

Interactive comment on “A new approach for assessing climate change impacts in ecotron experiments” by Inne Vanderkelen et al.

Anonymous Referee #1

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This review focuses mainly on the design and evaluation of the UHasselt Ecotron Experiment, as was requested by the editor. However, I did read through the entire manuscript.

I am unclear about the three compartments that the authors refer to when describing their ecotron facility. They cite Rineau et al (in review) which apparently describes these, and other essential details (e.g., like which ecosystem processes will be measured and how they will be measured). I have no access to this paper.

Regardless, there is sufficient information in the methods of the manuscript to give me pause and concern. With 12 ecotron units, and what looks to be 12 individual treatment combinations, it appears that only one macrocosm will be used per treatment combination, with no experimental replication. This looks like a so-called "regression design".

These designs are fine. However, the absence of spatial replication makes it essential to obtain robust baseline ecosystem response conditions under "control" conditions (i.e. the conditions under which the control macrocosm in the spatially-unreplicated experiment will be maintained). A robust baseline for a multi-year study would require using the first year of the study to obtain/quantify the particular "behavioral personalities" of each individual (each of the 12) macrocosm. Then, only once each "personality" has been measured, can a rigorous assessment of treatments be reliably measured in the following 4-5 years. Without such a pre-assessment, it will be impossible to know whether treatment responses—evaluated against a single "control" macrocosm—are due to the treatment(s) or to an anomalous "control macrocosm" (analogous to a random "crazy personality"). This is a really critical need, and critical shortfall in the study design, as I understand it, and should be addressed. Perhaps I missed this, but I also did not see any description of how the empirical data collected from the 12-ecotron experiment would be statistically analyzed, nor did I see any specific research questions or hypotheses articulated.

I'm wondering whether the problem of the lack of spatial replication could be addressed by reducing the number of treatment combinations to six, so that there would be at least two replicate ecotron units per treatment combination.

I do appreciate the approach of using data from downscaled climate models to guide which experimental treatments to include. I also like the use of real-time ICOS data to incorporate realistic climate variability to some of the treatments. It is my understanding that these models deliver daily (24 h means or sums) resolution data, that would not be suitable to understand sub-daily/diel climate/weather variability. Is that what the ICOS data will be used for? It would certainly be important to retain diel air T, RH, and precipitation patterns in the experiment.

Taken together, the paper on its own left me with many unanswered questions. These may be covered in the Rineau et al. manuscript. I would recommend placing the essentials of that paper in the next version of this paper, particularly items that address

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the questions and the issues I have identified above. Thus, based mainly on the section of the manuscript on which I was asked to focus, I feel compelled to rate the decision as "reject" at this stage of the manuscript. I would encourage improving the ms. and resubmitting, with the managing editor's approval.

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