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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Radiation transfer in stratus clouds at the BSRN Payerne site" *by* D. Nowak et al.

D. Nowak et al.

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Reply to referees' comments

Referee 1

Referee 1 noted that our paper provides estimates for cloud properties that are of interest for studies requiring such information, and, as such, is of relevance. However in a general comment, Referee 1 raises the concern that we misstate the validity of the test of the radiation transfer model MODTRAN^{TM} presented in the paper.

Comment 1. In particular, Referee 1 points out that a full test of MODTRAN^{TM} would require all model input parameters to be determined experimentally for the test of MODTRAN^{TM} to be fully valid, while we adjust the cloud vertical extinction in some cases to obtain agreement in the surface irradiance.

Reply: We agree that the research we describe does not constitute a test of MODTRAN^{TM} performances with full closure, because of the adjustment of the cloud vertical extinction. However, we still believe it is a valid test for two reasons. First, the cloud vertical extinction is adjusted within a range of values that were reported in the literature following observations in stratus clouds. Thus, the model can achieve a simulation satisfactorily reproducing the observations at the surface, using input parameters that are for the most part determined from measurements, except the cloud vertical extinction. For this parameter, the value used in the simulations yielding satisfactory agreement at the surface lies within a range that is physically reasonable for the type of cloud studied. Second, the adjustment made to obtain agreement at the surface also yields a satisfactory agreement at the top of atmosphere (TOA), even though we do not consider the TOA observations when adjusting the cloud vertical extinction. When stratus clouds are present, the reflected irradiance measured at TOA is more important than the irradiance diffused through the atmosphere and measured at the surface. In case MODTRAN^{TM} was not satisfactorily simulating the transfer of radiation and the adjustment we made was compensating the errors in the simulation for the surface irradiance, there would be no reason that we obtain a satisfactory agreement at TOA. We think these reasons were not explained sufficiently clearly in our paper and modified it at paragraph 4 of section 5, in section 6.1 and 6.2, as well as at paragraph 1 and 2 of section 7 to produce a revised version of our paper to be submitted for publication in ACP.

Comment 2. Following on his previous comment concerning the lack of full closure in the test of MODTRAN^{TM}, Referee 1 suggested using the events recorded during an intensive observation campaign (Temperature and hUmidity Campaign, TUC) for testing MODTRAN^{TM}, because the cloud characteristics are better defined in this case.

Reply: It is true that clouds were better characterized during TUC. However, the instrument yielding the most detailed information on the clouds during this campaign was a Frequency Modulated Continuous Wave (FMCW) cloud radar, and even this instrument did not provide information on the cloud microphysical properties. Hence, the information available during TUC would also not allow deducing the cloud vertical extinction, the main improvement desired by Referee 1 for such tests. In addition, there were two advantages in the approach we followed of selecting well-defined stratus cloud situations on a long period, even though the routine observations did not allow a perfect characterization of the stratus clouds. First, the selected single layer stratus cases concurring with noon balloon radiosounding are relatively rare, but can be treated with a column radiation transfer model such as MODTRAN^{TM}. More complex cloud cases would require models that can account for 3D effects and necessitate a very large amount of data on the localization of the clouds. Second, the minimum time interval between two cases is about 24 hours, which reduces the uncertainty due to autocorrelation. Using events separated by shorter intervals would demand considering the effect of autocorrelation on the resulting distributions (transmittance, absorbance and reflectance), because stratus occur mainly in stable meteorological situations. Thus our strict selection process admits only few events (2) during the TUC period, and the interest of a model test with so few cases is limited.

Comment 3. Referee 1 demands that we clarify how the absence of cirrus was ensured for the selected cases.

Reply: The selection process includes a requirement that nearby stations at higher elevation (not affected by the stratus cloud layer) report cloud cover less or equal to 1 octa. This ensured a minimal or no cloud coverage above the stratus cloud layer. We acknowledge that optically thin clouds (such as subvisual cirrus) may not be detected by observers, and add uncertainty to our estimate of stratus cloud optical properties. However, even though such subvisual cirrus are expected to be frequent (frequency up to about 20% at middle and high latitude according to Wang et al. (1996)) most of their effect is in the longwave (thermal infrared) part of the spectrum, and their shortwave effect is small compared to this of an opaque cloud such as a stratus. In the revised version of our paper, we changed the 2nd paragraph of section 3 to clarify this point.

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Comment 4. Referee 1 would like that we compare our results to similar estimates from other studies for evaluating the extent to which the findings can be generalized.

Reply: As mentioned in the discussion section (ll. 8-21, p. 11472 in the discussion phase version of our paper), we did compare our results with results of the ARESE (Valero et al., 2000) and ARESE II campaign (Oreopoulos et al., 2003), and they compared well. However, such comparisons are difficult because the absorbance and transmittance are not always given specifically for the cloud layer, but often from a low measurement point (most of the time the surface) and high measurement point (often altitude of plane flight) of the radiation fluxes. Therefore the reported absorbance and transmittance often include the cloud and other atmospheric layers between the two measurement points. In order to get comparable quantities we have to recalculate our results to include the same layers. Because of this added burden, we did not perform such comparisons for more than two other studies.

Comment 5. Referee 1 wants us to further clarify how the method presented in our paper can be used for long-term monitoring of the effect of stratiform clouds on solar radiation as we state in the conclusion.

Reply: The method that we use allows inferring, for stratus clouds, optical properties such as absorbance, transmittance and reflectance, which characterize a large part of the cloud effect on radiation. Currently, a growing array of routine monitoring from ground-based remote sensing instruments and space-based instruments on geostationary platforms provides increasingly detailed and continuous information on clouds and TOA radiation. Performed on long time periods, a method such as the one presented in our study, or development of this method with sampling of a larger fraction of the cloud situations, would allow monitoring of the cloud optical properties mentioned above. In order to specify more accurately how such method could be used, we completed the end of our introduction and rephrased the second paragraph of the conclusion in the modified version of the paper.

Comment 6. Referee 1 pointed out that in some cases (four cases are explicitly mentioned), we do not use the notation required by the ACP guidelines, and requested that we correct the notation.

Reply: We checked the citations we used through our paper, and corrected the cases where we did not conform to ACP guidelines, including those explicitly mentioned by Referee 1: Sect. 1 (Introduction) first paragraph, citation of Trenberth et al. (2007); second paragraph, citation of Cess et al. (1995); Sect. 2.4 (Radiation) first paragraph, citation of Ohmura et al. (1998); and Sect. 3 (Case selection...) last paragraph, citation of Nowak et al. (2008a).

Comment 7. Referee 1 also requests that we include citations to the lead papers on Cloud Anomalous Absorption in our references.

Reply: Because we did not intend to provide a review on Cloud Anomalous Absorption (CAA), we only cited a study by Li et al. (1999) where we introduce CAA. In the same paragraph and on the same subject, we also cited an extensive and recent review by Li (2004), which provides up-to-date information on this subject. In addition, we cited an older review by Cess et al. (1995). We felt this choice was helpful in illustrating our mention of this debate, and the fact that it now seems close to a conclusion. However, following the demand of Referee 1, we added in the modified version of the paper a citation of a review by Stephens and Tsay (1990) summarizing the state of knowledge on Cloud Anomalous Absorption at the end of the 1980's.

Referee 2

Referee 2 rated our paper as being of interest, because of the useful information provided about MODTRAN^{TM} in cases with single stratus cloud layers. However, Referee 2 regretted that the paper does not bring more novelty and originality, especially in the concepts, methods and results.

Reply: We appreciate the interest of Referee 2 for our paper. Concerning the relative

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lack of originality, our goal was not to introduce new concepts. However, we focused on developing a method that can use the type of information that is routinely monitored at aerological stations (stations monitoring meteorological parameters including the status of the atmospheric column using balloon radiosounding). In most cases, studies of radiation transfer including cloudy situations use data from intensive observation campaigns with a very complete array of instrumentation. Such campaigns are not well suited for studying the long-term evolution of cloud properties, since the instrument set and location is most of the time specific to the observation campaign, and the campaigns are rarely repeated at regular interval. On the other hand, our study indicates how a method for monitoring cloud properties on the long term could be devised. In addition, our study covers a period of a length that has very seldom been reached in previous studies.

Comment 1. Referee 2 wants us to clarify how the absence of cirrus was ensured for the selected cases as did Referee 1 in his second detailed comment.

Reply: This comment is addressed in the reply to Referee 1's detailed Comment 3.

Comment 2. Referee 2 wonders why we do not obtain a perfect match in surface irradiance since we adjust the cloud vertical extinction to obtain agreement in this parameter. Referee 2 thinks one can always find a suitable cloud optical thickness to reproduce the surface observed flux for reasonable single cloud layer atmospheric conditions.

Reply: We did not obtain a perfect match because we did not adjust the cloud vertical extinction (not cloud optical thickness since we measure the cloud physical thickness) until we exactly reproduced the surface irradiance. We performed an initial simulation using the model default cloud vertical extinction for stratus cloud. In case we obtained an agreement within $\pm$10Wm^{-2}, we used this initial simulation, because the uncertainty of our surface irradiance measurements were estimated in a previous study on clear-sky radiation transfer to be on this magnitude ($\sim$5Wm^{-2} calibration, $\sim$5Wm^{-2} statistical uncertainty and $\sim$3Wm^{-2} systematic uncertainty (Nowak et al., 2008b)). In case the

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simulated and observed value of surface irradiance differed by more than 10Wm^{-2}, we adjusted the cloud vertical extinction within the range of 30 to 85 km^{-1} (which are inferred as reasonable values for stratus cloud from a study by Lindberg et al. (1984)) until the difference was less than 10Wm^{-2}. Thus, in most cases differences less than 10Wm^{-2} were obtained between the simulated and observed surface irradiance. In addition, in some cases it was not possible to find a cloud extinction value within the aforementioned limits that allowed a match within $\pm$10Wm^{-2}. To clarify this point, we explain this adjustment in more detail in the last paragraph of section 5 in the modified version of our paper.

Comment 3. Referee 2 regrets that that cloud droplet size is not mentioned in our paper and wonders if information about cloud microphysical properties is available in this region?

Reply: Cloud droplet size and density are combined by MODTRAN^{TM} in the cloud vertical extinction, which is the parameter we used. As we mentioned in our paper, cloud microphysical properties are not routinely monitored at Payerne. To further clarify this point, we now specifically mention (in the last paragraph of section 5) that cloud vertical extinction is the parameter allowing taking into account the cloud microphysical properties in the radiation transfer model. In addition, we also mention in the conclusion that an important improvement would be the measurement of cloud microphysical properties. This last point would also allow us to perform full closure as requested by Referee 1.

Comment 4. Referee 2 wants us to clarify what surface albedo (reflectance or BRDF) were used for the MODTRAN^{TM} simulations, and in general clarify further the surface boundary conditions.

Reply: Our paper describes a research built on top of a previous study of radiative transfer for clear-sky situations that we cited as “in print” when our paper was published in the discussion phase and is now published. The information requested by Referee 2

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is described in this previous paper. We now added a specific mention that this previous paper also describe how surface albedo is determined (3rd paragraph of section 5), but still rely on citation of our previous research for keeping this paper reasonably concise.

Comment 5. Referee 2 thinks we link our research to climate change in an overambitious and awkward manner, because our study only considers a limited number of standalone cases.

Reply: In the abstract and introduction, we make clear that our goal is presenting radiation transfer calculation for real stratus cloud situations in a sufficiently accurate manner that we can match both surface and TOA irradiance, and in a second step, deduce the absorbance, transmittance and reflectance of the clouds (ll. 4-16, p. 11456). We linked our research to climate change in stating that clouds are a major source of uncertainty in climate research, and our study was helping in providing information on cloud (stratus) optical properties. In addition, as we mentioned in our answer to the general comment of Referee 2, our study is focused on developing a method based on information retrieved from routine long-term monitoring at aerological stations. Thus, such a method has a potential for allowing long-term monitoring of cloud optical properties (e.g., absorbance, transmittance and reflectance), and thus provide information for studies of changes in such properties, which should be considered in climate change research. This point was mentioned in our conclusions (2nd paragraph), which we changed in the revised version of our paper to further clarify what is described here. In addition, we were beginning our abstract by mentioning the importance of monitoring cloud optical properties for future climate research, which could lead the reader to think that the climate change effect on clouds was the main subject of the paper. We suppressed this part to avoid confusion.

Comment 6. Referee 2 wants us to clarify the type of aerosol information used in our simulations.

Reply: When the sky is covered with clouds, no aerosol observations are available

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for the Payerne station. However, the MODTRAN^{TM} User's Manual (Berk et al., 2003) indicates different default aerosol profiles for different climatological regions. In our study, we selected the MODTRAN^{TM} default profile for a rural atmosphere in mid-latitude regions for the radiation calculations, since it corresponds to the situation of the station.

Comment 7. Referee 2 is skeptical on the usefulness of MERIS data for our research.

Reply: Some MERIS products providing information on the cloud type (classified according their height and optical thickness) were initially used for a few cases. However, these were not used in the final version of our analysis, and we decided to suppress the mention of the MERIS product at the end of section 4, because it was only adding confusion.

Comment 8. Referee 2 disputes that we give real scientific explanations for the reasons of the significant positive bias seen between model-simulated and CERES-observed TOA irradiances (simulations minus observations). Referee 2 thinks we just give very general arguments about possible causes for the bias.

Reply: We are surprised that Referee 2 finds too vague the explanations we give. The first reason cited was that in some cases the CERES viewing zenith angle was large. This reason was mentioned to us by specialists of the CERES team when discussing the cause of the large discrepancies found, and the 40° limit we used was advised to us by the CERES team. The second reason mentioned is the time mismatch. A time mismatch between a simulation and an observation will produce a solar zenith angle mismatch. The solar zenith angle is a parameter with a major influence in determining the irradiance reflected above the cloud, and any significant mismatch in this quantity will produce a discrepancy in the reflected solar irradiance at TOA.

Comment 9. Referee 2 regrets that we limited our study to the broadband shortwave (SW) flux comparison and thinks we should also include longwave (LW) and spectral fluxes in our comparison to ensure that model and observations are in good agreement.

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Reply: We agree that our study would have been more comprehensive if it had included comparisons on a larger spectrum including the thermal infrared as well as detailed spectral bands. But we think it is a general problem of studies that have to be conducted on a limited funding and time that they cannot be comprehensive. Not only would we have liked to include LW irradiance comparisons, but we would also have liked to extend this study to other types of clouds, other locations (typically other BSRN stations with remote sensing capabilities), and longer time periods. Studying specific spectral bands on the other hand would be problematic since BSRN does not mandate monitoring of specific spectral bands (beside the broad SW and LW bands) and we would lack long-term surface measurements. We hope that future funding availability will allow extending our study as mentioned above.

Comment 10. Referee 2 objects that our statements starting the two first paragraphs of the conclusion seem to be an exaggeration of the significance of this paper. Namely the statements are: “This study presents a method to deduce absorption, absorbance, transmittance and reflectance of solar radiation in stratiform clouds, determined with a state of the art RTM and with widely available atmospheric observations.” and “The results presented in this study offer a method for the monitoring of the effect of stratiform clouds on the solar radiation.”

Reply: We assume that this comment is more specifically about the second statement, since the first statement is only a plain description of what our study presents. As mentioned in the reply to Referee 1’s fourth detailed comment, we mean in the second statement that the method we use allows inferring for stratus clouds, optical properties such as absorbance, transmittance and reflectance, which characterize a large part of the cloud effect on radiation, and which could be monitored in the long-term. More details are given in the reply to Referee 1’s fourth detailed comment.

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