Subject: Response to Anonymous Referee # 2 to the Manuscript titled "Permafrost Distribution Modeling in the Semi-Arid Chilean Andes" by Azócar, Brenning and Bodin.

We would like to thank to the anonymous referee # 2 for her or his helpful comments on our manuscript. We will carefully respond to these comments and suggestions. The referee's comments are given in italics and our response as regular text in blue colors, text changes in the manuscripts are in bold.

R.1	*General comments:* *The paper by Azócar et al. is well written and presents data that is very valuable given that permafrost distribution details in this portion of the world are very lacking. I think that this paper has what it takes to eventually be published in TC however, I currently would describe the paper as incomplete and thus requiring major revisions. As a result, my comments are relatively brief as I feel that I need to see more in order to evaluate the paper more effectively. I do not believe the authors should be discouraged by this but rather strive to include more detail and justification in the revised manuscript. The two major inputs to the model are PISR and MAAT which I agree are really the most important factors for this type of empirical-statistical modelling. I however, have two problems including a portion of the methods and really the what the paper says it does which listed below.* *Production of MAAT data* *The production of MAAT data is central to the model however, I feel the authors give little to no description on this in the methods and results. There could be an entire paper written on this MAAT model and you cannot use the proposed model without these data. I am not being critical of the methods used to create the MAAT model however, they must include more description and results including a map of MAAT distributions. In addition, there is also no mention of surface lapse rates in the area which I see as critical.* We agree with the referee # 2 that MAATs are a critical explanatory variable for the permafrost occurrence model. In addition to changes suggested by referee # 1 in regard to reporting the MAAT model, we will include more details about the model set-up and results. In particular, the beginning of new section 3.1.2.1. was changed as follows to provide additional context and model justification: **Air temperature in mountain areas is mainly controlled by latitude, altitude and topography (Barry, 1992; Whiteman, 2000), considering in particular the effects of global atmospheric circulation patterns, global as well as local differences in potential incoming solar radiation, and adiabatic temperature lapse rates. In order to regionalize (or interpolate) weather station data, most studies therefore utilize regression or hybrid regression-interpolation approaches with a combination of predictors representing elevation, geographic position and local climatic phenomena (e.g., cold air pools),**

depending on data availability and size of the study region (Lee and Hogsett, 2001; Hiebl et al., 2009; Lo et al., 2011).

Added references:

Whiteman, D. C. (2000). Mountain meteorology fundamental and applications. New York, USA: Oxford University Press.

Lee, H., & Hogsett., W.: Interpotaltion of temperature and non-urban ozone exposure at high spatial resolution over the western United States, Climate Research, 18, 163-179, 2001.

Lo, Y., Blanco, J., Seely, B., Welham, C., and Kimmins, J.: Generating reliable meteorological data in mountainous areas with scarce presence of weather records: The performance of MTCLIM in interior British Columbia, Canada, Environmental Modelling & Software, 26, 644-657, doi:10.1016/j.envsoft.2010.11.005, 2011.

The following sentences will be added to new section 3.2.1.1:

Temperature data for eight weather stations were provided by Chile's water administration, Dirección General de Aguas (DGA), and additional data were obtained from mining projects distributed throughout the study area. AAT for a particular year was calculated as the arithmetic average of that year's mean monthly temperatures. Since the consistency of elevation references (e.g., above sea level) of available weather station elevations was in doubt, consistent elevation values were extracted from ASTER GDEM.

In regard to the following comment:

…. they must include more description and results including a map of MAAT distributions. In addition, there is also no mention of surface lapse rates in the area which I see as critical.

Our discussion paper mentioned a (surface) lapse rate of -0.71°C per 100 m as the result of the MAAT model (95% confidence interval: -0.68 to -0.74°C per 100 m).
Due to the already large number (and size) of figures (and tables) in our paper, we would prefer not to include an additional map of MAAT; however, our MAAT map and gridded data is available to the public at the paper's companion website, www.andespermafrost.com. The interactive online map allows the interested reader to zoom to a finer resolution; map displays in the journal article, in contrast, would only allow a fixed map extent and resolution.
The model's residual standard error (RSE) furthermore provides a meaningful measure of model precision. The (between-station) RSE of 0.93°C is reported in Section 4.1 and further discussed in Section 5.2, where we point out that the unexplained variation in MAAT in this study is comparable to, for example, the results achieved by Hiebl et al. (2009) in the Alps. In the revised manuscript we emphasize more strongly than before the over-representation of lower-elevation

	sites among weather stations, and in particular we highlight a possible source of bias related to positive residuals at stations from valley locations.
R.2	*Rock glaciers as a PF indicator* *I fundamentally struggle with the idea that rock glaciers can be used as an indicator of permafrost distribution. How is this paper not a rock glacier favourability index rather than a permafrost favourability index? You can make the argument that it is because the morphological characteristics of the rock glaciers are not considered but aside from this where does permafrost occur in the area where there is not rock glaciers? How does the model deal with this? Again I am not saying this is incorrect however, I feel this is a major issue that you need to address directly in a revised manuscript.* We agree that this is an important question that should indeed be addressed each time rock glaciers are used as indicators as permafrost distribution, even though this has become more and more common in recent years (e.g., Janke, 2005; Boeckli et al., 2012a; Sattler et al., 2016). The model developed in this study is not a rock glacier favourability model: Such models would use rock glacier presence / absence as the response variable, and incorporate topographic characteristics of (e.g.) the upslope contributing area as predictors as proxies for gravitational debris and snow/ice supply (e.g., Brenning et al., 2007). However, the reviewer makes a valid point in raising the question of whether and why a model of permafrost occurrence within rock glaciers provides any clue about permafrost occurrence in their surroundings. While we discuss bias corrections at length in the Methods (Section on Model Adjustments) based on the treatment of this issue by Boeckli et al. (2012b), we take the reviewer's comment as an opportunity to discuss additional underlying assumptions in a revised Discussion section (new subsection: 5.3 Permafrost Favorability Model Assumptions). In particular, one assumption that has not been spelled out explicitly in previous work is that we assume that regression relationships between permafrost and non-permafrost within rock glaciers are the same as in other debris areas. New references: Brenning., A., Grasser., M., and Friend., D.A.: Statistical estimation and generalized additive modeling of rock glacier distribution in the San Juan Mountains, Colorado, United States, Journal of Geophysical Research, 112, doi:10.1029/2006JF000528, 2007.

The following sections will be added to the manuscript:

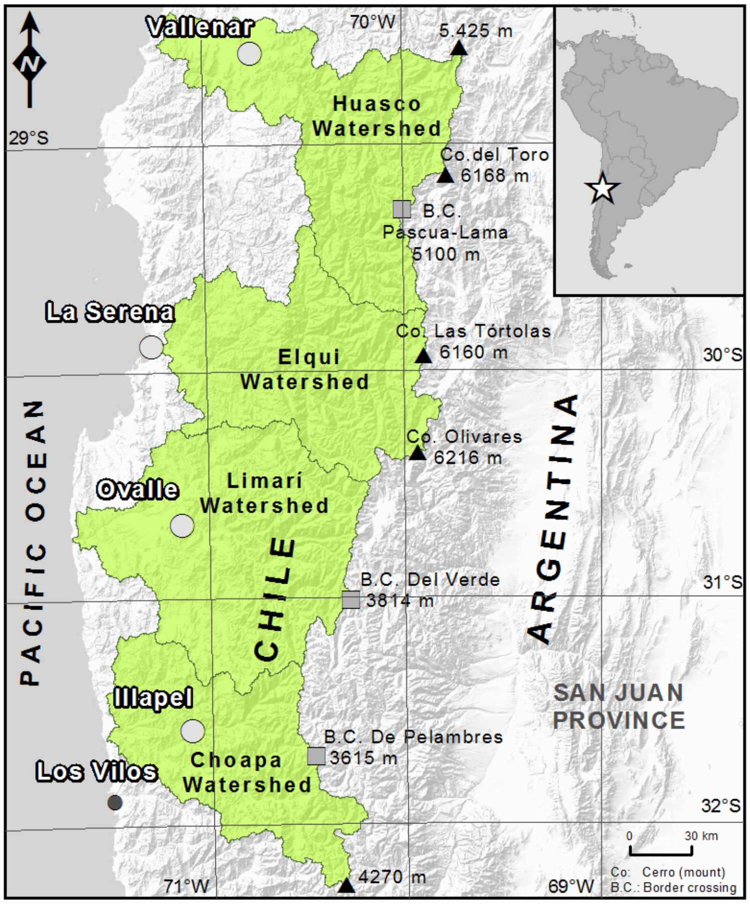
5.3 Permafrost favorability model assumptions

The use of rock glaciers as the empirical foundation for permafrost favorability models requires researchers to make several assumptions, not all of which are equally known in the literature. On the one hand, altitudinal biases related to the movement and characteristics of rock glaciers have been known (Boeckli et al., 2012b), and adjustments are available and have been used in this work, albeit not in all studies relying on rock glaciers (Janke, 2005; Deluigi and Lambiel, 2012; Sattler et al., 2016). However, additional regional calibration will be necessary in the future in order to obtain more precise adjustments. Geophysical soundings or direct borehole evidence would be suitable for this, at least if placed representatively according to a meaningful sampling design.
On the other hand, the transfer of relationships between permafrost presence and predictor variables (e.g., MAAT, PISR) from rock glaciers to non-rock glacier areas also requires the additional assumption that these relationships (e.g., model coefficients) remain more or less the same in debris areas as within rock glaciers. We will refer to this assumption as the transferability assumption. This assumption has previously not been made explicit in the literature despite the frequent application of rock-glacier-based permafrost distribution models.
In combining permafrost models based on rock temperatures and rock glacier activity status, Boeckli et al. (2012a) pointed to a mathematical relationship between (probit) presence/absence models on the one hand and linear regression models on the other (see Section 3.1 of Boeckli et al., 2012a). This mathematical relationship may shed some light on the transferability assumption. Based on this relationship between probit and linear regression models, a sufficient condition for the transferability assumption is that (1) ground temperatures in both model domains (i.e. rock glaciers and other debris surfaces) show similar relationships with MAAT and PISR, and that (2) such linear regressions of ground temperature would have similar residual standard deviations, or precisions. Evidence for or against this is, unfortunately, scarce, since sufficiently replicated ground temperatures have only been measured at shallow depths, and previous studies have paid little or no attention to such differences between rock glaciers and debris surfaces. In the semi-arid Andes, Apaloo et al. (2012) and CEAZA (2012) examined regression relationships between near-surface ground temperatures (NGST) and topoclimatic predictors including elevation (as a proxy for MAAT) and PISR both within and outside of rock glaciers. These studies showed no convincing evidence of differences in ground temperature between ice-debris landforms and other debris surfaces under otherwise equal conditions. While interaction terms of ice-debris landforms with elevation or PISR were not examined in these studies, a re-analysis of data from Apaloo et al. (2012) showed no evidence of relationships between NGST and air temperature or NGST and PISR varying between rock glaciers and other debris surfaces. Thus, while the transferability assumption merits further evaluation in future

	studies, we have no concrete evidence against this assumption in this climatic setting. **The following paragraph will be added to conclusion section:** **Even rock glacier landforms can be used an variable indicative of permafrost distribution in mountain areas, the lack of permafrost observations outside the boundaries of rock glaciers as indicative of permafrost presence or absence, should be address in future studies. Nevertheless, outside of the rock glaciers boundaries, there is not a systematic way to infer permafrost presence or absence in large areas, therefore, for general studies of mountain permafrost distribution, rock glaciers are possibly one of best proxy variable to infer permafrost outsides of boundaries of rock glacier areas.** Added References to the manuscripts: Centro de Estudios de Zonas Áridas (CEAZA).: Caracterización y monitoreo de glaciares rocosos en la cuenca del río Elqui, y Balance de masa del glaciar Tapado, Dirección General de Aguas, Unidad de Glaciología y Nieves, Ministerio de Obras Públicas, Santiago 2012.
R.3	*Other comments:* *The use of the word altitude is completely incorrect in many portions of the paper. Altitude is above the ground and elevation refers to locations on the earth surface above sea level. Certain terms like ELA can remain because this is used in the literature (although technically incorrect) but all others must be changed.* Changed as requested

<table>
<tr><td>R.4</td><td>Figure 1: use a hillshade rather than just the DEM. Additionally, use an inset map to show where in the world this is.

Changed as requested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<tr><td>R.5</td><td>Figure 3 (possible in text) comment on where permafrost is present outside of rock glacier locations

We don’t understand the suggestion, provide more details please.</td></tr>
<tr><td>R.6</td><td>Figure 3: include some mention of surface lapse rates in this figure.

Surface lapse rates are not calculated in this research.</td></tr>
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