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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.

L. Maisincho et al.

lmaisincho@inamhi.gob.ec

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We thank Reviewer 1 for his thorough reading of our paper and for the proposed discussion, demonstrating that the question of the possible application of the PDD within the tropics leads to very distinct viewpoints, very likely due to the interest of the authors in studying the physical processes with aim to accurately understand present climate impacts on glacier, or to get simple models to have ideas on past or future climate variations and impacts with a reasonable uncertainty range. We deeply considered his remarks, and agree that our paper may not sufficiently put into perspective the main precautions that PDD model users should take, but, we definitely do not agree with the conclusion presented by Reviewer 1.

If the Editor allows us to submit a new version of this paper, we propose: 1. to more accurately present the significance of every statistics, 2. to present an error estimate based on the sensitivity test and on comparison with field data, 3. to discuss the physical context of parameters. We also propose to attenuate our conclusions and write that this study confirms that glaciers in Ecuador are very sensitive to temperature variations, and justifies why PDD model generally offers good results in Ecuador, even if PDD model users should use this model with caution, due to the potential uncertainty of the model, to the need of further calibration and validation of degree-day factors (DDFs) for distinct periods (as for instance during El Niño or La Niña), and to the potential changes in DDFs over long time-scales.

However, Reviewer 1 comment on an inadequate use of the term "significant" is totally exaggerated (see hereafter in our response), since the insignificant correlation coefficients highlighted by the reviewer have never been considered as significant in the text. This comment is important, and we propose to include significance according to t-Student test in the discussion, but statistics was not a discrepancy in this paper.

Moreover, based on many unpublished data and analysis, this paper presents conclusion on a topic which is still interesting for the tropical glaciology, as reflected by recent literature. Indeed, Reviewer 1 write that applying PDD models under tropical climate has no advantage, but analyzing the interest of this approach is still within the framework of one of the active scientific groups working on tropical glaciology as demonstrated by the recent paper by M. Ortner, W. Gurgiser, B. Marzeion, L. Nicholson, G. Kaser: Skill assessment of simplified glacier mass balance models under observed and changed atmospheric forcing: A case study on Shallap glacier, Peru (see Gurgiser PhD, paper 3 p. 81, http://imgi.uibk.ac.at/sites/default/files/thesisWGurgiserLowRes26112013.pdf to get the pdf of this paper). As a consequence, we believe that getting information on the interest of very simple models already presents a high relevance for the glaciohydrological community working on glaciers of the tropical region. One should also notice that in the interesting analysis from Radíc and Hock (2011), which was based on a PDD approach, the authors were offering glacier wastage projections for tropical Andes (region 14: South America I) without DDFs specific to this region and consequently considering DDF retrieved from other places . As a consequence, offering DDFs values and uncertainty ranges for tropical glaciers is also of interest for large scale approaches. Such DDFs values were not available before.

Moreover, the paper by Ortner et al. presents several interesting elements for the present discussion. One of them is the application of the Kaser's ITGG model, from which the Reviewer mention that he has no news (point 3 section D of the review). This paper presents several conclusions about its interest for application in Peru and to discuss the suggestion made by Reviewer 1 in section D, points 2 and 3.

Indeed, Ortner et al. paper concludes that the ITGG model correctly works in Peru, but demonstrates that a very simple degree-day model would not work there. Unfortunately, the degree-day model used in Ortner et al. study does not separate snow and ice melting, which corresponds to use a degree-day model with only 1 DDF for snow and for ice. As a consequence, their results are not in contradiction with our results, since we clearly write that with a correct evaluation of the DDFs for snow and ice, AND a correct assessment of surface state, the PDD model is able to correctly reproduce melting. Moreover, this paper validates the use of the ITGG model, assuming the Juen et al. (2007) approach. In Juen et al. (2007), the ITGG model is actually an improved degree-day model in which every fluxes are related to T and P. This approach is the one proposed by Reviewer 1, but this is exactly what we already did in our study. It is crucial to observe that, on Antizana Glacier 15, net shortwave radiation (respectively, net radiation) explains 80% (resp. 92%) of melting variability. Here, we correlated every energy fluxes to T and observed that only SWi is significantly correlated to T. As a consequence, in Ecuador the ITGG model would be mainly controlled by a relationship between temperature and SWi: SWi = f(T), and consequently melting will be related to this function as follows: melting = f(T) *(1 – albedo). This is equivalent to our PDD model, if we assume that f(T) is linear. As a consequence, ITGG model would very likely give almost similar results than our PDD model. This gives answer to suggestions proposed by Revierwer 1 in Section D, point 2 and 3:

- point 2: we already offer information to show that a simple model based on a comparison between an energy balance model and typical meteorological variables will be mainly based on correlation between SWi and T, and hence to a typical PDD model with distinct DDFs for ice and for snow. - point 3: the ITGG should correctly work but will just be based on the confirmation of the strong relationship between T and SWi

As a consequence, we already present answer to the main discussion proposed by the reviewer, since point 1 and 4 are mainly opinions, for which we already give several information in the text (see below in our response).

Detailed answer to other comments is made hereafter: "(A) QUESTIONABLE PHYSICAL BASIS" "basically shows that net shortwave radiation is (a) the dominant energy source AND (b) the most variable energy balance term (in absolute numbers). Air temperature will therefore play a role for almost any mountain glacier (except for those clearly above the mean freezing level altitude), since air temperature changes are able to disturb this dominant term in the energy balance through altering the precipitation phase and thus the surface albedo." Yes, we agree with this point, but we also show here that even if the correlation coefficient is weak, SWi is correlated to T, next albedo variations are enhancing this relationship.

"However, a central thought in the PDD approach concerns the annual cycle in air temperature at mid and high-latitude sites, which causes great differences in PDDs between winter and summer." This is true to analyze the seasonal cycle of melting but not to justify whether the PDD model is adapted to analyze inter-annual variations of melting during summer only. The argument of seasonal cycle is thus not totally required. The main reason explaining that the PDD model works in the mid-latitudes is that the main energy fluxes that are related to melting in summer are the sensible heat flux (which is directly related to T), and incoming shortwave radiation which is indirectly related to T because during sunny days the incoming shortwave radiation induces a warming of the surrounding surfaces. In the case of Ecuador the link between melting and T is due to this second relationship, and despite very low seasonal variations of T, we observe that this link is persistent except during P2 because strong winds induce a mixing of the surface layer, disrupting the relationship between SWi and T. However, the lack of seasonal cycle may justify that the way the incoming shortwave radiation warms the surrounding surfaces is quite constant all the year round. In other words, the relationship between SWi amounts and temperature is similar all the year round.

"This central foundation is not met in the tropics, where annual temperature cycles are small and distinct moisture seasonality promotes the importance of sublimation in the mass-balance too (a term not considered in the PDD approach). The fact that the authors can reproduce measured mass-balance estimates to a certain extent is certainly the somewhat unique environment of Antizana Glacier, where relatively moist conditions are found throughout the year." Constant moist and temperate conditions indicate that we are always close to melting, and that any small increase in the incoming energy will induce higher melting. As a consequence, despite the relatively low correlation between SWi and T, any small change in T indicates a potentially high impact on melting. To us, this justifies why the approach works in Ecuador. And we agree that the relationship will not work where temperature cycle is higher (see hereafter).

"I therefore disagree with the statement on page 2664/L5 that the results may promote "widespread application". Equatorial glaciers in Africa will certainly be less suited for PDD modeling, since there are pronounced dry seasons, and the majority of tropical glaciers are in the outer tropics of South America, again with well pronounced dry seasons." Concerning the widespread application we agree that this conclusion was too strong, and we should say 'in Ecuador'. But we never wrote in the paper that we were validating the model for any other application in the outer tropics. We always

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strictly separate the inner and the outer tropics within the paper. Actually we agree with Sicart et al. (2008) concerning the application of the model in Bolivia. If we are allowed to present a second version of the paper we will demonstrate that applying the same approach to Bolivia yields inconstant results demonstrating that Sicart et al. (2008) results were correct in Bolivia, and that the case of Ecuador is specific.

"Even in the less strong "dry" period on Antizana the PDD approach fails, as noted by the authors (although the quantitative importance is limited due to the particular conditions at this site). In summary, my opinion is that the PDD approach is in principle not suited for the tropics by definition, philosophy, and physical background." If the philosophy or the main assumed concept is that a seasonal cycle is required, then the reviewer would be right. But if the main philosophy of the PDD approach is that the main melting processes are related to temperature, then there is no reason to exclude the inner tropics if temperature and incoming shortwave radiation are related, even slightly.

"New concepts and ideas are required to apply simple models to the tropics. It is disappointing that the present study is not heading in such a new direction." We agree that new concepts would be interesting, but the proposed new concept suggested by the reviewer (see section D, point 2) is exactly what we did here: we compared energy fluxes to temperature and retained that the relationship between T and SWi was the most significant. We should have presented that turbulent heat fluxes are related almost exclusively to wind speed and may have produced a new PDD model in which sublimation is retrieved, but it was beyond the scope of present study. We can introduce computation of sublimation point in a revised version of the paper to get a better model for P1 if this is required.

"(B) FLAWS IN THE STATISTICAL PROCEDURE" "A central criterion for the model is the correlation between temperature (daily mean or cumulative values) and daily melt amounts (Fig. 3). In addition to the technical problems in this regard (see item 2 below), my impression is that the authors are overconfident in their interpretation." We agree

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with the reviewer that we did not correctly present the significance of DDFs in Figure 3, and that statistics should be discussed more deeply in the paper. Nevertheless, we were not over-confident on our DDFs, and this is exactly why we performed a 4-level validation: 1. with daily melt boxes; 2. with daily energy balance results; 3. with annual mass balance; and 4. with the ELA and snow-line modeling. Consequently, we demonstrated that we did not base our results on Figure 3 only. We also demonstrated that DDFs may change according to years, this suggests that we know that our DDFs are not strictly validated.

"The key here concerns page 2652, where the authors state that the correlation between cumulative temperatures and measured melting (Fig. 3b) is similar to the results from a full energy balance model (Fig. 3c). However, for the most reliable case (blue; where the likelihood is high that comparable surface conditions were found at the AWS and the melting boxes, i.e. ice) the R2 is much higher for the energy balance model (0.84) than for the PDD approach (0.58). Assessing these two numbers as "similar" seems a questionable interpretation. Moreover, the mean daily temperature (Fig. 3a) performs obviously worse than the energy balance approach (Fig. 3c)." If we have the opportunity to submit a new version of this paper, we will discuss our coefficients and consider this relevant comment provided by Reviewer 1.

"(2) Even more important with regard to the central assumption that there is a clear correlation between air temperatures and melting, is the absence of a statistical significance test. If there are rather few data points for a regression analysis, as is the case in Fig. 3 for the different colors, a standard significance test must be added to check whether the regression is "real" or by chance. I requested the data for Figs. 3a/b and made such a test (based on t statistics). For mean daily air temperature, the correlations with snow and dirty ice cases do not pass the 95% confidence level and are therefore not significant. Their respective p values are 0.25 and 0.13 (i.e., >0.05). Hence, the existence of the claimed correlation between air temperatures and daily melting is not fully supported by statistics." This statement is clearly exaggerated. First of all, we never wrote that these coefficients were significant. We wrote (lines 4-5, page 2652): “However, despite this classification, mean daily temperature was poorly correlated with the measured melting rates (Figure 3a).” and (lines 16-17, page 2652) “The correlations were more significant when we assumed the cumulative hourly positive temperature values computed at a daily time step (Figure 3b)”. If Reviewer 1 refers to coefficients for Cumulative hourly positive temperature, the respective p values are systematically < 0.05 (even 0.025) even for snow, which is the less significant correlation coefficient of Figure 3b. Moreover, except for the mentioned coefficients of Figure 3 and those of Figure 4, all our R are systematically given with the number of points of the studied sample allowing any author to compute significance. In the paper, except for Figure 3, when we say that correlations are significant it is at $p=0.001$, but for a sake of clarity we will replace the number of point by the degree of significance if the Editor allows us to submit a new version of this paper. As a consequence, we propose to clearly write p values in the new version, and to include that DDFs should be considered with caution, in particular for snow, due to the limited significance of correlations. However, we feel that rejection of the paper on statistical arguments would be totally unjustified.

“(3) Most likely due to issue (2) above, the term "significant correlation" is used incorrectly throughout the manuscript. Whenever your model is based on statistics, you must use the term "significant" only for a statistically significant relation (i.e. for one that passes the significance test). In this paper the authors seem to use "significant" as a synonym for "strong" or "clear" or "real". For example, on page 2653 the authors state "Except during Period 1 when high winds and low moisture induced high turbulent heat fluxes, the cumulative hourly positive temperature values were significantly correlated with incident shortwave radiation and with the net shortwave radiation..." Due to the missing significance test, the reader can only guess what the authors mean by "significant". Is a R^2 higher than 0.4 significant for the authors? This is not in line with generally accepted statistics. Please change your wording in this respect.” We will include significance with each correlation coefficient.

“(4) Another issue tied to the low number of data points in the various classes of Fig. 3, is the fact that all data points are used for calibration but none are used for evaluation. Therefore we learn nothing about the uncertainty in the relation between temperatures and melt. Finding test data is not trivial for a small sample size, but a standard procedure in this case is so-called cross-validation, which has also entered glaciology literature more recently (e.g. Marzeion et al., 2012). This allows you to quantify the uncertainty in the regression, in this case uncertainty in the DDFs. Such an uncertainty assessment is a great addition to any statistically-oriented model.” We will take into consideration this remark and consider the method proposed by Marzeion et al.

“(5) The present model does not quantify the expected uncertainty (the 20 lines discussion of Section 5 are insufficient in this context). In particular if model application in forthcoming studies is intended outside the time of measurements (for paleo or climate scenario problems), which the authors indicate in the paper, a useful model must include an uncertainty quantification.” We do not agree with this remark, the sensitivity test clearly gives idea on the model uncertainty. However, if we are allowed to submit a new version of the paper, we can offer more details.

“(6) Finally, while the initial DDFs are calibrated from cumulative hourly temperatures, the final model uses mean daily temperature as input. This is (a) inconsistent and (b) a weak foundation since mean daily air temperatures fail the statistical significance test (see item 2 above). If you know from the start that cumulative half-hourly or hourly temperatures will be hardly available for future applications, you should not use it.” As stated in the paper, we applied the same model to cumulative positive temperature and it yielded very similar results. We do not see where it is inconsistent since we warn the reader that we make a strong assumption when we use DDFs values which where calibrated on cumulative positive temperature instead of mean daily temperature. Our comparison shows that it has only low consequences at the annual scale.

“(C) QUESTIONABLE MODEL QUALITY (1) Due to the problems outlined in Section (B), some model parameters seem ques- tionable. Most of all, the "optimal" threshold

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between clean and dirty ice is finally increased to 0.45, which seems very high to me. An ice surface with an albedo of 0.4 is usually not that dirty. For example, Oerlemans et al. (2009) shows that dirty ice measurements feature way lower albedos. In general, the authors do not discuss the optimal model parameters in their physical context." Clean or dirty ice clearly depend on the amount and on the origin of dusts accumulated on the ice, but without getting any composition of the mineral and concentration of dusts, any comparison with other sites would be purely speculative. This explains why we did not perform any description of this parameter in its physical context. However, albedo never goes below 0.25 on Antizana, and lowest values where only reached in 2002-2003, mainly after the eruption of the volcano Reventador. However, we propose adding a discussion of this parameter with other values available in the literature.

This also concerns the precipitation correction factors: 0.71, 0.74 or 0.5 (depending on data input type). While, in contrast to the albedo parameter above, a precipitation correction factor <1 seems reasonable to me from a physical perspective (since convection will play an important role in the precipitation generation at that site), it should be discussed what <1 or >1 means in the real world, and whether this makes sense." This parameter is directly obtained with a comparison between field data and other precipitation sources, and is due to the effect of shelter made by the Antizana Volcano on precipitation fluxes coming from the Amazonian side. Such effect is not visible in reanalysis data, and in precipitation at largely lower elevation in Quito. We propose to include elements on this point even if local foehn effect was largely discussed in Favier et al., 2004.

"(2) A central difficulty for the reader to assess model quality is the fact that the authors compare their final results to mass-balance measurements that are not published yet. These measurements are cited as Basantes Serrano et al. (2014), but the reference list shows that this paper doesn't yet exist. Hence it is hard to understand how good the basis of the model evaluation is, since the underlying measurements have not been peer-reviewed so far. I am sure that the circumstance of the non-uniform distribution of point measurements on the glacier will be important for generating observed glacier-wide mass-balances.” The paper by Basantes Serrano et al. presents two main points: an update of the mass balance time series of Antizana Glacier 15 (one of the longest of the entire tropical region), and an adjustment of the annual mass balance time series computed using the glaciological method with the use the geodetic method. The point mass balance data measured on Antizana Glacier 15, even on shorter periods, have been presented in former studies that allow any reader of the paper to know all the necessary information about the measurements and their quality. You can refer to Francou et al. (AMBIO, 2000), Francou et al. (JGR, 2004), Favier et al. (JGR, 2004), Rabatel et al. (TC, 2013). In addition, the Antizana Glacier 15 belong to the GLACIOCLIM network, all the data are freely available on the GLACIOCLIM website (http://www-lgge.ujf-grenoble.fr/ServiceObs/), allowing any reader to get a full knowledge of the used method. This glacier also belongs to the WGMS, and mass balance data are also available through the WGMS website. As a consequence, if we can present a new version of the manuscript, and if the paper by Basantes Serrano et al. is still under review, we will change this reference by former published studies to allow the reader to access available literature.

“(3) Also, the root-mean-square difference must be given in a comparison like Fig. 7, to get a feel for the error. If the error is at the order of the interannual variability, the model will need improvement.” This proposition is relevant and will be added.

“(4) In Fig. 9 some improvements are achieved when DDFs for ice are changed in certain years. This necessity will be a major limitation for model application to paleo or future climate questions (since one cannot know which DDF is better in what year). As indicated above, there should be an error estimate for each DDF. The model as it stands now has definitely low predictive skill (for the past and future).” This was already suggested in the submitted version when we were writing: “This suggests that degree-day factors should be recalibrated for reconstructions of past or future climate.” (lines 16-17, page 2663) We propose to reinforce this statement with one sentence on the uncertainty range of the model and using the results of sensitivity test.

"To summarize (B) and (C), in my opinion the model is not suited for the intended application before (a) an error estimate is included, (b) proper statistical methods are used in the model construction, and (c) model parameters are discussed in a physical context." We agree that these 3 points may be included in a new version of the paper and we propose to more accurately present the significance of every statistics, to present an error estimate based on the sensitivity test and on comparison with field data, and to discuss the physical context of parameters.

"(D) SUGGESTIONS FOR THE AUTHORS I very much hope the authors take my comments above as constructive critique, since I support their general idea to produce longer-term mass-balance in the tropics from simple models. However, please throw some new, exciting ideas in the glaciology community! - Instead of trying to make an old concept (PDD) work in an environment which, by nature, it is not designed for. With a lot of fitting some agreement will always be achieved with various input data (Fig. 7, 9, 10), but the real value of a simple model is a robust physical basis, and this is certainly not the PDD approach for the tropics. Some specific suggestions are:" We already discussed this point in the introduction of this response. Any simple model based on a physical analysis will be based on the relationship between T, SWi, albedo and melting, because S explains 80% of melting and the net radiation explains 92% of melting. As a consequence, any simple model will be very similar to the proposed PDD model. Actually we already performed the proposed analysis, and the present paper already offers the key elements to understand this point. However, we propose adding several sentences to clarify this point.

"(1) I agree with the authors that hourly values make no sense in a simplified model and daily ones are better, but I would even go with monthly input values. This temporal resolution is sufficient in the context of estimating past climates from glacier extents or future mass-balance from climate projections. And measurements are even more common at monthly scale than at the daily one. As long as you resolve the annual cycle (for which monthly steps suffice in the framework of a simple model), you should be fine." This comment made by Reviewer 1 is in contradiction with his comment on the shift we did on the use of DDFs obtained with cumulative positive temperature for a modeling with mean daily temperature. Moreover, please note that we initially wanted to present the Blard et al. (2007) model based on monthly distribution of temperature and precipitation, showing that this model allows retrieving variations of the ELA along the 20th century. Actually, we finally did not include this analysis with Blard et al. model, because the use of DDFs there was largely more difficult to justify. As a conclusion, we agree with the fact that uncertainty range should be more accurately presented, but we believe that validation of a model based on monthly data is beyond the scope of the present study and would require a specific analysis.

"(2) Run an energy balance-based mass-balance model in distributed mode (i.e. for the entire Antizana glacier). If this works, it will greatly help to develop a simplified model later. For instance, the energy balance-based model will reveal the physical processes that shape the VBP of the glacier" This has already been done as explained before.

"(3) I re-call an approach from Kaser and co-authors (e.g. Juen et al., 2007) who also tried to construct a mass-balance model for the tropics using a minimal input (monthly temperature and precipitation). However, in order to consider the tropical climate features, they tried to relate the various glacier energy fluxes to the two input variables. I have not followed the evolution of the Kaser approach in recent years, but for me it is an example how new flavor can be added to simple models." Results from Ortner et al. (see introduction) already give several response on this point. But here we try to demonstrate that such approach may find justification at a daily time scale. The two analyses are thus clearly complementary.

"(4) I would not use precipitation from NCEP or other reanalysis as input! Precipitation is a so-called class C variable in reanalysis and is known to have big problems, especially for mountain regions. If it is intended to run a model with reanalysis input, I would try to express measured precipitation as a function of more reliable reanalysis variables (e.g., temperature, humidity, air pressure). This means I would construct a statistical downscaling model for precipitation." We totally agree that NCEP precipitations are class C variables. Here we just wanted to show that assuming remote and NCEP precipitations has only low impact on modeling quality, demonstrating that temperature is the first order variable that causes mass balance variability. Then any precipitation data that almost correctly reproduces the long term trends of annual mean precipitation will offer correct results. However, for long term reconstruction we were aware in using NCEP data only when field data were not available, showing that we are not confident in NCEP precipitations data. However, as written by Reviewer 1, temperature is a more reliable variable in the reanalysis: this justifies why our modeling correctly works in the present study despite the bad quality of precipitation.

"OTHER COMMENTS (INCLUDING TECHNICAL REMARKS) (1) P2642/L17: "inter-annual climate variability" (add the the temporal reference)" This will be added.

"(2) Equations 1 and 2: There is something wrong with the unit; you have to multiply the factor LR [K/m] with the elevation difference (z - z_ref) [m], in order to obtain a temperature unit. This means there must be a space between LR and (z - z_ref) in the equations." This space will be added.

"(3) P2644/L14: –8 is lower (not higher) than –6.5. You mean a "stronger" gradient" We were meaning "absolute value". This will be corrected as proposed.

"(4) P2648/L8: "grid cell" instead of "pixel"" This will be corrected as proposed.

"(5) P2655/L15-18: You mean taking sublimation NOT into account? The model does not have a sublimation term." We will include the term "not".

"(6) P2664/L8-9: Please be careful with statements like "... temperature is generally assumed to be the only variable that climate models correctly reproduce ..." – First, what exactly does the term "correctly" mean in this context? Magnitude, acceptable error, or variability? Second, what you can say is that temperature is most probably the variable that climate models reproduce best. Third, climate is not only temperature and precipitation. Climate models often do a good job with regard to geopotential heights or air pressure, and thus with regard to dynamics on the synoptic scale." We will change this sentence as follow: "Since temperature is most probably the variable that climate models reproduce best".

"(7) Table 2: 30 min, hourly or daily data? Please specify the time interval for the correlation results. Enter "not available" for the correlation AWS-G1 versus AWS-M, to clarify the records do not overlap." Table 2 presents the availability of daily data. Other suggestions will be included.

"(8) Fig. 3b caption: As noted by the authors in their comment along my data request, the x axis label should be changed. I suggest "mean cumulative temperature" with the unit C per 30 min" This suggestion will be included.

"(9) Fig. 6 is not really necessary (text description suffices)." We will remove this figure.

"(10) Please stick to the glossary of glaciology for glaciological terminology. E.g., you should specify at least once that you study the climatic mass-balance, and sometimes I wasn't sure which type of accumulation (solid precipitation, net, ...) is meant." This will be considered.

"(11) "Negative" air temperatures do not exist. Please use below 0C or below melting point. The same problem is true for "positive", but in the special context of PDD modeling the term "positive" might wok." This will be considered.

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