Response to reviewer 1

+ Section 1 and 2:

The introduction has been entirely rewritten to take into account the reviewers comments, namely:

- The validation issue has been removed from the objectives in order to focus on the monitoring issues (reviewer 1 comment)
- The description of the SLTAC has been shifted to section 2. (reviewer 2 comment)
- The scientific context has been improved. In particular, references to peer-reviewed articles have been added for each existing indicators cited. (reviewer 2 comment)
- Some examples of application/purpose of the indicators have been added. (reviewer 2 comment)
- The advantage of using altimetry for defining these indicators has been better stressed: First (lines 35-37) it is a very well suited observational system (global and continuous) and second (lines 58-60), it is quite relevant to monitor changes in circulation patterns that imply –as it is the case in the Kuroshio and Ionian Sea areas- strong changes in the Eddy Kinetic Energy, a quantity that is easily derived from altimetry. (reviewer 2 comment)

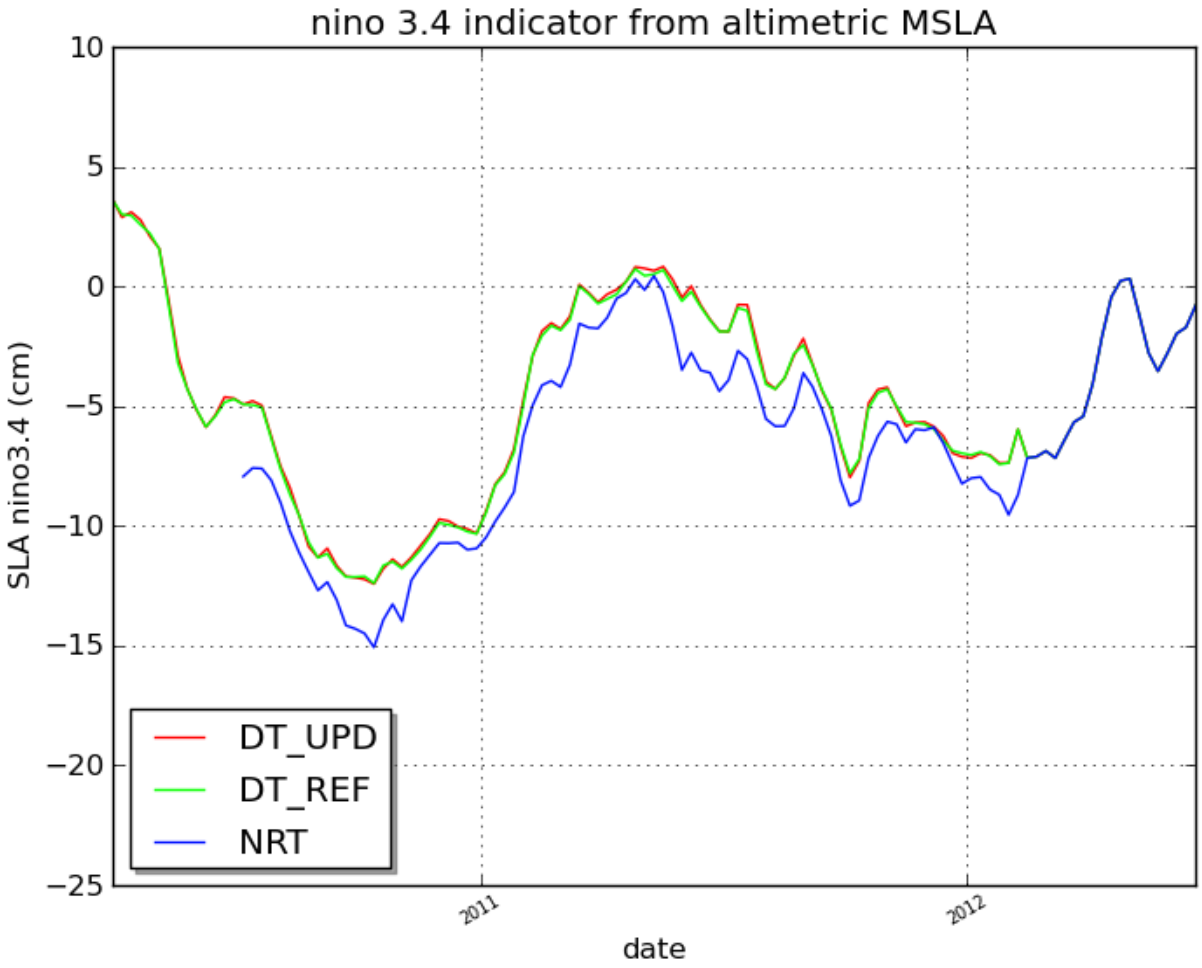
Response to Reviewer 1 main concern about "why the authors found it necessary to build some of the new indexes based on box averages that mainly seem to reproduce EOF amplitudes, instead of directly projecting observations (even observations that were not used to build the EOF base) on the relevant modes.":

In this paper, the objective was to define indicators for the Kuroshio and the Ionian Sea region in a similar way than what is done for the El Nino indicator, i.e. by defining simple boxes areas (so that this can be easily reproduced by everyone) in which the average of a physical quantity as the EKE catches the main pattern of variability of the tracked phenomena. The EOF decomposition is therefore intended here only as a tool that helps us define the most relevant box. Consequently, and as suggested by reviewer 2, we have deleted the EOF method description in section 2.

+ Section 3.1: I suggest adding some references.

+ Section 3.3: The motivation for using DT vs NRT should be better discussed, possibly avoiding qualitative adjectives as 'best' or 'slightly lower'. Given the authors introduce this issue, it might be interesting to know if and how the indicators would be affected by using one product instead of the other. Moreover, it is not clear if the authors used the 'reference' or 'updated' products (i.e. if they are using a consistent dataset in terms of data coverage/sampling).

➔ Done see paper.
For more information the figure below shows the impact of the data used for the calculation. Very little between DT-UPD et DT-REF, larger in NRT/DT but can be explained. A correction can always be added… is it really worth it knowing that the general tendency is still given.



More details on how the preliminary temporal filtering of the
data has been performed should be added, even considering that another temporal filtering is applied at a later stage (3-month smoothing).

➔ Since we give the reference to the data used, all is indicated there

It would be nice to see a
discussion (at least some comments) on the physical reasons driving the observed differences between the SLA based and SST based indexes and also on eventual advantages in considering these differences in order to better describe the ocean state. Actually, SLA data reveal changes in the steric and eustatic components of the sea level and it might be interesting to know (for example) if the authors think that the new index may identify changes to the stratification better than the SST index alone (if the steric component is assumed to dominate the variability at these scales).

➔ It is hard to go deep into the differences between altimetry and SST based indicator in this region since the two time series show a correlation of 0.88 which is very large and poses the problem of what is really related to signal and to data manipulation. Study of some other areas would be better for such a discussion and seem to show that either altimetry or SST leads to a more relevant index in regard to the physical processes involved.

+ Section 5.1:

- Section 5.1 has been reduced to approximately the same size of corresponding sections for the two other indexes: (i) the description of transiting water masses connecting the Western and the Eastern Mediterranean basins with the Adriatic and the Aegean Seas has been suppressed, and (ii) the detail of the mechanisms of origin of the decadal oscillation has been shortened.

+ Section 5.2:

- In this section, most of the detailed results are especially crucial for the approach of the Ionian index adjustment, explaining and preparing sections 5.3 and 5.4. Yet, in order to avoid comments on results that are not shown, the following passage concerning a specific point of detail of our methodology has been suppressed:

  "…According to Pujol (2006), the SLA variability associated with the reversals of the northern IoSC is taken into account by the second variability mode during the period 1993-2003 and explains 5,6% of the total variance in the whole Mediterranean Sea, while the first variability mode, that explains 68% of the total variance, is related to the seasonal variability including steric effects. For each mode, spatial patterns of variability and the associated temporal variations computed for the period 1993-2003 have been first reproduced for comparison purpose (not shown)…."
- Concerning the first mode obtained from the 13-month filtered SLA, we only discuss processes with which it is associated, as it seems impossible to describe the mode 2 without at least shortly discussing about mode 1. Nevertheless, as the 1st mode is not used thereafter in the paper, and still with the intention to reduce section 5.2 and also to lighten the number of figures, we found it not necessary to include this figure.