

Interactive
Comment

Interactive comment on “Dew formation on the surface of biological soil crusts in central European sand ecosystems” by T. Fischer et al.

Anonymous Referee #2

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General Comments:

Overall I think it is an interesting work, but authors should do an additional effort to improve and polish the paper to make a really important contribution. The paper predicts dew deposition on BSCs in different developmental stages using the Penman equation modified for unsaturated surfaces and suggests a delay in water saturation for more developed BSCs which affect dew deposition. In essence, the authors are attempting to answer a key question in the ecohydrology of BSCs, the patterns of dew deposition on BSCs, which ultimately entail the significance of dew for BSCs. However, the current manuscript is lacking in many facets. The key major points of concern are:

- The abstract, the introduction and the aims of the paper sound very promising, but the presented results and analysis actually do not completely show neither the hypoth-

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esized delay in water saturation involved in the dew deposition on developed BSCs in relation to less developed (no data showing this delay are presented) nor an adequate statistical analysis of all the possible factors influencing this delay to identify the most relevant ones. Respect the delay, probably a continuous monitoring of the weight of the Petri dishes, containing the BSCs samples, together with a shorter time step in the prediction of dewfall would have provided valuable information for the paper. Respect the factors, others crust characteristics such as crust roughness or hydrophobicity should be included in the analysis of influencing factors.

- In relation with my suggestion of predicting dewfall at higher temporal resolution, I recommend to calculate G instead of using a fixed value, because G is also a dynamic factor and it could have different values for the different developmental stages of BSCs and also it changes during the dew event. Consequently, assuming a constant value of -2 Wm^{-2} for all period and crusts could introduce an additional bias. Calculating G , at a shorter time step could be applied to predict and analyse the patterns of dewfall deposition. Moreover, dew is not a constant process and there may even be small evaporation events during a dew event, that could be detected with the appropriate temporal resolution and provide interesting information to the paper.

- I worry very much about the presented conclusions which are in many cases based on speculations as can be see for example in the first sentences of conclusions: “ The results support. . . improving their water supply by dew collection”. The paper does not provide results supporting all these statements. The same apply to “. . .which may be compensated by ecophysiologicalor hinder mosses from taking over”.

Specific comments:

Section 2.1: Authors describe different degree of pores occupation by filamentous cyanobacteria and filamentous and coccoid green algae and they cite Fisher et al (2010). However, additional information, such as some quantification of this occupation for each developmental stage should be included. Section 2.2: I am concerned about

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the construction of water retention functions in the laboratory with these small rings because of the representation of natural variability and the edge effect. Moreover, the number of repetitions for each type of crust should be indicated. Section 2.3: It would be interesting to specify better the calculation of aerodynamic resistances (r_a), how were they parametrized? Section 2.4: In equation 3, G was assumed to a fixed value of -2 Wm^{-2} . As authors know G is also a dynamic factor and I think assuming a fix values can introduce an important bias Section 3: I think subsections are no needed in this case.

Technical corrections:

Abstract: The acronym EPS should be defined the firs time it is introduced.

1.- Does the paper address relevant scientific questions within the scope of BG? No, in its present form (see general comments above)

2.- Does the paper present novel concepts, ideas, tools, or data? The paper applies a well known methodology to predict dewfall, the Penman equation, however, as far as I know the version modified by Heusinkveld (2008) for unsaturated surfaces has not been applied to predict dewfall on BSCs.

3.- Are substantial conclusions reached?

As I explained in the general comments the conclusions of this paper are not supported by the presented results.

4.- Are the scientific methods and assumptions valid and clearly outlined? Yes

5.- Are the results sufficient to support the interpretations and conclusions? No, see general comment.

6.- Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Some aspects,

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such as aerodynamic resistances should be better explained, see specific comments.

7.- Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8.- Does the title clearly reflect the contents of the paper? Yes

9.- Does the abstract provide a concise and complete summary? Yes

10.- Is the overall presentation well structured and clear? Yes

11.- Is the language fluent and precise? Yes

12.- Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes

13.- Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Subsections in results section are not needed in the present form of the paper.

14.- Are the number and quality of references appropriate? Yes

15.- Is the amount and quality of supplementary material appropriate?

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