I thank the reviewer for the constructive comments and suggestions. My response to the review can be found in the attached document.

R: Referee's comment
A: Author's response
C: Proposed changes in the manuscript

Summary

R: Sauter presents an evaluation of the precipitation magnitude across southern South America. The emphasis is given in the implications to the surface mass balance(SMB) of both Icefields. New data is mainly compared with previous SMB models. Based on the data obtained from an Orographic Precipitation Model, the main conclusions are that Patagonia is not the wettest place on Earth and that the previous SMB overestimates the accumulation. The scientific significance is high regarding the lack of information (and observations) of the actual magnitude of the precipitation on the Patagonian Icefields. The scope of the research is clear and prove (based on the used datasets and assumptions), that higher amount of precipitation previously reported are not sustained by the moisture flux over the region, although this conclusion, of course, will continue to be subject of discussion in the future. The manuscript is within the scope of HESS, it is in general well-written. The reason for the study must be well justified in the Introduction. The Methodology section needs to clarify and add some explanations, as well as some dataset and experiments details used for validation/comparison. The Results/Discussion section needs to be strengthened. I think this paper provides interesting data and results to the glaciological and hydro-climatological communities. In my opinion, it must be considered for publication after solving/clarifying some issues (see Major comments) and a careful revision of the text(see Minor comments).

A: I appreciate very much the overall positive evaluation of the script and will follow the suggestions of the expert in the respective sections. I also agree with the expert's opinion that the discussion about the precipitation in Patagonia is not yet finished and that further relevant studies will be necessary in the future to close the knowledge gap and open issues. I am convinced that this work is a step in the right direction.

Introduction

R: As one of the main topics is the implication of the precipitation on the mass balance on the Icefields, a detailed review of the previous estimations is necessary, considering also the title of the Manuscript. This is barely mentioned, but a comprehensive revision will be helpful for the reader and also will give a strong justification to the present work, mainly related to the fact that besides all the past efforts, still high uncertainties exist in the precipitation magnitude and/or accumulation rate on the Patagonia Icefields.

A: In the present version, only the range of previous mass balance studies with the corresponding literature sources was deliberately mentioned. Most of the literature sources mentioned here summarize the mass balance studies published so far. Since no scientific added value is generated by a new summary, only the previous studies were referred to here. In order to emphasize the importance of the study further I will follow the suggestions of the expert gladly and will work out more precisely the necessity of the study in view of existing work.

C: The last sentences of the first paragraph of the introduction (p2 l8-15) will be revised with respect to existing studies.

R: In this section, it is mentioned the importance of the atmospheric rivers as a source of moisture on Patagonia (first paragraph), but a previous work (Langhamer et al., 2019) indicated that the main source of moisture for the South Patagonian Icefield is the Pacific Ocean between 30◦and 60◦S. Please, clarify this.

A: Atmospheric rivers from the Pacific always lead to a strong increase in water vapour transport and consequently to the strongest precipitation events in Patagonia. These water vapor-rich outbursts occur in the tropical Pacific as a result of geostrophic turbulence. The statement in our paper (Langhamer et al., 2018) that the dominant moisture source in the South Pacific is between 80-160ºW and 30-60ºS does not contradict this statement but confirms the observation. Furthermore, looking at the identified moisture source patterns of Langhamer et al (2018) one can see that this also corresponds to the statistically mean tracks of atmospheric rivers.

C: At the beginning of the introduction (p1 l24-27) I will refer explicitly to the study by Langhamer et al. (2018) and its results.

Soundings

R: It seems that the soundings are used to validate and corrected the WVF from ERA-Interim. Although the information about the WFV trend for both soundings is interesting, it is not used in the analysis. A Figure showing the comparison of the observations with the ERA-Interim data will be useful to understand the bias correction (Page 5, Lines15-20). Please note that the SSMIS data is not mentioned in the Methodology section as a dataset used for comparison.

A: Yes, the radiosondings were used in this study exclusively to evaluate the ERA interim data. A detailed analysis of the trends was not in the spotlight here. Nevertheless, I fully agree with the reviewer regarding the relevance of these data. The WVF time series show very exciting phenomena, such as the influence of atmospheric modes or the presence of decadal signals (water vapor flow) in Punta Arenas while a continuous trend is observed in Puerto Montt. This raises the pressing question which processes can lead to this very different response and to what extent this is related to a changing climate. These exciting questions might open up new perspectives that need to be addressed in detail in follow-up studies and would go beyond the scope of this work.

C: In Figure 2 the time series of the WVF anomaly of the ERA interim were also shown in the first version. For the sake of clarity, the time series were taken out again and the statistical differences (bias, trend, etc.) were presented in the text. If desired, I will add the ERA interim anomalies again in Fig. 2. Many thanks also for the note that the SSMIS dataset was not introduced. I will introduce the dataset accordingly.

Spatial differences

R: It is well established that there are extreme climate gradients on the region, and the results of this work also provide evidence of this, however, in terms of SMB just a mean value is used for each Icefield (NPI and SPI). The author must consider the spatial differences and/or the heterogeneous response of theses glaciers determined by the geodetic method (e.g. Malz et al., 2018; Jaber et al., 2019). Overall, the discussion is related to the glacier shrinkage, despite that, some glaciers show positive (e.g. Pio XI) and stable mass balance.

A: The extreme climate gradients are probably one of the strongest worldwide and probably not only in terms of the drying ratio, as was shown by the reviewer in his relevant study (Bravo et al., 2019). These inevitably lead to a strong west-east differentiation of the surface mass balance and, in interaction with the dynamic effects, to very heterogeneous responses. In our study (Braun et al., 2019) we were able to show by means of TandemX satellite data that the dynamic adjustment of large glaciers plays a decisive role. Evidence has been provided that ice dynamic probably leads to temporary stable mass balances of some glaciers. Unfortunately, such a differentiated analysis is not possible within the

approach of this study. This is mainly due to the fact that the SMB is derived from the simplified SMB-accumulation relationship derived from the work of Schaefer et al. (2015) (see Fig. 4). This approach can only be applied to the overall mass balance and is not valid for a differentiated consideration. However, Table 1 can be extended accordingly to emphasize the strong west-east differentiation. Here new columns can be inserted which show the mean precipitation values for individual height zones on the west or east side.

C: I will extend Table 1 and point out again in Section 4 that the approach does not allow differentiation in the mass balance.

Mass balance uncertainty

R: The approach to calculating the mass balance is simple but useful to demonstrate the implication of the overestimation of the accumulation rate. However, it must be mentioned other source of uncertainty on SMB estimations. Recent work indicates that the method chosen to estimate the ratio of solid precipitation is also a source of uncertainty in accumulation estimations (Bravo et al., 2019). This even could lead to lower accumulation rate than those mentioned in the manuscript as the method used is quite simplistic.

A: This is absolutely correct and the other sources of uncertainty have not yet been sufficiently addressed.

C: Other sources of uncertainty, such as uncertainty in the solid-liquid relationship, are explored in more detail in the discussion at the end of Section 3.3.

Limitations and nonlinearities

R: This section presents a comprehensive analysis of the main source of uncertainty, related to the linear nature of the model. This is quite interesting and necessary in a work of this nature, and the last three lines of this section (Page 8, Lines 11-14) is a perfect summary of this section. However, this analysis should be useful if quantification of the uncertainty is given. This quantification could be obtained from the WRF experiment, which also needs to be mentioned in the Methodology section. This section also uses three of a totals of seven Figures to explain the limitations, please check if is really necessary the three Figures.

A: A detailed quantification of the uncertainties can hardly be carried out due to the sparse observations. In section 3.2 (and table S2), the mean deviations of the simulations from the observations (p6 l2-5) were discussed as far as possible.

"…*Comparison with in-situ observations from the Dirección de General de Aguas (DGA, Chile) indicates that the model slightly overestimates precipitation on the leeward side by 0.29±0.37 m yr^{-1}(see Table S2). Greater deviations (1.07±1.30 m yr^{-1}) occur at the stations on the west side which are located at the foot of the Patagonian Icefields. The overestimation is the result of the rapid increase in model terrain elevation and the absence of nonlinear processes in the linear model (see Sec. 3.4*)…"

A comparison with the WRF simulations is not suitable for the quantification of the uncertainty for two main reasons: (i) WRF simulations were only performed for single events to analyse the effect of atmospheric blocking on the precipitation distribution, and (ii) the simulations themselves cannot be considered as 'reality' and are themselves subject to very high uncertainties. For the mentioned reasons, a detailed quantification is only conditionally possible.

The three figures have been chosen so that the limitation of the approach and the nonlinear nature of the processes are evident. Figure 5 shows how often such atmospheric blocking events occur and how often the approach has to be challenged. The subsequent figure shows the underlying processes and theoretical effect on precipitation that may not be familiar to every reader. Figure 6 then finally provides numerical evidence for the effects.

C: If the readability of the article is impaired by the number of figures or the number exceeds the allowable number, I would suggest not to use Figure 5.

Consistency on percentages

R: Please clarify the percentage of overestimation regarding previous works. For instance, it is mentioned in Page 6 Lines 15-16 that the maximum precipitation (11.58 +-0.98 myr-1) represents a reduction of 60% compared to other numerical studies, while in the Conclusions section (Page 8, Lines 21-22) this same value represents a reduction of 30-50%. Please also check the other percentages given.

A: Thank you for pointing that out. This is an error and the correct information should be read ‘up to 60%’.

C: I will check the consistency of the percentages throughout the document and adjust them if necessary.

Minor comments

A: I will review and correct all the comments made by the reviewer.