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Interactive comment on "Eddy covariance carbon flux in a scrub in the Mexican highland" *by* Aurelio Guevara-Escobar et al.

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We would like to thank the reviewer constructive comments and annotations. Please find our reply marked along with the reviewers text.

The manuscript by Guevara-Escobar and co-authors present a brief description of CO2 fluxes in a xerophytic shrubland in Mexico. The topic is relevant for Biogeosciences as much more information is needed in water limited ecosystems, underrepresented regions around the world, and ecosystems with different metabolic strategies (e.g., CAM, C3, C4). That said, I found the manuscript and the information presented limited in scope and premature.

Response: We agree with the reviewer impression on the limited scope of our study

because it only analyzed one site and roughly one year of eddy covariance (EC) data. Also, the data set is small compared with recent papers reporting advances on C flux. We intent to improve the paper with a more complete elaboration of the main question, which is the agreement between two methods of measurement: the EC and the MODIS algorithm. MODIS method is based on the radiation use efficiency logic to predict GPP (gross primary production). To support this approach, we propose to:

1) Calculate adequate parameters for the radiation use conversion efficiency (ïĄěmax), the photosyntetically active radiation that is absorbed for the vegetation (APAR) at our site (Bernal) following Running et al. (2000) and use the parameters from the BPLUT (Biome Properties Look-up Table) corresponding to the most similar vegetation type (Heinsch et al., 2003) to calculate eight-day GPP.

2) Use the available data layers from MODIS (Aqua and Terra) and generate a random forest regression ensemble for eight-day GPP (Tramontana et al. 2016). The prediction of GPP applying machine learning algorithms used different forcing variables, including those of MODIS terra (Tramontanta et al. 2016, Jung et al. 2019), but apparently not from MODIS aqua. Therefore, something interesting would be to test if both MODIS products contribute to a better representation of GPP using random forest. The rationale would be that the visit time of these sensors is different during the day and different QC scores can be expected.

3) Compare these GPP values against the GPP modeled from the EC using a Bayesian approach (Stevens et al. 2017), and perform a validation exercise with the best GPP estimate using a small dataset that was not included in the present paper.

Perhaps it will be more interesting to the reader when current methods are applied to obtain a better estimate of GPP using the MODIS platform. We look forward to publish this work even when the data is limited. Please consider the following reasons and the response to the specific comments. We decided to summit the manuscript with the available data for a number of reasons: a) The Bernal site suffered important

changes in land use during 2019 and scrub was suddenly cleared, we are not able to present a longer time series for this site. b) Real estate development, feedlot beef production, cheese and wine production associated with tourism, and automotive industry development are very attractive options for landowners in the region. c) Local society (including authorities) do not recognize scrub and shrubs as resource-valuable or as an ecosystem with a role for better livelihoods; compared to forests. d) Although the landowner agreed in principle and allowed a monitoring site, we can see that people need better information about environmental services. Otherwise, this vegetation type will remain very vulnerable to land use change. We do not have any research station that would be desirable to assure research continuity. Selecting the Bernal site was a gamble, but getting a glance at an overgrazed site was important since there are few publications of C flux data from grazing conditions in general. The EC data showed that scrub at Bernal was a carbon sink. This estimate is not only important in science, but could have an impact on public policy and management. Only one year of EC data stresses the need for more work regarding number of sites and long term studies. Here, we presented only one year of data, but the assessment of interannual variability is desirable. We could develop an upscaling approach at Bernal to explore this aspect (as the reviewer suggests). Mexico as many other countries have few C flux monitoring sites and remote sensed data is an important alternative to represent vegetation functioning, modeling and other applications. Relaying on remote data is partially cost-effective since most data from NASA is open access. Therefore, the main question in the present work was: estimates of the CO2 flux are comparable between ground-based and remote sensed methods? This question is not fully answered yet, since some publications shows that the relationship is not very good. Other reports indicate that the relationships are specific to vegetation types. Although Mexico has 30% of scrub and shrublands they differ in their botanical composition and structure, not mentioning management practices. A multisite study would have interesting hypothesis to test, but that is beyond the capabilities of our group. In Mexico there is a handful of sites representing scrub but these are very different in climate and plant species. Mexflux

is a network supporting efforts for C flux research and data sharing arrangements are not complete. Lastly, Ameriflux, Fluxnet and other networks list few sites from Mexico.

Introduction: The introduction lacks a clear scientific question and related hypothesis. Providing new measurements of NEE at underrepresented ecosystems is important, as well as comparing GPP estimates with satellite-derived products. That said, this manuscript should emphasize what is new (beyond new measurements) and have a Discussion paper testable hypothesis if possible. The introductions resemble a technical report and could be improved by framing it around a clear scientific question.

Response: Background information required to better understand this paper will be included and the hypothesis will be explicitly framed to demonstrate that the vegetation at Bernal site was a sink of carbon and also to test the relationship between remote sensed GPP from MODIS and measurements from the EC tower. We will include in this section information about the importance of new measures in scrub ecosystems. The introduction includes elaboration about the world-wide relevance of MODIS products, the background of the MODIS16 algorithm and its upscaling validation. However, upscaling CO2 fluxes (GPP or NEE) from eddy covariance sites could have different approaches as proposed in FLUXCOM initiative using machine-learning methods, basically using MODIS data along with meteorological data. Following Tramontana et al. (2016), a range of forcing variables from MODIS will be used with a random forest algorithm to upscale data from Bernal site. A small dataset from a period of 2018, not included in the present work, could be used for validation. To support this analysis, the introduction will include a presentation of the radiation use efficiency process-based algorithm as used in the MODIS MOD17 products and an alternative bottom-up machine learning algorithm. Upscaling fluxes using this approach should have better performance in comparison to the obtained results using simple Theil Sen regression.

Methods: This section requires substantial reorganization and more information. The authors should link the methods to the research questions/hypotheses, provide more information about the site and how data was processed and analyzed. Maybe a section

about data analysis would help to improve this section.

Response: The section will be reorganized and will include time series of phenology and climatic variables. Recommendations will be followed accordingly to explain data analysis.

Results/Discussion section: I strongly recommend separating results from the discussion. Without a clear scientific question and testable hypothesis, it is difficult to evaluate this section and the novelty of the results. The authors touch different topics from leaf level photosynthesis, ecosystem level fluxes and remote sensing but I feel that there is disconnection between the results in this section. Finally, due to the limited dataset and analyzes, this section seems to be over-interpreting the results and consequently I wonder if this manuscript is premature for this study site.

Response: We will separate results and discussion in two sections. All the information related to the site description will be incorporated to the methods section, i.e. leaf level photosynthesis and vegetation structure. This section will focus on presenting the time series of CO2 and the relation between EC data and upscaled estimates.

Conclusion: I believe that this section is not fully supported by the data analysis and results. Again, it is difficult to evaluate this manuscript as the authors touch several topics, but none is analyzed in detail leaving the manuscript presenting a very broad (and potentially over interpreting) view of results. I respectfully believe that this is a good first step to summarize results from this study site, but this study requires substantial improvements in quality and quantity of data (e.g., longer datasets), conceptual organization (e.g., questions/hypotheses), and further analyzes to test clear hypotheses to provide a novel scientific contribution.

Response: The conclusion will use only the evidence presented.

Figure 1 is difficult to interpret because the legend is not informative. I interpret it as mean diel patterns for the months represented in the figure, where the top panel is 2017

and the bottom one 2018. It is not clear why the authors present diel means and not the actual data or how many days were used to calculate the diel means for each day. Consequently, the methods section needs much more description about data quality, data availability, and data analysis to fully evaluate these results and the discussion.

Response: Figure legends will be modified. In the methods we will describe the details of data quality and availably as requested. The reviewer is correct this figure presents data corresponding to two years. values are diel means and standard errors.

Comments in detail

Study site: Description of the study site could be improved by following BADM guidelines for AmeriFlux. Although compiling all variables is challenging, a better description of the site is needed in order to compare this site with others across the world.

Response: We will make the best effort prepare the description of the site following the BADM templates.

Lines 85-86 – I disagree with this statement as reporting energy balance closure is a good practice for data interpretation and data comparison.

Response: We will present the energy balance as recommended.

Lines 87-95 – This belongs to data QA/QC and flux partitioning but a better description is needed.

Response: This will be reorganized as suggested.

Section 2.3 – Why not simply using the ORNL DAAC MODIS/VIIRS land product subsets tool?

Response: The subsets tool is very useful and data retrieval is fast but the QC layer is not included.

Line 121-122 – LAI was only measured 2 times? Why only two days and not reporting

a seasonal trend? Where those dates representative for maximum LAI? Line 125 - It is unclear how the use of the Li-6400XT fits into the main purpose of the manuscript. How these data were used? Any upscaling approach?

Response: Time trends of MODIS LAI, NDVI and EVI will be included. Measurements of LAI and photosynthesis will be mentioned briefly in the site description in the methods section, as they were not used to support any hypothesis.

Section 5 data availability: The proper place to host the eddy covariance data would be a standardize repository such as AmeriFlux or FLUXNET. Zenodo is a good place for overall code and ancillary datasets from this study but I appreciate the effort for archiving the dataset.

Response: Half-hourly flux data and tower metadata will be uploaded to Ameriflux. Code and data used for figures will be available at Zenodo.

Figure 2 - How the eddy covariance data was aggregated for this analysis?

Response: The results of ReddyProc were averaged every eight days according to the MODIS MOD17A2 timestamp. Although in the methodology we explain it, the figure caption will include this information. Thanks for pointing this out.

Figure 3- More discussion about why this figure is presented and what does it means for addressing a scientific question is needed.

Response: We present the standard deviation of the MODIS data to indicate dispersion presented by the sensor data, especially in the rainy season, which helps to think that the adjustment should be made in two seasons.

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