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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 #1

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This discussion paper addresses the question of low-frequency variability and trends in an eastern boundary current system, the Benguela Upwelling System, in relation to large-scale climate/atmospheric conditions. The authors use a high-resolution numerical solution from an OGCM and build statistical relations between an upwelling index derived from vertical velocities and atmospheric fields (wind and SLP) as well as climate indices (e.g. the multivariate ENSO index). Some of the main results of this study are the different drivers of the Northern and Southern Benguela regions, as well as the concurrent long-term decreasing land-sea SLP difference and the (barely significant) weakly increasing winds, contradicting a popular theory of the response of eastern boundary upwelling systems to global warming (Bakun 1990). Such theory is however

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found to hold on interannual time scales. I was a reviewer of an earlier version of this manuscript, submitted a few months ago to Climate Dynamics and rejected, though the authors were invited to resubmit a revised paper. My recommendation to the editor was to either accept after major revision, or reject and invite to resubmit, while the other reviewer's recommendation was reject and invite to resubmit. I am not sure about the reason why the authors decided to resubmit to a different journal. In the original version, the authors were also considering the outputs of a regional ocean model, which was exhibiting peculiar behavior that was not very convincing to me. Instead of revising the analysis of the regional model, the authors seem to have preferred to drop it, which is fine to me. In this revised version, I was pleased to see that many of my comments had been taken into account by the authors. However, I still have a number of concerns. In particular, the oceanic drivers, the influence of wind stress curl, the trends in upwelling-favourable winds and the discrepancy between the simulated and the observed trends in oceanic upwelling are not discussed. Thus, my recommendation is to accept the paper after major revision. Detailed comments are listed below.

General comments: The paper focuses on the atmospheric drivers of the BUS. Although it seems like a meaningful choice in a coastal upwelling zone primarily driven by alongshore wind stress, it is well established that oceanic conditions also strongly influence upwelling. For example, the meridional sea level gradient between the equator and the pole drives an onshore geostrophic flow that counteracts the offshore Ekman transport and leads to reduced upwelling (Colas et al. 2008, Estrade et al. 2008). Such compensating geostrophic transport depends not only on the sea level slope, but also on the depth of the Ekman layer, which is modulated by stratification. The latter may be shaped by such things as large-scale oceanic conditions or air-sea heat and buoyancy fluxes. I would thus suggest the authors to discuss these effects in section 6. Another point that seems necessary to understand the upwelling variability is its relation to wind stress curl. Ekman pumping is responsible for open-ocean upwelling/downwelling (as acknowledged by the authors page 404, line 26), but also contributes to coastal upwelling, since the wind drop-off zone induces cyclonic curl near the coast. Such cyclonic curl is reasonably well represented in the NCEP reanalysis, despite its coarse 2.5° resolution (Chelton et al. 2004, their Suppl. Figs. S2-S4). These considerations are absent from this work and should thus be included. For example, the analysis presented on Fig. 7 could be repeated, replacing wind stress by wind stress curl. The biases of NCEP compared to for example QuikSCAT, although not very large, may also be discussed. According to the introduction, Narayan et al. (2010) show that the upwelling has increased in the recent decades in the four main upwelling systems, including the Benguela. However, the STORM model does not indicate clear long-term trends for increasing upwelling intensity in either the North benguela or the South Benguela. This is not directly dicussed by the authors. They do discuss the uncertainty in the NCEP SLP trends, but not the discrepancy between their study and that of Narayan et al. (2010) in terms of actual upwelling intensity. Along the same lines, the authors never actually compute an index for upwelling-favourable winds and show the consistence (or lack of) between the trends in this index and those of upwelling and SLP gradient. This would allow a better understanding of why Bakun's hypothesis does not hold on multi-decadal time scales.

Specific comments: page 406, lines 7-8: "by the cyclones... and by the pressure...". page 406, line 20: "eddy". Well, possibly also coastal-trapped waves (Rouault et al. 2007). page 407, line 7 to page 409, line 17: do you have any take on the possibly different influences of central Pacific and eastern Pacific El Niños (e.g. Kao and Yu 2009)? page 409, line 13: do you mean the Walker or the Hadley circulation? The Walker circulation is a tropical cell, while the Benguela is located in the subtropics. page 410, lines 17-18: actually, when in my original review I stated "see also Kim (Seon-Tae) et al. 2014", I meant the Nature Climate Change paper, which deals with the ENSO response to global warming. Sorry for the confusion. Also page 425, line 12. page 411, lines 12-13: this is still not only awkward, but unclear. Please have it read by a native english speaker and rephrase. page 411, line 19 (Tab. 1): Period: 1948-present for NCEP (see page 411 line 24). I believe the resolution of ERA-Interim is 1.5° rather than 0.75°. page 412, lines 7-8: the figure deserves to be included. page 412,

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lines 12-13: "remotely-sensed SST at 4 km resolution from the advanced...". page 412, line 13: please expand NOAA. page 412, line 20: "global" is repeated twice. page 413, line 16: what is the point in extending the grey box (N. Benguela) between 18°E and 30°E (fig. 1)? At these latitudes there is only land! page 414, lines 12-13: I think you should repeat the boxes for the N./S. Benguela regions on Fig. 2a,b to help the reader properly assess the validity of these boxes. In addition, if you include the figure of the upwelling/SLP correlation (see comment page 412 lines 7-8), you could indicate on it the boxes used for the SLP gradient computation, and then Fig. 1 becomes useless. page 414, lines 13-14: please use for HadISST the same domain as for STORM (Fig. 2c,e). Also, the color scale is not appropriate (too cold), it seems adapted to the HadISST domain. page 414, lines 16-19: why not include an extra panel (Fig. 2h) with a zoom in the Benguela region (as in panels b,d,f), to better see the different regional features you are describing (Agulhas and Benguela currents, westward drift)? Also, it would be good to see the SLP, wind and wind stress curl patterns from NCEP. page 415, lines 7-9: only significant correlations should be shown on Fig. 5. The domain of the upwelling index should be represented on each panel. page 416, lines 7-8: this statement should also refer to Fig. 4, not just Fig. 6. From Fig. 6 the reader cannot compare upwelling in the two regions. The reference to both figures should appear at the end of the sentence. page 416, lines 8-10: reference to Table 2 should be made here instead of the next sentence. page 416, lines 10-17: How is this supported by Table 2, which includes interannual variability and long-term trends, but does not explicitly separate interannual from decadal variations? page 417 line 9 (Fig. 7): Only significant correlations should be shown. The domain of the upwelling index should be represented on each panel. You should also present the correlation patterns with the wind stress curl (see general comments). Wind stress could cause coastal Ekman divergence, but also mixing, both acting to cool the SST. page 417, line 11: what is a downward wind stress? See also caption of Fig. 8. page 418, line 13 (Tab. 3). You should more clearly state that the trends are expressed in %. I assume the upper value for the MAM uncertainty range for the South Benguela

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trends is positive rather than negative. Otherwise, the estimated is outside of the range, which does not make sense to me. page 418, line 28: “if anything”. Please show only significant correlations on Fig. 8. page 419, lines 13-14: “the influence of the AAO is mostly restricted”. There are significant correlations in N. Benguela with AAO (see also Table 3). page 419, line 18: I wonder if cross-spectral analysis between up_wvelo and MEI would have been more appropriate. Here the correspondance between peaks in up_wvelo and ENSO frequencies is rather qualitative, no spectral analysis of MEI is actually made. page 419, lines 24-27: “In both regions...longer simulations”. This sentence is very vague, I wonder how useful it is at this point of the reading. page 419, line 25: “that is not... significant”. How is this determined? The vertical black lines on Fig. 9 do not make much sense to me. In my opinion, this analysis lacks estimates of 95% significance levels, which should be derived from red noise spectra (which have an analytical formulation - see e.g. Torrence and Compo 1998), allowing to determine whether all the peaks are significant. page 423, lines 4-6: please include values. page 424, line 1: "driven". page 424, lines 10-15: To me, these 2 sentences are redundant, except one deals with decadal time scales and the other with interannual scales. page 425, lines 2-3: 2.5 and 3.3 years could be part of ENSO, which is generally defined as a 2-to-7-year oscillation, with increased energy in the 3-to-4-year band. See my comment for page 419, line 18. page 425 lines 22-25: 41-3=38%? See page 423 lines 8-10. How did you compute 100%? In the previous version of the manuscript you indicated 44%.

References: Chelton, D. B., M. G. Schlax, M. H. Freilich, and R. F. Milliff, 2004: Satellite measurements reveal persistent small-scale features in ocean winds. Science, 303, 978–983, doi:10.1126/science.1091901. Colas, F., X. Capet, J. C. McWilliams, and A. F. Shchepetkin, 2008: 1997–1998 El Niño off Peru: A numerical study. Prog. Oceanogr., 79, 138–155 Estrade, P., P. Marchesiello, A. Colin De Verdière, and C. Roy, 2008: Cross-shelf structure of coastal upwelling: A two-dimensional extension of Ekman’s theory and a mechanism for inner shelf upwelling shut down. J. Mar. Res., 66, 589–616, doi:10.1357/002224008787536790 Kao, H.-Y., and J.-Y. Yu, 2009: Contrasting Eastern-Pacific and Central-Pacific types of ENSO. J. Clim., 22, 615-632. Kim, S.-T., W. Cai, F.-F. Jin, A. Santoso, L. Wu, E. Guilyardi, and S.-I. An, 2014: response of El Niño sea surface temperature variability to greenhouse warming. Nat. Clim. Change, 4, 786-790. Torrence, C., and G. P. Compo, 1998: A practical guide to wavelet analysis. Bull. Amer. Meteor. Soc., 79, 61–78.

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