Response to Anonymous Referee #1

We thank the reviewer for the constructive comments, bringing in a new perspective. The comments will help us to improve the manuscript for a broader scientific community, adding additional parts of information which we initially thought was not beneficial to include into the paper. Now we see that it is necessary to provide some background information and a broader context, and look forward to do so.

The review is cited underlined, answers of authors are formatted indented.

Review: Summary: Stocker-Waldhuber et al. presents a vast number of in situ velocity measurements on four Austrian glaciers spanning over one century. They examine the velocity variations in view of changes in mass balances and explore the possibility to use velocity data as an alternative climate indicators.

In principle, such a study would be very welcome. There is a growing number of publications dealing with velocity measurements from satellite data but the interpretation of the velocity variations, often over a period of ∼20 years, is far from straightforward.

We fully agree, as this was our motivation to start working on the data.

Accurate field measurements of annual velocity at the same location and during a long period as provided in this study could help to understand the drivers/processes of velocity changes (change in sliding vs. internal deformation) and in turn, facilitate the interpretation of regional velocity variations observed from space. This is what I was expecting when I read the attractive title "Ice flow velocity as a sensitive indicator of glacier state".

We fully agree, and are glad that the reviewer found the title attractive. The reason for us being very careful in our interpretation of the presented data is that for an in depth understanding governing processes, the ice thickness and temperatures, firn thickness, water content and temperatures, and basal velocities must be known to fully understand velocity variations. For a theoretical or quantitative interpretation of velocity variations, this data must be known not only on the location of surface velocity measurements, but on all other points having an effect on the surface velocity of the measured point. It is, similar to research on atmospheric processes, not possible to determine these parameters at all necessary points, causing equifinality problems at various scales. This is why we decided not to include the topic. We now understand that a presentation of this problem is necessary to set the frame for the analysis carried out. We are lucky to have in situ data of the depth of firn cover at one location at Kesselwandferner (Ambach et al., 1978), so that we are able to illustrate ambiguities resulting from firn thickness variability. We now understand that it could be an important contribution of this paper to not only present the results of ice flow velocity time series, but also the greater glaciological context.

Ambach, W., Blumenthaler, M., Eisner, H., Kirchlechner, P., Schneider, H., Beherens, H., Moser, H., Oerter, H., Rauert, W. und Bergmann, H. (1978): Untersuchungen der Wassertafel am Kesselwandferner (Ötztaler Alpen) an einem 30 Meter tiefen Firnschacht. Zeitschr. f. Gletscherk. und Glazialgeol., 14, 1, p61-71.

However and although the data are important to present and publish, the paper fails to address (or even explore) this question of the link between velocity change / mass balance / and climate forcing. Many relevant references are missing and the discussion of the results is nearly absent. I am afraid this is currently hardly more than a data paper and I an sorry that I cannot recommend publication in TC.

We will include a general introduction on which parameters can influence surface velocity variations, roughly quantify influences of the various parameters (e.g. thickness of firn cover) and go a bit in the depth of discussion. Nevertheless, on one hand we end up in a number of uncertainties, which, on the other hand, have no impact on velocity as empirical monitoring parameter. Thus we will improve the link between velocity change, mass balance and climate forcing, as all the data exist and can be presented straight forward together with the theoretical framework and statistics.

General comments

1/ Seasonal velocity variations are out of the scope of the paper. Poorly presented and not discussed despite an abundant recent literature on the topic.

We measured surface velocities at subseasonal resolution at Gepatschferner and Taschachferner. Subseasonal measurements are not available for the longer time series of Kesselwandferner and Hintereisferner. We will try to improve our presentation and discussion of the data. Currently, velocity data in subseasonal resolution is only presented in Figure 7. We will appreciate to extend this topic. It actually is not clear if the above comment suggests to skip or to extend the topic. For us, both would be ok.

2/ As said above, the main weakness is the lack of discussion of the results. There is no comparison to other glaciers for which velocity variations have been reported and analyzed. The authors will find at the end of my review a list of publications that, I think, are relevant to their study and hopefully could inspire them to go deeper into their data analysis and discussion. We also miss a clear comparison to mass balance measurements (and a discussion of how/why they are related). Currently this is only shown in Figure 3 and 4.

We are very grateful for the hints on additional literature and will work on the comparison of the data as well as on the discussion. We will include a broader discussion of the relation to mass balance and mass balance parameters which we skipped in the original draft to prohibit an 'overload' of the manuscript with the danger of losing focus.

3/ The title is misleading and results in too much expectation. The reader expects at some point an answer, even partial, to the question "Can large-scale ice flow velocity monitoring be a potential alternative to mass balance measurement for regional glacier monitoring on an annual basis"? This discussion/answer never comes.

We will discuss this topic more explicitly. In the current version, the existing material was not included to keep the original draft focused on ice flow velocities – obviously ending in a too narrow presentation.

Specific comments

Abstract. lack of results. Too general.

We will add more facts and numbers including additional results from revised parts of the paper as suggested above.

Introduction. I really enjoyed the historical perspective on glacier monitoring.

Thank you!

2.1 I do not really see the point of discussing velocity measurements for the large ice sheets. It is out of the scope of your paper. It seems much more relevant to discuss recent progress (and limitation) of measuring velocity on glaciers. Two references come to my mind (Heid and Kääb, 2012; Dehecq and others, 2015).

From the above comments we conclude that a clear focus on mountain glaciers and processes there would be beneficial. The question why in general flow velocity is an interesting parameter seems to be rather obvious, so that this part could be skipped – correct?

2.10 The Jacob et al. paper is from 2012, not 2013. Note that there results, obtained solely from GRACE data are somewhat controversial, especially for regions with a low concentration of glaciers and a high influence of hydrology. (Gardner and others, 2013) is a more accepted global compilation. [see for example the unreliable result for the European Alps]

With a clear focus on mountain glaciers, this discussion is out of scope. Nevertheless, we thank for the comment.

2.12. I do not understand why the authors take the example of the ELA to illustrate the need to document glacier velocity change. No link. [I do not challenge the value of ELA observations, just that the link with your study is unclear right now]

The obvious problem that on Alpine glaciers under current climate conditions ELA is undefined/above summits clearly shows the problem of conventional mass balance for present glacier monitoring. As for surface velocity and its governing parameters, we suggest to add an introduction and additional data plus statistics, improving readability.

2.16. Is "parameters" really the right term? "measurements" maybe?

"In this paper, four long time series of ice flow velocities are revisited and compared with classical in situ mass balance parameters."

We want to say that we compare to the results of the measurements, i.e. time series of mass balance parameters. In our opinions, 'measurements' could generally refer to a method, without necessity to refer to the results. We will discuss that with our editorial office.

2.17 I find it strange to look for an "alternative". Velocity measurements are valuable by themselves and are eagerly need for improved dynamical modelling of glaciers. No need to replace other variables! They rather complement themselves in understanding the glacier response to climate change.

In the Alps, conventional mass balance under current climate conditions fails as accumulation often is zero, ELA is above summits and specific mass balance is mainly influenced by loss of ablation area, not the total balance at specific sites. The interpretation of b=B/S for comparing various years fails when S changes more than B.

We agree that there is no necessity to present an alternative, but more a complementary method of establishing time series in glacier monitoring when conventional mass balance method do not provide results any more.

2.23 why "empirical"? Are not they just "measurements"?

As we stated before, we do not generally claim a complete theoretical discussion of our data, and rather stay with an empirical treatment on the relation of velocity data compared to mass balance data/climate forcing. As for example the degree day method can be clearly considered as an empirical method …

It will be completely reworded; we can split these two meanings in using measured data for an empirical analysis.

3.26. Is not "glaciological" (rather than "direct") the terminology recommended in the

UNESCO glossary by Cogley et al.?

We will go over the text to stay close at the Cogley at al. terminology.

3.29 rather "Difference of DEMs" I think

DEM of Difference is correct (e.g. Wheaton et al. 2010: https://doi.org/10.1002/esp.1886). We can explain that and add the citation.

4.6 Unclear to me. How do one find in the field the initial (last year) position to make the tape measurements?

The initial position (x,y) is revisited with DGPS and used as starting point for the tape measurements ending at the stone. If the question addresses the fact that the initial z position during recent years was higher than the z position at the time of the second measurement, we can give an error estimate for that. We will discuss that in detail, with a sketchmap.

4.9 does it mean that lateral variations (due to lateral drag) are not taken into account? Or are all the stones close to the centerline so that these lateral variations can be neglected?

This is an important point - see next comment. We can add some additional information on that topic. – Maybe you could give us some additional hint on the expected depth of the answer (change of slope and elevation at the stone line during years, changing number of stones, profile width?).

4.12. this is not a relation but a percentage (or ratio). How variable is this value from year to year? (will depend on the amount of sliding probably). What additional uncertainties arise from this assumption?

Thank you for this comment, we will write ratio instead of relation.

The stones are evenly distributed at the profile, so the lateral variations are taken into account. Thus, the mean value of the stone-velocities is about 80% of the maximum velocity at the centre flow line.

We will discuss this in more detail. The amount of sliding was measured by comparing the velocity of a stake in the profile with the stone velocity (so far not included in the manuscript).

4.17 what does " calculated to its base point" means?

The base point is the lowest end of the stake (x,y,z coordinates). We will add this to the text.

4.31 why does "distance" matter for uncertainties in a tape measurement?

During the measurements the tape touches the ground (at least at a few points) and cannot be stretched perfectly (e.g. in presence of wind). Thus, the surface roughness becomes more important with longer distances which leads to higher uncertainties.

4.31 can the authors give us some good reasons to trust their 5% error estimate? Right now we can only believe them.

We will include the underlying error calculation.

5.5 "for the period" is vague.

Thank you for this comment, we will reword this part. The error is given as the maximum error from two measurements, one in the beginning of the period and the other one at the end. Thus, the measurement accuracy depends on the number of measurements and is independent from the length of the period. We will clarify: ±5 cm per single measurement/survey or at least ±10 cm for the difference between the two readings.

5.19. Can the acceleration in the 1920s (and the 40s) be related to a period of positive mass balance?

Unfortunately, annual mass balance records in Austria started in 1952/53. Length change records show some advances in the 1920. During these years, Kesselwandferner and Hintereisferner separated, so that an interpretation of length change data in terms of mass balance is highly uncertain. Geodetic mass balance data from these periods has low resolution and high uncertainties, so that this hypothesis can be neither confirmed nor rejected. The uncertainties in modelling of past mass balance data of individual glaciers in absence of local high resolution meteorological data might not be allowing to draw a reliable conclusion on the topic of your question. We can add a short not on that.

Fischer, A., G. Patzelt, M. Achrainer, G. Groß, G. K. Lieb, A. Kellerer-Pirklbauer & G. Bendler, 2018. Gletscher im Wandel: 125 Jahre Gletschermessdienst des Alpenvereins. Springer Spektrum, 140 S. doi:10.1007/978-3-662-55540-8. http://www.springer.com/de/book/9783662555392.

Fischer, A., K. Helfricht, H. Wiesenegger, L. Hartl, B. Seiser, M. Stocker-Waldhuber, 2016. Chapter 9 - What Future for Mountain Glaciers? Insights and Implications From Long-Term Monitoring in the Austrian Alps, In: Gregory B. Greenwood and J.F. Shroder, Editor(s), Developments in Earth Surface Processes, Elsevier, 21, 325-382. http://doi.org/10.1016/B978-0-444-63787-1.00009-3

Is a positive mass balance measured in model such as those of (Marzeion and others, 2012) and (Huss, 2012)? Typically the sort of analysis/discussion that one expect based on these results.

Actually we do not think that the focus of the manuscript can include a comparison of model results (including a full discussion of uncertainties) with not-existing in situ mass balance data.

If your question refers to measured mass balance data: yes, the time series include years with positive mass balances.

Measurement data from these periods are very rare (e.g. mass balance on HEF since 1953) and thus, models often fail to reproduce these "early" acceleration rates. This emphasizes the importance of these historical long term data of velocity records. We will discuss this in more detail.

5.22 why is it more obvious in the geodetic results? Do you mean geodetic mass balance? Why not obvious in the glaciological mass balance?

Thank you for the comment. We will delete this sentence and we will add a separate part in the discussion with focus on the difference between the geodetic and the direct glaciological mass balance. E.g. for the period 1969-1979 on HEF in Fig 3 (Fischer, A. (2011): Comparison of direct and geodetic mass balances on a multi-annual time scale. The Cryosphere, 5, 107-124.).

5.24 surface mass balance? Glacier-wide mass balance? Make sure the vocabulary is clear and follow Cogley et al. (2011) glossary. Authors could also provide here and elsewhere % of velocity change to clarify the magnitude of the signal.

We will check the manuscript considering the definitions according to Cogley et al. and we will add some changes on a percentage basis.

6.10 I am not sure the authors defined their convention for vertical motion. Worth reminding in the text anyway. (positive upward I assume)

Thank you for pointing out this gap in our definitions. We define upward motion positive, downward negative, and we will add this to the text (and sketch).

6.12. Can the authors details/quantify what was this "response". They are generally too vague in the description of their results.

Thank you for pointing out the unclear terminology – we will add some state of the art discussion on response and reaction of glaciers to climate change. In addition, we will treat this topic in the revised/enhanced discussion.

6.16 Can we interpret the lack of velocity change for TSF in term of response time? Has this glacier already reached an equilibrium so that no further velocity change occurs? Would such an interpretation make sense according to your knowledge of this glacier?

Thank you for this comment! We will discuss this in more detail in the text, based on thorough definitions of the respective terminology. The lack of velocity change can be explained with the topography of the glacier and the response time. The shorter the glacier tongue will become the closer the glacier will be to an equilibrium state, but currently the glacier is still far from equilibrium.

6.23. I did not understand the causal link between the depression and a limited increase in velocity

„Limited“ in this case should mean limited to the area of the depression. This means during the sink process the velocities (horizontal and vertical) increased within this area. We will clarify this in the text.

7.8 Can the authors discuss why the response time of the velocity are so different between KWF and HEF?

We can refer to existing publications why these two glaciers respond different to climate change and some additional data on present state (Kuhn et al., 1985).

Kuhn, M., G. Markl, G. Kaser, U. Nickus, F. Obleitner, H. Schneider, 1985: Fluctuations of climate and mass balance: Different responses of two adjacent glaciers, *Zeitschrift für Gletscherkunde und Glazialgeologie*, 21: 409-416.

7.20 Should be named glacier-wide mass balance (even glacier-wide mass balance rate in principle, but I agree that "rate" could be skipped for the sake of simplicity)

“This is supported by a linear regression of annual mean specific balance (b) of the total glacier area of HEF and KWF versus the mean specific balance of their accumulation areas (bc) for the period 1965/66 – 1999/00 by Span and Kuhn (2003).”

So do you suggest to change specific mass balance in glacier wide specific mass balance? i.e. not divide by area? Or just rename specific mass balance in specific glacier wide mass balance?

We want to avoid the confusion between specific mass balance and mass balance, and would prefer to use the definition at page 25 of Cogley et al:

> *“**Area-averaged (adj.)***
> *Descriptive of a quantity that has been averaged over part or all of the area of the glacier. The area-averaged mass balance is simply the specific mass balance of the region under consideration. The adjective has sometimes been used to emphasize that the specific mass balance is that of the whole glacier and not of a “specific” location (see point mass balance). “Mean specific mass balance” has been used in the same sense.”*

(as defined on page 64)

7.27 Is this statement that TSF is closer to equilibrium than GPF confirmed in geodetic mass balance using Lidar surveys?

Based on a thorough definition of terminology (what means closer to equilibrium?) we will discuss that or cite a discussion of this topic. We will check this in the DoDs and add some comments to the text.

7.28 Do the authors mean seasonal here?

We actually refer to several measurements per year, and will discuss the question with our editorial office. We did not carry out an analysis for accumulation and ablation season.

8.3. do the authors mean glacier-wide mass balance? I would say that surface mass balance at selected sites are not influenced by the changes in area (but influenced by changes in elevation through the mass balance-elevation feedback).

"As conventional parameters like ELA tend to be above summit for the investigated glaciers under current conditions and specific mass balance is affected by rapid changes in area […]."

Specific surface mass balance is defined as glacier wide surface mass balance divided by the area, and therefore clearly influenced by a reduction of area.

Also glacier wide surface mass balance is influenced by the present loss of high ablation zones. We see that is necessary to explain that, and will add some literature. For a first impression how much mass balance changes with area loss see Fischer, 2010.

Fischer, A. (2010) Glaciers and climate change: Interpretation of 50 years of direct mass balance of Hintereisferner, Global and Planetary Change 71, 1-2: 13-26. https://doi.org/10.1016/j.gloplacha.2009.11.014

Figure 4. check title of the Y-axis. The Y-title use specific, the legend "direct". Make sure terminology is used correctly and follow the UNESCO glossary.

It turned out that not title and axis using different labels, rather specific was wrongly used instead of geodetic. We will correct for the revised manuscript.

Figure 5 (and4) Do authors need to show all stakes? Why not selecting the ones which are the most informative and used in the text (or show all stakes and highlight some in bold?). Also the velocity seems to be stable for most stakes since 2000. A fact not really discussed in the article.

We will add a discussion on the "stable" velocities. We aim keeping all stakes to present the overall variability of the velocities along the glacier flow line. We will highlight some of the stakes as suggested.

Good luck for your future work on this important but under-exploited dataset,

Thank you, we will do our best!

References about velocity changes on glaciers at annual and decadal timescale (certainly not exhaustive but a good start)

BHATTACHARYA, A., BOLCH, T., MUKHERJEE, K., PIECZONKA, T., KROPÁˇCEK, J.and BUCHROITHNER, M. F.: Overall recession and mass budget of Gangotri Glacier, Garhwal Himalayas, from 1965 to 2015 using remote sensing data, Journal of Glaciology, 62(236), 1115–1133, doi:10.1017/jog.2016.96, 2016.

Heid, T. and Kääb, A.: Repeat optical satellite images reveal widespread and long term decrease in land-terminating glacier speeds, The Cryosphere, 6, 467–478, doi:10.5194/tc-6-467-2012, 2012.

Mernild, S. H., Knudsen, N. T., Hoffman, M. J., Yde, J. C., Hanna, E., Lipscomb, W. H., Malmros, J. K. and Fausto, R. S.: Volume and velocity changes at Mittivakkat Gletscher, southeast Greenland, Journal of Glaciology, 59(216), 660–670, doi:10.3189/2013JoG13J01, 2013.

Neckel, N., Loibl, D. and Rankl, M.: Recent slowdown and thinning of debris-covered glaciers in south-eastern Tibet, Earth and Planetary Science Letters, 464, 95–102, doi:10.1016/j.epsl.2017.02.008, 2017.

Schaffer, N., Copland, L. and Zdanowicz, C.: Ice velocity changes on Penny Ice Cap, Baffin Island, since the 1950s, Journal of Glaciology, 1–15, doi:10.1017/jog.2017.40, 2017.

Tedstone, A. J., Nienow, P. W., Gourmelen, N., Dehecq, A., Goldberg, D. and Hanna, E.: Decadal slowdown of a land-terminating sector of the Greenland Ice Sheet despite warming, Nature, 526, 692, 2015.

Thomson, L. I. and Copland, L.: Changing contribution of peak velocity events to an nual velocities following a multi-decadal slowdown at White Glacier, Annals of Glaciology, 58(75), 145–154, doi:10.1017/aog.2017.46, 2017a.

Thomson, L. I. and Copland, L.: Multi-decadal reduction in glacier velocities and mechanisms driving deceleration at polythermal White Glacier, Arctic Canada, Journal of Glaciology, 63(239), 450–463, doi:10.1017/jog.2017.3, 2017b.

Vincent, C., Soruco, A., Six, D. and Le Meur, E.: Glacier thickening and decay analysis from 50 years of glaciological observations performed on Glacier d'Argentière, Mont Blanc area, France, Annals of Glaciology, 50(50), 73–79, doi:10.3189/172756409787769500, 2009.

Other references cited in the review

Dehecq A, Gourmelen N and Trouve E (2015) Deriving large-scale glacier velocities from a complete satellite archive: Application to the Pamir–Karakoram–Himalaya. Remote Sensing of Environment 162, 55–66 (doi:10.1016/j.rse.2015.01.031)

Gardner AS, Moholdt G, Cogley JG, Wouters B, Arendt AA, Wahr J, Berthier E, Hock R, Pfeffer WT, Kaser G, Ligtenberg SRM, Bolch T, Sharp MJ, Hagen JO, van den Broeke MR and Paul F (2013) A Reconciled Estimate of Glacier Contributions to Sea Level Rise: 2003 to 2009. Science 340(6134), 852–857 (doi:10.1126/science.1234532)

Huss M (2012) Extrapolating glacier mass balance to the mountain-range scale: the European Alps 1900-2100. The Cryosphere 6(4), 713–727 (doi:10.5194/tc-6-713-2012)

Marzeion B, Jarosch AH and Hofer M (2012) Past and future sea-level change from the surface mass balance of glaciers. The Cryosphere 6(6), 1295–1322 (doi:10.5194/tc-6-