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Interactive comment on "An ecosystem approach to assess soil quality in organically and conventionally managed farms in Iceland and Austria" *by* J. P. van Leeuwen et al.

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Dear reviewer,

Thank you very much for providing valuable and helpful comments on our manuscript. We have used your comments to revise and improve our manuscript in several aspects. Below we will react on all your points raised and describe how we have addressed them in the new manuscript. We have printed your comments point-by-point together with our response.

Comment-1: This manuscript showcases a purely descriptive study to compare physi-

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cal, chemical and biological indicators between conventional and organic management systems. The main objective is to support the theory that organic practices support soil quality processes that deliver important ecosystem functions. The manuscript is well written, and the majority of methods are clearly defined. The novelty of the study is the use of the Critical Zone Observatories in two countries, Austria and Iceland. Though the conventional and organic systems were paired at each of four sites, these sites differed drastically in their vegetation cover and management, nutrient applications, soil types and climates. These differences made it very difficult to discern any set patterns in indicators which could be consistently subscribed to one or the other management system.

Response-1: The sites were indeed very different, which may have hampered to find patterns, or to draw general conclusions. We have further emphasized this limitation in the new manuscript (see our response-4 below).

Comment-2: The authors go into great detail to discuss various issues. But have not fully examined the underlying causes of specific differences. For one, total biomass of nematodes was different, but this was dependent mainly on the difference in herbivorous nematodes. More information regarding herbivorous rather than just total nematodes would be beneficial.

Response-2: The differences in nematode biomass was indeed only statistically significant for the herbivorous nematodes, but they were also consistent for all other nematode groups, but then not statistically significant. To make this more clear, we have changed the respective paragraph in the discussion (page 18, starting at line 23) and also address here the difference in herbivorous nematodes in relation to similar findings in the literature.

Comment-3: For another, the apparent tillage effect in organic versus conventional grasslands was insufficiently supported with discussion. Tillage may have an immediate effect, but time since tillage can also impact recovery of organisms. Time since last

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tillage versus tillage intensity needs further discussion.

Response-3: This is an interesting issue. Tillage is indeed an important factor in agricultural fields, and therefore time since last tillage plays a role. For the pair of grassland farms on Haplic Andosols in Iceland there was a clear difference in time since last tillage (8 years for the organic farm compared to 16 years for the conventional farm). This difference could indeed have had an effect on the difference in Mean Weight Diameter (MWD) of soil aggregates for example. Because the last tillage was most recent on the organic farm, on which for example MWD was already more than twice as high, we expect the differences to be larger when the difference in tillage history would be smaller. For the pair on Histic Andosols the time since last tillage was comparable (13 vs 15 years). Although changes in physical and chemical parameters take time to establish, the time since last tillage could in our opinion considered as comparable. It is difficult to see how this time differences have had impact on the biological parameters. For micro-arthropods it is known that they are sensitive to soil tillage; yet we have found them in higher diversity at the farms with the more recent tillage history. The mite of highest abundance on both conventional farms was absent or had a low abundance on both organic farms (8 or 15 years since last tillage). Therefore the time since last tillage was not considered an important disturbance factor in the analysis of microarthropods. We have addressed the issue of tillage history in the discussion on page 19.

Comment-4: The conclusions rest mainly on patterns that are not statistically significant leaving outcomes somewhat suspect and requiring further follow-up, which is not discussed. In particular, the one outcome from the discussion as relates to a soil quality assessment is the apparent effect on diversity of microarthropods. This outcome is quite questionable because there is little replication which does not satisfy the following important questions: does this diversity hold through different seasons; does this diversity hold for different crops in rotation; does this diversity hold through time? One easy approach to overcome the limitations of number of sites, would have been to replicate at least the biological, and/or biologically based chemical parameters over time. As

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this cannot be done at the current stage, the authors should clearly address the limitations of the approach used, and make necessary adjustments to their conclusions with these limitations in mind. In other words, be more critical of the aspects that are weakly supported, and strengthen support for the aspects that are not as questionable (over time: physical/chemical).

Response-4: We agree with the reviewer that our discussion and recommendations should especially focus on the statistically significant results. We already had tried to do so in the 1st manuscript, in the revised version with have further strengthening this by adding a conclusive paragraph to the discussion (page 21). We are also aware that an important limitation in our study is indeed the number of replicates taken, both in space and time, especially also given the fact that the farms also differed in many aspects (see comment-1). This is a limitation, but on the other hand, the findings regarding the soil micro-arthropod diversity were statistically significant, despite the differences between farms and the limited number of replicates. The results are in line with earlier findings by e.g. Doles et al. (2001) and Macfadyen et al. (2009), and in addition, this indicated that the applicability of micro-arthropod diversity as indicator for soil quality can be based on ease of measurements. Regarding the effect of crop type, part of the microarthropod taxa present in the arable fields will indeed be adapted to the current crop. However most of the taxa found in the present study are likely generalists in this respect, given the crop rotation used and because measurements were done in both potato and in winter wheat fields, resulting in similar differences. We have addressed these points in the discussion.

Comment-5: Soil quality is a very broad term that clearly cannot be assessed by a single indicator, hence the multitude of parameters assessed in the present study. As a whole, the soil quality assessment does not clearly support one or the other management system as having better 'quality' or supporting more ecosystem services. As part of the reassessment suggested above, discussion of 'soil quality' as a whole should be conducted, even if that does seems to dispense the potential virtues of conventional

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practices.

Response-5: We agree. Principal physical and chemical soil quality parameters showed no differences between organic and conventional, and it is questionable how sensitive these measures are to land management practices. The biological measures did show differences, but as the reviewer addresses, assessing soil quality cannot be done on the basis of a single indicator. Our results therefore do support the notion that biological measurements, such as microarthropod diversity, can play a role in soil quality assessments, it might even be used as a soil quality indicator, but physical and chemical soil properties are indispensable for a assessing and understanding of soil quality. We have addressed this point in the concluding paragraph of the discussion.

We are also grateful for the textual and editorial remarks provided by the reviewer (in the supplement of review comment RC C41). Below we will mention for all of them how we have addressed them in the revised manuscript.

Page 3, number 3: Added “of soil aggregates”.

Page 3, number 4: Corrected throughout paper.

Page 3, number 5: Bacteria and all nematode groups were consistently higher, while herbivorous and total nematodes were also statistically significantly higher, but for sake of shortness and readability the word consistently was used, applying to both bacteria and all nematode groups.

Page 3, number 6: Change accepted.

Page 3, number 7: Organic farming “can” enhance soil organism biomass, this might not be always the case for all groups.

Comments 1, 2, and 8 (crossing outs) are in the side line, so tests?

Page 4, number 1: Change accepted.

Page 4, number 2: Adjusted to “processes that shape and support”.

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Page 5, number 1: Change accepted.

Page 7, number 1: Addressed in Response-3 (above).

Page 8, number 1: Added: “from 10-15 cores”.

Page 9, number1: Comment is correct, labile C = HWC, recalcitrant C = TOC – labile C. Text adjusted.

Page 10, number 1: Total concentrations of O2 and CO2 in sampling bottles were measured every week, and weekly rates calculated from that. Bottles were flushed and reset to environmental concentrations only when O2 concentration dropped below 15% to prevent oxygen limitation. Changed text accordingly.

Page 11, number 1: Enchytraeids were included in the sampling as part of the soil food web. No consistent pattern was found. Added to results (page 14, line 18).

Page 11, number 2: Microarthropods were extracted from 4*196 ml. Added in text.

Page 13, number 1: Indeed, twice as high, but due to high variation, differences were not statistically significant.

Page 15, number 1: Level of detail of the information is more appropriate for an appendix than for a regular table.

Page 15, number 2: Agreed on the limitation of this test for the purpose, therefore we deleted the results of this test.

Page 16, number 1: Fertilization history of Austrian farms added to table 2.

Page 16, number 2: Change accepted.

Page 17, number 1: Paragraph split up in three.

Page 18, number 1: Change accepted.

Page 18, number 2, 3: Yes, also in the study of Birkhofer et al (2008) herbivorous

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nematodes were the most abundant group, and the higher abundance in fields that received organic manure was consistent also for bacterivorous and omnivorous nematodes, only fungivorous nematodes showed no difference due to manure application in their study. Paragraph adjusted to emphasize the importance of herbivorous nematodes.

Page 18, number 4: Change accepted.

Page 19, number 1: Changed “confirm” to “support”.

Page 19, number 2: The astigmatid mite of highest abundance (Tyrophagus similis) occurs almost exclusively in Icelandic conventional grasslands, hence should strengthen the biomass in grasslands compared to arable fields. Despite this highly abundant species, there was no statistically significant difference between grasslands and arable fields (p=0.239, table 4).

Page 19, number 3: Difference in time since last tillage is present but not explaining the results (see Response-3 above). We’ve adjusted the paragraph accordingly.

Page 20, number 1: Diversity is a measure of both evenness and richness. Because the microarthropod communities in the conventional grasslands showed a very skewed biomass distribution due to the dominance of a (few) taxa, diversity was lower than the more evenly distributed taxa in the organic grasslands. We don’t think however that tillage was the most determining factor for the microarthropod biomass in the grasslands in Iceland, due to the long time since last tillage.

Page 20, number 2: Change accepted.

Page 20, number 3: Change accepted.

Page 28, number 1: Selected farms were the best possible matches, predominantly based on soil type and geographic proximity. Time since last tillage differs, but was thought of as lesser importance because 8 years since last tillage is still a reasonable time for stabilization.

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Page 28, number 2, 3: More detailed fertilization data added in table 2 for Austrian arable farms.

Page 28, number 4: The total amount of N fertilization is high, but correct.

Page 29, number 1, 2: Bulk density added to table 3. Bulk density was much lower in Iceland than Austria, as a result of difference in soil type (Andosols vs Chernozems). Differences were not tested statistically, because we only measured one sample per farm for calculation purposes.

Page 29, number 3, 4: Respiration is represented in [kg per ha per year], so [kg ha-1 y-1].

Page 31, number 1: Change accepted.

Page 31, number 2: Change accepted.

Interactive comment on SOIL Discuss., 1, 201, 2014.