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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A multi-angle aerosol optical depth retrieval algorithm for geostationary satellite data over the United States" *by* H. Zhang et al.

Anonymous Referee #2

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In my opinion this paper cannot be published in its current form. The methods are analysis are not very convincing. The whole premise is to improve the AOD product from GOES by improving surface reflectance assumptions of the GASP product using the 2.12 micron BRDF adjustments from MODIS. Seasonal averages are used from MODIS to accomplish this. The MODIS and GOES have very different spectral response functions and calibration is often poor for GOES. Even though vicarious calibrations coefficients are used, I am not sure that these are applicable for aerosols, especially at low to moderate aerosol loadings in moderate surface reflectance regions. I found numerous issues with the paper that I have highlighted below. To

make matters worse, I found the methodology rather convoluted and the paper as a whole needs to be written better. I am sorry that I cannot be more positive at this time.

The abstract of this paper is rather vague and qualitative. At every instance where words and phrases such as - high, more accurate, comparable, good agreement etc - are used, numbers must be provided. Otherwise the abstract cannot be used to glean important information. Also, the word BRDF for channel one for GOES is misleading in the abstract. What angular information can you get from for a pixel from GOES at a given instance? The GASP assumes Lambertian. The references for aerosol forcing and air quality/human health are rather old. Much has been done since Charlson and Kiehl's work. Plus it is not only the TOA that is important.

Page 12521, The writing is very sloppy and I am surprised at the lack of specificity in the writing (GASP retrievals) given that one of the coauthors is responsible for the current GASP algorithm. Case in point: AOD at the second darkest day is 0.02? If you are retrieving AOD then why assume this. I know what this sentence implies but it needs to be written carefully so the reader is not confused. The GOES uncertainty discussion is generic. You need to back that up with numbers. Take a look at either Chu et al's paper or Zhang et al 's paper in JGR that talks about uncertainties in calibration, surface reflectance and aerosol models – quantitatively! In 28 days the color of vegetation changes so much that the surface reflectance assumption is not good? Given the calibration uncertainties of GOES and the broadband of the channel, I am not sure that this argument is valid. If you want to say that, then we need proof (not just from one site with no mention of how you achieved that reflectance). The whole premise is to develop a new surface reflectance algorithm and therefore careful wording is necessary in the Introduction and elsewhere in the manuscript.

Remember that you make a major assumption that the polar orbiting 2.12 MODIS reflectance – seasonal average – represents the conditions that GOES would see. You are using that to compare against GOES visible channels, given that the spectral widths and the calibration are all completely different between sensors (GOES and MODIS).

ACPD

11, C5816–C5820, 2011

Data. Since small changes in surface reflectance for low to moderate aerosol loadings could cause large errors in retrieved AOD, the authors must not only show a web site reference for GOES calibration but must quantify the uncertainties. How good is this vicarious calibration FOR AEROSOLS? GASP provides values at 550 nm but the measurement is between 0.52 and 0.72. What are the uncertainties here?

MODIS data. How good is the cloud clearing and aerosol identification in these products? MCD43D19, MCD43D20, and MCD43D21 since this is critical for the development of this paper.

page 12524 : these types of sentences must be tightened up throughout the paper 'derive the aerosol properties with a 10 km resolution (Levy et al., 2007)'. What resolution? Nadir? It is km squared not km.

Aerosol optical depth retrieval algorithm. The first paragraph makes a dramatic statement about one site – GSFC. What season and what was this highly accurate source for this 28 day change. What data set was used? Remember that most satellite algorithms use a bi-weekly type of composite anyways and therefore daily changes are hard to measure. Plus GASP uses 28 day composites for each pixel for each hour. How much can SZA change for a 28 day period for one pixel that is 1 to 4 km? I am not convinced of the arguments in paragraph 1.

You cannot assume uniformity in 24 km grids based on Anderson's work alone. That was rather regional. You need to assess your data including air mass wind speeds to find the best spatio-teomporal fit to the data.

At the risk of sounding overly negative in this review, I do like the image coregistration part of the analysis.

I completely disagree with his approach» "Since the resolutions of two IR channels are 4 km, we break each pixel into 4×4 pixels with 1 km in size and assign each of the new pixels with the same value as the original one. With such arrangement, CLAVR

ACPD

11, C5816–C5820, 2011

algorithm can be applied at 1km resolution" » You are creating a finer spatial resolution data from a coarser spatial resolution data set simply to run an algorithm at 1 km. If we were do this then we can convert all the MODIS 1km pixels to 250 km which is meaningless.

In addition, we also apply the following criterion to determine cloud pixels that fail to be masked in CLAVR algorithm »> How do you know this, by visual inspection? What criteria?

if the standard deviation of a 3×3 box surrounding a pixel in channel 1 TOA reflectance is greater than 0.015, the pixel is also marked as cloudy, which is similar to the MODIS cloud mask algorithm by Martins et al. (2002) » Is this applicable for your domain? Martins et al applied this OVER OCEAN and your study is over land. Surface textures are completely different.

Page 12527, Line 13 – you are calling GOES surface reflectance as GOES BRDF. Why?

Page 12527, the second paragraph starting with 'after cloud masking', is painfully convoluted and I am not sure that I understand this at all. May be the other reviewers did. What is time sequence? How many time steps? Where from? If all you are doing is looking for differences in GOES and MODIS reflectance values for a certain pixel that comes from two different methods, this technique is not at all robust and highly questionable. We have not improved anything for GOES retrievals if we are using MODIS 2.12 micron values on a seasonal basis! Figure 3 did not help me any. You can probably use actual pixel values to demonstrate your point.

Section 4.2. The authors do not appear to believe in their cloud mask since they continue to remove data – again – 'we remove the pixels adjacent to cloud, require more than 10 effective pixels in the 25 pixels, and require standard deviation of AOD in the 5×5 box is less than 0.2'. Now notice that for GASP they use the Prados et al filters that are not the same as the filter above?

ACPD

11, C5816–C5820, 2011

In summary, I believe that the paper cannot be published in ACP in its current form and requires more thought and analysis. The fundamental question to ask is : Given the poor quality of the GOES sensor (the current one) can we really extract any more aerosol information? It has very few channels, lacks onboard calibration, spectral widths are large, etc.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12519, 2011.