

## ***Interactive comment on “Uncertainties of parameterized near-surface downward longwave and clear-sky direct radiation” by S. Gubler et al.***

**Anonymous Referee #2**

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“Uncertainties of parameterized near-surface downwelling longwave and clear-sky direct radiation” by Gubler et al.

General comments

This paper assesses the performance of empirical models describing downwelling shortwave and longwave radiation using measured data from a number of stations in Switzerland for comparison. The topic is suitable for Atmos. Chem. Phys. and the general approach is rational. However, the structure of the paper is partly confusing and the content is purely technical. The authors analyse extended high-quality data sets obtained over a multi-year period from seven observation sites at different altitudes. Given the extension of the data sets used and the modelling effort, the scientific output is very limited and confined to a list of RMSEs, MEs, and a collection of fit pa-

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rameters from which little can be learned scientifically. At several points there are open questions remaining as outlined in detail below. The results of the model validations are not satisfactorily analysed and discussed in terms of available literature.

Overall, the paper needs major revision before publication in Atmos. Chem. Phys.

Specific comments

1) Page 3357: The title is misleading, instead of "direct radiation", "shortwave radiation" or "shortwave irradiance" would be appropriate.

2) Page 3362, line 12: In my technical report I recommended the use of "visibility" instead of "visibility index" because an index usually has no dimension (km in this case). However, "visibility" is a quality and not a quantity and some textbooks therefore recommend "visual range" instead. That is a quantity and has a dimension.

3) Page 3363, Eq. 2: The stated unit of  $a_w$  does not fit to that given for the quantities. Obviously, a factor of 10 is missing. This factor of 10 can be rationalised from the units and is also consistent with the mean value of  $a_w$  given in Table 2. Please also correct the unit of  $a_w$  in Table 2 where the term  $\text{hPa}^{-1}$  appears as an exponent. As to the values in Table 2: I doubt that an uncertainty of <1% for  $a_w$  covers the actual uncertainty of precipitable water.

4) Page 3363, line 14: Avoid the use of °K.

5) Page 3363, line 20: The paragraph on the error estimate for the air mass factor is still irreproducible. Surely the uncertainty of the air mass factor increases with increasing SZA and the 0.1% relative uncertainty applies to an SZA of 86°. How useful is it to force this parameter into your scheme of a fixed standard deviation for all SZA and how was the value of 0.03 derived? For example for SZA = 0° and 86° an absolute uncertainty of 0.03 corresponds to relative errors of 3% and 0.24%, respectively, far greater than the lqbal estimate and with a reversed dependence on SZA.

6) Page 3364, line 6: Table 7 should become Table 3 because it appears first within the

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text.

7) Page 3365, line 1: Remove “and water vapour”

8) Page 3365, line 12: What is the difference between the terms “bulk emissivity” and “atmospheric emissivity”? Perhaps that should be rephrased: “. . .  $T^*$  and a parameterised atmospheric emissivity that depends on  $T^*$  and measured water vapour pressure  $p_v^*$  at screen level”.

9) Page 3365, Eq. 4: What is  $e$  in the brackets, shouldn't it be  $p_v$ ?

10) Page 3365, line 18: “water vapour pressure”

11) Page 3365, line 19:  $h_r$  should be  $h_r^*$  and  $p_v p_v^*$ .

12) Page 3366, line 10: In this study. . . in this study.

13) Page 3366, line 18: “To avoid the step of transforming . . .” There is not much to avoid because the relationship is trivial (Eq. 6). Moreover, Eq. 9 corresponds to Eq. 7, i.e.  $a = \tilde{a}$  and  $p_0 = \tilde{p}_0$  while  $p_1$  and  $p_2$  in Eq. 8 and 10 are indeed different because of the modifications in the parameterisations. There is no explanation why these modifications were made and it is not shown that this leads to any improvement compared to Eq. 8. On the other hand, the modifications prevent a comparison of parameters with literature data. There is probably no need to invent a new equation here.

14) Page 3367, line 3 and Eq. 11: Later in the text you discard this approach. It remains unclear why it was introduced at all although it is “not based on physical reasoning” as stated in 4.2.2.

15) Page 3367, line 10: In the previous version of the paper the sensitivity analysis was made for  $m_r=1$  which was clearly not representative for the measurement locations. But is  $m_r=4.3$ ? That corresponds to a solar zenith angle of about 77 degrees. A  $m_r=2$  corresponding to an SZA = 60° would be a reasonable choice.

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16) Page 3367, section 3.3: This approach should be reconsidered using reasonable uncertainties. Basically the uncertainties of air mass factors are negligible (see under 5). Ozone data can be obtained from satellite observations for any time and location. The remaining uncertainty is negligible for SDR. Uncertainties of ground albedo are probably negligible unless there is snow cover that could be treated separately. The uncertainties regarding prec. water were probably underestimated and aerosol was not adequately considered. Aerosol information could also be obtained from satellite measurements or from ground based stations (e.g. Aeronet). The uncertainties will not be greater than using visibility from Jungfraujoch at 3700 m and use this for all sites.

17) Page 3367, line 16: “. . . neglects the magnitude” ? You probably mean something like “. . . accounts for the sign”.

18) Page 3370, line 6: “. . .resulting in one set of optimal parameters”.

19) Page 3371, sec 4.1.1 Validation: It remains unclear how the SDR model was run for the validation. With “. . .mean values of all parameters and input variables” ? That would hardly work. Probably the statement applies for the ozone column and visibility but certainly not for the air mass factor. And I assume that the actually measured time dependent data of relative humidity, temperature, pressure and ground albedo were used?

20) Page 3371, line 17: “However, the fit for the diffuse SDR is poor. . .”. What is the reason for that? Clearly this is not a feature specific for that model. The high visibility adopted from Jungfraujoch explains the limiting values around 120 Wm<sup>-2</sup> but not the overestimation at lower values. Or did you use a mean value of 0.5 for the albedo as was apparently done in Figure 3? The performance of the model for global radiation seems to be satisfactory, but looking at the diffuse and direct it appears that it is right for the wrong reasons. There may be applications where diffuse radiation is more important and the problem should be sorted out based on the available data.

21) Page 3372, line 8: “. . . and the sun elevation.” That statement cannot apply for the

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diffuse radiation. Clearly, the deviations in Figure 2 depend on solar elevation. The data in Figure 2 in general are somewhat surprising. What is the reason for the constant width of the scatter for the global radiation, independent of the measured values?

22) A further aspect has not been considered in the analysis: The data the model is finally compared with correspond to one-hour averages. During these periods the SZA change considerably, dependent on SZA, but also dependent on the time of year. This corresponds to an additional uncertainty but the effect could be modelled as well. The one hour averages are, by the way, also the explanation for the strange periodic increases of data densities in Figure 2. The authors can reproduce the effect easily by calculating SZA for full hours in the course of a year. The resulting SZA do not cover the complete range with equal density. Because SZA is the most important parameter for SDR, the same applies for SDR.

23) For the validation the errors of the measurements were not considered at all. However, absolute errors in  $\text{Wm}^{-2}$  increase with SDR. Therefore weighed quantities should be considered for the quality measures introduced in section 3.4.

24) Page 3372, line 24: Is a global irradiance of almost  $800 \text{ Wm}^{-2}$  in Figure 3 reasonable for  $m_r=4.3$ ?

25) Page 3373, section 4.1.3: It remains unclear what can be learned from this section including Figures 4 and 5 with regard to the actual performance of the model. Obviously the errors for diffuse radiation are greatly underestimated in this evaluation. Regarding parameter uncertainties note the statements already made under 16).

26) Page 3375, line 18: How can the ME reach  $300 \text{ Wm}^{-2}$ ? For example, take the data of Figure 6. A simple, guessed constant of  $300 \text{ Wm}^{-2}$  would perform better. Or is that also a result of wrong units (see 27) below)?

27) Page 3375, line 19: "The Konzelmann et al. . . ." This bad performance obviously comes from using the wrong pressure unit for water vapour. Why is that still applied

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here at all? I thought that was an error in the work by Pirazzini et al.?

28) Page 3376: It is hard to follow that section. How useful are some of the fitted parameters in Table 5, e.g. the 17631.61 for  $swin_1$  (including the number of digits)? In particular if the parameters deviate wildly from the published values or even change their sign, the fit results do not seem to make any sense. There is no indication regarding fit qualities or systematic deviations. I don't see that all parameterisations were treated adequately.

29) What is surprising in Table 5 is that the Konzelman parameters from all sites together are not covered by those fitted for the individual sites. Are there significant effects of altitude or not? The available data set should contain this information.

30) In Table 6 available literature data should be included. If you use the same parameterisations also  $p_1$  and  $p_2$  could be compared.

31) Page 3379, Discussion: The discussion is too much focussed on this work. Usually other literature is considered here. I don't think it's sufficient to state that the validation of the models has already been made in other studies.

32) Page 3385, A8: The bracket should close after  $U_1$ , before the exponent  $-0.3035$ .

33) Page 3387: Appendix B can be deleted. The same information is given on page 3364.

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