

The answers and data provided by authors raise more questions than answers concerning potassium data, in particular. So sorry but your potassium data are likely incorrect.

In my first review, I asked to see in the manuscript the raw data (concentrations and not only calculated deposition flux) for levoglucosan and potassium. Indeed, if my estimate was correct, based on plots reported in the first version, I calculated fine potassium peak concentrations in the order of 0.2 ppb. I thus strongly recommend to the authors to show concentrations of potassium, sodium, and iron and error propagation estimates to evaluate the robustness of the potassium approach present at such low levels. In your response you indicate that you made a unit error and in fact potassium peaks reach 200 ppb.

I am strongly surprised by such high potassium values since many studies conducted in Greenland have shown that, if attributable to biomass burning, the fine potassium perturbations never exceed a few ppb. Also emission factors of potassium and fine potassium from biomass burning are in the same magnitude (Akagi et al., Andreae and Merlet, Gao et al) and if we assume a similar lifetime for the two species we may expect input of similar amplitude for fine potassium and levo. The difference between K_{bb} and levo in our data suggests a far shorter atmospheric lifetime for levo than for fine potassium !

So checking your chemistry Excel table, I report below two figures. Your sampling is 15 cm and I also report in Fig 2 the sodium profile reported from Schüpbach et al. (10 cm resolution). We can see that you have several sodium peaks exceeding 200 ppb (up to 1.3 ppm, Fig.1) and it is less frequent in the continuous profile from Schüpbach. Even more surprising for me, when I discover that your sodium peaks coincide with potassium ones (Figure 3). Why did you totally miss to comment that in the manuscript: it is a critical point (see below).

I scrutinize the plots of potassium versus sodium (Fig 3A) and also selecting samples with low sodium levels (<100 ppb) (Fig 3B). Even here you quasi never reach the seawater ratio (blue line in Fig 3B). That is surprising for antarctic ice. If I refer to Fe or Ca there is no way to explain that with terrestrial potassium (Fig 3C).

In conclusion, I don't see any issue to calculate fine potassium with your data (even as an estimate since your measurements indicate that the sea-salt potassium to sodium ratio is quasi never reached).

Finally, just a comment outside the potassium topic: I don't think that in your Fig 2A is accumulation rate: I am not sure but I think you reported ice annual thickness ??? Anyway, that does not fit with the plots of Schüpbach et al. (see figure 2 below).

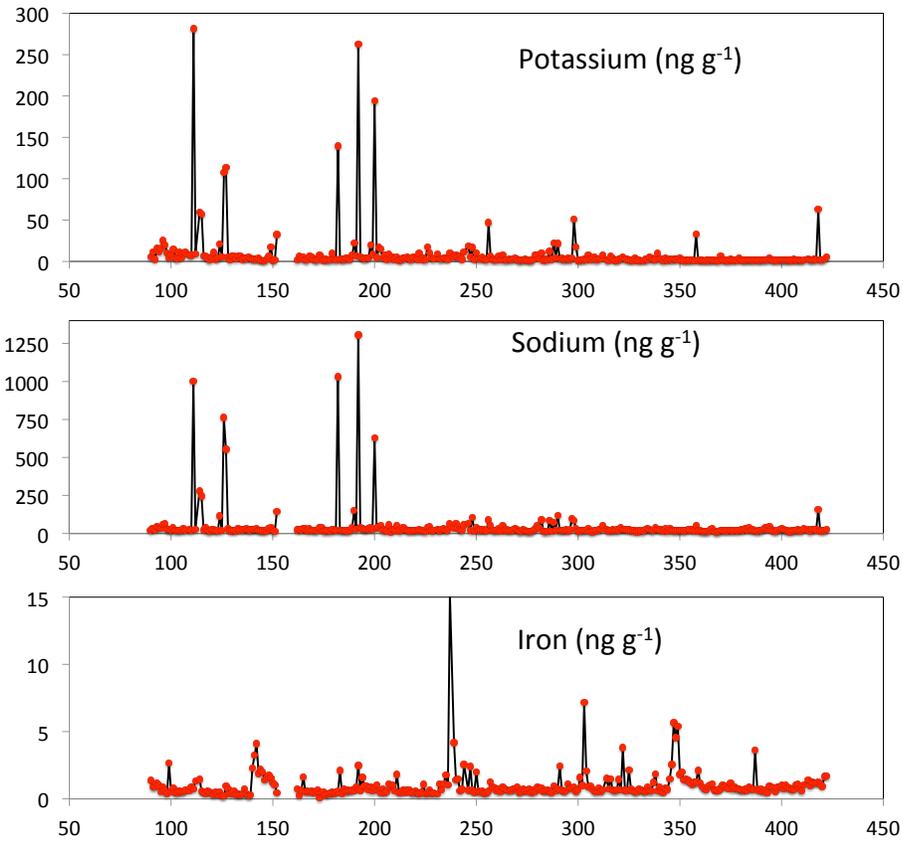
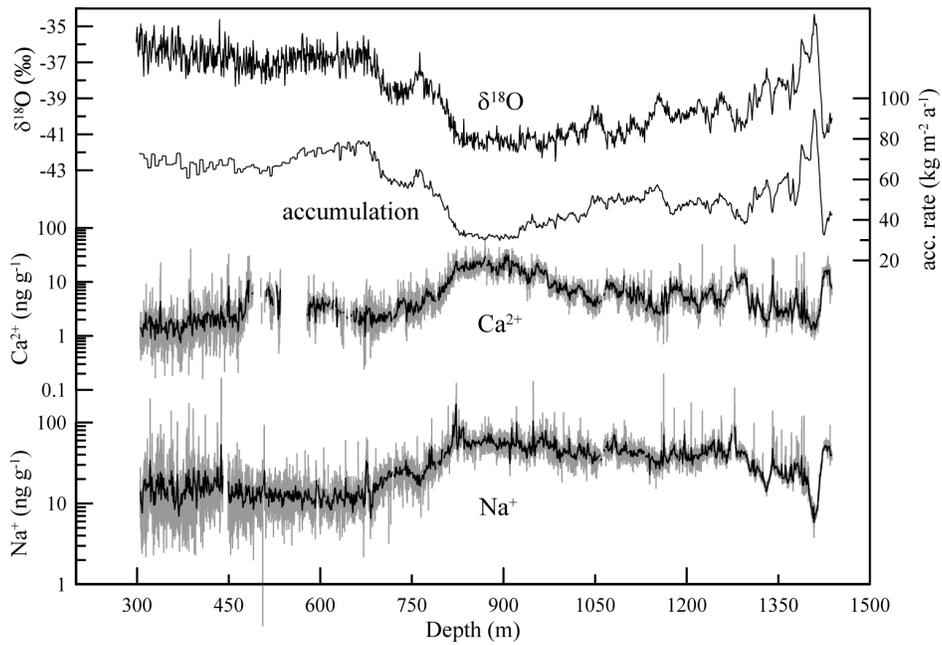


Figure 1 (your data)



From Schüpbach et al 2013 (Figure 3)

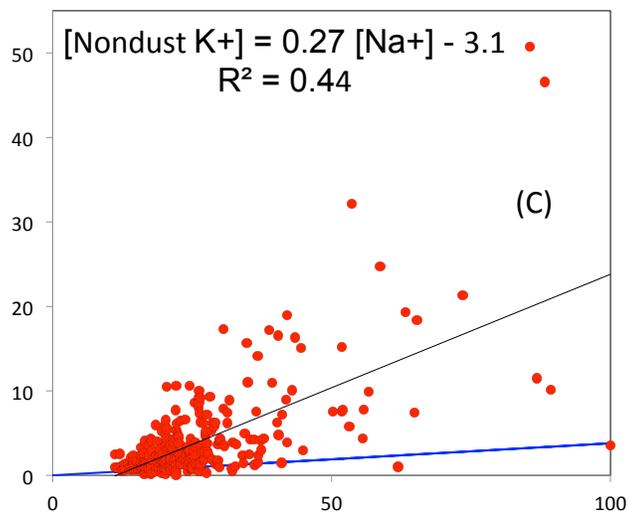
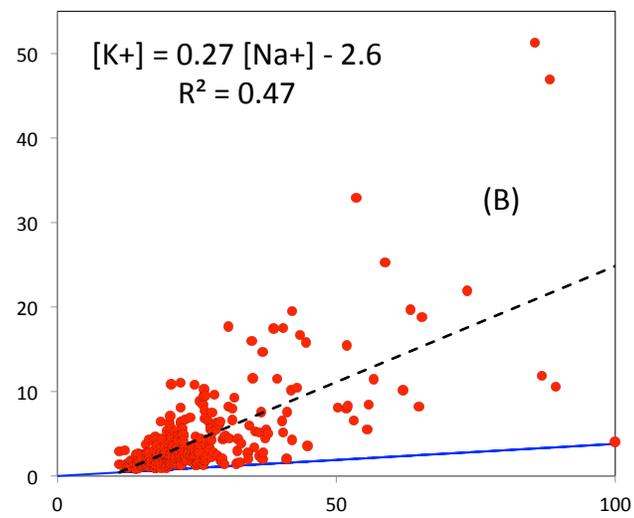
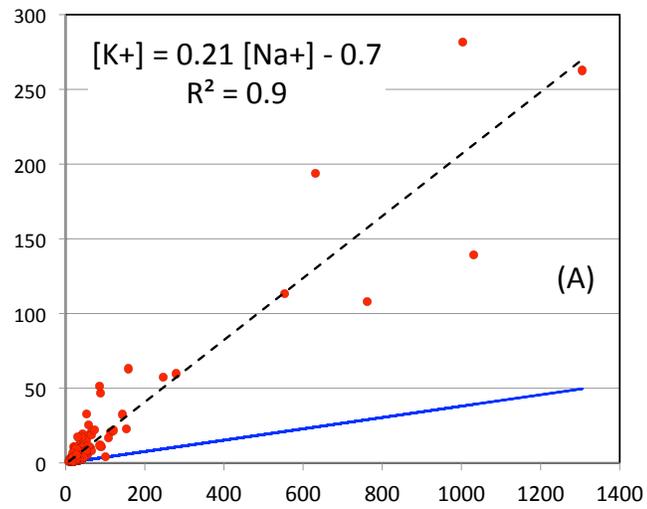


Figure 3 (your data)