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Interactive comment on "On the interest of positive degree day models for mass balance modeling in the inner tropics" *by* L. Maisincho et al.

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Maisincho et al (2014) provide a detailed PDD model to assess surface mass balance on Antizana Glacier 15 in Ecuador. The results are validated against both the surface mass balance gradient and the transient snowline. The model is also run using hourly and daily data. The result can be both a well constrained model, but also a model run with sufficiently different temporal time step and degree day function coefficients that some detailed findings are realized. At present the level of statistical evaluation of the results both for intercomparison and validation is insufficient. The validation of the surface mass balance record using the transient snowline data should be strengthened,

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as this is the only purely field data set to compare with. At present it is more of a comparison. Attention to two assumptions that precipitation is constant with elevation and sublimation is not important need a better defense. With better validation, statistical significance testing and assumption justification this paper can be a useful contribution

2638-5: Replace “melting amount” with “melt volume” or “ablation”.

2638-19: “were good” is not sufficiently quantitatively descriptive. This type of qualitative evaluation is used many times in this paper and is not a useful measure of statistical significance.

2639-23: The following statement cannot be justified “However, a direct link between higher temperature and increased ablation has never been clearly demonstrated”. In fact this has been repeatedly demonstrated in the Andes and other settings. So maybe something else was meant.

2640-12: Need to specify that the Andes is where short wave radiation is by far the most important.

2643-18: Valuable and important step that using daily as well as hourly values in the PDD model.

2644-7: It is reasonable to assume precipitation is constant with elevation, but is there any reference or data in support of this Urrutia. and Vuille (2003) indicate significant variation, though not specific to the Antizana.

2649-10: How is a 50 cm cylinder of ice obtained and then placed amidst a bigger box of surrounding ice? If it is not left as a whole unit there will be increased surface contact with the area versus glacier ice and melt rates will be inflated.

2649-16: VBP- is not the typical term for this usually mass balance profile or mass balance gradient, both are noted by Cogley et al (2010) but not vertical balance profile.

2650-28: Should be transient snowline.

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2653-6: This is important to cite what values were obtained from other glaciers in this setting.

2655-19 “this significant correlation” what specific correlation is referred to?

2656-17: How reasonable is the assumption that sublimation is negligible? Favier et al (2004) note the significance of sublimation on Antizana during windy days. This is an argument that has to be addressed.

2656-26: The model fit is quite impressive in the elevation range where surface mass balance measurements are made. There is an underestimation of the model for accumulation above 5300m where field measurements exist in 2001, 2004 and 2006. A brief discussion of this and likely causes would be appreciated anytime in the next page.

2659-13 Add transient before snowline. The transient snowline is a variable that has been long discussed in mass balance observation. This is a key validation measure since unlike the balance gradient it is a specific data point. Hence it is the most reliable validation, and I commend the authors for the efforts in building this record. If it is given more attention I am sure the model results will be even more compelling. Mernild et al (2013) talk about the utility of transient snowline variations in mass balance assessment. Why is n=91 when earlier it was stated there was 712 snowline observations?

In Figure 10 it is less important to show us the full time series for which the correlation coefficient is provided than to indicate a particularly representative period in richer detail. Such as 2007 and 2008, there are a few places where there is considerable offset that could then most likely be attributed to a specific observed snowfall event. We could also get a better sense of the standard deviation which should be reported.

2659-20: This should be treated as a more important validation versus comparison as stated earlier. Quantifying the fit should be with more than just correlation coefficient. Also since there were

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2661-15: A good discussion and analysis but the reliance on one station for comparison to Izobamba does not yield confidence. Can a better regional reference for the precipitation comparison come from the regional Andean climate work such as Vuille et al (2003) or Haylock et al (2006).

2662-18: This implies how critical a rise in the freezing level would be. A reference to this and the observations of this fact of rising freezing levels on Quelcaya Ice Cap from Bradley et al (2009) would be important

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Haylock, M. R., et al.: Trends in total and extreme South American rainfall in 1960–2000 and links with sea surface temperature, J. Climate, 19, 1490–1512, 2006.

Mernild, S., Pelto, M., Malmros, K., Yde, J., Knudsen, N., and Hanna, E.: Identification of snow ablation rate, ELA, AAR and net mass balance using transient snowline variations on two Arctic glaciers. J. of Glaciology 59 (216): 649-659, 2013.

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