

Interactive comment on “Seasonality in Planktic Foraminifera of the Central California Coastal Upwelling Region” by C. V. Davis et al.

Anonymous Referee #3

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The authors present an interesting dataset, monitoring the abundance of planktonic foraminifera off California over two years with repeated plankton tow surveys. Such data are immensely useful in constraining the primary habitat of the studied species and their relationship with environmental parameters. In this way, the data represent a useful contribution to the field, potentially suitable for publication in BG. However, the analysis of the data as presented in the manuscript is conceptually flawed and/or requires clarification.

First, in most of the analyses it is not clear whether the authors use relative abundances (percentages) or counts and if they use counts, it is not clear how these have been scaled to concentration (standing stock). The hauls were vertical so it should be possible to estimate the volume of water that was filtered during each haul and convert the data accordingly. This would also allow the authors to compare the observed

C1

standing stock with other plankton tow data. At any rate, plotting species percentages along with upwelling index as is done in Figure 4 bears the danger of making a false impression of causality. The authors must realize that a “bloom” of a species, whose abundance is expressed as percentage of the total assemblage could in fact reflect the period of the lowest standing stock of that species (as long as the standing stock of the other species is reduced even more). If the authors want to support their claim that a certain species is associated with upwelling then they should show that that species had a higher standing stock at times of upwelling, not a higher relative abundance.

Second, the multivariate data analysis if in my opinion based on inappropriate methods. It seems to be based on counts (log-transformed), which is fine, but surely these have to be normalized to volume or else they are not really informative? Next, I am puzzled by the meaning of the PCA biplots in Figure 7. What exactly were the input variables in each analyses? What does the % variance explained refer to? Since each plot represents one species, the % variance explained cannot refer to that species, as its analysis could not have possibly contained a second axis (what would it be made of?). I believe the authors should either ask how one can explain the assemblage composition by a combination of environmental variables or they may ask how to explain the standing stock of one particular species by environmental variables. The former would require a constrained ordination, the latter a generalized linear model. Both analyses permit post-hoc parameter selection and the GLM also allows to test for interactive effects. I also question the use of a variable termed lunar phase in the PCA biplots. A periodic variable cannot be analyzed in a linear manner, because such analysis does not correctly consider the periodic nature of this variable (day 1 is only 1 day apart from day 28).

Third, the detection of lunar periodicity as described in 4.2.1 is in my opinion flawed. These data cannot provide any support for the presence of lunar periodicity, because they are not derived from successive days within one lunation (and are not scaled...). Instead, they reflect the fact that one or two of the many sampling campaigns yielded

C2

unusually high numbers of a given species. These high values produce an impression of a peak, which happens to occur around full moon. To substantiate a claim for the existence of lunar cycle, the authors would have to prove that the sampling situation with unusually high standing stock does not represent a situation with unique hydrography, driving the standing stock high irrespective of lunar phase. This would be hard, because the authors have shown in their prior analyses that the standing stock of the analyzed species can be explained well by a combination of environmental parameters. So the high standing stock samples must reflect a unique oceanographic situation.

Finally, I urge the authors to make all data publicly available upon publication.

Minor points:

Next to missing on vertical resolution (which likely is not a big problem), the data are affected by the choice of sampling the > 0,150 mm. This means the counts excludes not only juveniles but also adult shells of small species.

Section 3 should be headed "Results"

Taxonomy is not up to date, generic names do not reflect phylogenetic relationships: *Turborotalita quinqueloba*, *Globorotaloides hexagonus*, *Globigerinella calida*, *Globigerina bulloides*

4.1.1 It seems strange to frame the discussion of seasonality in sediment traps by isotopically derived temperatures? The sediment trap data provide direct observations on the seasonality of the flux; there is no need to involve further surrogate variables. If a species has higher flux in winter than in summer then it is a winter species.

It is unfortunate that the discussion of seasonality and its potential driving factors does not reflect on the review by Jonkers and Kucera (2015). This review presents specific predictions on when during the year the peak fluxes (and thus presumably peak standing stocks) of the four species should occur in the studied region and how strong these peaks should be.

C3

Page 13 Line 8: the authors should explain somewhere that they are using the concept of *N. pachyderma* and *N. incompta* as introduced by Darling et al. (2006).

Conclusion of 4.1.1 on *N. pachyderma* applies specifically to the studied region. *N. pachyderma* is strongly linked to upwelling (rather than seasonal incursion of cold waters) off Benguela and off Somalia (Ufkes and Zacharias 1993; Ivanova et al., 1993).

4.1.3 Line 3: could you please explain how was the significance of the difference established?

4.1.4 The relationship between calcification and carbonate chemistry is not that simple. There are data indicating opposite trends (more calcification in more undersaturated waters) and there is increasing evidence (see review in Weinkauf et al., 2016) that calcification reflects factors other than carbonate chemistry.

4.3. This discussion is only valid, if all *N. pachyderma* genetic types behave ecologically identically. This is highly unlikely, considering the results presented by Darling et al. (2006, 2007).

Table 2: are p-values corrected for multiple hypotheses testing (see Bonferroni correction)? What has been correlated with the environmental variables? Absolute abundance or percentage? Is the use of linear correlation justified? Are the variables normally distributed?

Figure 2: could the authors indicate the position of the actual CTD casts?

References:

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C4

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