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Interactive comment on “Mirror contamination in space I: approach” by J. M. Krijger et al.

Anonymous Referee #1

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The authors have made a very useful review of how to handle in a consistent way the Mueller matrix description of a multi-mirror system including the necessary coordinate transformations. They also have applied existing formulas for mirror degradation to the on-ground calibration of the SCIAMACHY instrument, and thereby have shown the validity of their approach. They derive a simple and useful model for the polarisation properties of grounded aluminium diffusers.

However, the authors make some bold statements about the general applicability of their method that do not follow from the paper. These statements are in my opinion not warranted. Also it is not clear what this paper has to do with mirror contamination in space, since only application of the method on-ground is discussed, using calibration measurements that are not available in space.

The core of the problem, how to derive the contaminant properties in space, is not

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discussed. Perhaps part II of the series will make this clearer, but having only this paper, the title does not cover the paper's content.

Specific comments:

p.1214, line 12 "The method can be applied to any UV-VIS satellite instrument, and in any orbit, both low and geostationary" This phrase is repeated at least 3 times in the paper without any proof. But the method is at most applicable to instruments where a (scan) mirror is the most degrading optical component. This excludes e.g. the GOME-2 instrument, where the main degradation is untypical of mirror contamination. And it remains unclear why the orbit is mentioned at all (one might just as well mention that the method is independent of the optical engineer's hair color). Besides, the model assumes bare aluminium with Al₂O₃ overcoat. What if on top of the Al₂O₃ (which forms very quickly) a protective coating is applied? Then a 4-layer model would be needed which is not described. I suggest to remove the reference to orbit, and to make the applicability more precise.

p.1215 line 6 "due to their technical nature [..]" Really? perhaps the reason is that there is a lot of guesswork involved, but little quantifiable hard scientific evidence. Please remove the "due to" unless you are really sure.

p.1222: please mention that formulas 14-19 are taken from the literature (give reference)

p.1228 "likely due to the way they deal with the aluminum oxide layer" Isn't there also a dependence on how Aluminium films are formed, e.g. grain size (the SCIAMACHY mirrors are not bulk Aluminium, see also comment from p.1230)

p.1230 line 12: what is the justification for light oil?? Isn't this just the result of trial and error where optical constants of light oil appear to fit quite nicely and oil seem plausible too? But there might be other compounds as well that fit the measurement but weren't investigated. Please comment, and replace "is needed" by something like "would be

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consistent"

p1230. line 26: why is it more likely that the measurements are wrong?? The authors have made several assumptions that may be wrong instead: 1) SCIAMACHY mirrors are not made of Aluminium. They are polished nickel-plated mirrors with Aluminium overcoat. Is this Al coating so thick that the underlying material is 100% shielded to electro-magnetic radiation? Does the grain size of the deposited Aluminium play a role? 2) it is assumed that both mirrors have identical thickness of Al₂O₃ 3) it is assumed that both mirrors have identical thick oil contamination (wouldn't that be a coincidence?)

p. 1232 line 12 "can be easily derived" I do not see anything easy. It seems to me that for 4 layers there are phase shifts where 12 interferes with 23, 34, and 45, and where 23 itself interferes with 34 and 45. If the authors have a description of reflectance of the 4-layer case, that would be highly valuable as it may be applied to coated mirrors as well, not just to "bare" mirrors as in SCIAMACHY. Please add a recipe in Section 2.4 on how the model may be expanded to 4 layers if you wish to keep that, and further similar, statements.

p.1232 line 14 "is of interest [...] only wavelength-dependent degradation". Why is this model of interest for wavelength-dependent degradation without scan angle dependence? That may be derived directly from a transmission measurement (e.g. on Sun or internal lamp) and does not need a model.

p.1232 line 15 "vice versa" = scan angle degradation without wavelength dependence?? That doesn't give information to derive your parameters.

Section 2.2, Section 5: Nowhere it is mentioned how to derive the optical constants in space, nor on-ground. But this is exactly the core of the problem. For the on-ground measurements presented here, a "light oil" mysteriously pops up. And there were many dedicated on-ground polarisation measurements to justify that choice. But what is done in space where such measurements are not available? Perhaps that will

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be explained in paper II, III, .. ?? But why then is this paper titled "Approach" when the most important approach is missing, namely how to derive the optical constants in space. And how to derive these "for any UV-VIS instrument for any orbit" as is claimed. I do not see at all how this claim is justified. If the authors will justify this in a following paper they can claim that in the follow-up paper. But it does not follow in any way from the current paper.

I recommend to choose a more apt title for this paper I. And the authors should explain that the derivation of optical constants is a major issue, if not THE major issue (which will be discussed in paper II??).

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1213, 2014.

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