Cloud effects on the surface energy and mass balance of Brewster Glacier, New Zealand

Authors response to Anonymous Referee #2

We thank the reviewer for taking the time to make a thorough and careful review of the manuscript. We are glad the research was found to be both new and relevant, and the conclusions sound. We have made the suggested revisions and are confident this has improved the presentation of the results and the overall manuscript greatly. A point by point response to the reviewer's comments is made below, including textual changes where appropriate.

Responses to specific comments:
(Note the reviewers' text is quoted in blue)

Summary:
A study of the impact of cloud cover on the surface energy and mass balance for an in situ station on Brewster Glacier, New Zealand was conducted. Used measurements of atmospheric variables, fluxes, snow depth and density, in conjunction with a surface mass balance model to study the effects of clouds. They conduct a model sensitivity study to examine how clouds affect the sensitivity of SMB to air temperature. They find that for this location, clouds dramatically affect the SEB, and enhance the frequency, and to a lesser extent the magnitude of melting, primarily by changing the direction of net longwave radiation at the surface. A sensitivity study suggests that under cloudy conditions, SMB is more sensitive to fluctuations in temperature. The authors argue that the importance of clouds and atmospheric moisture should be recognized when studying glacier climate interactions.

General Comments:
This study is well written and I think that the conclusions are scientifically sound. The authors sometimes do not explain some statements carefully, and as a result, the results section is sometimes difficult to understand. I think that the study points to an important factor that is sometimes overlooked and should be considered, and therefore is new and relevant research. Therefore I believe the study should be published after the revisions suggested below, which are minor in the sense that they are related to the presentation of the material.

Some general points are:

1. The authors do not define what they mean by the "snowfall-albedo feedback" or "accumulation-albedo feedback", which is not necessarily a positive feedback. This should be clarified below (see specific comments). We have clarified the terms used – we now use "albedo feedback" to refer to the process where increased air temperature decreases the fraction of precipitation falling as snow, which reduces the duration of snow cover and increases the energy available for melt through the lower albedo of the ice surface. Thus the "albedo feedback" here can be seen as a positive feedback between air temperature and SMB as it increases the . Changes and additions to text:
P. 976, Lines 13-14: "The sensitivity of SMB to changes in air temperature was greatly enhanced in overcast compared to clear-sky conditions due to more frequent melt and changes in precipitation phase that created a strong albedo feedback."

P. 977, Lines 5-6: "Reduced solid precipitation often results in an albedo feedback that increases melt, thus increased air temperature can result in enhanced melt if the amount of precipitation that falls as snow decreases."
P 978, Lines 7 – 9 "While a change in precipitation phase and the associated albedo feedback has been shown to be an important component of the sensitivity of SMB to air temperature in New Zealand as in other glaciated regions, (Oerlemans 1997; Anderson et al., 2006), there is a suggestion that increased turbulent (mainly sensible) heat fluxes dominate variations in melt (Anderson et al., 2010)."
P 984, Line 23 "To enable the amount of solid precipitation to alter albedo within SEBpr,"
P990, Line 27 added: "It is worth clarifying here that changes in snowfall resulting from the perturbations in T_a in this analysis are due solely to changes in the fraction of precipitation falling as snow versus rain. This is distinct from the atmospheric feedback between air temperature and precipitation that can result in increased accumulation due to enhanced precipitation rates in a warmer climate."
P992, Line 13 added: "This albedo feedback occurs as increased air temperature decreases the fraction of precipitation falling as snow, thus decreasing the duration of snow cover and reducing summer snowfall. In order to isolate this albedo feedback, further runs of SEBpr were made for - 1 K and + 1 K scenarios."
P994, Line 17 "Indeed, roughly half of the sensitivity to T_a is due to an albedo feedback…"
P996 Line 16 "The high fraction of melt due to *SWnet* and large contribution of an albedo feedback to ΔSMB…"
P997, Line 11 "The large sensitivity of SMB to T_a was expressed primarily through changes in the partitioning of precipitation into snowfall and rainfall, as well as the associated albedo feedback."

2. The authors should comment further on the potential of the methods used to distinguish between cloudy and non-cloudy conditions to impact the results.
The paragraph describing the cloud metric has been extended to include a much through discussion of the method used, including the influence of atmospheric water vapour content on $LW\downarrow$, and a discussion of the uncertainty of the resulting cloud metric. Text changed/added on **page P. 981, Lines 8-15:**

"The longwave equivalent cloudiness ($N\varepsilon$) used in this study was determined from measurements of $LW\downarrow$ and theoretical upper (overcast) and lower (clear-sky) values of $LW\downarrow$ that are based on surface level meteorological variables, a method that has been used successfully in other glaciated areas (van den Broeke et al., 2006; Giesen et al., 2008). The dataset and specific methods used here are presented in Conway et al. (2015), but a brief summary is given below. At each half-hourly interval a theoretical upper limit for $LW\downarrow$ is set by applying the Stefan–Boltzmann law to the observed T_a and an emissivity of 1. A lower limit is set using the clear-sky model of Konzelmann (1994), which has both T_a and e_a as dependant variables. These two curves are assumed to represent the minimum and maximum $LW\downarrow$ at a given T_a and e_a, corresponding to cloudiness values of 0 and 1, respectively. By assuming that cloudiness increases linearly between these minimum and maximum values, $N\varepsilon$ is then calculated from measured T_a, e_a and $LW\downarrow$ at each half-hourly interval. Following Giesen et al. (2008), clear-sky conditions are defined when cloudiness values are smaller than 0.2 and overcast conditions are defined as cloudiness values larger than 0.8.

The inclusion of e_a, as well as T_a, as a dependant variable in the calculation of theoretical clear-sky $LW\downarrow$ was necessary as clear-sky $LW\downarrow$ is strongly dependent on both variables at this temperate location (Durr and Philipona, 2004; Conway et al., 2015). The effect of this is to include a larger proportion of days in the clear-sky category, as some clear-sky days with high e_a (and $LW\downarrow$) would have been excluded had only T_a been used in the calculation of clear-sky *LW*↓. A comparison to cloudiness derived from incoming shortwave measurements gave a correlation coefficient of 0.89 and a root-mean-square-difference (RMSD) of 0.19 (Conway et al. 2015), suggesting the method is a satisfactory approach to assess cloudiness at this site.

Though not directly comparable to traditional cloud fraction metrics based on manual or sky camera observations, $N\varepsilon$ effectively characterises the effects of clouds on surface radiation fluxes. It also has the advantage over metrics based on *SW*↓, in that it provides 24 hour coverage and is not affected by solar zenith angle or multiple reflections between the surface and atmosphere."

3. In general some statements, particularly with regard to interpretation of results are unclear, as mentioned below. Each specific comment is address below.

Specific Comments

1. P. 976, Lines 13-14: The impact of precipitation on the surface-albedo feedback depends on whether the precipitation falls as rain or snow. Snow would induce a negative feedback, while rain would contribute to the positive feedback. See General Comments #1. In the sensitivity analysis the amount of precipitation did not change as a function of air temperature, only the precipitation phase. Therefore, the analysis contains no mechanism to produce the negative feedback associated with enhanced solid precipitation as a result of increased air temperature, described by Box et al. (2012). The feedback we describe is related soley to the transition from snowfall to rainfall and the corresponding change in albedo. Text changed:

P. 976, Lines 13-14: The sensitivity of SMB to changes in air temperature was greatly enhanced in overcast compared to clear-sky conditions due to more frequent melt and changes in precipitation phase that created a strong albedo feedback

2. P. 977, Lines 5-6: I am not sure what the authors mean by the "strong positive feedback between accumulation and surface albedo". Warmer conditions can lead to increased precipitation, which increases surface albedo if the precipitation falls as snow, reducing the energy available at the surface for melting and grain size metamorphism. This is a negative feedback. (e.g. Box et al., 2012). However, a transition from snowfall to rainfall can lead to a positive feedback. Please clarify here and throughout the paper.

Box, J.E., Fettweis, X., Stroeve, J.C., Tedesco, M., Hall, D. K., and Steffen, K.: Greenland ice sheet albedo feedback: thermodynamics and atmospheric drivers, The Cryosphere, 6, 821-839, doi: 10.5194/tc-6-821-2012, 2012. See General Comments #1 and above comment. Text changed:

P. 977, Lines 5-6: Reduced solid precipitation often results in an albedo feedback that increases melt, thus increased air temperature can result in enhanced melt if the amount of precipitation that falls as snow decreases.

3. P. 977, Line 15: I am not sure I agree that the effect of clouds is "far more pervasive". Perhaps the authors mean to say that clouds have a strong effect on *variations* in the SEB? What timescales are being considered here? I would argue that all of the factors mentioned by the authors are important, and may be more or less important depending on the location or timescale being examined (e.g. surface albedo variations may be most important for the seasonal SEB variability, while clouds may dominate day-to-day or hour-to-hour variability. The authors also mention differences in the SEB for different glaciers in the Discussion section.) I think the authors should not diminish the importance of other factors, which does not diminish the importance of clouds to the SEB. We agree that many elements of the meteorological forcing are important to SEB. We have changed the sentence to: "The strong effect of clouds on glacier SEB has received increased attention in the last decade".

4. P. 979, Line 2: Please include the years covered during the 22 month period. Added: "in 2010 – 2012"
5. P. 979, Line 9: Please indicate what the dataset is a hybrid of. The sentence has been changed to clarify the dataset used: "To test the sensitivity of SMB to changes in surface climate and radiative components, a more heavily parameterised version of the model is used. This model allows us to separate the effects of changes to surface climate and radiative properties as well as assess the influence of clouds on the sensitivity. The sensitivity analyses are run using a two-year time series (sensitivity period) that was constructed from data collected in the measurement period."
6. P. 980, Line 19: Clarify whether the bias introduced by the instrumentation is -0.7°C, or whether the correction to the original dataset is -0.7°C. Changed to: "Raw T_a data were corrected for the overestimation of T_a measured in the unaspirated shields during times of high solar radiation and low wind speed. This resulting in a mean correction to the original dataset of -0.7°C."
7. P. 981, Lines 8-15: Can the authors provide further discussion of errors that may be associated with this method, and validation of the emissivity model? Can the authors be certain that the changes in LW radiation are indeed associated with clouds, and not other factors, such as atmospheric water vapor content? See General comment #2. This paragraph has been extended to include a detailed discussion of the method used to derive cloudiness, including the influence of atmospheric water vapour content.
8. P. 981, Lines 22-24: This sentence is unclear. Are the effects of evaporation and condensation on surface meltwater accounted for in the model? Yes, evaporation and condensation remove/add mass to the liquid melt water at the surface. Text clarified.
9. P. 982, Lines 9-13: Can the dates covered by SMBmr and SMBpr be reiterated here? We have clarified SMBmr is run over the measurement period and direct readers to Sect. 2.5 for further details on SMBpr.
Also, specify where the inputs to SMBpr parameterizations come from for clarity. Table 2 provides references for each parameterisation.
10. P. 982, Line 18: How is upward heat flux at the bottom of the subsurface model determined? The model holds the bottom temperature fixed at 0 °C, and the heat flux at the bottom of the subsurface model is calculated based on the temperature gradient between the lowest two model levels (5 and 7 metres). Text added to clarify the specification of the bottom temperature.
11. P. 984, Line 10: What were the values used? Reference to Section 2.2 added:
"The depth, density and temperature (iso-thermal at 0 °C) of the snowpack was prescribed at the start of the measurement period from snow-pit measurements (see section 2.2), while the bottom temperature in the subsurface module was held fixed at 0 °C."
12. P. 984, Lines 15-16: Why didn't the authors use the period 1 May to 24 October 2011? It seems that this would allow for a more continuous period of measurements. This period is included in the hybrid dataset. The particular periods used were (in order): 1 May to 1 September 2012, 2 September to 24 October 2011 and 25 October 2010 to 30 April 2012
13. P. 985, Line 5: How would this be a positive feedback? Does the increased albedo lead to more snowfall? The sentence was removed as it was not essential.
14. P. 985, Lines 10-12: This sentence should be moved to the previous paragraph, as it is describing another modification to the albedo scheme. It is not entirely clear, but I think this modification has also been applied in the generation of the modeled timeseries in Fig. 2a. The second paragraph describes the tuning of model parameters to local conditions, so it is preferred to keep the comment on t^* in this paragraph. The caption of Figure 2a has been modified to indicate that locally determined coefficients were used.

15. P. 985, Line 21: Table 6 is mentioned before Tables 4-5. Perhaps the authors can refer to the results section rather than Table 6, move Table 6, or simply mention the parameters that were changed here. Changed to "(introduced in Sect. 3.4)"
16. P. 985, Line 26: Change "multiplying" to "multiplying half-hourly ΔSMB" for clarity. changed
17. P. 986, Line 8: Figure 2b is mentioned after Figure 3. I think Fig. 2b needs to be mentioned sooner, perhaps when albedo is discussed, or a separate figure that follows Fig. 3 should be created. Fig. 2 has been split into two separate figures, with Fig. 2b (now Fig. 4) following Fig. 3.
18. P. 986, Lines 10-11: This sentence is unclear. What is meant by "winter accumulations", the total amount of accumulation during winter months? Please clarify. Changed to "Accumulation during each winter was similar"
19. P. 988, Line 15: The sentence makes it sound as if changes in *e*a are caused by increases in Ts. Perhaps change "associated with" to "accompanied by". Changed
20. P. 989, Line 14: Suggest changing "similar source of energy as Rnet" to "producing an amount of incoming energy comparable to that of Rnet". Changed to: "producing a source of energy comparable to that of…"
21. P. 989, Lines 20-21: This sentence is unclear. Isn't the higher level of melting during cloudy conditions a consequence of differences in the energy budget, rather than a cause? Or are the authors trying to say that if there weren't melting, the energy available for melting would be even larger? Please clarify. The sentence has been reworded to clarify meaning: "While mean QM was similar in clear-sky and overcast conditions, melting occurred much more frequently in overcast conditions (Fig. 6)."
22. P. 990, Lines 4-5: Do the authors mean "LWnet and QS" rather than "LWnet and QC"? Perhaps change "diverged strongly with cloudiness" to "changed dramatically during cloudy conditions" for clarity. LWnet and QC are both negative terms and sentence has been modified for clarity "On average, LWnet and QC were energy sinks during melting periods."
23. P. 990, Line 9: Change "large" to "large sensitivity of" for clarity changed to "large SMB sensitivity (ΔSMB)"
24. P. 990, Line 10: Can the authors briefly reiterate the meaning of ΔSMB here and in Table 7? Is this the average per year value over the two-year sensitivity period? We have added the following to Sect. 3.4: "The mass balance sensitivity (ΔSMB) is defined as the average change in SMB per year for both positive and negative perturbations in each climate variable. For clarity, ΔSMB is expressed as the SMB response to an increase in a given input variable or parameter."
Caption for Figure 7 changed to: "ΔSMB (mm w.e. a^{-1}) to perturbations in surface climate and shortwave radiation terms. While the values shown are the average change in SMB per year for both positive and negative perturbations in each climate variable, for clarity, ΔSMB is expressed as the SMB response to an increase in a given input variable or parameter."
25. P. 990, Lines 9-22: I think it would be helpful to reiterate here that the magnitude of perturbations is determined but the estimated errors for the input variables. The magnitude of the perturbations has been defined using regular steps in these variables that are similar to previous studies. It is noted in the text where the uncertainty in a variable has been used to define the perturbations.
26. P. 990, Lines 9-22, Table 7: I believe that ΔSMB is the difference between the + and – perturbation runs. This is not entirely clear from this section, and from Table 7. Please clarify this here and in the caption to Table 7. Also, while the left column of Table 7, always shows +/- values, this section discusses the effect of "increases" and "decreases". I think the authors mean an increase from the negative to the positive perturbation, and vice versa; but it appears as if the impact of positive vs. negative perturbations is being examined. Please clarify in the

text and caption. See comment #24. We have clarified the mass balance sensitivity is assessed using the average of both positive and negative perturbations. In order to clarify the direction of mass balance response in the text, "increase" and "decrease" are used to describe the change in an input variable that elicits this response.

27. P. 991, Lines 15-17: Can this calculation be explained in a bit more detail? We have added an extra row to Table 9 and text to clarify the calculation: "By multiplying the contribution of each SEB term to the increase in melt by the fraction melt contributes to the total ΔSMB (77%; Table 8), we find the contribution of each SEB term to the ΔSMB (Table 9, F).

28. P. 991, Line 25: Change "on the ΔSMB to Ta" to "on the relationship between ΔSMB and Ta" or something similar. Changed to: "relationship between SMB to T_a"

29. P. 991, Line 28: "accounting for 50%". Since the sentence begins with "In absolute terms", the absolute amount should be mentioned here, rather the percentage. The percentage values are also interesting, and could still be included. Alternately, the sentence could begin with "In relative terms". The sentence has been reworded: "Overcast periods exhibit the largest change in melt between T_a perturbation runs, accounting for 50% of…"

30. P. 992, Lines 9-10: Change "ΔSMB in clear-sky conditions showed a long period of minimal ΔSMB from May…" to "During May through October (inclusive) ΔSMB during clear sky conditions was minimal." Changed to: "From May through October (inclusive) ΔSMB in clear-sky conditions is minimal."

31. P. 992, Line 14: What is meant by "perturbing Tr/s with Ta"? This is unclear. Changed to "…perturbing $T_{r/s}$ by the same magnitude as T_a…"

32. P. 992, Lines 16-24: I'm not sure that Figure 9 supports the assertions being made here. An annual plot of ΔSMB (direct) as a fraction of ΔSMB (full) would reveal whether this argument is supported by the graph. Also it is not clear how changes in snowfall during cloudy conditions affect the change in SMB; is this due to a switch from snow to rain? Please clarify, and include the additional plot if possible. See earlier comment on snowfall in cloudy conditions. I presume the reviewer is referring to Figure 8 here. The argument made in this paragraph is that cloudiness has a strong influence on ΔSMB, which Figure 8 shows sufficiently. The separation of the change in accumulation and albedo are only one component of this, and the annual contribution of each is discussed in the preceding section, so we feel an additional figure is not needed. We have clarified the feedbacks contained in the model and that changes in snowfall are due soley to changes in the fraction of snowfall versus rainfall, rather than a change in the magnitude of precipitation. A Δ was missing from line 19 which should read "somewhat less than the full ΔSMB…"

33. P. 993, Line 3: Please clarify "The strong divergence of SEB with cloud condition", perhaps changing the phrase to "The large difference in SEB terms between clear and cloudy conditions…" Changed

34. P. 994, Line 11: Change "high sensitivity of SMB" to "high sensitivity of SMB to Ta". Changed

35. P. 994, Line 13: Suggest changing "overcast conditions which" to "overcast conditions which this study suggests", as it is not clear whether different conditions in the Alps would produce different effects. Changed

36. P. 994, Lines 25-26: Can the authors be sure of this, given that this study only covers one location? Perhaps change "appears to have been" to "may have been". Changed

37. P. 997, Line 11: I think the authors mean changes from snowfall to rainfall. Please clarify. Changed to "the partitioning of precipitation into snowfall and rainfall"

38. Table 8, Caption: Perhaps "sum" should be changed to "cumulative sum" for clarity. Changed

39. Figure 3: Can the authors include the 1:1 line as in Fig. 4, for clarity? A 1:1 line is included and caption has been changed to mention this.

Technical Corrections:

1. **P. 977, Line 25:** Change “properties” to “properties,” Changed
2. **P. 981, Line 23:** Change “surface temperature” to “surface” Changed
3. **P. 983, Line 23:** Do the authors mean “evolution” rather than “evaluation”? Changed to “simulation”
4. **P. 985, Line 9:** Change “responsible for decreased” to “responsible for reducing” for clarity. Changed to “responsible for reduced albedo at other sites”
5. **P. 989, Line 27:** Change “experienced” to “experienced during”. Changed
6. **P. 991, Line 1:** Change “snow fall” to “snowfall”. Changed
7. **P. 991, Line 24:** Change “cloud” to “clouds”. Changed
8. **P. 992, Line 19:** Change “SMB” to “ΔSMB”. Changed
9. **P. 998, Line 28:** This reference should be updated as the article has been published online. Updated