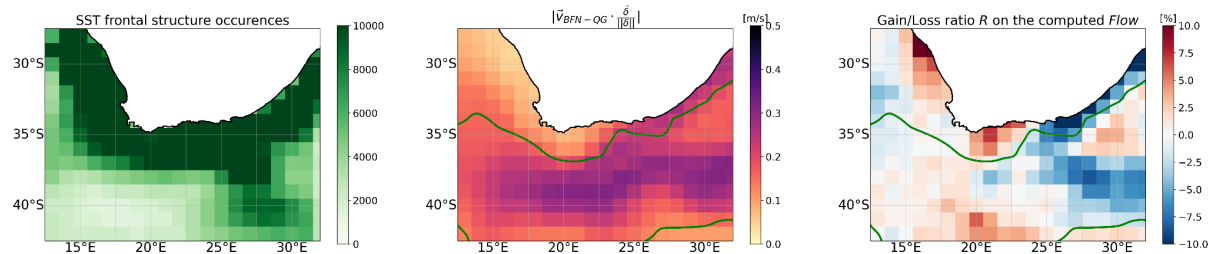
(new) Figure 9 with the inland pixels removed

In the discussion and conclusion it would be worthwhile to comment on possible use of SWOT and Sentinel-1 image data to further strengthen this study approach.

We agree with the Reviewer to add a paragraph in the conclusion section to discuss the opportunities (and challenges) to exploit synergies between conventional altimetry and other space-borne data to improve the reconstruction of surface currents. First, we discuss the opportunity of the next-generation altimetric mission (e.g. SWOT) to improve the reconstruction of geostrophic currents (e.g. Le Guillou et al., 2021) in a dedicated paragraph starting at line 270. A last paragraph starting at line 277 has been added to discuss:

- the possibility to use Sentinel-1 to assimilate radial ocean surface velocities (e.g. Moiseev et al., 2020);
- the use of of satellite tracer data (such as SST or Chlorophyll) to better constrain the reconstruction of total surface currents (e.g. Rio et al., 2018).
- the integration of future Doppler missions.

Specific editorial comments:

The submitted paper is attached with red-markings suggesting update of the text.

The additional editorial comments have been taken into account in the updated version of the manuscript.

Additional edisotrial comments:

Agulhas Current should always be with captial C.

This has been changed for all occurrences of Agulhas Current in the revised manuscript.

Be consistent in use of reference to figures (Fig., Figure, figure, I prefer Figure).

We use "Figure" consistently all over the revised manuscript.

Equation 10: The expressions in the exponent should have larger font. Difficult to see properly.

Equation 10 has been modified accordingly.

The space scale symbol D (in equation 10) must for consistency also be used in Table 1 (nudging space scale).

Table 1 has been modified accordingly.

Second referee comments

This paper considers a technique for obtaining geostrophic circulation from SSH maps (BFN-QG) and applies it to real altimetric SSH data. This method is compared to another method (DUACS) and shown to improve the mapping of short energetic mesoscale structures, producing finer scale coherent structures. While the method improves the mapping of smaller scale high energy structures where there is high variability, it does not perform well for larger structures when the variability is low. Overall this paper is scientifically sound, well written and merits publication in this journal. I have not found any major issues with the paper, and believe it is almost ready for publication, but below I list a couple of minor points that should be taken onboard before final submission.

Minor comments:

page 3 - equation (4): The authors might consider using a different symbol for SSH when applying it in the mathematical formulas, for example maybe using H for SSH, and then make subscripts for when referring to the MDT and SLA part, like H_{MDT}, H_{SLA}.

We thank the reviewer for this suggestion but we prefer not to take it into account for two reasons:

page 4 - equations (7a) and (7b): You have switched the symbol for the Coriolis parameter from f to f_0. I presume we are considering the f-plane here, i.e. f=constant, this should be stated in the text.

Indeed, we thank the reviewer for this comment. f_0 is now defined at line 63.

page 7 - table 1: Typo - nudging space scale should be denoted as D instead of tau which is used for time scale.

Table 1 has been modified accordingly.

page 7 - equation 11: confusing use of the square brackets containing the letter i, would be better to apply i as a subscript, i.e. SLA_i, and then have brackets all around the delta SLA term, then apply square, i.e. [\Delta SLA_i]^2.

Equation 11 has been modified accordingly.

page 11 - equation for flow crossing: This equation is not well defined, what is v[P_i] (presumably the velocity vector) and what is the angled symbol on the RHS inside the cosine term, even if referenced the equation variables needs to be defined here.

All the variables in equation 15 are now defined at lines 218-219.

References

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