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Interactive comment on "Black carbon concentrations from a Tibetan Plateau ice core spanning 1843–1982: recent increases due to emissions and glacier melt" *by* M. Jenkins et al.

Anonymous Referee #2

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This manuscript presents the first multi-decadal record of black carbon (BC) in an ice core from the Central Tibetan Plateau. The dataset itself is novel and should be published, though the authors describe numerous problems with the measurements that significantly reduce the utility of the analysis. Much of the discussion (especially section 3.3) is quite speculative in nature, and the argument that melt has caused recent multi-annual increases in BC concentration is not well supported. The paper is well-written.

Major issues:

The argument that recent (post-1940) increases in BC may be caused by increased

glacier melt is not convincing to me. Melt-induced enrichment of BC seems to occur either near the surface of melting snow or at the top of superimposed ice layers that formed during the previous melt/freeze cycle. In either case, the annual-mean BC concentration in the ice should reflect the total annual BC deposition, regardless of the amount of melt that occurred (assuming the melt did not run off). Exceptions to this would occur when substantial net ablation causes either (1) a decrease in the H2O mass of the column via runoff (hence increasing the column BC concentration), and/or (2) merging of deeper layers of enriched BC. (1) should manifest as an apparent decrease in net accumulation, which was not discussed. (2) seems to be discounted by the authors in section 3.5, where it is speculated that they should have seen enriched BC concentrations near the top of their ice core if multiple years of BC deposition had coalesced. Moreover, if (2) is the reason for increased post-1940s BC, it implies that BC deposited prior to 1940 became entrained in the post-1940's ice. If multi-decadal mixing of BC occurred in this core, then no meaningful inference of BC deposition trends can be made. Finally, one other potential reason for a measured multi-decadal increase in BC concentration could be that the annual sampling only occurred in melt-affected ice (e.g., right above each year's superimposed ice). However, the authors state that the ice was re-sampled to 3 samples per year, thus providing, I expect, a reasonable estimate of annual-mean BC concentration.

Large sources of uncertainty in the measurements are described in Section 2. Perhaps the most important of these originates from the amount of time that the samples spent in a melted state prior to measurements. The authors state that in a previous analysis of samples that had been melted for some time (Menking et al, 2013) "... the relative difference between high and low concentration samples was greater in the freshly melted samples than the stored liquid samples, confirming our suggestion that increase factor analyses will underestimate actual concentration changes for samples that have been stored in the liquid phase." (p.4860,line 13). This seems critical for the main conclusion of the study, which is that mean (median) BC concentrations in this region increased by a factor of 2.0 (2.4) in recent decades (abstract). By how much did

the "relative" differences between high and low concentration BC samples change with melting in Menking et al (2013), and how could this information be used to provide an improved estimate of the factor-of-increase in BC derived in this study?

In general, the measurement uncertainties and their impact on the inferred factor-of-increase in BC concentrations need to be acknowledged in the abstract.

Minor issues:

p.4858,6: Is it known that refreezing, as opposed to melting, of the samples resulted in the apparent reduction in BC?

p.4858,15: "acidification can cause a shift towards smaller particles (Schwarz et al, 2012)." - Does this have any direct impact on inferred mass concentration, or only particle size? Does it perhaps increase the measured mass concentration by increasing the fraction of BC mass that is nebulized?

p.4859,23: "Measured BC concentrations decreased until 55 days." - By how much (what range) did they decrease by?

p.4860,2: "However, the difference observed between low and high BC concentration samples is less than was likely preserved in the un-melted ice." - Perhaps I misunderstand this, but this statement seems inconsistent with the statement two sentences earlier that "BC losses are proportionately greater in low concentration samples relative to higher concentration samples."

p.4860,14: "... confirming our suggestion that increase factor analyses will underestimate actual concentration changes for samples that have been stored in the liquid phase." - This should be included in the abstract, if the reasoning holds.

Section 3.3 - This section came across as somewhat "wishy washy", and I am not sure it adds much to the paper, especially in light of concern about the potential role of melt in causing a multi-decadal increase in BC concentration. I suggest trimming this section down or merging it with section 3.5, which seems to concisely summarize the

issue of increased melt.

p.4867,17: "supports that" - reword for clarity.

Table 2: "...concentrations reported here have been corrected for nebulizer losses based on Aquadag standards." - How much of a correction of data from Kaspari et al (2011) did this result in? (This could be added either here or in the main text).

Interactive comment on The Cryosphere Discuss., 7, 4855, 2013.