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Hydrology and Earth System Sciences Discussions

Interactive comment on "Reference crop evapotranspiration derived from geo-stationary satellite imagery – a case study for the Fogera flood plain, NW-Ethiopia and the Jordan Valley, Jordan" *by* H. A. R. de Bruin et al.

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Reply to interactive comment by Prof. A. Thomas.

We highly appreciate the interest Prof. Thomas showed in our paper.

We agree with his remark that sentences such as "The correspondence is very good" have to be replaced by statistical information such as correlation coefficients. We revised our manuscript according to this comment, by adding the correlation coefficient

and the slope of the linear regressions both to the figure captions and in brackets in the text.

The main concern of Prof. Thomas about our paper is that we hardly pay attention to effects of wind, whereas his own findings reveal a relative large influence of wind speed on reference ET. We agree that the number of lines we spend on the subject is relatively small. However, in fact our discussions went fairly 'in depth' through the cited papers.

Firstly, we pointed out that for well-watered short vegetation it can be shown that conditions are fairly close to 'equilibrium' conditions. Then according to the Penman-Monteith equation itself wind speed does not affect evaporation strongly. This was shown for instance by Thom (1975). Unfortunately this citation was not in the list, this has been corrected now.

Secondly, we referred to the findings by De Bruin and Stricker (2000) where the relatively weak wind speed dependency of ET0 was confirmed experimentally. Moreover, additional theoretical explanations were given, i.e. the feedback mechanisms with the planetary boundary layer and the surface temperature are discussed, explaining why these tend to decrease wind speed sensitivity of ET0 of well-watered short vegetation.

Thirdly, we discussed wind speed effect implicitly in the section on advection by referring to the paper by de Bruin et al. (2005). In this section we pointed out that wind speed is expected to play a role, because advection effects will be enhanced when wind speed increases. However, advection is still a poorly understood phenomenon and it is not clear how to included advection in ET0.

In addition, we studied several recent papers revealing wind speed effects, for example:

Thomas, A., 2009: Evapotranspiration and water balance in mountain regions: lessons from the Tibetan Himalayas. Journal of Hydrology and Meteorology 6 (1), 27-36

Thomas, A., 2008: Development and properties of 0.25-degree gridded evapotranspi-

ration data fields of China for hydrological studies. Journal of Hydrology 358 (3-4), 145-158

Chen, S., Liu, Y. And Thomas, A., 2006: Climatic Change on the Tibetan Plateau: Potential evapotranspiration trends 1961 – 2000. Climatic Change 76, (3-4), 291-319

Prof Thomas (personal communication) acknowledges that in these studies evapotranspiration is evaluated with the Penman-Monteith equation (PM) using meteorological data gathered over non-irrigated grass. Especially, in the dry season using data over non-irrigated grass will lead to overestimation of the water vapor deficit, D, (temperature is too high and water vapor pressure too low). In the second term of PM, D is multiplied with a linear wind function. Consequently, we expect that this overestimation of D enhances artificially wind speed sensitivity of calculated evapotranspiration. But that is an artifact, because data collected over non-irrigated FAO-grass are used as input. Note that the physical meaning of this calculated evapotranspiration using PM, without further ado, is not clear to us. Is actual evapotranspiration being dealt with, in that case? Or is it considered to be the reference crop ET (as in our paper)?

In spite of the above, it is our intention to consider wind effects in future studies. We can imagine that in dry, warm regions with high wind speed climates, one has to include wind speed in the ET0 estimates.

Reply to review report 1.

First of all the authors thank reviewer 1 for his/her very valuable comments. We apology for our typing errors and the inaccurate way we dealt with citations and literature list. "OK" means that we will adopt the comment and that we revised the manuscript accordingly.

Point-to-point response:

P4928, line 2: the citation Allen et al. (1998) is not in References. The citation is now included in the references.

P 4930, equation 3: mm/day, change to mm day-1 OK

P 4930, line 13: kPa/oC, change to kPa oC-1 OK

P4932, equation 9 and line 12: change LV by λ OK. We have deleted under equation (10) "in which λ is the latent heat of vaporization", because this is a repetition.

P4933, line 7: "Choudhury and de Bruin (1998)", change to ""Choudhury and de Bruin (1995)" OK

P4933, lines 20-22: remove the paragraph "Then alternative . . .." because results of the application of the equation proposed by Hargreaves are not presented in the text. Also, it is necessary to remove the reference "Hargreaves et Allen (2003). OK

Page 4935, line 3: the citation Mazahreh (1993) is not in References. Corrected, we now only refer to Mazahreh (2001)

Page 4935, line 3: there is a disagreement in the references Shatanawi et al. (1986,1994) between this page and the References. We have removed this discrepancy in the revised manuscript, i.e. we replaced:

"...Fardous (1983), Shatanawi et al. (1986), Ghawi and Shatanawi (1986), Mazahreh (1993) and (Shatanawi et. al., 1986, 1994)"

by:

"..Fardous (1983), Shatanawi et al. (1986, 1994), Ghawi and Shatanawi (1986) and Mazahreh (2001)".

Page 4936, line 3: 0.67 oC/100 m, change to 0.67 oC (100 m)-1 OK

Page 4936, last paragraph: it is necessary a discussion about the underestimation of ETo using sunshine duration instead of solar radiation measurements. Perhaps this underestimation of ETo is caused by an underestimation of solar radiation using data of sunshine duration.

We adopt this suggestion and we added this aspect to the paragraph.

Page 4937, line 26: the citation Abreham (2009) is not in References.

This citation referred to Alebachew (2009) –first and last name were reversed-. In the revised version, we decided not to use this citation; it was removed from the references and the text.

Page 4938, first paragraph: conclusion of this paragraph is not justiïňĄed. Good estimations of solar radiation from sunshine duration are possible.

At this point we do not fully agree with the reviewer. As a compromise we added: ", but we acknowledge that good estimates of solar radiation from sunshine duration are possible provided that they are calibrated locally." . . . at the end of this paragraph.

Page 4938, line 8: PNFAO-Rs, change to PMFAO-Rs OK

Page 4938, line 20: the citations Allen (1996) and Allen et al (1996) are not in References. We included these citations in the literature list of the revised manuscript

Page 4938, last paragraph: this conclusion is not true. Accurate measurements of net radiation are possible using a four component net radiometer.

We did not change the text regarding this point. The reason is that the first author (HdB) had a discussion with the first author of the FAO-56 report, Prof. Allen, on the use of measured net radiation. When the measurements are taken over well-watered FAO-grass, measured net radiation is the best indeed. However, when measurements are taken over non-irrigated surfaces then measured net radiation will differ very significantly from that of the hypothetical FAO-grass. This effect is very pronounced in the dry season. There are cases where measured net radiation is 50

Page 4939, equation 11: change Lv by λ OK

Page 4939, line 20: the citation Thom (1977) is not in references. We included this citation in the literature list of the revised manuscript and replaced "Thom (1977)" into

"Thom (1975)"

Page 4940, lines 6-7: change Berengena and Gavilan, 2007 by Berengena and Gavi lan, 2005. OK

Page 4940, line 12: change De Bruin et al. (2004) by De Bruin et al. (2005) OK

Page 4940, line 15: change Eq. (1) by Eq. (2) OK

Page 4940, line 22: change kPa/K by kPa K-1 OK

Page 4941, lines 13-14: this reference is not in the text. The reference was removed from the list.

Page 4942: delete de reference Hargreaves and Allen We did not remove this reference, because we still use it in the last paragraph of section 6, Discussion and Conclusions.

Page 4943: the references Lecina-Brau and Martinez-Cob (2000), Pinker et al. (2000) and Rubio et al. (2003) are not in the text

These references have been removed.

Pages 4948 and 4949, Figures 3 and 4: the captions of these Figures are not correct. We corrected this in the revised manuscript.

Reply to review report 2 by Prof. W. Abtew.

1. Add statistics to all comparisons. The terms "the method performs very well, fairly etc." are subjective. At a minimum, correlation between the two sets of numbers should be presented. We added the requested statistical information both to the figure captions and to the discussion in the text.

2. The difference between Jordan Valley and Ethiopian Highland is not elevation only. Latitude and the meteorology are different. The difference in ET0 should be attributed to all variables.

On one hand this comment is entirely correct, but on the other the low altitude of the Jordan Valley is a unique feature. We therefore stressed explicitly this special feature on purpose. Not at least because pressure (elevation) effects of evaporation and ET0 are discussed rarely in the open literature. We added the following sentence: "Of course, other environmental differences between the two locations apart from elevation are relevant as well."

3. Define eo in equation 4. OK

4. Page 4938, paragraph 3, it needs to be pointed out that acquiring good quality netsolar radiation data is challenging.

We did not change the text regarding this point. The reason is that in the reference crop for which PMFAO is defined has an albedo of 0.23, whereas the measured net solar radiation depends on the actual albedo. Under dry hot conditions the difference can be large. See also our remarks above on measured net radiation.

5. Page 4940, units for γ is kPa/oC. OK

Editorial Comments.

1. Figure captions for 3 and 4 are not correct. Figure 9 caption should not refer to Figure 8 as Figure 8 shows Rs and Figure 9 shows T. We corrected for this mistake.

2. On page 4932, paragraph 3, please show Makkink as a citation. We included this citation.

3. Literature cited in manuscript but not listed in the References: Allen et al., (1998); Allen (1996); Allen et al., (1996); Thom, 1977 OK see reply to review report 1.

4. Literature listed in References but not cited in the manuscript: Alebachew, A., 2009; Lecina-Brau and Martinez-Cob, 2000; Pinker et al., 2000, Rubio et al., 2003; Schillings et al., 2004 OK see reply to review report 1.

5. Literature cited in the manuscript but not listed in the References: Abre- ham (2009)

This citation should have referred to Alebachew (2009) (we reversed first and last name). In the revised document, we decided not to use the reference and removed it.

6. Mismatches of year between literature citation in the manuscript and in the list in References. Hargreaves and Allen, 1985 vs 2003; Shatanawi et al., 1994 vs 2003; Kohsiek et al., 2008 vs 2007; Berengena and Gavilan, 2007 vs 2005. OK see also the reply to review report 1.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 4925, 2010.