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Discussions

Interactive comment on "Atmospheric aerosol deposition fluxes over the Atlantic Ocean: A GEOTRACES case study" *by* Jan-Lukas Menzel Barraqueta et al.

Anonymous Referee #2

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Review of Menzel Barraqueta Atmospheric supply of trace elements has been a central theme of GOETRACES and so this paper is an appropriate contribution to this issue. The paper attempts to use aluminium data in the water column to estimate atmospheric dust deposition in a refinement of the MADCOW model developed by Chris Measures and colleagues. The data and approaches involved are basically sound and I am happy to recommend publication but would suggest some modifications before publication. I have two general points. 1. These authors another paper submitted to this issue which is referenced here and which is partially repeated here. There is also a lot of information in the paper that notes the similarity of the data reported on aluminium concentrations to that previously reported. I cannot help feeling that much

of this material could be shortened in this paper if the focus of the paper is indeed on the utility of the MADCOW model. 2. The MADCOW model was always acknowledged to require assumptions about mixed layer depth, solubility and dAl scavenging. These are explored in detail here but firstly it should be clear that these limitations of the model have been acknowledged by the community for a long time. Secondly with at least these three parameters as numbers that, even with the careful regional evaluations here, are poorly known, there are limitations to how far the model can be used in a detailed area specific concentration mode. Specific points Line 23-24 I don't think that clouds compromise deposition flux estimates Line 12-20 There is no mention of filtration in the methods here – if the data were for unfiltered samples acidified in this way it would include much of the pAl. In the other submitted paper it says the samples were filtered which is I assume the case but this needs to be clarified. 2.2.3 The use of the Han residence time approaches seems appropriate but if the output is essentially that of Han the subsequent discussion of it could perhaps be shortened. 3.1 Mixed layer depth is a key component of the MADCOW model and clearly varies from place to place and from season to season. The discussion here emphasises the large resultant uncertainties but does not discuss how and why they arise or the best approach to dealing with them. It is not actually clear to me even which of the various MLD estimates were used. P7 section 3.2 is actually 3.3 I think. There is I think a lot of general review of other data throughout section 3.3 that seems to me could be shortened since it has been discussed in the cited papers and the dAl distribution in the Atlantic is quite well known. 3.2.1 line 23 what criteria are used to exclude continental input influenced data? Section 3.3. lines 8-10 and line 12 are contradictory. The different solubilisation methods do yield systematically different values but these difference can be accounted for and are not the main causes of the difficulties in estimating atmospheric deposition. Line 19-28 I am not sure that there is evidence for Al sources with very different solubilities in the way that has been shown to be important for anthoprogenic vs dust Fe sources. Atmospheric processing is important (line 26) as shown by Baker and Croot and Sholkovitch. P11 line 10 I would think Table S5 should be in the main paper given

its importance to the results. P13 Line 15. I wonder why the comparison is to the Duce et al 1991 paper when there are more recent maps for dust deposition at least. Line 25-30 the MADCOW model did not ever aspire to “accurately determine atmospheric deposition fluxes” P15 line 9 when the MADCOW and atmospheric dust deposition models diverge, it is not clear to me that it is possible to know which is right and wrong as implied here

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