

Reviewer comments in black, author replies in *italic blue*.

Very important summary of a major and largely successful effort to compile and make data available on plankton functional types. For the most part, well organized and well written, a useful introduction to and synthesis of the other 11 papers / datasets. I would have liked a summary / recommendations paragraph. Perhaps a few sentences on the successes: steps forward from prior situation, amount and types of data compiled, community response, utility of the MAREDAT data sets for green ocean modeling. Then perhaps a sentence or two on the weaknesses: fundamental lack of open ocean data; lack of compliance or cooperation by data holders - this no doubt varied within the community for each PFT - but did it limit the success for some PFT? Next steps: how biological oceanographers could collect samples and document data to improve utility for such compilation efforts; priorities for data gathering (or data rescue); which PFT groups most need improvement; time scale and processes for next iterations? I did not get a sense of the overall quality of the product - quite good I suspect - or of the success of the data gathering effort.

*We have added information on uncertainty from conversion factors, sampling bias towards the coastal ocean, the Northern Hemisphere, spring and summer. We have also added the time scale for the next iterations (MAREDAT2015), and the total number of data compiled (436,887 biomass measurements) throughout the text. A Summary section has been added after the Discussion section where some of the referee's more general suggestions have been added – those not explicitly addressed already in the text.*

Specific comments

Page 3, line 5: “products with coverage of all biological components of the global ocean”. I think the authors mean all planktonic components? ‘All biological components’ implies a larger survey of fish and other larger organisms?

*We have added the qualifier “, in due course,” to this sentence, to stress that it is indeed our eventual goal to include fish and other larger organisms, and that as the subsequent sentence points out, this is only the first step towards that goal.*

Page 3, line 18: “More data will be needed” From where? New data? Existing data but unavailable?

*Both new and existing data will be important to fill existing gaps, we therefor added :“Some of this additional data already exists in the literature, but was not available for inclusion in MAREDAT, so future efforts will be helped both by further archiving of historical data and future sampling at new locations.”*

Page 4, lines 3-8: Confusion arises here. Report of a 2009 kick-off meeting by Le Quere, and Pesant 2009. But in the next sentence, a quote evidently from the meeting cites Le Quere et al. 2005). Perhaps it should say (roughly) the meeting endorsed the earlier goals specified by LeQuere “key plankton ...”.

*The 2005 paper defined the key PFTs but set no data synthesis goal, which was decided on in the 2009 meeting. To clarify this we added “the previously defined” before the quote.*

Page 4, line 28: “have been included in the data products in this special issue. Nanophytoplankton” Should read ‘included in other data products in this special issue. For example, nanoplankton ...’?

*We have rearranged these two sentences to read: “Where the term nanophytoplankton is used in the literature it often includes members of PFTs that have been included in the data products in this special issue. Therefor, we excluded this PFT from the current collection of data, even though nanophytoplankton represent a significant part of the phytoplankton biomass.”*

Page 5, lines 4-7: “However, the most reliable way to prevent double counting and achieve a consistent dataset would be to measure the biomass of all phytoplankton groups in the same samples in transects that cross all ocean basins.” Do the authors recommend a biomass or carbon measurement of the total biomass in a given water

or net sample? Or, systematic biomass (and carbon) measurements of all PFT groups, from which they can assemble better composite measurements? If they mean the latter, but uncertainty about nanoplankton definition and identification remains, how will subsequent measurements improve the biomass estimates for the nanoplankton PFT?

*We have added “and cell size” and “distinct” after “biomass” to clarify that if all groups that are present are identified and filed separately, then subsequent data synthesis efforts can use their own definition, without excluding the possibility that this definition might change in future work.*

Page 5, line 13: Here, the “sum of all relevant phyla” represents a way to check the size overlap issues among the zooplankton PFTs? But, can groups actually do that check reliably?

*This was changed from “making an accurate assessment of the total biomass difficult”, to: “making an accurate assessment of the total macrozooplankton biomass difficult, and thus leading to a risk of underestimation, as opposed to the risk of double counting in the case of foraminifera and pteropods”.*

Page 5, line 28: “datasets that are filled by interpolation”. The authors mean that WOA achieves data coverage across all grid points by interpolation? Using this more precise description will help the reader later understand why the authors have not applied interpolation to the PFT data.

*This was changed to “achieve data coverage across all grid points by interpolation”.*

Page 6, line 2: Reference the CARINA or SOCAT products in ESSD?

*We added: “(e.g. the CARINA special issue in ESSD [http://www.earth-syst-sci-data.net/special\\_issue2.html](http://www.earth-syst-sci-data.net/special_issue2.html))”*

Page 6, line 3: “all biological components of the global ocean ecosystem”. Again, this phrase appears to imply a greater scope (fish, etc.) than intended.

*We have also qualified this with “eventually”.*

Page 6, lines 8-9: “We accept that using a 4D grid will not yet provide enough information to furnish filled (interpolated) datasets.” Dropping the time dimension (the 4th D) would not in fact make much difference when the real problem remains very sparse spatial coverage? E.g. even if the authors used only X, Y, and Z data (or even only X and Y data), they still would not have nearly enough global data coverage to satisfy any interpolation scheme? Thinking of Fig 1 in the macrozooplankton paper (Moriarty et al) where even for abundance, we really do not have enough data for interpolation through the central ocean empty spaces? According to Table 2 here, macrozooplankton actually has the best grid coverage, so all other PFTs have even less data? Main point - scarcity of data, more than 4D grid, prevents useful interpolation?

*We have removed the mention of the 4D grid and simply say: “At this point, there is not yet enough information to furnish filled (interpolated) datasets.”*

Page 6, line 15: What about the compiled data used in this paper? Also available? E.g. the data behind Table 2?

*In the Data section, after “(<http://maremip.uea.ac.uk/.maredat.html>).” we have added: “The latter also includes a technical corner with the gridding program, QC script etc.”*

Page 7, line 2: I do not understand why cell volume matters for abundance data for the diazotrophs. As I remember that paper, the largest issue had to do with abundance to biomass conversions, wherein size (or volume) played a large role in the uncertainty in biomass, not in abundance?

*We agree. We have added the abundance data to the gridded diazotroph file. This brings this file in line with the others, and we have removed the sentence about cell volume.*

Page 7, line 11-15. The authors should publish the microzooplankton corrections! But, have the authors and editors identified the best way to do this correction, and subsequent corrections and improvements to all the MAREDAT data sets? Should each correction have a least its own description and doi? How will authors / ESSD handle future corrections and improvements?

*We added the number of corrections that were made “(n=682)”. It would not serve much purpose to specify each of those 682 corrections, which fell into three classes: a typo in the use of the Menden-Deuer and Lessard (2000) conversion between cell volume and carbon content in one subset of data, an error in the relational logic that takes the reported total biomass if available and the sum of the reported groups (in most cases ciliates, dinoflagellates and flagellates) if not, and unit conversion corrections. We have not yet agreed exactly with Pangaea how future updates and possible corrections will be registered, but Pangaea has a very transparent way of dealing with parent and child databases, so I’m sure that this will pose no major problems. The 2010 version of the microzooplankton database has been taken off the web, to prevent people from using the uncorrected version, but if the reviewer wishes to compare the two versions we can provide it.*

Page 7, line 18: “all collated data”. Does this statement imply that the MAREDAT datasets include some ‘un-collated’ data, collected by each team’s solicitation process but then collated or otherwise quality-controlled by the authors themselves? Of, have they only used ‘collated’ data? If so, what exactly does that term imply?

*All data was included. The word collated was removed.*

Page 8, line 19: “justified” YES! Even necessary?

*We’re not sure whether replacing justified with necessary would make this sentence more clear, nor whether the reviewer asks for this change.*

Page 9, line 4: This should refer to Fig and Table 2?

*Since this is the first table mentioned in the text, table 2 was moved to be table 1 and the rest of the manuscript corrected accordingly.*

Page 9, lines 12-15. In addition to the regridding already mentioned do readers and users get or need to get more information about the other corrections? Does the doi registry as applied by PANGAEA ensure that users get and understand the corrected dataset?

*Yes, readers can download not only the gridded netcdf files but also Excel spreadsheets with all metadata. See also the reply to comment about Page 7, line 11-15. Only the corrected microzooplankton dataset is available from PANGAEA. The Buitenhuis et al. (2010) dataset, which used to be downloadable at [http://lgmacweb.env.uea.ac.uk/green\\_ocean/data/](http://lgmacweb.env.uea.ac.uk/green_ocean/data/) is no longer staged there, and it never had its own database doi, only the doi of the associated GBC manuscript. The new datasets of all PFTs are now available from both PANGAEA and the above webpage.*

Page 11, lines 12-16. The text refers to the MAREDAT global HPLC pigment database as the basis for comparison with global abundances and refers to Fig 3, but according to the legend for Fig 3, Fig 3 uses WOA 2005 Chl data, not the Peloquin HPLC data? *The reference to Fig 3 was removed. The remainder of the paragraph states more clearly which figure panels in the Peloquin paper can be compared to which abundance based figures, including some of the panels of Fig. 3.*

Page 12, line 11. The authors attribute the diatom abundance peak at 125m depth (Fig 2a) to sampling bias. Presumably, sampling focused at the base of euphotic zones? Why wouldn’t or doesn’t such a depth bias also apply to the samples for HPLC pigments, and why therefore does the depth profile of fucoxanthin pigments not show a similar peak?

*We have not attempted to compare to which extent HPLC pigments were sampled during the same cruises as the diatom abundances, but it seems entirely reasonable to suppose that no HPLC samples might have been taken at the few locations where the very high diatom abundances were encountered. Since these diatom abundances were not excluded by Chauvenet’s criterion, we have no formal way to decide which diatom abundances should be counted as “high”, but to give a rough idea, there are three values between 295 and 7209  $\mu\text{g C/L}$  between 112.5 and 137.5 m depth.*

Page 12, lines 25-26. For macrozooplankton that migrate vertically (over 10s to 100s of meters?), and assuming sampling occurred without day (deep) or night (shallow) biases, wouldn’t vertical migration result in a broad vertical distribution, not a single subsurface peak?

*This is true. We have replaced this with; “Macrozooplankton include some species that swarm, which could explain this sharp biomass peak, but it’s also possible that some sampling bias such as proposed for the high diatom concentrations at 125 m depth occurred at a biomass concentration that wasn’t quite high enough to be excluded by Chauvenet’s criterion.”*

Page 13, lines 15-17. Awkward. The “concentrations” are not “quite different from assuming”. Rather, the concentrations will require a re-assessment of the assumption that all deep sea activity derives from picoheterotrophs?

*This was changed to: “These biomass concentrations are low relative to the surface biomass, but the volume of the deep sea is much larger, suggesting that zooplankton could make a substantial contribution to global ocean biogeochemical cycles in the deep sea as well. Picoheterotrophs have received much more attention with respect to their role for the biogeochemical cycling of organic matter in the deep ocean, but the ecology and biogeochemistry of the deep sea with a significant zooplankton contribution could be quite different from what we expect for a picoheterotroph-dominated deep sea.”*

Page 13, line 23. Here we need a careful qualification. The authors interpolated the global biomass data by PFT across the depth bins, for purpose of an integrated biomass estimate - yes? They did not do any horizontal (grid point) interpolation?

*That’s correct, we have added “vertically” to make this more clear.*

Page 13, lines 24-26. Agree with these uncertainty factors. But, can the authors provide any quantitative information about these uncertainties. Do the abundance-to-biomass conversions account for half, or most, of the uncertainty compared to geographic biases. What about data completeness? Do the authors feel that they have gathered and compiled half (50%) of potentially available data sets? 20%? 80%? How would a more complete data set fill out the spatial coverage? Can we get an assessment of uncertainties due to incompleteness as compared to conversions and spatial biases? Even relative statement such as biomass conversions > spatial biases > incomplete datasets? By making such an assessment, the authors could help point the way to the most effective improvement strategies?

*We have added: “The conversion factors are probably the main source of uncertainty in all the datasets, while the importance of lack of coverage or of sampling bias appear to vary between datasets, e.g. for Phaeocystis there is a substantial sampling bias towards the coast as noted above which appears to explain the high horizontally averaged profile (Fig 2a), while for bacteria it was shown that a potential bias towards the coast (in this case due to an observed change in the conversion factor rather than overrepresentation of sampling) was unimportant (Buitenhuis et al. 2012a).”*

Page 14, line 15. But, doesn’t the WOA chl dataset also in fact have a coastal bias, just through numbers of samples? And a bloom bias?

*This is true. We have replaced these two sentences by: “While these are reasonable C:Chl ratios, even at a given light intensity C:Chl ratios are quite variable between taxonomic groups. Therefore, these C:Chl ratios do not constitute a very stringent test, and a biomass similar to the picophytoplankton could be added to the total and still result in reasonable C:Chl ratios.”*

Page 15, line 18. But, didn’t the authors just finish discussion of coastal biases? Here, we read that the data “are mainly representative of the open ocean environment”.

Page 15, line 19. This reader has a hard time to see Fig 5 as an inverted pyramid.

*We have removed the reference to the open ocean and have changed this to: “The global biomass data for each plankton group (Fig. 5) do not show a blunt food pyramid as is typically found in terrestrial ecosystems, but instead confirm the high H:A (heterotroph:autotroph) ratio of around 1 (see also Fig. 2D) that was previously found by Gasol et al. (1997).”*

Page 16, line 10. I think I know what the authors intend with the word “crude”, but they might consider more accurate and descriptive words such as ‘preliminary’ or ‘incomplete’?

*This was changed to preliminary.*

Page 16, lines 11-15. Good summary sentences! Do they belong in the abstract, as well?

*We have added to the abstract that: “The results show that abundance is much better constrained than their elemental composition, and coastal seas and other high productivity regions have much better coverage than the much larger volumes where biomass is relatively low.”*

Page 16, lines 20-28. This section seems to have a paleo-climate focus, almost retrospective. But in the introduction we read about MAREDAT meeting the needs of the DGOM community, with a more prospective function. Please can we get some assessment of the MAREDAT product, as realized and summarized here, with respect to the DGOM requirements?

*Although the main impetus of MAREDAT has come from the DGOM community, we recognized that a more collaborative approach would be much more valuable. We have therefore tried very hard to construct the raw databases with all metadata intact, to make sure that other users could add further data, do their own analyses, and resubmit the resulting database to a repository like PANGAEA for others to build on further. This paragraph focuses on paleo-climate in discussing the databases of foraminifera and coccolithophores, which have been extensively used by the paleo-climate community, and in other papers we have also emphasised potential uses for biodiversity or ecological research.*

*We’ve added some remarks on the total MAREDAT product in the new summary section. However, there is no threshold of requirements for DGOMs. As the data coverage increases the discriminatory power increases and new science becomes possible, as indicated by the reference to the Luo et al. 2013 paper. This new science inevitably generates new unanswered questions which can only be addressed by updating the databases.*

Page 17, lines 1-10. Only two expeditions, to fill all the gaps exposed by MAREDAT? I think I heard that TARA Oceans already ended, due to funding limitations. What about other data sources? What limits the gathering of additional data - ship time, investigator interest, data access?

*All of the above. E.g. the difference in coverage between the Northern and Southern hemisphere reflects the bias in sampling intensity. The response rate to our requests for data has been very variable, and as far as we can tell reflects different stages between research communities that develop around certain taxa within a more general trend towards seeing the information in papers as open source once they have been published. We very much hope that our policy of making all contributors coauthors and hopefully also the usefulness of the databases will help to make data access easier in future iterations of MAREDAT.*

Page 17, line 17. Global ocean biomasses of phytoplankton, zooplankton and picoheterotrophs are of roughly equal size?

*We do start this paragraph by pointing out the large difference between the median and the mean. And then again start the sentence the reviewer refers to with “Within the uncertainty in the data”. But with those caveats we do think that the data referred to (Table 2, Figs. 2d, 5, Gasol et al., 1997) support this statement.*

Page 17, lines 23-24. “including additional observational data and improving the cell to carbon conversion algorithms”. How? On what time scales? New data, or additional existing data not yet included? For all PFT classes? Or, do some have priority over others?

*We have changed the last paragraph of the discussion to say: “We hope to update all the databases in a MAREDAT2015 effort”. Most of the lead authors of the databases/MAREDAT papers are on temporary research funding, but apart from that uncertainty, all of us have agreed to participate in the 2015 update.*

Page 22, line 2. Peloquin et al. now submitted, has a doi?

*This has now been published in ESSD and updated accordingly.*

Page 26, Figure 1. Needs units on the color axis.

*Was there a file conversion problem? The figure on the ESSDD website does state the units in the legend and the numbers for each color interval on the right.*

Not a good choice of colors (see next comment).

*We changed this, see also reply to comment on Figure 3.*

How many total possible grid boxes for this land mask? Include the number in the legend for Table 1?

*We added as a footnote: "Out of 539 ocean boxes."*

Page 27, Figure 2. Very important figure but very poorly presented. The reader needs to go back and forth between complicated figure and complicated legend - awkward and distracting. Very poor color choices, especially mixture of red and green and especially in panel D. See [http://geography.uoregon.edu/datagraphics/color\\_scales.htm](http://geography.uoregon.edu/datagraphics/color_scales.htm) for much better color choices; many readers will have problems with this particular color scheme. Add text labels to each line?

*We have added labels in the panels to guide the reader in interpreting the lines.*

Make the Figure much bigger?

*Note from authors to ESSD: Yes, please.*

Page 28, Figure 3. Not particularly readable or useful in this format. Need clearer panel labels. Unless authors have a good reason for this arrangement, re-arrange the panels with 6 autotroph PFT stacked vertically on left, 6 heterotrophic PFT on right? Clarify the chl or HPLC pigment data used in panel F?

*We've labelled each panel with an abbreviation of the PFT. We rearranged the panels as suggested. We've changed the color scale, and have changed figure 1 to the same scale. Fig 3F is WOA2005 Chla. Although the pigment database also contains Chla we don't mention or present these results at all in this paper because coverage of WOA Chl is so much better.*