General Comments:

This is an interesting paper to read because of both the physical and the social context for the research. The social context is the gold mining operation, which entails removal of some surface ice to access the resource, thus raising concerns about reduction of ice cover supply of meltwater to local streams, an ice cover which is most meaningfully expressed in hectares. To the extent that sublimation loss rather than meltwater loss dominates the mass balance regime of the area, one might argue that the impact of mining operations on meltwater supply to streams is likely to be insignificant. On the other hand one might argue for ice cover retention to preserve as much of a small water supply to streams as possible.

The physical context is the high altitude, thus setting the stage for a predominantly sublimative ablation regime, only somewhat analogous to Antarctica because here there is no iceberg loss. This raises interesting questions about how to model this type of regime, some of which are reflected in my specific comments. They may even have a bearing on why penitentes tend to occur in the lower portion of Guanaco Glacier. Although I have not been to the area, I have seen the pictures and noted the operational challenges that penitentes present to a field measurement programme.

Another interesting aspect of the physical context is an apparent weak summer accumulation regime. Strong summer accumulation is typical of Himalayan glaciers, similarly distant from equator, fed each summer by the monsoon. Lacking such a mechanism for the Andes, some speculate that strong accumulation seasons would be sporadic, coinciding with El Niño events. Hopefully this work will persist to see if this is true. Specific comments follow.

Specific Comments:

p. 1834, l.6-7: '…high shortwave radiation receipt…' Relative to what? Here, rather than state the mean annual value, it would be more effective to state the seasonal high and low values on clear days shown in Fig. 2, and to point out that the summer values are not far below top of atmosphere values, as would be expected given that, at 5324 m a.s.l., the glacier is above almost half the mass of the atmosphere. Also, stating 45 mm w.e. annual precipitation is more meaningful if it is pointed out that sporadic larger precipitation events mark the record, usually during the austral summer. This suggests a summer accumulation mass balance regime, albeit one which is rather weak.

p. 1838, l.7-10: '…surface change measurements…' to end of paragraph. I prefer a raised cosine filter myself, though I am sure that the smoothing procedure used here is fine. More to the point is the snow density used. Would this be the model value of 285 kg m^{-3} stated in Mölg et al. 2008, or were field measurements used? Was fresh snowfall density different from the snowpack density used for sublimation and melt modelling?

p.1838, l.22-23: Restate sentence beginning with 'Mass or energy loss from (gain to)…' as 'Sign convention for mass and energy transfer is positive for surface gains, negative for losses.'

p. 1858, Table 1: The CNRI measures all four components of net radiation. Why not add reflected shortwave and outgoing longwave radiation to the table, with annual means? The associated albedo could be stated in the text. Also, the outgoing longwave mean could be stated as being with or without correction for window heating.

p. 1839, l.24-l.3, p.1840: Perhaps this is the occasion to publish the roughness lengths obtained from the eddy covariance measurements and to state the location from which they were obtained. The use of fixed roughness lengths does not appeal to me as much as the Andreas approach, but there is nothing wrong in doing so. I am more curious to know why the authors did not explore the approach of roughness element description because it strikes me as being relevant to roughness length estimation for penitente areas. Given the small effect of stability correction on the results I won't take issue with the use of the Richardson number.

p. 1848-1849: Section 5.2. I am not convinced of the need to include as many correlations as we see displayed in Table 5, particularly such variables as air pressure, u wind, v wind and snowfall. Albedo and SWI are basically bound up in SW*, so why not focus on SW*? The variable that I would really like know about is the saturation deficit of the air because sublimation of water vapour from Guanaco Glacier seems analogous to evaporation as modelled by the Penman-Monteith model, in which the key drivers are available energy (mostly SW*) and the saturation deficit of the air. To the extent that one is notably stronger than the other, there is an indication that either solar energy or air mass characteristics control the sublimation regime. Correlation with LW* may also be relevant in this regard.

p.1850, l.9, over to p.1851 and Fig. 6: The statements here make sense if days with melting are the colour curves and days without melting are the black curves. Then it is clear that relative gains in all energy components except QG are associated with surface melting. The results displayed in Fig. 6 c, d suggest that turbulent heat transfer tends to be a surface energy supplier on melt and non-melt days, so it may be sufficient in Fig. 6 f to plot the difference between turbulent energy transfer on melt and non-melt days. The same comment could apply to the radiative transfer components. This would make the diagram less 'busy' , allowing one to focus on the key elements of radiative transfer, atmospheric transfer and heat storage.

p. 1850, l.25: '…heat flux is attenuated compared to…' is better expressed as '…heat flux range is smaller compared to…'

p. 1851, l.6-7: Replace '…is observed the night prior…' with '…is observed on nights prior…'

p.1851, l.23: Replace …subsurface occasionally…' with '…subsurface melt occasionally…'

p.1851, l.27-l.4, p.1852: '…on average about 4° C higher temperatures…' According to Fig. 8b vapour pressures rose in accordance with higher temperature, though not to the ice point, so evaporation rather than sublimation would account for moisture loss to the atmosphere during melt periods, when the surface is presumably wet. I note the distinction because of the different latent heats for phase change.