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Biogeosciences Discussions

Interactive comment on "Differences in plant cover and species composition of semiarid grassland communities of Central Mexico and its effects on net ecosystem exchange" *by* J. Delgado-Balbuena et al.

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Answers to referee 2, comments on 19 Febraury 2013.

We thank the extensive comments by referee 2, in particular the specific comments and suggestion on the writing style. We have checked and addressed each of them and provided explanations on particular questions when requested. For those observations addressing writing style comments we had not included all of them in this answer. We will incorporate the particular requested change in the final manuscript. Ques-

tions about methods and discussion about particular subjects are included in this letter. Please consult PDF document included as suplement.

COMMENT. SpeciïňĄc examples of this include how the different sites were classiïňĄed. The authors describe a “moderately grazed” site but there is no description of how this was quantiïňĄed. This also holds true for the “high” and “low” plant cover; what method was used to quantify plant cover? How were the dominant plant species determined/quantiïňĄed? Much more detail on site classiïňĄcation needs to be discussed.

RESPONSE. Site description The semiarid grasslands in Mexico (North American graminetum, Aguado-Santacruz and García-Moya, 1998) belong to the shortgrass steppe ecosystem extending from the North American Midwest in the North to the subprovince Llanos de Ojuelos, in Northeast Jalisco in the South. This landscape follows a narrow strip along the Sierra Madre Occidental within the Chihuahua Desert. The vegetation is dominated by grasses; with Bouteloua gracilis H.B.K. Lag ex Steud. (blue grama) as the dominant species forming near mono-specific stands (García-Moya and Villa, 1976). Native grasslands are one of the most threatened ecosystems in Mexico, because intensive grazing by domestic livestock and land conversion to rainfed agriculture (Velázquez et al., 2002; Rzedowski, 2006) have created a patchy landscape exhibiting large diversity in plant cover and species composition (Riojas-Lopez and Mellink, 2005). The region presents a semiarid climate with mean annual precipitation of 424 mm (last 30 years) distributed mainly between June and September, followed by 6 to 9 dry months. Winter rains account for only < 5% of total annual precipitation (García, 2003). Mean annual temperatures are 17.5 $\pm$0.5 °C (m, $\pm$1SE), with mean monthly temperatures ranging between 1.6 °C for the coldest and >18.0 °C for the warmest months (dataset from Sitio Experimental Vaquerias, INIFAP). The topography is characterized by valleys and gentle rolling hills. The two dominant soils are haplic xerosols (associated with lithosols and eutric planosols), and haplic phaeozems (associated with lithosols) (Aguado, 1993). Soils are silty clay and sandy loams, shallow

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with average depth ranging between 0.3 to 0.5 m with a cemented layer of tepetate (Aguado, 1993) (COTECOCA, 1979). We selected five contrasting plant communities that have resulted from differences in grazing regimes and agriculture disturbance. All sites had similar soil type, topography and landscape position (Fig. 1). Plant cover at the sites was classified as either high (maximum reported for the region 35-38 % of soil covered by vegetation; Table 1, Aguado-Santacruz and García-Moya, 1998), or low plant cover (< 10 % of vegetation cover; Table 1). Sites also represented two conditions in terms of species composition; one in which the key native species, B. gracilis, was still dominant (independent of plant cover), while the other presented either subordinate grasses, subshrubs or exotic species as dominant species. The five site types examined were; i) a Moderate grazing site that was a recovered grassland to tillage and overgrazing ($\sim$50-y), currently under moderate grazing regime and prescribed burning (4-y previous to the study). Moderate grazing was defined by two vegetation traits; one related to the grazing intensity level allowing only 50% of standing biomass to be removed by livestock every year. The other one was related to the proportion of soil plant cover that in the case of high plant cover was above 30%. Even though this site exhibited a high plant cover, the native grass species B. gracilis has been largely replaced by subordinate native grass species (30-40% replacement of total abundance) including; Muhlenbergia rigida (Kunth) Trin., B. scorpioides Lag., B. hirsuta Lag., Aristida spp.; ii) the Exclosure site is a 30 year-old cattle exclosure characterized by a high plant cover dominated by B. gracilis (> 80% abundance); iii) the Overgrazing site (Table 1) had a low plant cover (<10%) with B. gracilis as the most abundant species. In this site, grazing intensity may overpass 90% of aboveground biomass removal by cattle; iv) the Shrub encroachment site is also an overgrazed grassland site with low plant cover (< 10 %) having co-dominance with a native shrub Isocoma veneta (Kunth) Greene and an exotic Mediterranean perennial herb Asphodelus fistulosus L.; and v) the Crop site, a shortgrass steppe field converted to rainfed agriculture to produce oat (Avena sativa L), that is covering the soil for around 4 months during summer. Biomass removal from grazing or additions from dung were not quantified. Site characteristics

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can be found in Table 1. . To select initially the sites, we used the line intercept method (Canfield 1941) to assess basal grass cover and species composition. Three to four 20 m transects were sampled per site. Once the sites were choosen and the plots installed, we used digital photography and image analysis (SigmaScan Pro v. 5.0, Table XX) to determine both the proportion of canopy cover per plot and the abundance proportion of each species respect to the total. Also, the "coat" method that was used to measure ecosystem respiration probably doesn't yield accurate ecosystem respiration. Although covering the chamber does result in a reduced PPFD, it really doesn't mimic nighttime. There are still too many carbohydrates around resulting in inaccurate ecosystem respiration, usually an overestimation. The methods used to calculate continuous daytime and nighttime NEE most likely overestimate NEE. Although the method used in this manuscript to calculate continuous daytime and nighttime NEE is probably no worse than using a linear interpolation method, it most likely overestimates NEE during the day and the nighttime. I would like to see some discussion about this and how the method used to calculate continuous NEE could have influenced the results presented here, especially with regard to how different plant species (study sites) respond to PPFD given that "ideal" conditions were assumed.

By definition, ecosystem respiration includes both autotrophic (above and belowground biomass, and mycorrhizas) and heterotrophic respiration (organic matter decomposition by soil microorganisms, Hanson et al. 2000). Using the dome black cover method we were able to account for all of these respiration sources (the same method has been used previously by Harpole et al. 2007). The issue with the use of the dome cover relates to whether photosynthesis influences soil respiration by increasing respiration rates by means of a fast translocation of photoassimilates to roots, a mechanism that has been described by several authors (Craine et al. 1999, Carbone and Trumbore 2007, Bahn et al. 2009). This influence of photosynthesis on ecosystem respiration can last for hours, days and even for weeks (Vargas et al., 2011). This is an effect that is consistently accounted also by our method however there has not been much discussion previously neither a quantitative estimation of the influence of

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photoassimilates availability in ecosystem respiration fluxes. Still, we believe that the dome black cover method used in this study is more accurate to estimate daytime ecosystem respiration than conventional soil respiration chambers that can underestimate fluxes because of its limitations to include the large ecosystem spatial heterogeneity. In regard to the overestimation of nighttime fluxes, it is correct the observation by the reviewer 2, that the use of daytime respiration fluxes could have led to a larger model sensitivity of ecosystem respiration to temperature and soil water content (Eq. 6). All current methods to estimate nighttime ecosystem respiration, in particular those temperature/soil water content-dependent models, and also those based on extrapolations of daytime light response curves suffer of this problem. Also, as was stated above,it has been shown that photosynthesis and soil respiration are related at different time-scales from hours to weeks, and even seasonally (Carbone and Trumbore 2007, Vargas et al. 2011). Thus, it is likely that night respiration in this study is also influenced by photosynthesis rates observed during daytime, making valid the use of daytime ecosystem respiration measurements to calculate NEEnighttime. In addition, adding nighttime NEE measurements (20:00 – 22:00 h) to estimate the curve parameters would partially help to correct this bias. Finally, low correlation coefficients for the nighttime model observed in some sites (Table 3, R2=0.44 – 0.73), not necessarily are caused by the problem discussed above. They also might be related to either signal delay (i.e. the delay of effects of environmental variables on ecosystem respiration as a cause-effect phenomenon, Vargas et al. 2011) or soil heterogeneity (Loescher et al. 2006). As indicated in page 17109, line 16, NEEdaytime was calculated under ideal PPFD conditions which led to an overestimation of the annual carbon uptake. This overestimation should be taken as the maximum carbon uptake capacity by the ecosystem type. Additional clarification is goint to be included in the manuscript in the terms shown previously about the implications of our methods to estimate NEEdaytime and NEEdaytime in annual balances.

COMMENT. Along these same lines, the calculation of continuous daytime NEE requires air temperature, but I didn't see air temperature mentioned in the methods sec-

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tion as it pertains to continuous daytime calculations, with the main concern being how temperature was estimated at each site for every day of the year since it doesn't appear as though each site had its own meteorological instrumentation.

RESPONSE. First, including air temperature did not improve the variability explained by the NEEdaytime model. We did try several light-response models reported in literature (e.g. Gilmanov et al., 2010), some of them including temperature, however the hyperbolic model (Eq. 3, Ruimy et al., 1995) showed the best fitting. For the dry months (vegetation was senescent), when PPFD did not explain NEEdaytime but air temperature, the exponential model (Eq. 4a, Lloyd and Taylor, 1994) was used to calculate daily NEEdaytime. To model light-response curves and exponential air temperature-respiration model, data collected in-situ was used (i.e. PPFD and Ta sensors placed inside and outside the dome). Continuous air temperature data was collected from a meteorological station nearby the Moderate grazing site (described in page 17109, line 18). Only one meteorological station was available for all sites, but we assume similar air temperature among sites because of their proximity (maximum distance of 7 km).

COMMENT. manuscript needs some signiïňĄcant grammar and sentence structure improvements. There are many sections of the manuscript where the writing style is C8298hard to follow making many sentences unclear. The unclear sentences make many sections of the manuscript difïňĄcult for the reader to understand. I've mentioned as many grammatical issues as I can in the speciïňĄc comments section (see below), but the manuscript could beneïňĄt from the help of a technical writer. SigniïňĄcant speciïňĄc comments are discussed in the following section Abstract: 1. Page 17100, line 3: place a semicolon after the word "changes" 2. Page 17100, line 7: "Five typical plant community types were examined in the semiarid grassland by encasing the entire above-ground ecosystem using the geodesic dome method." Change to "Net ecosystem exchange was measured in ïňĄve typical plant community types within a semiarid grassland by temporarily enclosing the entire above ground ecosystem using a geodesic dome." 3. Page 17100, line 15: Change "night time" to nighttime. Also do

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this throughout the entire manuscript. 4. Page 17100, line 17: Change “into” to “to” 5. Page 17100, line 20: Spell out PPFD, then abbreviate.

RESPONSE. Changes are going to be incorporated in next version

COMMENT. Introduction: 1. Page 17101, line 1: Change “ âĹij 17.7%” to “18%”

RESPONSE. Change inserted in next version

COMMENT 2. Page 17101, line 10: Do you mean capacity and not capability?

RESPONSE. Yes, we meant capacity. We adjusted term.

COMMENT. 3. Page 17101, line 19: Replace “ex.” with “e.g.,”. Please do this throughout the manuscript.

RESPONSE. Replacement was carried throughtout the text.

COMMENT. 4. Page 17101, line 20: Change “. . .directly to the observed grassland patchy landscapes. . .” to “..directly to patchy grassland landscapes. . .”

RESPONSE. Suggested modification was inserted in the text

COMMENT. 5. Page 17102, line 9: “Exposed bare soil contributes to carbon losses through increased soil respiration and wind and water erosion.” Why? Is this due to soil moisture difference between bare and not bare soil? Temperature differences? A combination of things? What role does heterotrophic respiration play in bare soils? Also, include a reference for the statement that you made.

RESPONSE. Regarding increases in soil respiration in bare soil sites we should add that this is a temporal effect observed in summer months. The increase occurs as a combination of the higher soil humidity observed in summer (rainy season) and greater temperatures in bare soil points as radiation impinges directly. Horizontal C losses through erosion are very well documented.

COMMENT. 6. Page 17102, line 12-15: Suggest adding soil moisture to this list (i – v)

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RESPONSE. Soil moisture was added to the mentioned list.

COMMENT. 7. Page 17102, line 12-18: ". . .have contributed to conform a patchy grassland. . ." this is a confusing statement. What do you mean by "conform"? Or do you mean to just say have contributed (omitting conform) to a patchy grassland?

RESPONSE. We modified the phrase as suggested omitting the word "conform".

COMMENT. 8. Page 17103, line 13: DeïňĄne PPFD in Abstract (see point 5 above) and just use PPFD in this location of the text.

RESPONSE. PPFD definition was inserted in the abstract and the acronym used afterwards

COMMENT. 9. Page 17103, line 14: "Therefore, NEE should reach maximum rates faster and sooner at sites with low plant cover relative to sites with high plant cover (H2)." What do you mean by "maximum rates"? Maximum uptake? Maximum release?

RESPONSE. In this case we referred to both, NEEdaytime and NEEnighttime. We adjusted the text to make it clear

COMMENT. 10. Page 17103, line 19: "To test these hypotheses, NEE ïňĆuxes of the most common plant communities found in a patchy landscape of semiarid grassland were examined in responses to site-speciïňĄc biotic (LAI) and abiotic (Ta, PPFD, and SWC) controls." It's not clear what this sentence means. Do you mean that you looked at the relationship of NEE and biotic and abiotic factors (whether these factors modulate/control NEE)? Why only the common plant communities, why not all of them, don't they all play a role in NEE? Finally, change "responses" to "response".

RESPONSE. Here, we are referring to one aim of our study related to assessing the influence of biotic and abiotic factors on day and night NEE fluxes observed in five study sites that differ in plant cover and species composition. We modified the original text to improve clarity. Regarding the second point on why not to monitor all the communities, we agree with reviewer 2 that all communities play a role on the regional NEE. The

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task as suggested by the reviewer looks unreacheable for two main reason. One, the geographic subprovince in which the study was carried out is very large (12000 km2) turning very difficult to reach each possible community. Second, the heterogeneity of vegetation communities is also very high such that there are still unaccounted communities. For that reason, we decided to monitor only five communities that represent the structure of most vegetation communities.

COMMENT. Materials and Methods: 1. Page 17104, line 1: Change “Chihuahua” to “Chihuahuan”.

RESPONSE. Suggested modification was inserted

COMMENT. 2. Page 17104, line 9: “Winter rains account for only <5%...” omit “only”

RESPONSE. Word was deleted from text.

COMMENT. 3. Page 17104, line 10-12: “Mean annual temperatures are 17.5 $\pm$ 0.5 _C (m, $\pm$ 1 SE),with mean monthly temperatures ranging between 1.6 C for the coldest and > 18.0 C for the warmest months (dataset from Sitio Experimental Vaquerias, INIFAP).” How much greater than 18 C? Please give exact mean high temperature for the warmest months. >18 C is too vague.

RESPONSE. We calculated and added the mean temperature for the warmest months and replaced in the manuscript with the value reported previously

COMMENT. 4. Page 17104, line 20-23: “The plant cover was classiïňĄed as either high (maximum reported for the region 35–38% of soil covered by vegetation; Table 1, Aguado-Santacruz and Garc0Ä′sa-Moya, 1998), or low plant cover (< 8% of ground covered by vegetation; Table 1).” It’s not clear how the plant cover was classiïňĄed for each site? Using reported values are OK if they were done at your exact site, if not, what method did you use to determine plant cover at each site? The way this sentence is currently written it sounds like cover was just estimated at each location. If so, this doesn’t seem accurate enough.

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RESPONSE. The reference included in this paragraph belongs to a previous study that carried out an extensive vegetation characterization that included all our research sites. In this mentioned study, authors used a line intercept method to estimate basal plant cover. We included in the manuscript a larger explanation about the method for vegetation characterization used in Aguado-Santacruz study. In addition, in our experimental plots we used digital photography and image analysis to characterize canopy cover of plots at the five sites. These values are included in a table in the next version of the manuscript.

COMMENT. 5. Page 17104, line 26-27: "The ïňĄve site types examined were; (i) a moderate grazing site that was a recovered grassland (âĹij 60-yr) to tillage and overgrazing,. . ." What constitutes moderately grazed and how was it quantiïňĄed? Also, does the 60 years refer to the number of years the site was grazed or no tillage or over grazing for the past 60 years? Please clarify.

RESPONSE. As mentioned previously, moderate grazing regime is defined as a grazing intensity regime that allows removal of 50% of standing biomass. It is estimated by first assessing aboveground biomass annual productivity (dry matter basis) to estimate its carrying capacity based on the dry matter demand of livestock (an animal unit = cow of 400 kg). With this information a stocking rate is estimated at a desired grazing intensity (in our case 50%). The desired grazing intensity has been defined from physiological studies examining the recovery of root growth following aboveground defoliation (Crider 1955, Caldwell and Richards 1987). The grazing regimes included here were not imposed for this particular study but observed under current use at each site. The 60 years mentioned in this paragraph correspond to the time when tillage and overgrazing was stopped and the site was allowed to recover to impose latter on a moderate grazing system. We are adding this explanation in the methods section of next version.

COMMENT. 6. Page 17105, line 3: "(ii) the exclosure site is a 30 yr-old cattle exclosure characterized by a high plant cover dominated by B. gracilis (> 80% abundance);"

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Change to “(ii)a 30 yr-old cattle exclosure site characterized by a high plant cover dominated by B.gracilis (> 80% abundance);”. Also, how was the high plant cover determined?

RESPONSE. We incorporated the suggested change in the text. Criteria to classify our experimental grassland sites as either high or low plant cover were derived from potential high and very low plant cover values reported in the region (Aguado Santacruz y García Moya 1998). This preliminary estimations of basal plant cover were determined using the line intercept method (Canfield 1941). Plant cover was estimated as the proportion of the transect (respect to its total, 20 m long) intercepted by the base of grasses . In our experimental plots, we also took digital photographies of the plots and analized the photography with image analysis software (SigmaScan Pro v. 5.0) to estimate the proportion of vegetation canopy covering the plot.

COMMENT. 7. Page 17105, line 4: (iii) the overgrazing site (Table 1) had a low plant cover 5 (<8%) with B. gracilis as the most abundant species;” change to “(iii) an overgrazing site(Table 1) with a low plant cover (< 8%) with B. gracilis as the most abundant species;”How was it determined that the site was overgrazed and had a plant cover of <8). Also, <8) is vague. A single canopy cover or range would be more appropriate than stating <8).

RESPONSE. We are changing the text as suggested and adding in the text a range from 5 to 8% basal plant cover instead of the original value. Indicators of overgrazing in these dry tropical grasslands include plant cover losses, substantial change in grass species composition due to the lost of key species, or both. In our overgrazed and shrub encroachment sites there is currently a reduction of around 30% of basal plant cover. For a natural low plant cover observed in the semiarid grassland this reduction means that under overgrazed conditions there is a predominance of bare soil (> 90 %). This has important implications on ecological processes, such as hydrologic (Medina-Roldán et al., 2007), and biogeochemical (Medina-Roldán et al. 2008). Determinations of overgrazed conditions were carried out using results from the char-

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acterization of vegetation (plant cover and species composition) following procedures mentioned above.

COMMENT. 8. Page 17105, line 5: “(iv) the shrub encroachment site is also an overgrazed site with low plant cover (< 8 %) having co-dominance with a native shrub Isocoma veneta (Kunth) Greene and an exotic Mediterranean perennial herb Asphodelus ïňĄstulosus L.;” change to “(iv) a shrub encroachment site which was also an overgrazed site with low plant cover (< 8 %) having co-dominance with a native shrub Isocoma veneta (Kunth) Greene and an exotic Mediterranean perennial herb Asphodelus ïňĄstulosus L.;” How was plant cover estimated?

RESPONSE. Suggested changes in the text were incorporated to the next version. Plant cover assessment was determined exactly the same as mentioned previously for the other sites.

COMMENT. 9. Page 17105, line 8: “(v) the crop site, a shortgrass steppe ïňĄeld converted to rainfed agriculture to produce oat (Avena sativa L), that is covering the soil for around 4 months during summer.” Change to “(v) a crop site, previously a shortgrass steppeïňĄeld converted to rain-fed agriculture to produce oats (Avena sativa L). The oat crop covers the soil for approximately four months during summer.”

RESPONSE. Change was incorporated as suggested

COMMENT. 10. Page 17105, line 16: “When measurements were made, this chamber was sealed to permanent iron bases. . .” change to “When measurements were made, the chamber was sealed to permanent iron bases. . .”

RESPONSE. Change was incorporated as suggested

COMMENT. 11. Page 17105, line 18: “Iron bases were installed and allowed to equilibrate with the ground for 2 months prior to the beginning of this study.” Change to “The iron bases were installed and allowed to equilibrate with the ground for two months prior to the beginning of the study.”

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RESPONSE. Change was incorporated as suggested

COMMENT. 12. Page 17105, line 24: “One fan was oriented horizontally, while the other was vertically oriented, both had a ïňĆow rate of 54.3 m3 h-1 CO2 and H2O concentrations and atmospheric pressure inside the dome were measured using an open-path infrared CO2/H2O gas analyzer (IRGA; Li-7500, Li-Cor Inc., Lincoln NE) lo- cated in the cen- ter of the plot and also mounted 0.5m a.g.l.” Split this sentence up into two separate sentences. First sentence: “One fan was oriented horizontally, while the other was vertically oriented, both had a ïňĆow rate of 54.3 m3 h-1. Second sentence (with mod- ïïňĄcations to the original): “Carbon dioxide and H2O concentrations, and atmospheric pressure inside the dome, were measured using an open-path infrared CO2/H2O gas analyzer (IRGA; LI-7500, LI-COR Inc., Lincoln NE) located in the center of the plot and also mounted 0.5m a.g.l.” Also, was the LI-7500 “white box” inside the dome? The white box contains the pres- sure sensor. The pressure sensor is not on the IRGA “sensor head”. If the white box was not in the dome, then pressure changes inside the dome during a measurement were not being recorded properly. Please men- tion this in the manuscript. Finally, 0.5 m a.g.l. was probably a good height, but how did you determine this height? Were there lab test to determine that proper air mixing was occurring with a fan at 0.5 m a.g.l? Please explain fully in the manuscript.

RESPONSE. Change was incorporated as suggested. Respect to the location of the IRGA circuit box, this was located outside the dome therefore we do not count with records of pressure changes. We followed the basic design and the operation param- eters set by Arnone and Obrist (2003) and other studies using the geodesic dome method (Huxman et al. 2003, Jasoni et al. 2005, Harpole et al. 2007). Character- istics of the fans, their orientation and height as well the IRGA position were tested and described in these previous studies. We just tested our dome for air leakages. The white box was placed outside the dome so available pressure data does not re- flect chamber pressure. Still for this large static chambers, changes of pressure by overpressurization that could lead to inhibition of soil respiration fluxes (Davidson et al.

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2012) (when for instance, the chamber is positioned on the iron base) are not expected (which would be expected by open-flow gas exchange systems) because of the large volume:area ratios. Large volume:area ratios also allow small changes of the gas density avoiding molecular diffusion across the soil surface driven by a gas concentration gradient (Livingston and Hutchinson 1995). The observed linear variation in [CO2] and [H2O] for most measurements is evidence that no changes in pressure occurred inside the chamber. Intermittent pressure changes during the 2-3 minutes monitoring periods would have been observed as erratic [CO2] and [H2O] values.

COMMENT. 13. Page 17106, line 8: "Meteorological sensors were used inside and outside the chamber during each measurement" Which sensors (the ones you list after this sen- tence) were used inside the chamber and which sensors outside the chamber?

RESPONSE. We had a complete set of the sensors mentioned in the text for outside and inside the dome. We will modify the text to make it clear about the disponibility of these sensors.

COMMENT. 14. Page 17106, line 9: "PPFD, (PARLITE, Kipp and Zonen, Delft, Holland), Ta, (PRT type, RTD-810, Omega Engineering Inc., Stamford CT) with a linearizer (OM5-IP4- N100-C, Omega Engineering Inc., Stamford CT), and SWC (Mini Trase, SoilMoisture Equipment Corp., Santa Barbara CA)." Soil water content (SWC) is listed but really it's not a meteorological measurement. Also, were there soil moisture sensors at every measurement plot? How deep were the soil moisture sensors? Did the soil moisture sensors measure continuously or was this a spot measurement? Much more detail is needed here for all the sensors to indicate how often the sensors measured, how high or how deep they were installed.

RESPONSE. Sensors were move from plot to plot together with the IRGA sensor so PPFD and Ta records correspond only to the time period when we got the NEE measurements. The PPFD sensor as well as air temperature sensor were located at the

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canopy height of monitored vegetation (from soil level-overgrazing, to 40 cm-oat). Sensors inside the dome were located close to the center whereas sensors outside were located around five meters away from the dome. In the case of soil water content, we inserted permanent steel rods to 15 cm depth, since this correspond with most of the root system distribution. In this case, readings on each plot (outside and inside) were carried out once during the day at noon. This paragraph will be added to the manuscript. Also, we will separate the SWC from meteorological variables.

COMMENT. 15. Page 17106, line 16: ". . .and to examine for potential chamber effects." change to". . .and to determine potential chamber effects."

RESPONSE. Change was incorporated as suggested

COMMENT. 16. Page 17106, line 22: "The duration for each chamber on the iron base, deïňĄning the sampling period was âĹij 120 s during daytime and âĹij 180 s during night time." Change to "During each ïňĆux measurement, the chamber was placed on a plot for âĹij 120 s dur- ing daytime and âĹij 180 s during nighttime." Also, "dome" is sometimes used and then "chamber", please select either dome or chamber and use this consistently throughout the manuscript.

RESPONSE. We will use dome throughout the manuscript. Also, change was incorporated as suggested.

COMMENT. 17. Page 17106, line 25: ". . .data for NEE calculations however, were only used after the ïňĄrst 20 s, i.e. once a constant rate of [CO2] change inside the dome was observed." Did you use all of the data after 20 seconds or was data at the end of the measurement also omitted? Typically in chamber measurements, the rate of change in CO2 tends to level off before 120 seconds has elapsed due to water vapor dilution. Did you only take the linear portion of the slope or the entire slope after 20 seconds? This needs to be clearly discussed and explained in the manuscript. If you used the entire 120 seconds and the rate of change in CO2 was not linear the entire time, then your ïňĆux measurements are not accurate.

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RESPONSE. For NEE calculations we used just the linear trend after 20 second (after stabilization). To eliminate effects of water vapor dilution, the flux was calculated as dry air basis (Eq. 2). A standard flux calculation procedure for static chambers was used to obtain CO2 fluxes, further information can also be found in other studies (e.g. Arnone and Obrist 2003, Jasoni et al. 2005, Richard et al. 1996, instruction manual for Li-8100, Licor 2010). Adding a phrase indicating these details we think will clarify doubts about NEE calculation.

COMMENT. 18. Page 17107, line 20: "Daytime and nighttime NEE were analyzed separately be- cause they differ in their controls and the way they were inïňĆuenced." InïňĆuenced by what? Is the "inïňĆuence(s)" different than the controls? This needs to be clariïňĄed.

RESPONSE. In this case, we are referring only to the effects of the controls. We will add the particular controls for day and night NEE and the direction of the effects if any.

COMMENT. 19. Page 17108, line 9: Missing minus sign in front of the subscript "2000 umol m2s-1". Should be "2000 umol m-2 s-1" please check that minus signs are present in superscripts (where needed) throughout the manuscript, it is an issue in several other places.

RESPONSE. We corrected the missing sign and we will check throughout the manuscript.

COMMENT. 20. Page 17108, line 11: "For the period from March to May, an exponential function was used to describe the relationship between NEEdaytime and air temperatures." Change "temperatures" top "temperature".

RESPONSE. Change was incorporated as suggested

COMMENT. 21. Page 17108, line 12: "For the period from March to May, an exponential function was used to describe the relationship between NEEdaytime and air temperatures. In this period, since vegetation was senescent there was not active grass

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leaves (LAI _ 0) in all sites, thus no C acquisition at daytime occurred and therefore NEE did not respond to PPFD but only to temperature since the predominant ïňĆux corresponded to Re.". This sentence needs major rewording and is very confusing as it is currently written. Also, were there biological soil crusts at the site? If so, they could be active when plants were senescent. Please discuss the possible role (if there is one) of biological soil crusts. Also, why were the plants senescent during March and May? Seems like it would be springtime at this southern latitude during March and May with lots of plants greening up during this time.

RESPONSE. The sentence was modified as following. An exponential function was used to describe the relationship between NEEdaytime and air temperatures in the period from March to May. In this period the semiarid tropical grassland is senescent therefore no active photosynthetic leaves are observed (LAI $\sim$ 0). Recorded fluxes correspond to ecosystem respiration (Re) that only responded to temperature. Regarding the comment by the reviewer he is right that this period correspond to Spring. However, the semiarid tropical grassland from central Mexico, exhibits a summer precipitation regime with only a 10% of rain distributed during winter (December to January). Winter rain is not big enough to produce vegetation responses. There is a large drought period lasting 6 to 8 moNths starting in October-November and ending in June-July. Hence vegetation is basically shut down until the next rain season. Biological soil crust is also an important component of the ecosystem playing fundamental roles in ecosystem processes. In the semiarid grassland, plant interspaces are covered by biological soil crusts (BSC) in a proportion of between 10 to 75%, mainly made up by cyanobacteria (Concostrina 2010). However, similar to vegetation, both photosynthesis and respiration activity in BSC is also controlled by soil moisture (Cable and Huxman 2004, Bowling et al. 2011, Lange et al. 1998). Therefore , during the long drought periods in this region it is not expected BSC activity. During the growing season it is likely that BSC had a role in the observed carbon uptake (Fig. 5), but would be difficult to discriminate between either plant or BSC uptake without direct measurements on BSC. In situ BSC net ecosystem exchange exhibit small maximum net carbon uptake rates ($\sim$0.5 μmol

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m-2 s-1, Bowling et al. 2011) during short time-periods, however most time BSC contribution appears to be counterbalanced by soil respiration (probably what happened in our study, Bowling et al. 2011, Cable and Huxman 2004). Even though BSC have the capacity to use smaller precipitation pulses than vegetation, BSC also require large precipitation pulses to achieve a net carbon uptake (Belnap et al. 2004). Finally, the small winter rains in our study were probably not enough to stimulate C uptake, but respiration by the BSC. A further discussion would be included in the manuscript.

COMMENT. 22. Page 17109, line 8: "Daytime CO2 ïňĆuxes: to calculate the continuous ïňĆuxes for each daylight measurement period (hereafter integrated daytime NEE). . ." change to "Daytime CO2 ïňĆuxes: to calculate continuous ïňĆuxes for each daylight measurement period (hereafter integrated daytime NEE). . ."

RESPONSE. Change was incorporated as suggested

COMMENT. 23. Page 17109, line 12: "Same latitude (21.7_N) and longitude (− 101.6), relative humidity (30 %), and altitude (2200m a.s.l.) were assumed in the model for the ïňĄve sites." According to the clear sky calculator website provided in the previous sentence,it appears that air temperature is also needed for this calculation. How did you estimate air temperature at all sites? Were there air temperature sensors at all the sites? Also,in order to make the model on the website work properly you have to enter every day of the year separately? Did the author manually type in every day of the year to get a PPFD value for each day of the year? Additionally, why assume 30% relative humidity for everyday? Changes in RH, and air T make a difference in the modeled PPFD. There seems to be a lot of assumptions here that then translate into assumptions in the continuous daytime NEE. As this section is currently written it casts doubt on the accuracy of the continuous NEE values.

RESPONSE. Relative humidity (30%) and air temperature (25 °C) were set as standard environmental conditions in all sites and months. The PPFD apogee model is sensitive to changes in relative humidity and temperature, for instance, the model gives a ∼8%

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deviation of the calculated PPFD at 25 °C with 50% change of relative humidity (model accuracy and sensitivity, http://clearskycalculator.com/model_accuracyPPF.htm). In our case, a comparison between modeled PPFD and measured PPFD for the moderate grazing site that counted with a meteorological station, showed a PPFD subestimation of 3 ± 1.5%, caused by an overall lower mean air temperatures than 25 °C (higher air temperature reduces PPFD due to increasing attenuation by water vapor molecules). Because of the low PPFD deviation and its homogeneity among sites, we think this is not an issue for our NEE estimations.

COMMENT. 24. Page 17110, line 4: . . ."NEE rates and climate was assumed to be representative for. . ." change the word "was" to "were".

RESPONSE. Change was incorporated as suggested

COMMENT. 25. Page 17110, line 6: Spell out leaf area index and then abbreviate (LAI).

RESPONSE. Change was incorporated as suggested

COMMENT. 26. Page 17110, line 7: ". . .six 0.25m2 quadrants, positioned next to- and outside the chamber. . ." change to "". . .six 0.25m2 quadrants, positioned next to- and outside of the chamber. . ."

RESPONSE. Change was incorporated as suggested

COMMENT. 27. Page 17111, line 4: "This allows us to homogenize environmental conditions. . ." change to "This allowed us to homogenize environmental conditions. . ."

RESPONSE. Change was incorporated as suggested

COMMENT. 28. Page 17111, line 10: Change the word "estimation" to "estimated".

RESPONSE. Change was incorporated as suggested

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COMMENT. Results: 1. Page 17111, line 22: " âĹij 442 mm yr-1. Why approximately (âĹij)? Why not an actual value? This occurs in several places within the manuscript and should be ïňĄxed where appropriate.

RESPONSE. Symbol was removed and other uses of the symbol were reviewed.

COMMENT. 2. Page 17113, line 3: . . ."(p > 0.05, Figs. 3a and 4, Table 2)." The P should be italicized.

RESPONSE. P was italicized

COMMENT. 3. Page 17113, line 6: "The crop was the only site showing positive NEE ïňĆux for November, when exhibited no plant cover." Change to ": "The crop site was the only site showing positive NEE ïňĆux for November, when there was no plant cover." I'm assuming when you state positive NEE you mean release of CO2?

RESPONSE. Change was incorporated as suggested. Yes positive NEE refers to release.

COMMENT. 4. Page 17113, line 7: "Still, in July the crop cover showed a positive C ïňĆux, but in August it changed to the largest NEEdaytime capture (6.08, asymmetric s.e. +4.82, − 0.94, gCm − 2 d − 1) coinciding with maximum leaf out and grain-ïňĄlling stages." Does the word "capture" intended to mean CO2 uptake? Also, in the parentheses there is a number of 6.08, if this is an uptake value then according to your previous deïňĄnition of negative values being uptake and positive values being release, then the value should be -6.08. Please be careful to maintain your sign convention, otherwise, things get confusing in a hurry and very difiňĄcult for the reader to ïňĄgure out.

RESPONSE. Yes, capture refers to uptake and we added the negative symbol to 6.08

COMMENT. 5. Page 17113, line 12: "Sites with lowest plant cover (overgrazing and shrub encroachment) showed three months lower C uptake than those found from the other sites." This is a very confusing sentence. Unclear as to what ". . .showed three

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months lower C uptake. . .” means? Do you mean three “times” lower uptake? Please clarify.

RESPONSE. We were referring to a period of three months. So we reworked and inserted in the text these lines as follow: Sites with the lowest plant cover (overgrazing and shrub encroachment) showed for at least three months lower C uptake as compared to any of the other sites during the same period.

COMMENT. 6. Page 17113, line 19: “NEEnighttime ïňĆuxes in the overgrazed site were the smallest throughout the year (< 0.45 gCm $-$ 2 d $-$ 1), resulting in signiïňĄcant lower annual. . .” Probably should not use a < sign here. It leads the reader to wonder how much less than 0.45. A range or a value with an error associated with it would be more appropriate. Also, is this an uptake value or a release? Also, change “signiïňĄcant” to “signiïňĄcantly”.

RESPONSE. We took the advice and included a range from 0.1 to 0.45 instead of the symbol. This value represents a nighttime flux. We also changed the word as suggested.

COMMENT. 7. Page 17113, line 22: “The Oat crop site maintained intermediate NEEnighttime rates that were signiïňĄcantly higher than the overgrazed site ïňĆuxes.” Not clear what the word “intermediate” is referring to. Are the rates intermediate in relation to the other sites except the overgrazed site? Please clarify.

RESPONSE. The term intermediate referring to NEE nighttime at the oat crop referred to the magnitude of the efflux.

COMMENT. 8. Page 17113, line 24: “We estimated annual rates of productivity, however, data for both December and April were not included due to sampling logistic problems.” Please omit the word “logistic”.

RESPONSE. The word “logistic” was removed from the text

COMMENT. 9. Page 17113, line 26: “Day and nighttime NEE rates were > $-$ 0.34 and

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< 0.43gCm − 2 d − 1, respectively, for winter months (November to March), with net diurnal rates around 0 μmolm − 2 s − 1." Please change "day" to "daytime". Is the <-0.34 and < 0.43 a range? If so then the <> signs are not necessary, just add the word "between" after "were".

RESPONSE. The comment is correct, values are referring to a range of maximum and minimum rates estimated during this period. As suggested we removed the <> symbols and added the term "between" instead of "were" as suggested.

COMMENT. 10. Page 17114, line 5: ". . .(net CO2 uptake of 0.47, 0.26 and 0.08 gCm − 2 d − 1, respectively, Fig. 4a, b, or 145, 77, and 25 gCm − 2 yr − 1, respectively, Fig. 4a, b). In contrast, the exclosure behaved as a source (net CO2 loss of 0.085 gCm − 2 d − 1, or 25 gCm − 2 yr − 1, Fig. 4a, b) whereas the moderate grazing site was carbon neutral (0.003 μmolm − 2 s − 1, or 0.26 gCm − 2 yr − 1, Fig. 4a, b)." Errors need to be presented for all of these values, especially the annual values. Also, since all these values are uptake they should have a negative sign in front of them to be consistent with your sign convention. Additionally, why is 0.26 g C m-2 yr-1 carbon neutral? Granted, it is a small value, but not neutral, zero would be neutral.

RESPONSE. We are adding error values to the annual C balance, we are also including positive or negative signs according to sign convention. Regarding the value of the annual C balance for the Moderate grazing site (0.26 g C m-2 yr-1), we considered this value as neutral since C captured is almost negligible. In addition, error for this C budget value overlapped with the zero value.

COMMENT. 11. Page 17115, line 14: "Sites of contrasting plant cover differed respect to what was the main NEEdaytime driver." This is a very confusing sentence. Please reword.

RESPONSE. Phrase was changed to: Drivers for NEEdaytime exerted different effects on sites with high plant cover in comparison to sites with low plant cover.

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COMMENT. 12. Page 17115, line 15: “Thus, while both SWC and LAI explained > 56% (linear relationship) of NEEdaytime variation in sites with good cover, the overgrazed and crop sites in contrast showed only a relationship to LAI, but through a quadratic and linear relationship, respectively.” This is a very confusing sentence, Please reword.

RESPONSE. Phrase was changed to: Thus, while both SWC and LAI explained > 56% (linear relationship) of NEEdaytime variation in sites with high cover, the NEEdaytime of overgrazed and crop sites were described by a quadratic and linear model with LAI respectively.

COMMENT. 13. Page 17115, line 20: “Although data did not allow to perform an homogeneity of slopes test, moderate grazing site showed almost doubled the assimilation rate per unit water stored in soil (0.1 ± 0.02 gCm $-$ 2 d $-$ 1% SWC $-$ 1) than exclosure and shrub en- croachment sites (0.0636 ± 0.017, and 0.062 ± 0.025 gCm $-$ 2 d $-$ 1% SWC $-$ 1, respec- tively).” Reword to “Although data did not allow a homogeneity of slopes test, the mod- erate grazing site showed almost twice the assimilation rate per unit of water stored in soil (0.1 ± 0.02 gCm $-$ 2 d $-$ 1% SWC $-$ 1) than the exclosure and shrub encroachment sites (0.0636 ± 0.017, and 0.062 ± 0.025 gCm $-$ 2 d $-$ 1% SWC $-$ 1, respectively).”

RESPONSE. Rewording has been incorporated into the manuscript.

COMMENT. Discussion: 1. Page 17116, line 1: “Sites were also grouped regarding the proportion of plant cover by empirical parameters of the exponential part of Eq. (6).” Confusing sentence, please reword.

RESPONSE. Phrase was changed to: Empirical coefficients a and b of the first term of Eq. (6) were contrasted between the two categories of plant cover.

COMMENT. 2. Page 17116, line 17: “Comparison of shrub encroachment and overgrazed sites revealed no differences in NEEdaytime, neither there were differences in NEEnighttime ($P > 0.05$, Fig. 4).” Reword to ““Comparison of shrub encroachment and

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overgrazed sites revealed no differences in NEEdaytime and NEEnighttime ($P > 0.05$, Fig. 4)."

RESPONSE. Rewording has been incorporated in manuscript.

COMMENT. 3. Page 17117, line 17: "If we assume belowground mass similar to that found aboveground, there would be 2- to 10-fold greater root biomass of these two sites compared to the sites with low plant cover, and this could account for the observed 3 to 5 $\times$ higher Re rates." This is a large assumption. Please provide a reference for this statement.

RESONSE. Reference supporting this statement is located in line 20 same page (Medina-Roldán et al., 2007). Additionally, the following figure including data from this study will be inserted in the final manuscript RESPONSE. SEE FIGURE 1

COMMENT. Figures and Tables: 1. Table 1: Not sure if comparing sites is what should be done in this study. Consider looking at changes and relationships within a site and not between sites.

RESPONSE. We are not sure about the meaning of this comment. Our results present comparisons as suggested within and between sites. The within site changes are temporal (monthly) and just for the year of this study. These annual temporal changes can be attested in Figs. 2, 3, 4b. We need more feedback about this recommendation.

COMMENT. 3. Figure 3 and 4: Please edit y-axis title to add a space between "g" and "C".

RESPONSE. Axis was edited

COMMENT. 4. Figure 4: I don't think the capital "A" is needed in panel (a) since all the bars are statistically similar. Letters are only needed if there are differences. However, it's not a huge issue so it is an author decision.

RESPONSE. We decided to keep the capital A, since differences among bars in the

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uptake part of the figure are wide, therefore if not indicated figure might be misinterpreted.

COMMENT. 5. Figure 5: Please use the alignment feature in your plotting program to properly line up the six different panels. Currently they are not lined up properly.

RESPONSE. Figure has been edited with a graph editor

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C9103/2013/bgd-9-C9103-2013-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 9, 17099, 2012.

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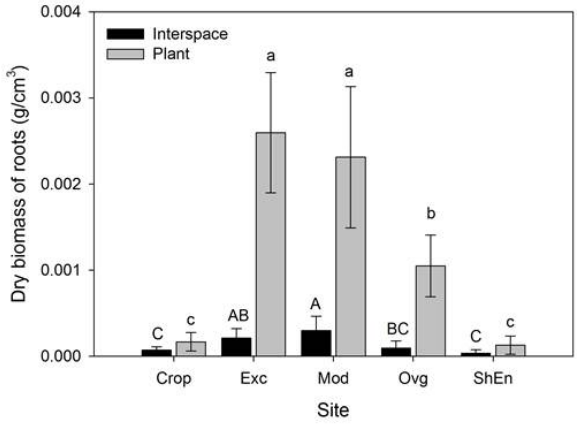

Figure 1. Root biomas distribution in five grassland sites subjected to Different land use

Fig. 1. ROOT BIOMASS DISTRIBUTION AMONG FIVE GRASSLAND SITES SUBJECTED TO DIFFERENT LAND USE

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