

Most of the comments on the first review have been fulfilled by the authors. These changes have made a much better and clear paper.

Particularly appropriate is the change of the title of the paper and changes in the introduction.

1) Unfortunately, the modifications suggested for point 2 have not been properly followed:

*2) Section 2 is failing to provide a pragmatic and consistent overview of the usefulness and validity of the techniques that are being described. For example, for some techniques the limitations are explained in much more detail than for others. It would be highly valuable to define, in a systematic way, the expectations of each technique, as well as its limitations in terms of accuracy, capability of deliver timeliness information, spatial resolution, etc. . .*

*In this sense, and being a review paper, it is obvious than additional information should be included on the pros and cons of these techniques when compared to the other main source of current information, the operational forecast models.*

*Finally, given the nature of the paper (a review by experts) some insight should be included on the value of the present techniques to address different specific problems, that at the end are linked with different spatial and temporal scales. Maybe some of the techniques are not valid for some uses like, for example, oils spill forecast, but could be very useful to derive a climatology. This is never addressed, and is vital. A possible solution to most of these problems could consist on a table explaining, for each one of these techniques, the status of development, limitations and possible uses.*

*Authors reply:*

*In the new version we have been careful to provide a balanced account of details for each of the techniques reviewed. Note however that these products are not yet been used in global operational forecasting models.*

*We have followed your suggestions and we have now added some new material in the sense you mention. Now, a new figure illustrates (figure 3) the current status in terms of spatial and temporal scales of sea surface currents observations according to the GOOS panel. We have also included in the summary section a table listing some key parameters for future use in operational assimilation systems (latency, resolution,)*

The new table is a step forward in this direction, but part of the problem remains. From the reader point of view, it is difficult to conclude whether a technique is accurate or mature, or just experimental. Without this information, the paper is a more a theoretical review than a review of the state of the art of the methodologies. It would be highly valuable to define, in a systematic way, the expectations of each technique, as well as its limitations in terms of accuracy, operationality, etc. Now thanks to the table, horizontal resolution (Delta x grid vs delta x min must be explained) and latency are clear.

The rest of the ideas by the reviewer in this point (the pros and cons of these techniques when compared to the other main source of current information, the operational forecast models; and insight on the value of the present techniques to address different specific problems) were not followed for this second revision

2) Reading of section 2.2 is still difficult. This section is too long, and I recommend at least sub-sectioning, leaving more clear what is historical development and new approaches.