

Review of the Article hess-2016-474

Monitoring soil moisture from middle to high elevation in Switzerland: Set-up and first results from the SOMOMOUNT network

by C. Peller and C. Hauck

This paper presents soil moisture observations collected along an altitudinal and climatic transect in Switzerland. The soil moisture dynamics is discussed (mostly qualitatively) with respect to elevation, soil properties and climate.

General comments:

I have mixed feelings about this paper.

On the one hand, it is a very nice paper to read. The soil moisture observations and the calibration procedures are well presented, in a didactic way. Observations are also interpreted in a clever way, the reasons of the differences among the stations clearly identified. At the end, the paper provides a good conceptual scheme to understand the role of elevation in controlling the yearly soil moisture cycle. I'm working with a similar soil moisture dataset collected in an Alpine region, and I recognize similar trends.

On the other hand, most of the results are based only on qualitative interpretations, and the paper does not add very new concepts on what is already known on the soil moisture behavior in cold/mountain climates.

I also share the doubts of the first reviewer on the unreliability of soil moisture observations when the soil is frozen. It is not so straightforward assuming that soil immediately freezes with negative soil temperatures. Soil could remain unfrozen in small pores also well below 0 C. Also assuming that TDR devices correctly detect low soil moisture when the soil is frozen it is also not so obvious. All the discussion is quite misleading on this point. You cannot consider the same situation having low liquid (but high total) water content due frozen soil and having a low total water content because the soil is dry. Those are completely different situations of low (liquid) water availability, and this should be clearly differentiated in the discussion.

Moreover, reading the introduction the paper seems here really focused on permafrost. Also the broad literature review on mountain SWC dynamics is biased toward permafrost. Then, later in the results and discussion sections, the topic permafrost is barely covered. I suggest a broader motivation. There are other important reasons to monitor mountain soil moisture, runoff production, climatic impacts, vegetation seasonality ...

Nevertheless, I would recommend publication after a careful revision, since the paper could provide useful guidelines on how to interpret soil moisture observations in Alpine regions.

Specific comments:

Abstract

Please add more concrete results on which are the main soil moisture patterns.

Introduction

Line 25-30. The paper seems really focusing on permafrost. Then, later in the results section, the topic permafrost is hardly covered. I suggest a broader motivation. There are other important reasons to monitor mountain soil moisture, from runoff production to climatic impacts to vegetation seasonality.

Instruments

This section is too long, with a long revision of the advantages and disadvantages of different measurement techniques and many details on the calibration procedures. This part could become shorter. However, I found this part useful. I learned something new.

Network design

Line 25-30. This part could become shorter.

Field sites

Pages 5-6-7 This part could also become shorter, moving more details to Table 3.

Soil moisture temporal evolution. This paragraph is very qualitative, but well written. However, I share here with Reviewer 1 some doubts on the methodology.

It is not so straightforward assuming that soil freezes when soil temperatures are negative. Soil could remain unfrozen in small pores also below 0. Also assuming that a TDR device measures low soil moisture when the soil is frozen it is not so obvious. See the comments of the other reviewer on this point.

Page 11, Line 34. *“During the snow melt period only a small VWC increase is seen, which could be attributed to conditions close to saturation throughout the winter.”* Interesting observation, but it would be nice to quantify better the impact of snow melt on initial VWC in spring. It is a relevant research question in the context of climate change.

Page 12, Line 5. Simply stating that VWC is minimum is not correct. The water is in the soil, but likely frozen. It is more correct to specify minimum liquid water content. Therefore, the whole the discussion is quite misleading. You cannot consider in the same way having low liquid (but high total) water content due frozen soil and having a low total water content because the soil is dry. Those are completely different ways to have low liquid water content.

Soil moisture spatial distribution. This paragraph is not very informative. It only informs us that there is (as expected) a large spatial variability, and where falls more rain is more wet. I suggest to skip or strongly reduce.

Page 14, Line 28. How thick is the organic layer? What about organic soil in the other sites? Soil type dependency is not discussed for all the sites. Why?

Page 15, Line 4. Do you mean Figure 10?

Page 16, Line 3 and line 20 *“From Fig. 6c, it can be seen that, at the time of the precipitation event, the VWC at 10cm and 30cm depths were unusually low, enabling the water to pass through easily.”*
“As for MLS above, the VWC is low at all depths enabling the precipitations to infiltrate quickly down to the deepest layers”

Those statements apparently contradict hydrologic theory. In fact, larger is the water content, larger is the soil hydraulic conductivity and therefore larger is the amount of water that can infiltrate.

Please motivate your statements. Do you refer to the speed of the infiltration front or to the total amount of water infiltrated? (i.e. see Green – Ampt theory?). It is not only because if the soil is already wet you do not see a big change in soil moisture, but the infiltration amount is already significant?

Page 16, Line 18. Do you mean Figure 10?

Altitude dependency. This part of the paper is well written and informative, but very qualitative. This altitudinal dependency is already known in literature. Even if it is important to show how the new data published in this paper follow or not follow what is known in literature, a more robust quantitative analysis would be beneficial for the paper.

Page 16, Line 28 . *“Below 2000 m.a.s.l. the maximum VWC is recorded in winter and the minimum in summer, whereas above this threshold the inverse occurs (maximum VWC in summer and minimum in winter).”*

This refers only to the liquid water content. The total (frozen + unfrozen) remains high all the winter even at high elevations. Please specify this better.

Page 17, Line 5-6 and following. Ok, such processes are somehow clear, but ... what are the implications for SWC?

Page 17, Line 18 *“This is confirmed by the observed negative ground temperatures as well as the increasing freezing degree days. With increasing elevation, air and ground temperatures decrease yielding increasingly long duration of seasonally frozen ground and thus explaining the decreasing trend of VWC”*

Could you qualitatively estimate a functional relationship? Which is the temperature threshold? (I have seen that later the paper partially answers to those questions ...)

Page 17, lines 30 and followings. Such “frost holes” are geological peculiarities well known in other places of the Alps (I.e. Kaltern, Italy; Lases, Italy). Their effect on SMC is interesting. May be there is more in the geological literature.

Page 18, lines 25-32. Well written, but it could be shortened. Too many unnecessary details.

Conclusions In general, I see here nothing on the implications on permafrost, which seems to be one of the major motivations of the paper. Either downplay this in the introduction, or develop more the permafrost topic in the discussion/conclusions sections.

Page 19, lines 5-12. This part should be moved to the method section. It partially addresses the major objection of Reviewer 1. You say “*Although the sensors are not specifically designed for freezing conditions, we found the measurements to be consistent, both regarding inter-sensor comparisons as well as in comparison with related variables such as ground temperature and precipitation*”. This should be better motivated in the method section.

Page 19, lines 15-16. I suggest to add here more details on the result that it does exist a SWC “peak” at about 2000 m a.s.l. This is an interesting result that should be highlighted.

Figure 11. I suggest to have the same x and y range in all the subplots.