

Response to comments of H. F. Goessling

We like to thank Helge Goessling for his constructive comments, which help us to improve our manuscript. Below, detailed responses to all comments are given. Note that the Referee’s comments and specifically the page and line numbers obviously refer to the submitted pdf document, which slightly differs from the official ACPD version (there have been a few minor adaptations).

1. P4L13 and P4L: If I am not mistaken, Stohl and James (2004) applied their particle dispersion model forward in time. The methodological difference is subtle, but because elsewhere you are talking explicitly about backward trajectories (e.g. P2L6) I suggest to state this explicitly.

This is true and will be mentioned in the revised manuscript.

2. P4L2324: The term “numerical model simulations” seems a bit imprecise here: also the Lagrangian method involves “numerical model simulations”. The involvement of a full general circulation model is seemingly meant. However, there are also offline methods using Eulerian coordinates, such as those applied in Goessling and Reick (2011) and van der Ent et al. (2010), which largely share the disadvantages described here for the Lagrangian methods. The categories “Eulerian” and “Lagrangian” as discussed here seem to correspond rather to “online” and “offline” methods.

The term ‘numerical model simulations’ will be changed to ‘numerical simulations with complex atmospheric circulation models’. The offline Eulerian methods mentioned by the Referee are usually applied for obtaining regional moisture budgets on longer time scales, and not for single events as discussed in this paragraph (therefore they are not mentioned here). We would like to stick to our classification of ‘Eulerian’ and ‘Lagrangian’ approaches, as it is intuitive and directly linked to the methodology of our study. We will mention more clearly that ‘Eulerian’ always refers to the online tracer methods here.

3. P5L23: I suggest to replace “convective transport” by “advective transport”, because as I understand it in atmospheric science terminology the former includes (or even means only) turbulent (vertical) mixing due to dry and moist “convection”.

Convection has been mentioned in addition to turbulent mixing since there

are separate parameterisation routines for these processes. We will adapt the sentence as follows: 'On the way to precipitation the moisture tracers experience all processes of the atmospheric water cycle that are explicitly simulated or parameterised in the model, such as advective transport, cloud formation, convection and turbulent mixing.'

4. P6L4: *The primitive equations imply the use of the hydrostatic approximation, which is not used in COSMO. I would therefore remove the word "primitive".*

The term will be omitted.

5. P7Eq3: *I think it would help the reader to mention that $q_{sfc}^t = q_{sfc}$ for the tracer associated with the current location and $q_{sfc}^t = 0$ for the other tracers. Or is that incorrect?*

This has already been noted in the official ACPD version.

6. P8L2: *"mixing of tracers close to the surface". I think that "mixing of tracers between the atmosphere and the (sub-)surface" or the like would be more accurate.*

Since the first level of the vertical discretisation is some meters above the ground, also mixing in the lowest atmospheric layer below this first level is not considered in the Evap_tot approach. The sentence will be adapted as follows: 'In summary, the Evap_tot approach does not account for mixing of tracers close to the surface nor between atmosphere and surface, while ...'

7. P8L34: *"The strength of mixing in reality can be assumed to be intermediate between the two approaches" - I do not agree with this statement. My point of view is rather that the two methods provide different information. The Evap_tag method tells where the water molecules actually come from, whereas the Evap_tot method tells where a considered air parcel has been fuelled by net evaporation. I think that it is an interesting open question which of these flavours is more relevant when it comes to quantifying the dependence of precipitation somewhere to evaporation elsewhere. A short discussion of this issue is given in Goessling (2013), pp. 9597.*

Thank you for this comment. We will introduce a short discussion on this aspect and omit the statement on the 'mixing in reality'.

8. *First paragraph of Sect. 4 and first paragraph of Sect. 5: Do these*

paragraphs not fit better in the methods section?

We prefer to keep the description of the actual model setup in the beginning of the results section and describe only the general approach in the methods section, as the specific setup is based on the details of the event provided in section 3. Moreover, we find it easier to understand for the reader in this form, because otherwise she/he has to remember a lot of technical details of the two different approaches.

9. P11L10: *To me it is unclear what the term “statistical” shall imply here.* The formulation will be adapted as follows: ‘The results from the tagging simulation are evaluated with respect to this box, ...’

10. P11L23: *“... its impact on the event is negligible”. I would generally avoid this kind of phrasing where a causal link is implied. This touches upon the discussion brought up in Goessling and Reick (2011): it is unclear to what extent the source-sink relations of atmospheric moisture tell something about the sensitivity of precipitation somewhere to evaporation elsewhere. In this case I suggest something along the lines “its contribution to the precipitation associated with the considered event is negligible”. The subtle difference is important.*

We agree and will change the phrasing accordingly.

11. P13L1114: *This is an interesting remark that points to the fact that the causal link between evaporation and precipitation is more complex than could be accounted for by determining source-sink relations.*

This is true, and should certainly be a focus of future research activities. We will add a note to the manuscript stating that the source-sink relations diagnosed with our different approaches of course do not capture the full non-linear dynamics associated with precipitation formation.

12. P13L1718: *Please clarify that “close” is meant with respect to time rather than space (right?).*

This will be changed to “close in time”.

13. P14L1: *Please state whether the backward trajectories are isentropic or at constant pressure (or ...).*

We calculated three-dimensional kinematic trajectories; this will be mentioned.

14. P14L34: *I would be interested to know which fraction of all trajectories fulfills this condition.*

5% of all trajectories fulfil this criterion (roughly corresponding to the 3% of African moisture sources diagnosed with the Lagrangian approach).

15. P14L1921: *Again I think that a clearer distinction should be made between the quantification of source-sink relations and the causal link between evaporation and precipitation.*

This statement will be adapted as follows: 'In summary, the tagging experiment shows that both local moisture recycling through evapotranspiration from land surfaces and long range transport from the North Atlantic and western Africa constitute important moisture sources for the precipitation falling during the heavy rainfall event in eastern Europe in May 2010.'

16. P15L13: *How are the starting points distributed horizontally in the (25x25)km cell? Regularly? Randomly? But more importantly, why are they distributed vertically using equal pressure intervals rather than moisture mass intervals (i.e. weighted according to the profile of specific humidity as e.g. in Dirmeyer and Brubaker (1999))? Does the use of equal pressure intervals not introduce a bias towards higher-level moisture as only a small fraction of the moisture resides there?*

In the horizontal, starting positions are distributed on a regular grid. In the vertical, the distribution on equal pressure intervals ensures that every trajectory represent the same total mass. The source contributions from different backward trajectories are weighted according to their contributions to the total precipitation, which is estimated based on the moisture decrease during the last trajectory time step (a note on this will be added to section 2.2). Therefore, no bias to higher-level moisture is introduced.

17. P15L1718: *Is it not astonishing that the magnitude of uptakes is almost as large above the ABL compared to within the ABL? Could this be a hint that there is indeed a bias as suggested in the previous comment, or is there a different explanation?*

The contribution of above ABL uptakes indeed is relatively high in this case (compared to other events that have been studied with the same method). Most probably, this is due to a rather strong convective activity over the continent and the Mediterranean Sea. This is particularly evident for the

moisture contributions from tropical Africa (see Fig. 7).

18. *P17L2425: What is meant by “completely independent”? After all, the same thing shall be quantified, and the methods work on the same physical fields.*

The formulation will be adapted to 'Given that the Lagrangian diagnostics is methodologically and conceptually different from the Eulerian tagging approach, ...'

19. *P17L2729: Can you explain why the results of the Lagrangian method tend to be between the two Eulerian variants? I would have expected that the Lagrangian approach yields results closer to Evap_tot because the Lagrangian approach diagnoses net rather than gross surface fluxes, right?*

The Lagrangian approach diagnoses net evaporation and thus is closer to the Evap_tot in a conceptual sense, this is correct. We think that the results in between the two tagging realisations are closely related to the fact that in the Lagrangian diagnostic moisture uptake is directly linked to surface evaporation from the same location. As an example, consider the trajectory shown in Fig. 13: there is a lot of moisture uptake in the last 48 hours, over the Mediterranean Sea and the European continent. The tracer distribution shows that part of this moisture originates from the North Atlantic, a fact that cannot be captured by the Lagrangian concept. Most probably, this leads to an under-representation of remote moisture sources and thus shifts the source distribution towards the Evap_tag approach.

20. *P21L15: For my taste the sentence starting with “Between” and the subsequent one do not belong into the conclusion and could be omitted.*

Sentences will be omitted.

21. *Tab1: I think this table can be omitted as it conveys the same information as Fig. (10) but in a less beautiful way.*

We would like to keep the table, as it summarises the numerical values which are quoted at many different places in the text. Since these are the most important quantitative results of our study, we think that this small redundancy can be justified.

22. *Fig1: “atmospheric tracer”. I think it would help to state again in the caption that the atmospheric tracer is the one initially contained in the*

atmosphere.

This will be added.

23. Fig2: The right column has “RH” (relative humidity) in the title which should be “q” (specific humidity).

Will be corrected.

24. Fig8: Here I suggest to add a box that indicates where the target region is located.

Box will be added.

25. Technical corrections:

P4L29: With “Validating such ...” I recommend to start a new paragraph.

P7L5: “A positive values ...” - remove “A”. P15L19: Replace “too” by “two”. P17L11: “... between 14% ...”. Something seems to be missing here. P19L3: “area source”. Should these words be swapped? P19L6: Please remove “s” from “concepts”. P23L20: “Schar” has its umlaut points in the wrong place.

These corrections will be implemented.