Replies to reviewer 1 acp-2013-741-discussions

by F. Aemisegger, S. Pfahl, H. Sodemann, I. Lehner, S. I. Seneviratne, H. Wernli

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We would like to thank reviewer 1 for her/his constructive and detailed comments on the manuscript that helped us to improve our paper.

The reviewer's comments are repeated in normal case, our replies are given in italics. Specific comments line numbers are given for the online version of the manuscript. We address each comment point by point (see below).

1 General comments

1. [...] My main comment is that the authors should **better explain the limits of their method, and insist that it is not a definitive estimate, but rather an attempt, or a "proof of concept" of a method.**
   We fully agree that the approach we present in section 7 of this paper is only a first attempt to estimate the transpiration fraction of continental evaporation using the d-h_s *relation, also for individual events. This should be seen as a feasibility study. The main aim of this paper is to better understand the mechanisms behind variations of the continental water vapour deuterium excess signal at the daily timescale. We made this point clearer in our revision of the paper and added some explanations on the limits of our approach to estimate the transpiration fraction using the d-*h_s *relation in Section 7 (see in particular our reply to specific comment 15).*

2. I found the paper too long to read and I think the writing could be made more concise. Maybe focus it more exclusively on its main point (attempt to use surface water vapor isotopic measurements to estimate the proportion of continental recycling originating from transpiration).
   We do agree that this paper is rather long. We shortened the abstract as well as the conclusions. In the abstract we now address more clearly the uncertainty of our transpiration fraction estimation. In the conclusions we removed the paragraph on the discussion of the analysis dataset used for the trajectory calculation (p. 29751, l. 13-29). However, we do not wish to limit this paper to the sole aspect of the use of surface water vapour isotope measurements to quantitatively estimate the transpiration fraction of continental evaporation. This is not the only aim of this paper and it is necessary to fully outline our novel approach, which has a substantial level of complexity such that assumptions, limitations and need for further work can be clearly outlined.

2 Specific comments

1. p. 29725, l. 24-25: "and in which precipitation": this assumption is useless to derive the closure assumption. Besides, if we actually do this assumption and re-calculate the vapor composition based on it, we find a different result. I think this was a mistake in Merlivat and Jouzel 1979 that we don't need to repeat everywhere.
   We removed the second part of the sentence. The text is now as follows:
   "The relationship presented by Merlivat and Jouzel (1979) involves the so-called closure assumption of a climate in steady state, in which ocean evaporation is the only source of moisture for the atmospheric boundary layer."
   Furthermore, we now use "closure assumption" instead of "global closure assumption" throughout the paper.

2. p. 29726, l. 21: cite also Risi et al 2013b
   We added the reference.

3. p. 29728, l. 19: cite Landais et al. 2008
   Probably you mean p. 29727, l. 19., we cannot see a connection between the paper by Landais et al. (2008), which focuses on ^{17}*O-excess and the discussion here.*

4. p. 29728, l. 20: cite also the pioneering work of Moreira et al 1997.
   We added the reference.

5. p. 29736, l. 21: has h_g^ℓ, been defined already? l. 22-23: what is the connection between this sentence and the previous ones? What point are you trying to make?
   This is a mistake, it should be h_s^ℓ*, which has been defined on p. 29729, l. 25. The point in l. 22-23 is that we would like to mention that the differences in the distribution of the measured* h_s^ℓ *and* h_{2m}^ℓ *cannot be explained by the presence of a snow cover. We adapted the text as follows: "We can exclude any effect of snow cover on the observed difference between* h_s^ℓ *and* h_{2m}^ℓ*, as only a short period with snowfall not resulting in lasting snow cover occurred in December 2011."*

6. p. 29738, l. 25-27: It was not obvious to me at first sight why $d_s^r(h_s^r) = 100\%$ provides information on the moisture source d_s^r. Only when I wrote down the Merlivat and Jouzel closure equation and replaced h_s by 1 I convinced myself. I think the Merlivat and Jouzel closure equation should be written in the paper at least in an appendix, so that the readers who are not used to this equation can still follow the paper. The limit case where h_s=100% could also be explained there.
   We added an appendix in the paper with the closure assumption and an explanation of why $d_s^r(h_s^r = 100\%)$ *provides information on the moisture source* d_s^r *at the limit of* h_s*=100%. The appendix of the paper is reproduced in Appendix A of this reply.*

7. p. 297397 l. 1-7: this is true only if land evapo-transpiration includes some bare soil evaporation. Otherwise, the d-excess for ocean evaporation and evapotranspiration should be globally approximately the same.
   We added a note on this aspect: "In the warm season, the larger $\mathrm{d_s^r}$ *points towards much stronger continental moisture recycling and less large-scale advection of water vapour directly evaporated from the ocean (for which* $\mathrm{d_s^r} = \mathrm{d_o} \approx 0\,‰$*). Continental recycling increases d as the soil moisture* $\mathrm{d_c}$ *is generally higher than ocean* $\mathrm{d_o}$ ***if at least part of continental evaporation occurs in the form of soil evaporation and if there is loss of soil water by drainage. Otherwise,*** d ***for ocean evaporation and evapotranspiration are globally approximately the same."****.*

8. p. 29743, **a)** l. 26-27: why do you exclude the possibility that variations of d and h_s^r within a HRA event are due to shifts on geographical origin of the moisture? The fact that HLA and HRA have the same moisture source region doesn't exclude this possibility. What needs to be checked is whether the moisture source region remains constant within all HLA and HRA events. **b)** Similar comment p. 29741 l. 4—9: can't the $h_s^r - T_s^r$ anticorrelation be due to variations in moisture source regions?
   a) *There is a misunderstanding here: we mean that the average moisture source distribution over all HRA events does not strongly differ from the average moisture source distribution over all HLA events. We removed this paragraph, as it does not add much more information and may be confusing.*
   b) *On p. 29741 l. 16-19 we mention the possibility that the* $h_s^r - T_s^r$ *anticorrelation may be due to variations in moisture source regions: "Strong changes in the geographical location of the moisture sources over the time frame of a HRA event can be a further cause of high* $\mathrm{corr}_{5\,\mathrm{d}}(h_\mathrm{s}^\mathrm{r}, T_\mathrm{s})$*, if the large-scale advection pattern changes from a cold dry region to advection from a warm moist region."*

9. p. 29744, l. 7-10: "and whether... availability.": this is useless in the rationale and makes the paragraph more complicated that it is.
   We removed this part as recommended.

10. p. 29744, l. 14—17: this is a different effect that should have a different number → line jump before "Furthermore" l. 5: "threefold" → "fourfold".
    We changed the text as recommended.

11. p. 29744, l. 23-25: cite papers linking d to the type of weather system: e.g. Barras and Simmonds 2009, Guan et a1 2013.
    We added the references.

12. p. 29745 l. 5-6, and elsewhere: it is not obvious that continental recycling increases d. Rewrite the sentences to ensure that readers understand that it is not obvious. Continental recycling increases d only if (1) there is bare soil evaporation and (2) there is a loss of modified soil water by drainage. If (1) is not valid, then the d of the evapo-transpiration will be the same as that in the soil water and in the precip. If (2) is not valid then the d of the soil water will decrease until the d of the total evapo-transpiration equals that of the precipitation. This should be clarified somewhere.
    We changed the text as follows:

    - *p. 29745 l. 5-6: "...by repeated bare soil evaporation from soil water that typically has higher d compared to ocean water."*

    - *See also our reply to specific comment 7.*

13. p. 29746, l. 2—3: "slightly stronger": is it really significant? There are so few HRA and HLA events, couldn't such a small difference be due to luck?
    We changed the text as follows: "When selecting only HRA data with a direct contribution of ocean evaporation of more than 50 % the sensitivity of d to changes in h_s^r *is* $-0.28\,‰\,\%^{-1}$*. The continental moisture contribution for these events is still between 20 % and 50 %, which explains the weaker slope and the larger offset (*$d_\mathrm{s}^\mathrm{r} = 36\,‰$*) than in the study of Pfahl and Wernli (2008) (*$-0.52\,‰\,\%^{-1}$*)."*

14. p. 29746, l. 9: "HRA slopes" do you mean "HRA $d - h_s^r$ slopes" ?
    Yes, we changed the text as follows:
    "In summer the d*–*h_s^ℓ *slopes of the HLA events are similar to the HRA* d*–*h_s^r *slopes..."*

15. p. 29747, **equation 3: this is my more serious concern**

    - How this equation is derived should be explained. For example in an appendix, just after explaining the Merlivat and Jouzel 1979 closure as suggested above.
      We added the derivation of Eq. 3 in the Appendix B of the revised version of the paper and attached it to this reply in Appendix B.
    - All the hypotheses behind this equation should be better explained. I think the strongest hypothesis is that you assume same R_v and same h_s above all evapo-transpiration and ocean fluxes: this should be explained and a warning should be issued. Can you also discuss the expected effect of this hypothesis on your results? I expect that using same h_s above all surfaces will lead your equation overestimate the slopes, and thus you overestimate the transpiration component?
      on p. 29747 l. 3 we changed the text as follows:
      "The theoretical slope of the d*–*h_s *relation of continental evaporation (Fig. 11a) can be computed using the Craig and Gordon (1965) model under the following assumptions:*
      (a) *The only source for boundary layer water vapour is surface evaporation (closure assumption, see Appendix A for more details, Merlivat and Jouzel, 1979) A sensitivity study on how the closure assumption is applied (on each evaporation flux separately or globally) is shown in Appendix C. Furthermore, as mentioned in Sect. 1, the* d*–*h_s *slope may be overestimated when applying the closure assumption (Jouzel and Koster, 1996). This implies that we may in turn slightly underestimate the transpiration fraction with our approach.*
      (b) *Transpiration is assumed to directly transmit the signature of the soil moisture, which is assumed to be constant over the 5 day time period of an HRA or HLA event.*
      (c) h_s *and* R_v *represent average conditions at the moisture source for each time step."*

      The impact of the type of closure assumption is discussed in Appendix C of the paper and attached to this reply in Appendix C. The effect of using an average h_s *over all moisture sources is difficult to estimate and will be investigated in a future study.*
    - The α and k: symbols are defined differently compared to all previous papers on this subject. For clarity and consistency with all previous papers, **can you please use properly these symbols?** Normally:

    * α is the equilibrium fractionation coefficient, i.e. isotopic ratio of the liquid divided by the isotopic ratio in the vapor. It is >1.
    * $k = 1 - \frac{1}{\alpha_{\mathrm{K}}}$ where α_{K} is the kinetic fractionation coefficient (e.g. Merlivat and Jouzel 1979, Hoffmann et al. 1998).

  The notation is not as uniform in the literature as the reviewer suggests. Nonetheless, we now use the suggested notation. This means we changed the symbols in Eq. 3, Table 1 and adapted the numerical values in Table 1. Furthermore, we changed the sentence on p. 29748 l. 25 to "This value is very sensitive to the non-equilibrium fractionation factors and increases with larger $\alpha_{\mathrm{c}}^{\mathrm{k}}$*".*

  - I think this equation is wrong I tried to derive it myself and I found something different even using your wrong symbols *(For the detailed derivation of reviewer 1 please refer to her/his review).*

    You can check that this is different from your equation 3: the denominator is different, and you forgot the (1 — h) factor in front of Re. I guess you forgot to multiply both sides by (1 — h), plus some additional calculation errors. If you explain how you derive this equation in an appendix as I advice, you will reduce the likeliness of doing calculation errors.

    Please **correct this equation, and modify figures and tables accordingly**.
    As mentioned in our earlier specific comment on this issue, the discrepancy between reviewer 1's derivation and the one in our ACPD manuscript is not due to a calculation error but is due to a different treatment of the closure assumption. In his/her derivation reviewer 1 assumes that both R_v *and* h_s *are the same for all evaporation sites (ocean or continent). In the original version of our paper, we also assume that* h_s *is the same at all evaporation sites, but we apply the closure assumption for ocean evaporation, soil evaporation and transpiration separately, whereas reviewer 1 applies the closure assumption globally over all the evaporation sites. It is this specific treatment of the closure assumption and not a calculation error that leads to our equation 3 for the isotope ratio of the boundary layer water vapour in the ACPD manuscript. There are three possibilities to apply the closure assumption: 1) closure is applied globally for ocean and continental evaporation; 2) closure is applied for ocean and continental evaporation separately; 3) closure is applied for each individual type of evaporation (ocean evaporation, soil evaporation, plant transpiration). In the revised version of our paper we now use version 1) and adapted the numbers in the text. We added the derivation of Eq. 3 in the Appendix B of the paper and also included a sensitivity study concerning the application of the closure assumption involved. The result of this sensitivity study is shown in Appendix C of the revised version of the paper. The three new appendices are attached to this reply. The sensitivity of the transpiration fraction estimates obtained using Eq. 3 of the paper with respect to different non-equilibrium fractionation factors for soil evaporation is much more important than the way how the closure assumption is applied. The transpiration fraction estimates are affected by average uncertainties of 11% particularly for low transpiration fractions due to their strong dependency on the choice of the non-equilibrium fractionation factor.*

16. p. 29747, l23: the Merlivat and Jouzel 1979 is not necessarily a global approximation, see earlier comment on the uselessness of the global hypothesis.
    We now use "closure assumption" instead of "global closure assumption" throughout the paper. See also specific comment 1.

17. p. 29747 , l. 26: this was not the conclusion reached by Uemura et al 2008 based on observations. This is also not what I understood from Jouzel and Koster 1996. Read also Risi et al 2013b.
    We changed the text as follows:
    "[...] the d–h_{s} *slope* ***may*** *be overestimated with this assumption (Jouzel and Koster, 1996)"*
    We do not think that this sentence contradicts Uemura et al. (2008) or Jouzel and Koster (1996). We refer here to Jouzel and Koster (1996) p. 22938 and the discussion about the systematic bias caused by the "global-scale closure assumption ($\delta_v = \delta_e$*)" and their statement: "The closure assumption, however, exaggerates the sensitivity of deuterium excess to* T_e *and h."*

18. p. 29748 l. 17-18: How do you apply equation 3 to HLA and HRA events? How do you set f_{o}? Please explain this step better.
    We changed the text as follows on p. 29748 l. 6:

"To obtain the transpiration fractions for each HRA and HLA event, the theoretical linear relation between the d-h_s slope and the transpiration fraction is computed using Eq. 3. In the case of HRA events the average ocean evaporation fraction for each event is obtained from the moisture source diagnostics. For HLA events the ocean evaporation fraction is set to 0."

19. p. 29748 l. 23: is 0 to 99% a realistic range? I guess this range is sufficient to cast doubt on the method. A warning should be issued and the caveats of the method better discussed.
    The mentioned range is the one obtained for HRA events with closure assumed for each individual flux, which may indeed not be realistic. With closure assumed for total evaporation we obtain a more realistic range of 0-89% for the plant transpiration part. A sensitivity study showed that the transpiration fraction estimate depends strongly on the non-equilibrium fractionation factor used for soil evaporation, particularly at low transpiration fractions. Using $\alpha_\mathrm{c}^\mathrm{k}$ *for dry soils from Mathieu and Bariac (1996) we found a transpiration fraction range of 31%-92%. This is in agreement with other experimental studies (e.g. Yepez et al., 2003; Sutanto et al., 2012; Raz-Yaseef et al., 2012). We added Appendix C showing the results of a sensitivity study on this issue and changed the text as follows:*

    "The event-to-event variability of the transpiration fraction associated with the different HRA events is large and varies between 0 and 89 % with an average value of 62 %. These values are very sensitive to the non-equilibrium fractionation factors and increase with larger $\alpha_\mathrm{c}^\mathrm{k}$ *(see Fig. 11b as well as the sensitivity study in Appendix C). The sensitivity of the transpiration fraction estimates obtained using Eq. 3 with respect to different non-equilibrium fractionation factors for soil evaporation is much more important than the way how the closure assumption is applied. When using the non-equilibrium fractionation factor for dry soils of Mathieu and Bariac (1996) we obtain an average transpiration fraction for HRA events of 73% with a minimum value of 31%. Particularly for low transpiration fractions, when soil evaporation is strong, the choice of the non-equilibrium fractionation factor becomes very important."*

20. p. 29748 l. 25: can you be more quantitative on the sensitivity to kinetic fractionation? What is the range associated with this uncertainty? For example, how is affected the 63% value?
    See response to comment 19. The average difference between estimates of transpiration fractions, when using the non-equilibrium fractionation factors for wet or dry soils (Mathieu and Bariac, 1996) is 10% for HRA events and 4% for HLA events. The smaller difference for HLA events is due to the higher transpiration estimates obtained for HLA events. The choice of the non-equilibrium fractionation factor becomes particularly important at low transpiration fractions.

21. A main caveat of this method is that it neglects other possible sources of $d - h_s$ covariations. For example, when the boundary layer is deeper, it entrains more free tropospheric air that has a higher d, and lower h. This could explain the observed correlation and even maybe the slopes without even considering variations in f_t. Can you discuss this issue in the paper?
    In the case of HRA events the mentioned source of $d - h_s$ *covariation should not affect our analysis, since moisture source points are identified where air parcels take up humidity. Strong entrainment rather leads to a drying of the boundary layer. However, for HLA events this could be a plausible interference factor. This effect would have to be studied in more detail. It is clear that entrainment of free tropospheric air affects the daily cycle of water vapour isotopes in the boundary layer. We changed the text as follows on p. 29749 l. 4:*
    "Entrainment of free tropospheric air may be a further confounding factor, by bringing dry air with low h and large d values into the boundary layer. This effect is however thought to mainly affect the d signal at timescales < 24 h and may be studied in more detail by analysing the driving processes of the daily cycle in d."

22. p. 29749, 117: "regionally integrated": is it true also for HLA, or only for HRA?
    This is true only in the case of HRA events. We changed the text as follows: "For HRA events, this approach could be useful for model verification as it provides regionally integrated estimates of the transpiration fraction".

23. p. 29750, l. 1: do you mean "NRA" and "NLA"?
    Yes, we corrected this mistake.

24. p. 29750, l. 12: Pfahl et a1 2012 don't explain how to estimate a transpiration fraction from their model. Please explain.
We changed the text as follows: "In future research a comparison of the transpiration fraction estimates obtained here with traditional isotope mass balance methods as well as with estimates from regional climate models could provide further constraints for the robust modelling of the land surface evaporation components."

25. p. 29750, l. 18-19: "interception reevaporation small in summer": no, interception and (rain?) reevaporation are highest in summer.
Yes this was a mistake, we changed the text as follows: "Furthermore, the importance of other non-fractionating evaporation fluxes like intercept evaporation or dew evaporation should be investigated."

26. Conclusion: too long, for a paper that was already too long. The conclusion should be all the more concise as the paper is long. Focus on your main point. For example, p. 29751 l. 13-19: useless in a conclusion.
We removed p. 29751 l. 13-19.

27. Table 5, caption: how do you calculate f_{o}: from your moisture source diagnostics?
Yes, f_{o} is obtained from the moisture source diagnostic. We now mention this explicitely in the caption of Table 5.

28. Table 6: what values did you assume for f_{o}?
For the HLA events the ocean contribution is 0 as the moisture source is assumed to be local. We added this information in the caption of Table 6.

29. References: Salati et al. 1979, Gat et Matsui 1991, Risi et al. 2013 are relevant references for this paper.
We added theses references on p. 29727 l. 26 and now start the paragraph with the following sentence:
"Water isotopes have been used as tools to investigate continental moisture recycling in different studies in the past (e.g. Salati et al., 1979; Gat and Matsui, 1991; Risi et al., 2013)".

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A The closure assumption and d of the moisture source

According to the closure assumption of Merlivat and Jouzel (1979) the only source of vapour in the boundary layer is surface evaporation. The isotopic ratio of the evaporation flux R_e in this case equals the isotopic ratio of boundary layer vapour R_v:

$$R_\mathrm{e} = R_\mathrm{v}. \tag{1}$$

The isotope ratio of the evaporation flux following Craig and Gordon (1965) is

$$R_\mathrm{e} = \frac{\frac{R_\mathrm{l}}{\alpha} - h_\mathrm{s} \cdot R_\mathrm{v}}{\alpha^\mathrm{k}(1 - h_\mathrm{s})} \tag{2}$$

where α is the equilibrium fractionation factor, α^{k} the non-equilibrium fractionation factor, R_{l} the isotope ratio of the liquid water, h_{s} the relative humidity with respect to the surface temperature.

If the closure assumption (Eq. 1) is applied, Eq. 2 simplifies to:

$$R_{\mathrm{v}} = \frac{R_{\mathrm{l}}}{\alpha(\alpha^{\mathrm{k}} + h_{\mathrm{s}}(1-\alpha^{\mathrm{k}}))}. \tag{3}$$

For $h_{\mathrm{s}} = 1$ we have:

$$R_{\mathrm{v}} = \frac{R_{\mathrm{l}}}{\alpha}. \tag{4}$$

d is derived from $R_{\mathrm{v}}^{^{18}\mathrm{O}}$ and $R_{\mathrm{v}}^{^{2}\mathrm{H}}$ and is not substantially affected by equilibrium fractionation, thus $d_{\mathrm{s}}^{r}(h_{\mathrm{s}}^{\mathrm{r}} = 100\%)$ provides information on the average d of liquid water at the moisture source.

B Derivation of Eq. 3 for the estimation of the transpiration fraction of continental evaporation

When assuming closure for total evaporation, the heavy isotope ratio of the water vapour mix R_{v} containing moisture originating from ocean evaporation R_{vo}, plant transpiration R_{vt} and soil evaporation R_{ve} is

$$R_{\mathrm{v}} = f_{\mathrm{o}} R_{\mathrm{vo}} + (1-f_{\mathrm{o}}) \cdot [f_{\mathrm{t}} \cdot R_{\mathrm{vt}} + (1-f_{\mathrm{t}}) \cdot R_{\mathrm{ve}}]. \tag{5}$$

If we replace R_{vo} and R_{ve} by their respective expression using the Craig and Gordon (1965) model (Eq. 2) and assuming that $R_{\mathrm{vt}} = R_{\mathrm{c}}$ we obtain:

$$\begin{aligned} R_{\mathrm{v}} &= f_{\mathrm{o}} \frac{R_{\mathrm{o}}\frac{1}{\alpha} - h_{\mathrm{s}} R_{\mathrm{v}}}{\alpha_{\mathrm{o}}^{\mathrm{k}}(1-h_{\mathrm{s}})} + (1-f_{\mathrm{o}}) \cdot [f_{\mathrm{t}} \cdot R_{\mathrm{c}} + (1-f_{\mathrm{t}}) \cdot (\frac{R_{\mathrm{c}}\frac{1}{\alpha} - h_{\mathrm{s}} R_{\mathrm{v}}}{\alpha_{\mathrm{c}}^{\mathrm{k}}(1-h_{\mathrm{s}})})] && (6) \\ \Rightarrow \quad R_{\mathrm{v}} & [1 + f_{\mathrm{o}} \frac{h_{\mathrm{s}}}{\alpha_{\mathrm{o}}^{\mathrm{k}}(1-h_{\mathrm{s}})} + (1-f_{\mathrm{o}})(1-f_{\mathrm{t}}) \frac{h_{\mathrm{s}}}{\alpha_{\mathrm{c}}^{\mathrm{k}}(1-h_{\mathrm{s}})}] \\ &= f_{\mathrm{o}} \frac{R_{\mathrm{o}}\frac{1}{\alpha}}{\alpha_{\mathrm{o}}^{\mathrm{k}}(1-h_{\mathrm{s}})} + (1-f_{\mathrm{o}})(f_{\mathrm{t}} R_{\mathrm{c}} + (1-f_{\mathrm{t}}) \frac{R_{\mathrm{c}}\frac{1}{\alpha}}{\alpha_{\mathrm{c}}^{\mathrm{k}}(1-h_{\mathrm{s}})}) && (7) \end{aligned}$$

Dividing through the brackets on the left hand-side yields Eq. 3.

C Sensitivity of transpiration fraction estimate on closure assumption and non-equilibrium fractionation factor

The closure assumption can be applied to Eq. 5 in three different ways:

1. Overall, for the total evaporation flux, to obtain Eq. 3.
2. For continental and oceanic sources separately to obtain:

$$\begin{aligned} R_{\mathrm{v}} \;&=\; f_{\mathrm{o}} \frac{R_{\mathrm{o}}}{\alpha(\alpha_{\mathrm{o}}^{\mathrm{k}} + h_{\mathrm{s}}(1-\alpha_{\mathrm{o}}^{\mathrm{k}}))} + (1-f_{\mathrm{o}}) \left[f_{\mathrm{t}} \frac{R_{\mathrm{c}}(1-h_{\mathrm{s}})}{1 - h_{\mathrm{s}}(1 - \frac{1}{\alpha_{\mathrm{c}}^{\mathrm{k}}}(1-f_{\mathrm{t}}))} \right. \\ &+\; \left. (1-f_{\mathrm{t}}) \frac{R_{\mathrm{c}}}{\alpha\alpha_{\mathrm{c}}^{\mathrm{k}}(1 - h_{\mathrm{s}}(1 - \frac{1}{\alpha_{\mathrm{c}}^{\mathrm{k}}}(1-f_{\mathrm{t}})))} \right]. \end{aligned} \tag{8}$$

3. For each individual evaporation type, i.e. ocean evaporation, soil evaporation and plant transpiration separately, to obtain:

$$R_{\mathrm{v}} = f_{\mathrm{o}} \frac{R_{\mathrm{o}}}{\alpha(\alpha_{\mathrm{o}}^{\mathrm{k}} + h_{\mathrm{s}}(1-\alpha_{\mathrm{o}}^{\mathrm{k}}))} + (1-f_{\mathrm{o}}) \left[f_{\mathrm{t}} R_{\mathrm{c}} + (1-f_{\mathrm{t}}) \frac{R_{\mathrm{c}}}{\alpha(\alpha_{\mathrm{c}}^{\mathrm{k}} + h_{\mathrm{s}}(1-\alpha_{\mathrm{c}}^{\mathrm{k}}))} \right]. \tag{9}$$

The transpiration fractions for HRA and HLA events obtained from these three approaches differ by maximum 2% and 6% respectively. When using version 1 the average f_{t} is 62% for HRA events and 76% for HLA events; when using version 2 the average f_{t} is 61% for HRA events and 78% for HLA events;

when using version 3 the average f_t is 63% for HRA events and 82% for HLA events.

When using the closure version 1 and the non-equilibrium fractionation factors for wet (dry) soils from Mathieu and Bariac (1996), the transpiration fraction range for HRA events is 0%-89% (31%-92%) and for HLA events 65%-86% (73%-89%). Particularly for low transpiration fractions, when soil evaporation is important, the choice of the non-equilibrium fractionation factor becomes very important.