Clim. Past Discuss., 8, C3090–C3092, 2013
www.clim-past-discuss.net/8/C3090/2013/

Climate of the Past Discussions

Interactive comment on "Iron fluxes to Talos Dome, Antarctica, over the past 200 kyr" *by* P. Vallelonga et al.

Anonymous Referee #1

Received and published: 11 January 2013

This paper presents a new record of iron fluxes at Talos Dome, a coastal dome in Antarctica. The data are likely of high quality, and the paper is clearly written. It does what it says in the title. I can't say that the information in it is very exciting or novel, as the conclusions mainly duplicate those already made with the Dome C ice core. There is some new information here about the possibility of a local addition of material in the Holocene, and this could be explored a little more. But mainly this is a solid paper, presenting confirming evidence that is useful to see without being spectacular.

A couple of issues perhaps need more discussion in the paper. The first one concerns the eternal problem of solubility. Here the authors present acid-leachable (pH 1) iron. This is clearly stated and that is fine. But then later in the paper, the data-based calculation of the effect of Fe on CO2 is performed, repeating what was done

by Rothlisberger et al (2004), and hinting that doing the calculation with Fe is superior. It may be so, but the authors should caution that solubility at pH1 is far from relevant for the amount of Fe deposited and available into seawater at pH8. While there is no published data on this, the authors should acknowledge this discrepancy.

A second issue that is not covered completely concerns the additional deposition at Talos Dome in the Holocene. The authors point exclusively to local sources. Firstly, the nature of these local sources should be discussed a little; what is their location and therefore area of possible influence. But also the authors don't discuss the extra material that might be wet deposited at Talos Dome because of its somewhat higher snow accumulation rate. At 7-8 cm/a, Talos is intermediate between sites where dry deposition dominates (and therefore we may expect flux to be the relevant indicator of what is in the atmosphere), and sites where wet deposition dominates (so concentration is more relevant). At least a part of the additional deposition at Talos can be due to wet deposition and it should at least be discussed (but of course does not explain changing source signatures in the geochemistry).

A few more detailed points:

Page 6094, Line 14: "we conclude that" should be "we confirm that" as this is only a repeat of earlier calculations.

Page 6095, lines 5-7: "coupling between SSA dust.....and meridional ..transport". I know what you mean but am not sure this says it (as written it appears to say that a change in the dust source affects the strength of the transport; this may even be what Martinez-Garcia wrote, but still I don't think it reads quite right). How about "marine sediments demonstrate an increased production and/or transport of dust from SSA sources"?

line 14: "have validated the basis" is too strong, these expts have only shown that some steps in the argument can work at times, they have not shown a large scale effect occurs. Perhaps "have reinforced the possibility that past changes in iron could have been significant for CO_2".

Page 6097, lines 15-16. In Fig 1, its a little surprising that Talos and EDC dust have similar fluxes to each other in both MIS2 and 6, while the Fe flux is similar at the two sites in MIS6 but not in MIS2. This at face value implies a missing source of Fe at EDC – comment specifically on this difference between MIS 2 and MIS6?

Table 1. Talos Holocene acc rate is given here as 0.07, but in text as 0.08, please chnage one or other or explain if the time base is different.

Figure 1 caption. I am pretty sure that CO_2 before 138 ka is from Vostok not EDC (only before 400 ka is it again from EDC). You need to check this in Luthi and Siegenthaler papers.

Interactive comment on Clim. Past Discuss., 8, 6093, 2012.