**Atmospheric
Chemistry
and Physics
Discussions**

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Comment

Interactive comment on "Airborne hyperspectral surface and cloud bi-directional reflectivity observations in the Arctic using a commercial, digital camera" *by* A. Ehrlich et al.

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We would like to thank the reviewer for the extensive list of comments. Addressing these comments has been very helpful to improve the manuscript. Especially, by removing the grammatical errors and misleading statements the revised manuscript will be easier to understand for the reader. The detailed replies on the reviewers comments are given below. The reviewers comments are given italicized while our replies are written in roman letters. Citations from the revised manuscript are given as indented text.

Detailed Replies

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1. This paper must be edited for grammar and clarity. There are so many grammatical mistakes and unclear statements in nearly every page as shown below in Technical comments.

Thanks to the reviewer for reviewing the manuscript so intensely. To our excuse we can attest that an extensive copy-editing procedure by Copernicus was performed with the manuscript before publishing. Unfortunately this was obviously not sufficient. In the revised manuscript we did follow all suggestions of the reviewer to improve grammar and clarity. Thanks again for providing as many comments. For details, see below.

2. In order for airborne BRDF measurements to have the intended meaning, it's important to do atmospheric correction. BRDF or BRF or HDRF are intrinsic properties of a surface, uncontaminated by atmospheric effects.

The reviewer is right, one intention of the measurements is to provide a new instrument to derive surface properties as BRDF, BRF or HDRF. For the original manuscript we initially decided that we first want to show that the measurements themselves are useful without any correction. That is why we took the measurements at flight altitude and compared them with corresponding simulations representing the atmosphere as well as possible. Sure, BRDF, BRF or HDRF are mostly used as surface characteristics. But there is no reason to restrict the definition of those quantities to the surface. We can define BRDF, BRF or HDRF for any flight altitude. Then it is just the characteristic of the entity of the underlying surface and atmosphere. In case of clouds we do not have any other choice. There is no constant reference altitude of cloud top. That's why we would like to keep the $HDRF$ in flight altitude as the quantity we want to compare with the radiative transfer model.

For the measurements above sea ice and open water we agree that a surface $HDRF$ should be presented in the manuscript. For the sea ice measurements we can easily argue that the flight altitude with 100 m above ground is low enough and the sur-

face albedo high enough that no atmospheric correction has to be applied. The measured signal is dominated by the surface reflection. In simulations (not shown in the manuscript) we found only negligible differences (about 0.1 % or 0.001 in absolute values) between the $HDRF$ in 100 m altitude and the surface $HDRF$. Unfortunately, we did not measure in such low altitudes above open water. Here the surface albedo is much lower than for sea ice and atmospheric contribution higher. For this case we did apply an atmospheric correction using the iterative correction method by Wendisch et al. (2004). In Section 4.2.2 we added:

> Due to the high altitude at which the measurements above open water have been conducted, an atmospheric correction has been applied to extract the contribution of radiation reflected by the atmosphere below the aircraft. We adapted the iterative correction method by Wendisch et al. (2004) for the radiance measurements of the camera assuming a Lambertian-reflecting surface in the radiative transfer simulations.

Figure 6 now shows the corrected $HDRF$ for open water. In Figure 9 we include both uncorrected and corrected measurements. As mentioned above, we are convinced that a comparison of the uncorrected measurements with radiative transfer simulations at flight altitude better suits our intention to show the quality of the measurements.

3. Radiometric calibration. Describe how calibration coefficients were obtained. Figure 3 demonstrates how the noise was removed, but not how calibration coefficients for each pixel was obtained. Why are the coefficients different for different pixels? Did the authors check linearity of the detectors – at low/high light levels?

The reviewer is right, we did forget to give the equation which was used to calculate the calibration coefficients. This is corrected in the revised manuscript by adding:

The calibration coefficients k_λ were calculated for each camera pixel (x, y) and each camera channel using the camera signal $S_{\lambda,\text{C}}(x, y)$ (digital counts) and the NIST traceable radiance emitted by the integrating sphere $I_{\lambda,\text{IS}}$,

$$k_\lambda(x, y) = \frac{S_{\lambda,\text{C}}(x, y)}{I_{\lambda,\text{IS}}}. \tag{1}$$

From our point of view, it is obvious that the calibration coefficients are different for different pixels. Each pixel can be seen as a single detector with its own characteristics (noise). Additionally, the lens of the camera causes a different sensitivity of each pixel to the illumination (vignetting effect). We do not think that additional explanation is required in the text.

The sensitivity of the camera was checked for linearity. The good linearity was not surprising for us. So we did not discuss it in the original manuscript. In the revised version we added:

> The sensitivity of the CMOS image sensor was additionally tested for linearity. The results (not shown here) agree with the study reported by Kaufmann (2010) who showed an almost perfect linear response of the CMOS image sensor to the intensity of the incoming radiation.

4. Geometric correction. Describe how the camera images were corrected for the aircraft roll and pitch, and may be yaw.

The correction of the images for the aircraft roll, pitch and yaw is a regular rotation of the image coordinates with roll, pitch and yaw. The method is well known and follows simple math known as *Euler*-rotation. Therefore, we do not want to include a detailed

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description of the rotation matrices, etc. In this context, the original text might have been misleading. To clarify the method of the correction we rephrased the section to:

> θ_v and φ_v have been corrected for the aircraft roll and pitch angle. Therefore, Euler rotations of the pixel coordinates with roll and pitch angles were applied. The rotation of θ_v and φ_v gives θ_r and φ_r, the zenith and azimuth angles of the reflected radiation in Earth fixed coordinates. Finally, the images have been rotated into the azimuthal direction of the Sun φ_0.

5. Figures are too small and difficult to distinguish between the objects being displayed.

We agree with the reviewer that the images are to small in the ACPD layout. Most figures have been designed for a portrait vertical layout. In this case they will fill out a full A4 page and appear much larger, what we have tested with the Tex-style provided by ACP. However, we checked the figures again and made some adjustments. E.g., the arrangement of Figure 9 was changed into a vertical layout.

Technical Comments

1. Title: too confusing and has no meaning. What do the authors mean by the terms "hyperspectral surface" and "bi-directional reflectivity."

The sequence of the words was not chosen right. "Hyperspectral" is related to the observations not to the surface. The use of reflectivity is justified below for Page 24593: 1. line 1. We removed "bi-" as it is primarily the HDRF what is derived by the measurements. "directional reflectivity" is chosen to give a general and short expression for quantities like BRDF/BRF or HRDF. We changed the title accordingly and removed "in the Arctic" to shorten the title to:

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Airborne hyperspectral observations of surface and cloud directional reflectivity using a commercial digital camera

Page 24592: 1. line 3: The "bi-directional reflectivity" – is not part of the nomenclature for reflectance. Refer to Nicodemus et al. - cited in the references, pg. 24616 – line.

Reply is given below for Page 24593: 1. line 1.

Page 24592: 2. line 6 – What does the acronym SMART stand for?

The full name of the SMART (Spectral Modular Airborne Radiation measurement sys-Tem) has been added to the abstract.

Page 24592: 3. lines 6-7: specify the uncertainties of the instruments.

The uncertainty of 6 % for both instruments is now quantified in the abstract as well.

Page 24592: 4. line 8 – enclose in brackets: (HDRF).

Has been changed.

Page 24592: 5. line 9: specify whether "albedo" is broadband or spectral.

In the abstract of the revised manuscript, we now specify the wavelength with 530 nm.

Page 24592: 6. line 11 – since the bow is over clouds, why not call it "cloud bow" instead of "fog bow"

That is a good point. We struggled to find the best name for the phenomenon. "Cloud bow" seems to be the best choice. So we changed it throughout the manuscript.

Page 24592: 7. line 24 – change "reflectivity" to "reflectance"

Reply is given below for Page 24593: 1. line 1.

Page 24592: 8. line 25 – add "a" between "As" and "lower"

Thanks for this correction.

Page 24593: 1. line 1: we don't have "bi-directional reflectivity distribution function" in the nomenclature for reflectance. May be the authors are talking about "bidirectional reflectance-distribution function." Refer to Nicodemus et al. or Schaepman-Strub et al –cited in the references, pg. 24616 – line 14 and 23, respectfully.

The reviewer is right, we do not completely follow the nomenclature of Nicodemus et al. or Schaepman-Strub et al.. The only thing we changed is to use "reflectivity" instead of "reflectance". There are good reasons why we decided to do so. In our view all quantities with suffix "-ance" are radiometric quantities and have a dimension of, e.g., radiance ($\mathrm{W\,m^{-2}\,sr^{-1}}$) or irradiance ($\mathrm{W\,m^{-2}}$). Quantities which are the ratio of two radiometric quantities are dimensionless and own the suffix "..ivity" like transmissivity, emissivity. Contrarily, transmittance or emittance have the unit ($\mathrm{W\,m^{-2}\,sr^{-1}}$). Following these rules, reflectance is the radiance reflected by a surface in units $\mathrm{W\,m^{-2}\,sr^{-1}}$ but not normalized with the incoming irradiance. Normalizing with the incoming irradiance results in the quantity reflectivity in units $\mathrm{sr^{-1}}$. This is usually once more normalized with the reflectivity of a Lambertian surface, $\pi\,\mathrm{sr}$, which gives the dimensionless reflectivity factor.

This nomenclature follows the discussion by Bohren and Clothiaux (2006). However, we would like to keep the references to Nicodemus et al. or Schaepman-Strub et al. as the general definitions and geometry of BRDF, BRF and HDRF and their differences are well presented in both references.

Page 24593: 2. line 28 – delete "a radiance" and replace "inlet" with "system." Change "a" to "an"

Has been changed.

Page 24593: 3. line 29 – end the sentence at "view" and begin a new sentence: "The mirror rotates at 100 r min..1 ..."

Has been changed.

Page 24593: 3. line 29 – the sentence "An entire scan of the lower hemisphere is obtained within 2–3 min" is not correct the way it's written. Replace "scan" with "BRDF measurements"

Has been changed.

Page 24594: 1. line 6 – replace "area" with "angle"

We changed the sentence into: "...to cover zenith angles $\pm 60°$ from the nadir direction."

Page 24594: 2. line 7, 21, and 22 – The switch between "BRDF measurements" and "HDRF measurements" is confusing.

The reviewer is right, we did not consistently use $BRDF$ and $HRDF$ in the manuscript.

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Thanks for finding this. We checked and corrected the use of $BRDF$ and $HRDF$ once more throughout the entire manuscript.

Page 24594: 3. line 11 - insert "have" between "studies" and "used"

Has been changed.

Page 24594: 4. line 13 – replace "have been" with "were"

Has been changed.

Page 24595: 1. Line 2 – replace "employed" with "deployed" 2. line 4 – replace "have been" with "were"

Has been changed.

Page 24596: 1. Line 1 – add comma after "camera" and replace "and" with "which"

Has been changed.

Page 24597: 1. Line 13 – the statement "the dark current of the images was determined in the laboratory for different camera settings and environmental conditions and subtracted from the data" – define/specify the environmental conditions. Also, dark current of the images would be different in the field than in the laboratory. How was this taken into account during the processing of the field data?

In general, the dark current depends on temperature and exposure time. However, for CMOS sensors, the dark current is very low as shown by (Kaufmann, 2010). (Kaufmann, 2010) stated that dark current is no problem at all for exposure times below

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33 ms. Our camera settings used an exposure time of 1/2656 s=0.38 ms which ranges two magnitudes below. We tested the dark current in the lab for temperatures between 10° C and 25° C and for exposure times between 1/8192 s and 2 s. For all measurements the dark current (quantified as mean value of the signal from all pixels) was below one digital unit and thus not detectable. Sure, the pixel show individual dark noise, which makes the images more noisy at long exposure times, but the mean does not change. We changed the section in the manuscript to:

> Finally, the dark current of the images was determined in the laboratory for different camera settings and environmental conditions. Images without illumination were taken for temperatures between 10° C and 25° C. All data were taken with the same exposure as used during the airborne measurements (1/2656 s) and showed that the dark current does not exceed one digital unit. Thus the dark current is negligible which agrees with Kaufmann (2010) who found that the dark current is no issue for exposure times below 33 ms.

Page 24598: 1. line 7 – I have never heard of "full width of half mean" only " full width at half-maximum" . Check proper definition of FWHM.

Thanks again. "Full width at half-maximum" was meant here.

Page 24598: 2. line 9 – how was "center wavelength" determined.

The center wavelength is given by the median value of the relative spectral response function. We indicated that in the revised manuscript both in the text and in the caption of Figure 2.

Page 24598: 3. line 23 – The tests where "a series of images was taken while the

camera was moved horizontally and vertically" only confirms the camera alignment. But it's more important to check the angular sensitivity of the camera. How sensitive is the camera to the light coming in at oblique angles? It's also important to check sensitivity to distance between the camera and the integrating sphere. Measurements were done at two distances from the integrating sphere (5 cm and 15 cm); how were these two distances selected?

As explained in the manuscript, the exit port of the integrating sphere is not large enough to cover the entire field of view of the camera. That is why we had to move the camera and merge several images. As the radiation emitted by the integrating sphere is almost perfectly Lambertian, independent of direction, it does not matter in which direction the camera is looking at the exit port. The lambertian characteristic of the integrating sphere is known and has additionally been confirmed with radiance measurements by the SMART-Albedometer. The same holds for the distance to the sphere. The radiance emitted and measured is always the same independent of the distance to the exit port. We confirmed this by calibrating at two distances. There is no difference in both measurements except that the exit port covers areas of different size in the images. The distances have been selected arbitrarily for practical reasons of the calibration setup. The sensitivity to radiation coming in at oblique angles was tested and is negligible. Otherwise, the camera would not provide sharp images.

Page 24560: 1. line 13 – shouldn't this equation be given an equation number?

An equation number is included in the revised manuscript.

Page 24600: 2. line 17 – remove hyphen in "bidirectional." If the authors are following Nicodemus nomenclature for reflectance, then BRDF should be expanded as: bidirectional reflectance-distribution function.

The hyphen has been removed. The reply to the uses of reflectivity is given above for Page 24593: 1. line 1.

Page 24600: 3. line 20 – delete "or layer" – it has no meaning.

Has been changed.

Page 24600: 4. line 24 - remove hyphen in "bidirectional."

Has been changed.

Page 24601: 1. line 1 – delete "sample"

Has been changed.

Page 24601: 2. line 4 – It's not clear what's meant by "With Eq. (4) this equals to"

The reference to Eq. 4 was wrong. We deleted this in the revised manuscript.

Page 24601: 3. line 6 – This statement needs to be explained or deleted from the text as it's incorrect: "However, both BRDF and BRF can be measured directly only when an artificial radiation source is applied." On page 24612, the authors seem to contradict themselves when they say, in line 21, "To obtain the more general surface BRDF, an atmospheric correction has to be applied, which is not done here but planned for future studies." Where are they going to get the "artificial radiation source" in this case?

Yes, we again have not been consistently and did forgot the mention that $BRDF$ or BRF can be derived by an atmospheric correction. We deleted the statement in the conclusions as we now did apply an atmospheric correction at least for the measure-

ment above open water. Further, we added the following sentences at Page 24601 line 18:

From $HDRF$ measurements at a certain altitude, the $BRDF$, BRF, and the $HDRF$ at surface level can be derived by applying an atmospheric correction using radiative transfer simulations as shown by Gatebe et al. (2003) and Lyapustin et al. (2010). With the intention to validate the radiance and $HDRF$ measurements of the camera as they are (at flight altitude), we do not apply an atmospheric correction for the comparison of measurements and simulations and present $HDRF$ measurements instead of atmospherically corrected $BRDF$ or BRF.

Page 24601: 4. lines 12-14 –explain what's done to Eq. (5) to get Eq. (6). A reader would like to know this.

We are not sure what the reviewer meant here. We do not state that Eq. 6 follows from Eq. 5. We just introduce the definition of the $HDRF$ here. So we do not add an explanation how Eq. 6 is derived. If the transition from Eq. 6 to Eq. 7 is meant by the reviewer, we think it is sufficient to refer to Schaepman-Strub et al. (2006) who showed this in much detail. The intention of our study was not to review the definitions of reflectivity quantities. We only wanted to introduce the quantity we measured. However, for clarification of this step, we added the following sentence at P24601 line 13:

Using the definition of F_{glob} and introducing the fraction of direct incident radiation $f_{\mathrm{dir}} = F_{\mathrm{dir}}/(F_{\mathrm{dir}} + F_{\mathrm{diff}})$, Eq. **??** can be transformed to:

Page 24602: 1. line 3 – Does the 16000 pixels of the camera represent the same view geometry as the SMARTAlbedometer? This needs to be explained.

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Sure, this is the case. The camera images have been corrected for the aircraft roll and pitch. The SMART-albedometer has an active horizontal stabilization. Thus we know where the nadir pixels of each camera image are and only used these to calculate the mean radiance. To clarify this in the manuscript, we changed P24602 line 3 to:

> The radiance optical inlet of the SMART-Albedometer is horizontally stabilized into nadir direction and has a field of view of 2.1°. This spot corresponds to about 16 000 pixels of each camera image.

Page 24602: 2. line 8 – what does "exemplary time interval" mean?

We rephrased the sentence to:

> In Figure 5a measurements of channel 1 are compared for an exemplary time interval on 17 May 2010 that have been chosen to cover different surfaces such as sea ice, open ocean, and clouds.

Page 24602: 3. line 11- what does "temporal course" mean?

We rephrased the sentence to:

> ... radiances obtained from the camera images agree with the SMART-Albedometer measurements ...

Page 24602: 4. line 15- the statement "This difference of 4% mainly resulting from the radiometric calibration ranges in the uncertainties of both instruments" is not clear. It seems to imply that the differences are explained only by the radiometric calibration differences. Is this the only reason?

The uncertainty of the calibration is about 6% for both instruments. The different in the measurements is 4%. So, there is no need to identify any other reasons for the differences as the calibration uncertainty may already cover the total uncertainty. However, we rephrased the sentence to:

> This difference of 4 % ranges in the uncertainties range of the radiometric calibration of both instruments.

Page 24602: 5. line 18 – what does this mean: "spatial allocation?"

The sentence should explain that two data points of both instruments do not sample the exact same spot. This is not due to geometry reasons. It is only because the time when both instruments collect the radiation is not necessarily identical. The camera takes images every 12 seconds with 1/2656 s integration time while the SMART-Albedometer works with an integration time of 0.5 s and measures almost continuously. As the spatial differences result only from these temporal differences, we removed "spatial" from the sentence and added "(integration times and sampling frequency)".

Page 24602: 6. line 21 – replace "has been" with "was"

Has been changed.

Page 24602: 7. line 24 – replace "to" with "of"

Has been changed.

Page 24602: 8. line 26 – this sentence is not clear "As channel 2 shows the lowest deviation to the SMART-Albedometer data and has the lowest electronic noise, ..."

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To clarify our statement we rephrased the sentence to:

> In the following, results will be shown for channel 2 (530 nm) only. Channel 2 was chosen because it shows the smallest differences (1 %) to the SMART-Albedometer data and has the lowest electronic noise, as discussed in Section 3.3.

Page 24603: 1. line 3 – the function Fglobal with the variables set to zero gives a sense that the measurements were made in a specific direction. Is that the meaning?

We do not understand the question of the reviewer. $F_{\mathrm{glob}}(\theta_0, \varphi_0)$ was already defined before and is the downward global irradiance. Global means it composed of the incoming direct solar radiation $F_{\mathrm{dir}}\theta_0, \varphi_0$ and of the downward diffuse radiation. F_{dir} is a function of solar zenith θ_0 and azimuth angle φ_0. Both angles do not imply that the measurements are made in any direction.

Page 24603: 2. line 25 – check the spelling of Lambertian

Has been changed.

Page 24604: 1. Line 1 and 5 – replace "principle" with "principal"

Has been changed.

Page 24604: 2. line 12 – This sentence is not clear; rephrase: "The maximum ranging outside the camera angle of view (specular reflection for 61..."

We rephrased the sentence to:

The maximum of the sun glint (specular reflection for 61°) ranges outside the camera angle of view and might have even higher values.

Page 24604: 3. line 14 – what is the difference between "hot spot" and "sun glint?" The whole sentence "the hot spot is caused by sun glint at the surface waves, ..." does not have clear meaning!

The reviewer is right, one name for this feature is sufficient. Therefore, we removed "hot spot" from the entire manuscript and do only refer to "sun glint" when discussing the increased "HDRF" of the water surface.

Page 24605: 1. line 6 – the differences between the glory and the fog bow are not clear. The authors need to elaborate.

We changed the use of glory and cloudbow in the revised manuscript. As we did not find any suitable general word defining both optical phenomena, we now used both glory and cloudbow separately for there initial meaning. Glory is the optical phenomenon in the backscatter region. Cloudbow is the optical phenomenon at about 138° scattering angle.

Page 24605: 2. line 13 and 25 – averaging seems to suppress fog bow, but enhances glory features. Why is this?

That is not what our study has shown. The cloudbow becomes more pronounced when increasing the number of images used for averaging. This is clearly visible in both $HDRF$ and $HDRF(\vartheta)$ of Figure 8. With regard to the glory the results have to be analyzed with caution, because the glory was not covered by all images and thus the statistics of the average is not very good. Therefore, we do not want to analyze the glory and added the following sentences to section 4.3:

Although the average of 50 images indicates the backscatter glory for scattering angles larger than 176°, the glory was not perfectly covered on most images, being situated at the edge of the images. Therefore, the glory was not analyzed in the following.

Page 24606: 1. line 4 – replace "avoid" with "ensure."

Has been changed.

Page 24607: 1. line 24 – The sentence: "Mainly the surface wind ..." is too long (run on) and difficult to understand. Also, don't begin sentences with "Mainly" or "Already ..." – as was the case on line 4. These are adverbs and are used to modify verbs.

We rephrased both sentences to:

The cloudbow can be already identified in the mean of 5 images.

The magnitude of the sun glint and the shape of the $BRDF$ are mainly determined by the surface wind speed. Therefore, the parametrization was adjusted to the surface wind speed measured by the drop sonde.

Page 24608: 1. line 1-2 – The sentence "For the pigment concentration ..." mentions default values for pigment concentration and salinity, but it is not clear whether these are the same as the (0.01mgm..3) and (0.1 ppt).

We rephrased the sentence to:

For the pigment concentration and the salinity default values (0.01 mg m^{-3} for

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pigment concentration and 0.1 ppt for salinity), were used.

Page 24608: 2. line 14 – what is "a sun glint hot spot"?

As mentioned above, we removed "hot spot" from the entire manuscript.

Page 24609: 1. line 7 – Are the authors talking about peak reflectance decreasing with wind speed or area of the sunglint decreasing with wind speed?

We rephrased the sentence to:

> Compared to the $HDRF$ using $9\,\mathrm{m\,s^{-1}}$ wind speed it strikes out that the maximum $HDRF$ values, located in the sun glint area, decrease with increasing wind speed.

Page 24609: 2. line 10 – rewrite "the simulated 5ms..1 and 15ms..1 HDRF differ for these scattering angles" as "the simulated HDRF at 5ms..1 and 15ms..1 differ for these scattering angles"

Has been changed.

Page 24609: 3. line 11 - What's the meaning of "wind speed ranges ..."

We rephrased the sentence to:

> The $HDRF$ using a wind speed of $5\,\mathrm{m\,s^{-1}}$ ranges...

Page 24610: 1. line 10 – delete "to" and replace "be" with "been"

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Has been changed.

Page 24610: 2. line 25 – what's the meaning of the statement "values measured higher than calculated"

We rephrased the sentence to:

> ...correspond to cases where the measurements showed higher values than calculated by the simulations.

Page 24611: 1. line 2 – what's the meaning of "which is simulated significantly higher than observed ..."

We rephrased the sentence to:

> In the direction of the Sun these differences are related to the sun glint where the $HDRF$ is enhanced. Here, the simulation did calculate significantly higher $HDRF$ values than observed by the camera.

Page 24611: 2. line 4- You can't start a sentence with "Especially ..."

Has been changed.

Page 24611: 3. line 24 – what's the difference between "surface HDRF" and "cloud HDRF" as implied in the sentence.

We do not understand the problems the reviewer has with "surface $HDRF$" and "cloud $HDRF$. It is common to speak about "surface albedo" and "cloud albedo" which is from the notation not much different to $HDRF$. If the misunderstanding results from

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the sentence structure, we rephrased the sentence to:

> Images measured with a commercial digital single-lens reflex camera have been analyzed to produce the $HDRF$ of different surfaces and clouds.

Page 24612: 1. line 4 – replace "to measure" with "of measuring"

Has been changed.

Page 24612: 2. line 11. The following statement is not clear "obtain a representative HDRF if the observed scene is inhomogeneous"

We rephrased the sentence to:

> However, to obtain a representative $HDRF$, averaging is necessary. Due to the high spatial resolution of the camera, small-scale inhomogeneities of the surface or cloud are resolved by the observations and have to be averaged.

Page 24612: 3. line 12 – replace "at" with "to be"

Has been changed.

Page 24612: 4. line 14- the following sentence is vague "With a sampling frequency of one image per 12 s, this corresponds to sampling times of 10 min and 2 min, respectively"

We rephrased the sentence to:

> With a sampling frequency of one image for each 12 sec, this corresponds to

flight times of 1 min, 2 min, 4 min and 10 min.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 24591, 2011.

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