

Interactive comment on “Trends in coastal upwelling intensity during the late 20th century” by N. Narayan et al.

Anonymous Referee #2

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The manuscript explores the hypothesis put forward by Bakun in 1990 linking the rise in temperatures due to the increase in anthropogenic greenhouse gases in the atmosphere and the increase in the intensity of upwelling at the western continental margins. Base on the prediction that continental temperature should rise more rapidly than ocean surface temperatures, the air pressure difference between continents and ocean would tend to decrease and intensify the equator-ward winds along the meridional coastlines. The wind forcing would then drive a more vigorous upwelling.

The authors analyse different proxies for upwelling in four locations at Western coasts in the subtropics. These proxies are wind-stress indicators and sea-surface temperature gradient perpendicular to the coast. Not all proxies for upwelling tell a coherent story but the authors tend to put more credibility in the sea-surface-temperature indi-

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cators, which can be interpreted as an increase in upwelling over the 20th century. A correlation analysis has been also applied to rule out the influence of quasi-oscillation patterns on the upwelling strength, leaving only the greenhouse gas forcing as sole plausible explanation for the increase in upwelling.

My impression of the manuscript is that, although the authors have made an effort to present a solid case, the main conclusion remains still quite speculative. I would be very carefully when interpreting the estimations of trends and correlations when the time series have been strongly smoothed (8 years) even in the case that the reduction of degrees of freedom and auto-correlation of the data has been taken into account (see my comment below, though). The reader would wonder why the data have been smoothed in the first place? The estimation of trends can be perfectly carried out with annual data, avoiding all the possible pitfalls that may be caused by the smoothing. The manuscripts just states that 'this was done to reduce the effect of interannual variability on the long term trend', but I think this step should be much more clearly justified. Also importantly, for many of the series, the evolution seems to be far from a linear trend. In some cases, the existence of statistically significant trends is due just because the initial or final years are far from the otherwise flat mean behaviour of the series. This is clearly the case in Peru for NCEP and ERA wind stress, Peru for HadISST, NW Africa for HadISST, California for COADS and NCEP.

The manuscript finds that the trends in wind-stress derived from ERA and NCEP are not consistent with those found in COADS. They argue that intrinsic deficiencies in the reanalysis data make them prefer the COADS data. It seems to me that all three data sets look very different, and this is worrying since they purportedly represent the same variable. The trends in the COAS wind-stress seem more consistent with the SST upwelling index from derived HadISST, except in Peru (so one case out of four). The Peru case could be explained by the ENSO phases and the strong smoothing (a reason to not smooth in my opinion). But the similarity between the COADS wind-stress and the HadISST up-welling indices for the other 3 cases is very questionable

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as well (Figure 3 and Figure 7). If I understood properly this is the key comparison to support the conclusions of the manuscript and it is so stated in the last paragraph before the conclusion section ('the major physical factor that controls upwelling is the meridionally directed wind stress'). But it seems that only the linear trends agree in 3 cases out of 4. Other than the linear trend, the series look very different. I guess that the correlation between the series in Fig 3 and 7, even after smoothing is close to non-existent. What could be the reason for the COADS series and the HadISST series to disagree at decadal time scales and agree in their linear trend? The HadISST upwelling index should be, in my opinion, closely driven by the COADS wind-stress even at interannual time scales if there is a real physical connection between both. Here, I would recommend to analyse the correlations between the COADS indices and the HadISST indices after different degrees of smoothing.

To reach the main conclusion we have to dismiss two data sets (NCEP and ERA), explain out one case among 4 (Peru) and ignore all variations other than the linear trend. All in all, the conclusions could be right but the evidence seems to me weak.

I would also have problems in accepting, from the evidence presented in the paper, that the ultimate driver of the variations in up-welling is anthropogenic global warming. Again this conclusion could be true, but I feel that a much more detailed analysis to rule out other external forcings would be necessary. If the mechanisms between the anthropogenic greenhouse effect and upwelling is the differential warming of the continents, we should be able to see the effect of other forcings that may have a similar fingerprints (solar variations, aerosols, volcanism), but that vary at shorter timescales. However, the upwelling indices in the four locations display rather different behaviour over the past decades. The agreement with the Baku hypothesis is then basically the upward linear trend in 3 of 4 cases, but could this be due to chance?

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