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Discussions

Interactive comment on "Ideal climatic variables for the present-day geometry of the Gregoriev Glacier, Inner Tien Shan, Kyrgyzstan, derived from GPS data and energy-mass balance measurements" *by* K. Fujita et al.

Anonymous Referee #2

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Review of "Ideal climatic variables for the present-day geometry of the Gregoriev Glacier, Inner Tien Shan, Kyrgyzstan, derived from GPS data and energy-mass balance measurements" by K. Fujita et al.

Summary

This paper presents the results of a mass balance/modelling study for a glacier in the Tien Shan mountains. As such, it reports interesting results, using appropriate methods, from an area which is under-reported in the literature. As such, it should

form an interesting addition to the literature. I do have some concerns, however, which I think the authors need to address before publication, as detailed below.

Concerns

My first concern is in many ways at the heart of the paper, and even in the title. I am not sure what the authors really mean by 'ideal climatic variables'. I assume they mean the climatic regime needed to maintain the current glacier geometry. This is an interesting idea, but I don't think it is important in the sense of understanding mass balance change, as presumably there are many climatic regimes which could also maintain the current geometry, but with different temperature/precipitation combinations. I am not here disputing the findings of the paper, or the methodology; the idea of calculating the summer balance needed to melt the average winter balance is an interesting one, and it fits well with the subsequent discussion in Section 3.6 on mass balance reconstruction, and the finding that the main driver of mass balance variations before 1980 were summer temperatures, but this regime changed to be precipitation-limited for $\sim$20 years before again becoming temperature-dominated. I think this 'story' is a more interesting one than the idea of claiming an 'ideal' set of climate variables for the present-day geometry, and that the paper would benefit considerably from a re-working of this section (and a change in title) to draw this idea out. This could possibly even be extended to allow calculation of the changing glacier geometry as implied by the mass balance evolution with the observed geometry, which would allow some inferences to be made about the role of flow processes within the glacier, which the authors currently acknowledge are not calculated. I also note that all the stake measurements are located in effectively the accumulation area. This obviously cannot be 'fixed' in the context of the study, but I think it should be acknowledged by the authors, especially when discussion the 'whole glacier' mass balance profiles which will be based just on the EBM results, plus the DEMs discussed. This is also relevant for Figure 4b, and in the discussion in section 3.4. The results for the calculated mass balance in 2006-7 match the dGPS profiles, but for 2005-6 do not - whether albedo corrected or not. I think this mis-match needs further discussion in the text.

In these sections, more specifically, I don't think that the close agreement between the measured annual precipitation in the study year and the long-term average can be used to claim that the 'long-term precipitation is a threshold amount...' (p866 lines 15-17), any more than a single year's precipitation being different could be used to claim that the climatic regime had changed.

The wording on p 868 concerning the correlation coefficients is also strange. A coefficient of -0.732 is not 'worse' than -0.675, it is actually a stronger negative correlation. The wording on the discussion of the coefficients for precipitation is also confusing. I can see what the authors are claiming; it just needs to be expressed more clearly.

My next concern is with the level of detail in the methodology in particular. Quite a lot of detail is given on the field methodologies used, which is good; however, the energy balance model itself I think is under-reported. I realise it has been published elsewhere, and the study can rightly refer to those other papers, but I think that two aspects of the model do need further elaboration in this work to make it self-contained. These are the Qg calculations, and particularly if the subsurface model allows for percolation of meltwater and its re-freezing within the snow pack, and the albedo scheme. The authors themselves state 'special attention is paid to the treatment of albedo because it varies enormously in space and time' (p 861 lines 11-13), but readers are left completely in the dark as to how the scheme works. This is especially important given the discussion of dust events subsequently; the dust-tracking algorithm discussed in Section 3.4, p 865, line 4-5 should also be given in the methods.

Finally, although the paper is generally quite clearly written, a few sections could be clearer or use more standard English. I've already highlighted the discussion of correlation coefficients above, but I also think the usage 'vertical depletion' (e.g. p 863 line 24) is obscure. I assume this means vertical downward flow of the surface (as opposed to surface lowering due to ablation), but is this the 'at a point' vertical velocity,

or the combination of 'at a point' vertical velocity plus the overall downward surface movement at the stake as it moves downhill due to the horizontal flow of the glacier? The Abstract also uses the term 'vertical surface deletion' which needs amending (p 856 line 3).

I think it would be useful to have more effective keys for the Figures, especially 2, 4 and 6; more like Figure 5, where the various symbols etc are presented graphically in the Figure, rather than as descriptions in the Figure captions.

Conclusions

I think the paper is publishable with revisions. I am uneasy about the concept of 'an ideal climate' due basically to equifinality, but I think the idea of discussing the various components of the mass balance, and how the glacier has changed from melt-dominated to accumulation-dominated at various times is interesting. I think this is a key change I would look for before publication. I would like to see some more detail on the mass balance modelling as indicated, and some thought given to the language used in some parts of the paper. I more balanced treatment of the limitations of the current study, and mis-matches between data and model results should also be given.

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