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Interactive comment on "High latitude Southern Hemisphere fire history during the Mid-Late Holocene (750–6000 yr BP)" *by* Dario Battistel et al.

Dario Battistel et al.

dario.battistel@gmail.com

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Anonymous Referee #1 This manuscript reports on levoglucosan and the calculated fine (i.e., non sea-salt and non dust) potassium fraction measured on sub-samples from a Talos Dome (Antarctica) ice core spanning the second part of the Holocene. The aim of the work is to reconstruct past fire activity in the southern hemisphere. The data presented in the manuscript possibly contain information in view to better understand past variability of fire activity. This topic is clearly relevant for Climate of the Past journal. As it stands the manuscript, however, requires major revisions on several key aspects and a re-evaluation prior to publication. Reply: Thanks for your review and your observations that are surely useful for improving the quality of the paper. We substantially revised and rewrite the paper following your indications. We reply to specific

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Discussion paper

comments below. RC1 First, the wording “We reconstructed the high latitude Southern Hemisphere fire history by using the specific biomarker levoglucosan and potassium in ice cores from the TALDICE during the Mid-Late Holocene (750-6000 yr BP) is clearly an overstatement since your data are discontinuous (only 15% of the ice core was measured). Please, make this point more clear in the manuscript and change the wording in your discussion. Reply: Surely our data are not continuous. As you underlined, we analyzed the uppermost part (15 cm) of each 1 m section as reported in the experimental section. In this view some fire peaks can be missed. We tried to be more precise in this sense adding a sentence (P.4 L.7 of the revised version), where we stated that: “The samples therefore have the potential to miss fire peaks due to their discontinuous nature”. RC1 Second, I would like to see in the manuscript the raw data (concentrations and not only calculated deposition flux) for levoglucosan and potassium. If my estimate is correct, you calculated fine potassium concentrations in the order of 0.2 ppb. I strongly recommend to the authors to show concentrations of potassium, sodium, and iron and error propagation estimates. Only with that the reviewers (and the readers) can evaluate to robustness of the potassium approach. Reply: In our view, adding concentration values in the manuscript may potentially burden the paper. To this purpose, we opted to include the concentration plots in supporting material and we’ll add a raw data file (rawdata.xls), where original values (concentration) are reported for the readers and the reviewers. Error propagation was estimated using propagation error formula (that will be reported in Supporting Material). We noticed also a typo in the graph, where Kbb flux was reported in μg m-2 y-1, instead of mg m-2 y-1, thus probably arising your doubt about original concentration values. Fig. 2 strongly changed, also following the suggestions of the reviewer #2 and we corrected the typo in the units. RC1: Also concerning the discrepancies between levoglucosan and potassium: line 8 page 7: what other sources for potassium in the pre-industrial atmosphere (apart from sea-salt and dust) you have in mind here ???? Reply: Well, we don’t have in mind other sources of potassium in the pre-industrial atmosphere. What we wanted to underline is that when disentangling the different contributions, you make an assumption

(i.e. terrestrial and marine composition is constant). Terrestrial sources, in particular, can change, where local or regional sources may affect your assumed composition and, in turn, your record, leading to anomalous values. We tried to clarify this point at P. 7 L. 21 in the revised version where we stated that "Differences [between Kbb and levoglucosan] may be due to the non-specificity of potassium for biomass burning where local to regional terrestrial sources may influence the trend, leading to differences with the levoglucosan signal". RC1 Third, you have ignored the study of black carbon from Antarctic ice covering the Holocene from Arienzo et al. (2017) published in the JGR in 2017. Even though the ice core discussed in this recent paper is from another Antarctic site, it is needed to refer to this previous work and discuss similarity and difference with your work. Note that this JGR paper is based on a continuous back carbon record. I attached below the abstract, showing how deep was the discussion of this record in terms of climatic condition changes in the south hemisphere and particularly in South America. Reply: We developed our discussion also considering the Arienzo work. It will be found in the section currently named "drivers for biomass burning", as suggested by RC#2. The paper that you indicated is surely interesting and it must be mentioned. In the Arienzo paper a correlation between black carbon (BC) and solar radiation is proposed, where a difference between October/February insolation is used. Considering that we hypothesized that Patagonia is the main source of levoglucosan signal in Talos Dome (based on charcoal records comparisons), we opted to compare the interval (October-February) in order to take into account of the major influence of the growing season (the driving force for North Patagonia fires) and the burning season (from December to February) that mainly drives South Patagonia fires. We opted for discussing the BC record reported by Arienzo et al., but we also are aware that the Antarctic site reported in this paper are quite distant from Talos Dome (as you recognized) and the BC record in the interval between 6000 and 2000 yr BP showed only few values, in contrast with the high resolution during the Early Holocene and the last two millennia. The few BC data in the Arienzo et al. paper doesn't allow to properly use a robust statistical approach to determine correlations between BC and

our levoglucosan record.

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