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Interactive comment on "How big is an OMI pixel?" *by* Martin de Graaf et al.

Anonymous Referee #3

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General remarks: This paper tackles an interesting problem: the on-orbit estimation of the size and shape of the point-spread function (PSF) of an OMI field of view (pixel) using collocated data from MODIS, which passes over a ground scene between 8 and 16.5 minutes before OMI does, and measures with a much smaller field-of-view. The idea is a good one.

The authors choose to follow the tradition of approximating the sensitivity function within the FoV by a two-dimensional super-Gaussian function. One should bear in mind that this is a parameterized approximation, and, as such, may fail to deliver a good representation of the actual FoV sensitivity function. It may in fact not be suitable. Rather, it tells one the spatial extent of the bulk of the FoV's integrated sensitivity function and an idea of how "soft" the edges are. Certainly, retrieving values near or less than 1 for the n parameter calls into question either (a) the suitability of this function as a model, or (b) the suitability of the data set to estimate the parameter. The super-

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Gaussian function cannot well represent a narrow FoV with a flat top and relatively soft edges, because the extent of its flat top is tied to both its FWHM and its "softness" parameter n. The function chosen, then, may be too highly constrained. The authors have chosen to use a sub-family of functions in which, while the shape is characterised by two width parameters (one along-track, one across-track), the parameter n is constrained to be the same with respect to both these directions. At various points in the discussion, the authors "freeze" the width parameters to be equal to the values they have in the publicly available OMPIXCOR data product, and attempt to optimize the n parameter alone without regard to the sensitivity of their chosen goodness-of-fit statistic, the Pearson correlation coefficient, to the frozen parameters, and how they might move the optimized n value.

In my specific comments below, I note a number of statistical issues that the work has not addressed. Most importantly, no attempt has been made to characterize the uncertainty of the retrieved parameters. This is a serious drawback when comparing results coming from different scenes and different data selection schemes. In this paper, the authors observe that for different scenes, their estimated parameters are quite different. In fact, they ultimately abandon the task of estimating the along-track and cross-track widths in favor of simply accepting the OMPIXCOR FOV75 values, because their data and analysis cannot be used to support a different answer. That is not to say, though that, the OMPIXCOR values are proven by the data. That, then, is the authors' answer to the question posed by the title. The rest is a question of how soft are the sides of the FoVs. The answer to that question is complicated: Figure 10 shows that the answer is scan position dependent (which was not assumed in the calculations up to that point in the paper), and subject to large uncertainty, even when using all available scenes to determine the values of n.

Peer review questions from AMT:

Does the paper address relevant scientific questions within the scope of AMT? – YES Does the paper present novel concepts, ideas, tools, or data? – YES – Are substantial conclusions reached? –NO– Are the scientific methods and assumptions valid and clearly outlined? NOT AS MUCH AS THEY SHOULD BE Are the results sufficient to support the interpretations and conclusions? – NO – Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? – YES – Do the authors give proper credit to related work and clearly indicate their own new/original contribution? – YES – Does the title clearly reflect the contents of the paper? –YES– Does the abstract provide a concise and complete summary? –YES– Is the overall presentation well structured and clear? –YES– Is the language fluent and precise? –YES– Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? –EXCEPT AS NOTED, YES– Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? –NO– Are the number and quality of references appropriate? –YES– Is the amount and quality of supplementary material appropriate? –NOT APPLICABLE–

Specific comments:

l. 3: This is awkwardly stated. The "shape" that is not quadrangular is spatial. The projection of a notional field-of-view on the Earth's surface is often thought of as being a quadrangle that we could draw out in latitude-longitude space, for example. What is "Gaussian-shaped" is a section through the sensitivity function of the instrument FoV, as a function of latitude and longitude.

Further, the sentence implies that the "Gaussian-shaped" nature is due to "light from neighbouring pixels enter[ing] the FoV. That is also not an accurate statement: In the absence of measurements made in the adjacent FoVs, a chosen FoV's sensitivity function would remain the same.

The term "pixel" should be either defined or, if it's used synonymously with "FoV," abandoned in favour of the latter, particularly in the Abstract, where it may cause undue confusion.

l. 11: What is meant by "optimal OMI PSF?" The PSF is a physical quantity. I believe "optimal" probably refers to the imposition of a parameterized model for the PSF, and the determination of an optimal set of parameters through the procedure then sketched.

l. 13: Omit "semi-official."

l. 14: I don't think the word "fix" is correct. Do you mean "fits?"

l. 15: I realize that the paper goes on to describe "super-Gaussian" functions, but you have just characterized the same function as "Gaussian."

l. 33: It might help a reader if you mention that these instruments are all in polar, sun-synchronous orbit, so that "global coverage" makes sense.

l. 52: In the previous paragraph, at l. 37, you said the radiation was split into a UV and VIS channel. Here, you refer to three channels. Also, it is not clear what you mean by "spatial sampling distance."

l. 53: What does the number 115.1 deg refer to? Is this actually the field of regard (for the whole swath)? Does it only go from the leftmost to the rightmost FoV centers?

l. 59: The physics behind the shape of the sensitivity function is Fraunhofer diffraction. The classical solution for a circular aperture is an Airy function (with wavelength as a parameter). The use of the relatively simple Gaussian function is as an approximation to the Airy function. The asymmetry of the OMI instrument aperture (along-track and across-track) gives a more complicated geometry, but the diffraction physics is the same. Not saying this suggests that the choice of a Gaussian function is arbitrary.

l. 60: I would suggest removing the word "normal."

l. 64: The satellite motion is not a "function," so it is confusing to say that the Gaussian is convolved with it. It is convolved with a boxcar function whose width is the $\sim$13 km the subsatellite point moves during the 2 second exposure.

l. 81 (and elswhere): Should read "FOV75"

l. 87: See comment at l. 64.

l. 89: "adjacent swaths" is a little confusing, since the word "swath" was used before (e.g., l. 36) to refer to the entire field of regard. (And, of course, there is the confounding use of the word in the context of the data archive.) Perhaps, "successive scans?"

l. 145 and Figure 3: You say sigma is the standard deviation. Standard deviation of what? Is it the RMS deviation of the points from the model line, in the vertical direction? Why would that be preferred to the horizontal direction? That is, your least-squares linear fit to the data is based on the assumption that the OMI reflectances are error-free. Why do you make that assumption? On the right-hand panel of the figure, you highlight the "...points [that] have the largest sigma." What does that mean? Does it mean the S.D. of the reflectances of the MODIS pixels that are collocated to a single OMI FoV? Or the largest deviation of the points' ordinates from the model, along the vertical direction? Or something else?

If there is no discussion of the numerical values characterizing the least-squares linear fit, then why repeat the values in this paragraph, if they are all tabulated in the figure?

l. 152: Why would you assume the same n for the x and y directions in (1)? Is there empirical evidence to support this? If the across-track n turns out to be large, along with w, giving a wider, flatter top in this direction than in the other, the along-track n could still be $\sim$2, because it is dominated more by diffraction, and less by spacecraft motion. I think this is an important point, and the decision to restrict the functional form in this way deserves solid justification, either in terms of the optical physics or in terms of the empirical data.

l. 167 and Figure 5: The discussion in the text more or less replicates the figure caption. The figure caption would be easier to follow if it used the panel leters (a, b, ..., f), instead of just saying "reading order."

Which OMI row is represented in this figure? The axis orientation changes, FoV by

FoV, as you go across the OMI swath, so the way the MODIS pixels pack into the OMI FoV is different for different FoVs. In Figure 5, you show only 12-15 MODIS pixels along an OMI pixel in the along-track direction. This may be different at wide-of-nadir FoVs, not only because the FoVs are a little larger, but also because the MODIS grid direction cuts through the OMI FoV's x and y directions at a different angle.

l. 176 and Figure 6: If I understand the Figure 6 analyses correctly, you are scaling all the data (for all the different OMI FoVs) to the OMPIXCOR dimensions. If that is so, then your effort is to find optimal wx and wy values that scale *all* OMI FoV positions. I don't know if that is justified. Furthermore, the sizes different OMI FoVs will overlap with different numbers of MODIS pixels, so you may have different uncertainties in r or SD for different FoVs, which would, in turn, bias your optimal wx and wy estimates.

l. 189: Concerning the comparison of the values of r, you do not provide estimates of the uncertainties in your r-values, so can you say that the difference between the r of 0.9974 for the optimized PSF shape and 0.954 for the quadrangle (OMPIXCOR) is significant at some level of confidence?

It appears that your blue curve asymptotes to ~ 0.9972 when n gets large. I am even more skeptical of the implied claim that the difference between 0.9972 and 0.9974 is significant. These are very small differences in a statistic that may be sensitive to sampling artifacts, to the fact that you are using a particular functional form, and the fact that you are constraining the n value to be the same in the along-track and cross-track directions.

Could I ask that you mention, in the caption of Figure 6, that the horizontal scales for all curves (i.e., top and bottom axes) are logarithmic?

l. 192: As I noted before, at l. 145, I do not know how you define SD. I suspect it is the RMS deviation from the best-fit line (perhaps with N-1 in the denominator rather than N, which should not matter much, given your large N). If you are scaling all the FoVs together (see at l. 176), then you should realize different numbers of MODIS pixels within FoVs at different scan positions. Did you weight the data accordingly (in Figure 3, and its least squares solution)?

If you are using the ordinary least-squares formula (whether weighted or not), you are implicitly assuming that all of your uncertainty is in the MODIS data.

l. 193: I would note, looking at Figure 6, that the maxima in all the curves are pretty broad. That means that your "optimal" values are not very well defined. What would you claim are the uncertainties in the optimized parameters. How are they correlated?

Sec. 3.1: The discussion in this section is problematic. Most of the problems stem from things I have remarked on in the foregoing discussion. The substance of this section is really the difficulty of pinning down values of the optimized parameters. The analysis would be greatly helped by computing uncertainties, including uncertainties due to sampling: How much of the difference you see in the different match-up cases is due to sampling, how much is due to the flatness of the goodness-of-fit functions (r or SD), and how much is due to the way you have chosen to parameterize your PSF model (including the specific choice of a goodness-of-fit metric)?

If you want to claim to have an answer to the question proposed by the paper's title, then indeed the parameter values you obtain should not vary from one case to the next, by more than a certain physically reasonable range.

By the way, in your two Sahara cases, the over-ocean portion of the sample may be quite important: You do not claim to eliminate clouds from either one, and there appear to be large differences between their over-ocean cloud field. The clear scenes over the ocean should contribute very little to the determined slope, since you expect the values there to populate a very limited range in the scatter plots in Figure 3.

You may be correct to attribute the difference between Figure 1 and Figure 9 (and the difficulty, in the latter case, of getting a physically reasonable set of parameters) to the time difference. However, it may simply be that the different scenes have sufficiently

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different distributions in their reflectances, and the uncalculated parameter uncertainties are so large that this accounts for a good bit of the difference. I think this may be why you see a notably smaller value of r in the Figure 9 case. In essence, finding a value for n that is near or below 1 challenges the suitability of the super-Gaussian function in (1) to describe the OMI PSF.

Sec. 3.2: To continue my comments from the previous section, you have fixed on the idea that it is the time interval between measurements (8 minutes in Figure 1; 14 to 16.5 min in Figure 9) explains the differences over the broken cloud regions. Certainly, those clouds can evolve significantly on those time scales. But that is not the only possible explanation. For example, MODIS viewing zenith angles (VZA) can be much smaller than those at OMI's off-nadir scenes, and OMI will, in these cases, see a much more homogeneous scene. I would like to suggest that the instantaneous structure of the reflectance field, which is naturally different from one scene to another, may be at least as important as the evolution of the structure from one overpass to the other.

Sec. 3.3: Polarisation may be sufficient to explain the dependence on VZA (i.e., scan position), but it's not necessary. It may be that the diffraction is different at the edges of the far-off-nadir FoVs to those at the edges fo the near-nadir ones.

Figure 10 appears to make my case about the uncertainties. First, by constraining the wx and wy values to the OMPIXCOR (or any) values, you reduce the problem to a single degree of freedom, but if the wx and wy values are not correct, and r is sensitive to them, you have a problem: There is no reasonable physical interpretation of the optimized n value. Furthermore, if you look at the wiggles in the red line in Figure 10, this strongly suggests not erratic behaviour in the instrument or its characterisation vis-a-vis wx and wy, but rather that n is insufficiently well determined, and your scene sampling selects from a parent distribution of n values, and that is different from one scan position to the next.

l. 279: An alternative explanation is that the super-Gaussian may not be an adequate functional form to describe the FoV PSF. It may also be, as I said before, that an additional problem lies in how you have constrained the wx and wy values.

l. 337: intercompare is one word, no hyphen.

l. 339: This is multiplication, not convolution

l. 344: Set off "as much as possible" with commas.

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