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Interactive comment on "Quantifying mass balance processes on the Southern Patagonia Icefield" *by* M. Schaefer et al.

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Schaefer et al (2014) have developed a robust mass balance model for the SPI that incorporates the best available regional climate data. The model itself has much to recommend it and is not the focus of this evaluation. The determination of calving flux is also an important step in mass balance assessment in Patagonia. The validation of the model presented here is insufficient. As a result though the model results may be quite good, this is currently unknowable. Some of the suggestions below may have been considered or even applied and simply not reported here. With increased validation this paper I am sure will be a fine addition to this group's ongoing important work quantifying the mass balance components of Patagonian Icefields.

Ablation zone validation: Compare the results of this model for ablation to the basic well used degree day models for which there is local validation of the coefficients. For example, Stuefer et al (2007) noted mean degree-day factors during summer periods amount to 0.61-0.64 cm w.e. C-1 d-1for ice ablation. These values are comparable to those of other glaciers in maritime regions (Hock, 2003). For snow they indicate typical maritime values of between 0.27 and 0.43 cm w.e. C-1 d-1. De Angelis (2013) modeled ablation using a degree-day factor of 0.65 cm w.e. C-1 d-1 for ice, and 0. 35 cm w.e. C-1 d-1

Precipitation trend verification: The authors identify a recent increase in precipitation that I cannot discern from their data. Aravena and Luckman (2009) identified the dominant spatial and temporal patterns of a network of 23 homogenous instrumental rainfall records of Southern South America but do not identify this increase. The same group of authors in reporting on NPI Figure 5 and 6 do not display this trend in precipitation (Schaefer et al, 2013). Garreaud et al (2013) examine Patagonia climate in detail and derive maximum precipitation of 9000 mm. They also identify no trend, though data ends in 2001. I am not arguing that the trend does not exist or that the modelled results for precipitation are not correct. However, without better comparison and verification the cited increase in precipitation is not demonstrated. The 8.36 m of average accumulated precipitation could also be compared to other model results such as Garreaud et al (2013).

ELA-Balance Gradient verification: The paper does not present a balance gradient which is the standard graph for surface mass balance reporting by the WGMS. Since, there are lots of directly measured balance gradients, the range of possible gradients is well constrained. DeAnglis (2013) Figure 1 provides a range of balance gradients. Is the derived balance gradient from the model used here appropriate?

The ELA is a key measure of mass balance and WGMS plots the relationship between ELA and annual balance for each reporting glacier. The ELA can be approximately observed using satellite imagery and hence can be used for verification. ELA is not mentioned in this paper. For a given year is the ELA correctly modelled? Barcaza et

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al (2009) use satellite imagery to report annual ELA for many years during the 1979-2003 period on NPI glaciers. Willis et al (2012) and Schaefer et al (2013) do use ELA observations for comparison on NPI, so maybe it was done for SPI as well. Table 6 in the latter mentioned paper provides just the comparison that would be ideal. DeAnglis (2013) identified the snowline for SPI glaciers using cloud free MODIS images. For the NPI Schaefer et al (2013) note that nearly all ELA's obtained from the simulation are higher than the observed snowline altitudes at the end of the ablation season. This is the type of comparison that is important.

Verification of overall surface mass balance: The authors assert that the mass balance loss is due to calving for SPI and the surface mass balance is somewhat positive. This implies that glaciers that are not calving should not be losing significant volume. An easy validation therefore is to compare what the mass balance of some non-calving glaciers is with the observed area and volume losses of recent studies. That non-calving glaciers have a positive mass balance does not fit with findings of Davies and Glasser (2012). Of course many of these glaciers are smaller and due not reach the highest elevations. Hence, without a specific validation by the authors this discrepancy is suggestive but not indicative of model issues. Glasser and Davies (2012) note that small annual rates of area loss increased dramatically after 2001 for mountain glaciers north of 52 S including the large icefields. For SPI they noted the fastest SPI loss since 1870 was from 2001-2011. Further for SPI though some calving outlet glaciers are shrinking rapidly in general, small, land-terminating glaciers are experiencing the highest loss. In Table 3 the land-terminating glaciers are shrinking at rates of 0.29% a–1 from 2001 to 2011, compared with 0.08% a–1 for calving glaciers. If the model does not generate negative balances for these land based glaciers that have been losing volume without calving, then the surface mass balance model must be adjusted.

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