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Interactive comment on "Patterns of longer-term climate change effects on CO_2 efflux from biocrusted soils differ from those observed in the short-term" *by* Anthony Darrouzet-Nardi et al.

Anonymous Referee #2

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Dear Editor of Biogeosciences and Dear authors, I have read with interest the manuscript bg-2017-543 entitled:" 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. The work describe a long term climate change manipulation experiment over soil C fluxes in a semiarid region of the US, the Colorado Plateau, an area with well-known history of active research in the field of biocrusts. I think that the work made and the data provided are novel and interesting, mainly because of the relevance of the methodology used. In this sense, this research line is a step forward in the knowledge of the services that the biocrusts can provide to soils and ecosystems in general. This is because most biocrust gas exchange data available in the literature be-

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long to the isolated crusts or the biocrust isolated with soil attached underneath. Data sets like this give a broader perspective to relevance of biocrusts at the ecosystem level, which is always welcome. Despite of this, there are several points that I would like to comment to authors. The main problems (in general) that I have found with this manuscript are: - 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. 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; Ferrenberg et al. 2017; Tucker et al 2017. . ...)

- 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?) (ii) 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) (iii) biocrust coverage information in the control plots and treatments. Is this information provided in other research works? It should be stated somewhere, and (iv) 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). I bet that at least part of this information is available at other works of the group and should be easily available for readers. This would improve, under my view, the understanding of the manipulated climate change experiment (which, at the same time, points to interesting results) My detailed comments are: INTRODUCTION - L 45-47: Written that way it seems that soil respiration contribute to C uptake of the soil, please rephrase - L 70-74: Besides the hue included saying that photosynthesis may have a negative relation with temperature, I would sug-

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gest 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 authors say under water availability conditions) is normally positively correlated with T without saturation - L 95: The word “years” was included two times by mistake MATERIAL AND METHODS - L 129: Please quote the source of the prediction - 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 - 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? - 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 surface 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. - 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 statiscally significant compared with the control) RESULTS - Table 2. I understand that the negative values provided in the table mean that the control mean fluxes where 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 - Figures 1a and 1b are great pieces of information for all the biocrust community, this is a very strong point of this manuscript -

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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? DISCUSSION - 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 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° as the one assayed? I would welcome a bit of debate about this - 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 - L287-288 What about the evolution of biocrust cover at this experiment 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. - L 294-298: It seems to be important part of the discussion but I do not see the point clearly, please 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?

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