Author Responses to Reviews

Paper: "Which are the highest peaks in the US Arctic? Fodar settles the debate"

Authors: Nolan and DesLauriers

The authors would like to thank the reviewers for their thoughtful reviews. Below we extracted all of the recommended changes and included our response **in bold** beneath them.

Reviewer #1

While I think that the authors have successfully demonstrated better than 20 cm vertical and geolocation accuracy for fodar overall, I would like to strike a slightly more cautious note on the potential of bias (in addition to the higher random noise levels there that is noted by the authors) in areas sub-optimal to feature matching such as deep shadows inside steeper gullies etc. By bias I mean the possibility of subtly but systematically higher or lower elevation results correlated with low amplitude or other data properties. The real changes that the authors clearly demonstrate likely would not allow to measure or exclude such effects with the present datasets. An ideal final test to check for such biases would probably be a repeat fodar test on the same day with significantly different sun angles to produce complementary shadows. While this is of course beyond the scope of the current presentation; the potential of bias and how it could be assessed should at least be mentioned.

Response: We agree that such an experiment is a good idea and will give it a try in the future. We haven't noticed this over rock areas, but snow covered areas could have unique challenges. We modified the text to address this.

Specific comments:

 * Abstract: Elevations in short summary should be consistent with abstract; currently they differ by 0.4 m

Response: Yes, between the time the summary was written and the paper was submitted, we discovered an error caused by a vertical component of the horizontal transformation from WGS84 to NAD83. Unfortunately we're not sure how to edit the summary, so could use some help from the editorial staff in changing the summary to match the paper.

* Introduction: (p6875,l26): the emphasis on software is misleading (makes one think of differences in the price of software at first) ; suggest changing to: ". . . the difference in price is the Structure-from-Motion algorithm allowing for prosumer grade cameras to be used without . . ."

Response: Agreed, corrected.

* Methods: (p6879, l9): ". . . based on co-registration with the poor 2008 DEM . . ."; do not understand: why poor if the claimed accuracy is 16 cm and the posting is finer for the 2008 DEM?

Response: There were two DEMs acquired in 2008- a large poor one and a small good one, so here we were referring to the large poor one and have tried to make this clearer in the text.

*Discussion: (p6885, l28): replace "boost" with "back-project";

Reponse: Agreed, corrected.

(p6886, l) ". . .We tested the experimental 15B model and found it gave all peaks 1.4m downward shift, but given that the current models indicates a 1.5m gradient over this distance, future higher resolution data could yield gradients of that size but with opposite sign, suggesting that this debate is not fully settled . . .": The discussion on the implications of using different geoid models is not clear: is it "current model" (not models) and does this refer to the currently used geoid? Is "gradient" a spatial gradient and if yes over which distance does it apply (some remark on the spatial resolution of the geoid models in question is warranted). What is the spatial gradient across the 40 km from Mt Chamberlin to Mt Hubley?

Response: Agreed, corrected.

Technical corrections:

* Abstract: (p6872, l1) Grammar of first sentence is incorrect, suggest changing to : "While being outstanding accomplishments of their time, the United States Geological Survey's topographic maps of the eastern Alaskan Arctic nonetheless had significant errors introduced when made in the late 1950s."

Response: Agreed, Corrected.

* Introduction: (p6873,l6); typo "mounatins".

Response: Agreed, Corrected.

(p6873,l11 ff): change ' to ft?.

Response: We leave this for the editorial staff to decide.

(line 24): "on an annual basis" or "annually".

Response: Agreed, Corrected.

* Methods: (p6876,l16): drop the ' symbol;

Response: We again leave this for the editorial staff to decide.

(p6879, l10); typo: " . . . based on some limited ground control . . ."

Response: Agreed, Corrected.

* Discussion: (p6885,l25); duplicated word: "that"

Response: Agreed, Corrected.

* Tables and Figures: (Table 1 caption): ".. USGS peak elevations in feet are from labels of the printed map sheets, except for Mt Okpilak at 1:63630, which was interpolated from the contours bracketing the symbol for peak on the map;Mt Okpilak is our unofficial . . .";

Response: Agreed, Corrected.

(Figure 3 caption): " . . . The consistency of color shift between mountain faces is not due . . ." ("s" was missing); " . . . Again, there is a lot of real change . . ." ("lots" is colloquial); typo: "guassian"

Response: Agreed, Corrected.

Reviewer #2

1.1 Orginality I believe the very nature of the section heading "originality" almost speaks for itself. This is clearly a very 'original' manuscript. It does indeed offer progress beyond our current scientific understanding; not only 'substantial' in that it resolves a long-standing debate about the height of the highest U.S. Arctic peak, but more significantly by providing a well characterized case study that offers metrics of a new and relatively unproven technique. Some of these metrics were addressed by the first reviewer, B. Rabus, who highlighted and reiterated the authors's points about the potential for bias errors due to poorly sampled scenes. But personally, I find some of the most intriguing aspects of the study the demonstration and quantification of the amount of variability these peaks have due to dynamic processes. This case study not only further validates the technique of fodar, but demonstrates further how valuable it may be as a method to track highly variable geomorphological processes.

Response: We agree.

1.2 Scientific Quality The purpose of this work, stated by the title, is quite straight forward. I would argue, however, that the manuscript is not well titled given the level of enlightenment the work itself provides. That is, through this case study, we learn more about the level of accuracy and applicability of fodar for geoscientific applications, but the title only implies a novelty of the application of the method. Regarding rigour and scientific quality, I find no substantial errors in the processing steps. As for the discussion of results, this is most interesting. The work is well referenced, and demonstrates a broad understanding of the topic of both geomorphology and climate-related destabilization of mountain glaciers as well as the technical details of the methodology.

Response: We agree.   It certainly would have been possible to focus on the broader significance of the work, but in the end we felt telling this story had its own merits without distracting from the broader impacts.

1.3 Significance The significance of this publication lies not so much in the debate it purports to resolve, but rather in the demonstrated application of the method, and the illuminating discussion of the future potential. Fodar will likely be a 'disruptive' technology for map making and we are provided an entertaining case study in this manuscript.

Response: We agree.

1.4 Presentation Aside from some colloquial points raised by the first reviewer, the manuscript is well written and the figures provide an appropriate level of detail to support the arguments presented.

2 Conclusion While I am discouraged personally to see National Geographic publish a news piece on an article that is in review, that is a modern result of the 'open access' and 'open review' process Copernicus provides. It is beyond the scope (or factually, not even a part of the scope) of this review to assess the appropriateness of the process. Reviewing the manuscript becomes somewhat of a more technical exercise as it's 'newsworthiness', or 'Impact', has been pre-assessed. Still, as discussed above, my sense is that the true contribution of this work is not in the debate the title addresses, but in providing an excellent case study in which future fodar applications may bench-mark their results. Further, this is an inspiring work that demonstrates the value of this method to the field of geomorphology. To a degree, I would have preferred to see Dr. Nolan reach out from the comfortable 'cryosphere' community and present this work in another manuscript such as Earth Surface Dynamics, where it may potentially have an even larger impact

Response: The reviewer raises a spectrum of interesting philosophical questions here that we think all scientists are grappling with given the evolution of digital publications. To address the easiest question (from the introduction), this review was not at all meant to be a rubber stamp, though of course we would not have submitted it if we didn't feel it was worthy of publication. More philosophically, what does it really mean to submit a paper for 'review' when the paper itself is online? That is, by making the paper available publically during review, it becomes essentially a public announcement of what the authors believe is true. From our perspective an online peer-review has essentially become a more rigorous approach to the common practice presenting data and results in meetings and conference proceedings, for the purpose of both sharing information and soliciting feedback from the scientific community. And in the end, these reviews are not intended to pronounce results as correct, but rather to serve as a filter to indicate that the methods are sound and verifiable, both to busy scientists (who will evaluate the results on their own anyway) and to the public (who may not have the capability to make such evaluations on their own). As soft-money scientists, we also all have to find a balance between sharing results rapidly and sharing them rigorously, for the purposes of continued funding. We felt submitting to TCD was a means towards that end, though not a perfect one. Because the paper is already online, however, we have little control of which news sources will report on it, so our choice is either to team up with news sources to help limit their misunderstanding of it or let them report on this announcement completely on their own. Given that some of our funding came from National Geographic and we knew they were going to publish something about it anyway, we felt it better to team up with them, as this way we could also dissuade them from writing an article *before* we submitted our paper. Our major qualm with this new way of submitting papers is that journals give the submitted drafts DOI numbers and make them referenceable and discoverable forever, which allows errors in the original manuscript to resurface indefinitely. It seems TCD is changing its policy on this, and that is a good thing in our opinion. In any case, we appreciate the thoughtfulness of the review and raising issues associated with online peer reviews.