Reviewer 2:

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.

Indeed, the dissolved aluminium data from GEOTRACES section GA01 has been published in a different manuscript in the special issue. However, in this manuscript we are describing the dissolved aluminium signature within the mixed layer depth and as such it varies in comparison with the other manuscript. Also, in order to understand and explain the MADCOW model outputs it is necessary to describe the dAl signature within the mixed layer depth. We have attempted to keep the discussion of dAl as brief as possible, but were requested by reviewer 3 to add some further references and text to explain the geographical variations.

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.

We acknowledged the comment by the reviewer, and indeed explore these limitations in the manuscript. Reviewer 3 makes a similar comment.

Specific points

Line 23-24 I don't think that clouds compromise deposition flux estimates

Clouds itself do not compromise deposition fluxes. However, deposition fluxes derived from satellite derived climatologies often are biased to clear sky conditions. Aerosol optical depth properties suffer then from cloud presence.

We have reformulated the sentence as follow:

Modelled atmospheric deposition fluxes rely on satellite-derived climatologies. The latter climatologies use properties (i.e aerosol optical depth) which suffer from interferences from cloud coverage and are biased towards clear sky conditions (Huneeus et al., 2011).

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.

Yes, all the samples were filtered. In table S2 you can find the filter type and pore size. We now mentioned it in the main text (section 2.1.)

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.

We feel that the subsequent discussion is needed in order to provide background information on the variability of the residence time regarding different oceanic regions.

We have shortened the section.

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.

We acknowledge your comment. We do acknowledge the factors that drive changes in the depth of the mixed layer and which ones do play a major role within each area. The best approach would be to assess values on a station per station basis. However, this would difficult the intra-comparison of dust fluxes within the same cruise.

As input parameter for the MADCOW model we have chosen to use a single mixed layer depth value for each cruise. This single value is the median value of the in situ MLD and the annual MLD from the Argo project.

We now explicit acknowledge the value used in the text.

"As input parameter for the MADCOW model we have chosen a single MLD value for each cruise. The latter is the median value between the MLDms and MLDar. We acknowledge that this may not be the best approach but it gives us the opportunity for intra comparison of atmospheric fluxes within the same cruise"

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.

Indeed, this is a mistake from our side. It is section 3.2. The following subsections have been re-numbered accordingly (3.2.1, 3.2.2, 3.2.3, 3.2.4).

We have shortened the section regarding GA01. However, dAl data for GA06, GA08, and GA10 are new and need to be discussed and compared with previous data.

3.2.1 line 23 what criteria are used to exclude continental input influenced data?

It is written some lines above. Normally, background concentrations are used. The stations excluded are all "coastal stations". In the previous manuscript dealing with the GA01 dataset (Menzel Barraqueta et al., 2018) we explained the different sources which could have increased the dAl levels in these waters.

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.

We acknowledge your comment. Indeed, the different leaching methods do yield different results due to difference pH of leach media, longer exposure time to HAc leach than UHP water leach, different ionic concentrations of leach media etc. Results should not be extrapolated from one method to another method. However, the GEOTRACES data suggests that there is roughly a tenfold increase in solubility of aerosol Al from samples leached with HAc compared to UHP water. You are right, the main difficulty in estimating atmospheric deposition from aerosol concentrations remain in the large uncertainty in deposition velocities and in extrapolating a snap shot measurement into an annual deposition value.

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.

We acknowledge your comment. Indeed, atmospheric processing during transport is an important factor. However, it has been demonstrated that aerosols from different sources and from different nature do show different solubilities (Baker and Jickells, 2017, Baker et al., 2013, Baker et al., 2006).

P11 line 10 I would think Table S5 should be in the main paper given its importance to the results.

As suggested, we have moved Table S5 to the main paper. Now it is Table 1 and the original Table 1 has been change to Table 2.

-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.

Our main comparison is against Mahowald et al., 2005. We have included Duce et al., 1991 as additional information and because it was one of the first global ocean maps for atmospheric deposition. Following to comments of reviewer 1, we have added atmospheric fluxes derived from the DEAD model.

Line 25-30 the MADCOW model did not ever aspire to "accurately determine atmospheric deposition fluxes"

We have modified the sentence as follow:

These results do not match the observations (from field data and satellite retrievals) and suggests that atmospheric deposition fluxes calculated with the MADCOW model are less reliable in the tropical North Atlantic Ocean…….

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

You are right. It is not possible to know which one is correct. We have rewritten the sentence to avoid confusion.

"Our atmospheric deposition fluxes were lower than model fluxes in areas of the Atlantic Ocean regions removed from the main aerosol sources regions. This observation suggests that these regions receive less atmospheric inputs than the models indicate or that MADCOW underestimates atmospheric inputs to these regions."