Answers by authors to reviewer C. Völksen

Black, normal text: reviewer's comments/suggestions.
Blue, italics: author's answers

Interactive comment on "A 14 year dataset of in situ glacier surface velocities for a tidewater and a land-terminating glacier in Livingston Island, Antarctica" by Francisco Machío et al.

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Dear Authors, it was my task to read your paper and to review it. I found your data set very interesting and valuable. I was an enormous effort to collect these data over 14 years in order to estimate the velocities from the positions. Nevertheless, I have some comments and suggestions, which I hope will improve the paper.

General comments

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I would recommend to provide - with the data - an additional file (e.g. Readme) that describes the content of the Zip-archive. As a non-GIS user I found it a bit complicated to access the data. After installing an open source GIS on my Linux PC I was able to read the data and to visualize them, which was helpful but took some time. Therefore, providing a simple ASCII file that contains the data of the shape file would be very useful. It makes the access to non-GIS users easier. Nevertheless, I have understood the advantages of a GIS environment.

> *Following this suggestion, we have added a folder with the same information but in non-GIS users format (.xls and .txt), in addition to a text file including the description of the fields.*

The authors have provided a complete data set open to any potential users. The large advantage is also that every user can derive its own velocity field using a different approach. But in order to do so, the authors should add some additional information. For example, the provided data file contains additional stations, which are present in the available table and the appendix A. The reason is unknown.

> *Throughout the years some of the stakes have been lost (e.g. by iceberg calving at Johnsons Glacier front, or fallen because of intensive melting, or buried by heavy snowfalls) and new ones have also been added. This is why the PANGAEA dataset contains more stakes than those shown in the paper figures (which are snapshots in time). This has been clarified at the beginning of the new Methods section:*

"We note that, over time, some of the stakes have been lost (e.g. by iceberg calving at Johnsons Glacier front, or fallen down because of intensive melting, or buried by heavy snowfalls) and new ones have also been added as replacement or to enlarge the original network. Because of this, there are differences in the set of stakes shown in the various figures in this paper, as they correspond to different snapshots in time. Also, the set of stakes included in the PANGAEA database (see Section 4) is larger than that in any of the figures, because it includes all of the stakes that have existed at any time within the complete measurement period."

Specific comments
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[P: page; L: Line]

P3/L16-20: Considering the standard deviations of the mass balance I would prefer a formulation like "... are not significant different from zero..." Therefore, a discussion like "... a slightly more negative balance because of" can be misleading based on the data.
Can you add in a short sentence how the mass balances were estimated?

We have changed the first statement following your suggestion ("have not been significantly different from zero…"). However, we have remarked that the ranges indicated next to the averages are standard deviations (indicating a noticeable interannual variability), but that the actual estimated errors for each annual mass balance measurement is much lower, of the order of 0.1 m w.e. (by the way, there was a typo in the earlier version of the paper, and all values should be m w.e,, NOT mm w.e.). This means that it is fair (significant) to state that Hurd's balance is slightly more negative than Johnsons' balance, so we have kept this comment and its associated explanation.

We have also added, as suggested, a brief explanation on how the mass balances were measured: "Summer, winter and annual mass balances have been measured using the glaciological method on the same network of stakes used for the glacier velocity measurements, and then integrated to the entire glacier basins."

P4/top (3. Methods): In this context, you should speak about differential GNSS methods, since you used two GNSS-receiver: one as a base at JCI and one as the rover in the network of stakes. This should be clear for the reader from the beginning of this section.

We have added the "differential" qualifier to make this more evident since the beginning of the section.

Please mention the distance between the base station and the rover.

The distance was between 2 and 4 km, depending on the stake position. We have added this in the new version of the manuscript.

Since the base station operates permanently, I would have expected an accuracy better than 1 cm for the horizontal components.

You should also be able to derive plate tectonics. Well, this does not matter so much for the local estimation of the velocity field.

> *Sorry, this was a typo (the previously given data made reference to typical accuracies of our differential GNSS measurements at the stakes (rover). Indeed the error in the base station coordinates is much lower: 0.007 m in horizontal and 0.012 m in vertical. We have corrected this and added a reference for it (Ramírez-Rodríguez, 2007).*

Concerning the applied "fast static mode" with a site of 10 seconds only. I believe that this is a very short occupation time. I wonder if you were able to resolve the ambiguities and estimate a precise position. Is it possible that the "fast static" sites do not fit well with the second-degree polynomial approximation? Can you comment on this fact?

> *Thanks for pointing this out, as in fact there was a typo in this part of the text: the occupation time of 10 seconds actually corresponded to RTK measurements, while for fast-static measurements it was of 3-5 minutes, depending on the number of available (visible) satellites. It now reads as follows:*
>
> *"The measurements were performed either in real-time kinematics (RTK) or in fast-static (post-processed) mode; for the former, an occupation time of 10 s was set, and for the latter it was of 3-5 minutes depending on the number of satellites available."*

I would also mention here that the estimated coordinates of the stakes were projected into the UTM-System using Zone 20S.

> *We agree and have included it into the modified text.*

P4/L16: My first impression was that the variable t with the index n represents any possible time. While reading the paper, equation (2) and equation (4) imply that "n" stands for the last observation in time.

> *You are right. In fact, we recognize that the notation was confusing, so we have replaced the subscript "n" by "i" to make it clearer (and kept n as total number of measurements).*

In this context, I also do not understand the purpose of equation (1). For the derivation of the second-degree polynomial approximation there is no need to form any delta x (or delta y etc.). To keep it clear I would simply remove it. It continues with equation (2), which in my eyes is rather the vector of observations (reduced to one component (e.g. X or Y)) containing all the positions of one stake for the entire period. Is it not the case that X(t_n) (eq. 2) is the same as the vector X in equation (4)? I find this mathematical presentation confusing and recommend a simplified form.

> *You are right, and we have removed former Eq. (1) and changed the writing of the new Eq. (1) (formerly Eq. 2) to make it more understandable. Together with the mentioned change in subscripts and the rewriting of new Eq. (5) (formerly Eq. 6), we believe that the entire set of equations is now more understandable.*

Page 5 (top): You have decided to use a least squares approach treating all the observations with equal weight. On the previous page you write that the accuracy ranges between 0.07 to 0.6 m. I wonder now why you have decided to use equal weights. Would it not be possible to improve the results by using different weights? Can you comment on this?

> *Yes, it is right that the position accuracies for individual stake positioning measurements range between 0.07 to 0.6 m, but this does not mean that a particular stake shows systematically positioning accuracies in the low or the high end of the range. In practice, most stakes, over their histories, show both large and small accuracies, mostly depending on the tilt that they undergo at particular times. Tilt in the ablation zone (due to intensive melting) is in general more common than in the accumulation zone, but in the latter also many stakes undergo tilt because of strong winds (being anchored in softer snow/firn as compared to stronger ice in the ablation zone). In fact, we did not find a reasonable systematic way to assign different weights to different stakes, so we preferred to weight all of them equally.*

Page 6 (equation (6)): The term Sigma_X0 is not explained. I suppose it is the a priori unit weight. In the same context I would consider e_x to be the "estimated variance of unit weight" a posteriori. In the next section you treat it as the root-mean-square error in position. I do not understand this expression and would rename it. The value of e_x rather indicates how well the approximation fits the positions of the stakes, as you say in the following sentences (P5/L11-L12). You could estimate the accuracy of the coefficients with (e_x**2)*inv(N). The elements on the diagonal of this matrix are the variances of the coefficients.

> *This comment has been addressed in the fully new writing of the equations and the associated text.*

Page (6) equation (7) and (8): I do not understand the origin of these two equations. My approach would be based on the following relation based on error propagation.

Velocity is given by two positions X2 and X1 and the time difference delta t [t2-t1] (assuming the error in time can be neglected):
V=(X2-X1)/(delta t)

Error propagation:
(S_v)**2= (-S_x1/(delta t))**2+(S_x2/delta t)**2

Assuming equal precision for the coordinates X1 and X2 gives:
Standard deviation of velocity: S_v= sqrt(2)/(delta t)*S_x => e_v=sqrt(2)/(delta t)*e_x

Therefore, I cannot see that the error of the velocity component is dependent on the size of the velocity or the distance between the two positions. It is dependent on the precision of the estimated positions and the time between the reoccupation.

> *We acknowledge this comment, as indeed the mentioned equations were improper. The new equations (6) and (7), and their associated text, clarify now the distinction between the interval velocity calculations (and associated error estimates) that the reviewer is*

indicating and the distinct error estimate associated to the polynomial function describing the velocity, derived from the polynomial approximation for the positions of each stake.

Description of the datasets:

I found in the shape file four more fields addressed with dias, prevista_x, prevista_y and movxy. They are probably not necessary. But could you please describe their purpose or remove it? Could you also add information on the fields ”t38_max_x” and “t38_may_y”.

The information corresponding to these fields has been added both in the description made in the manuscript (now moved to Appendix A) and in the files available in PANGAEA.

How did you obtain the maximum error? I believe it is absolute value of the maximum residual (Rx/Ry).

The maximum error is the maximum residual of the polynomial interpolation (i.e. the maximum difference between the observed position and its corresponding polynomial approximation). This is included in the PANGAEA dataset, but not in the paper text.

I used the data of the shapefile and converted them into an ASCII readable text file. Based on that I re-computed some of your coefficients and velocities using Octave/Matlab. Testing it on EJ14 I was able to recover your results with small differences.

Xa(tn)= -8.31294e-06*tn*tn+5.62765e-03*tn + 635350.468
Ya(tn)=1.90564e-05*tn*tn+-1.11635e-02*tn+3048898.201
Ex: 1.68
ey: 4.45
Vmax: 57.31
Azi: 336.68

But for some others like EJ35 I could not get the same results. Is it possible that you have removed outliers that were ignore during estimation?

We did not remove outliers for EJ35, so we are not certain on the reason for the apparent discrepancy (we have redone our calculations and obtained our same earlier result). EJ35 has few observations (just 7) and this results in differences in the least significant digits. This could be a reason for the differences, but it is just a guess. On the other hand, EJ35 is a stake that shows an anomalous trajectory.

In the same context I noticed that some stations were called EJ14r or EJ14R. First, what is the difference between EJ14 and EJ14R? Is there a difference or are these stations identical?

This is a fully different problem. Stakes with and without the r (or R) suffix are totally different (though spatially close to each other). The short version is that, at a certain point in history some stakes were installed at exactly the same position that was occupied some years earlier by another stake (if the original stake was named e.g. EJ14, the new stake installed in its former position was named EJ14r).

The longer version is as follows: an earlier network of stakes, with fewer stakes, was deployed in the late 1990s in Johnsons Glacier –none in Hurd Glacier– by glaciologists from the University of Barcelona; they are the ones who did this experiment of installing new stakes in the former positions of another one, and in fact existed e.g. EJ14, EJ14r, EJ14rr, … This was an attempt to do some kind of "Eulerian" measurement of velocities (at fixed locations) versus the "Lagrangian" way of measurement, in which one follows the stakes as they move through the glacier. However, this procedure revealed to be too cumbersome and time- and resource-demanding. When we "inherited" in 2000/01 this network of stakes, we deployed additional stakes on Johnsons Glacier and deployed a new network on Hurd Glaciers, and we discontinued this practice (of r, rr, … stakes) from our colleagues, though kept the "r" stakes until they were lost (this took many years, and some are currently still "alive").

Of course, we should not explain this story in the paper text, but we have remarked on the stake list in PANGAEA dataset that stakes such as EJ14 and EJ14r, etc. are different.

Also, I find in the list more stations than shown in the table (e.g. EH14, EH16, EH18, EH23, EH26 etc.). Is there a specific reason to remove those? It might be necessary to revise the table and explain the reason for removing those stations from the table.

This has already been explained earlier in this "Answers to reviewer" file (and included in the new version of the manuscript).

Technical comments:
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Please consider most of my remarks as suggestions. I do not insist on a complete implementation. Since I myself do not speak English as a mother tongue, some corrections have to be treated carefully.

[P: page; L: Line]

P1/L14: ...repeated GNSS measurements in a dense network of 52 stakes...

Done.

P1/L16: ...2000-2013 and were "performed/carried out" at the...

Done.

P1/L18: ...This dataset "is" useful as input...

Done.

P1/L20: D-InSAR (not D-inSAR)

Done.

P1/L24: ...source of information for “the study of glacier dynamics”.

Done.

P1/L31: ... commonly used as input data “for” numerical models.

Done.

P1/L33: ...are used instead for tuning “the” model’s free...

Done.

P1/L36: ...more and more “common” to establish .

Done.

P1/L44: are still of “large” interest, since...

Changed to “wide interest”.

P1/L48: , “the” GoLive project ... and “the” ENVEO CyroPortal

Done.

P2/L2: ...measurements “in” a dense “network” of stakes...

Done.

P2/L5: ...in the late 1990s “on” Johnsons...

Done.

P2/L26: ... (Fig. 1c)

Done.

P3/L26: Fig. 2. Network of stakes on...

Done.

P4/L2: The glacier surface velocities were estimated based on repeated differential GNSS measurements in a network of stakes deployed...

Done.

P4/L3: ...The “network” of stakes consisted (as of the end of the reported measurement period) ...

Done.

P4/L5. The GNSS measurements were “carried out/performed” using a Trimble 5700 system, with “d”ata “c”ontroller TSC2. The observations were “performed/carried out” either in ..

Done.

P4/L7: for the latter, an occupation time of 10 s”econds” was “used” ...

Done.

P4/L9: e.g. The GNSS base station was located at the neighbouring Juan Carlos Station I (Fig.1) ”in a distance of 2-5 km from the two glaciers”.

As we have previously indicated, this clarification has been added to the text.

P4/L14: From the collected positions of the stakes at different epochs, a surface velocity map …

Done.

P4/L27: ... by “least squares fitting method”, minimizing …

Changed to “…. by the least-square fitting method, minimizing …”

P5/L3: X). For a least squares “approximation, assuming observations of equal weight,” these equations are:

Done.

P5/L6: This first sentence can be removed.

Done.

P6/Table1: Please define e_xy in the description of the table 1. It is not given in the text. I have checked it for EJ14 and there it is probably sqrt(ex*ex+ey*ey).

Your interpretation is correct, but this was already included in P5/L1 of the former text.

P8/ Figure 3: I find it very difficult to read the legend in the Quickbird map of this figure. Please make sure that the final print is showing the legend.

Done

P9/ Figure 4 and 5: Same problem, I cannot read in the provided pdf-file the legend of the left map. The upper right map is not really necessary and also not readable. Both upper right maps of figure 4 and 5 show the same glacier (Johnsons

Legends (and other aspects) of these figures have been improved. Furthermore, former figures 4 and 6, and 5 and 7, have been combined into a single figure (each pair) at the suggestion of the other reviewer.

P10/L2: ...value of the velocity for the same date “(13/02/2013)” ...

Done

P10/Figure 6+7: Again, the legend is too small. I could imagine that a wider color spectrum makes it easier to identifier different zones. The here used spectrum from yellow to red makes it difficult to separate different zones.).

Done

P11/L5-10: Can you support your findings with numbers. Give values for the velocities and support terms like "decreasing pattern" or "high-velocity zone". What are the velocities for these?

We believe that, with the combination of the pairs of figures 4 and 6, and 5 and 7 (former numbers) these comments are now self-explanatory, as it can be easily visualized which stakes are faster and which are the corresponding velocity values.

P11/L11: . . ., which has extremely "steep" slopes. (?)

Done

P11/L21: As mentioned before, I would not consider this as "root-mean-square position error".

Removed mention to "root-mean-square".

P14/Appendix A: EJ14 and others are missing in the table!

The reason has been explained earlier. A mention to the loss of this particular stake has been added to Figure 3 caption: "The stake fell down to a newly opened frontal crevasse during 2010-2011 and was subsequently lost by iceberg calving, so it does not appear in Figure 2."

Also I would avoid the error term in this table as long as it estimated based on formulae (7) and (8).

We believe that the error term (which is now computed using the new Eq. (7) is the most significant piece of information in this table, as it gives an idea on the estimated positioning accuracies for the polynomial approximations to each stake trajectory. Consequently, this info has been kept, though the Table has now been moved to the Appendix (now Table B.1 in Appendix B) at the suggestion of the other reviewer.