The manuscript presents a comprehensive description and analysis of the reflection of solar radiation and its transmission back through the atmosphere to a satellite. My detailed comments mainly address clarification, but there may be an error in the equation for the view factor. Once made available, the REDRESS model will be a valuable contribution to snow science.

The paper perhaps understates the ways in which the model could be used to retrieve the snow properties that affect its albedo. The traditional approach presented here postulates that REDRESS could be inverted to retrieve the snow BRDF at the surface (the bottom of the atmosphere), and from those retrievals snow properties could be derived. Methods to estimate snow properties from MODIS (Painter et al., 2009) and Landsat (https://www.usgs.gov/land-resources/nli/landsat/landsat-fractional-snow-covered-area) use this approach.

However, Nolin and Dozier (2000) point out the difficulties this approach poses. Each step in the modeling process from calibration to atmospheric correction to accounting for the terrain introduces uncertainty and possibly error. An alternative, which this paper would nicely support, is instead to focus on attributes of the shape of the spectrum, hence my comments toward the end of the review about examining the spectral angle between the model and the measurements as a way to cut through some of these uncertainties that especially benefit when a continuous spectrum is available (Dozier et al., 2009).

Detailed comments:

Line 40: In this context, what does “surface-atmosphere coupling” mean? Multiple scattering between surface and atmosphere?

Lines 60-75: In this description of the difficulties, you should include surface roughness, on which you have already worked. A rough surface introduces the question of whether calculating the BRDF is necessary, given the uncertainty and subpixel heterogeneity in illumination and viewing angles. Also, for multispectral sensors, the signal convolves the spectral albedo (or BRDF) with the spectrum of the irradiance, which varies with atmospheric properties and the elevation of the surface.

Line 105: My reading of the AART model for snow is that its advantages lie in avoiding Mie scattering calculations. However, the computational burden of Mie scattering can be avoided by lookup tables. Some of the light-absorbing particles, both dust and soot, have traveled long distances and have sub-μm diameters. Calculating their scattering and absorption properties requires very careful programming: Code from Wiscombe (1980) works, whereas code based on Bohren and Huffman (2007) fails under some likely circumstances.

Line 112: “evaluate” not “evaluated”

Line 132: From the description, it appears that the viewing and illumination angles are for a flat surface. Please clarify here.

Lines 149-150: In reference to equations (3) and (4), $T_{dir}\uparrow$ and $T_{dir}\downarrow$ also depend on atmospheric properties.

Line 160, equation (8): Something seems wrong with this equation, and I do not see it in Sirguey (2009). The sentence following the equation defines $H(\phi)$ as a “horizon elevation,” so if the pixel is flat and completely unobstructed, then does $H(\phi) = 0$? But then $V_d = 0$ from equation (8) where it should = 1. Figure 2 in Sirguey (2009) appears to define H downward from zenith, as Dozier and Frew do. Moreover, Dozier and Frew (1990, eq 7b) incorporate slope and aspect into their formulation for V_d, whereas Sirguey (2009) optionally incorporates slope and aspect into the complement of V_d, a "terrain configuration" factor (his equations 1 and 2). You must clarify and reconcile the definition of $H(\phi)$ and the corresponding correct equation for V_d.

Line 165, equation (9): How many iterations are needed? Does the equation converge?

Lines 170-180, equations (11) to (14): What are the assumptions about the shape of the surrounding terrain and its albedo? Dozier and Frew (1990) assume a bowl extending to the horizon in all directions. A viewshed would be more appropriate, but calculating a viewshed for every pixel doesn't take advantage of the fast horizon calculations (Dozier et al., 1981) that enable getting the horizon for every pixel in every direction. Therefore, we make some assumptions, please explain what they are in REDRESS, beyond what Line 184 says. Perhaps consider defining the assumptions *before* you present the equations.

Line 271, section 3.1.3: You should introduce and cite 6S at the beginning of this paragraph, rather than at the end.

Line 314, section 3.1.4: The quality of DEMs varies worldwide. Data from the Shuttle Radar Topography Mission (SRTM, Farr et al., 2007) are available nearly everywhere between 60°N/S. GDEM data (https://earthdata.nasa.gov/learn/articles/new-aster-gdem) from ASTER have slightly lesser coverage but extend to higher latitudes. Finer resolution DEMs from photogrammetry from aircraft or fine-resolution satellite imagery (Shean et al., 2016) are available in some regions. At the finest scale, DEMs from airborne lidar instruments are used in some mountainous areas (Painter et al., 2016; Trujillo et al., 2007), and terrestrial scanning lidar (Deems et al., 2013) and structure-from-motion analyses of imagery from small drones have provided topographic information at very fine scales (Fonstad et al., 2013). The point though is that the differencing operations that are needed to calculate the illumination and viewing geometry introduce noise or, if the calculations are filtered, smooth the calculations. Moreover, at the scales of Sentinel-3, Sentinel-2, Landsat 8, MODIS, and VIIRS, topographic variability occurs within the pixel. Because of these limitations, one must be cautious about the accuracy, precision, and internal heterogeneity of calculations of angles $\tilde{\theta}_{i,v}$ and $\tilde{\phi}_{i,v}$. This section should address those limitations, particularly in how they affect the BRDF estimates.

Line 352: "given that the model considers a fixed SSA value across the scene" seems like an unnecessary constraint.

Around Line 360: How are the atmospheric parameters from CAMS adjusted for surface elevation? I am not familiar with the product, but I hazard each 0.4° cell has an elevation associated with it. From that, you could use some sort of pressure weighting scheme to estimate water vapor, ozone, and aerosol optical depth pixel-by-pixel (Bair et al., 2016; Rittger et al., 2016).

Line 366: The model apparently considers clean snow only. Make this clear upfront. Given that constraint, would the difference between clean and dirty snow wash out some of the details about, for example, multiple reflections?

Line 380, equation (27): Display the equation in a way that makes clear the position of the second term on the right.

Line 399: Statements such as "an excellent agreement between the measured and modelled TOA radiance is observed at both wavelengths" are unsatisfactory. Use the metrics presented in the following paragraph (RSME, bias, etc) to characterize the relationship, rather than an adjective.

Lines 416-421: Refer to my earlier comments (section 3.1.4) about DEMs generally. For the 1 arc sec (~30 m) DEMs from SRTM or GDEM, you're kidding yourself if you invest too heavily in the accuracy of the illumination geometry. While elevation itself is mostly a continuous variable, illumination angle is not. The discussion should separate uncertainties in REDRESS vs. those in the input data.

Lines 440-450: the section heading (4.1.2 Spectral performance) misleads a bit, as just 2 wavelengths are presented. The idea of a spectrometer is that the shape of the spectrum enables analysis; a spectrometer is not just a multispectral sensor with lots of bands. Therefore, a useful addition would include information about how well the model matches the spectrometer. How does the Euclidean norm of the residuals between measurement and model vary across the landscape? What about the spectral angle?

$$Norm = \left\| \overrightarrow{\boldsymbol{L}_{\boldsymbol{mod}}} - \overrightarrow{\boldsymbol{L}_{\boldsymbol{meas}}} \right\|_2$$

$$\cos \measuredangle = \frac{\overrightarrow{\boldsymbol{L}_{\boldsymbol{mod}}} \cdot \overrightarrow{\boldsymbol{L}_{\boldsymbol{meas}}}}{\left\| \overrightarrow{\boldsymbol{L}_{\boldsymbol{mod}}} \right\|_2 \times \left\| \overrightarrow{\boldsymbol{L}_{\boldsymbol{meas}}} \right\|_2}$$

Lines 605-620: The discussion about the quality of the DEM is insightful. One issue not mentioned though is that although the estimation of the illumination and viewing geometry improve as the pixel coarsens in comparison to the resolution of the DEM, the subpixel heterogeneity in the topography becomes more problematic. Perhaps mention that?

Lines 625-630: Indeed the quality of the knowledge about the atmosphere is important, but so is sensor calibration. The paper is already long, so I avoid asking you to address the effect of uncertainty and drift in calibration, but at least mention it.

Table 2. Indicate that Aspect is measured clockwise from North. This is the common convention, although it is inconsistent with a right-hand coordinate system. When I started working on topographic radiation problems, my go-to text was *Physical Climatology* (Sellers, 1965), with Aspect 0° to the South, positive east, negative west (as we use for longitude). Clockwise-from-North is most common, but not universal, hence the need to specify.

Citations in the review

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