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Interactive comment on "Which are the highest peaks in the US Arctic? Fodar settles the debate" *by* M. Nolan and K. DesLauriers

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1 Overall Impression

Since Dr. Nolan's initial presentation of the 'fodar' technique in The Cryosphere, V.9, 1445-1463, 2015 http://www.the-cryosphere.net/9/1445/2015/ doi:10.5194/tc-9-1445-2015 as predicted the usage of SfM for mapping and geophysical applications in general has exploded. This manuscript uses the methodology to settle a long standing debate and provide, in a rather entertaining fashion, a case study of the level of detail that may be achieved from the method.

It seems, however, the manuscript reviews may not be necessary, as

the 'word is out', and apparently Dr. Nolan's results are already accepted by what some may consider the most famous geographical society: http://news.nationalgeographic.com/2015/12/151216-anwr-highest-peak-mt-isto-fodar-remote-sensing/

Note that the date of the article and the date of publication of this work in TCD are both 16 December. This then raises an interesting philosophical debate for a reviewer. What is the purpose of the review? To validate the result? To 'judge' whether the work is worthy of publication in TC? Or simply to offer a 'stamp of approval'?

As a reviewer, we are asked to grade the manuscript on a scale from poor-fair-good-excellent on the following points:

1) Originality (Novelty) Within the scope of The Cryosphere, does the manuscript represent substantial progress beyond current scientific understanding (new insight, concepts, methods, or data)?

2) Scientific Quality (Rigour) (A) Is the purpose of the work clearly articulated, reflected in an adequate methodology, and its achievement compellingly underpinned by the evidence presented? (B) Are the applied methods and techniques valid and suitable? (C) Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?

3) Significance (Impact) Does the manuscript contribute to changing our scientific understanding of a subject substantially or to introducing new practical applications of broad relevance?

4) Presentation Quality Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?

Given the nature of publication in TCD, that results are often accepted prior to acceptance in The Cryosphere – as exemplified by the above article by National Geographic

– I'd like to use this review to go through these points carefully.

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.

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.

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.

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.

Interactive comment on The Cryosphere Discuss., 9, 6871, 2015.