

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

Anonymous Referee #1

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General comments

Gallego et al. use a snowfall frequency time series, established from old newspaper reports, to assess snowfall variability at the central Chile-Argentina border. In absence of long nearby observational records, they instead try to validate their proxy record by comparing it with large-scale climate and the associated atmospheric circulation. The beauty of this paper, in my opinion, lies in its attempt to assess the large-scale influence on Andean snowfall and in their ability to link a local proxy not just to nearby station data and hence wet or dry local conditions, but to large-scale climate dynamics. Too often proxy-based reconstructions simply involve qualitative estimates of local climatic conditions without any attempt to put the proxy record in a larger context of a dynamically consistent and meaningful framework. The authors therefore deserve a lot

of credit for this kind of study. Below are a few instances where I think some corrections and modifications are needed, but these are mostly minor issues and I suggest accepting the paper after these revisions have been incorporated.

Specific comments

-Page 979, line 6 and throughout the paper: the term "central Andes" is inappropriately used here, as it should be reserved for the region of southern Peru, Bolivia and northernmost Chile (~12°-25°S). The region of this study is the central Argentinean or central Chilean Andes.

-Page 979, line 13: The paper Garreaud and Aceituno (2001) is not a good reference for this statement as it deals with the ENSO influence on the Altiplano region, but not in central Chile. More appropriate references are: Rutllant and Fuenzalida, 1991, *Int. J. Climatol.*, 11, 63-76, or Montecinos et al., 2000, *J. Climate*, 13, 746-758.

-Page 979, line 26: When discussing advances in our understanding of large-scale circulation anomalies on central Chile snowfall, maybe also add the reference Falvey and Garreaud, 2007, *J. Hydrometeorol.*, 8, 171-193, which provides the most recent and detailed assessment of this topic.

-Page 981, line 21-23: it is not clear on what time scale this correlation between SF and SOI was established (monthly or seasonal data) and if it is based on the raw data or with the seasonality of snowfall removed (anomalies)? Please explain.

-Figures 3 -5: How was the statistical significance assessed in these Figures? With a students t-test? A more detailed discussion of the statistical methods used is needed here.

-Page 984: section 4: The anticyclonic anomaly is located once at 65°W and once at 75°W. Please correct.

Figure 5 and Section 5: The suggested impact of sea-ice extent on snowfall frequency in the Andes is intriguing but needs more explaining. The evidence presented in my

judgment is not sufficient to draw any firm conclusions. The fact that the two appear to vary in phase may as well be related to a common response to atmospheric forcing. Both sea-ice extent and snowfall variability may be influenced by the anticyclonic anomaly imposed in the Bellingshausen Sea and as such variations in sea ice extent may well occur in response to the same atmospheric forcing influencing snowfall frequency, rather than representing a genuine forcing mechanism. The authors briefly mention this possibility in one sentence saying that correlations in a lagged-analysis were highest when in phase. This leaves open the option of sea ice variability acting as a forcing agent as well as being a result of atmospheric forcing. In any case, if sea ice does indeed have a significant influence on Andean snowfall, it begs the question through which dynamic mechanism such an influence would occur.

-Page 986, line 8: change "precipitation increases" to "changes in precipitation" as El Nino can lead to both a decrease or increase in precipitation, depending on the region in South America.

-Page 988, lines 17- 22: It is true that the resolution of the SF time series is daily, but since it is binary (snow, no snow) for many applications (including in this study) the data need to be aggregated into monthly or seasonal averages to be truly useful. In this sense the data is not of much higher resolution than some of the other available proxies. In the same vein, the statement that the other proxies, such as ice cores, lack the resolution to study changes in the jet stream or SLP is not correct. In fact, Vuille et al., 2003, J. Geophys. Res., 108, D6, 4174, have done just that (see their Figure 10).

Technical corrections

-Page 978, line 17: "high-resolution"

-Page 980, lines 1-2: advances are "made" or "achieved" but are not "carried out"

- Page 981, line 23: change to "a significant negative correlation"

-Page 982, line 5: change to "at mid- and upper tropospheric levels"

- Page 982, line 6 and throughout the paper: "close to the vertical of" change to "aloft"
- Page 982, line 17: "out of the scope" change to "beyond the scope"
- Page 982, line 20 and throughout the paper: change "geopotential" to "geopotential height"
- Page 982: line 25: "definition based in" change to "definition based on"
- Page 987, line 26: "have provided"
- Page 988, line 14: "as the tree grow rate" change to "such as the tree growth rate"

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