Referee report

I am pleased to see that this manuscript presents a compelling and well-written vegetation reconstruction from a single site in Central Patagonia that covers most of the transition out of the last glaciations (T1). The data itself is high in detailed and quality, and the pollen analysis is excellent as usual for this group of investigators. I see clearly that the regional vegetation/climate relationships are satisfactorily explained in the *Study Area* section and supported by several atmospheric and palaeoclimate studies cited throughout the text. Thus, the climate interpretations of the pollen data, in particular the ones pertaining shifts of the Southern Westerly Winds (SWW), follow a reasonable logic, adding great scientist relevance to the interpretations.

Apart from minor mistakes regarding the figure captions, my main criticism of this manuscript is that the data presented in the result section do not provide with indisputable evidence to sustain the vegetation/climate trends inferred in the final part of the text, at least not in such emphatic manner. Alternative interpretations of the pollen trends, on the other hand, are heavily missed. In particularly, I do not see clear evidence for the allegedly continuous presence of several rainforest trees around the site during T1. The development of a parkland with scatter, rain-tolerant trees could have perfectly been the case in other areas of this region, and the authors may have unpublished or previously published data supporting the interpretations made from Lago Edita, but they should at any case be extremely careful about the broad and high-sounding climate assertions based on the pollen evidence presented in the manuscript. This is especially important since the past activity of the SWW is and highly relevant ongoing discussion among the palaeoclimate community today.

More specifically, in line 336 the authors mention that *Nothofagus* pollen is usually transported long distances and thus its deposition at Lago Edita does not necessarily translate into local presence of this taxon. However, further in line 352 about 15% of *Nothofagus* is interpreted as evidence for the local presence of beech trees around the site. I note that this interpretation is made despite that the specific *Nothofagus* mistletoe, *Misodendrum*, is completely absent before 16.8 ka, which to me highlights that fact that the extra-local component of *Nothofagus* is maximized. When taking into consideration that *Misodendrum* is not present in regular abundance before 14 ka, there isn't in my opinion robust evidence for sustain the presence of *Nothofagus* trees around the site before that time. This is not considered or discussed at all in the manuscript and it should be. As a result of this, the claimed presence of low density, scattered hygrophilous trees around the site between 19-14 ka relies uniquely on the discontinuous 3-5% of *Fitzroya/Pilgerodendron*, 0-3% of *P. nubigena* and 0-1% of *Drimys*. This is in my opinion a weak palynological signal to argue in favor of local hygrophilous vegetation. Actually, a look at the pollen diagram reveals the presence of several herbs listed as member of the "High Andean Desert" (i.e. Apiaceae, *Gunnera* and *Valeriana*) in percentages that are, in sum, well above the trees uses as wet indicators. Hence, a climate interpretation that includes a dry phase between 19-13 ka could equally be drawn. I strongly recommend discussing this in the manuscript.

In summary, the presence of a parkland of hygrophilous trees on this region during T1 could certainly be a possibility when considering other published or unpublished records, but my point is that the evidence from the single site presented in this manuscript is not concluding enough to propose a sequence of vegetation change valid for the whole eastern margin of the Patagonia Ice Sheet. Furthermore, a vegetation interpretation based on less that 5% of the total pollen is used to infer the dynamic of a hemispheric-scale atmospheric system such as the SWW, which is in my opinion going too far away with the data.

A similar high-sounding climate interpretation is made from the pollen trends observed after 13 ka, when the percentages of hygrophilous trees are heavily reduced and *Nothofagus* increases rapidly. At first, these changes can be interpreted as a drop in moisture. Yet, how can we know that the decrease in the hygrophilous trees is not an artifact of the relative increment of *Nothofagus* if not pollen influx data is supplied? I note that the abundance of the hygrophilous trees after 11 ka is actually not lower than the period 19-13 ka, with the latter being interpreted as a relative wet period. This seems to be contradictory. There is more. According to the authors, a trend towards decreasing precipitation is also suggested by rise in CHAR. Yet, could this rise result from the densification of the tree coverage and associated fuel continuity rather than exclusively due to a climate forcing? This possibility is only mentioned but not taken into account in the interpretations. Additionally, to my understanding the time for the rapid CHAR increases is within the interval of the first *H. Sapiens* colonization of South America. Perhaps the CHAR rise was associated with the appearance and intensification of human-related ignition events summed to a more continuous distribution of fuel. Yet again, none of these arguments are discussed in proper depth in the main text.

In summary, my general impression is that some of the interpretations should be toned down and that alternative scenarios should also be considered and discussed in more detailed. It seems to me that the authors are trying to push the data to match the regional climate trends inferred from previously published pollen profiles, dismissing any alternative interpretation of the data. Beyond these important caveats, this is a persuasive and well-written manuscript.

Minor considerations

The authors rule out the long distance transport of *Nothofagus* since its presence has not been documented in western sites, but then again only two sites westward from the Andes are mentioned in the text attesting for the extremely low density of pollen profiles in a vast geographic region extending for several hundred kilometers across the pacific coast. Thus, in my opinion a western source from the *Nothofagus* pollen grains found at Lago Edita cannot be completely dismissed.

Please note that there are several mistakes in the Figure captions and their correspondence citations in the main text. Here I list a few of them, but I would recommend verifying that all the terminology used in the captions match the one used in the manuscript.

1. The figure captions do not state what “NAP” means in Figure 5. I am lean toward “Non-Arboreal Pollen”, although this figure is cited in Line 322 to mention a correspondence between CHAR and % of *Nothofagus*. This is very confusing, please clarify.

2. In figure 4, the zones in the upper diagram are different to the zones in the lower diagram. There is a gap between zone Edita-2 and zone Edita-3 in the lower diagram.

3. Figure 2 is cited in Line 449 to mention the observation of massive pebbly layer as increases in the “Inorganic density data”. Yet, there is no such as thing as “Inorganic density” in Figure 2 so that the reader is impeded to check the deposition of the pebble layers.

4. A look at Figure 2 reveals that organic sedimentation actually starts at about 700 cm or about 13 ka, and not at 19 ka as stated in Line 480.

5. There is no precipitation data for the High Andean Desert (Line 179); whereas all other vegetation zones have rainfall ranges.

6. In Line 204 it is mentioned that Lycopodium tablets were added to calculate pollen concentration and accumulation rate, but none of this data is provided.

7. The timing for the culmination of glacial advances in New Zealand is commented in Line 575 without any reference. Please add the corresponding citation.