

Interactive comment on “Variability of black carbon deposition to the East Antarctic Plateau, AD 1800–2000” by M. M. Bisiaux et al.

Anonymous Referee #1

Received and published: 22 December 2011

This manuscript presents and analyzes black carbon records from six ice cores collected on the East Antarctic Plateau. This is a companion paper to another submitted to ACP by Bisiaux et al. As with the other paper, this study is valuable because it adds to a very small collection of black carbon records from Antarctica. Moreover, the records are about as high of resolution (~annual) as can be expected, given uncertainty in accumulation and dating. The records span about two hundred years (1800–2000), and are thus relevant to understanding variability in both fire- and fossil-derived aerosol emissions on different timescales. This study also includes interesting analysis of the influence of elevation, snow accumulation, and large-scale transport/emissions on variability in ice core concentrations.

Although valuable, I think this study would be better if it were merged with the com-

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panion paper as a single study. The papers share common analytical (SP2 measurements) and statistical (e.g., coherence calculations) methods, and both manuscripts present black carbon data from Antarctic ice cores. Table 1 even presents results from both papers. I believe the coherence/ENSO analysis presented in this paper would be particularly useful to readers of the other paper. Some of the questions that were raised by reviewers of the companion paper regarding ENSO analysis are answered in this paper. Moreover, I don't think either paper is sufficiently lengthy such that that a combination of these studies would have produced an excessively long paper, and the combined paper would have eliminated some redundancy in methods description.

General issues:

I thought the application of statistics was a bit opaque, but this might reflect my lack of familiarity with some of the statistical techniques and software packages applied. A bit more detail on the interpretation and methodology of statistical results may improve the manuscript for general readership.

More specifically, I felt the discussion on non-linear trends was confusing. Can the Z-scores shown in Fig. 3 be interpreted as a timeseries of the normalized first-derivative (trend) of the rBC concentrations? Or, do these curves reflect smoothing of the raw data? If the former, over what time-scale are the trends shown in this timeseries computed? Is it fixed, or does the time-horizon of trend analysis vary? (The answers to these questions are probably embedded in the Methods statement that non-linear trends were calculated with "singular spectrum analysis using Kspectra software". Please provide more detail for those unfamiliar with this technique. i.e., Which "singular spectrum" was used for Fig. 3?)

Related: p31098,24: "Significant non-linear trends are shown in Fig. 3" - Over which time period(s) are these trends significant? Is there any of way of determining from Fig. 3 that these trends are significant? (i.e., does a particular Z-score on these plots

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define a threshold for significance?)

Specific comments:

abstract, 12: The units of flux should be $\mu\text{g}/\text{m}^2/\text{a}$, rather than $\mu\text{g}/\text{kg}/\text{m}^2/\text{a}$, I assume.

p31092,21: "low albedo light absorption properties" - Reword.

p31092,24: "prediction of future rBC emissions is therefore a key parameter..." - I suggest rewording this and maybe including "incorporating scenarios of future emissions", or something similar, instead of "predicting", as predicting human behavior is challenging.

P31093,12: I don't think these studies actually "reconstructed" past combustion.

p31095,20: Why was mapping between the cores needed, if nssS was measured in each core? Is it because annual layer counting was only conducted on the WAIS core? How many total volcanic events were used for the dating?

p31096,16: What is the resolution of the snow accumulation rate analysis from Anschutz?

Related: p31097,9: Why were "longer term average accumulation" estimates used for derivation of the fluxes? Is it because of temporal uncertainty in the rBC concentrations?

p31096,24: Why was 21 years chosen as the period for smoothing?

2.4.3 heading: I suggest renaming this heading to "Deposition fluxes".

p31097,21: Why was 0.4 yr chosen as the timestep for this analysis? Are results sensitive to this choice?

p31099,8: "Finally, the last 20yr (1980-2000) show an increasing trend..." - Fig. 2 shows that 3 of the records terminate before 2000.

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p.31100,11: "wet removal processes limit the lifetime of rBC near the boundary layer..." - The next paragraph seems to suggest that dry deposition plays a leading role at higher elevation, so this statement was a bit disorienting for subsequent discussion. Is this statement derived from Schwarz et al? Does it only apply to boundary layers over low-altitude terrain where precipitation is greater?

p.31100,14: " $r^2=0.72$ " - This appears to be inconsistent with the legend of Fig. 4c, listing $r^2=0.74$ for 1963-onward.

p.31102,1: "... the direct comparison of rBC with ENSO index is not significant..." How is this known? Was this analysis conducted, but not presented elsewhere in the paper? If so, I suggest briefly mentioning this.

p.31102,2: "records do not have temporal resolution to adequately resolve the signal..." What resolution would be needed to resolve this signal?

p.31102,6: I don't think "NT" has been defined.

p.31102,14: "... increased emissions from fire... during decadal time periods dominated by La Nina." - I think this is an important conclusion. Please mention which regions have greater fire emissions during La Nina periods.

Conclusions: "On the Antarctic Plateau, rBC may on the contrary be linked to Atlantic-sector cyclonic activity." I don't recall this being discussed elsewhere in the paper. If not, please either include more discussion of this inference in the Results section, or exclude it from Conclusions.

p.31103,11: "ENSO-long term emissions" - Clarify to "emissions associated with decadal-scale ENSO variability" or something more clear/precise.

Table 1: Please make the text larger for the print version.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31091, 2011.