

Anonymous Referee #1

We wish to thank the referee's for their time and effort reviewing the paper. Please see our response to their comments below.

General comments

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 companion 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 a combination of these studies would have produced an excessively long paper, and the combined paper would have eliminated some redundancy in methods description.

1-1 Response:

We initially sought to combine the papers, but soon realized that the paper would either be too long or wouldn't properly describe the sites. Given that these records are the first of their kind from Antarctica (and the Southern Hemisphere) and that the sites are extremely different we felt that papers needed to be separated, but published together. An important difference between the papers is that the first paper deals with high resolution coastal sites, with sub annual to annual resolution, while the second paper focuses on low temporal resolution sites located inland on the East Antarctic plateau. In many respects the sites couldn't be more different (wet and dry deposition, atmospheric circulation etc.). In addition the papers don't focus on the same period (1850-2000 for companion paper, 1800 and up to 1980 – 2000 for this paper). Lastly, we added a discussion on ENSO and the coherence analysis to the companion paper, which, does not easily fit within the current paper or vice versa. While, the suggestion to combine the papers is well taken we still believe that the papers are more readable and logical when read separately.

General issues:

1. 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.

1-1 Response:

We have rewritten the initial statistics description in the concentration paragraph in section 2 for clarification. This includes further references for kernel density estimates etc. If necessary this can be further expanded, but at the risk of making the text inaccessible to the general readership.

2. 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?).

1-2 Response:

We have rewritten this section. The non-linear trend reconstructions are from the combination of significant oscillatory modes (single spectrum analysis) determined to also represent significant trend components (in log-space) based on the Kendall significance test at 95%. These components were reconstructed from linear combinations of the corresponding principle components (Ghil et al., 2000). In essence this is a partial reconstruction of the time series using the trend principal components. The back transformed data (from log-space) was then normalized (Zscore) to isolate the non linear trend variability from its magnitude for site to site comparison. We have rewritten this section and included further references for the audience not familiar with single spectrum analysis. Also, see response to **comment 1.3**. We tried to improve the description of non-linear and linear/monotonic trends.

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 define a threshold for significance?).

1-3 Response:

Our calculations were performed on the whole record period from 1800 to 1989-2008 depending on the ice core. This was specified by the wording "from 1800-onwards". We modified the sentence to make this point clearer. There is no particular threshold indicating that the trends are significant, however the non-linear trends are defined as significant by the Mann-Kendall significance test at the 95% level, which we find good enough for non-linear trends. The calculation of the Z-scores was performed to be able

to compare the variability of one record to another more easily, but it doesn't determine by itself if the trend is significant or not.

We modified the sentence in 3.2 "concentrations" to make this point clearer:

"Monotonic trend analysis was performed in log-space using the non-parametric Mann-Kendall test (Önöz and Bayazit, 2003; Mann, 1945), while non-linear trends were calculated with singular spectrum analysis using Kspectra software (Ghil et al., 2002) at a 95% Kendall level of confidence. Significant non-linear trends were reconstructed using Kspectra back-transformed from log-space and normalized (Z-scores calculated using the arithmetic mean and standard deviation of the complete trend reconstruction), for easier comparison of variability between records."

Specific comments:

4. abstract, 12: The units of flux should be ug/m2/a, rather than ug/kg/m2/a, I assume.

1-4 Response:

Yes this was a mistake we have corrected the units.

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

1-5 Response:

We replaced by "high light absorption properties".

6. 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.

1-6 Response:

Good point. We modified the sentence as: *"The incorporation of future emissions scenarios is therefore a key parameter for global climate modeling research."*

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

1-7 Response:

We replaced reconstruct by "estimate": *"Recently, several studies have directly used rBC from ice cores to estimate emissions from past combustion spanning the transition from the preindustrial to the modern era."*

8. 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?

1-8 Response:

Yes, annual layer counting wasn't possible on the NUS cores. So we used mapping of volcanic horizons to tie the records together (7 tie points were used for dating: 1810, 1816, 1837, 1863/4, 1884, 1965, 1991/2, added to manuscript). We added this sentence to the dating paragraph to make this point clearer: "*Annual layer counting was not possible for those NUS cores*". We also tried to make clearer the whole dating paragraph in section 2.

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

1-9 Response:

The resolution is shown in the Table 1: e.g.: 52.0 ± 2.0 kg/m²/a for NUS07-1, since 1815, and 55.9 ± 3.9 kg/m²/a since 1963 etc...

10. 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?

1-10 Response:

Annual layer counting wasn't possible for those records, notably because accumulation uncertainty associated with surface processes (wind blowing...) is too high in proportion to annual accumulation. Thus, the calculation of average flux is more robust when we use an average, obtained through the dating of volcanic eruption and up and down extrapolations.

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

1-11 Response:

We choose 21yrs because it was the longer average we could use that was still capturing the increased concentrations at the end of the records (notably in Nus 07-1, 07-2, 07-7, 08-4) and also the ~40yrs periodicity, notably in the cores 07-1, 08-4 and 08-5, and we choose to keep the window for all records. This sentence was added to the text: "*The time window used for the smoothing represents a trade-off between, the temporal resolution of the different records and the time span of the records, and variability in the later half of the 20th Century.*"

12. 2.4.3 heading: I suggest renaming this heading to "Deposition fluxes".

1-12 Response:

This change was made accordingly.

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

1-13 Response:

We choose 0.4yrs to limit the noise for spectral analysis, but to keep an eventual annual cycle. We included a sentence to the text: *"Here we use raw-data re-sampled to a 0.4yr step with piecewise linear interpolation to perform the calculations, in order to remove lower frequencies but to keep the eventual annual cycles"*.

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

1-14 Response:

The complete sentence was "Finally, the last 20yrs (1980-2000) show an increasing trend for all cores recording this period, ...". We replaced "all" by "the", to make the sentence clearer.

15. 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?

1-15 Response:

Yes, we assume that relative enrichment of rBC in the upper troposphere is linked with wet removal at lower and wetter terrain of the rBC particles (Stohl 2006 paper). Thus, we suggest that if the snow is enriched in rBC at the NUS sites, it may be linked to inputs from higher altitudes rather than from low altitude air masses that could have been lifted up from the coastal areas to the plateau.

16. p.31100,14: "r2=0.72" - This appears to be inconsistent with the legend of Fig. 4c, listing r2=0.74 for 1963-onward.

1-16 Response:

We corrected the text to r2=0.74.

17. 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.

1-17 Response:

This wasn't mentioned earlier, thus we modified the sentence as: "*However, even if an ENSO "signature" is present, none of the rBC records is statistically correlated to the ENSO index, which may be explained by two reasons.*"

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

1-18 Response:

Since ENSO as a returning time of about 2 to 8yrs, we would need records that have better temporal resolution in this time window (at least a bi-annual resolution).

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

1-19 Response:

The complete words (non-Transport) were added to the acronym.

20. 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.

1-20 Response:

We specified "la Niña, (colder, wetter in the Western Pacific) and El Niño (warmer, wetter in the Eastern Pacific).

21. 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.

1-21 Response:

The sentence was removed from the text.

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

1-22 Response:

We replaced the sentence by the suggested one.

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

1-23 Response:

We will make sure to ask the editors for larger text.