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Climate of the Past Discussions

Interactive comment on "On the quality of climate proxies derived from newspaper reports – a case study" *by* D. Gallego et al.

D. Gallego et al.

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We thank both referees for the constructive comments which will help us to improve the precision and clarity of the paper. Below we discuss point by point the specific comments raised by the referees.

Anonymous Referee #1

Page 979, line 6 and throughout the paper: Our original term "Central Andes" was too general. In the revised version we use "central Argentinean Andes" along the paper.

Page 979, line 13: The reference of Garreaud and Aceituno (2001) will be changed for the reference of Rutllant and Fuenzalida (1991) and Montecinos (2000) when the text refers to the ENSO influence on the central Argentinean Andes. We have also included the recent paper of Falvey and Garreaud (2007, J. Hydrometeorol., 8, 171-193).

Page 981, lines 21-23. The SOI series is that of the Climatic Research Unit at http://www.cru.uea.ac.uk/cru/data/soi.htm. So SOI data are non seasonal and standardized. A complete description can be found in Ropelewski and Jones (1987, Mon. Wea. Rev., 115, 2161-2165), we averaged the SOI at seasonal scale prior to computing correlations. For the snow frequency (SF), the annual cycle was first removed at monthly scale and then, the seasonal averages were computed. Correlations were finally done using these so-computed series at seasonal scale (i.e. one correlation for the annual value, and four individual correlations for JFM, AMJ, JAS and OND). A phrase will be added to the revised paper to clarify the procedure.

Figures 3-5. The significance along the paper is based in Montecarlo tests. In the case of the composites of daily data (Figures 3 and 4), 2500 random series of snow occurrence were generated under the same monthly distribution observed. Simulated anomalies for position and speed of the jet and geopotential were computed for each random snow frequency series and they were compared with the true value. In the case of monthly series (ice edge, Figure 5) we compared the ice edge under above/below average years for SF (+/- 0.5 sigma). To test the significance of the differences we randomly disarranged the seasonal SF computing the corresponding ice edge anomalies and comparing with the true value (10000 times). In the revised paper the method for computing significances is now explicitly included.

Page 984 Section 4: The erratum was corrected in the revised version of the text.

Figure 5 and section 5: The interpretation of the relationship between the Antarctic ice edge and the snow occurrence in the central Argentinean Andes is the main issue raised by referee #1. In fact our results are not intended to suggest the influence of the Antarctic ice on the snow occurrence. The starting point of our interest in the Antarctic ice has its origin in the work of Kwok and Comiso, 2002 (J. Climate, 15, 487-501). These authors (and others thereafter) found a very consistent relation between the ENSO and the Antarctic ice extension around the Antarctic Peninsula through the PSA pattern which on its part, can be seen as one effect of the ENSO-related changes

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in the jet stream over the SH (see Yuan, 2004, Antarct. Sci., 16, 415-425). During our early work with the reconstructed snow series we found the expected correlation between ENSO and the Argentinean Central Andes precipitation at seasonal and annual scales (see Prieto et al. 2001, Aust. Meteorol. Mag., 50, 164-168) but at the time, we were not able to take advantage of the daily character of the SF series. In addition, currently there are no similar series in the area to test our proxy. As our paper establishes, the new databases now available over the Southern Hemisphere led to the idea of assessing the snow occurrence series by searching answers to the following questions: 1. Are daily anomalies in the jet stream consistent with the reconstructed snow occurrence? 2. Is the SLP anomaly pattern during snow days consistent with the jet changes and snow occurrence? In this process, we were trying to test the consistency of the SF series with the second best thing in absence of precipitations series to compare with: -the atmospheric conditions related to precipitation-. As the process for answering these questions progressed, the evidence of a PSA-like pattern related to the snow occurrence in the area and the mentioned work of Kwok and Comiso raised the third question: Can we detect a relationship between the extension of the ice edge and the snow frequency in the central Andes? In retrospect, it is direct to see that both, increases in SF and changes in Antarctic ice have a common physical cause, i.e. the anomalous atmospheric circulation related to ENSO (the PSA). So, in fact there is not any new teleconnection or physical linkages to explain, but a chain of steps that our reconstruction should fulfill in the case it contains a significant climatic signal (changes in the jet, then changes in the SLP and then changes in the ice). In the revised version of the paper we will modify part of the text in section 5 to clarify this point, stating this argument explicitly. Summarizing, the "novelty" of our paper is not the description of a new teleconnection but it has a double aspect: 1-The use of the atmospheric dynamics to assess the climatic reconstructions and 2- The view of the ENSO-jet-SLP-Antarctic ice edge and now the snow occurrence as part of the same ENSO triggered phenomenon.

Page 986, line8 we changed "precipitation increases" for "changes in precipitation"

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Page 988, lines 17-22. We concur with the referee in the sense that part (but not all) of our results are based in monthly/seasonal aggregates. However, we think that the daily character is really useful and offer interesting testing possibilities hardly found in lower resolution proxies. Probably in the present work, the best example is the jet stream. The anomalies on the jet shown in our figure 3 are computed at daily scale, the seasonal average is computed afterwards in order to present the result. Should purely seasonal precipitation data had been used from the beginning of the process, the signal would be greatly diminished and the subtle but significant changes in the southern hemisphere jet (displacements in the order of 1-3 latitude degree) would be hidden in the process. Not withstanding, as the referee points out, our initial statement suggesting the impossibility of studying changes in the jet stream using lower resolution proxies is exaggerated and this phrase will be changed in the revised text.

Technical corrections: They consist mainly in language issues and some technical precision. All the suggested changes will be included in the revised text.

Anonymous referee #2

During the writing phase, the structure of the paper was a constant concern. We found extremely easy to incur in apparent circular reasoning, as noted by referee #2. This possibility led us to choose a relatively high number of separate sections -seven- for a paper as short as this one. Our intention was to present in independent sections the evidences pointing toward the true climatic signal in the SF, at least for four decades. We found anomalies in the jet stream consistent with the occurrence of snow. Independently, we found SLP anomalies consistent with precipitation. Finally -and again independently- we found consistent Antarctic ice anomalies related to the occurrence of snow. So, at this point we were much more confident in the goodness of the SF reconstruction that at the beginning of the process, when only a calibration with nearby precipitation data for a single decade had been performed to test the SF. Therefore, it was never our intention to present the snow series as a proxy for the jet, or the Antarctic ice edge, so circularity is not applicable.

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Of course, every anomaly described in sections 3, 4 and 5 of the paper fits beautifully together as part of a number of recently described ENSO impacts on extratropics (section 6), so as a bonus of the study, the statistical relation between our snow frequency series and ENSO that we found in previous works (Prieto et al. 2001 for example) has now solid physic roots.

However, it is true that the use of the term "validation" as it is in the original version of the paper can be confusing and the use of "test of consistency" suggested by referee #2 reflects better the spirit of our study (see also our reply to referee #1). The revised paper avoids the term "validation". In addition, the revised text makes emphasis in the intended approach of the paper by improving the clarity of the abstract and the last part of the introduction section to help the reader to grasp the sense of the paper.

Interactive comment on Clim. Past Discuss., 3, 977, 2007.