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14 May 2018

Prof. A. Cracknell

Co-Editor-in-Chief, *International Journal of Remote Sensing*

University of Dundee, UK

**Re: Submission of Manuscript for Review**

Dear Prof. Cracknell,

I would like to resubmit a manuscript entitled “Radiometric Homogenisation of Aerial Images by Calibrating with Satellite Data” for possible publication in the *International Journal of Remote Sensing* as a peer-reviewed paper. It is a revision of manuscript ID TRES-PAP-2017-0624, originally entitled “Coarse Surface Reflectance Homogenisation of Aerial Images by Calibration with Satellite Data”.

In this revision, we have addressed referees’ comments where appropriate, and made an important addition and improvement to the accuracy assessment. A comprehensive response to the referees’ comments begins on page two of this letter.

The manuscript describes a method for homogenising the digital numbers of aerial imagery to estimated surface reflectance values. A collocated and concurrent, well calibrated satellite image is used as a surface reflectance reference to which the images are calibrated. The relationship between the surface reflectance of the reference image and digital numbers of the aerial images is approximated with a spatially varying local linear model. The results of an accuracy assessment compared well with those of existing aerial image calibration techniques. The method corrects for coarse-scale atmospheric and bidirectional reflectance distribution function (BRDF) effects and does not require spectral measurements of field sites or placement of known reflectance targets. Due to its relative simplicity and efficiency, I believe it is an attractive alternative to existing aerial image calibration methods.

I believe that this manuscript is appropriate for publication by the *International Journal of Remote Sensing* because it is concerned with remote sensing data collection and theory. Specifically, it proposes a calibration technique for reducing unwanted radiometric variation in aerial imagery of the earth surface.

There are no conflicts of interest to disclose. I hope you find that the paper meets the required standards for publication.

Contact details of corresponding author:

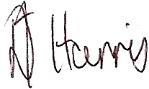
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Sincerely,



Dugal Harris

**Response to Referees**

We would like to thank the referees for their valuable feedback. The original referee comments are included below with our responses in red. Where referee comments are not responded to below, we have addressed them in the manuscript.

The results of the comparison with the SPOT 5 image have been improved by improving the accuracy of the SPOT 5 orthorectification (Sections 2.7 and 3.4).

**Referee: 1**

Comments to the Author

The main idea is interesting and has a good potential for practical application. But I still have some questions.

My main concern is that there is a large difference between the spatial resolution of DMC and MODIS reflectance products yet the uncertainty caused by the difference was not addressed. Why not use a finer spatial resolution, perhaps Landsat data as reference given that one scene of Landsat 7/8 ETM+/OLI covers 185km by 170km?

Unfortunately, a suitable Landsat scene was not available for our case study. There was only a cloudy Landsat 7 scene with SLC error concurrent to the time of our case study. We have added some further discussion on the possibility of using Landsat in Sections 2.5 and 4. Use of alternative sources of reference data will be investigated in future work.

In page 14 line 25-27 the author claims that despite the big spatial resolution gap between aerial images and the MODIS MCD43A4 product, the final mosaic do not need to further reduce seam line (feathering) and the overlapping areas can be chosen from any of the overlapping images, from my experience, this is highly doubtable.

While feint seam lines may be present in some circumstances, we did not find it necessary to employ further measures for reducing seam lines in the case study. Section 3.2 presents some figures to support this claim. We have expanded the discussion on seam lines and mosaicking in Section 2.4 to better address this concern.

The following are some detailed comments and questions.

Page 2

Line 3: I am not a native English speaker, but may be the title should be "by calibrating with"

Line 17: “It is shown that ...” what shows? Please rephrase the sentence.

Line 24: maybe “lease squares regression”? Please clarify.

Line 25: the acronym DMC should be in parenthesis, please check the journal style.

Line 27: BRDF used before defined in line 38.

Line 35-39: the sentence is not clear to me, please rephrase this sentence.

Line 55: VHR, maybe need to be defined before use in main text, please check the journal style.

Page 4

Line 23-28: as I understand, aerial image such as DMC usually took in a very small yaw and roll angle, why there are large view angles? Or perhaps large field of view（FOV）? Why solar varies? This sentence is confusing, please clarify?

Page 7

Line 19: the acronym DN should be in parenthesis, please check the journal style.

Line 30: Equation (2) should be , where d is the distance between the sun and the earth in astronomical units.

We use in Equation (2) which is the irradiance at the sensor, *d* would only need to be included if was the solar irradiance.

Line 34: should be described as “TOA reflectance” rather than “reflectance”, since the reflectance and TOA reflectance are quite different.

We prefer the term “at the sensor”, rather than TOA, to describe these quantities as they refer to an aerial sensor which is not strictly at TOA. We have adjusted the terminology to be “at the sensor” for all relevant quantities.

Page9

Line 36: The size of sliding window should be specifically defined. The BRDF and RSR are both related to the type of the targets. It is quite important to define the window size to make sure that the pixels in the window represent the same targets.

We do not define the window size in Section 2.2, as we want to leave the method generic and applicable to different combinations of aerial and reference images. We have, however, added a discussion around factors influencing the choice of window size in this section. The window size for the case study is defined and discussed at the end of Section 2.5, and further justified with new results in Section 3.4.

Figure 9：Since the effects of BRDF and RSR are both related to the type of the targets, the linear relationships between the reflectance of the DMC and MODIS should be different for different target type. The results should be showed independently for each sampling type.

While the BRDF and RSR effects are related to the target type, the surface reflectance produced by the homogenisation is an absolute quantity and therefore comparable between sensors. The homogenisation method compensates (approximately) for the BRDF and RSR effects (see Section 2.3).

Page 14

In my opinion, section 2.4 should be placed as 3.1, or 2.1, please check the journal manuscript guidance.

We have found no specific journal guidance on this. Section 2.4 refers to Sections 2.1 and 2.2, and Section 2.6 refers to Section 2.4, so we believe Section 2.4 is best left where it is.

Page 15

Line 23: since “except NIR”, then not good in all bands. Please phrase？

Page 26

Line 57: “and mosaic normalization techniques to reduce seam lines” sentence is not complete.

Page 27

Line 34-57: you point out that the varying size of the sliding window should be investigated and a higher spatial resolution reference such as Landsat OLI, yet in my opinion, they need to be discussed in the manuscript, or at least the varying size of the sliding windows should be discussed if there were no concurrent Landsat data available.

We have added some discussion around Landsat in Sections 2.5 and 4. A new experiment investigating the effect of changing the sliding window size is now described in Section 2.7, with results presented in Section 3.4.

**Referee: 2**

This manuscript is aiming to perform the radiometric normalization of aerial images by collocated  
and concurrent, well-calibrated satellite images. The content shows no novelty but is somehow  
useful for those experiments without radiometric calibration of aerial camera. However, the  
description is too poor to be accepted for the publication.

Detailed comments are as follow:

Section 2.1 and 2.2, in my opinion, is almost useless. However, this part occupied 6 pages.  
For example, equation 5 is a standard relationship between DN and reflectance for any  
optical camera, which means equation 1-4 is unnecessary. In the algorithm, the authors  
assume that effect caused by RSR difference is linear. They didn’t take this effect into  
account. If so, it is unnecessary to give detailed description here.

In homogenization procedure, if we perform step (2) and (3) directly at course resolution  
without step (1), what’s the difference, please clarify.

The purpose of this manuscript is to increase the radiometric accuracy of the aerial images.  
However, in whole manuscript, the authors talked too less about the uncertainty. What is  
the accuracy of the MODIS MCD43A4? What is accuracy of the algorithm? The accuracy  
impacted mostly by atmospheric correction, geometry, and RSR difference. But I cannot find  
any explanations.

The validation by SPOT 5 is meaningless, which gave only the comparison between MODIS  
and SPOT 5. I suggest the authors conduct the comparison between with and without BRDF  
correction.

The most important thing is the description of aerial experiment and images, as well as  
surround conditions. In this manuscript, however, I almost can find nothing. Where is the  
study area? What are the specifications of DMC? When did the images take? How many  
days? What is the AOD in those days? Is it possible that the impact from no atmospheric  
correction is much larger than BRDF correction?