EGUsphere, referee comment RC2
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Comment on egusphere-2022-603

Anonymous Referee #2

Referee comment on "Climatic control of the surface mass balance of the Patagonian Icefields" by Tomás Carrasco-Escaff et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-603-RC2, 2022

Summary: this paper describes the climatic controls on the surface mass balance (SMB) of the North and South Patagonian Icefields (NPI, SPI). This is achieved by estimating the annual and seasonal SMB with a simple snow, firn and ice accumulation and ablation model, subsequently regressing the SMB-anomalies time series to a suite of local, regional and climate indices. Results indicate that winter precipitation and summer temperature anomalies are the main drivers of SMB interannual variability. Also, the authors find that a pressure anomaly over the Drake's passage (the Drake low) is the dominant feature related to SMB departures, seemingly driving increased westerly winds and cooler conditions off the coast of Patagonia. No significant correlation was found between the SMB and major climate indices such as ENSO, which confirms previous work published in the area.

General comments: this is a well written paper, and is a nice contribution to the understanding of the NPI and SPI present-day behavior. The authors have taken preemptive actions to prevent the inevitable modeling uncertainties from affecting their conclusions, by focusing on correlations/anomalies only and by ensuring that potential biases in the meteorological forcings of the SMB model don't result in major changes in the year to year variability, measured through correlation and standard deviation of the time series. The organization of the manuscript is very intuitive and the use of English language is appropriate but for a few minor issues. Because the analysis rests so strongly on the simulated mass balance, the manuscript should devote a bit more space to discussing the calibration of the four main parameters of the model, namely the threshold at which precipitation falls as snow (here set as 2°C), and the ablation parameters (albedo, c_0 and c_1). The sensitivity of the model to these parameters should in turn influence the interplay between precipitation and temperature during the accumulation season, and the relative influence of radiation and temperature during the ablation season. It may be that the main conclusions don't change with respect to what is shown in the current version, but so far the paper seems to gloss over this topic in a manner too succinct.

Specific comments:

L132: It is not clear to me what the verification of RegCMv4 against CR2MET intends to achieve. There are clear biases shown in Fig2, which could result from several factors. Because you have threshold term in accumulation that depends on T, this bias in temperature could have compounded effects on the simulated SMB correlations. Do you do anything after verifying the two products against each other?

L201: snow, firn and ice should not be called "soil". Please use something like "land cover".

L210: in modeling parlance, "true" has a very specific meaning. Please revise.

L218: See general comments regarding the detail that is needed about the SMB model calibration process. Also, why do you compare the 2000-15 simulation with the 2000-19 Minowa estimates? Is it not possible to compare a common period?

L229: These biases could also result from inadequacies in the CR2MET product. In particular, if it is station-based, previous research has shown that meteorological station data in Patagonia is unreliable, particularly precipitation.

L231: a similar analysis could be performed by perturbing some of the model parameters (see general comments).

L260: If I understand correctly, AAO was calculated only for the 1979-2000 period? But SMB is available until 2015? Maybe it'd be useful to have a summary table with all datasets used, indicating time window, time-step, and citation.

L272: please reword to remind the reader that all these numerical quantities are estimates from your model. Also, the fact that annual SMB is positive means that for the ice fields to be in equilibrium (or decreasing in mass, as the literature suggests) then calving should account for the excess mass. Is that right? Also: there appears to be a slight increasing trend in the simulated SMB? Could you comment on this?

L298: How do you interpret the fact that although insolation shows the second-highest correlation with annual SMB (line 287), then the local-scale control indicates exactly the opposite? Maybe I'm missing something, but these two results seem inconsistent. Please clarify. Is this result sensitive to assumptions regarding the seasonal evolution of snow, firn and ice albedo? Nevertheless, it is expected that, unlike glaciers in mediterranean regions, solar radiation should have a minor role compared to temperature in Patagonia. High relative humidity and high very persistent cloud cover are coherent with this result.

L406: a small detail: probably “good” is not an appropriate adjective for describing correlation.

L446: suggest replacing “maintains” with “remains”.