

## ***Interactive comment on “GPS based surface displacements – a proxy for discharge and sediment transport from the Greenland Ice Sheet” by B. Hasholt et al.***

**Anonymous Referee #1**

Received and published: 8 August 2014

### **Summary**

This paper presents GPS position data (1996 - 2013) from a bedrock station (Kelyville - KELY) close to the western periphery of the Greenland Ice Sheet (GrIS) along with observations of ice sheet meltwater runoff, winter precipitation, sediment transport and solute transport (2007 - 2013) from a nearby proglacial river.

The GPS data show a marked increase in de-trended uplift since approximately 2006 and a concurrent increase in north westerly displacement away from the ice sheet. The authors find a strong correlation ( $r^2 = 0.83$ ) between uplift and annual ice sheet runoff and a less convincing relationship ( $r^2 = 0.68$ ) between uplift and winter precipitation

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for the overlapping time period (2007 - 2013). Based on these relationships (effectively linking surface uplift and depression with proximal ice sheet mass balance), the authors extend their runoff and precipitation records back in time to 1996. These extended records suggest that since 1996 annual runoff has markedly increased while winter precipitation has decreased. The combined picture suggests that from 1996 - 2006, the mass balance of this portion of the ice sheet was essentially stable but that since 2006 mass loss has increased significantly.

Although the data presented do look interesting (especially the GPS uplift observations), and would no doubt be relevant within the scope of TC, the paper is presented in such a seemingly careless and rushed form that it is difficult to be convinced by the conclusions drawn. Many of the methods are described in insufficient detail to be sure of their robustness and the presentation of the data themselves is imprecise and inconsistent. Furthermore, I have some serious issues with the way the paper is structured and written.

### **General points**

1. It is not clear exactly what GPS data are being presented: e.g. absolute position vs de-trended displacement. This uncertainty has an important influence on the significance of the results presented.
2. There is insufficient detail describing the methods used. For example runoff measurements, sediment concentrations, solute concentrations, de-trending, error estimation are not explained in enough (or in many cases any) detail.
3. The structure of the paper is very muddled. The introduction is lacking in background, motivation and justification. Bits of the ‘Methods’, ‘Study area’, ‘Results’ and ‘Discussion’ sections are in the introduction. The results are too brief. The data and figures are presented in an odd order (relationships between data before the data themselves). Figure captions are woefully inadequate.

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4. Writing, spelling and grammar are poor. There are numerous examples - see specific points below.

5. I am not convinced about the application of statistics in the paper. There are some surprisingly high correlations based on looking at the plots. Did the authors use a simple linear regression or a more robust regression technique? I'd like to see a table of all data used to make the figures, perhaps as an appendix.

6. Might it be useful to compare the GPS-derived runoff record with a similar record derived from air temperatures or mass balance based on meteorological reanalyses?

7. Surely it is not just present unloading of ice that affects the earth's elastic response. Does the earth's surface not respond to ice volume changes over much longer time scales? The GPS data surely contain responses to ice volume changes over a wide range of timescales? Presumably such effects are removed if/when the data are detrended, but we are not told.

Specific points (by page and line number, my additions IN CAPITALS)

Pg. 3830; L1: 'elastic respond' should be 'elastic response'.

Pg. 3830; L1: Abstract structure: I think the abstract should start with a sentence outlining the authors' motivation and justification. Also, it seems a little odd to present the results part of the abstract before mentioning the location and duration of the study.

Pg. 3830; L2: 'mass loss' should be 'mass lost'.

Pg. 3830; L5: It might be useful to include the p values as well so that the reader can see that the relationship is significant.

Pg. 3830; L9: Tell the reader where Kelyville is relative to the ice sheet catchment.

Pg. 3830; L9: 'are used ALONG WITH THE ABOVE RELATIONSHIPS to calculate. . .'

Pg. 3830; L10: 'to calculate AN 18 YEAR time series of. . .'

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Pg. 3830; L12-15: Might it be useful to compare the GPS-derived runoff record with a similar record derived from air temperatures or mass balance based on meteorological reanalyses?

Pg. 3830; L17: Introduction structure: It seems strange to me to go into detail about the site locations in the introduction - particularly as you have a separate section called 'Study area'. The introduction should give some background and explain the motivation for and justification of the study. The current introduction also presents the results and even includes some interpretation of the results. The introduction needs to be rewritten.

Pg. 3830; L17: I'm not sure that the authors should state so early on that they think the vertical and horizontal displacements are due to recent/current ice mass loss. Just say that the study will focus on elucidating the controls on the displacements, then give some background on possible mechanisms etc.

Pg. 3830; L19-23: This is a very long and awkward sentence which should be rewritten and shortened.

Pg. 3831; L1: Measured over what time period?

Pg. 3831; L2: 'whereof' should be 'of which'

Pg. 3831; L2-3: 'Hasholt et al. (2013)' should be '(Hasholt et al. 2013)'.

Pg. 3831; L5: Comma after 'Kelyville' is unnecessary.

Pg. 3831; L7-8: 'presentation' should be 'paper' (this perhaps gives some clue as to the origin of the very brief treatment of the introduction - it was originally the introduction to a presentation?)

Pg. 3831; L7-10: Another long sentence that doesn't really make any sense.

Pg. 3831; L12: At the moment the 'Study area' section seems far too brief because most of what should be in this section is in the introduction.

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Pg. 3831; L13: I don't like the 'see Fig. 1'; just '(Figure 1)' is better. Also, you should specify which of the sub-panels of Figure 1 you are referring to. For example, on line 13 it should be '(Figure 1a,b)'.

Pg. 3831; L15: Above comment applies again to figure reference.

Pg. 3831; L16: 'coastS'

Pg. 3831; L16: Consider replacing 'divides against' with 'boundaries with'.

Pg. 3831; L18: 'THE GrIS'.

Pg. 3831; L18: No need for 'see' as above.

Pg. 3831; L19-20: 'Henriksen et al. (2005)' should be '(Henriksen et al. 2005)'.

Pg. 3831; L22: Surely it is not just present unloading of ice that affects the earth's elastic response. Does the earth's surface not respond to ice volume changes over much longer time scales? The GPS data surely contain responses to ice volume changes over a wide range of timescales? Should these longer-term responses not be removed from the GPS data prior to the present analysis?

Pg. 3831; L24: Is it really necessary to have nine references for this point?

Pg. 3832; L7: 'Data' are plural.

Pg. 3832; L7: 'there are A few gaps...'

Pg. 3832; L9: 'any influence of...' should be 'any influence on...'

Pg. 3832; L11: This is a very local, colloquial description of where the pressure transducers are - particularly given that neither the airport or the bridges are shown in Figure 1.

Pg. 3832; L12: 'upstream OF the bridges...'

Pg. 3832; L13: the velocity x area method needs more detailed explanation: how

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was velocity measured? The Watson River is seasonally very large and turbulent and difficult to monitor.

Pg. 3832; L13: How was the rating curve established? The reader is given no information.

Pg. 3832; L14: Similarly, how was the 'accuracy' of these measurements estimated?

Pg. 3832; L19: Another reference to 'the airport' which is not included on any figure.

Pg. 3832; L20: Can winter precipitation not fall as rain if the air temperature is above 00C (not unheard of at Kangerlussuaq in winter)?

Pg. 3832; L21: Why use uncorrected precipitation given that there are known large uncertainties? Is it not worth attempting to correct the data?

Pg. 3832; L29: What does "error" refer to exactly in this instance? Do you mean uncertainty in the 30-second GPS position data?

Pg. 3833; L4: 'Related statistics ARE shown...'

Pg. 3833; L5: 'highly significant' is vague. Present the p-values or F statistics to demonstrate the significance.

Pg. 3833; L6: This only holds if the relationship between earth surface uplift and runoff and sediment discharge is constant through time.

Pg. 3833; L7-8: Again, provide significance numbers rather than saying 'lower but significant'

Pg. 3833; L11: The entire GPS record is not shown in Figure 5. Only the vertical component is shown.

Pg. 3833; L12: Do the authors have any thoughts about the cause of approximately 10 mm apparently annual cycles in the vertical GPS data? I'm not sure 'nearly horizontal' adequately describes the line pre-2006.

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Pg. 3833; L12: The record does not move upward, the record shows that the GPS moves upward.

Pg. 3833; L14: There is already displacement to the west and north prior to 2006, but the rate of displacement increases around 2006.

Pg. 3833; L15: Here the transition period is stated as 2004-2006 whereas previously it was 2005-2006.

Pg. 3833; L16: 'Figs. 7, 8 and 9 RESPECTIVELY'

Pg. 3833; L18: 'indicateS an increasing mass loss FROM THE ADJACENT ICE SHEET CATCHMENT during the 17 year period'

Pg. 3833; L20: How were the sediment and solute transport calculated/estimated? No details are given at all.

Pg. 3833; L23: Why is it not mentioned in Figure 6 that these are relative displacements? Also throughout this section, the authors should make absolutely clear which are absolute measurements and which are relative measurements.

Pg. 3833; L24: What about the long term effects of ice volume changes this close to an ice sheet?

Pg. 3833; L24-25: How was the trend removed? Needs more explanation.

Pg. 3833; L25: 'study RELATIVE changes...'

Pg. 3834; L1: 'The RELATIVE/DE-TRENDED horizontal displacements...'

Pg. 3834; L5-6: Consider replacing 'response of the surface upon the weight changes...' to 'response of the surface to changes in loading...'

Pg. 3834; L6: 'Slightly HIGHER...'

Pg. 3834; L15: 'trend lines DO not PASS through the ORIGIN...'

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Pg. 3834; L16: then ACCORDING TO THE BEST FIT LINE the winter precipitation...'

Pg. 3834; L17: I think there should be a sentence here that says something like: 'There are several possible reasons for this. Firstly... Secondly...'

Pg. 3834; L18: If having uncorrected precipitation data is a clear source of uncertainty, why not correct them? Or at least tell the reader why this is not possible.

Pg. 3834; L20: 'evapotranspiration COMPONENT which may...'

Pg. 3834; L20: 'from THE ORIGIN.'

Pg. 3834; L23: The sentence starting 'An important point...' is very long and awkward. Consider replacing it with something like: 'An important point is that in this catchment, there is no mass lost TO calving, which could elsewhere blur the response DUE TO the released melt water alone. This implies it may be possible to calculate calving indirectly...'

Pg. 3834; L28: The authors mention that the precipitation has to be calibrated prior to use in mass balance studies. How is this done, and wouldn't it be useful to do something similar before using the precipitation data in the present study?

Pg. 3835; L4: 'land surface changes' is a bit vague and suggests to me land use changes? Maybe 'land surface ELEVATION changes' would be better.

L5 and L11: I'm not sure 'discharge' is the right term here. Discharge to me suggests a transient measurement. What I think the authors mean is seasonal runoff?

L13: 'longER'?

Table 1: Column two header should be 'n years' to be consistent with Table 2. There are no units for some of the terms in each row. For example uplift (mm) for the first row. I'd like to see a table with all the data required to make the plots (perhaps as an appendix). The r<sup>2</sup> and p value for winter precipitation vs surface depression seem very high and low respectively based on the data shown in Figure 4.

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The figures are presented in an odd order (relationships between data before the data themselves).

Figure 1: Subplots need a, b and c labels so that they can be referred to individually from the main text. Panel b shows contours derived from a DEM, not the DEM itself (also give a reference for the DEM). When was the Landsat image acquired? Provide details.

Figures 2-4: Make sure the axis labels are consistent with terms used in the main text. The authors need to be clear in the figure captions (as well as in the main text) whether these plots show absolute or relative displacement. The significance of the relationship shown in Figure 4 seems very high based on the plot which suggests weakly correlated data. Did the authors use a simple linear regression or a more robust regression technique?

Figure 5: No y-axis label. Is this absolute or relative displacement?

Figure 6: No y-axis label. Is this absolute or relative displacement?

Figures 7-11: Show significance numbers - p value of F statistic for example.

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Interactive comment on The Cryosphere Discuss., 8, 3829, 2014.