

Interactive comment on “Patterns of longer-term climate change effects on CO₂ efflux from biocrusted soils differ from those observed in the short-term” by Anthony Darrouzet-Nardi et al.

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Received and published: 11 April 2018

First of all, we thank both reviewers for their comments. They were thoughtful and contained many good suggestions that will improve the manuscript. Reviewer comments are in CAPS and our responses are in normal text.

AN EFFORT SHOULD BE MADE IN ORDER TO CLARIFY THE TEXT AND DATA OUTPUT FOR POTENTIAL READERS (EXAMPLES IN “DETAILED COMMENTS” SECTION). SOMETHING THAT COULD BE DONE IN THIS DIRECTION IS, IN THE DISCUSSION, TO ADD WHICH TABLE OR FIGURE OF THE ONES PROVIDED IN THE MANUSCRIPT ARE SUPPORTING AUTHOR’S FINDINGS OR EXPLANATIONS.

C1

We can add a few strategic figure/table references in the discussion to help readers. It's a good suggestion.

I THINK THAT, IN ORDER TO UNDERSTAND THIS MANUSCRIPT PROPERLY, READERS HAVE TO GO TO TOO MANY OTHER RELATED WORKS (REED ET AL. 2012; DARROUZET-NARDI ET AL. 2015; FERRENBURG ET AL. 2017; TUCKER ET AL 2017. . . .) -

This is a fair criticism. We will work to make this manuscript more “self-contained.”

I THINK THAT CONCLUSIONS LINKED WITH THE CLIMATE MANIPULATION EXPERIMENT CAN BE ANALYZED MORE DEEPLY. THIS IS BECAUSE I MISS: (I) THE C FLUXES DATA SET BETWEEN 2008-2012 PERIOD (I SUPPOSE THAT THE REASON BEHIND THE LACK OF THEM IS THE HUGE AMOUNT OF TIME NECESSARY TO ANALYZE THE DATA, BUT THE GAP BREAKS THE POSSIBILITY OF TRACKING ANY TREND C FLUXES-ENVIRONMENT, ANY POSSIBILITY OF ANALYZING THIS IN THE FUTURE?)

Unfortunately, this is not an analytical gap, it would be manageable to replicate existing statistical analyses for additional years. However, data during these years were not collected due to project logistics and budgets. Keeping the chambers running is a large amount of work and we could not do so for all years of the experiment. It is expensive to run these automated chambers mainly due to the large amount of personnel time they require (weekly physical checks and repairs, frequent data quality monitoring and processing, etc). As it stands, a huge amount of resources were expended to get the data shown here and it would have gone from huge to astronomical to keep them running for all years. As such, the data we have are rare, with other similar studies measuring ~1-10 times per year as compared to our hourly data (365 * 24 measurements per year with some gaps of course). This is why we are able to estimate and contextualize annual totals, which is not possible with other approaches. We believe that this approach provides a valuable point of reference for other studies with spot

C2

measurements. Now with 20/20 hindsight, it would have been ideal to fill in those years with some spot measurements but unfortunately those data were not collected.

MORE DETAILED INFORMATION ABOUT THE MACROCLIMATE AND THE MICROCLIMATE OF THE RESEARCH AREA BETWEEN 2006-2014 (TABLE 1 AND SUPPORTING INFORMATION ABOUT SOIL MOISTURE ARE NOT ENOUGH UNDER MY POINT OF VIEW FOR A WORK OF THIS DIMENSION)

We agree and will include much more of this information in our revision.

BIOCRUST COVERAGE INFORMATION IN THE CONTROL PLOTS AND TREATMENTS. IS THIS INFORMATION PROVIDED IN OTHER RESEARCH WORKS? IT SHOULD BE STATED SOMEWHERE,

The other reviewer had the same comment and we agree it should be included. We will include these data.

AN EVALUATION OF THE EFFECT OF THE PASS OF THE TIME OVER THE SAME PLOTS (E.G COMPARISONS IN C FLUXES AND BIOCRUSTS COVERS IN CONTROLS IN 2006 WITH THE SAME CONTROL PLOT IN 2014).

Figures 1 and 3 show the control plot data over time but we can also include statistical comparisons of these plots among years.

L 45-47: WRITTEN THAT WAY IT SEEMS THAT SOIL RESPIRATION CONTRIBUTE TO C UPTAKE OF THE SOIL, PLEASE REPHRASE

Agree this is awkward and we can rephrase.

L 70-74: BESIDES THE HUE INCLUDED SAYING THAT PHOTOSYNTHESIS MAY HAVE A NEGATIVE RELATION WITH TEMPERATURE, I WOULD SUGGEST TO INCLUDE THAT THE RELATION BETWEEN PHOTOSYNTHESIS AND TEMPERATURE IS NORMALLY POSITIVELY CORRELATED UNTIL A SATURATION TREND IS FOUND. THIS CONTRASTS WITH THE PATTERN IN RESPIRATION THAT (AS

C3

AUTHORS SAY UNDER WATER AVAILABILITY CONDITIONS) IS NORMALLY POSITIVELY CORRELATED WITH T WITHOUT SATURATION

Our data suggest that both photosynthesis and respiration reach a threshold with temperature in which the positive relationship is reversed. It may not occur at the same temperature. We agree that juxtaposing this against wetter systems where temperature effects are often positive is a great idea and we can work to make this more clear.

L 129: PLEASE QUOTE THE SOURCE OF THE PREDICTION

Good point. We will include.

L131: WHY DID THE AUTHORS CHOOSE THE PERIOD END OF MAY/BEGINNING OF JUNE TO MID-SEPTEMBER FOR THE WETTING OF THE PLOTS? ARE CLIMATE CHANGE PREDICTIONS IN THE AREA GOING TOWARDS HIGHER PRECIPITATION DURING THAT PERIOD OF THE YEAR? I THINK THAT THE REASON SHOULD BE INCLUDED IN THE METHODOLOGY

The reason is that this is mostly a spring rain system, with variable monsoons later in the year that are often insubstantial. As such, the more reliable spring rain makes spring the main growing season for plants. We will clarify this.

L145: WHICH IS THE FREQUENCY OF GAS EXCHANGE MEASUREMENTS INSIDE THE CHAMBER DURING THE 3 MINUTES PERIOD THAT IT GETS CLOSED? HOW IS THE FLUX EXACTLY CALCULATED AT EACH MEASURING POINT?

We will include these technical details.

L152: WHAT ABOUT INORGANIC C FLUXES? THEY ARE NOT INCLUDED IN THE THEORETICAL BALANCE OF NSE. BESIDES, I THINK THAT THE NSE CONCEPT IS INTERESTING AND USEFUL FOR UNDERSTANDING RELEVANCE OF BIOCRUSTS OVER SOIL C FLUXES, BUT IN ORDER TO HAVE A COMPLETE UNDERSTANDING OF THE CONTRIBUTION I THINK THAT SOME DATA ABOUT BIOCRUST COVERAGE SHOULD BE PROVIDED. WHICH % OF THE SOIL SUR-

C4

FACE ENCLOSED BY THE CHAMBER IS COVERED BY BSC AT EACH OF THE TREATMENTS AT THE BEGINNING OF THE EXPERIMENT? AND AT THE END? WE KNOW THAT VASCULAR PLANTS ARE EXCLUDED FROM THE SURFACE, BUT WE DO NOT KNOW ANY THRESHOLD OF BSC COVER IN THE PLOTS CHOSEN.

As stated above, we will include information on biocrust coverage. Inorganic C fluxes could be playing a role but in our previous study we describe why we think it is probably not a large factor. A major goal for the revision will be to make this information more self-contained in this manuscript so we will touch on this as well.

I UNDERSTAND THAT AUTHORS HAVE USED STATISTICAL METHODOLOGIES TO EXTRAPOLATE MISSING DATA INSIDE THE DATA SET TOGETHER WITH OTHER METHODOLOGY TO CALCULATE THE TD COMPARING CONTROLS AND TREATMENTS. I FEEL CURIOUS ABOUT THE FACT OF NOT HAVING STATISTICAL COMPARISON BETWEEN TREATMENTS AND CONTROLS (E.G, IF THE EFFECT OF THE CHANGE IN THE FLUX IN ONE PARTICULAR YEAR IN THE WARMING TREATMENT (OR IN ANY OTHER TREATMENT) IS STATISTICALLY SIGNIFICANT COMPARED WITH THE CONTROL)

There is a statistical comparison but we focus on effect sizes instead of statistical "significance." The cited Nakagawa and Cuthill paper provides a good justification for this approach. We calculate uncertainty surrounding τ_d using bootstrapped confidence intervals. Though we have explicitly avoided using the null hypothesis statistical testing (NHST) paradigm, we note that as a heuristic, confidence intervals that do not contain 0 would be marked as "significant" in NHST. Thus, confidence intervals provide more complete information as compared to what a p-value would provide: they constrain effect size with bounds instead of only telling us the probability that that bounds on the effect size contain zero. Tukey (1991) "Philosophy of multiple comparisons" provides another strong and concise argument on why significance testing is too black and white. As an example, both the -11.8 [-21.7, 0.4] and the -1.2 [20.3, -15.1] would technically be "not significant" but these are different results that warrant different interpretations.

C5

The first implies a much greater likelihood that the effect is in the direction of less C flux in the control and could well be of substantial quantity whereas the second implies poor constraint and lack of good information on the effect size due to high variability among chambers. It could be high, low, or negligible, with greater sample size needed for better constraints.

TABLE 2. I UNDERSTAND THAT THE NEGATIVE VALUES PROVIDED IN THE TABLE MEAN THAT THE CONTROL MEAN FLUXES WERE LARGER THAN IN THE TREATMENTS, CORRECT? THIS CAN BE ALSO INTERPRETED AS A MEAN PHOTOSYNTHETIC FLUX, THAT I THINK THAT IS NOT THE CASE. SOME CLARIFICATION ABOUT THIS COULD BE INCLUDED IN LEGEND OR TEXT.

Yes, it is correct that this is the difference between control and treatment plots and is not indicative of photosynthesis. We can clarify in the caption, which we will do. All plots showed net C losses to the atmosphere at all times - but some of them more so than others.

FIG 3, PLEASE EXPLAIN A BIT MORE HOW WERE CUMULATIVE NSE CALCULATED. IS IT POSSIBLE TO INCLUDE SOME STATS ABOUT THE DIFFERENCES IN NSE CREATED BY THE TREATMENTS?

The stats from table 2 are directly associated with these numbers. One option would be to graphically display the confidence intervals from table 2 in a separate panel. If the Editors or Reviewers would prefer that please, let us know. If the Reviewer is suggesting NHST-based graphical components such as significance stars or letters, we would prefer not to for the reasons stated above.

L250-270: AUTHORS EXPLAIN THAT THEY FOUND HIGHER C LOSSES IN THE INITIAL PHASE OF THE WARMING EXPERIMENT, BUT THAT THIS EFFECT WAS REVERSED IN THE LONG TERM. THIS IS INTERESTING, AND IT SEEMS TO ME THAT TWO POSSIBLE EXPLANATIONS ARE GIVEN FOR THIS (I) REDUCED SOIL C AVAILABILITY, WHICH SEEMS TO BE SOMETHING LIKE A NEGATIVE BIOTIC

C6

IMPACT DUE TO A LEGACY OF HIGH C LOSS IN THE SOIL AND (II) AN EFFECT OF LAMPS DRYING THE SOIL. I DO NOT KNOW IF AUTHORS ARE GIVING MORE WEIGHT TO ONE OR ANOTHER TO EXPLAIN THE RESULTS, BUT AFTER READING WITH INTEREST THE TUCKER ET AL. 2017 PAPER QUOTED, A CLEAR QUESTION ARISES: ARE THE HEATING LAMPS AFFECTING THE T AND MOISTURE OF THE FIRST MILIMITERS OF SOIL UNDERNEATH THE CRUSTS? (IF SO, C FLUXES WOULD BE AFFECTED ALSO FOLLOWING TUCKER ET AL.). IF THE ANSWER IS YES, A SECOND ONE WOULD BE, IS THIS CREATING ANY EXPERIMENTAL BIAS OR THE SAME EFFECT COULD BE EXPECTED OVER THE UPPER MM OF SOIL UNDER A GENERAL RAISE OF T BETWEEN 2-4°C AS THE ONE ASSAYED? I WOULD WELCOME A BIT OF DEBATE ABOUT THIS

We agree that this is one of the crucial details in interpreting these data and we did try to address the point, but in reading back over what we submitted, more could be done to illuminate this issue. We will work to do this in the revision. Our basic argument was that we in fact do not see obvious moisture differences among treatments (the supplemental figure provides some information on this). However, the Tucker paper implies that our current measurement may have been missing the surface moisture dynamics. In addition, it is to be expected that the change in community composition of the crusts will play some role. So we think both likely play a role and unfortunately from this study, we can't totally disentangle which is more important. The inclusion of the biocrust cover data, which we should have provided will help to inform this for the reader as well.

L 259- 261: THE IMPACT OF WARMING OVER VASCULAR PLANTS PHOTOSYNTHESIS IS INFORMATION COMING FROM THE QUOTED PAPERS ABOVE, CORRECT? PLEASE MODIFY A BIT THE SENTENCE TO SHOW THIS CLEARLY

We can certainly do this. Thanks for pointing out the lack of clarity here.

L287-288 WHAT ABOUT THE EVOLUTION OF BIOCRUST COVER AT THIS EXPER-

C7

IMENT IN CONTROL AND EXPERIMENTAL PLOTS, LET ME KNOW PLEASE IF I AM MISSING ANY POINT HERE. IF IT IS NECESSARY TO GO TO OTHER PUBLISHED PAPER TO SEE THIS DATA (OR SIMILAR), AUTHORS SHOULD AT LEAST EXPLAIN IT CLEARLY. I WOULD SUGGEST TO ADD A TABLE OR FIGURE TO SUMMARIZE THIS INFORMATION.

Both Reviewers said this and we agree with them. This will be in the next version.

L 294-298: IT SEEMS TO BE IMPORTANT PART OF THE DISCUSSION BUT I DO NOT SEE THE POINT CLEARLY, PLEASE REPHRASE.

We agree this is not as clear as it could be and we will work to rephrase.

L 306-309. COULD AUTHORS ADD SOME TYPE OF INPUTS TO SHOW HOW IS THE EVOLUTION OF THE NSE IN CONTROLS UNDER NATURAL ENVIRONMENTAL CONDITIONS DURING THE MONITORING (ENVIRONMENTAL EFFECT OVER NSE)? SHALL WE EXPECT A SIMILAR OR DIFFERENT SHIFT IN TREATMENTS?

Environmental associations with NSE is something we addressed in our previous paper and we agree that summarizing the expected effect of moisture and temperature based on these relationships would be helpful here. We can add some expectations of temperature and moisture effects based on this.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-543>, 2018.

C8