This paper presents the application of a new high-resolution (~200 µm) analytical technique to a period of abrupt climate change in the NGRIP ice core (actually a 2.85 m section representing about 250 years). I actually find this paper quite hard to review: on the one hand the technical achievement is good and worth documenting. On the other hand what we learn from it is minimal, and there are many more interesting things the authors could have done. I therefore think the authors have two choices. One alternative is that they should shorten the paper and just present it as a proof of concept. The other is that they should add to it – possibly involving new analyses but certainly new data treatments, to try to give new insights into what benefits such a technique might bring.

The positive part is that the authors have successfully used laser ablation to determine 5 elements at 200 µm resolution. They describe the way they cleaned the samples (partly with the laser) and the novel way in which they produced quasi-homogeneous standards. I congratulate them on this.

The headline findings from the study are not new: that dust elements change very rapidly (annual scale) at the start of a D-O event (this was already said as far back as Fuhrer et al 1999), and that they appear to change before the water isotopes (already covered by Steffensen et al and Thomas et al). Sure, this is the oldest section on which such a finding has been confirmed, but as only one event is studied it just adds an example rather than offering a generalisation, and certainly doesn't provide evidence to make new ideas about the mechanism. Of course, this is not the authors' fault. On the other hand they could have taken the opportunity to really discuss what the advantages and drawbacks of such high resolution might be. I can suggest several lines of study they could have taken:

1. An obvious issue is how reproducible the data from such narrow tracks are. The authors say they ran parallel tracks but then do not show us the data so we can assess. I don't know how far apart the tracks were, but parallel tracks across the core at cm distances would have given a crucial clue to reproducibility, which in turn would allow a conclusion as to whether the advantages of high resolution are real (providing evidence of climate variability) or illusory (providing evidence of depositional noise).
2. A second issue concerns diffusion. It is generally assumed that water isotopes diffuse a few cm in the firn and then also in solid ice, sulfate peaks appear to diffuse, while dust probably does not diffuse. What about these elements? Here are data apparently showing the retention of mm scale structure at 80 ka ago. This is interesting in its own right and would be even more so if compared to the structure at the start of DO events in the younger part of the record. It might even have been possible to derive diffusion coefficients, which might be crucial when investigating even older ice (eg in Antarctica).
3. What is this method actually analysing and how does that compare to what CFA and IC measure? We are shown a comparison only for Na (not counting dust which cannot be compared quantitatively). Why? This seems crucial and even if the data are not yet available from the CFA for eg Ca (which is odd if Na has been measured), it would have been trivial to prepare a few 1 cm samples for IC analysis. This seems critical because Fig 5 seems to show unexpectedly poor agreement for Na, which certainly needs discussion. But in general the consideration of whether this method measures more of the insoluble component than CFA/IC would have been an important analytical discussion that could have been included.

I will discuss a few details below, but as already outlined, the issues above could be discussed; if the authors prefer not to then the paper should be cut back to an analytical proof of concept.

Detailed comments:

Page 2, para 1. You seem to come down on one side of an ongoing discussion about whether the cold period enhancement is mainly due to increased transport or to the presence of a sea ice source. It would better reflect the science if you left that open.

Page 2 line 23. It gives a misleading impression to state that rge resolution is "nominally…weekly" because precipitation intermittency and snowdrift mean that weekly resolution is certainly not available. I suspect you know that with your use of the word "nominally", and you should explain that.

Page 3, line 1. You say that the section "covers" GI21.2, and then give an age range of 370 year (84.70-85.07 ka) for that. But in the abstract you refer to it as a 250 year section, even though Fig 1 shows that it is actually wider than GI21.2. This is incompatible – please correct.

Page 3, line 16. Sorry to be picky but you cite Fig 5 before Figs 2-4.

Eq 1 and line 25 is confusing. If I understand it m_i is the slope of intensity vs time, whereas your wording made me think it was the slope of the calibration (intensity vs standard concentration). Please clarify. I think Fig 1 would be better shown as linear rather than log plots, as the log plot hides the extent of the drift.

Page 4, line 25. Is this the R^2 of lin-lin or log-log plots? You show log plots but then describe it as a linear regression. Please clarify.

Page 4, line 31. Here is where you say you analysed two parallel tracks to assess reproducibility but then you never do so.

Results, page 5-6, seems repetitive (last para page 5 and first para page 6). Combine them into something clearer?

Page 6, line 9, should be Figs 6 and 7 not 8 and 9.

Page 6, data comparison, lines 27-32. It is clearly not true that Na is comparable between the two techniques. While they match OK at 2689.7-2690.0, they are at least a factor 3 off in the shallower section. This needs a better and more correct discussion. (And of course I would like to see the same for Ca).