

## ***Interactive comment on “Optical Characterization of Three Reference Dobsons in the ATMOZ Project – Verification of G. M. B. Dobson’s Original Specifications” by Ulf Köhler et al.***

### **Anonymous Referee #1**

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#### General comments

This manuscript reports on very important work that has been long-awaited by the Dobson community. Accurate characterisation of the optical properties of the Dobson instrument by specialist metrological laboratories offers the potential to improve the Dobson algorithm and more accurately determine the uncertainty of ozone measurements made by Dobsons and the consistency of the global network. It is certainly well within the scope of AMT.

I see the two key questions the community would be hoping such an investigation could answer as being:

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- What difference to total ozone values do the measured departures from the assumed slit functions make? (Is the difference negligible?)

- How different are individual Dobsons to each other in this regard? Could an overall correction be applied across the whole network, or is there too much variation?

I note that a significant portion of this manuscript has already appeared in an article in the ATMOZ edition of UV News (edition 11 2016) by a subset of five of the current authors. There, the focus was more on the details of the laboratory measurement.

This paper tries to have a somewhat different focus by concentrating on the impact of the laboratory characterisation on ozone measurements and thereby on the Dobson network.

Therefore, my major recommendation is that section 3.2 "Implications of the new effective absorption coefficients" be expanded and made much clearer. The values of the changes given in Tables 3 and 4 are large enough to be worthy of discussion (most notably 0.98% for D074 AD wavelengths calculated with Serdyuchenko), as well the differences between the three instruments (eg CD corrections in Table 3 differ from each other by ~1%). If the three reference instruments differ by the amounts given, wouldn't field instruments be expected to be at least this divergent?

Secondly, in a number of places the statements should be made more precise and quantitative.

Thirdly, there were a large number of small mistakes in English in the text, which could have been removed with more careful proof-reading.

Fourthly, I don't think the paper is clear enough whether it is actually Dobson's manuals that are being referred to or in fact later work (such as Komhyr's), for example in Table 3 the coefficients are called "Dobson/Komhyr" but these are not the values that appear in Dobson 1957. The authors should be accurate about which points go back to Dobson and which do not.

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## Specific comments

### Page 1

16 The wording within the parentheses is awkward to read, especially "primary=world standard"

18 The name of the ATMOZ project is not given correctly

19-23 These sentences should be made more quantitative – the abstract should include the key numbers

25 I think this statement is very misleading because the difference in AD is up to 0.98% with Bremen cross-sections.

28 I can't find any example given later on of how the new information could be used to explain unusual results in field calibrations – this seems to be just speculation?

28 More importantly, if the statement in the abstract is correct, does this imply that field instruments might be significantly different from the reference Dobsons if they were to also be characterised? This should be discussed explicitly. The implication is that field instruments might be quite different and that is why the TuPS is needed. I would like to see this stated clearly.

29 TOC is not defined.

29 1920ties should be 1920s.

31 station should be stations

### Page 2

3 – 1960ties -> 1960s

6 – Should be "The fundamental constants"

10 – My impression of this is that while the assumption that all Dobsons share the same

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coefficients is clearly a simplification, the effect was always assumed to be relatively small considering other approximations in the algorithm, such as fixed stratospheric temperature and linear height of the ozone layer and many others. If the authors have a similar view this is worth explaining to the reader who otherwise might be puzzled by it.

12 1970ties/1980ties -> "1970s and early 1980s"

18 "valid" would be better said to be "prescribed" or a similar word, valid implies they were physically correct.

25 The term "Effective" is not explained

27-30 These statements should be made quantitative. How much of the difference was claimed to be explained in this way by the studies listed? In particular you should state the findings of Redondas et al., being the most recent work, as it represents the current understanding of the Dobson & Brewer community, and is often referred to.

### Page 3

3 should be "has offered \_an\_ opportunity"

8 A problem with section 2 is that 2.1 and 2.2 appear to have been written quite independently of each other and simply inserted into the document. The writing styles are noticeably different but more importantly, there is no discussion of the similarities and differences between the method and equipment used in the two laboratories. You should add this.

Page 3 line 29 to Page 4 line 14 There are a number of minor English errors which should be corrected in 2.2.

### Page 4

26 The notation is not quite right because S is written here as a function of lambda and lambda-prime, but then later S is just a function of one wavelength and not two – if you

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are integrating over all lambda you need to say what lambda-prime is.

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4-6 I think this is a very good idea to show results using both the old and new cross-sections!

7 It would be good to include the Dobson equation to show the relation between total ozone and the absorption coefficients

10 "bandpass/slit functions" is awkward terminology, it would be better to choose your preferred terms at the start and use them consistently from then on.

17 Deviations of the "central" wavelength?

20 Why has the threshold of  $\pm 0.05$  nm been chosen as being a "good optical alignment"?

25 This statement ("With this knowledge . . .") is not very clear

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4 Is CD really only a "minor" data set? I am not sure of the proportion of CD observations in the WOUDC but I would have thought there would be a lot given AD-DS is officially restricted to  $\mu < 3$ .

7 This seems to contradict Redondas et al which said the difference between AD and CD was due to the cross-sections, not the slit functions. Please clarify.

10 Please reword this section to be clearer about what has really been shown – ie how much of the difference between AD and CD can be shown to be the cross-sections and how much the slit functions?

12 This reads to me to mean the Dobson – Brewer difference will be reduced because of the new cross-sections, but not because of the slit functions – is that what you mean? Later on down the page (Line 19) you say it is the slit functions.

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17 It is implied here that Dobson data should or could be re-evaluated to use these improved slit functions, but how would you actually do it, given the three Dobsons here had differences ranging from -0.310 % to 0.559 % ? (Table 3)

19 I don't see how you know that the Dobson-Brewer difference will be reduced?

20 You say less than 1%, but there is a value of 0.98% in Table 4, so wouldn't it be likely that some Dobsons in the network would be greater than 1% ?

23 In my opinion, the writing here is too colourful for a scientific paper ("perfect", "optimistic prognosis").

23 You should explain how an intercomparison campaign can confirm that the AD TOC values only need correcting by less than 1%.

30 This seems to me to be the real conclusion of the paper, that if the TuPS can be used to characterise each individual Dobson in the network, the overall quality of Dobson data will be improved by a noticeable amount. If this is also the view of the authors it should be stated more explicitly.

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30 Evans et al 2012 – does this article exist? I can't find it. Are you able to give a more specific reference?

Page 10

Figure 2 Carriage return symbols that appear in the diagram should be removed

Page 13

Figure 7 I would suggest replacing this figure – by eye the reader can't see much difference between the three instruments and we have already seen figures 4, 5 and 6

Page 15

5 German-style quotation marks in the table caption

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