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# *Interactive comment on* "Spatial and temporal variations of glacier extent across the Southern Patagonian Icefield since the 1970s" *by* A. White and L. Copland

**A. White and L. Copland**

awhit059@uottawa.ca

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The authors would like to express their appreciation for the detailed comments. Below are the responses.

RESPONSE TO ANONYMOUS REFEREE #2

COMMENT: The introduction section needs to reason much more strongly why the authors believe area changes are important, especially when the dominant causes for length and area change may be only indirectly linked to climate. The work needs to more fully investigate and cite the increased number of recent relevant studies that concern the various mass loss estimates from the icefield over more recent timescales



e.g. [Dietrich et al., 2010; Ivins et al., 2011; Jacob et al., 2012; Willis et al., 2012]. These would help put the behavior of any individual glacier in to context. Area and glacier length changes are relatively easy to measure, but thickness and mass changes are a better estimate of the health of the glacier system as a whole.

RESPONSE: We agree that thickness and mass changes provide the ideal measure of glacier health, but this information simply isn't available since the 1970s for the majority of the SPI. We have therefore improved discussion of controls on glacier area changes, including a discussion why they are likely not only related to climate „ but other factors such as calving dynamics as well. The recent studies suggested by the reviewer have also been incorporated.

COMMENT: The rationale for simply splitting the icefield in to four quarters is somewhat arbitrary and probably not useful – are the four segments expected to act differently? A more elegant classification method could be based upon terminus type - lacustrine vs tidewater calving vs ending on land. Geographic position could be used - on the maritime west side vs on the drier east side; latitude based, or geometry based - AAR ratio, or aspect, for example. The paper could provide new insight if the area changes were examined in such a context and ideally resubmission would include an even more nuanced classification scheme, mixing the above suggestions.

RESPONSE: The icefield was split into four quarters to facilitate analysis of the spatial variability in measured glacier area changes for the large number of analyzed glaciers. We agree that there are many ways that the ice mass could be divided, but this way allowed us to assess the main controls on glacier variability that have been identified in previous studies –i.e., difference between maritime west side vs. drier east side, and difference in gradient from north to south. Our analysis has now been expanded to include an analysis of the relationships between glacier shrinkage rates, glacier size and frontal environment (water vs. land terminating)

COMMENT: The GLIMS catalogue, which should be cited fully, is used to delineate

basins. These outlines are then “verified and updated” using ArcMap routines applied to the SRTM – but SRTM is a lower resolution raster than either the vector polygons from GLIMS or the higher resolution orthorectified images that GLIMS uses to derive basins. This seems like a path to degrading the original basins.

RESPONSE: We now fully reference the GLIMS catalogue. For our verification process, our point is that we wanted to independently check the GLIMS outlines, particularly in areas of snow covered, low angle terrain in the upper parts of basins. In these locations it is very difficult to define basin boundaries using visible images such as ASTER or Landsat as there is little surface definition. Instead, basins are best defined from flowpaths defined from DEMs; the SRTM data provides the best available elevation data for the majority of the SPI. We have now modified our wording in the manuscript to provide more detail about the reasoning behind our verification process.

COMMENT: Absolute rates of area change are of dubious value, normalizing to the total area of the glacier at a set point in time would provide more insight into which glaciers are changing most.

RESPONSE: Both absolute and relative measures of area change have now been included throughout the manuscript

COMMENT: It is not clear in the methodology whether the authors are truly measuring area changes, or are inferring them from length changes. The small section on debris covered ice is confusing. It notes that if flow features are not observed on the debris covered part of the ice, then the glacier ice limit is taken to the boundary between ice and debris – It is not clear how changes in this boundary equate with changes in the area of the glacier.

RESPONSE: The methodology has been updated to clearly state that area is being measured. The section on debris covered ice has also been updated and clarified

COMMENT: The spatial coarseness of the climate data (2.5 degrees by 2.5 degrees)



included in the paper preclude their use for this type of analysis. Relying on two cells from the NCEP reanalysis that provide air temperatures at 2m above surface level means little in this region of high relief, as the relief that is not reflected in the NCEP resolution. The passive microwave analysis of Monahan and Ramage [2010] provides an idea of the real spatial scales involved when considering the seasonal and annual effects of changing atmospheric variables on the icefield. They show surface melting and refreezing through time that is distinctly related to local climatology (maritime versus in the rain shadow etc). They indicate a lengthening of the melt season over a short timescale - 2002 to 2008.

Koppes et al., [2011] provide a good example of refining the NCEP reanalysis for a local region at the Northern Icefields. We acknowledge that the region is data sparse and more intense investigation in to the local climate is a major undertaking. We note that the changing ratio of snow to rain at the more meaningful 850 hPa height has been previously investigated in Patagonia [Rasmussen et al., 2007] and should be cited.

RESPONSE: We appreciate that NCEP reanalysis of surface temperatures can be problematic in areas of high relief, so we have omitted our climate analysis and have provide a thorough literature review that summarizes the results of previous climate studies in the region (Escobar et al., 1992; Ibarzabal y Donángelo et al., 1996; Carrasco et al., 2002; Giese et al., 2002; Bown and Rivera, 2007; Rasmussen et al., 2007; Aravena and Luckman, 2008; Monahan and Ramage, 2010).

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Interactive comment on The Cryosphere Discuss., 7, 1, 2013.