We thank the referees for their efforts, and answer to their concerns as follows:

Anonymous Referee #1

General

1. p. 9527.The re-analysis of meteo data is crucial for the results. More information about the final uncertainty and level of confidence have to be given. In particular, how such data can be extrapolated to areas where no local measurements are available (e.g. Africa)?

The best available answers to this comment can be found in Dee at al (2011), which is already referenced on p9527.

We are of the opinion that including a discussion of the uncertainties and level of confidence of the ERA-Interim data set is not necessary nor within the scope of this paper because (1) ERA-Interim is a publically available data set produced by ECMWF and the authors of this manuscript did not play any role in creating or validating this data set, (2) numerous studies already exist in which the validity and error estimates of ERA-interim are presented (e.g. Betts et al (2009), Mooney et al (2010), Bao and Zhang (2013)) and (3) over 2000 articles have already been published in peer-reviewed journals which apply the ERA-interim data set in a similar manner to what we do in this manuscript.

In response to the second point of this comment, via the 4D-Var data assimilation system observations from all regions of the Earth (including Africa and Antarctica) are included in the ERA-interim reanalysis. Although surface, local observations are indeed sparse in regions such as Africa they certainly do exist and are augmented considerably by satellite observations, aircraft observations etc. Please see Dee et. al (2011) for details of the type and number of observations which are assimilated.

However, we do acknowledge that the ERA-interim cannot be regarded as “truth”. Therefore, we have modified Section 2.2 and the Conclusions to make sure this is clear to readers. We do not include quantitative error estimates as this would be exceeding complex as (1) all meteorological variables taken from ERA-interim to run the dew model have different errors and (2) the magnitude of errors strongly depends on the time of day, season, and global location.

Betts, A. K., M. Köhler, and Y. Zhang, 2009: Comparison of river basin hydrometeorology in ERA-Interim and ERA-40 reanalysis with observations. *J. Geophys. Res.*, **114**, D02101

Mooney, P. A., F. J. Mulligan, and R. Fealy, 2010: Comparison of ERA-40, ERA-Interim and NCEP/NCAR reanalysis data with observed surface air temperatures over Ireland. *Int. J. Climatol.*, **31**, 545–557,

Bao, X and Zhang, F. 2013: Evaluation of NCEP–CFSR, NCEP–NVAR, ERA-interim, and ERA-40 reanalysis datasets against independent sounding observations over the Tibetan plateau. *J. Climate*, **26**, 206–214.

2. p. 9529-30. The exchange coefficients h and k that have been used do not correspond to what is measured, as the authors themselves are noting. Thus the dew yield that is calculated is far from the reality and can give to erroneous interpretations.

To our knowledge no directly comparable measurements on dew formation exist, which makes it impossible to say that the chosen coefficients are wrong (or right). But even if the approaches in measurement and large-scale modeling differ, the underlying physical processes (and their descriptions) are the same.

The revised manuscript features a new Subsection 2.3 on the transfer coefficients, including a sensitivity study. Additionally, all results are based on a different parameterization for the transfer coefficients.

As a matter of fact, it is well known that dew does not form for wind larger than typically 4-5 m/s, but is at maximum for zero-wind (which is not zero because of the natural convection) . This is in contradiction with the model, presumably because of the wrong values chosen for the exchange coefficients.

There is no physical reason for dew not occurring at higher wind speeds, as long as the collector is able to cool itself faster than the wind is heating it. Please note that our paper does not concern naturally occurring dew, but dew on an artificial, efficiently cooling collector surface.

We have further clarified this difference in the revised manuscript by modifying the title.

3. p. 9531. Due to the model that does not account properly of the exchanges and the extrapolation of the meteo data, it is not surprising to see a difference with the collected dew. It would be interesting to have both and simulated values, to figure out what is the difference. In the Zangvil paper, the mean dew yield is on order of 0.08 mm/day, in the present model, it is about 0.3 mm/day. There is nearly a factor of 4 between both values.

We are very happy that our model predicts significantly higher values than those reported by Zangvil (1996), who used a Hiltner dew balance, and the author reports the values of dew as representative to those observed in the desert "naturally". The values presented in our paper represent those from the efficiently cooling OPUR film.

We have further clarified this difference in the revised manuscript by modifying the title and the mentioned paragraph.

More specifics

4. p. 9520, L28. There are studies based on measurements in some countries (India). There is a recent study based on another approach (artificial neural networks) for Morocco, see I. Lekouch, Lekouch et al., Journal of Hydrology 448–449, 60–72 (2012).

The mentioned studies are not global, but useful references nevertheless. These are added in the revised version of the manuscript.

5. p. 9520, L6. The paper is not published, this ref. is not useful

This reference is replaced in the revised manuscript.

Others: 6. Abstract. It has to be noted that the estimation is based on a calculation and reanalysis of meteo data. A level of confidence (uncertainty) has to be given.

The abstract does mention the usage of reanalysis data. Please see our answer to the first comment.

Anonymous Referee #2

1. The authors do not discuss performance of the used model and its applicability to the considered problem. The only phrase gives a reader some information on these issues: "...we followed the approach presented by Pedro and Gillespie (1982) and Nikolayev et al. (1996), which has been found to agree well with empirical measurements of dew collection (e.g. Beysens et al., 2005)" (page 2; lines 110-113). However, in my opinion, the mentioned paper (Beysens et al., 2005) does not support this conclusion. In fact, the results of the model verification are not presented in (Beysens et al., 2005), except for one picture comparing simulated mass of the condensed water with the mass measured in 1 experimental site during 8 hours. This result does not give any basis for use of the model without any additional verification, especially for its use for the global and multi-year scales. I suggest the authors to present more references which could demonstrate for a reader that the model is applicable for different physiographic/climatic conditions, seasons, etc. Note that the authors of

the model (Nikolayev et al., 1996) did not use any empirical measurements at all for the model verification.

This is a very good comment. It is true that Beysens et al. (2005) present only one figure in support of their figure, but their text says: "This software was verified against the experimental data in Ajaccio, Bordeaux and Grenoble. As a rule, the variance of the difference between the measured and simulated temperatures of the condenser plate did not exceed 0.5 ^{o}C." They do, however, acknowledge limitations in the model.

We have included several more references to studies that have applied a similar energy balance model. The revised manuscript features a new Subsection 2.3 on the transfer coefficients, including a sensitivity study. Additionally, all results are based on a different parameterization for the transfer coefficients (Richards, 2009).

Nilsson, T, 1996. Initial experiments on dew collection in Sweden and Tanzania. Solar Energy Materials and Solar Cells 40, 23-32.
Madeira, AC, Kim, KS, Taylor, SE, Gleason, ML, 2002. A simple cloud-based energy balance model to estimate dew. Agricultural and Forest Meteorology 111, 55-63.
Jacobs, A., Heusinkveld, B., Berkowicz, S., 2008. Passive dew collection in a grassland area, The Netherlands. Atmospheric Research 87, 377385.
Richards, K., 2009. Adaptation of a leaf wetness model to estimate dewfall amount on a roof surface. Agricultural and Forest Meteorology 149, 13771383.
Maestre-Valero, JF, Martinez-Alvarez, V, Baille, A, 2011. Comparative analysis of two polyethylene foil materials for dew harvesting in a semi-arid climate. Journal of Hydrology 460-461, 103-109.

2. Any model has some parameters, which can not be assigned a priori and have to be adjusted though calibration against the available measurements. Without calibration, as well as without any comparison with experiment data, simulation results look rather arbitrary. I suggest the authors to give complete list of the model parameter values and refer to publications from where the values are taken. Also, it would be useful to add small discussion on the parameters variability in space and time.

All the parameters used in our model are listed or explained either in the section Methods or in Table 1 (or among the relevant part of the Results section). Furthermore, the source code of the model will be made public, although only after the article may be accepted for final publication. Although some of the used parameters are at least semi-empirical, the energy balance model itself has a solid theoretical foundation.

Some more discussion on the parameters, including a Subsection 2.3 dedicated to the transfer coefficients, have been added in the revised version of the manuscript.

3. There are many sources of uncertainty of the obtained assessments of the global potential for collecting water from dew. Among the most important, the uncertainties of the model structure, parameters and meteorological inputs can be mentioned. I have no doubt that these uncertainties affect the obtained assessments and their credibility in a large degree. I suggest the authors to take this issue into account in the discussion section and to moderate some conclusions. In particular, I do not see any basis for the conclusion that "the long (simulated) time-series in our study provides information about the seasonal variation of dew formation as well as long-term trends in dew yield, which may be associated with climate change"

Discussion on uncertainties has been added in the revised version of the manuscript. The overall conclusions have been "tuned down" by noting the various related uncertainties.

The reference to the impacts of climate change has been changed (may -> could) in the Introduction (it was never mentioned in the Conclusions).

The changes in the manuscript have been highlighted. Parts of the Results sections have been changed to better accommodate the new parameterization. All figures are new. Additionally, some minor corrections and additions have been made throughout the manuscript.