

Interactive comment on “Calibration methods for rotating shadowband irradiometers and evaluation of calibration duration” by W. Jessen et al.

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1. Does the paper address relevant scientific questions within the scope of AMT? Probably. But the paper uses procedures introduced elsewhere (in the reference papers) to study only one specific site. Hence, it is not clear to the reviewer that the paper is of interest to the general community. It is true that the techniques are generally described well and in detail but the conclusions related to evaluating the calibration duration are clearly site (meteorological) specific and would not be likely to be applicable elsewhere. 2. Does the paper present novel concepts, ideas, tools, or data? Possibly. The techniques could be found in other papers and their application is rather site-specific so the novelty and applicability are limited. 3. Are substantial conclusions reached? Yes. Again site-specific. 4. Are the scientific methods and assumptions valid

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and clearly outlined? Yes, although I would have expected comment on the level of accuracy required for the calibration method. Otherwise, it is not clear that appropriate calibrations are being achieved. This makes it difficult to assess the quality of the scientific method. Also, there is reference to an 'expert' who checks when calibrations may be in error. How the expert makes the necessary decisions may be obvious in practice, but it is not clearly a good 'scientific method'. Additionally, the data are subjected to selection effects, 'calibration limits', which are presented in Table 2 such that some data are flagged as 'potentially erroneous'. It appears that such data are actually 'marked for exclusion'. Together with expert intervention, it seems likely that systematic errors could be introduced into the calibration process through the discarding of data. The level of such a (potentially important) error is not discussed. It is true that figure 2 shows improvement of statistical fluctuations with increasing calibration durations but one needs confirmation that the calibration approaches the desired 'correct' value. I think that the neglect of possible systematic errors is not best scientific practice.

5. Are the results sufficient to support the interpretations and conclusions? Yes, for this site.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? Maybe. Perhaps "optimising the calibration duration."

9. Does the abstract provide a concise and complete summary? Yes

10. Is the overall presentation well structured and clear? Yes

11. Is the language fluent and precise? Sufficiently so.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No

14. Are the number and quality of references appropriate? Yes

15. Is the amount and quality of supplementary material appropriate? N/A

General Comments This paper discusses the application of calibration methods (two) to shadowband radiometers at the PSA site in Spain. The paper gives sufficient information and references for the reader to understand the calibration process, and

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this is a strength of the paper. Such shadowband irradiometers are generally calibrated for use when assessing potential solar power station sites and it is clearly important that their calibration process is well understood. An important part of that process is to know what length of dataset is necessary for evaluating a site. This issue is addressed for the PSA site. The problem with this is that the evaluation result is not obviously transportable for use at any other site with different meteorological variability. This would seem to limit the interest in the results presented in the paper. As noted above, there is no discussion of systematic errors in the process and this, together with the lack of a statement on the required level of uncertainty, is a significant problem for this reviewer.

I thought that equation 12 was un-necessarily trivial.

The paper is generally free of major expression errors. Although some of the english expression isn't elegant, it is perfectly satisfactory for the reader.

Line 14 of page 10267 should read "two tables were constructed which allow one to choose..."

Interactive comment on *Atmos. Meas. Tech. Discuss.*, 8, 10249, 2015.

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8, C4483–C4485, 2015

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