Biogeosciences Discuss., 10, C9541–C9558, 2014
www.biogeosciences-discuss.net/10/C9541/2014/

BGD

10, C9541–C9558, 2014

Interactive Comment

Interactive comment on "Surface circulation and upwelling patterns around Sri Lanka" *by* A. de Vos et al.

A. de Vos et al.

asha.devos@lincoln.oxon.org

Received and published: 6 May 2014

reviewers have made substantial comments and suggestions to improve the paper which we have addressed these below. We would like to thank the reviewers for their comments which are very constructive, resulting in a much improved manuscript.

Reviewer #1

Recommendation Major revision / resubmit. This paper discusses the oceanic circulation around Sri Lanka, and the processes that control the upwelling that generally occurs along Sri Lankan Southern coast. In particular, this paper suggests that wind driven off shore flow is not the only process that contributes to the upwelling there, but that interactions between the South Monsoon Current and topography also induce

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

divergence and upwelling. This discussion is motivated by the aggregation of blue whales in this region during the South East monsoon. While I think that the subject of the paper is interesting, I have several objections (detailed below) to its publication in Biogeosciences (at least in its current form):

1) does the topic of the paper really fits in this journal?;

2) I think that the paper does not convincingly demonstrate some of the mechanisms it hypothetizes

The suitability of the paper is a decision for the editor. We have added additional text to answer the reviewer questions and incorporated reviewer's suggestions.

3) I think that the paper could be re-organized in order to become clearer. I detail these issues below. I however found promise in this work, and encourage the authors to work on a revised version and re-submit it to this or another forum.

We have reorganised the paper including additional test/sections and modified some sections, we believed that this paper is much more clear and easy to read now.

General comments

Is Biogeosciences a good forum for this paper? This is really for the editor to answer this question.

The suitability of the paper is a decision for the editor. However, it should be noted that the study region is one of the least studied areas in the world with a huge paucity of field data. This paper addresses not only hydrodynamic modelling results, but also available observational (satellite) data to discusses the upwelling processes, seasonal changes in surface chlorophyll concentrations and Blue whales aggregation south of Sri Lanka in the revised version. We have modified the text to highlight the Blue Whales aggregation south of Sri Lanka.

The paper is motivated by the aggregation of Blue Whales south of Sri Lanka during

BGD
10, C9541–C9558, 2014

the NE monsoon, and discusses surface chlorophyll maps, but the actual topic of the paper are a description of the circulation around Sri Lanka and of the upwelling processes at its Southern Coast, i.e. this paper is mostly concerned with ocean dynamics and circulation. Probably that Journal of Geophysical Research-Oceans would be a more suitable forum for this paper. I found a lot of the discussions in this paper rather qualitative. For example, a lot of the discussions are based on snapshots of the simulation, so that one does not get a feeling of how these snapshots are representative of variability on the longer term (which is inherently due to the short duration of the simulations analysed here: one year, which is not enough in my opinion to discuss the seasonal cycle and its intra-seasonal variability).

We conducted ROMS simulation for two years (2010 and 2011), however, we agree that most of the modelling results presented in this paper are based on 2011 simulations, but in order to incorporate inter-annual variability, we have included observational results using longer period records, e.g. Figure 9 and observation from different years for Figure 3. We used model experimental results to evaluate possible variability during different months in SWM and inter-annually, including the role of SWC on the formation if the Sri Lanka dome.

Also the validations that are presented are very qualitative, while some data are available to perform more quantitative validations (e.g. gridded sea level data, TMI or AVHRR SST data, etc, see suggestions below). In addition, I found that a lot of the statements in the paper were not supported by the analyses that are presented.

We have included as many independent data as possible for validation. Note that gridded sea level data and SST have already have been indirectly included in the model through the HYCOM model which uses data assimilation

For example, does the sensitivity experiment with varying wind strength really demonstrate that the Sri Lanka dome is the effect of a recirculation in the Lee of the Island (see detailed comments)?

BGD
10, C9541–C9558, 2014

The sensitivity experiment with varying wind strength and without Island showed interaction between the SMC and the Island of Sri Lanka, our experiment with zero SWM winds did not produce dome as well as experiment without Island but with SWM winds also did not produced dome. Pl see our answers to detail comments

Is the idealized experiment with wind forcing of differing intensities on both side of Sri Lanka representative of actual wind fluctuations in nature, and how do you explain dynamically the results of that experiment (see detailed comments)?

Yes, idealized experiment with wind forcing of differing intensities on both side of Sri Lanka representative of actual wind fluctuations during NE monsoon, as example pl. see below wind fields during Dec and Jan (also see Figure 7a and 7 b), these figures shows that wind directions were nearly southward in December-January, during NE monsoon and some days winds are stronger on the western side and some days on the eastern side (see also Fig. REF1.2)

In general, I feel that more in-depth analyses are needed to back up the hypotheses that are presented in the paper. Finally, I also think that the paper could be re-arranged to ease its reading. For example the motivation of the paper (aggregation of blue whales) should be explained and detailed from the very start. Some of the theoretical background that is presented in the discussion section could be very useful in the introduction, so that the reader now what's the general idea of the paper from the beginning. My impression is that it would be easier to follow that way.

We agree with the reviewer suggestions. We have rearranged paper as suggested.

In general qualitative (snapshots of single events, rather than composites, or time series to get a better handle on the temporal variability) and descriptive, without dynamical explanations.

We had to use snapshots of single events as this one of the features we aims of the paper. The satellite (MODIS) imagery is a snap shots – if a composite of several images are made then the feature is not visible.

Detailed comments P14954: the abstract is long and not very synthetic. Try to shorten it.

We have shortened the abstract

L20-22: isn't always upwelling due to flow convergence / divergence?

We agree that there is some ambiguity in this statement – we have modified the text

L23-27: this is not obvious to me. Why would the intensification of the flow to the east shift the convergence to the west?

This is mainly due to continuity. Stronger currents on one side of the island driven by stronger winds have a higher momentum/inertia which shifts the convergence.

Last point: from the abstract, one wonders if this paper really belongs to Biogeosciences: it is mostly concerned with circulation modelling around Sri Lanka, with only colour of the sea and blue whales aggregation briefly mentioned.

We have included more details on blue whales aggregation around Sri Lanka,

P14955, L11-12: if you focus on the wind pattern, the SW monsoon is rather May to September and the NW from November to March.

Agree, but as mentioned in the text – the monsoon months can change year to year. We have used the definition which is generally listed in the literature.

L23-26: why? Upwelling occurs quite close to the equator in the Peru coastal upwelling system.

We agree. We have deleted this sentence.

P14956: L25- 27. Is it useful to mention SLP? And the tides on p P14957,

We have added sea level pressure variability in the study region as it is important.

The mean sea level pressure (SLP) in the North Indian region is maximum during December-January and minimum in June-July with seasonal range of about 5-10 hPa (Wijeratne, 2003), hence mean sea level is 5 cm lower in January compared to July, due to the inverse barometric effect.

Tides are important as we have used it as a validation. Getting the tides correct means that we have used a bathymetry that is close to reality, particularly in the shallow regions between India and Sri Lanka.

L13: 1 Sv=103 m3s-1.

We disagree: the unit of 1 Sverdrup is defined as 106 m3s-1, as listed in the manuscript.

P14960, L5-32: One important characteristic of the Northern Indian Ocean variability at intraseasonal (e.g. Vialard et al. GRL 2009), seasonal (e.g. McCreary et al. Prog. Oceanogr. 1993) and interannual timescales, is the propagation of signals around the Bay of Bengal and southern tip of India under the form of coastal Kelvin waves. This remote forcing for example contributes non negligibly to seasonal variations of the EICC. Similarly, the southern boundary of you domain is also strongly connected to the equatorial circulation. Because your regional domain solution is going to be strongly constrained by the HYCOM boundary conditions, one would like to see a few basic validations of that solution, for example to gridded sea level products.

We agree that this is an important point but as this paper is a first attempt at dealing with the circulation patterns, we believe that it outside the scope of this paper. We have addressed the inter-annual variability in the wind field (see response to Reviewer#2) where we compared the winds fields for different monsoon periods in different years. We have also examined the sea level variability around Sri Lanka using sea level measurements around the Island of Sri Lanka as shown on Fig. 1. These analyses did not reveal any propagating waves along the southern coast of Sri Lanka. The results are not shown here but have been

BGD

10, C9541–C9558, 2014

Interactive Comment

published as Wijeratne and Pattiaratchi, Sea Level Variability in Sri Lanka Waters, http://wcrp.ipsl.jussieu.fr/Workshops/SeaLevel/Posters/2_1_WijeratneRevised.pdf). Also as mentioned by the reviewer, Kelvin waves, if significant, would be included in the model forcing through HYCOM boundary forcing.

I would also like to hear more details about the surface forcing of you model: to you directly specify heat, momentum and freshwater fluxes from ERA-I, or does the model compute them using a bulk formula, with near-surface air temperature, humidity and winds and downward radiative fluxes specified?

We have specified heat and fresh water fluxes using ECMWF ERA data, the net heat flux at air-sea interface were estimated based on balance of incoming solar radiation, outgoing long wave radiation, sensible heat and the latent heat flux.

For net freshwater supply calculation, we simply estimated this based on precipitation and evaporation data with the river inputs ignored.

Another important issue is the model bathymetry. For example did you close the channel between India and Sri Lanka in your model? Available navigation charts in this region show extremely shallow waters that block the flow between India and Sri Lanka almost entirely.

We used GEBO 30 sec bathymetry data to generate model bathymetry. The channel between India and Sri Lanka was open. It is relatively shallow. We get the tides predicted by the model to be close to observations in this region which indicates that the bathymetry we used is close to reality

Section 2.3.1: I don't find this section very useful. A lot of the discussions in this paper are related to large and meso scale variability, so what is the need to discuss tides in details?

As mentioned above the tides are important as they reflect that the bathymetry used is correct.

BGD

10, C9541–C9558, 2014

Section 2.3.2, L8-9: not so clear from the figure I have: it is difficult to see the red and black vectors. Maybe plot less vectors for the model and bigger vectors. Otherwise, I think this is a nice validation. I don't think that ship drift climatologies (Mariano et al. 1995 USCG Report CG-D-34-95) would be very helpful. On the other hand, geostrophic currents derived from altimetry (e.g; the AVISO merged gridded product for absolute currents available from http://www.aviso.oceanobs.com/index.php?id=1271) may be another useful data source to validate your modelled circulation against (in particular because you could compare the observations and model for the same dates).

We have revised the figure and now the arrows are clearer.

Section 2.3.3: Colour of the sea pictures are only available when the sky is relatively free from clouds: AVHRR 4 km resolution SST images are hence probably also available for those dates (http://data.nodc.noaa.gov/pathfinder/Version5.2/). Using such images, you can provide a more quantitative assessment of the model (mean bias, correlation with observations, etc:) than you currently do through a single example.

The data we have shown is at 1 km resolution – the AVHRR 4 km images are not that clear

Section 3.1. In what is this a "result"? This section rather describes the monsoonal forcing of the model and these figures could have been incorporated in the introduction, for example, when discussing the wind patterns over Sri Lanka.

This is a result because they are data presented here for the first time. The coastal station data from Hambantota has not published before. ECMWF data may not be new but sets the scene for the rest of the paper and the section.

Section 3.2.1: since you discuss the influence of the circulation on the SCC field, you could maybe overlay the surface circulation from your model on the SCC climatology maps. I think that a lot of the circulation patterns that are discussed in the text are not really obvious from the SCC map (eg is it so easy to distinguish an open ocean

BGD
10, C9541–C9558, 2014

upwelling from the offshore advection of SCC from a costal upwelling region ?): the discussion here is very qualitative.

It should be highlighted that the SSC maps are based on climatology (means over several years) and hence there is no relevance to the instantaneous circulation maps in a given year. What we wanted to highlight is that the upwelling system is changing on the scales O (days) but the SSC figures from climatology shows a different (mean) picture which may be interpreted as classical wind driven Ekman dynamic.

L20: 2700 km??? The full extent of the Hovmoeller is about 700 km only! I guess that you mean 270 km.

We have changed the distance to 270 km.

P14966, L9-10: how do you compute this value exactly (over which depth and which latitude range do you integrate, at which longitude ?). Please be more accurate. The paper of Durand et al. (JGR, 2009) provides a state-of-the art description of alongshore currents at two points along the coast of Sri Lanka that may be useful here.

As described in the response to Reviewer#2, We wanted to highlight the results from Schott et al. (1994) who estimate 8 and 12 Sv for the SMC and NMC respectively, indicated that the transport during the south-west monsoon was weaker than those during the north-east monsoon. Our results indicate higher values during the south-west monsoon as would be expected under a stronger wind regime. It could also be due to the inter-annual variability – however – the comparison of the wind field indicates that there were no major changes in the wind fields in different years.

P14967, L5-6: well, the divergence of the surface flow also provides an indication of regions of upwelling. You could have added that information. Figure 11 also shows an obvious influence of topography, with a strong divergence of the surface circulation at the edge of the shelf that extends south of Sri Lanka, and upwelled waters that occupy most of the shelf. The details of the circulation that result in this distribution would have

BGD
10, C9541–C9558, 2014

been interesting to discuss.

We have changed the sentence to indicate that it is easier to identify regions of upwelling by examining SST distribution. We have also expanded the discussion.

L9-10: it would have been nice to show time series of the SST in selected boxes to illustrate those seasonal / sporadic SST upwellings. A situation map with the standard deviation of the SST would also maybe help locating the main upwelling areas.

L15-17: The mean wind pattern during that season is indeed not favourable to upwelling, but did you investigate the wind variability associated with the snapshots of figure 11?

The figure shows both NE and SW monsoon periods.

L20-22: you would need to perform a heat budget within the mixed layer to be able to demonstrate that lateral advection in the mixed layer also contributes to the cooling.

We believe that this is outside the scope of this paper.

P14968, L1-3: I don't really get the point here. Is your point that the wind does not change so much between July and August but that there is a significant change in the position of the upwelling? Or that the upwelling can be the result of the blocking of monsoon current by Sri Lanka (and that this current is not the result of local winds only)? Because another possible explanation is that, at the seasonal timescale, this upwelling is indeed wind-forced, but that it is modulated by meso-scale variability or by equatorial waves (intraseasonal equatorial Rossby waves or mixed gravity waves can significantly modulate the surface circulation close to 6_N).

It is not changes in the wind field but as is the strength (volume flux) of the SMC increases, the upwelling centre shifts to the east. There may be influence from different waves as suggested by the reviewer but we consider that a detailed analyses of these phenomena is outside the scope of this paper.

BGD

10, C9541–C9558, 2014

P14968, section 3.3: this intra-seasonal variability is interesting. Can it be related to the passage of a (remotely forced) coastal Kelvin Wave as those described in e.g. Vialard et al. (GRL, 2009)? Or is it related to local intra-seasonal wind variations (see several papers by Rao et al. on that topic (e.g. their 2006 paper in GRL and references therein). You could look at wind variations for that event and other events (for example using a composite analysis, or simple indices). P14968 bottom and 14969 top: a longer simulation would allow to characterize that type of variability better.

Please response above with regard to the Kelvin waves

Is it a hydrodynamic instability that creates rings over the mean current? The effect of wind-forced equatorial waves? Of coastal Kelvin waves coming from the Bay of Bengal?

Please see comment above with regard to the Kelvin waves in the Bay of Bengal

P14969, L10-21: I quite like the sensitivity experiments here, but the type of wind stress perturbations that are applied are poorly justified. The only season when mean wind flows southward along both coasts is the winter monsoon. And even for that season, can you prove that the main pattern of wind perturbations correspond to southward winds of varying intensities along both coasts? Finally, the reason why the upwelling sets on the side of the weakest wind perturbation is not explained here (and the explanation is not obvious to me).

Yes – we can prove that wind perturbations correspond to southward winds of varying intensities along both coasts see Fig. REF1.2

P14970, L1-17: in what does this experiment demonstrate that the SL dome is due to a recirculation of the current in the lee of Sri Lanka? A more convincing experiment would be to suppress Sri Lanka and see if the recirculation disappears. But the current experiment can be interpreted in many different ways.

Yes we have done this – we have removed the Island of Sri Lanka to show that the flow pattern disappears (see Fig. REF1.1)

P14970, L24-25: you show comparisons of the model SST with SCC but not with observed SST, so you can't really say that here.

We agree and have changed the sentence

P14971, L1-8: you speak about the model as if it was ground truth, while it is not even validated quantitatively in terms of the transports it simulates.

Please see response to rev#2

P14971 bottom and top of 14972: many readers won't be familiar with those scaling arguments. If you want to use them, you need to introduce and explain them in more details. Also explain how you chose the scaling values U, L and Kh for both seasons. And again, the SL dome is quite possibly created by a recirculation of SMC in the lee of Sri Lanka, but I don't think that your experiments demonstrate it.

Thank you for the suggestion – we have now introduced the scaling arguments in the Introduction.

014973, top: it would be more convincing to show this for an average over the entire SWM rather than just a snapshot. I guess that the point here is that classical coastal upwelling dynamics (alongshore wind stress and offshore transport) are not the only source of divergence and upwelling, but that the interaction between an incoming current (the SMC) and the island is as well.

We agree however as these events occur over a few days, the mean over the whole SWM will not show the feature.

This point is actually further explained on page 14974: I think it would have been nice to state all of this motivation as an introduction to the study, because it would help the reader to understand your hypotheses better all along the text.

Thank you for the suggestion – we have now done this

BGD

10, C9541–C9558, 2014

Interactive Comment

P14975: L11: you did not demonstrate that the transport was more realistic than previous estimates. L12-15: I do not think that you have demonstrated this either (although I agree that this explanation sounds sensible).

The previous estimates of transport indicated higher transport during the (weaker) north-east monsoon period which intuitively does not appear to be correct. The winds during the south-west monsoon are much stronger and thus would expect the transport to be higher. Hence, we believe that the predicted estimates are more realistic. We have also examined the wind fields for a few years to demonstrate that there is no strong inter-annual variability in the wind field (see also response to Reviewer#2).

L23-26: it is not obvious to me that your idealized experiments correspond to actual patterns of wind variations on both side of the Island.

We have included (see Fig. REF1.2) snapshots of wind field around the island is shows the differences in the wind speeds on either side of the island.

Figures The axis labels on almost all figures are too small to be read properly. Try to be more consistent in your figure choices (e.g. use same layout for figs 7 and 8, use same months for figs 7_8 and 10).

We have revised the figure as recommended and used the same months for Figs 7, 8 and 10.

Fig3 can be improved (bigger label on axes, short title that gives variable and data, e.g. "SST, 19th June 2013"

We have revised the figure as recommended.

Fig4: the caption says "Roms currents in blue" but they are in black. It is quite difficult to judge the quality of ROMS simulation from this plot.

We have changed the caption to say ROMS currents are shown in black.

Fig 5: add some bathymetry contours on figure 5b (some current features are obviously topography-driven).

We have overlayed bathymetry into Fig. 5b.

Fig6: Are the red curves and dots needed on that figure? Say that 0_ is northward.

We have indicated that 0 is northward in the caption. The red dots shown the daily variability

Fig 9: indicate the averaging box for that plot one of the other figures (e.g. fig 8).

We have done this – the box is shown on Figure 8.

Figs 7, 8, 10: it could be better to make consistent choices for the months displayed in all those figures, and maybe to plot SCC & the modelled circulation on the same plot. If just plot 4 panels (say SE monsoon / NW monsoon and transitions) you can have all the info (wind forcing / modelled circulation + observed SCC) on the same page.

We have revised the figures to be bi-monthly on Figures 7, 8 and 10.

Fig 10: you should plot the actual model coastline on this plot, not the coastline from your visualization software.

Our model is relatively high resolution (<2 km) and the model coastline and topographic coastline are almost same (pl see below). See Fig. REF1.3

Fig 11: again, the labels are small, and you should maybe plot one vector in 4 and bigger vectors, for an improved readability.

We agree and have changed the caption and have revised the figure

Fig 16: there must be a mistake in the caption. I imagine that the simulation with the Coriolis effect included is the one on the right, not on the left.

We agree and have changed the caption

Please also note the supplement to this comment:

BGD

10, C9541–C9558, 2014

Interactive Comment

http://www.biogeosciences-discuss.net/10/C9541/2014/bgd-10-C9541-2014-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., 10, 14953, 2013.

BGD
10, C9541–C9558, 2014

Interactive Comment

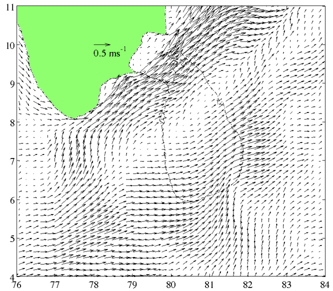

Fig. REF1.1 – Idealised simulation with the Island of Sri Lanka removed. Note the absence of Sri Lanka dome, downstream (east) of the Island.

Fig. 1.

BGD

10, C9541–C9558, 2014

Interactive Comment

BGD
10, C9541–C9558, 2014

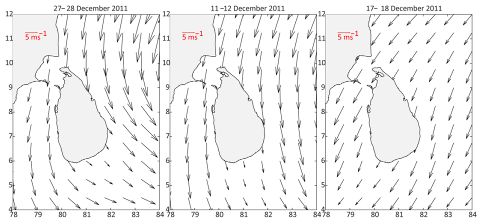

Fig. REF1.2 – Demonstrating different wind strengths (northerly component) along the east and west coasts of Sri Lanka: (a) winds stronger along the west coast; (b) winds equal on both sides of the island; and (c) winds stronger along the east coast;

Fig. 2.

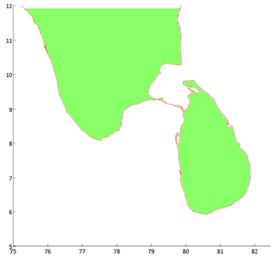

Fig. REF1.3 – The model mask (in green) and the actual shoreline (in red) are coincident.

Fig. 3.