Reply to interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13595, 2013.

Author's Response

This file includes the following:

1. Author's response to Editor
2. Author's final response to Anonymous Referee 1
3. Author's final response to Anonymous Referee 2
4. Author's final response to J. Szilagyi
5. List of all relevant changes in the manuscript

It should be noticed that the authors did reply to every specific comment separately and the relevant changes in the manuscript is specified at the end of each reply.

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1. Authors' reply to "Interactive comment on "Improving the complementary methods to estimate evapotranspiration under diverse climatic and physical conditions" *by* F. M. Anayah and J. J. Kaluarachchi" by Editor

Received and published: 7 February 2014

1. It is interesting to compare the CR methods in the contrasting land and climate conditions. Also, it is an ambition to try to develop a universal CR model which is calibration free. This paper has obtained some useful and promising results.

Reply: The authors would like to thank the Editor for the time and effort made available to manage the editing process and comment on the manuscript.

Your comments and the comments of each and every reviewer (2 anonymous reviewers and Dr. J. Szilagyi) have been carefully addressed. The numbers of lines and pages of the manuscript where comments has been addressed is highlighted accordingly in red.

2. However, the reviewers also raised some important questions. I would like to ask the authors to revise the manuscript according to all the comments, especially the following two major points:

   1) Please pay more attention to the 'universal' of the proposed GG18 model. J. Szilagyi (comment #9) mentioned the comparison of different models. Referee #1 (major comment #1) mentioned the physical consideration about the definition of ETp or ETw and wanted more discussion/explanation on the physical (not pure empirical better) basis of the proposed variations of CR models. For this, I would like the authors to refer to the following papers.

   Han S. et al. A nonlinear function approach for the normalized complementary relationship evaporation model. Hydrol. Process, 2012. DOI: 10.1002/hyp.8414

Lhomme, J.P. and L. Guilioni, Comments on some articles about the c omplementary relationship. Journal of hydrology, 2006. 323: 1-3.

Lhomme, J.P. and L. Guilioni, On the link between potential evaporati on and regional evaporation from a CBL perspective. Theoretical and Applied Climatology, 2010. 101(1): 143-147.

Reply: The authors will edit the manuscript as needed for further clarity and explanation.

Please see: lines 126-127 (page 6), lines 136-144 (page 7), lines 343-355 (page 17), lines 391-396 (page 19) and lines 554-568 (pages 26-27).

2) Please pay attention to the height of the EC instruments, which is an important detail for the representative scale of the ET measurement.

Reply: The height of the EC instruments is not provided for each and every EC tower. Personal communications with PIs of eight towers were made to provide the information required. It should be mentioned here that the FluxNet website mostly provided the EC tower height and so did the AmeriFlux website. The AsiaFlux, however, clearly provide the measurement height. The EC tower height indicates the instrument height, yet this statement is not necessarily true in all cases. Sometimes, the height of each instrument (temperature, humidity, LE, etc.) in the same EC tower varies. This is one of the reasons why such information were not given in the original manuscript. The authors will edit the manuscript as needed and give information required.

Please see: lines 261-271 (page 13), lines 499-501 (page 24) and the updated Table 1.

2. Authors' reply to "Interactive comment on "Improving the complementary methods to estimate evapotranspiration under diverse climatic and physical conditions" *by* F. M. Anayah and J. J. Kaluarachchi" by Anonymous Referee #1

Received and published: 7 January 2014

This paper proposed a complementary relationship model without calibration through the inter-comparison of CRAE, AA and GG model, as well as variations of them. The work is valuable because large number of FLUXNET sites were used for validation of CR model.

Reply: The authors would like to thank the anonymous reviewer for the time and effort made available to comment on the manuscript.

Major comments:

1. "This study aims to develop a calibration-free universal model using the complementary relationships to compute regional ET in contrasting climatic and physical conditions with meteorological data only". This purpose is very interesting.

However, I doubt that the proposed GG18 model may be not a "universal model". There are two methods on complementary relationship model. The first one is trying to give suitable estimates of ETp or ETw but keep the original complementary relationship (Eq. 1 or 8). This paper looks like the first one. The authors proposed several combinations of the equations, variables of the complementary relationship models. But there is little physical consideration about the definitions of ETp (or ETw) and the complementary relationship during the study. The results may be limited since there would be many other variations. For example, there may be other relationship except Eq. (1) or (8). It is only proved that GG 18 is the best between the 33 models used in this study. Please give more discussions about that GG18 is a "universal model" or not? Why?

Reply: The authors believe that the GG18 model is close to a "universal model." The GG18 model shows a better behavior among the 34 sites and its results are more consistent across the spectrum of climatic classes as shown in Figure 6. The ET estimates of the GG18 model for the moderate-climate sites are comparable to both those of wet or dry climatic classes (Figure 6), and the most recent ET studies (Table 6). None of the original (CRAE, AA and GG) methods, however, succeeded to estimate ET under sub-humid and Mediterranean climatic classes (see Table 2). The discrepancy is clear when compared to the more extreme conditions, i.e., dry and humid categories (Table 2).

As for the physical consideration about the definitions of ETW and ETP, the discussion in the paragraph starting line 25, page 15 provides some explanation. The authors used equations in the original methods of CRAE, AA and GG in different ways. In the original CRAE and AA methods believed that the Priestley–Taylor equation can better estimate ETW. This contradicts with what the original GG method that says simply the Penman equation is superior in predicting ETW. In this study, it is meant not to explore all forms and formulae of ETP and ETW relationships since the focus here is to developing a model for reliable prediction of actual ET using readily vailable data for a wide variety of physical and climatic conditions. Therefore, some of the formulae from the original methods were used in this work to model selected processes given the original methods has proven the physical basis of each process.

As for the number of model variations, Figure 3 shows that further model variations based on both the CRAE and AA methods is of less or no value than those based on the GG method. The largest possible number of model variations is considered in developing the 33 models originated from the basic complementary methods. Although other potential formulae were neglected, yet this was justified by lessons learned from prior studies. For instance, Hobbins et al. (2001) found that changes to the AA method did not necessarily produce superior results especially by perturbing β (see page 15 lines 4-6). Therefore, the authors worked consistently in narrowing the number of model variations in a way that a comparison between variations can be plausibly and effectively made while not repeating previous findings.

As for the complementary relationship represented by equations (1) and (8), those are the two formulae used by the original complementary methods. It is true that there could be more formulae developed to simulate the complementary relationship between ET, ETW and ETP, however, these formulae need to be calibrated to a specific location under specified conditions and this is contrary to the purpose of this study.

Again, the authors believe that the results obtained by the GG18 model are encouraging and promising. This does not necessarily mean that further research cannot be conducted to make further improvements.

Please see: lines 126-127 (page 6), lines 136-144 (page 7), lines 343-355 (page 17), lines 391-396 (page 19), lines 554-568 (pages 26-27).

2. Other studies were trying to propose a better model and calibrate the parameters. According to the Granger and Gray's work, equation (8) is not comparable to equation (1), because equation (8) is just a rearrangement of the energy balance. The key of the GG model would be the function describing ET/ETp (equation 11 or 12). Is it possible that there are other parameters of Eq. 11 or 12, or other relationships describing ET/ETp? And the GG model performs better than GG18 with these relationships? I suggest more calibration work on the relationship (Eq. 11), or proposing a more universal relationship. The model with calibrated Eq. 11 may perform better than GG18.

Reply: Actually, equation 1 is a special case of equation 8 that occurs only when $\gamma = \Delta$ as indicated in page 8 line 12. The rate of change of saturation vapor pressure with temperature (Δ) is dependent on air temperature. Equation 8, therefore, makes the complementary relationship a function of air temperature. The value of air temperature at which $\gamma = \Delta$ can be back calculated and then conclude that equation (1) = equation (8) at that particular temperature.

The authors agree with the reviewer that equation 11 or 12 is of great significance to the results as it describes the ET/ETP relationship that influences the procedure to estimate actual ET. Yet, equations 11 and 12 were developed by Granger and Gray and there was no attempt to perform calibration in this study. The word "calibration" indicates validity to a given location or region and neglect the "universal" applicability. For this reason, this study did not attempt to conduct calibration to the equations proposed by the original methods.

Please see: lines 203-206 (page 10).

3. AA and GG models are usually used at daily timescale, while the CRAE model is designed at monthly timescale. Since daily data is included in the datasets used in this study, I suggest that

the AA and GG model should be calculated at daily timescales. If the AA and GG model is used using monthly data, the parameters may be changed. Please give some explanation or discussion.

Reply: The authors wanted to propose a universal ET model that can be successfully used under data deficit conditions under which daily data are missing or unavailable. It is believed that the regional estimates of ET entail monthly time resolution. The proposed GG18 model, however, was applied to a countrywide study of Ghana where daily data were available and the model performed well (Anayah et al., 2013). Monthly data from 2000 to 2005 were used for comparison with daily estimates of the GG18 model. The results suggest that the GG18 model can accommodate both daily and monthly time steps to produce consistent results. Please refer to Anayah (2012) and Anayah et al. (2013) given below for more details.

Please see: lines 570-582 (page 27).

Specific comments to the authors:

1. There is no need to give Fig. 7. It would be more clearer if GG18 is given as E/ETPT=?

Reply: The ET/ETP relationship is given in the lower right-hand box as ET/ETP = $2G_1 / (G_1+1)$. We will edit the manuscript as needed for further clarity.

Please see: the updated Figure 7 (attached).

2. It is better to list the mean value of ETpen, ETPT of the 34 sites in Table 1.

Reply: Table 1 is crowded enough to add additional information (or data) about each of the 34 sites. Furthermore, Table 1 serves as an introductory table that depicts the physical and climatic characteristics of each site and explains the reason for selecting these sites. In addition, the available EC-based actual ET data are given in Table 1 while ETpen and ETPT estimates represent potential, not actual, ET. This may cause confusion to the reader and therefore avoided. We will attempt to improve clarity in the revised manuscript.

References

Anayah, F.M., 2012. Improving complementary methods to predict evapotranspiration for data deficit conditions and global applications under climate change. PhD Dissertation, Utah State University, Logan, Utah, U.S.

Anayah, F.M., Kaluarachchi, J.J., Pavelic, P., Smakhtin, V., 2013. Predicting groundwater recharge in Ghana by estimating evapotranspiration. Water International 38(4), 408-432. DOI: 10.1080/02508060.2013.82164

3. Authors' reply to "Interactive comment on **"Improving the complementary methods to estimate evapotranspiration under diverse climatic and physical conditions"** ***by*** **F. M. Anayah and J. J. Kaluarachchi" by Anonymous Referee #2**

Received and published: 27 January 2014

This paper evaluates three existing complementary methods compared with EC observations, identifies the major model components contributing to predicting ET. Then, a universal model, which is calibration-free, is proposed to predict ET independent of land cover/use. This research is quite comprehensive and interesting.

Reply: The authors would like to thank the anonymous reviewer for the time and effort made available to comment on the manuscript.

The proposed GG18 model shown in Fig 7 has the best performance compared with other combinations of components. The empirical equation for computing Gi is very important for the method. More discussion on this equation is necessary, particularly when it is combined with equations (1) and (7).

Reply: The authors will edit the manuscript as needed for further clarity and explanation.

Please see: lines 203-206 (page 10).

Lines 18-19 on page 13611 "Overall, GG22 has the lowest median and average values of RMSE that are 16.20 and 20.23mm month^{-1}, respectively." It is good to mention the uncertainty of EC observation compared with RMSE.

Reply: The authors will edit the manuscript as needed and give information required.

Please see: lines 454-461 (page 22).

Table 6 compares the GG18 and recently published ET studies. The GG18 performance can also be compared with the original CRAE and AA model shown in Table 2.

Reply: The authors will update Table 2 or an appropriate location in the revised manuscript to provide this comparison.

Please see: lines 554-568 (pages 26-27).

4. Authors' reply to "Interactive comment on "Improving the complementary methods to estimate evapotranspiration under diverse climatic and physical conditions" *by* F. M. Anayah and J. J. Kaluarachchi" by J. Szilagyi

szilagyi@vit.bme.edu

Received and published: 20 November 2013

My comment on this manuscript does not strive to be comprehensive. I just list a few problematic issues.

Reply: The authors would like to thank Dr. Szilagyi for the time and effort made available to comment on the manuscript. The comments are constructive and helpful to better present the concepts and interpret the results.

1/ In lines 27-29, pg. 13598 the authors mention some previous studies that used the complementary methods (CM) with "little success" and they list two of my recent works I was the principal author of: Szilagyi and Kovacs 2010, 2011. I am totally confused because in these studies the application of the CM was a clear success, as anyone can check. They also list in this context the recent study by McMahon et al. (2013) who concluded that the CM-based ET estimation methods are the best available practical ET estimation methods.

Reply: First, the authors wanted to inform that the original complementary methods did not perform well specifically under different physical and climatic conditions. The authors did not attempt to undermine the work of others and instead, the authors were attempting to solicit more attention to the complementary methods and to show the need for improvements. Many other studies can be added here such as those of Hobbins et al. (2001) and Xu and Singh (2005) that showed the original complementary methods should be further studied.

As for the work of Szilagyi and Kovacs (2010, 2011), there was a systematic underestimation of 44% from measurements in one of the three EC sites used for validation. Although the difference was referred to the physical variation of that particular site (i.e., Hegyhatsal), the three sites still lie within a small area that shares similar climatic and geographic conditions. In the results of Szilagyi and Kovacs (2010), the authors corrected the ET estimates of the Hegyhatsal site due to the poor performance. The focus here is to say that the original complementary methods need further corrections and modifications to perform under variety of climatic and physical conditions.

As for the study of McMahon et al. (2013), Table 4 showed that CM-based models are not preferred or not recommended to use in most applications. It is only the Morton method that is preferred in the application of shallow and deep lakes. Additional studies failed to predict ET using the original complementary methods are shown in the supplementary material attached to the article in particular Sections S7 and S8.

We will edit the manuscript as needed for further clarity.

Please see: lines 73-74 (page 4).

2/ In the Penman equation the second, aerodynamic term accounts for local advection and not for "large scale advection effects" as the authors claim in line 21, pg. 13602. What accounts for large scale advection is the value of the Priestley-Taylor parameter, alpha, being larger than unity.

Reply: As stated by Hobbins et al. (2001) when defining the terms of the Penman equation: "the second term of this combination approach represents the effects of large-scale advection in the mass transfer of water vapor and takes the form of a scaled factor of an aerodynamic vapor transfer term E_a."

As defined by McMahon et al. (2013), the aerodynamic component of the Penman equation accounts for regional drying power of atmosphere. However, alpha coefficient of the Priestley and Taylor (1972) is actually computed under advection-free conditions. It is known that the alpha value of 1.26 represents 26% of additional power of evaporation induced by advection (sensible heat transferred by wind).

Please see: lines 176-177 (page 9).

3/ In line 15, pg. 13604 the authors claim about the GG method that it does not need "surface parameters (temperature and vapor pressure)". I am asking them: which CM method asks for such values, because I am not aware of it, at least what concerns the CRAE or AA methods.

Reply: The unique feature about the GG method is that it can estimate actual ET with no prior estimate of ETP. This is the first half of the sentence that is not true for either the CRAE method or the AA method. As for the other half which is describing the temperature and vapor pressure requirements, there is no indication that this is exclusively true for the GG method only. We will edit this sentence for clarity.

Please see: lines 215-218 (page 11).

4/ It would have been much more informative to use a mean BIAS value, not an abso- lute one, to see where the models overestimate and where underestimate EC-derived ET rates. From the published BIAS values this cannot be deduced, since they are all positive values, yet the authors discuss under and overestimation of the different mod- els under different climates before they do their analysis with the model-components.

Reply: The authors decided to use the absolute value of bias to better assess and demonstrate the behavior of different models for accuracy. The problem of having the true value of the bias is that the results could be misleading especially when comparing a large number of model variations. This can be explained simply in the following example.

Consider two model variations A and B of bias range from -5 mm to 4 mm and from 2 mm and 3 mm, respectively. Which is better? Although the mean bias value of model A ((-5+4)/2=-0.5 mm) is smaller than that of model B ((2+3)/2=2.5 mm), yet model B is better as the discrepancy (bias) from the origin (true measurement where bias should approach zero) is lesser. The spread of bias values around the origin (zero value) is larger for model A which is an unfavorable for a better estimates of ET. It should be noticed that using the absolute value of bias simply avoids this confusion. Mu et al. (2011), for instance, had used the absolute bias value to assessing model performance since 46 AmeriFlux sites had been considered in this study. However, Mu et al. (2007) had applied their model on a fewer

number of sites (19 AmeriFlux sites) and therefore used the bias value for comparison. Huntington et al. (2011) had used the percent bias which cannot have a negative value.

5/ In line 28, pg. 13608 the authors say that the GG method has the lowest bias, but I do not think a value of 15.7 vs 15.5 marks a statistically significant difference, considering the errors in the EC measurements.

Reply: Although the mean values of BIAS may not be statically different, but the AA model and GG methods both share the lowest mean BIAS value. Therefore, the average values of the different models when compared together, some characteristics of the distribution may be concealed. This is one of the major shortcomings of using simple arithmetic average. It is suggested to compare minimum and maximum values of the BIAS, for instance, and the difference will be clearer.

6/ In lines 23-27 the authors discuss the study of Szilagyi and Kovacs (2010) and they say that at the third EC site the CM-based model gave a difference of 44% in ET rates in comparison with EC measurements. Unfortunately, they do not tell the reason why, which when explained turns out to yield the best ET results of the three sites. As is discussed by Szilagyi and Kovacs (2010, 2011), at that site the EC instruments were installed on a radio-transmitter tower at a height of 82 (as in eighty-two) m above the ground. Under certain wind directions the instruments were in the wind-break of the tower making the method unusable in such periods. Consequently, the derived ET and sensible heat rates added up to 44% less than the energy balance. Accounting for it, the CM-based monthly ET estimates explained 95% of the variance found in the EC measurements, with practically no bias. And this leads us to the question of footprints. The 82 m height of the EC instruments above ground translates into a footprint really comparable to the scale of the CM-based ET estimation methods: most likely the reason for the best, unbiased performance in comparison with EC data.

Reply: In the original manuscript, there is no mention of such details (measurement heights of the other two towers) and whether there is a threshold to distinguish the validity of the complementary relationship. The authors used the results and information provided in the original manuscript. We will attempt to make these points clear in the revised manuscript.

Please see: lines 532-533 (page 25).

7/ The CarboEurope site (Bugac) from Hungary, listed in Table 1 has a measurement height of less than 2 m above the ground. I am not familiar with the other sites listed in Table 1, but I would risk to say that they may have comparable heights (i.e. a few meters). I ask the authors to list these values in Table 1. If I am correct then the footprints of the majority of these sites are just a tiny fraction of the scale the CM- based ET rates represent. Since surface properties, soil moisture status, vegetation may vary significantly at this fine scale (a few hundred meters) how representative are they then at the scale of the CM-based method? In my opinion a better validation would have been for the CM-variants to use water-balance data for the involved catchments.

Reply: The height of measurement is not mentioned for each and every EC site. In addition, the canopy height matters and therefore, there is an argument whether to use the height to ground or to canopy. In few cases, it was found that sensors are set at different levels making it even more difficult to decide which one to consider. To avoid confusion to the methods, this parameter was not mentioned. The focus of this study is to explore the validity

of the CM-based model variations globally under diverse conditions. The authors agree that some discrepancy in the results may be justified by this parameter while quality of data is of the greatest importance.

As for the water balance data, this is interesting; while this is beyond the scope of this work, the authors may consider this in future work.

Please see: lines 261-271 (page 13), lines 499-501 (page 24) and the updated Table 1.

8/ I wish the climate of Hungary were Mediterranean as Table 1 claims. It is still continental despite all climate change claims.

Reply: While we agree with this comment, the aridity index is a metric used in this study comparing all sites based on specified criteria using long-term average values of annual precipitation and temperature. It is understood that some of the aridity index values may not represent the actual climatic conditions of some sites. The authors made a comparison using other aridity indices (not shown here, namely De Martonne, Thornthwaite and Mather, Budyko and UNEP indices) and found that the climatic class significantly varied among the different aridity indices for the same site. The aridity index used here provided the most stable results. The authors are happy to share this information if needed.

9/ The winning GG18 variant is only slightly better than the original CRAE model. The R2 value is the same, the absolute BIAS value is 11 vs 15.7 mm/mo, and the RMSE value is about 20 vs. 27.8 mm/mo. Yet there is a big difference in input data require- ments between the two models: the former (GG18) needs wind velocity measurements, while the CRAE model does not (every other model inputs are the same). So the CRAE model performs almost the same as the GG18 model with fewer data input. Wind data is something not at all universally available historically. I still wonder if the GG18 model would outperform the CRAE model with the help of watershed water balance data.

Reply: One may compare the original GG method and the GG18 model and come up with the same conclusion. Similarly, the CRAE2 model is also comparable with the original CRAE method (see Figure 3). In the same way, many similar comparisons among the models can be performed but probably will not lead to a focused conclusion.

The comparison is not between overall average values given by the CRAE method and the GG18 model. There are some other statistics (e.g., standard deviation) that show the accuracy (or distribution) of the ET estimates among the 34 sites. As discussed in the present study, one of the main problems of the CRAE method is that it fails to estimate ET under sub-humid and Mediterranean climatic classes (see Table 2). The discrepancy is clear when compared to the more extreme conditions (Table 2). The GG18 mode, however, shows a better behavior among the 34 sites and results are more consistent regardless of the climatic class as shown in Figure 6. The ET estimates of the GG18 model for the moderate-climate sites are comparable to those of either the wet or dry climatic classes.

Please see: lines 554-568 (pages 26-27).

5. A list of relevant changes made by authors according to the comments of the Editor and the three Reviewers

The authors have indicated the changes made for the particular comments of the Editor and the three Reviewers each on his point-by-point response attached with the updated manuscript.

A list of all these changes is following:

Page	Line	Update
1	10	March 2014
4	73-74	Doyle, 1990; Hobbins et al., 2001; McMahon et al., 2013; Szilagyi and Kovacs, 2010; Xu and Singh, 2005
6	126-127	and the definitions of various terms are discussed in detail
7	136-144	In the literature, the complementarity relationship between ET and ETP shown in Eq. (1) is of controversy among scientists who claimed that many inherent assumptions of Bouchet theory lack sufficient evidence (Granger, 1989; Lhomme and Guilioni, 2006). Recently, there have been several attempts to improve the complementary relationship and its predictive power of different ET definitions (see Brutsaert and Stricker, 1979; Granger and Gray, 1989; Morton, 1983). Han et al. (2012) developed a nonlinear approach to the complementary relationship but the results require further study and verification. Yet, Lhomme and Guilioni (2010) proposed a different model that can describe the complex relationship between ET and ETP based on the convective boundary layer.
9	176-177	(see Hobbins et al., 2001)
10	203-206	In the GG method, the selection of the function to calculate relative evaporation (G) has great impact on the actual ET estimates and any modification to this empirical formula may be significant in improving the predictability of the GG method. In essence, there is more research required in this effort. Thus, Eq. (11)
11	215-218	Although the CRAE, AA and GG methods enable the direct prediction of ET without the need for surface parameters (temperature and vapor pressure), but the GG method is the only method that does not require
13	261-271	The EC tower heights vary from 2 m at site 24 to 103 m at site 14 with a median value of 10 m at site 7 and an average value of 17.1 m. The EC tower height reflects the vertical flux footprint that usually indicates the upwind area captured by the instruments mounted on the tower. Starting from very humid, humid, sub-humid, Mediterranean, semi-arid to arid climatic classes, the average EC tower heights are 24.8, 28.2,

		15.8, 10.2, 4.6 and 6.8 m, respectively. It is no surprise that the tower heights are highest in the very humid sites where the land cover is dominated by forests of high canopy altitudes. However, low tower heights are required for arid and semi-arid sites naturally characterized by grass or shrub land covers. The high range of EC tower heights explains the suitability of selecting these particular 34 EC sites that have flux footprints of the scale of the complementary methods. This observation may lead to the conclusion that a perfect correlation between the EC and complementary methods may exist.
17	343-355	Selecting the correct equations to calculate ETP, ETW and even ET may significantly influence the accuracy of the net radiation estimates. This work used the original model equations of the CRAE, AA and GG methods in different ways. This study is not meant to explore all possible relationships between ETP and ETW; instead the focus here is developing a reliable predictive model of actual ET that is applicable under a variety of climatic and physical conditions. Therefore, the relationships and model equations of the original methods were used here in a manner to preserve the physical processes controlling ET. Similarly, there are two formulae to describe the complementary relationship, namely equations (1) and (8). It is true that there may be other possible formulae to simulate the complementary relationship between ET, ETW and ETP. The drawback of these approaches is the need for calibration for which the revised model will be applicable for a given site or region. This condition is against the original purpose of this study that attempts to develop a model that is widely applicable for many different climatic and physical conditions.
19	391-396	Although ETP is usually given under saturated conditions by in the equation of Penman (1948) as shown in the original AA method, yet the definition of ETW still has some ambiguity (Lhomme and Guilioni, 2006). One important difference of the original GG method compared to the other two methods is the equation describing ETW. ETW of the original CRAE and AA methods is derived from the ET_{PT} equation (Eq. (7)) while the original GG method uses the ET_{PEN} equation or Eq. (5) (Brutsaert and Stricker, 1979; Granger and Gray, 1989; Morton, 1983).
22	454-461	The performance metrics (RMSE, BIAS and R^2) for the three model variations can be compared with uncertainty associated with observed EC-based fluxes to assess the overall accuracy of the methods. For example, Mauder et al. (2007)

		showed that RMSE and bias of LE sensors normally range from 38 to 61 mm/month and from -29 to 30 mm/month, respectively. In another study, it was found that EC data are comparable to weighing lysimeter ET measurements (Castellvi and Snyder, 2010) when the RMSE was 26 mm/month and R^2 was 0.98. These results indicate the high efficiency of the three model variations, namely GG18, GG20 and GG22, in predicting the actual ET.
24	499-501	It also indicates that there is no evidence that the flux footprint (EC tower height) plays a major role or directly impacts the accuracy of the results.
25	532-533	due to physical conditions at that particular EC tower (see Szilagyi and Kovacs, 2010)
26-27	554-568	The GG18 model is close to a "universal model" and shows better behavior among the 34 sites and the results are more consistent across the spectrum of climatic classes as shown in Fig. 6. The ET estimates of the GG18 model for the moderate-climate sites are comparable to both wet or dry climatic classes (Fig. 6), and those of the most recent ET studies (Table 6). None of the original (CRAE, AA and GG) methods, however, succeeded to estimate ET under sub-humid and Mediterranean climatic classes (see Table 2). The discrepancy is clear when compared to the more extreme conditions, i.e., dry and humid categories (Table 2). For example, one may argue that the average values of performance metrics of the GG18 model are slightly better than those of the original CRAE method that does not need wind measurements. The comparison cannot be made only between the overall average values given by the CRAE method and the GG18 model. There are other statistics (e.g., standard deviation) that show the accuracy (or distribution) of the ET estimates among the 34 sites. As discussed earlier, one major problems of the CRAE method is that it fails to estimate ET under sub-humid and Mediterranean climatic classes (see Table 2). Under the diverse physical and climatic conditions, the GG18 model variation is quantitatively and qualitatively outperforming all original complementary method.
27	570-582	One last concern is about the most proper temporal resolution of the GG18 model. It is known that the original AA and GG methods are usually used at daily timescale while the original CRAE method is typically used at monthly timescale. The goal of this study is to propose a universal ET model that can be successfully used for data deficit conditions under which daily data are missing or unavailable. It is believed that the regional estimates of ET entail monthly time resolution.

		Thus, the question now is whether applying the GG18 model at monthly timescale may change the parameters of the model used at daily basis or not. In order to answer this question, the proposed GG18 model was applied to a countrywide study of Ghana where daily data were available and climate varies from semi-arid in the north to tropical humid in the south (Anayah et al., 2013). The predictions using monthly data from 2000 to 2005 were very much comparable to the daily estimates of the GG18 model. These results suggested that the GG18 model can accommodate both daily and monthly time steps to produce consistent results. The reader may refer to Anayah (2012) and Anayah et al. (2013) for further details.
30	645-647	Anayah, F. M.: Improving complementary methods to predict evapotranspiration for data deficit conditions and global applications under climate change, Ph.D. dissertation, Utah State University, Logan, Utah, USA, 186 pp., 2012.
30	648-650	Anayah, F. M., Kaluarachchi, J. J., Pavelic, P., and Smakhtin, V.: Predicting groundwater recharge in Ghana by estimating evapotranspiration, Water International, 38(4), 408-432, doi: 10.1080/02508060.2013.82164, 2013.
32	689-691	Han, S., Hu, H. and Tian, F.: A nonlinear function approach for the normalized complementary relationship evaporation model, Hydrol. Process., 26, 3973-3981, doi: 10.1002/hyp.8414, 2012.
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36	Table 1	Height of every EC tower is added as Editor and Reviewers recommended and land cover of each site is updated
43	Figure 7	Updated according to recommendation of one of the Reviewers (attached)