egusphere-2026-3611 - Sensitivity of time-lapse magnetic resonance sounding to vadose zone hydrodynamic parameters: monitoring of an intense meteorological event

Response to the Anonymous Referee #1

We wish to thank the reviewer for the numerous constructive comments and suggestions, which have significantly improved the quality of our manuscript. Below is a detailed point-by-point response to each comment. Our responses are highlighted in blue, while excerpt from the manuscript are highlighted in *italic*, and the modifications in the manuscript are shown in ***bold italic***.

Guillaume Gru,

On behalf of all the authors

Referee comments 1 - egusphere-2026-3611

The study is interesting and presents a useful hydrogeophysical framework to assess the sensitivity of time-lapse MRS signals to vadose-zone hydrodynamic parameters during a transient infiltration event. The combination of hydrological modeling, MRS forward modeling, and global sensitivity analysis is valuable, and the results could help guide future monitoring and inversion strategies. However, I think several points need clarification before the manuscript can be fully convincing.

1. The title mentions “monitoring an intense meteorological event” but the manuscript only performs numerical simulations and sensitivity analyses.

We agree that the original title did not accurately reflect the scope of the work. Following the suggestion of Anonymous Referee #2 (who raised the same issue), we have revised the title to: **"Sensitivity of time-lapse magnetic resonance sounding to vadose zone hydrodynamic parameters during an intense meteorological event."**

2. The abstract should be more specific about the main findings. In particular, the authors should state which parameters can potentially be determined or better constrained from the experiment, and which parameters appear to have little influence on the MRS signal. The current abstract states that influential and non-influential parameters are identified, but it does not name them.

We have revised the abstract to explicitly name the influential and non-influential parameters.

Changes made:

1. L11-12: *Our results identify the parameters which exert the strongest control on time-lapse MRS signals* ***(the saturated water content and hydraulic conductivity, the pore distribution index n, and the MRS undetectable water content for the highest saturation)*** *and reveal how their influence changes during the infiltration event.*
2. L13-14: *We also identify parameters whose uncertainties have insignificant contribution to MRS signals* ***(the residual water content, the inverse air-entry value α, and two other parameters related to MRS undetectable water content)***.

With these modifications, the abstract now clearly states which parameters exert the strongest influence on MRS signals and which have little influence and thus are unlikely to be well constrained by MRS observations.

3. The intense meteorological event should be described more precisely. The authors should specify the exact dates, and if possible the timing, of the main event. It would also be useful to state how intense this event was compared with previous years or with the long-term record available at the Strengbach catchment. This would better justify the use of the term “intense” or “extreme.”

The paragraph describing the meteorological event was indeed not clear enough on the timings and intensity of the hydro-meteorological event. We incorporated the dates and timing of the main event, and additional information to explain how exceptional this event was.

Changes made :

1. We moved the sentence *Day zero of the numerical experiment is set to the 26th of - January 2021* to the beginning of the paragraph (L175-176) to avoid ambiguity between dates and day number in the rest of the manuscript.
2. L176-178: *at the end of January and early February* -> ***between the 29th of January 2021 at midnight and the 4th of February at 10 a.m.*** (Fig. 2d). ***The water table reached 0.77 m below the surface, which is the highest level recorded over the period 24 October 2020-12 December 2025.***
3. L180-181: *Firstly, a strong and long-lasting rain event: effective precipitations were higher than* $20\ \mathrm{mm.d}^{-1}$ *at most and reached* $107\ \mathrm{mm.d}^{-1}$ *cumulated over a period of 12 days* ***starting on the 27th of January***.
4. L182-184: *Secondly, it is due to the accumulation of a snow cover before the rain event: the measured snowpack height reached 0.80 m.* ***This is the maximum value recorded since the installation of the snow depth sensor in 2015, followed by 0.76 m in winter 2018-19 and 0.57 m in winter 2015-16.***

4. The event appears to involve rainfall combined with snowmelt. The authors should explicitly clarify whether this event should be considered a rain-on-snow event, and discuss how snowpack accumulation and melt contributed to the infiltration pulse and water-table response.

We have updated the terminology in the manuscript to explicitly characterize the nature of the event and added quantitative information regarding the effects of snowmelt.

Changes made:

1. L175: We replaced the term *meteorological event* with ***rain-on-snow event*** to provide greater clarity regarding the specific hydrological processes involved, as suggested by the reviewer.
2. L184–187: We have expanded the discussion to explicitly address the contribution of snowmelt to the infiltration pulse. We now state: *We notice this coupled effect by comparing Fig. 2b and Fig. 2c: during the period from day one to day 12, the modeled infiltration flux is larger than the effective precipitation.* ***Notably, on day seven, the infiltration flux was more than 10 mm/d larger than the effective precipitation, highlighting the substantial contribution from snowmelt.***

This addition explicitly quantifies the maximum impact of the snowmelt contribution.

5. The vertical discretization of the hydrological model should be specified.

We thank the reviewer for noticing this omission. We added the description of the HYDRUS subsurface model discretization L136-137 : ***The vertical profile $z \in [0,7]$ m is discretized into 701 nodes with a homogeneous spacing of $\Delta z = 0.01$ m.***

6. Some parts of the theoretical framework are described in too much detail for the main manuscript. The detailed descriptions of AMAE/AMAV, Sobol indices, Polynomial Chaos Expansion, and the numerical resolution of Richards' equation could be shortened in the main text.

We think that the relevant information should be given, as potential readers may not be experts in this field. We hope we provide enough information to get some intuitive understanding.

7. The authors should explain how they determined that 7000 simulations were sufficient for the sensitivity analysis.

The original text presenting the number of simulations was indeed not clear enough regarding the number of simulations we ran to compute the GSA indices, and how we verified that this number was enough. We actually generated a total number of 10 000 simulations : at first 3000 to train the PCE (which was enough as confirmed by the LOO validation error), and then 7000 additional simulations to ensure the stability of the AMAE and AMAV indices. The latter was verified by ensuring that the maximum variation of both indices was limited to 0.011 (AMAV, q_2), and 0.002 (AMAE, q_1) between the estimation with 8000 and 10 000 simulations.

Changes made:

L342-347: *We base the computation of Sobol indices on the coefficients of the PCE. We produced 7000 additional simulations* ***(with the physically based forward model)*** *to compute AMAE and AMAV indices using Eq. (11) and Eq. (12).* ***We verified that the number of simulations was sufficient to train the PCE with the LOO validation error. Concerning AMAE and AMAV indices, we assessed the stability of the indices by computing them for a subsample of 8000 simulations and checking the maximum difference between the derived sensitivity indices for this subset and the full simulation set. This maximum difference was of 0.002 for AMAE (pulse q_1), and 0.011 for AMAV (pulse q_2).***

8. The manuscript should better justify the choice of the five pulse moments used in the MRS simulations.

We added the justification of the pulse values variation interval which was indeed missing. The choice of the number of pulse was justified L231-232 (*This representative subset was selected to ensure a thorough GSA while maintaining computational efficiency*), and the choice of logarithmic spacing was justified L233-234 (*This logarithmic pulse moment spacing is typical for MRS acquisition sequences*).

Changes made :

L237-239 : ***The pulse values variation interval was designed to focus the sensitivity of the numerical MRS experiment within the depths $z \in [0.7, 20.0]$ m. These depths represent the target of a time-lapse MRS experiment to study both the unsaturated zone and the relatively thin aquifer located in the colluvium zone of the Strengbach catchment.***

9. The discussion of parameter interactions should be clarified. Why do the interactions depend on the pulse moment? Which parameters interact, and are these interactions physically meaningful or negligible?

We have expanded the discussion on parameter interactions to clarify their dependence on the pulse moment. We now explain why the interactions are relatively stronger for lower pulse moments. This is because lower pulse moments investigate the unsaturated zone, where the non-linearities in the Mualem-Van Genuchten model induce interactions between parameters. However, our investigation of second-order Sobol indices reveals that these interactions are composed of many weak pairwise interactions. Specifically, the maximum pairwise interaction index is low ($S_{2,i,j} < 0.02$). Therefore, while these interactions are physically rooted in the unsaturated zone physics, they are negligible in terms of their contribution to the overall signal variance.

Changes made :

L495-502 : *Finally, the sum of first-order Sobol indices is always close to unity, indicating that parameter interactions have a negligible contribution to the variance of the MRS signals.* ***The interactions between parameters are stronger for the lower pulse values (q_1 and q_2) because the depths investigated by these pulses are unsaturated during most of the experiment, inducing interactions between Mualem-Van Genuchten parameters. As noted with the investigation of the second order Sobol indices, the contribution of interactions among parameters is composed of multiple weak pairwise interactions. These interactions are not physically meaningful in terms of variance contribution, as the maximum pairwise interaction index remains low ($S_{2,i,j} < 0.02$).***

10. The added value of MRS compared with classical hydrological measurements should be made clearer. Which parameters can MRS specifically help improve or constrain? If some parameters can already be estimated from classical hydrologic observations, then what additional information does MRS provide?

We thank the reviewer for suggesting a stronger emphasis on the added value of MRS data compared to classical hydrological measurements. We have expanded the conclusion section to highlight these advantages, using our GSA results to demonstrate which parameters MRS has the potential to specifically constrain and why this is valuable for hydrological studies.

Changes made:

L542-547 : ***The significant sensitivity of MRS signals to θ_s and K_s (due to the important water table rise) in the saprolite layer indicates that time-lapse MRS experiments have a strong potential for constraining these parameters at depths which are hardly reachable for classic hydrologic measurements like water content or matric pressure sensors ($z > 2$ m). Furthermore, the stronger pulse moments yield valuable information on the water content***

stored in the fractured bedrock. This makes MRS an interesting non-destructive alternative to borehole drilling, particularly for characterizing deep or inaccessible hydrogeological units.

11. The limitations of using a 1D instead of a full 3D hydrological model should be discussed more explicitly.

This is a usual assumption for vertical infiltration modeling. We consider here that the water fluxes are mainly vertical *i.e.* water content modifications are due to vertical water movement. We agree that this assumption might be strong close to the groundwater surface but, since the water content is close to saturation in this zone, 1D or 3D considerations will not provide significant changes in the time evolution of that variable. Furthermore, the considered loop is located on a single pedological unit, therefore we assume that the scale of the lateral heterogeneities is higher than the scale of the MRS measurement footprint. We have added a discussion of the limitations of using a 1D hydrological model after introducing the equations.

Changes made :

L125-127 : ***While the assumptions of the 1D hydrological model (water content variations are due to vertical water movement) might be strong close to the groundawter surface, 1D or 3D modeling are not expected to provide significant changes in the transient evolution of that variable. Indeed, the water content is close to saturation in this zone.***

12. Another important limitation is that the simulated MRS signals are not validated against actual time-lapse MRS observations during the 2021 event.

Global sensitivity is not constrained by observation, which makes its application quite general. The models used to simulate water infiltration and MRS signals are very common and have been validated over many test cases. However, we understand the reviewer’s concern. The MRS signals for numerous parameter sets were consistent with the observations. Based on this sensitivity analysis, we are planning to perform model calibrations, but this is out of the scope of this paper.