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Interactive comment on "Decadal variability and trends of the Benguela Upwelling System as simulated in a high-resolution ocean simulation" *by* N. Tim et al.

Anonymous Referee #2

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This study is a statistical analysis of a 60-year simulation run with a 1/10° ocean model, with a focus on the Benguela upwelling system. Several indices are used to investigate the interannual and decadal variability of the system, and to go back to the main likely atmospheric drivers. An index based on the model vertical velocity is also analyzed in the light of several modes of variability that are known to affect the climate over Southern Africa.

The paper is rather well-written and the introductive section is properly handled, but the study itself fails to convince me fully for the following reasons:

- The choice of a high-resolution (1/10°) ocean only model is not enough backed up,

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especially because of the somewhat coarse spatial resolution of the atmospheric fields that were used to force it (the global NCEP/NCAR reanalysis); the validation of the model results is rudimentary, especially in the Benguela upwelling system: why would readers believe the results of this model in this region?

- One strength of the study could be the discussion of very careful statistics; however, on several occasions, the discussion focuses resolutely on results that are not statistically significant.

- The geographic extent of the upwelling index used to characterize the model variability is poorly justified.

- The paper has the virtue of confirming some known features of the variability noted previously in the Benguela upwelling system, but does not bring much enough innovative results, especially because much uncertainty lies in the atmospheric fields that force the ocean model.

- The study focuses only on statistical properties and examples of covariability, and does not provide clues for the physical mechanisms at work. The interpretation restricts itself to the vague description of influences.

Specific comments

Page 406, lines 15-19: More care should be brought to the explanation of what is called “upwelling” here (vertical volume transport, cold SST signature, etc.).

Page 411, lines 5-8: Agreed!

Page 412: Knowing the fine spatial scales of the oceanic response over the continental shelves, a 1° resolution SST product may not be optimal for model validation purposes.

Page 412, line 4: The choice of these two areas should be better justified.

Page 412, line 21: Is this global forcing product different from the gridded observational data introduced in section 2.1?

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Page 412, line 22: The horizontal resolution of the global model can be qualified as high, but nothing is said about the relevance of this high resolution in view of the spatial scales of the model forcing.

Page 412, line 18: The presentation of the model simulation is too short. Of course, more information can be accessed in the papers cited as reference, but the present study should at least present and discuss the restoring strategy used at the air-sea interface (both for salinity and temperature) to prevent the model from diverging too much from known climatological or observed values. The interference of this restoring with the development of interannual variability and trends in the ocean model should be carefully discussed too.

Page 413, lines 16-20: The offshore extension (as far as 8°E) of both regions troubles me, especially for the Southern Benguela domain that includes likely part of the retroflection of the Agulhas Current. What does a vertical velocity-based index calculated over such large domains really correspond to? Can it be truly linked to coastal upwelling processes?

Page 414, lines 6-7: I am glad the authors favor a robust statistical analysis, but every assumption made to this end should be very carefully introduced and discussed, with more text than simply referring to “usual” hypotheses.

Page 414, lines 16-19: Showing model maps of surface currents (speed and vectors) is not enough to state that they correspond to a realistic description of the ocean dynamics.

Page 414, lines 24-26: How much of the cold bias can be ascribed to the absence of fine spatial scales of variability in the atmospheric forcing, and especially in the wind stress?

Page 415, lines 7-12: How much of the correlation is explained by the seasonal cycle, and how much by interannual and decadal variability (the true focus of this study)?

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Page 416, lines 19-20: This is another assumption that needs to be better justified in the framework of the current study.

Page 417, lines 1-7 (and especially line 3): Not seeing in figure 6 the trends put forward by the statistical analysis is quite disturbing.

Page 417, lines 10-11: “downward”? “southward” more likely.

Page 417, lines 15-20: This discussion should discuss the drawbacks of using coarse resolution wind stress values over the shelf.

Page 419, lines 25-27: I hardly understand that a study aiming at promoting statistical results discusses scales of variability that are not strictly statistically significant. This is somewhat counterproductive

Page 421, line 9-10: I do not understand the statement “calculate the difference of their respective area averaged SLP difference ocean minus land”

Page 421, line 23: “predicts”, and not “redicts” (?)

Page 421, lines 25-28: The difference of statistics between NCEP/NCAR and ERA-Interim must be underlined. How much confidence can be put in the discussion of Bakun’s hypothesis if the significance of correlations with the upwelling index differs according to the atmospheric product in use?

Page 422, lines 6-10: The uncertainty in the model forcing function is for sure a key element here. Can it call into question the general reliability of the model results?

Page 424, line 1: “driven”, not “driving”.

Page 426: The last item of the conclusion should jump over the uncertainties noted in the atmospheric forcing and, thus, in the model results.

Figures: Some labels (especially for the color palettes) are unreadable (e.g. Figure 2)

Interactive comment on Ocean Sci. Discuss., 12, 403, 2015.