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Interactive comment on "The electrical self-potential method is a non-intrusive snow-hydrological sensor" *by* S. S. Thompson et al.

Anonymous Referee #2

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This paper studies the possibility of using self-potential (SP) measurements to estimate the in situ water content of snow pack. Indeed, the method has the advantage of being low-cost, rather easy to implement, and passive. Through two field case, the authors try to answer three questions: 1) is SP a good "sensor"?, 2) how the water estimated water content is sensitive to snow properties, and 3) what about the future? I suggest to remove objective 3, which is not really an objective, but the perspectives that conclude a scientific communication.

The introduction and the objectives are clearly explained, as well as the brief description of the SP theory in the case of snow (based on previous wroks by Kulessa et al., 2012). From equation (3), it is clear that the SP signal strongly depends on snow prop-

erties, such as water saturation, conductivity, pH (through zeta), permeability, among others. The relation between the measured electrical potential and the water content is thus absolutely not straightforward, all the more as these properties may be not well determined - and this is the difficulty of the question.

To test the SP methos, the authors performed two experiments in two natural sites, where the snowpack has encountered significant melting. The protocole are well described. Some results are given in figure 2 (discharge and SP): if discharge clearly evolves with time, the correlation with the SP signal is not so clear, whereas equation (3) predicts a linear relation, if all other parameters are kept constant. Would it be possible to add a subplot SP vs. Q, to evidence a correlation (or not)?

For applying equation (3), all parameters were recorded or estimated with well-known relations. The main difficulties is the estimation of the zeta potential, which strongly changes with pH and conductivity. I am somehow confused with the method used here. Indeed, it seems that the authors chose the value of zeta so that equation (3) gives a value for the water content in agreement with the measured value (see Figure 3). To my mind, this is not modelling, but trials and errors. For a better understanding, I suggest to add a new graph superimposing in the different Sw curves predicted by equation (3) for different values of zeta.

The section about the sensivity is not clear and somehow hard to understand. In particular, the sense of figure 4 is unclear to me. What was the method? For a considered parameter, all the others were kept constant at their average value, and Sw was estimated with the maximal and minimal value of the considered parameter??? If yes, it provides uncomplete estimate. The N parameters should varies together... This part should be reconsidered and rewritten for clarity.

The conclusion present the future works to be achieved in order to make SP a routine method. To my mind, the most important is the laboratory study of the zeta potential in function of snow properties...

To conclude, the subject is of interest for snow hydrologist (and geophysicist), and the in situ experiments are original. The fitting approach provides a correct value of the water saturation, but strongly enhances the need for knowledge about the zeta potential values in function of snow properties. The weakest part is the sensitivity analysis, which deserves rewritting.

p3 line 9: "modelling" (i.e., SP equation 3) instead of "numerical modelling"

p10 line 7: is the small, negative value of zeta determined by Sw fitting coherent with what we know aboutn the pH and the conductivity?

Figure 2a: The spatial variability of the SP measurements is well estimated by averaging each profile. The value of this variability are in the classical ranges for the Rhone glacier, but it rather high for the Jungfraujoch. How this differenc can be explained?

Interactive comment on The Cryosphere Discuss., 9, 4437, 2015.