

Interactive comment on “Where microorganisms meet rocks in the Earth’s Critical Zone” by D. M. Akob and K. Küsel

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Interactive comment on “Where microorganisms meet rocks in the Earth’s Critical Zone” by Akob and Küsel

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We would like to both referees for his/her very constructive comments which gave us the opportunity to improve the quality of our manuscript. The common main concerns were the length and focus of this manuscript. Thus, the text was revised and reduced to 18 pages, 9 lines from 21 pages by removing redundant information.

All of the suggestions were taken into consideration during preparation of the revised
C1745

manuscript. We have provided a point-by-point response to each of the reviewer’s comments.

RC = Referee’s Comments; AR = Authors’ Response

Response to Reviewer #1

RC #1: The development of the holistic concept of the Earth’s critical zone (CZ) has stimulated integrated biogeochemical research to better understand the functioning of our earth and to increase our capability to predict the response of biogeochemical processes to changing environments. Recently, Lin (2010) published a review paper to describe this concept in detail but without going into details of microbial processes. The authors of this review paper aim “to summarize the factors controlling where microbes (. . .) live within the CZ and what is known to date about their diversity and function”. I admit the large efforts of the authors to compile the information but I cannot recommend to publish the current version of this manuscript.

The paper is much too long in relation to the new knowledge presented.

AR: See main comment above.

RC #2: The authors touched a wide range of different topics. They tried to combine the description of the different habitats (e.g. soils, groundwater, caves) with processes controlling important properties of these habitats and the diversity and functioning of the microbial communities living there. This approach resulted in a lot of textbook knowledge presented making the paper very long.

AR: We agree that in some cases unnecessary “textbook knowledge” was presented. In order to shorten this paper, we deleted some sentences and shortened different paragraphs. Nonetheless, we feel that some textbook knowledge is necessary for an interdisciplinary audience like the readers of Biogeosciences. Our aim was to share what is considered textbook for microbial ecologists and recently published information in the field of environmental microbiology with a broader audience (e.g., geologists or

organic geochemists or biogeochemists) who may be unfamiliar with this subject.

RC #3: At page 2534 (lines 1-4) the authors wrote that they will focus on subsurface habitats but they did not follow this scheme.

AR: we removed “subsurface”

RC #4: I had some problems with the definitions of the different parts of the CZ as illustrated in Figure 1. To my opinion, the definitions as given by Lin (2010, Fig. 1) are more straightforward. Soils are not restricted to the A and B horizon.

AR: It is an on-going debate if soils are restricted to A and B horizons. We follow in our terminology the idea that soils are much more than just the “upper (physical and chemical) weathered layer of the earth’s crust“ (c. f. Ramann, 1918). In line with pioneering pedologist (e.g. Dokuchaev (1883), Jenny (1994, 1994), or Kubiena (1948)) and following a genetic based soil classification approach as followed by, e.g., in the German classification system, our understanding of soils is that of a natural body, which is differentiated in horizons formed during soil genesis. These horizons are severely different from geologic layers unaffected by biogeochemical weathering. In contrast to bedrock or just physicochemically weathered materials, soils *sensu stricto* are characterized by diagnostic horizons, composed of mineral, organic and biological components and are variable in depth (from a few centimeters Entisols) to tens of meters Ultisols (e.g. Richter and Markewitz, 1995).

Thus, we chose to use the term “soils *sensu stricto*” to differentiate the surface layers of geologic material which have been affected by soil forming processes from the rock material. The point we like to make is that due to soil formation a new natural entity is formed which is fundamentally different from just weathered rock. By referring to this region as the “C horizon” it gives the false impression that the altered rock has undergone soil formation processes although the lack of pedological development is one of its defining attributes. Therefore, we feel that labeling this region “altered rock” we are providing a more descriptive term and showing that parent materials are different from

C1747

the surface horizons of soils that are fundamentally changed.

RC #5: I not agree with the presentation of the different processes in Figure 2 as well. The reader might get the impression that subsurface processes do not occur in soils although many of the mentioned processes are typical soil processes. I would indicate that the intensity of these processes varies with increasing depth.

AR: We thank the reviewer for these constructive comments. After reviewing our figure we found that indeed Figure 2 did not correctly illustrate the distribution and intensity of different processes occurring in the CZ. Therefore, we completely revised Figure 2 to provide a better, more detailed illustration of the CZ biological cycle. We now show the cycle in three parts, which show the major pathways in which fixed carbon enters and leaves the CZ. In addition, we showed how the relative intensity of each pathway can vary in different regions of the CZ by drawing different sized arrows that were color-coded for surface (green) or subsurface (red). (The figure has been included in this response as Figure 1, although it is Fig. 2 in the paper). We again thank the reviewer for their comments that we feel gave us the opportunity to greatly improve the figure.

RC #6: Most difficult might be the definition of the (deep) subsurface. Maybe we can use the borderline where the biosphere is independent on photosynthesis which might be much deeper than the C horizon of a soil profile.

AR: We agree with referee 1 that the definition of the deep subsurface is very difficult. However, we strongly disagree with the idea to use the borderline where the biosphere is independent on photosynthesis. This concept does not hold for sedimentary rocks with deposited organic matter originally derived from photosynthesis.

RC #7: I would recommend to re-write the paper completely and to transform it into a much shorter discussion paper. It should really focus on the concept of the critical zone and the role of microbial processes in this concept. Particularly I would expect to get a stimulating discussion about the special features of the deep subsurface and the interactions / response of microbial communities and their functions. For instance

C1748

the role of fungi in anaerobic environments might need a reappraisal. That would also mean the implications for future studies should be given in more details. We know that we need to understand the exact role of microbes in weathering and geochemical cycling. We would need more detailed research questions and approaches to improve this understanding.

AR: We have made major changes to shorten and reduce redundancy, thereby, clarifying our main points focusing on the role of microbial processes in the Critical Zone.

RC #8: Particularly I would expect to get a stimulating discussion about the special features of the deep subsurface and the interactions / response of microbial communities and their functions. For instance the role of fungi in anaerobic environments might need a reappraisal. That would also mean the implications for future studies should be given in more details. We know that we need to understand the exact role of microbes in weathering and geochemical cycling. We would need more detailed research questions and approaches to improve this understanding.

AR: We appreciate the reviewers comment. In the revised version we tried to highlight more some potential research fields for future work (e.g. Page 17, line 19-21). We did revise the implications for future studies section to provide more directed ideas for future and reorganized this section to make it less confusing. In addition, we revised the abstract to make a stronger concluding/future work statement.

Response to Reviewer #2

RC #1: The authors of this paper attempt to summarize our current knowledge about the role of microorganisms (bacteria, fungi, and protozoans) in the critical zone, and the nonbiological factors that control where these microorganisms are found. This is an important topic, as interdisciplinary research in biogeochemistry has significantly expanded in recent years, contributing to our understanding of the interrelationships driving critical zone processes.

C1749

While the authors have provided a relatively thorough overview of biogeochemical processes in the critical zone, the primary issue that I see with this paper is that it is sometimes repetitious and confusing, such that the reader can lose track of the intended focus of a particular section. I recommend editing to streamline and reduce repetitive material.

AR: See main comment above.

RC #2: Some minor errors were noted as well: Page 2540, line 25 – should this be cells ml groundwater-1, rather than groundwater-3?

AR: corrected

RC #3: Page 2546, line 29 (last line) – should be “. . .rely on the addition of. . .”

AR: corrected

RC #4: Page 2547, line 11-12 – should be “. . .reveal that a simple, previously unknown anaerobic respiration process could support. . .”

AR: corrected

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C1750

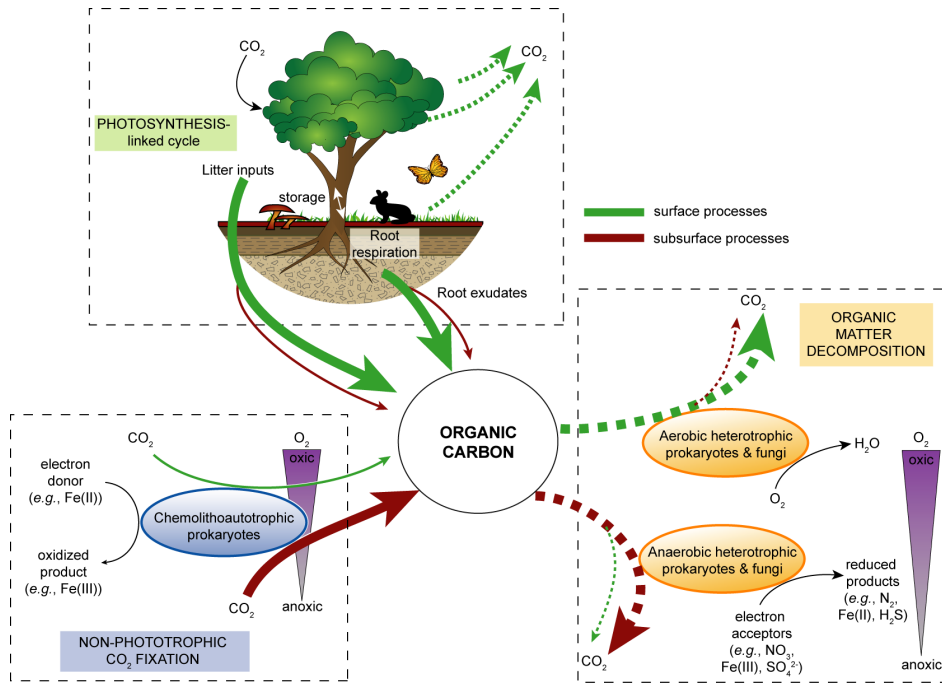


Fig. 1. Figure 2: The CZ biological cycle.

C1751