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Interactive Comment

Interactive comment on "Biogeochemical records of past global iron connections" *by* Z. S. An et al.

Anonymous Referee #1

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"Biogeochemical records of past global iron connections" by Z. S. An, J. J. Cao, K. K. Anderson, H. Kawahata, and R. Arimoto

General comments

The enigmatic correlation between high rates of Antarctic dust deposition with relatively low atmospheric CO2 concentrations and cold glacial climatic conditions, and the potential for a strong control my dust generation and atmospheric transport on the marine carbon cycle is a worthy topic. Despite the crowded literature field on the subject, the approach that Zhisheng An and colleagues take, in attempting to illustrate biogeochemical (and potentially causative) connections between dust, marine productivity, and atmospheric CO2 over a broad spectrum of time-scales is quite innovative. However, many of the paleoclimatic records chosen for illustration appear to show little or no evidence of the 'global iron connections' that the authors claim and only very

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weak support is offered in substantiating the claims made in the abstract regarding the causative linkage dust->CO_2, particularly with respect to the influence of Asian dust on CO_2 as mediated by productivity in the North Pacific. Thus, although there is clear value in what and how the authors are trying to illustrate, I cannot recommend publication of this manuscript in its present form, or at least, not without significant revision.

Specific comments

Firstly, a great number of terrestrial, ice, and marine sediment core locations are mentioned throughout the text. Although their locations are stated variously in the body of the text and/or figure captions, for some cores only a relatively large geographical region is given, whereas in other places, the text becomes crowded with lat,lon values. It would be extremely helpful if all locations mentioned could be marked on a map. This could be done as an additional panel to Figure 1, which would help the reader relate sampling locations to major dust sources (panel a) and interpolated ocean dust deposition rates (panel b). The addition of a table would also help, and all the body of the text to be freed from numerous lat,lon values.

Section 2.2 (“Paleoproductivity variations in the past”) is a good example of where a map of the core locations would be useful. Previous work assessing glacial-interglacial changes in paleoproductivity should be discussed in detail, particularly that of Kohfeld et al. [2005] (Science 308), and the sediment core results presented here put into that context. For instance, to what extent are the records selected for representation in Figure 3 consistent with the board-scale patterns shown in Kohfeld et al. [2005], or are some of the records in Figure 3 ‘anomalous’ in some way? That Kohfeld et al. [2005] find productivity in the Western North Pacific at mid (ca. 30°) latitudes higher during periods of high dust (LGM) than low dust (Holocene, and Stage 5a-d) is helpful to the authors’ arguments (regarding a potential Asian dust influence on Pacific productivity). In the Figure 3 caption and/or in the body of the text, it should also be stated which organic carbon MAR records have been (230Th) corrected for sediment focussing, because this has an important bearing on whether glacial-interglacial changes in organic

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carbon MAR can be interpreted as reflecting changes in productivity in the overlying ocean or not. The addition of a table listing the various cores would allow important details such as the application of 230Th normalization to be concisely presented.

More importantly, I have some serious reservations regarding the attempt to illustrate the existence of iron-CO2-climate linkages over a spectrum of time-scales. I will discuss the sections I have the most difficulty with in reverse order;

3.4 “The influence of Asian dust on past changes in global atmospheric CO2”. To begin with, the uppermost ice-core CO2 curve appears to be from the Taylor Dome, as stated in the body of the text, not “Dome C” as stated in the Figure caption. I have no problem with the plotting of 2 different CO2 curves, but there is no reason not to plot them on top of each other as they should ideally be identical. Indeed, plotted overlain one gets a better sense of the uncertainty (mainly measurement error) in the reconstructions of past atmospheric CO2, which is highly relevant when attempting the sort of analysis that the authors try to make. Dust variability must be plotted as an accumulation rate rather than dust concentration (labelled “Dust mass” in Figure 7) since dust concentration is affected by glacial-interglacial changes in annual snow fall. These are minor points, however. I am much more concerned by the inference made from the correlation between the small apparent decrease in atmospheric CO2 between 23 ka and 18 ka and the sharp increase in Lingtai loess dust accumulation rates. I think that this is an artefact of the low resolution of the 2 ice core records used (even the Tayor Dome CO2 record as presented has a relatively low resolution over the 23-18 ka interval of interest, although without the data points plotted, it is hard to be completely sure). The Dome C CO2 record as published in Science in 2001 by Monnin et al. has a much greater sampling resolution over the critical interval. Between 22 and 17 ka, the Dome C CO2 record clearly shows no trend (i.e., no change in CO2) to within error (+/- 1 ppm), yet the sharp dust increase recorded in the Lingtai loess occurs wholly within the same 22 (or 20) to 17 ka interval. One must a priori conclude from this that either there is no evidence of any link between changes in Asian dust

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recorded in Lingtai loess and atmospheric CO_2. One could argue and play around with relative chronologies, but at best, the evidence for any causative Asian dust2 link is exceptionally weak. It cannot therefore be safely concluded (in the abstract, and discussed at the bottom of Page 249) that; “one-tenth to one-third of the global change in CO_2 due to dust-supplied Fe could be ascribed to variations in the dust supply flux from Asia and its associated effects on productivity in the Pacific Ocean.”.

3.3 “The last 50 years”. I find myself wholly unconvinced by the inferred correlation, even though the authors are commendably honest enough to add the caveat that “Ě this is clearly a simple and crude first assessment” which “deserves further investigation.” The assumed lag between peaks in dust and d15N of 2 or 3 years is meaningless without some sort of mechanistic justification for how the lag arises. Primary features of the 2 records are not discussed at all, but are equally deserving of explanation - for instance, the most prominent d15N peak (year ca. 1968) is not preceded by a dust peak within 3 years. Why? Also, there is a clear long-term declining trend in dust from the early 1970s onwards, yet over the same period d15N exhibits an increasing, not decreasing, trend, contrary to the hypothesis of a causative link of dust->d15N. To be honest, I cannot see how these records offer any support for the authors’ hypothesis.

3.2 “The last 1200 years”. Again, I am rather less than convinced that any correlations can be drawn with any confidence between dust and d15N, let alone between dust and CO_2. For instance, there is a pronounced minimum in dust storm frequency ca. year 1500, yet d15N remains at some of its highest values (and there is a slight drop in atmospheric CO_2). When making such detailed comparisons with records such as that of CO_2 as is attempted here, the addition of error bars to the figure becomes critical. This goes for Figures 6 and 7 as well. Application of some statistical measure for presence/absence of correlation would also be valuable and would avoid arguments having to rely purely on subjective judgements made ‘by eye’?

Technical/minor comments

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* Page 236/lines 7-9; although I understand what the authors mean, the term “geological” in this context is a little misleading.

* Page 237/lines 12-14; what about Patagonia (both deserts and exposed shelf), which appears to be a key glacial dust source, at least in terms of potential for Fe fertilization?

* Page 238/line 22 through page 239/line 7; an entire paragraph is devoted to the wordy description of glacial-interglacial variability in dust accumulation in a marine sediment core - this record should be reproduced here. (Terrestrial and ice core records of dust have already been reproduced - the obvious missing dimension to make for a fuller review-type article is the marine realm.)

* Page 241/line 24; I am assuming that “C4402” is a core location. A little more description is needed and a labelled figure of core locations (plus table) would be helpful here.

* Page 241/line 23 through page 242/line 3; I am far from convinced that the suite of Eastern Equatorial records of carbonate content, originally published by Farrell and Prell [1989], necessarily demonstrate what the authors claim. Records of wt

* Page 243/line 5; typo - “GtC”, although the trendy IPCC thing to do these days is to use units of PgC (S.I. units).

* Figure 4; the GRIP dust count and Vostok CO2 record do not appear to be appropriately referenced (i.e., no reference is given at all) either in the figure caption or in the body of the text.

Interactive comment on Clim. Past Discuss., 2, 233, 2006.