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Interactive comment on "Climate variability and human impact on the environment in South America during the last 2000 years: synthesis and perspectives" *by* S. G. A. Flantua et al.

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Dear Editor,

I am pleased to see this compilation and assessment of pollen records from South America. The manuscript is well written (although it could largely benefit from proof-reading and language revision) and the figures are clear. I find the manuscript to be a valuable contribution to the understanding of past ecosystem dynamics in an area with relatively few records of long-term environmental change. To my understanding, however, clarification/further discussion of the following points would help improve the paper:

1) I think the title is a little misleading. From its content, I would expect the manuscript to review multiple climate proxies (possible models as well), provide a synthesis and discuss how anthropogenic impact has altered the landscape. As opposed to this, the authors a) provide an overview of modern South American climate; b) attempt to reconstruct climatic variations in seven sub-regions of South America; c) describe some indicators of land-use.

2) The climate overview is very good, concise and with enough details to avoid over-simplifications. I believe that it would be beneficial to include a short discussion of the similarities and differences of Figs 2 and 3, and 4 and 5, respectively. At the moment, those figures are always cited in pairs (i.e., 2 and 3, and 4 and 5), and the advantage of showing both correlation and regression is not obvious.

3) For analytical purposes, the authors divided South America in seven sub-regions and provided a characterization of their modern setting. Although climate is described in all cases, the text is somehow unbalanced in as much as, in some cases, geological data are reported and modern climate-vegetation relationships are parameterized, while in other areas only a very superficial description of the dominant plant types is given. Whereas for this manuscript the geology of each sub-region might not be crucial, it might be important to discuss modern climate-vegetation relationships, at least, qualitatively.

4) Pollen records from each sub-region are assessed in terms of their potential for climate reconstructions. Records are chosen according to very high standards (and I agree with the authors in that more flexible criteria could be applied). However, the authors state that, 'To use pollen as a palaeoclimate proxy, the degree of human impact on the vegetation needs to be considered minimum or absent over the last 2 ka' (p. 3840, lines 13-14). I believe that the minimum requirements for using pollen as a climate proxy are a) vegetation needs to be assumed in equilibrium with climate (i.e., no disturbance –anthropogenic or natural-, no biotic interactions); b) the pollen-vegetation (land cover)-climate relationship needs to be calibrated and c) the existence of ana-

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lytical and natural noise in the pollen time series needs to be taken into account (i.e., high frequency fluctuations do not necessarily represent changes in the landscape). Although requirement (b) can be (and has often been) relaxed -qualitative reconstructions are highly informative-, (a) and (c) cannot be ignored. In the literature, there are plenty of examples of ways in which these limitations can be overcome (compositing, Bayesian and frequentist models, use of plant functional types) and robust climate reconstructions achieved (eg, Peyron et al. 2000, Quaternary Research; Davis et al. 2003 QSR, Trondman et al 2015 Global Change Biology). My questions are:

4.1) Why are records with 'signs' of human impact discarded? Disentangling natural and anthropogenic drivers of environmental change is an extremely challenging task. As the authors state 'Indirect indicators such as change in forest composition (e.g. due to deforestation) or species known as disturbance indicators (Cecropia and Mauritia) need additional proxies to derive conclusive findings. Only by looking at pollen changes in context with other evidence – e.g. charcoal, limnology, sedimentology, archaeology- can the correct origin of these changes be identified.' (p. 3505, lines 5-10). I assume (and might be mistaken) that for most records there is no independent evidence of human impact (and, of course, absence of evidence is not evidence of absence). If that is the case, the climate reconstruction is likely to be biased towards sites with fewer proxies. Additionally, the discussion results a little contradictory in that decreased arboreal pollen, for instance (but the same is true for Cyperaceae, Asteraceae and Chenopodiaceae, among other taxa) is sometimes interpreted as anthropogenic deforestation and in other occasions is inferred to be a response to decreased moisture. I believe that perhaps a better approach would be to include all records, account for local-scale variability (see references above. They are a few of many examples of robust climate reconstructions in areas of long histories of intense human impact, such as Europe and Africa) and, when available, draw on the archeological record to test the assumptions of the chosen method.

4.2) Why is fire considered as anthropogenic disturbance?

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4.3) Why are the climate reconstructions based on the interpretation of the authors of the records (when the dangers of doing so are clearly pointed out in the manuscript) and not on some sort of synthesis/compositing/re-analysis?

Thank you for taking my comments into consideration. I would like to congratulate the authors for such impressive data compilation.

Sincerely, Virginia Iglesias

Interactive comment on Clim. Past Discuss., 11, 3475, 2015.