Volcanic forcing is one of the most important inputs to modelling studies. Until recently it has been challenging to estimate it accurately even for recent centuries. Through a huge amount of work this paper and dataset now offers the chance to do it, albeit with large caveats and uncertainties, for the whole of the Holocene period. The dataset is based on painstaking synchronisation of records within Antarctica and Greenland, and between the two ice sheets. The data are used to assess S emissions in three crudely defined latitude bands. The output is then used to derive datasets of volcanic sulfur injection, and a latitude/time dataset of stratospheric aerosol optical depth.

I consider this a very useful and worthwhile dataset that certainly warrants publication in ESSD. The authors describe the steps they have taken and the weaknesses in a clear way. At the present moment this is the best dataset that could be produced, and the documentation of it is good.

The authors go quite a long way beyond the description of the data to discuss some implications of the data. This makes the paper extremely long and I fear some of their conclusions that deserve further discussion will be lost in what will be perceived as simply a presentation of the data – the authors may want to consider whether they want to take some of those points out into a separate paper, although I certainly would not insist on it. There are some aspects of the latitudinal classification that I find a little puzzling and that I will query in the detailed comments that follow. However, provided the authors consider the (mainly very minor) points listed below then I believe the paper could be published with minor revision.

Specific comments:

Line 26 “follows the global distribution of landmasses”. Taken out of context in the abstract I think this leads the reader to think it means something much more detailed than it does. You only mean (as far as I can tell when it comes up later (line 741)) that the proportion of eruptions in the 3 boxes (NH, tropic, SH) is somehow related to the distribution of land between these 3 boxes. I think the reader seeing the abstract would think at the very least you were implying a relationship between eruption numbers/strength and the area of each continent. I’d suggest removing this from here or else spelling out the limited meaning you have.

Line 67 East Antarctica is generally considered a place name and therefore East is capitalised.

Page 3. I appreciate they are mentioned later but I’m a little surprised that the potential of S isotopes to discriminate stratospheric and hence bipolar eruptions is not mentioned here.

Fig 2. Lower 2 panels. It’s quite hard to discriminate the black and blue lines, I wonder if a different combination of colours would be better. This is quite a crucial figure to show that your methodology works so I think it needs to be presented in such a way that the synchronisation is really obvious.

Lines 194-6. Same sentence repeated.

Line 204. S is 32 g/mol, SO4 96 g/mol, NOT kg/mol!

Line 205. I’m not sure I understand the rationale for scaling the deposition at each site to that of WD. If you just want to give each record equal weight, then normalising the three records without scaling them would have worked. But does your method imply that somehow the deposition at WD is a “better” number than that at the other sites. I may have misunderstood the purpose, so it might just need a better explanation.

Line 207. I also think you should discuss the philosophy behind doing a stack as an average of the three (normalised) records. One could wonder whether the most accurate deposition rate is the average of the sites, or whether a missing peak at one site should be ignored and the best estimate

is the maximum value at the three sites. I'm not arguing for that, but I think it's not self-evident (and therefore needs discussing) why the average is the best answer.

Line 213. Can you give a statistician's estimate of how many non-volcanic outliers you'd expect above 2xMAD?

Line 265. Why have you used 1.5xMAD for Antarctica but 2xMAD for Greenland?

Fig 4d: the colours on the plot in the pdf don't look like the ones named in the caption. In place of red and dark red, I see orange and brown (and am not sure which is which).

Line 245. I wondered why you only used GISP2 for Greenland when you obviously could in theory have used NGRIP, NEEM and GRIP (DEP) in at least part of the record.

Section 2.5, line 328. I realise you need to choose something, but I found this assignment of 48°N, 37°S and 5°N to all unidentified eruptions a bit abrupt. Why these latitudes. And given that I would have assumed that a very high proportion of short-lived eruptions in Greenland were from Iceland, and from other Arctic locations, so why has 48N been chosen. I just think you need to be much more explicit about what a huge simplification this is, even if inevitable.

Line 490 and sup fig S4. You identify that there are more eruptions in the early Holocene, and later suggest this is mainly due to ice unloading. I can accept this, but surely that would only affect the high northern latitude source whereas it looks to me in Fig S4 and Fig S5 as if the tropics might also show a difference. But this is illogical as ice unloading will have had no effect at all, and indeed I think elsewhere you may say that there is no change in the rate of Antarctic S deposition (it certainly says that in the Lin et al paper which includes the lead author of this paper, in CPD). Should this be a concern?

Line 650 and surrounds. Related to the last comment, please be very careful here. Post-deglaciation increases of subaerial volcanism can indeed be imagined to be a feedback on CO2, but if it's only the volcanoes in recently deglaciated areas, then it will only be a small proportion of the total volcanic emissions that are affected. It's therefore important to state that this will not apply to the (generally much more numerous and larger) tropical eruptions (why my question about Fig S4 and S5 is important). Also please be careful to put the number from Ilyinskaya et al (which is not in the ref list) in context – the paper estimates that this one volcano "contributes up to 4% of global emissions from nonerupting volcanoes". The total volcanic emissions from all volcanoes are estimated (I believe) at 0.3-0.6 Gt/yr, about 1% of human emissions. I just think it's important the reader is not left with the impression such volcanoes could be a really significant and variable source of CO2 to the atmosphere as I do not believe this is correct in the context of human emissions.

Section 4.2. I didn't really know what you were trying to say in this section. Is it necessary?

Line 778-780, maybe mention S isotopes here as well.