The authors' comments on Dr S. Clemens' review have been inserted in the text. They are written in *italics*.

Review: Bassinot et. al., Holocene evolution of summer winds and marine productivity in the tropical Indian Ocean in response to insolation forcing: data_model comparison.

This is an excellent paper, appropriate to publication in Climates of the Past. It is among the best data_model comparison papers I've read. The authors clearly make the case for entirely out_of_phase responses among two proxy records of summer_monsoon_induced upwelling and offer clear and well_reasoned explanations for the underlying mechanisms, derived from coupled AOGCM_ecological model simulations. I recommend publication with minor modification at the discretion of the authors.

Below I offer a number of comments and suggestions that authors may wish to consider in revisions. The more extensive comments center on two issues: (1) the choice of an insolation forcing curve and (2) the consequent implication of that choice on assessing the timing of the Holocene (orbital_scale) monsoon maximum relative to insolation forcing. I try to make the case that there is a measurable 3 to 5 kyr lag in the Holocene response to insolation forcing and that this implies mechanisms beyond a direct and sole response of the summer monsoon to northern hemisphere summer insolation forcing. This line of discussion in no way disputes the overall thrust/findings of the work but does reflect an ongoing debate in the community regarding the timing of the summer monsoon response at the orbital time scale (including the Holocene and extending into the late Pleistocene).

With Regard,
Steve Clemens

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Line by Line comments:

P487, 11. The circulation at the tip of India also is affected by.... (why 'also'?). 'Also' implies the East African current is fresh as well.

« Also » has been removed in the revised version.

P488, 19_21. Seems unlikely that such a local factor as salinity at the southern tip of India would drive the onset of the large_scale summer monsoon. One might make the case that salinity is more of a response to hydrological processes, rather than a driver.

We never intended to say that monsoon-related changes in surface salinity at the southern tip of India could drive the onset of the large scale summer monsoon. Clearly, the paragraph was misleading if that is what the readers understand.

After briefly describing the seasonal circulation changes at the southern tip of India, our intention in this paragraph was to remind the readers that these circulation changes affect the upper water stratification. Then, at the end of the paragraph, we pointed out that model results indicate that the upper water stratification has a potential impact on the timing of the progressive warming of the surface ocean (in response to insolation forcing) and has, therefore, an impact on the setting of the summer monsoon.

In order to make this more clear, in the revised manuscript we added a sentence in the middle of the paragraph to strengthen the stratification aspect (« This affects the characteristics of the mixed layer depth by stratifying the surface ocean »), and we removed « local » to change it by « ,northern Indian Ocean » in the last sentence of the paragraph (to emphasize the fact that stratification should not only be considered at the tip of India).

P 490, 26_27. ...the growth of height foraminifer species... unclear, text error?

Wrong spelling. « Height » should read « eight ». It has been corrected in the revised version.

P492, 5_7. On orbital time scales, productivity records do not indicate that the strongest summer winds occurred during times when perihelion was aligned with summer solstice. A recent synthesis of 18 orbital_scale records (~300 kyrs in length) indicate that the strongest winds occurred ~50° after ice minima at the precession band (~125° after precession minima) [Clemens et al. , 2010] and are inconsistent with a direct/sole northern hemisphere summer insolation forcing mechanism. This 125° phase is inconsistent with the current interpretation of cave speleothem d18O as being forced only by the strength of summer monsoon precipitation, a strongly contested interpretation [Clemens et al., 2010; Dayem et al., 2010; LeGrande and Schmidt, 2009; Maher, 2008; Hu et al. 2008]. The orbital scale timing issue has been addressed in a number of past and recent publications [Clemens et al., 2010 ; Clemens et al., 1991 ; Clemens and Prell, 2003; 2007; Liu et al., 2006 ; Ruddiman 2006 ; Wang et al., 2008 ; Weber and Tuenter, 2011 ; Ziegler et al., 2010].

The sentence has been modified in order to acknowledge the fact that paleo-data indicate that a phase-lag exists between insolation forcing (precession minima, or maxima in mean summer (JJAS) insolation) and the maxima in wind and upwelling intensity.

This is the new end of the paragraph :

"On orbital timescales, productivity records obtained in these studies clearly indicate that the strongest summer winds occurred in interglacial times, and lagged the times during which perihelion was aligned with the summer solstice. Within the Holocene, the SW monsoon reached

a peak between ~10 and 8 ka (Naidu and Malgrem, 1995, 1996; Gupta et al., 2003), in good accordance with independent paleo-monsoon records such as the speleothem oxygen series from the Qunf and Hoti caves, in Oman (Fleitmann et al., 2003). A recent synthesis of eighteen orbital-scale records (~300 kyrs in length) even suggests that the phase lags relative to insolation may be large, with the strongest winds occurring ~50° after ice minima at the precession band and, therefore, ~125° after precession minima (Clemens et al., 2010).

P496, 9_12. These rates are consistent with those measured by Honjo et al [1996] and Clemens [1998] in Arabian Sea sediment traps.

The consistency with trap data has been indicated by inserting a new sentence in the manuscript.

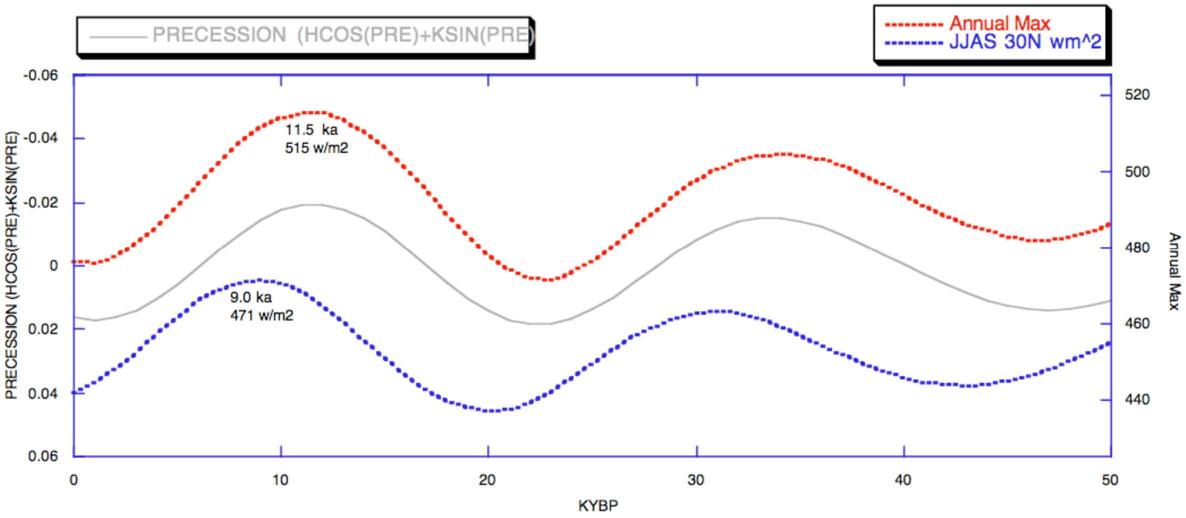
P498, 11_13 (global comment). It is unclear as to why JJAS 30°N is chosen as the forcing against which to compare records of G. bulloides upwelling strength and timing at the orbital time scale. The JJAS 30°N curve is the same as that calculated for August 14 (omega at 143° from the vernal point). This JJAS (August 14) radiation curve is never the strongest radiative forcing at 30°N as is seen from the figure below spanning the past 50 ka. In this figure, the dashed red curve shows the absolute annual maximum radiation at 30°N; it is not restricted to any particular omega value (timing of the date of perihelion). The maximum annual radiation forcing at 30°N always occurs either in May, June, or July as indicated in the figure below spanning the past 400 ka (after Clemens et al., [2010]). All of the 30°N insolation maxima (and minima) at the precessional_scale are associated with insolation at times of June perihelion (never august perihelion).

To the extent that modern Arabian Sea winds are already southwesterly in May, it is evident that times of high insolation during May perihelion are equally as important as times of high insolation during July perihelion with regard to the strength and duration of summer monsoons. Beyond this, the JJAS curve is always 30 to 50 w/m2 weaker than the annual max curve. An intuitive way to look at this issue is to pose the following question, comparing the monsoon response to insolation at 9 ka and 11.5 ka as follows. Should the summer monsoon be stronger at 9 ka (based on a radiation forcing of 471 w/m2 as on the blue dashed JJAS curve) or at 11.5 Ka (based on a radiation forcing of 515 w/m2 as on the red dashed Annual Max curve)?

In summary, the JJAS curve is not a particularly useful forcing curve with regard to the strength or timing of strong summer monsoons relative to external insolation forcing ; it never captures the strongest forcing. The authors might consider using the absolute maximum curve in this regard. Note that the timing of the precession_scale highs and lows of the absolute max insolation curve is the same as that of the precession curve (grey solid curve). Orbital (precession) extremes do reflect the max and minimum radiation forcings.

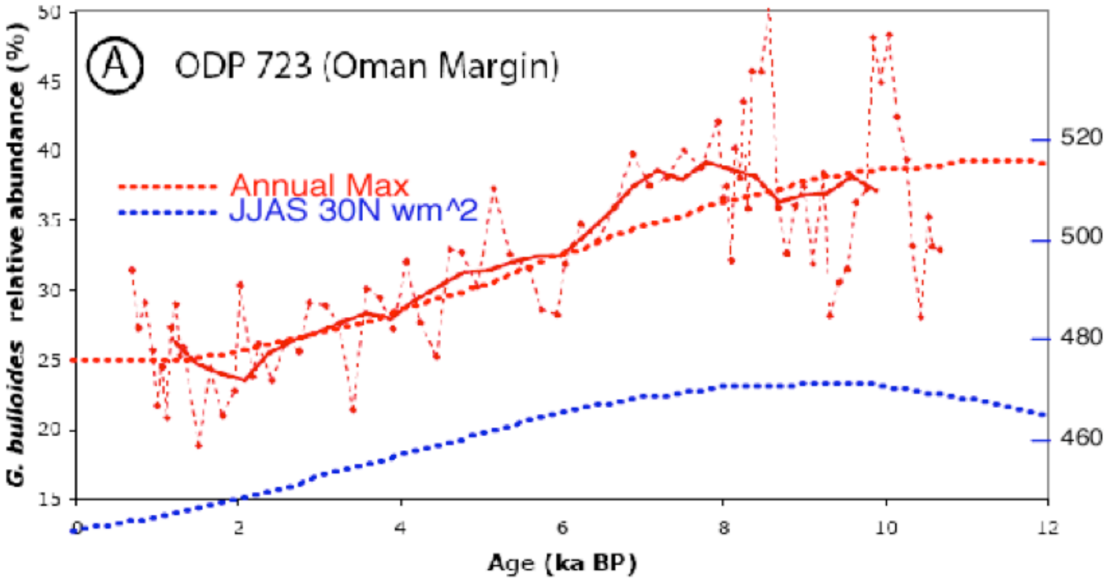
We agree with the reviewer on the fact that the 30°N summer (JJAS) insolation curve might not be the best forcing curve for the Indian monsoon. But discussing the best *choice and proposing alternative curves (i.e. to capture the strongest forcing) is beyond the scope of this narrowly focused paper. Our only objective with this work is to look at the long-term evolution of two upwelling cells in the Indian Ocean and compare the paleo-data with coupled OA 3D model outputs (IPSL-CM4 model). Time slices available are 9ka and 6ka BP (+control run). Given the lack of transient model outputs to which we could compare our paleo-data, we did not attempt to discuss lead/lag issues. In this context, the summer insolation at 30°N simply represents a convenient « reference » curve, which makes it possible to visualize the long-term evolution of northern hemisphere insolation across the Holocene. In support for this choice, we took into consideration the fact that the Indian monsoon maximum intensity occurs today during July–August, so that it does make sense to consider that its amplification could be favored when low latitude (near tropical) insolation forcing is in phase with this maximum, as it is the case at near 9 kyr BP for the JJAS insolation curve.*

It should be added that, as far as the comparison between data and model is concerned, the choice of this reference curve has no impact on our conclusions since the IPSL-CM4 model is forced by the daily, geographically gridded insolation at the top of the atmosphere and not by a specific, averaged insolation curve.



P498, 19_21. On the basis of the Annual Max forcing curve shown below with the manuscrit data, the increase in productivity since ~1.5 ka does not require any ad_hoc explanation as to why it diverges (with the JJAS 30°N insolation curve). Both the G. bulloides and the forcing curve flatten at the same time. I take this is evidence that the annual max curve is more appropriate.

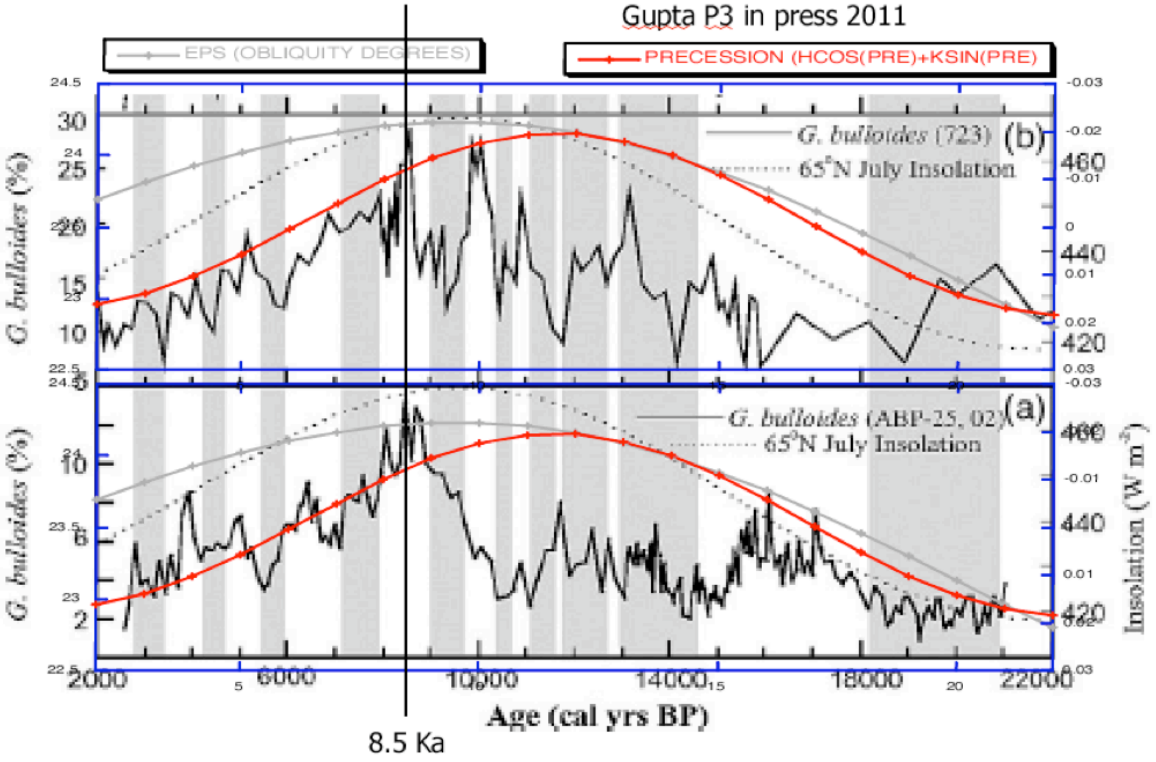
We agree that using a different reference (forcing) curve might provide alternate (better) explanations for the increase in productivity since 1.5 ka (although we note that the Annual Max Forcing shows a rather constant value since 2ka, and not a clear increase). But, once again, discussing phase-lags and the best "reference curve" is beyond the scope of the present paper, which only deals with the long-term, opposite behavior of the two upwelling cells, and the comparison with OA model outputs at 9, 6 and 0 ka. As far as the recent Oman productivity is concerned (for which no data-model comparison is possible), we just provided in the text the possible explanations given by the authors who published and discussed the paleodata in the first place (i.e. Anderson et al., 2010). Discussing new possible explanations for the recent (last 1.5 kyr) increase in productivity would be beyond the main scope of our paper.



P499, 1_4. On the basis figure 5A, the summer monsoon maximum is at either 7.5 Ka (smoothed) or 8.5 Ka (max unsmoothed). Including other very well_dated G. bulloides records from the Northern Arabian Sea (Gupta 2011 in press, P3 – figure below) one might set the date at 8.5 Ka, 3,000 years after the maximum radiation forcing at 11.5 Ka. However, an 8.5 Ka max falls 4,500 years after the timing the Indo_Asian monsoon precipitation maximum indicated by the 280,000 year time_dependent modeling using insolation only forcing [Kutzbach et al. , 2007]. _d18O records from speleothems [Hu et al., 2008] point to an even later maximum response at 6 Ka (a lag of 5.5 ka after the 11.5 ka absolute max). My effort here is to convince the authors that a real lag of 3_5 kyrs exists in the Holocene data and, on this basis, to refrain from stating that the summer monsoon is a direct response to northern hemisphere summer insolation forcing, a point made in Clemens et al [2010] relative to interpretation of cave d18O.

Proxies that are not in phase with orbital extremes (maxima or minima) to within a few degrees likely have more than one process influencing their timing. This may seem like a relatively minor point when considering the large difference the authors are really trying to address (timing differences between the northern Arabian Sea and southern tip of India) but it is important and both data and models have identified other processes, in addition to direct insolation forcing, as important [Liu et al, 2006].

We agree with the reviewer that there are important aspects that are not covered by our work. Because we do not want to mislead the readers, we clearly state in the revised version of the manuscript that « timing »(lead/lag) is not a major issue of our work, which is mainly devoted to comparing the long-term trends over the Holocene. Thus, we have also re-phrased several sentences in order to avoid giving the reader the impression that direct insolation forcing is considered the only mechanism at play (for instance, we have removed « direct response » in P503, 13_15 and 23_26 – see below). We are fully aware that internal feedbacks (such as changes in ocean stratification – see our comments above, snow ice cover, etc..) play an important role, beside the main insolation forcing.



P499 15_22. Not clear that possibilities (1) and (2) are really distinct from one another. Please clarify.

What we wanted to say is that modern data clearly indicate that seasonal changes in productivity are associated to upwelling intensity and, thus, wind forcing at both the ODP Site 723 and MD77-191. However, on the long (orbital) timescale, we cannot be sure that the data should be necessarily interpret as reflecting an opposite evolution of summer upwelling intensity at these two sites (option 2). There could be a possibility that the long-term (orbital) productivity signal at core MD77-191 (which has been given less attention in the litterature than the Oman system) was not monsoon/upwelling related (option 1). Thus, using model outputs, we show, first, that productivity changes at the orbital timescale at the location of Site 723 AND core MD77-191 remained consistent with a summer monsoon/upwelling interpretation… Then, we interpret the data in terms of upwelling intensity (wind) changes.

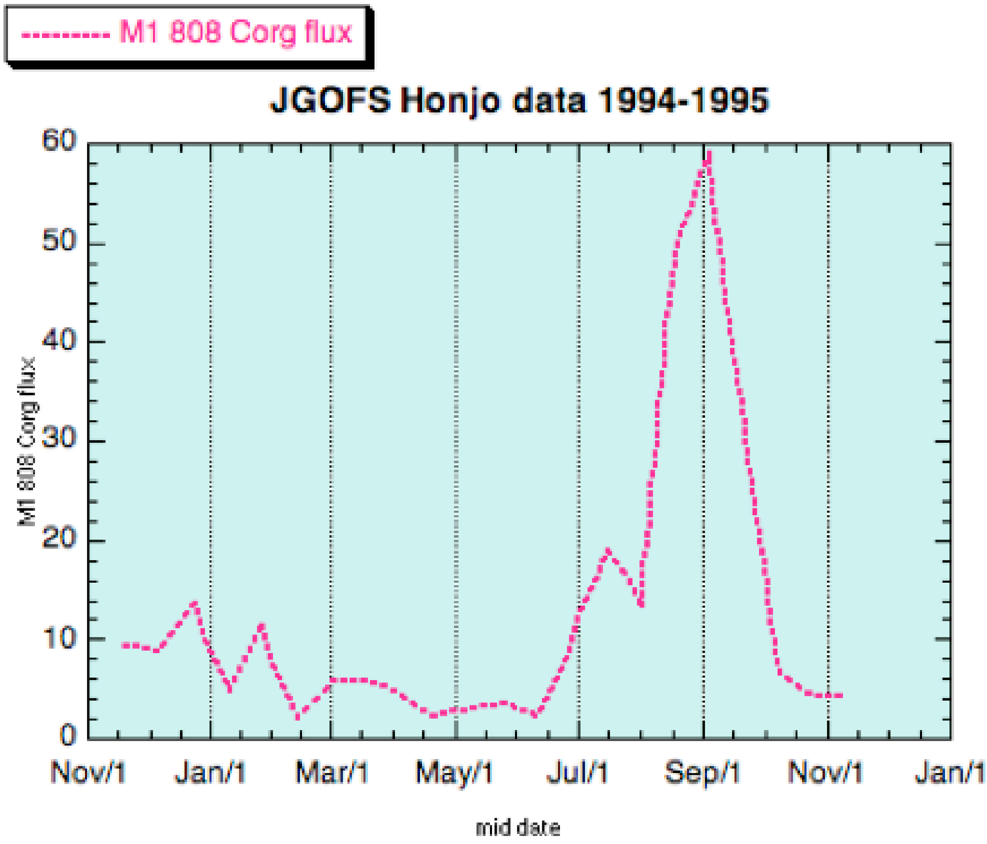
We hope that our point is more clearly explained in the revised version.

P501, 1_5 and figure 7A. Why is export production shown in Figure 7 if the model includes the relative % of G. bulloides? Why not show flux of G. bulloides? In either case, Northern Arabian Sea trap data exists for comparison as shown below [Honjo et al., 1999; Lee et al., 1998]. I do not recall if/where the G. bulloides data are published but they can be made available to the authors. The strongly bimodal northern Arabian Sea response in the PISCES model (fig 7b) where the March peak is larger than the Aug_Sept peak is not supported by trap data which shows the vast majority of Corg (808 mbsl trap depth) and bulloides (3141 mbsl trap depth) export in Aug.

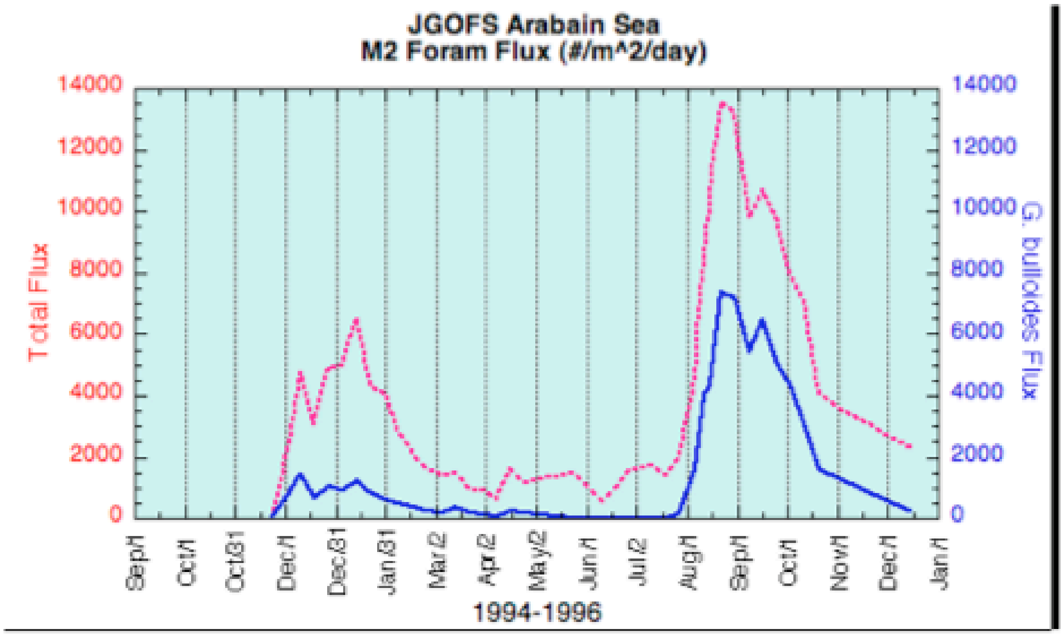
The foraminifer model being directly forced by the seasonal outputs of the PISCES model, we believe it is more informative to show the PISCES results rather than the modeled G. bulloides *fluxes. Beside, the comparison of core data and modelled* G. bulloides *relative abundances is provided in Table 2 and discussed in the paper.*

In addition, performing a direct comparison of trap data with foraminifer model outputs would probably end up putting too much emphasis on potential discrepancies between the two (i.e. flux differences, slight differences in seasonality patterns). This would divert the paper from its main goal, which is to focus on the comparison of the relative, long-term changes over the last 9 kyr. Yet, it is clear that future works will have to go into more details and comparisons. The present quality of simulations is just a first step and will require further improvements.

As far as seasonality is concerned, the main reason for the discrepancy between PISCES results and SeaWiffs climatology data is linked to the coarse resolution of both the atmosphere (LMDZ) and ocean (OPA) general circulation models that force the biogeochemical model, and that precludes a good representation of coastal upwelling zones. It should be noted (and this is stressed out in the text, in the presentation of the models), that the high-resolution PISCES model (0.5°), which is forced using re-analyses, provide much better results.



--------- M1 808 Corg flux
JGOFS Honjo data 1994-1995
M1 808 Corg flux
60
50
40
30
20
10
0
Nov/1
Jan/1
Mar/1
May/1
Jul/1
Sep/1
Nov/1
Jan/1
mid date



JGOFS Arabain Sea
M2 Foram Flux (#/m^2/day)
Total Flux
14000
12000
10000
8000
6000
4000
2000
0
G. bulloides Flux
14000
12000
10000
8000
6000
4000
2000
0
Sep/1
Oct/1
Oct/31
Dec/1
Dec/31
Jan/31
Mar/2
Apr/2
May/2
Jun/1
Jul/2
Aug/1
Sep/1
Oct/1
Nov/1
Dec/1
Jan/1
1994-1996

P502, 9 : Why « Somalian margin » - do you mean « Oman » margin ?

Yes. It was a mistake. Somalian should indeed read « Oman ». Text has been modified.

P503, 8_10. In the broadest sense, yes, in reality there is a significant lag implicating more than one (insolation) forcing mechanism.

See previous answers above

P503, 13_15 and 23_26. Uncomfortable with the 'direct response to insolation forcing' phrasing. The system is more complex than that at orbital time scales, especially in the late Pleistocene.

« Direct » has been removed. Again, as timing (lead/lag) is not the major issue of this paper, we want to avoid using words or sentences that could be misinterpreted.

Figures. Please plot core locations directly on Figures 1, 2, and 6 (making figure 3 unnecessary).

Figure 3 has been removed. Locations of cores have been put directly on Figures 1 and 6 (re-labelled Fig 5).

This is an excellent paper, a pleasure to read.

Steve Clemens

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