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Climate of the Past Discussions

Interactive comment on "Climate impacts on deglaciation and vegetation dynamics since the Last Glacial Maximum at Moossee (Switzerland)" *by* Fabian Rey et al.

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We thank the reviewer for the positive comments and the few critical points. We are glad that the reviewer was impressed by the labor we invested for the pollen analysis and the chronology. We went through all the comments and the replies are attached below.

"1. Title : the ms does not really investigate the impact of climate on deglaciation, except in the first paragraph of the discussion where extent and retreat of ice sheet is briefly discussed in reference to radiocarbon age from lacustrine record north and south of the Alps. Therefore, this seems to be a little off the main topic of the paper.

I would advise to change the title to reflect the focus on vegetation and fire (that does not appear in the title's current form)."

We thank the reviewer for this suggestion. We included the word 'fire' in the title. However, we retained the word "deglaciation" because it is an important part not only of the discussion but also in the introduction. It was one of our aims to reconstruct the timing of the deglaciation (see point (1) in the introduction). Furthermore, we show the first compilation of old radiocarbon dates on terrestrial plant remains in a map. We set the adjusted title as followed: 'Climate impacts on deglaciation, vegetation and fire dynamics since the Last Glacial Maximum at Moossee (Switzerland)'.

"2. In such paper discussing post-LGM and HE1 vegetation dynamics it is quite surprising not to see any references to paleoecological records in the same area/latitude covering these time slices and beyond such as La Grande Pile, Les Echets or Bergsee Lake (Becker et al 2006 is not the most uptodate reference). Please add."

We thank the reviewer for this important remark. We included the most up to date reference for Bergsee (Duprat-Oualid et al., 2017) in the manuscript. Indeed, Bergsee has a very profound chronology for the time window we are discussing. Since we start the discussion with the onset of deglaciation after 19000 cal. BP, La Grande Pile and Les Echets mostly fall beyond of what we want we are discussing in our paper and we could unfortunately not include them as references.

"3. Paleodiversity analysis are interesting but are not really informative (only multimillenial patterns to the best) and are not much used in the discussion. I am wondering if it is really necessary to keep it in the paper?"

We thank the reviewer for this comment. It was not our intention to discuss decadal or centennial patterns and trends in palaeodiversity. We rather wanted to show the long-term trends and the stability of the patterns. The results are still useful because they clearly indicate a higher pollendiversity with increasing human impact. This, we only briefly discuss in the paper because part of it has already been published (Rey et

al., 2019a). However, we would like to keep it in the paper because we can now show for the first time the estimates for the complete Moossee data set.

"4. Use of Sporormiella and link to megafauna. First, Sporomiella is expressed as % of total pollen sum, which is somewhat problematic (eg. Baker et al 2013; Etienne & Jouffroy Bapicot, 2014) . Indeed, fungus and pollen does not belong to the same ecological community and expressing Sporormiella in percentage of the total pollen sum can lead to flawed results: for instance, the relatively high levels between 19 and 15.2 ka BP can either be related to higher presence of Sporomiella or to fluctuations in total pollen sum, or to change in sediment accumulation rate. Moreover, the chronology of the record is robust enough to calculate concentrations and accumulation rate, as it has been done for charcoal, which is also made possible by the use of Lycopodium in pollen slides preparation. I then strongly recommend to express Sporormiella in accumulation rate (nb.cm-2.yr-1), as it is the only way for authors to evidence their claim of more abundant coprophilous fungi between 19 and 15.2 ka BP and its potential link with megafauna presence and then extinction. As currently presented, the data do not support the interpretation. Second, the discussion about the link between continuous abundance (not evidenced in the current dataset) and the presence of Pleistocene megaherbivores is too affirmative in its current form (eg p10 l 363-365) and should be reformulated. Sporormiella is strictly coprophilous, and then megaherbivores are not the only explanation for potential high abundances. Moreover, with Sporormiella expressed ad % the authors cannot state that the extinction of megaherbivores is "also evidenced by decreasing numbers [of] dung spores after 16000 cal. BP" (p10 l 370-371), since we do not know if the numbers of spores is actually decreasing (% are, which is different)."

We thank the reviewer for this important remark. We understand that it can be critical to only show the percentages of Sporormiella. As suggested by the reviewer, we included the influx values in Fig. 4. The trends of the influx values are very similar to the percentages (due to the robust chronology) and we are confident to still state

that the high values of Sporormiella could be connected to the presence of megaherbivores. Theoretically, Spororomiella could also indicate the presence of other (smaller) animals. However, many studies in North America could show that Sporormiella values are indeed linked to high numbers of megaherbivores (Robinson et al. 2005; Davis and Shafer 2006; Gill et al. 2012). In Siberia, frozen dung next to an excavated mammoth carcass revealed high numbers of Sporormiella spores (Mol et al. 2006). Hence, we do believe that our Sporormiella values could also indicate high numbers of megaherbivores on the Swiss Plateau and the decreasing numbers of dung spores after 15500 cal BP may be related to regional extinction of e.g. mammoths. However, to take into account the reviewer's concerns, we slightly adjusted the text to be less affirmative.

"5. Main pollen diagram is described in the results section with a phase beginning at 19200 cal. BP. But Figure Figure 3 seems not to exhibit pollen data prior to 18800-19000 cal. BP? Please check & correct."

We thank the reviewer for this comment. 19200 cal. BP is actually the calibrated age of the oldest radiocarbon date (based on a Salix herbacea leaf fragment and rhizome). The reviewer is right: for this depth, we do not have a pollen sample. Hence, we corrected this mistake. The onset of the pollen diagram is now correctly set at 19000 cal. BP in the text.

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