Interactive comment on “COSMOS: The COsmic-ray Soil Moisture Observing System” by M. Zreda et al.

Review of the manuscript entitled:
COSMOS: The COsmic-ray Soil Moisture
Observing System
Submitted by M. Zreda et al.
Hydrol. Earth Syst. Sci. Discuss., 9, 4505–4551, 2012

Reviewers’ comments are in black.
Our responses are in green.

General comments

This manuscript is a useful contribution. It is a complete collection of the basics and technical details needed to sensibly use this method. However, it reports little new information. Hence, it is an educational summary of the innovation process of the past few years after making this method operational, as stated on p.4508 l.15-19 (“comprehensive description of information distributed in precursor articles”). Some chapters are a long manual-like documentation. In each chapter the authors try to be complete , which leads to repetition.

Response: We agree that the manuscripts has some parts that are repetitive. Where possible, these repetitions have been eliminated or reduced (see also our response to comment 5 by reviewer #1, who brought up the same issues).

We do share the view that the manuscript is in small part a summary of previous work. But it is not “comprehensive description of information distributed in precursor articles“ as stated by the reviewer. Rather, on p. 4508, lines 15-19 of the original published paper we wrote “Building on the short articles of Zreda et al. (2008) and Desilets et al. (2010), this paper presents a much more comprehensive discussion of scientific background and technical details at a level that is intended to be accessible to the diverse communities interested in this novel method and the data from the COSMOS project.” We used these short articles as the foundation on which we built this comprehensive paper. Little overlap exists between the previous short papers and this manuscript. What readers could not find in the 2008 and 2010 papers, they will find here. Separately, HESS ran a “similarity test” on the submitted manuscript and found <1% similarity with previous work, which supports the originality of this manuscript.

The example given in Fig. 12 is the most simple case for demonstrating the positive features of the method because the site has almost no vegetation, little soil organic matter, and low lattice water. This eliminates many possible complications. It would be worthwhile to show another example, e.g. from a humid site to demonstrate the shortcomings, which are mentioned here and there in the manuscript.

Response: We agree that more examples would be useful. We included two new examples: one from a forested site in a mountain setting (snow and vegetation play a role); the other from two tropical sites (high soil moisture, high lattice water and low count rate are complicating factors). [These examples are in sections 3.4.2 and 3.4.3 of the new manuscript.]

This method yields mass of water in g cm^{-2}, but at best soft information about the vertical distribution of this “mass” below surface. It closes a methodological gap. I fully agree with your conclusions on p.1, l .21-22, but you should not downplay the significance of knowing the spatial distribution of local soil moisture at smaller scales. The variance of soil moisture, the soil moisture pattern is what the plants encounter – not th mean –, what is heavily influencing the spatial routing of water on and into the soil, information which is needed to get a better handle on evapotranspiration and heterogeneous in- and exfiltration processes. The latter may funnel the water out of reach for this and other soil moisture detection methods. Sure, this is not the topic of this manuscript, but the reader could be carried away with the wrong impression if the deficiencies of local soil moisture information are weighed against the benefits of area-averaged soil moisture information. This is the big misunderstanding between climate-focused scientists and soil hydrologists.

Response: Yes, the method gives one average value of soil moisture over a large area and thickness. However, numerical methods exist to convert integrated measurements into profiles; how successful they will be when used with COSMOS data is unknown at present.The reviewer is correct in pointing out that COSMOS data are missing small-scale information, but this is the nature of the method. Also, an argument could be made that plants act not only as specimens on a small scale, as pointed out by the reviewer, at which scale local variations are important, but they also act as an ensemble, in which case average properties and states are important. These are interesting issues, but this paper is not the right place to discuss them.

I agree only in part with your assessment of SMOS data. This technique is also being used to obtain ground based areal soil moisture on a similar scale as the COSMOS probes, at the very surface though. These measures are temporally as continuous as the COSMOS probes and could be used to complement the COSMOS data, discriminating the role of the temporally highly dynamic surface soil moisture and soil water storage in the upper rooting zone. However, the vegetation is largely transparent at the L-Band, but still contains some information about the vegetation structures with characteristic scales of >0.2 m .
Hence, I suggest a clear message recommending that this method must be combined with other soil moisture observation methods.

Response: We know that microwave methods similar to that implemented in SMOS can be used from a platform on the ground. Our Toulouse COSMOS sensor is collocated with a microwave sensor. The two methods are complementary because of their different measurement depths. We work with SMOS and explore the idea of using COSMOS and SMOS data together (CO-SMOS, perhaps?) to produce better soil moisture profiles. This work, however, is in early stages of development, not yet ready for public disclosure. It is probably true that multiple methods give more and perhaps better information than one. But it does not mean that a single method is useless without others, nor that every method must be combined with others.

Editorial comments

p.4506, l.2: Kodama who first described this method is being cited later in the manuscript. However, he used the sensor installed below surface. The authors of this paper and his partners definitely contributed more in the past few years than Kodama to make this areal soil moisture observation method operational than Kodama. Nevertheless, the way the author’s self-citation appears in the first sentence of the abstract creates the wrong impression of intellectual property.

Response: We meant a cosmic-ray method for measuring “area-average” soil moisture. Kodama’s was essentially a point measurement. So the first sentence in the abstract is true to facts. Consequently, no changes have been made.

p.4506, l.6: The term “neutronavka” does not sound familiar to me. Is it really a must to know it being an average reader of HESS? If not, delete it because it does not convey an important message. In earlier papers this instrument has been introduced under a different name. To place this “ad” in the abstract is a too obvious marketing spot. Redefining introduced terms adds to confusion.

Response: It was an attempt to create a new unique name. But it has generated criticisms from the HESS reviewers and from others, so we decided to change "neutronavka" to "cosmic-ray probe" or "COSMOS probe".

p.4511. l.27: Is the variation in water vapor pressure not more important than the barometric pressure ? Is this statement in conflict with that on p.4515, l.25 and p.4517, l.11/12?

Response: Variations of pressure can be as much as 30 mb due to a passing front and as much as 50 mb within a year, which results in a correction factor of several tens of percent. For comparison, water vapor correction factor is less than 10%, and at most sites much less than 10%. Whereas the two variables, pressure and water vapor, are inversely correlated (high water vapor means lower pressure), the two corrections are fundamentally different. Pressure variations affect the total mass shielding, while water vapor variations affect scattering properties of the air. Thus, the statement on p. 4511, l. 27 (mass shielding due to pressure variations) is not in conflict with that on p. 4515, l. 25 (partial pressure of H_2O) or that on p. 4517, l. 11/12 (dependence of the footprint on humidity). No changes have been made to those places in text. However, we have added Fig. 15 that shows the magnitude of corrections for pressure, water vapor and incoming neutroon intensity; this figure answers the reviewers' question about the relative magnitude of water vapor and pressure corrections. We also added a paragraph to section 3.3.3 describing these corrections. [p. 22, l. 7-12]

p.4513, l.20-27 and p. 4514 l.26 : How about soil organic matter (C and H). This constituent is probably as important as the mineral soil matrix? This remark relates to almost all parts of the manuscript where the role of the H in the mineral lattice is addressed.

Response: Soil organic matter contains C (not very important) and H (somewhat important, given its low concentration), and therefore it will moderate neutrons just as soil pore water does. If the amount is constant in time, it will be part of the calibration parameter N0 (just like lattice water is). We measured organic C and total C in soil samples from all COSMOS sites, but did not find that organic matter correlates with neutron populations. This does not mean that organic matter is unimportant, just that other factors are more important at our sites. This should be investigated further. [We added new section 2.4.2 that discusses soil organic mater. We also added a column with soil organic C at COSMOS sites to Table 2.]

p. 4514, l.2 … travelling …penetrating

Response: The original "travel" and "penetrate" seem correct.

p. 4518, l5: I really wonder why ? … The COSMOS network is getting large and larger and this information is more than needed.

Response: We do not know how to design an experiment that would provide suitable data. This information can be obtained indirectly by comparing COSMOS data with data from TDR or similar probes installed at various depths. Where extensive networks exist, the data suggest that COSMOS probes are most sensitive to top 10-20 cm, in broad agreement with theory. But controlled experiments are lacking because of the large horizontal scale of COSMOS measurements (660 m).

p.4518, l.10-15 and l.19-23: The neutron probe literature shows that the calibration of organic surface horizons differ from that of the subsoil, even when the sphere of importance (the 86% of H contributing to the signal) does not extend beyond the soil surface. The main difference is probably not due to the lattice water nor due to the mineral composition but to the presence of H in the soil organic matter.

Response: We have not investigated this. It is quite possible that in some areas soil organic matter is important. We have added new section 2.4.2 on water in soil organic matter, added a column with organic C to Table 2, and added “soil organic matter” to “lattice water” in various places text (most notably in section 2.5.2 and 2.6).

p. 4521, l.21: 108 samples in an area of roughly 30 ha is a very good data support for estimating the mean of the usually normally distributed ⎝. The rather large literature on spatial variability of soil water contents shows that the coefficient of variation (standard deviation/mean) of (local) soil water content is in the order of 0.10-0.15 for high water contents but increases above 0.30 in drier soils. Your error estimate (standard error) is apparently somewhere in the middle range of the values. I add a graph at the end of the review. There is much more of spatial variability information than this in the literature of the eighties.

Response: We know about the coefficient of variation and its changes with the mean value. For our calibration we chose 108 samples so that an accurate mean value would be obtained (most of our mean values have standard error <0.01 $m^3 m^{-3}$, and many <0.005 $m^3 m^{-3}$). We could probably calibrate on fewer samples, but we decided to be conservative and have more samples than necessary.

p.4522, l.22: Do you mean the COSMOS probe at the San Pedro site or is this instrument a special version of a (preliminary) design? Stick to one expression for one thing and define it when it is first mentioned.

Response: This means the COSMOS probe at the San Pedro site. It has been clarified at the beginning of new section 2.6.3.

p.4522, l.22: Looking at Fig.7 I do not understand this explanation since this figure is used for a different message.

Response: Old Figure 7 (new Figure 6) shows a calibration function. The reference to new Fig. 6 is correct: the figure shows that as soil moisture decreases, the neutron intensity increases, thereby decreasing the uncertainty of neutron measurement.

p.4522, l.23: same comment as above (p. 4521 l.21)

Response: The same response: increased count rate due to altitude and latitude gives smaller uncertainty.

p.4524, l.5-10: Lattice water is time invariant. So is the concentration of H contained in the below-surface organic matter, at least in the medium term. To account only for the former is inconsistent, especially regarding many soils of temperate and even more so of boreal and alpine regions.

Response: We have added new section 2.4.2 to discuss water in organic matter, added column with organic C to Table 2, and changed wordage in relevant text.

p.4531/32. “ Concluding remarks” (entire paragraph): This is not really a scientifically based conclusion, but rather like a commercial advertisement.

Response: We agree. This entire section has been deleted.

p.4537: You should not cite web pages with a short life expectancy ! *Shuttleworth, W. J.: The Langbein Lecture at the Fall 2011 AGU Meeting, San Francisco, available at: http://hydroinnova.com/video shuttle 2011 long.html (last access: 1 March 2012), 2011.*
On this web page you get the answer **Page Not Found**
The page you are looking for might have been removed, had its name changed, or is temporarily unavailable. Please try the following: If you typed the page address in the

Address bar, make sure that it is spelled correctly. Click the Back button in your browser to try another link. Use a search engine like [*Google*] *to look for information on the Internet.*

Response: Agreed. This reference has been removed.

Figures and Table

Table 1: It would be worthwhile to list also elements abundant in soils that are literally "transparent" for neutrons such as Al (as mentioned on p.4512, l.15)

Response: We listed the ten elements that contribute most to the total cross section. Among these, eight are major rock and soil forming elements; the other two are H and O. The common element Al is also included in Table 1.

Fig. 1: With the following remark, I am swimming upstream, I know, but the term soil moisture is the envelope of all the water components somewhere in the soil profile. Soil water content would be more precise. E.g. in Fig. 1, the "soil water content" is being plotted and the axis should be labeled with values < 1.0 as indicated by the dimension [m3/m3]. This remark applies also to Fig. 8).

Response: Thank you for pointing this out. We have corrected the mismatch between the units (m^3 m^{-3}) and the numerical values (0 to 40, a legacy from the previous version that used volumetric percent as the unit). Now all soil moisture values are given in units m^3 m^{-3} with numerical values between 0 and 1.

Fig 3: The spatial resolution of this figure is too small. Even zooming-in does not make it easier to distinguish the various symbols and lines

Response: Thank you for pointing this out. A new figure has been produced; it has higher resolution and it has been edited for clarity.

Fig 6: Use small symbols for all elements (as for H). This disentangles the piling up of element symbols B,C, and Mg, (?), or Na (?) onto each other, which makes them nonlegible.

Response: This figure (5 in the original paper) has been deleted per suggestion of Heye Bogena (reviewer 1).

Fig.8: Caption: The "differences" are ABS[θ_{meas} (t) - θ_{neutr} (t)]? It would be interesting to plot the θ_{neutr} (t), which results without compensating incoming neutron intensity, atmospheric pressure and atmospheric water vapor.

Response: We appreciate this request. We have added Figure 15 that shows the correction factors for pressure, water vapor, intensity, and the combined correction.

Terminology

The SI (système international) recommended abbreviations for seconds are "s" , for years it is "y" (or "a"), for meters it is "m" … the dimensions used here are "SItolerated", but not all of them are recommended. In HESS, let us converge to the SI agreement.

Response: This is easy to do on graphs and in tables (and we have done so), but it may look awkward in text. For example, in the abstract, p. 4506, lines 8-9 we wrote: "they mix instantaneously at a scale of hundreds of meters". How might this be written in the strict SI units? Perhaps: "they mix instantaneously at a scale of hundreds of m"? On p. 4507, lines 23-24 we wrote: “satellite microwave sensing can provide integrated values of near-surface soil moisture over tens of kilometers squared”. It would have to be something like this in strict SI convention: “satellite microwave sensing can provide integrated values of near-surface soil moisture over tens of 1000000 m2.” In all three examples the text that adheres strictly to the SI convention is awkward. For text to read well sometimes we have to depart from the strict and rigid scientific lingo, and use more of the everyday language. On p. 4514, line 7 we wrote “neutrons have velocities of tens to thousands of kilometers per second.” We could change it to “neutrons have velocities of 10^1-10^4 km/s” but this would have a negative effect of suggesting a greater accuracy than there is.

So there seems to be no good way out of this criticism. I suggest the following compromise: wherever text allows for strict SI units without introducing awkwardness, we will use strict SI units; otherwise we will use derivative SI units for better readability.
Another potential problem is that strict SI units may not be the best, most reasonable units. For example, in (old) figure 12 rainfall is given in mm/d, with values between 0 and 40; in the strict SI units (m/s) those values would be between 0 and 4.6E-7 m s^{-1}, which readers may not find easy to follow. Therefore, I ask that the use of derivative SI units be permitted.

Coefficient of variation of the soil water content (example)

Each of the data points shown below refers to a large sample of gravimetric water content.
Schulin, R., H. Flühler, H. M. Selim, B. Sevruk, and P. J. Wierenga, 1993: Soil moisture, WMO-Report No. 749. Snow cover measurements and appeal assessment of precipitation and soil. World Meteorological Organization.