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Atmospheric Measurement Techniques Discussions

Interactive comment on "Assessing recent measurement techniques for quantifying black carbon concentration in snow" *by* J. P. Schwarz et al.

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This is important work and definitely worthy of publication. My main concerns are related to the extrapolation of these specific results and findings to the results of other groups using similar but not identical methods. For example, all of the SP2-based measurements reported in Schwarz et al. are on discrete measurements but they extrapolate their findings to "current estimates of BC mass concentration in snow and ice." However, many (all those from my group at least) measurements in firn and ice are made using continuous flow techniques where sample handling is completely different than for discrete samples. This is important because much of the uncertainty described by Schwarz et al. comes from how the BC particles behave in the sample vessel (Do they form aggregates? Do they stick to the walls of the vessel? What is the impact of temperature and pH on the particles after melting but before analysis? How much do the particles change with time after melting?). With continuous flow such as ours for example, (1) the sample stream is in a closed system and so cannot interact with the lab air (so there are no increases in carbonic acid and no pH changes); (2) the sample is analyzed about 4 minutes after melting and this time period is constant; (3) the sample is only in contact with Teflon tubing and peristaltic pump tubing before reaching the USN. We routinely measure replicate longitudinal samples from our ice cores for quality control and we find that both the magnitude and especially the variability in time and space of the BC concentration is extremely repeatable. This strongly suggests that while recovery in continuous flow systems may or may not be 100%, the "errors" have to be very, very consistent.

Another concern is the generalization of the USN nebulization efficiencies. We too have tested the nebulization efficiency of our USN and we find the efficiency to be nearly flat over the range of particle sizes expected for BC (150 to 450 nm). Operational settings can influence the nebulization efficiency so I would encourage Schwarz et al. to be careful about over generalizing their findings.

Much of the uncertainty that Schwarz et al. conclude is inherent in SP2-based measurements in "snow and ice" is from large aggregated particles that fall outside of the measurement range of the SP2 or because of changes in the nebulization efficiency of the USN. A major concern is their treatment of "snow and ice" as being the same everywhere. This is very misleading since snow close to burning sources undoubtedly has much large particles in it than snow and ice far removed from burning sources (e.g., the Arctic and Antarctic where the nearest burning sources are hundreds to thousands of km away). Much of the uncertainty that Schwarz et al. determined was based on measurements of seasonal snow from near Boulder CO and a few seasonal snow

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samples from the Arctic. The former are likely dominated by very local burning sources and so have lots of larger BC particles. The latter samples were melted and refrozen before analysis and so nearly worthless. Therefore, while BC measurements in snow close to burning sources may have the level of uncertainty determined by this study, measurements on the polar ice sheets very likely have far less uncertainty.

Modifying the text to make the conclusions and findings less general would be trivial but definitely would strengthen the manuscript in my opinion.

The authors need to go through the manuscript to make sure all the citations are in the references. I noticed that at least one of the citations in the text was not in the reference section.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 3771, 2012.