Dear Prof. Persson,

With reference to the paper: **hess 2019-149**, by A. Comegna et al., please find below the replies to your review.

The authors would like to thank Prof. Persson for his useful suggestions which have been **fully accepted**. We explain below how the revised paper was reorganized.

- Major Comments:

1. …**and modeling** was removed from the title. Moreover, in order to eliminate any misunderstandings about the development of a new dielectric mixing model (we have only rewritten it for our purposes), we substituted in the text (line 18) the term *develop* with *calibrate and validate*, and (line 66) the term *build* with *revisit*.
2. In accordance with your comment we emphasized in the text (**introduction section**: lines 45-68) the novelty of the present research.
3. You are right with reference to the possibility of investigating, during remediation, the dielectric response of an initially four-phase medium (i.e. soil+NAPL+water+air), but (as you already wrote in your review) the present research is a first attempt to monitor in real time (with TDR) the dielectric response of the medium during a decontamination process. Thus we chose a simple initial scenario to avoid possible dielectric "interferences" related with other phases. This aspect could be explored in further research (a specific sentence regarding this possibility was introduced in the conclusion). Anyway to carry out our research we followed the approach of **Francisca and Rinaldi (2006)**, who published a paper entitled*: Removal of immiscible contaminants from sandy soils monitored by means of dielectric measurements* (doi: 10.106/(ASCE)0733-9372(2006)132:8(931)).
4. I agree with you that the dielectric response of a multiphase medium depends not only on the NAPL (and eventually water) volumetric content, but is also influenced by their internal distribution; **In accordance with your comment we sought to emphasize this aspect in the text** (lines 206-209) . Moreover I would like to stress the fact that TDR (as you already know) cannot allow us to infer how fluid distribution affects dielectric measurement; this aspect could be a further research topic, which should be developed by coupling TDR with different geophysical methods, such as the *Gamma Ray Attenuation* technique, that gives more accurate information on fluid distribution within the contaminated soil sample.

- Technical corrections:

1. In accordance with your comment we changed *diverse* to *different* (line 14) and *diverse* to *varying* (line 15).
2. In accordance with your comment we substituted in the abstract (and where possible in the whole manuscript) the terms *hydrocarbon* and *oil* with NAPL.
3. In accordance with your comment we introduce in the paper the dimensions of the TDR probe (line 136).

4. In accordance with your comment we introduced in the text the dimension of the volumetric NAPL content θ_{NAPL}: m^3/m^3 (line 19).
5. In accordance with your comment we better commented in the paper how was the oil content determined on the effluent (lines 148-150) and in agreement with the actual description, we modified figure 1.
6. In accordance with your comment we better describe how the α parameters were determined (lines 187-188). Furthermore, we made some new comments in the paper regarding the *calibration and validation* data set that we employed for model calibration and validation (lines 151-152). Finally, we introduce in the text (line 143) the term **initial**, in order to specify that: θ_{NAPL}=0.15, 0.20, 0.25, 0.30, 0.35 and 0.40 was the volumetric NAPL content at the beginning of the different experiments conducted.
7. With reference to parameter α in the dielectric mixing model adopted, I would like to emphasize that α, in our application is a pure fitting parameter, obtained from the calibration data set. This means that for a fixed θ_{NAPL} value and washing solution, the dielectric model was fitted (using a least square algorithm) to the whole set of experimental calibration data (i.e. the data obtained from the beginning to the end of the remediation test). For this reason α must be considered constant. This aspect is now commented in the **Model calibration and validation** section (lines 187-188).
8. In accordance with your comment we introduce a series of 1:1 scatter plots (figure 5a, b, c, d,e, f).
9. See comment #6.
10. In accordance with your comment we elaborated the section **Model calibration and validation**.
11. No more comments can be made in the manuscript with reference to parameter α for the reasons of comment #6.
12. In accordance with your comment we revisited the conclusions.
13. In Table 1 the α=0.05 value for wda#2 and θ_{NAPL}=0.20 was wrong. Thank you for your observation. The correct (α=0.45) value was inserted.

PS: following the Journal submission procedure, the revised version of our paper will be uploaded after the interactive discussion session has been closed.

Sincerely

The authors