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

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We thank the referee for his or her detailed, dedicated and constructive comments. Based on this we have been able to substantially improve the study. Before answering the reviewer's specific comments, we shortly summarize the main modifications of the reviewed manuscript.

1. We think that the aim of this paper was not well described in the first version. We have therefore rewritten the introduction of the manuscript. To summarize: Although measurements of the atmospheric variables ozone, precipitable water and aerosol are nowadays available, we think that many impact modelers run their models with fixed estimated values of these (and other) parameters, while

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for input they use variables measured at ordinary meteo stations, or coming from global circulation models. We estimate the error and the uncertainty in modeled downward clear-sky shortwave (SDR) and all-sky longwave radiation (LDR) due to these simplifications. In a first step, the SDR model by Iqbal (1983) is validated (see point 2) using high quality measurements at Payerne to show that the model performs sufficiently well for our studies. In a next step, estimated parameter values and respective uncertainties (see point 3) are propagated through the model to estimate the total SDR output uncertainty. Diverse clear-sky LDR parameterizations are discussed (see point 4) and fitted to the measurements at the six locations in Switzerland. Cloud transmissivity is estimated using modeled and measured SDR (Greuell et al., 1997), and thus the uncertainties coming from modeled SDR are propagated to LDR, together with errors in input data and parameters.

2. Validation of the clear-sky SDR: In the first version, we modeled SDR using fixed values for the atmospheric parameters ozone, precipitable water and aerosol (aerosol was estimated using a visibility index), which lead to considerable errors in the diffuse SDR. In the second version, SDR is modeled with measured precipitable water and aerosol ($\text{\AA}ngstr\"{o}m \alpha$ and β) at Payerne. We could thereby show that the Iqbal (1983) model C performs satisfactorily (see Fig. 4, upper figures in the manuscript). Measurements of ozone were not available, but since SDR is not sensitive to that variable (Gueymard, 2003b), this does not change the model outputs. In a second step, SDR was modeled using the fixed atmospheric parameters to identify the error a modeler makes when he does not have the respective measurements.
3. Determining uncertainties of the atmospheric parameters: We have reconsidered all the estimated parameter uncertainties for SDR (ozone, precipitable water and aerosol) and compared our estimates to previous studies (Gueymard, 2003b). Two main changes (a) and (b) were made to estimate the total SDR uncertainty:

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(a) The air mass factor is considered error free since it SDR is not sensitive (see point 16, comment referee 2).

(b) In the first version, aerosol content was replaced by a visibility index measured at Jungfraujoch, and aerosol transmissivity was estimated according to Mächler (1983). In the present version, this approach has been changed, the aerosol transmissivity is estimated using the formulation found in Iqbal (1983) model A which is based on the Ångström parameters α and β . The parameters and their uncertainties are estimated based on Aeronet data at Davos.

(c) The approach to determine the uncertainty in precipitable water remains the same, since it leads to reasonable uncertainties going from around zero to 4 cm (see Fig. 5 in the manuscript).

4. Calibration of the clear-sky LDR parameterization: Twelve LDR parameterizations found in the literature are fitted to the ASRB measurements. The origin of the chosen parameterizations are, in the reviewed version, discussed, and the parameters which are fitted are reconsidered. The reviewers criticized that not all parameterizations were treated adequately in the first version of the manuscript since some of the non-linear least-square fits provided strange parameter values. This did not happen in the revised version due to a more adequate selection of the parameters based on the origin parameterization. We think that the parameterizations are now treated adequately, and show that when modeling clear-sky LDR, the most important step is not to choose the “correct” parameterization (which does anyway not exist), but to choose a parameterization which was developed for similar environmental conditions, or to previously fit the chosen parameterization to the local conditions.

The above mentioned comments are the main issues that were very carefully treated in the modified version. All other comments of the reviewers were, of course, also

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treated with caution, and answers can be found below where all referee remarks are listed. Typing errors and grammatical mistakes were corrected in the text without further comments.

Specific comments:

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

The title was changed to "Uncertainties of parameterized surface downward clear-sky shortwave and all-sky longwave radiation."

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.

Thank you for this recommendation. We have now changed the parameterization of aerosol absorption by Maechler to the formula of the Iqbal (1983) model A, which is based on the Ångström exponent α and the Ångström turbidity parameter β . The two parameters are estimated based on Aeronet measurements in Davos according to Gueymard (2011).

3. Page 3363, Eq. 2: The stated unit of aw 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 aw given in Table 2.

True.

Please also correct the unit of aw in Table 2 where the term hPa^{-1} appears as an exponent.

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Done.

As to the values in Table 2: I doubt that an uncertainty of <1% for aw covers the actual uncertainty of precipitable water.

We have double checked the spread of precipitable water as we model it in this study (with the assumed uncertainty for a_w with measurements of precipitable water from Aeronet at Davos. The spread in precipitable water is slightly underestimated (Aeronet mean and st.dev: 0.94 and 0.53, whereas mean and st.dev in our study: 1.14 and 0.5). Since the difference is however not that large (7% in st.dev.) and since the sensitivity results show comparable outcomes as Gueymard (2003b), we keep the approach.

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

Done.

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 Iqbal estimate and with a reversed dependence on SZA.

Yes, that is true. This paragraph is taken away, and the uncertainties for air mass factor are neglected, since they do not account for much uncertainty as previous preliminary studies have shown (and because of the reviewer comment 16).

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

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Table 7 is meant to be in the appendix and appear at the end of the text. However, in this discussion paper, this is not visible.

7. Page 3365, line 1: Remove "and water vapour"

Done.

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".

The paragraph was changed to: "In practice, T_{atm} is replaced by the temperature T^ , and ϵ_{atm} is parameterised with T^* and 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 ?

Yes, of course.

10. Page 3365, line 18: "water vapour pressure"

Changed.

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

h_r was changed to h_r^ , but p_v is not measured.*

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

That was changed, thank you.

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. $a = a$ and $p_0 = p_0$ 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

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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.

In general, you are right, we have probably not explained the reasons for our approach here well enough. The point is that, as you state, the relationship $N = 1 - \tau_c$ indeed is trivial, but it is not the only parameterization for N found in the literature. For example, Greuell et al. (1997) proposed a quadratic dependence. In a previous study, we compared the two relationships and found better behaviour for the Greuell et al. (1997) relationship. Eq. 9 and Eq. 7 are correspondent only if $N = 1 - \tau_c$, which we did not want to presume. Your argument concerning the comparability with other values found in the literature is however convincing. Therefore, we adopt our approach and use $N = 1 - \tau_c$ and the original parameterizations from Pirazzini et al. (2001). Further, we use one modified formula, and show that it performs most satisfactorily. The problem with the Pirazzini et al. (2001) formula is that we obtained parameter values that are physically not reasonable.

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.

You are right, we don't use this approach anymore.

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

Thank you for your input. The error was to estimate the mean of the path length at Jungfraujoch instead of the mean zenith angle at Jungfraujoch (around 60%)

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and the path length corresponding to that zenith angle. It is now changed to: "The sensitivities are estimated for constant path length $m_r = 2$, the path length estimated for a mean zenith angle of around 60% at Jungfraujoch."

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.

We have adopted and double checked the uncertainties assumed here, and shortly summarize the changes here:

- *Air mass factor: Uncertainties for air mass factor are neglected, they do not aport much to total output uncertainty.*
- *Ozone: The distribution and the parameters (mean and standard deviation) for ozone were double checked using Aeronet data at Davos. The mean deviates by 3 DU, and the standard deviation of the Aeronet data is 13 DU less than the assumed standard deviation in this manuscript. Therefore, the location parameter for ozone is a reasonable estimate, and the spread of ozone is larger in our approach than in the Aeronet data. We therefore assume that the chosen uncertainty is reasonable.*
- *Ground albedo: Uncertainties in ground albedo are important for the diffuse radiation, as shown in the sensitivity study. Since ground albedo varies strongly over the year (snow), a separate ground albedo distribution was*

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chosen for each month. We find this approach reasonable to cover the variability of ground albedo within a year.

- Precipitable water: see 3.
- Aerosol: there have been major changes. The visibility range is unprecise (manually recorded, subjective errors), and therefore we have chosen to model τ_a using the formula by Iqbal:

$$\tau_a = (0.12445\alpha - 0.0162) + (1.003 - 0.125\alpha) \cdot \exp(-\beta m_a (1.089\alpha + 0.5123)), \quad (1)$$

where α and β are the Ångström coefficient and turbidity parameter, resp.. We estimated α and β according to Gueymard (2011, Eq. (30)) using Aerosol optical depth measurements from Aeronet at Davos for the wavelengths 380, 440, 500, 675, 870 and 1020.

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

We do mean "the magnitude", since you can have very large errors (of positive and negative signs) which compensate each other, and obtain the same ME as if you had very small errors compensating each other.

18. Page 3370, line 6: " : : resulting in one set of optimal parameters".

Changed.

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?

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You are right, the formulation was probably unadept. Please note however that μ of the input data is zero, and therefore we do take the actual measured values for these quantities. To avoid confusion the sentence was changed to: "The models are evaluated for a simulation which is performed with the measured input time series (assumed error-free) and the fixed parameter values μ (Table 2).".

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 Wm² 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.

We investigated that "strange" behaviour of the Iqbal (1983) model using measurements of precipitable water and aerosol content at Payerne, plus measurements of the diffuse SDR, and could show that the Iqbal (1983) model performs well (Fig. 4 in the attached manuscript, upper figures), upper figures). This assures that the model performs well and the code was correctly implemented. The scatter for diffuse SDR is in the normal range (personal communication with Christian Gueymard). If however measurements of the atmospheric parameters are missing, diffuse (and to a smaller degree direct) SDR are not represented very well by Iqbal (1983) (Fig. 4, lower figures). In the sum, the errors in diffuse and direct SDR compensate, so global SDR is modeled well (and indeed "right for the wrong reasons", as you state). However, this is exactly what we want to investigate in the present study, and is thus of great interest for us.

21. Page 3372, line 8: " : : and the sun elevation." That statement cannot apply for the diffuse radiation. Clearly, the deviations in Figure 2 depend on solar elevation.

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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?

Errors do indeed correlate with sun elevation, but only slightly ($R^2 < 0.4$). We do therefore not further investigate this. The corresponding paragraph has been changed to: "To check for systematic errors, the residuals e_t were correlated with the input variables and sun elevation. While for the input variables the correlations are low (less than 0.2), errors slightly correlate with sun elevation (between 0 and 0.4 for direct, around -0.4 for diffuse and between -0.3 and 0.2 for global SDR). For direct SDR, the residuals scatter more (towards positive values) above the freezing point and for relative humidity of around 60%, similarly diffuse SDR (but in opposite direction). Due to compensating effects, this is not observed for global radiation. Since the correlations are not large, systematic errors are not further investigated."

The constant width of the scatter in global radiation is since it is more difficult to predict radiation at large zenith angles, so the relative errors are larger there (personal communication with Christian Gueymard).

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.

We modeled the effect of estimating direct radiation as an instantaneous value at one hour and comparing it to averaged measurements by calculating downward

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solar radiation for (A) SZA at at one hour (always at -30 minutes, since the time stamps of our data frame corresponds to the end of the averaging period), and (B) SZA every 10 minutes, and then average it over the hour. This was done at Cimetta for the year 2007. Deviations between model experiment (A) and (B) for direct radiation are around 3%. This has in the manuscript been included as “An additional uncertainty comes from estimating SDR at an hourly value for an instantaneous sun zenith angle. By calculating the solar zenith angle every 10 minutes and averaging the estimated SDR to hourly values, a mean error of less than 0.5 %, and a root mean squared error of 3 % was estimated for all direct, diffuse and global SDR. ”.

23. For the validation the errors of the measurements were not considered at all. However, absolute errors in Wm² increase with SDR. Therefore weighted quantities should be considered for the quality measures introduced in section 3.4.

*We changed the validation measures to **relative** mean bias deviance and the **relative** mean squared deviance according to Gueymard (2011). We find this a reasonable approach since it is used in many publications, and allows for a comparison with other studies.*

24. Page 3372, line 24: Is a global irradiance of almost 800 Wm² in Figure 3 reasonable for $mr=4.3$?

We use now $mr = 2$ and changed the absolute sensitivities in Fig. 3 to relative sensitivities. For the absolute global SDR, we obtain 1000 W/m² as the mean estimate, however at the current state, this is not visible in the publication anymore for the above mentioned reason (relative sensitivities).

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 eval-

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uation. Regarding parameter uncertainties note the statements already made under 16).

We have reformulated the introduction to motivate the selected approach. It should, hopefully, be clear now that we are interested in the errors a modeler must count with if he does not have accurate measurements of the atmospheric parameters ozone, aerosol and precipitable water, as it is often the case for impact modeling studies. However, by using the new approach to estimate the uncertainty in aerosol content (with the Ångström parameters α and β , see 2.) the uncertainty in diffuse radiation is much larger (around 40%) compared to the first version of this study. The uncertainties of all parameters have been estimated again, but mainly the change to from visibility to the Ångström parameters resulted in more reliable model uncertainties.

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

In the specific case where we reached large MEs (down to -300 W m^{-2}) this was due to wrong units when using the Konzelmann et al. (1994) parameterization. In the reviewed manuscript, we do not use the absolute ME and RMSE, but the relative values (as percent from mean measured radiadion) according to Gueymard (e.g., 2011). For the published parameter values, we reach errors of more than 30 %, indicating that the parameterization really does not fit local conditions. However, after fitting, MBDs and RMSDs are strongly reduced in all cases.

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

Yes, you are right, we deleted all corresponding issues, and use the Konzelmann et al. (1994) parameterization as it was published, and only show these



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validation results. The error made by Pirazzini et al. (2001) is now mentioned in the discussion as “Pirazzini et al. (2001) presented comparably large MBD and RMSD ($MBD = -63 \text{ W/m}^2$, $RMSD = 64 \text{ W/m}^2$) values using the Konzelmann et al. (1994) parameterization. We found that this is due to a misuse of the unit of the water vapor which is pascal in the original Konzelmann et al. (1994) publication, but used as hectopascal in Pirazzini et al. (2001). Using the correct unit for the water vapor, the Konzelmann et al. (1994) parameterizations performs acceptable (Fig. 7, left).”

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 swin1 (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.

This problem has been addressed by going through original publications, and reconsidering the parameters which can and which cannot be used for this study. Thereby, some parameters remain fixed now (the exponent of 0.5 in the Brunt (1932) parameterization, for example). Thereby, we obtain only “reasonable” parameter values by fitting, which indeed makes more sense and shows that the parameterizations have been implemented the way they were meant to.

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.

There is a trend with altitude for the estimated Konzelmann et al. (1994) parameters, as for many of the other parameterizations as well. However, we do not exactly know why the estimated exponent of the Konzelmann et al. (1994) parameterization when fitting all stations simultaneously is not covered within the

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30. In Table 6 available literature data should be included. If you use the same parameterisations also p1 and p2 could be compared.

This has been done for the Pirazzini et al. (2001) parameterizations.

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.

We have revised the discussion, comparing with and to other works recently published. Some very recent publications (Gueymard, 2011; Badescu, 2012) treat similar questions as addressed in this study, and thus helped to better put the results into a broader context.

32. Page 3385, A8: The bracket should close after U1, before the exponent 0.3035.

Done.

33. Page 3387: Appendix B can be deleted. The same information is given on page 3364. *As already mentioned in the previous answer, we would like to include Table 7 as an Appendix and not within the text. But if the editor insists, we can obviously change that, delete appendix B and include Table 7 earlier in the text, where it appears.*

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Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/12/C2270/2012/acpd-12-C2270-2012-supplement.pdf>

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