Comments from the Editorial Board Member and Reviewers:

Editorial Board Member (Comments to the Author):

Thank you for the submission of your Data Descriptor entitled, "Fourteen years of continuous soil moisture records from plant and biocrust-dominated microsites" (SDATA-21-00655), to the SCIENTIFIC DATA. Your manuscript has been reviewed by two experts in your field of research. Fortunately, both reviewers highly appreciate the importance of the data. However, Reviewer #2 raised some important concerns about technical issues. In general, I agree with these suggestions. The systems covered by this data are very specific and require special attention to the data acquisition and validation methods. Properly addressing the points made by Reviewer #2 will make the validity and limitations of this data set clear to users. So I would like to request the authors to improve the manuscript. I look forward to receiving your revision.

*The manuscript “Fourteen years of continuous soil moisture records from plant and biocrust-dominated microsites” (SDATA-21-00655) has been revised, reorganised and modified according to the comments of the reviewers. It should be noted that we have agreed with the comments and they have been really useful to improve our manuscript.*

Reviewer #1 (Remarks to the Authors):

The MOISCRUST provides long-term data (14 yr) of soil moisture at different vegetation and biocrust microsites from a Mediterranean semiarid area in central Spain. These data are very valuable as no long-term datasets of soil moisture on common ground covers from drylands are currently available. These data can help improve our understanding on hydrological and biogeochemical processes and vegetation functioning in drylands, and improve hydrological and ecosystem modelling in drylands, for which information on soil moisture temporal dynamics is rather scarse.

The experimental design is correct and the methodology used for data acquisition and processing is accurate and rigorous. Description of the database, data processing and validation, and potential data applications is well reported in the manuscript. Besides, data have been already uploaded in a repository (“figshare”).

Nonetheeless, I want to point to some minor details that have not been sufficiently explained in the text and that may help improve description of data processing and interpretation:

L86. Soil texture is a key factor affecting soil moisture content. Please, provide data on soil texture at this site.

*L86. Data on soil texture have been detailed in the manuscript.*

L112. Why were sensors installed vertically and not horizontally? Inserting them vertically in the soil can interfere with the natural water flow movement. If possible, cite other studies where vertical installation of moisture sensors has been used.

*L112. The sensors were vertically installed because we are interested in register only the soil moisture from 0 to 5 cm of depth. If we had installed the sensors horizontally, the soil moisture data registered by the sensor could be affected by the air fraction outside the soil. Also, the size of the sensor is small (c. 5 cm) so the interference will be so quite small. An example where vertical installation of moisture sensor has been used is:* *Sharma, H., Shukla, M. K., Bosland, P. W., & Steiner, R. (2017). Soil moisture sensor calibration, actual evapotranspiration, and crop coefficients for drip irrigated greenhouse chile peppers. Agricultural water management, 179, 81-91. It has been cited in the text.*

L109-110. Were the sensors calibrated for the specific soil type and the obtained equation applied to convert the raw data to VWC, or were standard equations applied?

*L109-110. We have indicated in the text that standard equations applied were used to sensor calibration. We used this methodology because both bare and crust soils have the same soil texture so the error would be the same.*

L113-116. Solar radiation is also provided in the weather data file. Please, explain how these data were obtained. Also, provide units for solar radiation in the supplementary material.

*L113-116. The units for solar radiation are W/m². Details about how these data were obtained have been included in the text. The units for solar radiation have been included in the Supplementary material.*

L125. Treatment of negative values has not been explained in the manuscript. This should be described in the main manuscript.

*L132. We have included the following sentence in the text: “Besides, the MOISCRUST database has several negative values (anomalous values) falling within the margin of error of the sensors. These anomalous values were set to zero to removed them.” to resolve it.*

Very minor editing comments:

L50. I would say “surface and subsurface hydrology studies”

*L50. This change has been included.*

L92. well-developed

*L92. This change has been done.*

L117. at the five different microsites

*L117. This correction has been included.*

Reviewer #2 (Remarks to the Authors):

Tracking no: SDATA-21-00655

Fourteen years of continuous soil moisture records from plant and biocrust-dominated microsites

The authors provided a data set of 14 years of water content collected in surface soil material, some of which is capped by biological soil crusts (BSC’s) or by shrubs, and all located in a semi-arid ecosystem in Spain. The paper briefly discusses the goals of the field experiments, how the data were collected, and how the data could be used by modelers and experimentalists. Indeed, the value of the dataset is in its length, and in the quality of the workflow developed to make the data useful for various purposes, at the discretion of the user base. Obviously, long-term datasets like this are unusual. All this said, some concerns are raised about whether the sensors are measuring what the authors expect, and how limitations in the data may significantly restrict how the data are used by the biological and/or soils communities. Comments are articulated below, using the categories provided by the publisher.

Review and comments focus on the questions posed by Sci Data:

Experimental Rigor and Technical Data Quality

Regarding the technical quality of the data, the purpose of the field deployment for measuring water content is unclear from this manuscript, or the previously published works from their laboratory group. Specifically, if the authors are seeking to measure the effect of the crust on water content of near-surface soil—i.e., does the crust restrict infiltration—then the field monitoring method seems reasonable and supportable, because the soil water content is important. If the authors are seeking to measure the water content of the crust itself, then the region of influence along this vertically-oriented, 5-cm long probe will preferentially sample soil material deeper than the crust, which mostly sits atop the soil. In other words, the vertical orientation of the sensor, embedded mostly in soil underlying BSC, will mute nearly any changes in the crust material itself, thus underestimating crust water content during and immediately following an initial precipitation event in dry soil, and overestimating crust water content during dry down periods when underlying soils are still moist. Indeed, Figure 3 in this paper shows a comparison between the ECH2O and TDR probes at the site. The scatter plots shows a reasonable correlation (R2 = 0.72). The (corrected) line slope of 0.83 indicates that either the ECH2O probes are overestimating water content, or TDR is underestimating it (we would expect a 1:1 line). The readers don’t know either way. The implications of this error on carbon dynamics or water exchange modeling is uncertain but should be addressed.

*Effectively, we are seeking to measure the effect of the crust on water content of near-surface soil so the field monitoring method is reasonable and supportable, because the soil water content is important. As we mentioned above (in reviewer #1 response), the sensors were vertically installed because we are interested in register only the soil moisture from 0 to 5 cm of depth. If we had installed the sensors horizontally, the soil moisture data registered by the sensor could be affected by the air fraction outside the soil. In addition, the size of the sensor is small (c. 5 cm) so the interference will be so quite small. We have included an example in the text (Sharma, H., Shukla, M. K., Bosland, P. W., & Steiner, R. (2017). Soil moisture sensor calibration, actual evapotranspiration, and crop coefficients for drip irrigated greenhouse chile peppers. Agricultural water management, 179, 81-91) where vertical installation of moisture sensor has also been used, to support our decision.*

*The figure 4, which shows the comparison between the ECH2O and TDR sensors, has been repeated using the same scales at the both axes. This graph shows R2= 0.72 and line slope is 0.83…*

The authors did not indicate whether any other calibration work was done to improve confidence in the sensor readings; for example, how were sensors originally calibrated to the site soil material, so that users have confidence in the magnitude of the water content readings? A short description would be appropriate and help the reader to place readings into proper context.

*We agree with the fact that another calibration should have been performed. Nonetheless, the reason which we did not do it was we do not want to obtain a really specific data, we only wanted to obtain comparable values between microsites. We used a standard equations applied to sensor calibration, as we have indicated in the text (L109-110), because both bare and crust soils have the same soil texture so the expected error would be the same.*

Figure 3 shows the comparison between the ECH2O and TDR sensors, but they use fractional water content on the abscissa and percent water content on the ordinate. Of course, these should be the same scale, so that the regression line makes better sense for the reader. Also, authors should also include the estimation error in the statistics.

*Figure 4. This figure, which shows the comparison between the ECH2O and TDR sensors, has been repeated using the same scales at the both axes. The estimation error…*

Regarding the depth, size and/or completeness of their data given the research questions, the authors discuss the broad topics of vegetation mosaics in semi-arid environments, the role of near-surface water content on hydrologic/climate processes, and how these data could be assimilated into regional and/or global models. But, the authors do not articulate any specific questions, goals or hypotheses in this manuscript, or how the data could be used outside of the carbon dynamics research they referenced therein. The scientific community is well aware of the universal limitations in using point-scale measurements for regional and/or global studies, so they don’t need to be listed here, but it is surprising that they are not listed in the manuscript.

*The purpose of the manuscript is to introduce the MOISCRUST dataset, which is specified in L76-80. Thus, any specific questions, goals or hypotheses have been included in this manuscript. At the section “Possible use of these data”, we indicate how the data could be used.*

The authors did fully describe the approach used to impute missing data, but they did not justify why correlation of data pairs exceeding 0.85 were flagged as acceptable, and why correlation of pairs below 0.85 were flagged as poor. What is the reasoning behind the 0.85 value? Perhaps another approach would be to consider 1X, 2X, or 3X standard deviations (68%, 95%, or 99.7%, respectively, assuming a Gaussian distribution), which would improve repeatability.

In section 3.5 of the supplemental material, the authors note they are replacing negative water content values with zero, using the justification that these values are within the margin of error, but obviously not physically possible. A few issues here. First, by revaluing the data in this way, the user will not know whether the sensor readings are slowly drifting downward overall, or whether the sensors are measuring very dry conditions. It is recommended that the data be flagged and noted as NAN or some other numerical value (e.g., -9999). Second, the authors have not described how the margin of error was determined; thus, the user cannot incorporate uncertainty into subsequent analyses. Third, and related to issue #1 above, electronic sensors are notorious for baseline drift, but the authors did not mention this in the main manuscript or the supplemental material. Were any sensors returned to the laboratory for recalibration or testing? Were replacement sensors run through a calibration process (see line 130)?

*According to the question “Were any sensors returned to the laboratory for recalibration or testing?”: No sensor recalibration was done when data was observed outside of the baseline. When an anomalous data was observed, what was done was to check if the sensor continued to measure well by comparing it with another sensor that was known to measure well. Equal measurements were included and anomalous measurements were discarded.*

*According to the question “Were replacement sensors run through a calibration process (see line 130)?”: No sensor replacement was performed during a calibration progress.*

Completeness of the Description

With respect to the methods and data processing steps, the authors did an excellent job mapping out the workflow needed to prepare the data. All the supplemental data files that contained the R code appeared complete and the data file should allow assimilation into other datasets. Data descriptor appears compatible with FAIR standards.

Integrity of the Data Files and Repository Record

Files are downloadable and fit the description in the supplemental file. Files were stored in FigShare, a readily available warehouse for datasets. This reviewer has used PANGEA, which seems to be a more mature, familiar and discoverable solution for enduring datasets, though FigShare does allow code search options.