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Comment

## ***Interactive comment on “Validating the MYSTIC three-dimensional radiative transfer model with observations from the complex topography of Arizona’s Meteor Crater” by B. Mayer et al.***

**B. Mayer et al.**

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We thank referee #2 for the comments which we address in the following. The referee comments are printed in italics.

*1) My first point of criticism is the way obtained results are commented. At many instance you only say whether agreement e.g. between model simulations and measurements is good or less good without really mentioning what this means in terms of deviation in percent or in W/m<sup>2</sup>. What is good today may not be good enough in 20 years, so I think that it is more important to mention the numbers, the magnitude of the agreement than whether the agreement is good or not.*

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This comment is of course fully correct. I usually tell my colleagues not to use “good”, “very good”, “excellent” etc. but to provide numbers instead, but somehow these terms slipped in. We replaced all qualitative terms by numbers.

*2) The uncertainty of the radiation sensors should be mentioned (see e.g. Gueymard and Myers, 2009) and taken into account in the analysis of the comparison between measurements and model. E.g. there is already an uncertainty arising from the fact that you are using shadowbands for the measurement of the diffuse radiation.*

In the revised manuscript we provide more information about the uncertainty of the radiation sensors which was in most cases sufficient to explain the differences between observations and calculations in the downwelling components. For the upwelling components the differences are generally larger since those are directly affected by two of the least-known model input parameters: surface albedo and surface skin temperature.

*3) For scientists who are not radiation experts but whose interest is more the explanation of the observed extreme temperature inversions some sentences to explain what is the specificity of this Arizona Meteor crater (e.g. add one sentence in section 3) and what may be the impact of the presented results on temperature profile (e.g. 1 or 2 sentences in the conclusion) are missing. You may only mention some hypotheses regarding this point*

We added some explanation in section 3 and in the conclusions, as suggested.

*Regarding fig 3. more explanations are needed. Since these simulations were performed using a topography, the fig presumably shows the irradiance incident at one pixel. This is however mentioned nowhere. => Change text p. 13379 line 21-29*

Good point! It is actually not the irradiance incident at one pixel but rather the domain average diffuse downward irradiance (that is, the average over all pixels). Clarified that in the text!

*p.13383, line 5: Please mention again whether the instruments were in horizontal po-*

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sition or parallel to the slope.

We added several “slope-parallel” to the text.

*P.13384 line 3: “...from this lower terrain may enhancee..” => “...from this lower terrain may enhance..”*

Changed

*P. 13385, line 9: “An underestimation of the outgoing longwave irradiance is seen at these times,....” What is the magnitude of this underestimation? (see my first remark)*

... between 20 and 60 Wm<sup>-2</sup> (added to the text).

*Table 1 p. 13393: A location has always an inclination even if the inclination is equal 0. Therefore please replace for RIM and FLR the “-” with a number.*

Correct. But the azimuth is still “-”.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 13373, 2010.

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