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

General comments

R: Generally I think it is a study which has the potential to make an important contribution to our understanding of precipitation patterns in the study region. However, giving that the results of this study are quite different from previous numerical studies, I am missing are more dedicated search for the reasons of this differences in the discussion section (I am giving some ideas in the specific comments). Furthermore I think that an analysis of the implications of this study for the surface mass balance of the Northern Patagonia Icefield (NPI) would add very much. Here the results could be validated much better against geodetic glacier mass balances since the losses by calving are much better constraint than for SPI (only one tidewater calving glacier on the NPI). Also readability of the manuscript (especially for not climate modelers) could be easily improved. Please find more detailed comments below.

A: Many thanks for the fundamentally positive assessment of the study. I will implement the constructive remarks and comments conscientiously and make the manuscript more accessible to a wider readership. The extension of the study to the NPI would certainly be interesting, but the linear relationship between precipitation and SMB does not necessarily apply to the NPI. Therefore, I would prefer to omit the NPI analysis.

Specific comments

R: Line11: "volume loss of the Patagonian Icefields, for example, contradicts the reported positive surface mass balances" there is no contradiction if the difference can be attributed to calving fluxes (or other mass losses).Example Antarctica: positive SMB, negative overall MB.

A: That's correct.

C: I'll re-write the sentence, for example:

"… The Patagonian Icefields, for example, are one of the largest contributors to sea-level rise outside the polar regions, and robust hydroclimatic projections, in particular estimates of precipitation, are needed to understand and quantify current and future changes. The reported projections of precipitation from numerical modelling studies tend to overestimate those from in-situ determinations and the plausibility of these numbers have never been carefully scrutinised, despite the significance of this topic to our understanding of observed environmental changes. …"

R: Line11-14: Reformulate: you are using a model in this study (and not only a simple physical argument) and get some results. Describe the model briefly: what are the input data and main assumptions.

A/C: I will reformulate the text.

Introduction

R: Line 12/13: up to 30 m w.e. yr -1 are suspected at isolated locations (Lenaerts et al., 2014; Mernild et al., 2017; Schaefer et al., 2013, 2015;Schwikowski et al., 2006). Revise citations! In Schaefer et al., 2013, 2015 no precipitation of up to 30 m w.e. yr^{-1} are suspected (I think only Lenaerts et al., 2014 is mentioning this value!)

A/C: In fact, the figure of 30 m w.e. yr^{-1} was mentioned only by Lenaert and the estimates of the other studies are lower. For this reason the formulation 'up to 30 m' was used and not 'all studies show values of 30 m'. I confess that the formulation can be somewhat confusing and will correct this.

R: Line 29-34: to improve readability, I would recommend to leave the assumption ii) shorter. 1-2 sentences. You can explain the technical details in the method section.

A: In order to increase the readability I will shorten the second assumption slightly.

C: I will move the following sentences to the method part: "This criterion requires a stably stratified atmospheric flow, more precisely given by a positive moist buoyancy frequency. During the study period from2010 to 2016, the condition was fulfilled in more than 99% of all days. … The linearity requirement was met in 82% of the cases (see Sec. 3.4)".

R: Line 7: Replace "and critically reviewed (Section 3.4) " by "limitations of the linear precipitation model are discussed in Section 3.4"

A/C: Will be done.

Methodology

R: To improve readability I recommend to divide the methods section in three subsections:
2.1 DR-scaling using a constant precipitation gradient
2.2 DR-scaling using the linear precipitation model
2.3 Examination of non-linearities using the Regional Climate Model WRF (new section)

A/C: In order to make the chapter clearer, I will divide the section into the following subchapters:
2.1 DR-scaling
2.2 Linear orographic model
2.3 Numerical Simulations with the Weather Research and Forecast (WRF) Model

R: p3 l16: "by optimizing (Newton-Raphson algorithm) the vertical precipitation gradient" How does this optimizing work? Describe in one sentence and give a reference for further reading.

A: The Newton-Raphson algorithm is a standard optimization method and in my opinion only needs a corresponding reference.

C: I will add a corresponding reference.

R: p3 l17: "determined from the GPM measurement" : determined : how? Or do you mean taken from? Acronym GPM not explained!

A: The abbreviation was indeed introduced only in the caption of Figure 1 and not in the text.

C: I will introduce the abbreviation 'GPM' on p3 l17 and replace 'determined' with 'taken from'.

R: p3 l18/19: “The optimization resulted in a vertical precipitation gradient of 0.00052 m m^{-1} (~0.02% m -1), which represents a slightly smaller lapse rate than previously reported (Schaefer et al., 2013)” In Schaefer et al. a precipitation of 5% per 100m was employed. That is more than double the one you used. You could create another scenario using a lapse rate of 5% per 100m!

A: The numbers mentioned in the text still refer to old simulations and are not correct. The optimized lapse rate is $5.29*10^{-2}$ % m^{-1}, and thus 5.3 % per 100 m.

C: I will change the numbers in the text accordingly.

R: p3 l27: “many processes” : name some (or all)!

A/C: Will be done.

R: p3 l31: “it solves two steady-state advection equations” : show the equations or give reference

A/C: I’ll give the references to the corresponding papers.

R: p3 l32: “conversion from cold water to hydrometeors” : cold water IS a hydrometeor? Do you mean cold vapor?

A/C: The sentence must read as ‘cloud water’ and not ‘cold water’. Will be corrected.

R: p4 l2: “and properties” : properties of what? Which properties do you mean?

A/C: The uniform flow characteristics refer to wind speed, atmospheric stability, scale height etc. (i.e. all assumptions made for this model and explained in the following sentences). Only under these assumptions a linear model can be developed at all. I will present this more clearly.

R: p4 l3: “five parameters” : could explain each parameter in one sentence?

A/C: I will revise the description of the linear model so that the purpose of the parameters is easier to understand.

R: p4 l8: “background precipitation is scaled by a constant” : which range of values does this constant take? What happens if you do not force the model to a fixed drying ratio?

A: Since the model is based on the linear (mountain wave) theory, the precipitation is extended into a background value and a perturbation (orographic part). Here it is assumed that the background value does not vary at all or only very slowly. It must also be considered that the perturbations have no influence on the background value. Thus the orographic precipitation model can only represent the orographic induced part of the precipitation and not the total precipitation. The synoptically induced precipitation must therefore be added. In the study there is no constant value for the background value but is adjusted by the drying ratio (including white noise). So if no background value is added, only the orographic part is obtained, which leads to unrealistic total precipitation.

R: p4 l10: “C_w and N_m are calculated from 6-hourly ERA-Interim fields” how?

A: C_w relates the condensation rate to vertical motion in the atmosphere and can be derived from the ratio of the moist adiabatic and atmospheric lapse rates. The effective moist static stability is approximated from the commonly used equation (Fraser et al., 1973; Smith and Barstad, 2004).

C: I will clarify in the text how these parameters have been derived.

R: p4 l14/15: "produce remarkable similar results" which kind of results? Precipitation fields? You should show that in the results and discussion section!

A: Garreaud et al. (2016) has used the WRF model and the linear model to study the orographic precipitation in coastal Southern Chile. They found similarity and high spatial correlations on longer time scales (not daily) between the two models. The findings cannot be directly applied to the study presented, but indicate that the linear model leads to consistent results under realistic setups.

Results and Discussion

R: p4 l27/28: "Along the coast ..." to which data refers this sentence? ERA-Interim cells located at the coast or measurements in Puerto Montt or Punta Arenas?

A/C: 'Along the coast' refers to the Pacific coast where Puerto Montt is located. I agree, this is confusing and will be re-written.

R: p4 l 29/30 Sentence starting with "There is also clear evidence ..." needs citation. Or are you referring to the data in Puerto Montt?

A/C: The evidence follows from Fig. 2 which provides the basis for the discussion (paragraph) here. I will add another citation to Fig. 2 here.

R: p5 l6 : " The ERA-Interin data ..." : please add these data in Figure 2.

A/C: (see also response to reviewer 1). 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.

R: p5 l12: "SSMIS data" : explain! Are these reliable data?

A/C: (see also response to reviewee 1). The SSMIS dataset was not introduced at this point, but I will provide more information on the dataset. It is reported that the data is reliable over the ocean but underestimates the vertical integrated water vapor over cold surface such as glaciers.

R: p5 l18: "and corrected accordingly" : this means you multiplied the original ERA-interim WVF by 1.1?

A: Yes, the WVF has been bias corrected with a factor 1.1

R: p5 l22: “Pre-Cordillera region” add (see Figure 1)

A/C: Will be done.

R: p5 l29 “.. values agree with precipitation estimates from discharge measurements “ this statement is not true (see table 1).

A: One would have to say is closer to the estimates from the discharge measurements.

C: I will remove this remark.

R: p5 l32: “windward side” and “leeward slopes” what is the extend of this regions? Can you indicate them in Figure 1 or 3?

A/C: The terms refer to all slopes of the Patagonian Icefields facing to or away from the wind. The west-east differentiation is easy to see in Figure 3.

R: p5 l33: “The spatial pattern on the plateau is consistent ...” how can a precipitation pattern be consistent with elevation change measurements? Explain! How do you define “the plateau” ? Indicate this region in Figure 3!

A/C: The remark by the reviewer is justified because the statement can only valid if hardly any ice dynamic processes take place, which was assumed here for the higher area near the ice divide. This is a very simplistic assumption and not necessarily based on hard facts. But the simulated accumulation patterns are very similar to the observations of the geodetic method. It is clear to me that this statement raises further questions which cannot be answered in the context of this study. I would therefore suggest to discard this sentence.

R: p6 l3: “Greater deviations” better say “higher overestimations”

A/C: Will be implemented accordingly

R: p6 l7: “Shiraiwa et al. is not a presenting simulations but measurements. Please indicate that clearly!

A/C: Ok.

R: p6 l7 : “ The large ensemble spread” : how much is it? You have to show that!

A: The ensemble spread is reflected in the uncertainties of the mean values (see e.g. Table 1).

R: p6 l9-10: “ the responsible mechanisms explaining the significant differences remain unclear.” That’s a very sad statement for a scientific contribution! At least try! Potential candidates are: higher drying ratios or higher precipitation gradients in other studies. You also ran the regional climate model WRF. What drying ratios and precipitation gradients did you get from this simulations? Also different study period could be a candidate.

A: The source of uncertainty in such cases can be very diverse. In addition to the actual errors in the input data, uncertainties in the parameterizations are particularly relevant. Some microphysical parametrisation schemes are more 'grauple-friendly' than others which can lead to strong hyrdometeor formation. In addition, a multitude of parameterization combinations can lead to very different results. Each model setup must therefore be examined individually. Errors in the drying ratio are the direct consequence of inadequate process mapping. The own WRF simulations do not provide any further information since no longer time periods were calculated. For single events the DR can deviate strongly from the long-term mean and the isotope measurements. The sources are manifold and can only be speculative in the context of this study.

C: Even if it is very speculative, I will revise this paragraph and work out possible sources.

R: p6 l26-28: "…, we use the significant linear relation (), between annual precipitation sum and annual SMB derived from data of Schaefer et al. (2015). The relationship is: …

A/C: Will be changed accordingly.

R: p6 l30/31 "...would result in a mean SMB between 0.56±0.45 m w.e. yr -1 (7.82±6.28 km -3 yr -1, extreme scenario) and -0.14±0.39 m w.e. yr -1 (-1.95±5.45 km -3 yr -1, realistic scenario) on theSPI (Fig. 4)

A/C: Will be changed accordingly.

R: p7 l1-2: "… the mean mass loss due to calving ranges between -1.5±0.64 (-20.95±8.94 km -3 yr -1) and -0.8±0.58 m. w.e. yr -1 (-11.18±8.10 km -3 yr -1)" Are these values realistic considering that recently calving fluxes of up to 3.81 km³w.e. yr-1 for one single glacier were observed during the study period (2015)?
Citation: Bown F, Rivera A, Pętlicki M,Bravo C, Oberreuter J, Moffat C (2019). Recent ice dynamics and mass balance of Jorge Montt Glacier, Southern Patagonia Icefield. Journal of Glaciology 1–13. https://doi.org/ 10.1017/jog.2019.47

A: The values presented here are mean values over the entire period (7 years). Individual events can lead to large mass losses which increase the total mass loss in individual years. The event of 2015 cannot be represented with the approach presented here, since in this study the mean SMB is used together with the geodetic mass balance observations which also represented an integrated value. Any error in the SMB estimate inevitably leads to further uncertainties in the calving fluxes.

C: To remind the reader that such individual events are not considered in this study and that individual years can deviate strongly from the values, I will take up the study of Brown et al. (2019) in the discussion.

R: More results of the WRF simulation should be presented in the results section: which precipitation gradients, total precipitation values of the icefields and drying ratios do you obtain from these simulations?

A: As already mentioned in the previous answers, high-resolution WRF simulations were only calculated for single blocking events. The values derived from a few single events such as precipitation gradients, drying ratio and total precipitation would not be representative.

Conclusions

R: p8 l16: " … simple physical arguments ..." again: you are using a numerical model, not a simple physical argument. Better repeat model essentials.

A: A linear (analytical) model is not a numerical model in a strict sense. A numerical model generally consists of a model conception, a closed physical system of equations which are solved by a numerical solver. A analytical model has a mathematical closed form solution. DR-scaling in turn is a clear scaling approach by scaling precipitation by water vapor flux.

C: I suggest to continue to use the term 'scaling' or 'physical argument' for DR-scaling approach and to use the term 'analytical model' or ‘linear model’ for the orographic precipitation model.

R: p8 l18: “.. other parameter combinations ...” of the model employed here or generally?

A: The statement refers to the models used here. Since these are linear models, all parameter combinations that lie between the realistic scenario and the extreme scenario must also must lead to precipitation values that lie in between.

R: p8 l20: “ … clearly defined assumptions. “ add which are: …

A/C: Will be done.

R: p8 l22: Shiraiwa et al.,2002 is NOT a modeling study!!!

A/C: I will make that clear.

R: p8 l30/31: “WVF changes would result in a glacier surface mass gain of about 0.57±0.06 m w.e. per degree warming. TAKE CARE HERE!!! The relationship between precipitation and SMB you derived from the data of Schaefer et al. 2015 will not be valid forever. Percentage of solid precipitation will decrease for higher temperatures!

A/C: I'm fully aware of that. For this reason I will repeat at this point the statement of page 7 line 12 'Ignoring the fact that the solid-liquid ratio changes' and explicitly point out that this trend can only be correct for short periods (including a reference to the work of Bravo et al. (2019)).

R: Line 4 : “While the change in ice masses ...” to which change are you referring to?

A/C: This is a general statement suggesting that the change in ice masses is a very illustrative example of how precipitation uncertainties can affect systems.

Figures

R (Figure 1): I am a bit surprised about the many dots! Can you indicate a list of stations (supplementary material) and indicate to which time span the color coding of the measurements corresponds?

A/C: Yes, I will add a column with the corresponding time periods to Table S2.

R (Figure 2): As indicated before, I would like to see the closest ERA-interim gridpoint data added for each station.
would prefer very much absolute data instead of anomalies!

A/C: See previous comment. I prefer to consider the anomalies to get rid of the seasonality without interfering with the trends. It also helps us to compare the different Puerto Montt with Punta Arenas.

R (Figure 3): Both plots are qualitatively very similar. Could you better compare the linear prec. gradient approach with the numerical model (using the same drying ratio)?

A/C: This is not possible since the WRF simulations are event based simulations (see previous comments).

R (Figure 4): revise caption! The red, green and orange dots are the results of this study! Schaefer et al. (2013), Mernild et al. (2016) are indicated as red circles.

A/C: I will revise the figure caption.

R (Figure 4): I not very necessarily I find. Better add a nice figure about the model validation at weather Stations.

A/C: will remove the figure (see also comment of Reviewer 1).

R (Table 1): Lenearts et al. (2013) are indicating mean precipitation estimates in gigatons. You can calculated specific values from that. Also revise the max values (I think 30 m was obtained at SPI).

A/C: Thank you for pointing that out. I will check it out.

Supplementary material

R: A nice graphical representation of Table S2 should be added to the main part of the manuscript. Results of all scenarios should be validated against station data. Perhaps you could realize two scatterplots: one for the windward side and one for the leeward side? Add observation period to the table (or new table, see comment Figure1!).

A/C: I will extend table S2 with additional columns that indicate the observation period and the results of the extreme scenario and DR scaling. If the scatterplots are meaningful I will gladly add them.