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Interactive comment on "Estimation of volume changes of mountain glaciers from ICESat data: an example from the Aletsch Glacier, Swiss Alps" *by* J. Kropáček et al.

Anonymous Referee #2

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TC/TCD Estimation of volume changes of mountain glaciers from ICESat . . . Kropacek et al.

The paper derives and compares elevation change trends over Aletsch Glacier, CH, between ICESat data and various DEMs (aerophotogrammetric, SRTM, ASTER GDEM) and in-situ GPS data. Whereas this topic is in principle timely and of interest, I am confused about the paper in two ways:

(1) What is the focus of the paper? What is actually tested? ICESat? SRTM? GDEM? ICESat accuracy? ICESat representativeness? DEM accuracy? Most of the findings of the authors are actually not due to ICESat but rather due to the difference be-

tween aerophotogrammetric DEMs, SRTM, ASTER GDEM. ICESat just provides the sparse sampling points. However, to compare the above DEMs, why not to use their entire overlap area, not just a few points? I think the paper has to be completely re-designed with a clear focus, e.g.: - ICESat representativeness: How does the sparse ICESat coverage capture the volume changes over, e.g., the entire Swiss Alps? Or, for which hypothetical ICESat coverages would the volume change captured at which accuracy? Your present results are very specific to Aletsch Gl., and it is thus not clear what general conclusions you could draw. - ICESat accuracy over mountain glaciers from aerophotogrammetric DEMs and GPS. Perhaps also look at the full waveforms. - Accuracy/performance of SRTM and ASTER GDEM compared to aerophotogrammetric DEMs, ICESat and GPS. ... To name only some ideas.

(2) I am confused about the almost complete disregard of related work by others. As a result, the paper is outdated by several years. The authors should read work from, e.g. G. Moholdt, A. Gardner (where actually G. Moholdt did the ICESat work), C. Nuth, A. Kaab, E. Berthier and the references in their work. Based on the state of knowledge from these studies, the authors should redesign their study. Symptomatic for my concern is the final conclusion that "... ICESat data is a valid source of information on surface elevation changes and on mass balance of mountain glaciers". From the above work the authors may find that work similar to this work has already been done for a number of other glaciers, also on regional scales, even globally, in parts using much more sophisticated methods and error assessments.

Due to my general concerns that require, in my view, a new study/paper and cannot be covered by revisions alone, I have only a selection of more specific comments:

Page 3262/line 7: why is one approach statistical, the other analytical?

3264/5: Aletsch only 'large' glacier in the Alps with good ICESat coverage? Details? Definition of 'large'?

3265/7: You should refer/give credit to the original source not (only) to GLIMS. I assume

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the outlines you are using are the ones from F. Paul et al.

3266/20: What about F.Paul and W. Haeberli, GRL, 2008, DOI: 10.1029/2008GL034718, and Paul, F., 2008, Journal of Glaciology, 55 (188), 945-946.

3267/18: What are 'independent identical points'?

3268/15: Contain ICESat records not EGM2008 undulations, instead of EGM96?

3268/23: What is the effect of cutting off all ICESat points on terrain steeper than 10 deg on volume change/mass balance calculations? For Aletsch? E.g. simulate based on your aerophotogrammetric DEMs. Is this a feasible procedure in general?

3269/14: Are the GPS data from the footprint centres? How did you accurately find them with loss of lock?

3270: Did you assume the same snow depth for all ICESat points as for Eggishorn? Couldn't there be some elevation gradient?

3272/2: no significant trend: do you mean not significantly different from zero, or is there actually a trend, but weakly defined?

3272/3: Are the elevation differences over few points over rough high mountain terrain really suitable to detect/exclude an ICESat drift? Can you show the off-glacier trends/results?

At many places you refer to one of the DEMs while you actually mean the differences between ICESat and the respective DEM. This is in parts confusing. E.g. 3274/25.

3272/8: How good is the aerophotogrammetric DEM over snow (=accumulation area)?

3272/19 and else: are the +- errors statistical errors of fits? Or standard deviations of samples? (What sigma level?) Or the results of an error budget? (Which one? You should present one in any case.)

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3275/27: But what about the penetration (variation) in the ablation area. SRTM was taken in winter and the penetration could have varied spatially.

3276/23: A (false) trend could be induced by a systematic spatial variation of footprint locations over 2003-2009.

3273/7: Is that new? See above (2).

3274/15: ok, but this is completely specific to Aletsch and its ICESat coverage. No conclusion can be drawn based on that.

3274/20: Don't you need to use the standard error here?

3277/5: No, because your result is fully dependent on the situation on one glacier, selected on purpose, and not representative for other glaciers in the region, as you state earlier.

In the Acknowledgment you refer to airphotos from 1980. I didn't find this data set in the study (I may have overlooked).

Tabs 3-5 could be combined.

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Interactive comment on The Cryosphere Discuss., 7, 3261, 2013.