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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Drought-induced biomass burning as a source of black carbon to the Central Himalaya since 1781 CE as reconstructed from the Dasuopu Ice Core" *by* Joel D. Barker et al.

Joel D. Barker et al.

barke269@umn.edu

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We thank Referee #1 for their thoughtful and insightful comments. We have responded to their comments in the manuscript as described below.

We appreciate Referee #2's point regarding presenting data vs depth before presenting data vs time. However, the rBC data, for example Figure 2a, is presented using the Thompson et al (2000) time-depth chronology that was established using d18O, dust, and NO3- measurements and annual layer counting confirmation using the location of

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the 1963 CE beta radioactivity peak, and further calibrated using 2 major monsoon failures at 1790-1796 and 1876-1877 as benchmarks (L156-164). This chronology is used in Thompson et al (2000). Data in this publication are presented relative to time (years AD), and not depth. We have adopted the same convention.

Fig 2a presents raw data and 5 year median data. We do not specify that these are annual averages.

The point that "only averaged data can be interpreted with respect to signal intensity" is well taken and something that we considered during the data analysis. However, we purposefully performed the spectral analysis using individual samples rather than annual averages for precisely the reason that the Referee mentions. By inputting data as dimensionless samples (with respect to time, depth, annual layer thickness, sampling resolution) in chronologic order, we are a) not introducing artifacts due to variable sampling resolution and annual layer thickness; b) we preserve the signal of sudden increases and decreases in rBC concentration that is inherent in the dataset and is an important feature of the rBC record (this information is greatly muted using annual averages). We agree that the principle of spectral analysis is that values represent similar intervals, and we suggest that these intervals need not be "time" as suggested by Referee #2, but instead that the intervals can be similar entities (for example samples, or rBC concentrations), as we've done here.

Referee #2 states that they are "surprised that BC was purely interpreted as emitted from biomass burning. Potential contributions from combustion of fossil fuels were not discussed". We thank the Reviewer for their perspective, but we believe that we do not discount a potential contribution to the rBC record from fossil fuels, but rather show that a significant contribution from this source is not supported by available trace element data and rather aligns strongly with records or regional drought and, by extension, biomass burning. Most other studies examining BC records from ice cores in the region find that contributions from fossil fuels increase in the 1970s, which is a time period that is not well resolved in the Dasuopu BC record. We simply point out that the

trace element and drought indices suggest that biomass burning may be an important source of rBC. We agree that our presentation is descriptive and we value Referee 2's suggeswion that we use a "more robust statistical approach" to support the correlation between regional drought and periods of high rBC concentration in the Dasuopu core. Unfortunately, in the time provided, we have been unable to conduct an analysis such as a spatial correlation analysis between the regions described in the PDSI maps and the Dasuopu glacier drilling cite. However, we would like to point out that we provide evidence from a trace element record, as well as 3 independent climate records, so support our conclusions that rBC may be associated with dry conditions and associated biomass burning events.

We agree with Referee #2 that a direct comparison between the rBC record presented here for the Dasuopu ice core and the cores presented for East Rongbuk glacier by Ming We agree with Referee #2 that a direct comparison between the rBC record presented here for the Dasuopu ice core and the cores presented for East Rongbuk glacier by Ming et al. (2008) and Kaspari et al. (2011) is an excellent idea, we are limited by the discontinuous sampling of the firn layer of the Dasuopu core that spans the time periods presented by the East Rongbuk cores. We do provide a qualitative comparison on lines 303-305 where we state that "The discontinuous firn section of the core has elevated concentrations during the late 1960s – 1970s, consistent with observations from East Rongbuk glacier by Ming et al. (2008) and Kaspari et al. (2011) , and for Tanggula glacier by Xu et al. (2001)." This comparison, as well as a comparison with other glacier cites is shown in Fig. 1.

Referee #1 also questioned whether we had assessed BC particle loss during sample melting. I have repeated our response here: "No, we are unable to estimate the portion of BC that could potentially be lost during sample melting in a polyethylene sample bag. However, Wendl et al. (2014) report that no significant BC particle loss occurs until $\sim$3 days of storage in polypropylene vials at room temperature, and that there is less adherence at cooler temperatures. Lim et al. (2014) confirm these results

and indicate that melting at room temperature is preferable to melting in a warm bath. Meinander et al. (2020) suggest that some EC adherence to polyethylene sample bags may occur, but that EC heterogeneity in the sample (in this case snow) exceeds any particle adherence that may have occurred.

Our ice samples were melted over the course of no more than an hour (less than 3 days) and our samples did not reach room temperature prior to transfer to the polypropylene vial followed by sonication. While we cannot rule out the possibility of particle adherence to the polyethylene sample bags during melting, we suggest that any adherence would be minimal because of the short melting period (< 1 hour) at cool temperatures (< room temperature). Further, we suggest that the extent of any possible particle adherence would be similar between individual samples because all samples were melted in an identical manner."

We appreciate the suggestion that we cross check the annual layer counting with seasonal rBC increases in the Dasuopu rBC record. This is something that we attempted during our data analysis. In the end, we found that the presence of missing rBC samples (as described in the manuscript) introduced error to the cross-dating effort, rendering it unreliable.

L 57: Thank you. We have changed "European Industrial Revolution" to "Industrial Revolution".

L 99: Thank you, we have removed the false statement that EC is a form of BC.

L118: We explain that the importance of the Dasuopu core being from high elevation is that it allows us to sample from the free troposphere, distant from local sources of BC contamination (lines 126-134).

L 127: Implicit in the term "free troposphere" is that we are not influenced by sources of local BC contamination. The drill site on Dasuopu glacier is not influenced by down-valley meteorological conditions, as described by Li et al (2011) and cited in

the manuscript). While not measured for this location specifically, and so not noted in the manuscript explicitly, the free troposphere in the central Himalaya begins at ∼2.5 km elevation in the winter and 3.3 km elevation in the summer (Solanki and Singh, 2014). We have added this sentence for clarity (Line 135): "Generally, the lower limit of the free troposphere in the central Himalaya occurs at ∼2.5 km in the winter and 3.3 km in the summer seasons (Solanki and Singh, 2014)."

L 136: Thank you. We have added this reference to line 138.

L 137: By "sub-annual resolution" we mean that we were able to obtain multiple samples per year of accumulation. We have replaced "sub-annual" with "seasonal" for clarity.

L 152: Information regarding the recovery of Dasuopu core 3 is described in detail in the Thompson et al (2000) publication, that is referenced in the manuscript. Thus, we believe that repeating this information is unnecessary and beyond the scope of this manuscript. Likewise, as described my Thompson et al (2000), the chronology was developed for core 3 (used here).

L 174: We're not sure that we ever used the term "ultra-pure water". We do write MQ water, and have replaced this initial description with "type 1 Milli-Q water" for clarity.

L 184: Thank you. We have added "2N" descriptor to HNO3.

L 186: Thank you. We have replaced the brand "Ziploc" with "polypropylene".

L 249-258: Yes, this is the data that Gabrielli et al (2020) used, and we have clarified this by writing "using methods described in Uglietti et al. (2014) and reported by Gabrielli et al. (2020)."

L 261: Thank you. We have replaced "geomorphology" with "topography".

L 286: We think that this is a good suggestion, but at this point, we think that the effort involved to determine how much snow was deposited during the monsoon for the 64 m

ice section of the core analyzed here for a piece of information that doesn't contribute strongly to the findings of the manuscript is unwarranted.

L 289: Our mis-use of the term "deposition" throughout the manuscript was noted by Referee #1 as well. We have replaced "deposition" with "concentration" throughout the manuscript.

L 289-291: The point that we approach the relationship between d18O content, dust, and rBC in a qualitative way for discrete intervals in the core was noted by Referee #1 too. We respond to their, and your observation as follows: "manuscript. It is difficult to demonstrate a process occurring at an annual or seasonal scale over the broad period covered by the ice core. This is why we chose 3 intervals to highlight the relationship between isotopic composition, dust concentration, and rBC concentration. In an effort to show this relationship over a broader analytical "window" we have included a new analysis using spectral coherence of rBC concentration and d18O over a $\sim$50m section of the ice section. This analysis shows the strength of correlation between d18O and rBC concentration at multiple period scales as well as any phase lag in this correlation. We hope that this proves to be a more effective way of showing the seasonality of rBC concentration though this section of the ice core."

L 551: Thank you for noting that omission. We have corrected the sentence as per your suggestion.

Fig 2: The secondary x axis (the 5 year median scale) is different than the rBC scale on the primary axis. I believe that this is the source of the confusion.

Fig 5: Thank you for pointing this out. We have added the following to the Figure caption: "Red and blue indicate a higher frequency and lower frequency air mass flow paths, respectively".

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