Remote sensing of aerosols with small satellites in formation flight
By K. Knobelspiesse and S. Nag
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This article assesses the potential for aerosol remote sensing from a formation flight of small satellites via comparisons with a multi-angle single platform (MISR-like) satellite. The authors present their case that a formation of small satellites, each with a single view angle, could perform as well for aerosol remote sensing as a single platform, multi-angle, satellite with the added bonus that small satellites in formation could be replaced as they age at lower overall cost. They use a combined systems engineering approach and information content analysis to support their conclusions.

The authors performed a large amount of simulations for this study: they tested "16 different observation configurations each with more than a hundred orbital geometries, for six different scenes, for a total of nearly 10,000 individual assessments" [line 20, pg 10]. Due to limitations and assumptions in the information content analysis, the authors state their assessment is "ideal for relative comparisons" of information content between a formation of single satellites (each with a single view angle) and a multi-angle satellite. The authors also state that their primary focus is on measurement geometry for determining the portion of parameters that are assumed to be representing aerosol (line 3, pg 4).

In light of the intended impact of the paper, I focus my comments on the orbital geometry findings and the information content assessments. However, it's clear from the text and the results that aerosol remote sensing is very challenging and the retrievals are underdetermined. I admit it's possible that I've missed important and subtle concepts regarding the physics of aerosol remote sensing or misconstrued them.

Comments on the Systems Engineering Aspect

Here, I focus on the pointing control of the formation flight small satellites because I am not finding a clear relation between the orbital geometry metrics presented in the text and how those uncertainties propagated into the information content analysis. I'm no expert in orbital mechanics, so perhaps I missed it, or did not understand what I read.

Attitude vs Position: The authors make the point that the relative positions of the formation flight satellites are less important than their relative attitudes. I believe this makes sense because for aerosol remote sensing, what you are aiming to achieve is a measurement of the spectrum of reflected light at enough view angles – over the same patch of surface area – to observe the BRDF or BPDF function. Thus, the ability of the satellites to be pointed at the same patch of surface area is more important than position control, although the satellites would have to be near enough to each other at a given instance of time to be able to point at the same surface patch, of course. Conversely, a situation where very tight position control of formation flight satellites would be more important could be where the apertures of individual instrument are combined (in processing analysis) to make a "pseudo" large aperture for viewing very distant objects.

In light of the above, could you explain how the RAAN and Mean Anomaly uncertainties presented in Table 1 that were propagated in the systems engineering model into root mean square (RMS) differences between predicted BRDF and the "truth" BRDF provide a metric of pointing control? My current interpretation is that the RAAN and Mean anomaly uncertainties are rather supporting the case that the small sats can be controlled in close enough formation to be able to point at the same surface patch, not that they actually did point at the same surface patch.

I did note some capabilities listed on page 12 – "adjustments to maintain the formation can guarantee > 80% overlap between ground spots for 0.5 degrees of pointing control and 2 km GPS error." It's this "agility" aspect, and its associated uncertainty of 0.5 degrees of pointing control, that I'm not finding was propagated into the BRDF/BPDF (p6). Was it and how so (and how did the implementation in the analysis differ for the formation flight satellites as opposed to the multi-angle sensor)? Are the capabilities listed in the paper based on measurement requirements for aerosol remote sensing?

Observing geometries: I observe some characteristics in Figure 2 that were not discussed in the paper. Firstly, when I compare the simulated geometries of the formation flight satellites (left plot) with the multi-angle satellite (right plot), I notice that the formation flight satellites did not cover the negative view zenith angles beyond approximately -60 degrees. Does this meet BRDF measurement requirements for aerosol remote sensing (referred to on pg 5), and what are these measurement requirements (list in paper or provide citation)? In the subsequent analysis, how does that absence of observations factor into the RMS differences in observed (simulated) BRDF relative to the "truth" airborne CAR measurements?

Secondly, I also notice that the flight formation architectures have viewing geometries under overhead sun conditions (SZA = 0), whereas the multi-angle satellite configuration did not. Are there benefits for aerosol remote sensing that you can identify that would be aided by overhead sun angles? I could imagine that there may be benefits and limitations. For example, measured signals would be larger, but perhaps the surface component of that signal becomes even more dominant than the aerosol component?

Finally, because the study's strength is in relative comparisons between formation flight architectures and a multi-angle sensor, it's fair to ask if these differences were incorporated into the subsequent information content assessments, or if the analysis was performed such that the set of viewing geometries and solar zenith angles were first pre-filtered to those common to both small satellites and multi-angle sensor prior to doing the information content assessments. The text, however, does make clear that spectral sensitivity and measurement uncertainty assumptions have been kept the same for both small satellites and multi-angle sensor measurement error assumptions.

Regarding Figure 3, I don't understand the difference between "science performance" and "science error for engineering design"? Also, it's a small thing, but what is the yellow symbol below the BRDF plot –is it the sun?

Comments on the Information Content Analysis

The authors utilized a statistical technique derived from general inverse theory commonly called optimal estimation [Rodgers, 2000] in their analysis. This approach "connects measurements to the expected retrieval success of geophysically relevant parameters" (pg 6). Optimal estimation describes the mean and standard deviation of retrieved parameters, based on the information provided in a measurement, apriori information and a model, when one makes the assumption that the forward model relating the parameters and measurements is linear and the measurements themselves are linear (Gaussian) distributed. The authors acknowledge that the forward model for aerosols is highly nonlinear, but that for small perturbations of parameters in the forward model, the sensitivity of the modeled values to that parameter perturbation will be linear (p9). Hence, to build up information content for a broad space of parameter ranges, a number of sensitivity analysis of modeled values to linear parameter perturbations must be created for different locations in the parameter spaces. The "retrieved" parameters are defined by a mapping from a volume in the measurement space to a volume in the parameter space. The forward model, representing aerosol physics as best known, provides the mapping between measurement space 'y' and parameter space 'x'- for the constructed sensitivities (Jacobians) of the measurements to the parameters.

It's too simplistic to represent the forward model as 'F(x) = y' [line 4, pg 7] and in Figure 4. The forward model will also require some assumed model inputs, some (all?) of which you list in Table 2 (the non-bolded values) that will have their own set of uncertainties. Let's call the necessary, but assumed, model inputs 'b'. So, the mapping from observation space, 'y', to parameter space 'x' will also rely on the accuracy of these 'b' values (line 8-9, p7), and how well natural variability in these 'b' values is captured.

I'm missing discussion of the physics of aerosol retrievals and what parameters are retrieved as opposed to what parameters are derived from the retrievals. I initially read Table 2 such that bolded variables were the retrieved parameters. However, discussion of AOT (centered around Equation 4) suggests that AOT is actually a derived parameter.

Then, regarding Equation 4, while I do understand that it has been applied in other studies under presumptions that variables can be differentiated I feel I should point out that it's a weakness in the information content analysis. I'm not trying to lessen your efforts or look for a citation, but I've recently begun grappling with those kinds of challenges and made some advances in quantifying the information that is shared between physical variables (cloud optical thickness and particle size in cloud remote sensing) that wouldn't need to rely on those presumptions and I wanted to point it out to you for your interest. Here I'm speaking of mutual information content metrics that I think would hold promise for aerosol remote sensing challenges (see for example, Section 6.1 in Coddington et al., [2017], J. Geophys. Res. Atmos., 122, 8079–8100).

The assumption that Sa is diagonal (i.e. no a priori correlation between parameters) (Line 19, pg 17) would seem to be weakly supported, given your results in Figure 10. Is this a common assumption in aerosol information content analysis? The same assumption for Se (measurement uncertainty) seems reasonable.

I'm missing a clear definition of how you use the term "scene" (ex. Line 14, pg 9). Do you mean different surface types, or do you mean a variety of different atmospheric conditions for the same surface? If the latter, would "state space" be a better word choice than "scene"? Based on your decision, you might need to do a word search through the paper to find instances of "scene". Then a follow up question, regarding the iterative computation of

Equation 2 for the different scenes: is the a priori error covariance matrix (Sa) held fixed for all scenes? Based on Table 2, I believe that is the case, however, the Jacobian (K) would change.

It's probably a small thing, but I'm actually not sure what is meant by "perfect algorithm ability to converge to the best retrieval from the observations" (Line 16, pg 10). Is it just that you are referring to a retrieval with multiple local minima and you're looking for the "best" one (where "best" is ideally the correct solution).

Lines 23-29, pg 15: You are performing an information content assessment without using an aerosol model to constrain the parameter space because you need "realistically retrievable conditions". Do aerosol models diverge so widely that non-physical or even just different regions of the parameter space would result if aerosol models were utilized with the different formation configurations (even when "scene" conditions were held fixed)? If so, then I guess a correct interpretation is that the imposed constraints listed in Table 2 are more extreme than those that would come from aerosol models. A follow up question would then be: How significant are any of the results in Figures 6-10, for a *stand-alone* aerosol information content study as opposed to a constrained, restricted analysis, of comparative orbital geometry impacts?

I also have some questions about the impact of the results in Figures 6-8. It's mentioned several times throughout the results section that the degrees of freedom of signal gradually increases with number of viewing satellite (for the formation flight) until the 9-satellite configuration and the multi-angle satellite with 9 view angles have nearly indistinguishable mean values. A similar argument is given for the results of Figure 7 and Figure 8, where here a reduction in uncertainty in AOT and fine mode effective radius is shown with increasing number of satellites in the configuration until mean values achieved match those of a multi-angle satellite sensor. What I'm missing in the discussion is that the vertical spread of the results (indicating the variability due to observation geometry) is often times larger for the flight-formation architectures than the multi-angle single platform satellite. The text focuses on the mean values, but does not the larger variability due to observation geometry suggest greater potential for larger uncertainties in global aerosol properties and also that the "time to detect" a trend in aerosol properties would increase? For example, if aerosol models are built using accumulated statistics from global aerosol retrievals, it would seem possible that these models become more uncertain. It would help the interpretation of these figures if you could provide a way to understand how significant the changes in DFS, AOT, or aerosol fine mode effective radius are. In some cases, the change with formation architecture is rather subtle. Could you, for example, extract an answer from your analysis (or compute a result for one representative case) on how the variability in vertical spread of these results would change if an aerosol model was used to constrain the state space as opposed to the imposed constraints in Table 2? Would the vertical variability change (increase) and by such a degree that the changes due to formation architecture differences become non-important?

In this section, I've identified some concerns with the information content analysis. However, I admit that for "relative comparisons", which is the stated goal of this study, much of the fallout from these concerns is diminished because the analysis as described was performed in the same way to every orbital architecture, whether a formation of small satellites or a multi-angle platform.

Comments on the Conclusion Section

The paper is long and detailed so I think what would be really helpful would be to summarize in the conclusions the aspects of the study that weren't covered. Additionally, I identified above some aspects of the results that weren't addressed in the text; these should also be summarized in the conclusion. Also, the conclusions as currently written do not provide a path forward for the next analysis nor identify potential studies for future analysis.

For example:

a) You state the aerosol remote sensing performed equally well with formation flight small satellites.
- Were new science capabilities using small satellites identified?
- Are their potential caveats for this statement (for example, time to detect aerosol property trends if geometry variability induces larger variability in retrieved aerosol products)?

b) The formation flight satellites did not cover the negative view zenith angles as well. The formation flight satellites had more instances of viewing geometries under direct overhead sun.
- What are the pros/cons for aerosol remote sensing?

c) Were uncertainties in pointing considered for the formation flight satellites or multi-angle satellite?
- How does the cumulative effect of pointing uncertainties compare for the formation flight satellites as opposed to the multi-angle satellite?

d) The information content analysis used to determine geophysical parameter retrieval capability did not include an aerosol model for constraining the retrieval parameter space. Instead, imposed constraints were placed that were tighter than those that would have been obtained by an aerosol model.
- Should the readers pay more attention to the absolute values (of DFS, AOT uncertainty, etc.) or the relative change in these values as a function of formation architecture?

e) I'm missing in the conclusion how you would take this kind of analysis to the next step.
- Can you list a couple of the many aspects of such observations left to explore.
- Can they be performed with your info content technique, or is an OSSE required?

Minor comments.

Line 11, pg 1: variety *of* view zenith (missing 'of').

Line 25, pg 10: one to many "other"

Line 33, pg 13: Remer et al., 2006 should be in brackets.