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Discussions

Interactive comment on "Fluxes of sedimenting material from sediment traps in the Atlantic Ocean" *by* S. Torres-Valdés et al.

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Fluxes of Sedimenting Material from Sediment Traps in the Atlantic Ocean [essd-2013-23] by Torres-Valdés et al.; replies to reviewers

Referees' comments are in *italic* type, our responses in plain type. Our paragraphs are identified by number (for reviewer) and letter (for sequence) to allow cross-referencing if required.

Introductory comments
We want to thank the referees for their comments, suggestions and criticisms, which we found useful and have helped improve this manuscript. We would also like to stress that when making the data compilation, we made all efforts to "dig" as deep as we could. We are confident that the dataset presented is as clear, accurate and well-documented as is possible for a composite of this nature. With over 5000 observations from a wide variety of sources and studies we feel that it is unrealistic to expect a perfectly homogeneous dataset. Following reviewers comments we have made revisions to the manuscript and to the data set. The updated version of the dataset has been submitted to PANGAEA.

Anonymous Referee #1 Received and published: 18 November 2013
Major comments: This study complied most of the datasets of particle fluxes derived from sediment trap, which I think will be valuable for better estimation of the magnitude of particle export and the interpretation for the function of the basin-scale biological pump in the entire Atlantic Ocean. The complied dataset will also be useful for the future studies. My major concern about this study is on the mergence of those datasets which is definitely not enough judging from the main text of the MS, although this reviewer agree with the authors' statement of "A major challenge involved in the compilation of this dataset was the inconsistency with which authors refer to the same variable". There are several aspects that I suggest the authors to dig deeper.

1. A. Thank you for the comment. We do agree that trap design is important, and one would hope that all authors would specify the main characteristics of the traps they use, but typically this is not the case. Furthermore, for some of our sources, although the trap is named, the papers that are cited do not provide the technical specifications. Instead, these citations may refer to yet another indirect reference for the specifications. Similarly, for much of the data extracted from PANGAEA, details of trap design and

specification are significant issues. In the best cases, the metadata section for 'Device' specifies "Device: sediment trap". In several cases, this is only reported as "Device: trap" or "Device: drifter". We have nevertheless made additional efforts to trace trap designs and now include this information in Tables 3 and 4. However, in a few cases, information could not be found, which we also indicate in Table 4.

Second, I am not convinced on the definition of some parameters, e.g. CaCO3, biogenic silica and lithogenic flux. Although these parameters was expressed in different ways in the literature, I strongly ask the authors to standardized them using converting factors, and that will be easier for the following researchers to use this dataset.

1. B. We appreciate the comment, but with no examples provided as to why the reviewer is not convinced of the definitions, we cannot be sure that we have interpreted this comment correctly. In the text of the manuscript, we mention all the names by which variables are referred to in the original sources and have attempted to provide enough information to indicate to the Reader whether a name is analogous to another or not. Regarding conversion factors, we prefer to make reference to molar mass ratios in the text and we have now included a column in the data set where these are indicated. We disagree with the use of off-the-shelf 'conversion factors' since application of them generates confusion and ambiguity due to the uncritical manner with which these are often used. This is particularly confusing for students and researchers who are less familiarised with the topic. We instead aimed to make references to molar mass ratios, which are not ambiguous.

Third, although this study is mainly on data compilation, the discussion on the science part is still too weak, for example, All the descriptions of main findings is hand waving and not based on the numbers. In addition, several correlation plots between different

paraments could be shown, like POC vs CaCO3, POC vs biogenic silica and POC vs Lith. The improvement on those aspects needed to be done before this MS will be published

1. C. Again, we appreciate the comment. As far as we understand, the formal aims of ESSD are to provide an arena within which it is possible to describe a compiled data set, including information concerning the methods by which it was generated, rather than a more scientific focus on the interpretation of such datasets; though we recognise that this is highly variable between ESSD papers. The ESSD website states that "Any interpretation of data is outside the scope of regular articles." Consequently, there are no "main findings". Our main aim for this collection was to bring together data that resides in different repositories (or hard drives) and/or data that are not publicly available into a single usable dataset. Additionally, our aim for the ESSD manuscript was to provide enough information about the dataset so as to motivate and to allow potential users to make their own informed decisions as to how they want to further handle the data. Our intention here was to briefly describe the general distribution of the data and highlight general regional differences and trends. There is wide temporal and spatial data distribution, covering several oceanographic regimes (i.e. different ocean, atmospheric and biological dynamics), which we are confident will offer a rich resource/motivation for other authors to tackle specific questions.

Minor comments:
P1: The title should be changed into "Compilation of sedimenting material fluxes from sediment traps in the Atlantic Ocean"

1. D. We have modified the title to "Data Compilation of Fluxes of Sedimenting Material from Sediment Traps in the Atlantic Ocean."

P5, Line 8-28: List the specifications for all the traps, and classified into different group.I don't like negative sentence like "some of these may be identical. However, there is insufficient information to ascertain this from the source. . .. . .", the authors can at least communicate with those scientists through email to ask.

1. E. We do sympathise with the referee as the statement that "some of these may be identical" reflects the lack of degree of detail and inconsistency in the original data sources behind this compilation. We have contacted PIs for further information, but for some we are still waiting for a response after over 2.5 years. We therefore feel that the only practical option is to list the information available and to note where information is ambiguous or missing. Concerning trap design information, please see response **1.A.**

P6, Line 25: PN should be better than PON, please replace it within the main text.

1. F. We disagree with this suggestion. Most studies report particulate organic nitrogen (PON) and it is analogous to POC.

P7, Line 4-5: Please give out the duration of the rinse.

1. G. There is no mention of the duration of the rinse in any of the papers examined, nor in any analytical protocol that we are aware of. Furthermore, duration of rinse without flow rate information would still be relatively uninformative.

P7, Line 8: Give out the drying time, 80oC is way too high for POC.

1. H. Drying time is not reported in any of the source papers (datasets). However we have now included slightly more information to point out that some filters are oven

dried, air dried or dried on a hot plate surface. Protocols for the drying of POC samples appear to differ widely between studies but are rarely fully documented.

P7, Line 24-25: The factor of 2.5 is derived from the ratio of molecular of calcium and calcium carbonate (100/4).

1. I. Thank you, we did miss the fact that the factor comes from the molar mass ratio. It has now been included in the text.

P7, Line 26: Explain the factor for lithogenic-Ca (0.5).

1. J. Done. The 'factor' derives from the ratio of Ca to Al of the average composition of the continental crust, but the source study does not specify it nor it cites an appropriate reference. We have included a couple of references where this information can be found; Wedepohl (1995) Geochim Cosmochim Ac. 59(7): 1227-1232 and Kemp and Hawkesworth (2003) In: Treatise on Geochemistry, p 349-410.

P8,Line 13-17: Would you please at least do some comparison between the two methods?

1. K. This is beyond the scope of the paper. The manuscript is intended as a collation of current observations in the Atlantic not a critique of the methods used to collect them, which would constitute a paper (or several) in itself. However, we have now briefly described the fundamentals of the two methods most commonly used.

P8, Line 18-22: Show this in the table.

1. L. We have now included a new column in the data specifying whether corrections have been made or not, to account for dissolution of biogenic silica. This is also now stated in the text too.

P9, Line 10-11: The authors need to make it clear, and derive the conversion factors

1. M. Please see response **1. B**., above.

P9, Line 27-28: Make it certain on how the factor of 12.15 comes from.

1. N. Done. With thanks to reviewer #2, see **2.C**., below.

P12, Line 10-12: Show the ranges here in the text.

1. O. Done.

P12, Line 19-20: Show the numbers so that the readers can well understand.

1. P. We do not feel that it is necessary to add numbers here (line 19-20) since this sentence only talks about the 'expected' vertical pattern (i.e., decay and dissolution of particles as these sink). Nevertheless a few words have now been added to make reference to this fact.

P12, Line 23-29: Please describe the findings with numbers, ranges, means, etc.

1. Q. Again, we would like to clarify that there are no findings in this manuscript. It is not an analysis; it is a compilation of data. Concerning ranges, we have now added numbers in the text. Concerning means, we refrained from using them because we feel they would be misleading and risk future erroneous quotation since they would gloss over considerable variability, which is not normally distributed.

P45, Figure 1: It is hard to see the blue dots in the Arctic Ocean on the blue background, please change.

1. R. Blue dots have been made darker and are now clearer.

P47, Figure 3: It is too blur to see.

1. S. This figure has been modified to make it clearer.

P50, Figure 6: I would like to see the mean and standard deviation of those profiles.

1. T. Please see **1.Q.**

P51, Figure 7: Show the mean and standard deviation as recommended for Figure 6.

1. U. We disagree with this suggestions for the reasons provided above (responses **1.Q.** and **1.T.**).

P55, Table 2: Show the mean and standard deviation here.

1. V. Please see responses above (responses **1.Q.** and **1.T.**).

***Anonymous Referee #2** Received and published: 24 November 2013*
The authors compile sediment trap data from the Atlantic and Arctic basins and Mediterranean. It is nice to see these data compiled, and have, at a glance, some figures showing the major particle composition in these regions. That said, there are some details that the authors should address to ensure data quality and to make this dataset much more useful. I appreciate the challenge in sorting through all the variable names, methods, and units. It's a tedious and messy business. However, this frustration is perhaps a little too apparent in the description of each major parameter (subsections in section 2.2). This is partly a style issue, but also an issue with giving confidence to the reader/data user (and in this case, reviewer) that the authors have adequately dealt with the unit conversions. There are a couple of unit conversions that the authors appear not to understand:

2. A. We appreciate this comment and acknowledge that perhaps our frustration was a little too apparent. We have accordingly tried to address this in the revised text.

p. 547, lines 24-25: factor of 2.5 for biogenic Ca is simply the weight ratio between $CaCO_3$ and Ca=100g/mol $CaCO_3$ / 40g/mol Ca=2.5

2. B. We thank the reviewer for this observation. There was indeed confusion generated by the use of different terms without description as to what the source authors meant by it, and out of the use of 'conversion factors'. We are of the opinion that referring to molar mass ratios is a lot more sensible than just conversion factors (this is related to response **1. B.**, above).

p. 549, lines 26-29: multiplying Al by 12.15 to estimate lithogenic mass assumes a crustal Al composition of 8.2% (1/12.5=0.082), which is certainly reasonable (even if not referenced).

2. C. We thank the referee for this information as we have no experience with the crustal Al composition nor with Al measurements. With no reference or indication as to where the conversion factor derives from in the revised references, it was hard for us to find an explanation for it. We have now included this explanation in the text.

I would suggest that the authors step back and present a more synthetic overview of the methods, variable names, and conversion factors used. It isn't necessary to document each minor variation in the calculation of a parameter, particularly if they're all ultimately based on the same principle

2. D. We have slightly modified the wording but still consider it necessary to note the inconsistency and ambiguity that exists with the use of terms in such a relevant topic, if only as a warning.

This is especially the case for sections 2.2.3 and 2.2.4, which could be merged into one subsection, and for which all the variations described are simply variations on unit conversions to relate measurements of Ca to CaCO3 or measurements of PIC to CaCO3.

2. E. We have now merged both sections.

The point to doing this more synthetically is not just stylistic; it would also allow the authors to give a sense of the overall error introduced by the range of conversion factors used. For example, they state that conversion factors for POC to POM range from 2 to 2.5 and for biogenic silica to opal range from 2.1 to 2.4. With the exception of converting all major particle phases to units of mg/m2/d, the authors do not generally attempt to further standardize the data. This means that there is a built in 20% error in

the organic matter and biogenic silica parameters by putting these datasets together. Ideally, a compilation would try to standardize the conversion factors so that the data are internally consistent.

2. F. We have modified the Quality Control section and have added a comment highlighting the fact that an error is introduced by the use of different conversion factors and that attention should be paid to that.
We also acknowledge that the data was not harmonised (I would use this word here rather than standardised) further, say, by using a unique conversion factor. We decided against this because we did not want to alter the original data as submitted by the originating PIs. Instead, our approach was to provide as much information as possible (some of which is actually lacking in the original studies) allowing potential users to further scrutinise the data for their particular questions. See also **1. B.**

I realize this is a lot of work, and it might not be possible to do this given the data reported, but at minimum, the authors should discuss the overall error introduced by the range of conversion factors used, and at best, the authors should consider applying a consistent standardization to all parameters where possible.

2. G. Thank you for the comment. Please see **2.F.**, above.

The issues raised in the biogenic silica flux section are more worrisome. The authors say that they sometimes cannot tell whether their source data are reporting Si or SiO2, which would introduce a potential factor of 2 error. I should think that a closer reading of the papers would make it obvious which one is reported from the context in which the data are discussed. If there are studies in which it is truly impossible to tell which it is, I strongly suggest adding a column to the data table with a quality flag for the Si numbers, so that users can easily flag the ones that are suspicious. As it is, it is quite

difficult to read through all the notes to determine which ones are truly uncertain.

2. H. We think that we were not sufficiently clear in the manuscript: we did undertake the checks suggested by the Reviewer and converted all data reported as Si to SiO2. The preferred variable is the mineral SiO2 due to its ballasting effect. We now make clear in the manuscript that a thorough inspection of the data, and conversion to SiO2 where appropriate, was carried out.

Additional comments on the data tables: The dataset lists the location (latitude and longitude) of the traps up in the header. It would be much more useful if the latitude and longitude were listed as columns in the data table.

2. I. We fully agree. However, having originally submitted the dataset organised exactly as described in the ESSD paper (see our Table 1) to the PANGAEA repository, this key information was subsequently moved to the dataset header, presumably for compliance with a house style. We had no control over this and in this case we feel strongly that this policy has significantly affected the usability of the data, so we will also aim to lodge the data with the British Oceanographic Data Centre in a more easily handled table form suitable for Matlab, ODV, etc.
House style has also been imposed to the name of some variables; CaO3 was replaced by PCa and Si(OH)4 by Si.

I also strongly suggest adding quality flag columns, ideally associated with each variable, to indicate when reported values may have been ambiguous in the source text (eg. biogenic Si), but also to indicate which values have had a conversion factor applied, what deployments were short/long: : :

2. J. We have now added flags to bSiO2 data. Concerning deployment duration, the

dataset already carries columns for start and end of deployment, and also duration of deployment in days.

In its current form, it is very difficult to go through the notes column to pick out what data have had additional processing, etc. Having a flag column for each variable will ultimately make the dataset much more usable.

2. K. We understand the motivation for this comment but such a set of flags would be extremely time-consuming to create and would be of limited value in most cases as it is really only the bSiO2 flux which is a major issue. We have therefore restricted ourselves to creating a flag for this variable (see **2.J**).

Figures: Figure 3: outline of continents is not visible; color bar choice makes it impossible to distinguish between the lowest end and highest end observations (they're both pink indeed, almost all stations look pink).

2. L. We have modified this figure.

The authors should define what they mean be an "observation point". If POC is reported at 3 depths every month for 12 months, or POC, CaCO3, bSi are reported at 1 depth over 12 months, are those both counted as 36 observations each? What about variables derived from each other (eg. Al and lithogenic mass) are those 2 observations or 1?

2. M. We have modified this wording and referring instead to "data points"; it is explicitly stated in the text (section 2.1 Data Sources) that each data point is an independent measurement or calculation.