Review of Aemisegger et al

December 12, 2013

1 General comments

- The main point of this paper is an attempt to use surface water vapor isotopic measurements to estimate the proportion of continental recycling originating from transpiration. Estimating this proportion is an important goal. Although some estimates exist at the local scale, estimates at the regional scale are difficult, uncertain and contradictory. I'm not convinced that the method presented in this paper should give a correct estimate, as detailed below. However, this attempt deserves to be published, since another method not less uncertain has recently been published in Nature (Jasechko et al 2013). 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.
- 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).

-> I recommend to accept with major revision.

2 Specific comments

- 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.
- p 29726, l 21: cite also Risi et al 2013b
- p 29728, l 19: cite Landais et al 2008
- p 29728, l 20: cite also the pioneering work of Moreira et al 1997
- p 29736, l21: 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?
- 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.
- p 29739, l 1-7: this is true only if land evapo-transpiration includes some bare soil evaporation. Otherwise, the d-excess for ocean evaporation and evapo-transpiration should be globally approimately the same.
- p 29743, 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. 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?

- p 29744, l 7-10: "and whether ... availibility.": this is useless in the rationale and makes the paragraph more complicated that it is.
- p 29744, l 14-17: this is a different effect that should have a different number -> line jump before "Furthermore" + l 5: "threefold" -> "fourfold"
- p 29744, l 23-25: cite papers linking d to the type of weather system: e.g. Barras and Simmonds 2009, Guan et al 2013
- 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.
- 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?
- p 29746, l 9: "HRA slopes": you mean "HRA s$d - h_s^r$ slopes"?
- 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.
  - All the hyptheses behind this equation should be better explained. It 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?
  - 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.
    - $k = 1 - \frac{1}{\alpha_K}$ where α_K is the kinetic fractionation coefficient (e.g. Merlivat and Jouzel 1979, Hoffmann et al 1998)
  - I think this equation is wrong. I tried to derive it myself and I found something different, even using your wrong symbols: for example, in the simpler case where $f_T = 1$, using your wrong symbols:

$$R_v = f_0 \cdot R_{evap\,ocean} + (1 - f_o) \cdot R_{transpiration}$$

$$\Rightarrow R_v = f_0 \cdot \frac{k_o \cdot (R_o \cdot \alpha - h \cdot R_v)}{1 - h} + (1 - f_o) \cdot R_c$$

$$\Rightarrow R_v \cdot ((1 - h) + f_0 \cdot k_o \cdot h) = f_0 \cdot k_o \cdot R_o \cdot \alpha + (1 - f_o) \cdot (1 - h) \cdot R_c$$

$$\Rightarrow R_v = f_o \cdot \frac{R_o \cdot \alpha \cdot k_o}{1 - h \cdot (1 - f_o \cdot k_o)} + (1 - f_o) \cdot R_c \cdot \frac{(1 - h)}{1 - h \cdot (1 - f_o \cdot k_o)}$$

You can check that this is different from your equation 3: the denominator is different, and you forgot the $(1 - h)$ facor in front of R_c. 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**.

- p 29747, l 23: the Merlivat and Jouzel 1979 is not necessarily a global approximation, see arlier comment on the uselessness of the global hypothesis
- 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.
- 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.
- 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.
- 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?
- **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?
- p 29749, l17: "regionally integrated": is it true also for HLA, or only for HRA?
- p 29750, l 1: do you mean "NRA" and "NLA"?
- p 29750, l 12: Pfahl et al 2012 don't explain how to estimate a transpiration fraction from their model. Please explain.
- p 29750, l 18-19: "interception ... reevaporation ... small in summer": no, interception and (rain?) reevaporation are highest in summer.
- 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.
- Table 5, caption: how do you calculate f_o: from your moisture source diagnostics?
- Table 6: what values did you assume for f_o?
- References: Salati et al 1979, Gat et Matsui 1991, Risi et al 2013 are relevant references for this paper.

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