

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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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.

TO CORRECTLY CONTEXTUALIZE THESE RESULTS, IT COULD BE NECESSARY TO COMPARE TEMPERATURE AND WATERING EFFECTS WITH CLIMATIC PREDICTIONS OBTAINED BY THE DIFFERENT IPCC SCENARIOS.

We will add some discussion of IPCC projections in the Introduction. We agree it could

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be helpful. The original proposal for this project was based on this type of information and, as the Reviewer suggests, this would be an appropriate way to contextualize.

MOREOVER, I WOULD LIKE TO EMPHASIZE THE POTENTIAL EFFECT OC AND ESPECIALLY SOIL LABILE ORGANIC MATTER ON SOIL RESPIRATION. LARGE EFFORT HAS BEEN MADE TO EXPLAIN THE POTENTIAL EFFECT OF ROOTS RESPIRATION HOWEVER I RECOMMENDED TALKING ABOUT SOIL RESPIRATION (INCLUDING ROOTS, MICROBIAL, AND OTHER HETEROTROPHS) AND THE RELATIONSHIP BETWEEN SOIL C POOLS AND RAIN OR WATER PULSES (SEE LOPEZ-BALLESTEROS ET AL., 2016).

We appreciate this comment and agree that partitioning sources (e.g., heterotrophic consumption of OC vs. root respiration) is important in understanding the mechanisms that drive CO₂ exchange with the atmosphere, both now and into the future. With that in mind, the Lopez-Ballesteros et al. 2016 is a very nice exploration of the different components of CO₂ efflux. Here we cannot perfectly partition the sources of flux – our goal with this study was to carefully quantify soil CO₂ flux with different climates using a manipulative experiment – and thus the mix of eddy covariance, ANPP assessment, and soil chambers as performed by Lopez-Ballesteros et al. 2016 was outside of our experiment's scope. The case we are making here is that, though we are aware of all these sources, the data we have suggest roots are playing a large role in regulating soil flux as observed at the surface. One mistake we made in constructing this argument was referring readers to the appendix in our previous paper (Darrouzet-Nardi et al. Biogeochemistry 2015), where this issue was discussed in detail, including calculations of the size of the organic matter pool and whether it was a plausible source (it is). Instead we should have laid out this logic in this manuscript as well. We didn't want to repeat too much of our previous paper's analysis from there, but we erred too far on the side of redirecting to other papers, which the other review commented on as well. So, we will modify this section to be a more complete and self-contained discussion. Hopefully this will allow a more balanced discussion of sources in this paper to address

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this critique.

WATERING MAY PROMOTE MICROBIAL AND DECOMPOSITION OF DEAD BIOCRUST ORGANISMS, WITH A DEPLETION OF LABILE OC IN LATTER STAGES (BY THE CUMULATIVE EFFECT OF SEVERAL WATER PULSES, THAT DID NOT OCCUR IN CONTROL PLOTS. MOREOVER, THE AUTHORS ONLY ANALYZE NET CO₂ FLUXES BUT THEY MENTION PHOTOSYNTHESIS AND RESPIRATION PULSES WITHIN THE RESULT SECTION (SEE LINE 184), I RECOMMENDED CHANGING IT BY NET CO₂ FIXATION OR RELEASE BUT NOT RESPIRATION AND PHOTOSYNTHESIS SINCE THERE IS NO PARTITION BETWEEN THESE TWO FLUXES.

Assuming this is referring to line 194 at the beginning of the results, we agree, we will change these statements to negative and positive NSE.

MAYBE 9 YEARS ARE NOT ENOUGH FOR THE ANALYSIS OF A NATURAL (NON-INDUCED BY THE TREATMENT) CLIMATIC TREND. BUT I WOULD LIKE TO SEE AN EXPLORATORY ANALYSIS OF CURRENT CLIMATE TREND (AT LEAST DURING THE STUDY PERIOD). THIS COULD HELP TO IDENTIFY ANY TREND IN TEMPERATURE OR PRECIPITATION THAT COULD ACT IN A SYNERGISTIC MANNER WITH EXPERIMENTAL TREATMENTS.

We agree that more comprehensive descriptions of the climate during the entire study period would be useful to provide and we will both provide these and incorporate them in relevant parts of the discussion.

METHODS SECTION SHOULD BE IMPROVED. THERE IS A REFERENCE TO A PREVIOUS STUDY WITH FURTHER DETAILS; HOWEVER THERE ARE SOME KEY QUESTIONS THAT SHOULD BE EXPLAINED IN THE DOCUEMNT: 1) I UNDERSTAND THAT THERE ARE A TOTAL OF 20 PLOTS (5 PER TREATMENT). IS IT CORRECT?

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The Reviewer is correct that there are 20 plots, five replicates of each treatment. We do state "The experiment contained five blocks of four treatments each: control, warmed, watered, and combined (warmed + watered) for a total of 20 2 × 2.5 m plots, each of which contained an automated CO₂ chamber." We will review the methods to ensure clarity and if there are further suggestions for how we can make the Methods section even more clear (a figure perhaps?), we would be amenable.

WERE BIOCRUST COMMUNITY COMPOSITION, BIOMASS AND COVERAGE OF ALL PLOTS COMPARABLE AT THE BEGINNING OF THE EXPERIMENT?.

They were comparable. We will add a supplemental figure in which we show these data. They have not been reported before so it is a good suggestion for an improvement to the paper.

"WATER WAS ADDED IN 1.2 MM EVENTS MANUALLY WITH BACKPACK SPRAYERS AND WAS APPLIED 40 TIMES FROM MAY 31-SEP 20, 2006 AND 36 TIMES FROM JUNE 14-SEP 20 IN 2007, WITH AN AVERAGE TIME BETWEEN WATERING OF 2.8 DAYS (TABLE 1)" ACCORDING TO THIS SENTENCE, WATER WAS ADDED IN 2006 AND 2007. HOWEVER AS YOU EXPLAIN WATERING WAS STOPPED IN 2012. EVEN TAKEN IN TO ACCOUNT THAT 2008-2009-2010-2011 WERE NOT INCLUDED IN THIS ANALYSIS, THIS INFORMATION SHOULD BE INCLUDED AS IT IS EXPECTED TO AFFECT NEE MEASUREMENTS AT 2013 AND 2014. DID YOU EXPECT THAT THIS COULD HAVE SOME EFFECT ON RESPIRATION PATTERNS OBSERVED ON 2013-2014.

We will include information on the watering for the years that we left out. This is also a good suggestion for inclusion. It does make sense to report this since it was part of what contributed to what we later call the "legacy" watering treatment and it is not reported elsewhere.

WAS THE SIZE EFFECT OF EARLY WARMING, WATERING, AND COMBINED TREATMENTS ON NSE CALCULATED FROM THE RANDOM FOREST MODELS?

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IT IS NOT CLEAR IN THE CLEAR IN THE CURRENT FORM.

The random forest models were only used for gap filling. The effect sizes were calculated using subtraction (treatment - control) and the uncertainty around those differences were calculated using a bootstrap confidence interval. We can include more detail on the bootstrap technique used and we agree we can make it more clear which techniques (random forest vs. bootstrap) were associated with which parts of data processing and analysis.

DID YOU ANALYZE SIGNIFICANT DIFFERENCES BETWEEN TREATMENTS?

Please see our response to the other reviewer on this issue.

INTRODUCTION COULD BE BETTER STRUCTURED BY: I) BETTER DESCRIPTION OF BIOCRUST COMMUNITIES

We agree and can include this information.

II) FIRSTLY DESCRIBING THE IMPORTANCE OF DRYLANDS IN GLOBAL C FLUXES IN A CONTEXT OF CLIMATE CHANGE AND THEN BIOCRUST IMPORTANCE IN THESE ECOSYSTEMS. DOING THIS SOME SENTENCES THAT ARE NOT CLEAR COULD BE BETTER EXPLAINED (SEE LINES 44-48)

The other reviewer noted this section as well and we will work to clarify.

LINES 54-58: DID YOU CONSIDERED THE EFFECT OF WATER AVAILABILITY ON BOTH PROCESS? POSITIVE AND NEGATIVE C FLUXES ARE RELATIVE, I WOULD RECOMMEND TO USE C GAIN AND C LOSSES OR EMISSIONS

In the Wertin et al. 2017 paper we did take into account the effect of water availability (and temperature) on both plant photosynthesis and soil CO₂ efflux and the strong relationship we observed between the two fluxes (i.e., photosynthesis and soil CO₂ efflux). We postulated that these patterns could have been due to (1) the independent regulation of each flux by climate (i.e., both fluxes were reduced by lowered soil

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moisture), (2) reduced soil CO₂ efflux being the result of reduced plant C allocation belowground, and thus less root respiration and/or C exudation for heterotrophs, or (3) and/or a mix of both controls.

As far as the terminology, we're not currently understanding the suggestion and would appreciate clarification.

FIG 2: IT COULD BE INTERESTING TO SHOW A CONTROL PLOT AS FIGURE 2.B.

We can add this as suggested.

MOREOVER, NATURAL RAIN PULSES COULD HELP FIG 3: ARE DIFFERENCES SIGNIFICANT?

Annual rainfall is shown in table 1. We could add these to the figure if desired. We are not sure what is meant by "pulses." For the statistical significance issue, please see our comments to reviewer 2.

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