We thank the editor and the two reviewers for their time and their constructive comments, which will certainly help us to improve the manuscript. All the comments have been responded with detailed explanations, especially for the second reviewer's, and related text has been revised. Below we show the reviewer's comments in *italic*, and our responses (together with revised parts in manuscript) follow in normal font.

Reviewer # 1

General comments: By carrying out a series of field and laboratory experiments, the authors proposed a novel mechanism which may explain the observed CO_2 sequestration by the saline/alkaline desert ecosystem. The authors focused on a heated debate over whether and to what degree a terrestrial inorganic carbon sink could contribute to the "missing sink" for carbon. They found that the passive leaching of CO_2 through groundwater table fluctuations seem to explain the downward CO_2 fluxes measured by both the eddy-covariance technique and the chamber method. This manuscript is quite interesting and was well written in general. Although I feel that the conclusion offered by the authors could not be fully evidenced by their experiments (see specific comments), publication of this paper may foster further studies that reveal the role of inorganic processes in regional or global carbon budgets. Some revisions and clarifications are needed, however, before this article can be accepted for publication in Biogeosciences.

Response: Thank you for the positive feedback concerning the importance of our work. Our responses to specific comments are listed below point by ponit.

According to the authors' conclusion, the observed downward CO_2 fluxes were dissolved into the saline/alkaline soil and then taken away by the rises and falls of the ground water table. Even if the "passive leaching" observed in lab did occur at the field site, there is no reason to say that this process is everywhere in arid or semiarid areas. As the authors stated in the article, such a passive leaching process requires saline/alkaline soils and fluctuating groundwater table. Both conditions, however, are typical of desert-oasis ecotones. For the vast area of deserts, the groundwater could be deep and never reaches the shallow soil layers. In addition, the saline/alkaline soils, which could dissolve a substantial amount of atmospheric CO_2, are usually associated with a shallow groundwater table in arid and semiarid areas. To my understanding, it is hard to reach a solid conclusion at this stage that this phenomenon could aid in the global carbon budgeting by contributing to the "missing sink". The passive leaching may occur within a limited geographic range which does not represent the vast majority of arid and semiarid ecosystems. The authors should mention this caveat when trying to extrapolate their results to other regions.

Response: We fully agree and will introduce this important point in the discussion. (P22. L5-13)

Based on the authors' investigations on plant biomass, the vegetation seemed to have no contribution to the carbon absorbed by the ecosystem (section 3.3). However, they also showed that the downward CO_2 fluxes occurred during the growing season for a ten-year period (Fig 1c). Both the gross primary productivity (GPP, Fig 1b) and net primary productivity (NPP, Fig 3) demonstrate substantial carbon sequestration by the vegetation. In addition, they used a light response model (Michaelis-Menten) of photosynthesis to fill the gaps in the dataset, indicating that plants did assimilate carbon during the growing season through photosynthesis. The question is why plant photosynthesis did not result in increases in biomass? Remember that the dominant vegetation there is perennial shrub species, which could accumulate biomass year after year. Some discussions are necessary to explain the invariant biomass. Is it because plant biomass had reached a carrying capacity so that new biomass offset dead biomass?

Response: Yes, the unchanged plant biomass is due to the equilibrium between the new biomass and dead biomass. This shrub-dominated stable vegetation has long reached its maturity. Discussion on the account of this invariant biomass has been added. (P19. L8-22)

If it was the case that new biomass offset dead biomass so that the standing biomass was in an equilibrium state (0.78 kg m-2 in 1989 and 0.74 kg m-2 in 2009; line 26, page 10431), the soil should have received a substantial amount of organic litter input. However, the authors also showed similar soil organic and inorganic carbon contents between the starting and ending of the 20-year period (line 27, page 10431). Again, it is needed to explain where did the dead biomass go? Is it because the decomposition rate offset the litter input?

Based on the above two points, can readers of this article draw the conclusion that the biotic component of the ecosystem is carbon neutral, i.e., CO_2 assimilated by plants was all respired by autotrophic and heterotrophic respiration? Therefore, both the plant carbon pool and the soil carbon pool were unchanged.

Response: As referee #1 analyzed, we can conclude that the biotic component of the ecosystem is carbon neural. In the study site, the shrubs are sparely distributed (plant coverage is approximately 17%) and organic litter mainly dispersed under the canopy, where the microbial activity is strong. In addition, the desert shrubs have strong canopy interception effect, which induces higher soil water content under canopy than in bare area, which also speeds up the litter decomposition rate to equalize the organic litter input rate. For the bare soil without almost any litter input, the carbon content hardly changes. Therefore, in the long run, CO_2 assimilated by plants is all respired by autotrophic and heterotrophic respiration. New references have been in the discussion to clarify this point. (P19. L8-22)

The dissoluble organic carbon may also be leached from the soil. How to rule out this possibility in explaining the downward CO_2 fluxes?

Response: While we can not totally rule out the possibility of dissoluble organic carbon leaching from the soil, the organic matter content in study area is very low (less than 1%) and dissolvable organic carbon must be even lower. More importantly, soil organic carbon mainly concentrates at the topsoil and decreases with soil depth, where the dissoluble organic carbon are hardly leached by limited rainfall in "passive leaching" pattern. Therefore, within this context, we assume the leaching carbon is in the dissolved inorganic carbon form. New references have been added in discussion to verify this point. (P20. L21-22; P21. L1-6)

If atmospheric CO_2 was indeed sucked into the soil (line 15, page 10431), then it is problematic to use the term "ecosystem respiration" to represent nighttime fluxes measured by the eddy-covariance technique. Similarly, the term "soil surface flux" should be used instead of "soil respiration". Respiration, by definition, describes biotic processes that release CO_2 into the atmosphere. In addition, I am curious about whether and how this inorganic process may obscure the relationship between nighttime net ecosystem exchange (NEE) and environmental factors (e.g., soil temperature).

Response: We fully agree the term "respiration" by definition is not appropriate to represent the process of atmospheric CO_2 downward into soil. In a previous study (Ma et al., 2013), we found that an "inorganic respiration" − the effusion and dissolution of CO_2 into and out of the soil solution − can lessen nighttime soil surface flux or even make it negative (atmospheric CO_2 moves downwards into the soil), but enhance soil flux during the daytime. Namely, with the involvement of inorganic process, soil respiration may be significantly underestimated during night and overestimated during the day. Therefore, the underestimation of night time flux could obscure the relationship between nighttime net ecosystem exchange (NEE) and environmental factors. A good example is the relationship between respiration and temperature at night that is commonly used to extrapolate ecosystem respiration for the daytime. In ecosystems with saline/alkaline soils, underestimation of night time flux would significantly underestimate the C efflux and thus result in an overestimation of the net primary productivity. Thus, in this context, "soil respiration" will be replaced by "soil surface CO_2 flux". Related contents in the text have been revised. (P6. L16; P16. L2. 5. 9; P20. L14-15)

Section 2.7 describes leaf photosynthesis measurements, but I did not see results related to these measurements. Were they used to estimate NPP canopy? If so, how stem respiration was determined?

Response: Sorry for not making this clear in the manuscript. Leaf net photosynthesis was scaled up by leaf area index (LAI) to estimate NPP canopy. For the stem respiration, results of preliminary experiments shows that the respiration rate of stem is so low in terms of contribution to the total ecosystem respiration (no more than 2%, unpublished data) that it can be reasonable ignored. The method part has been rewritten and a figure, presented diurnal variations of leaf photosynthesis rate and LAI dynamic during the growing season in 2009, was added in the Supplement. (P8. L12-21; P9. L1-21; P15. L17; Fig. S1)

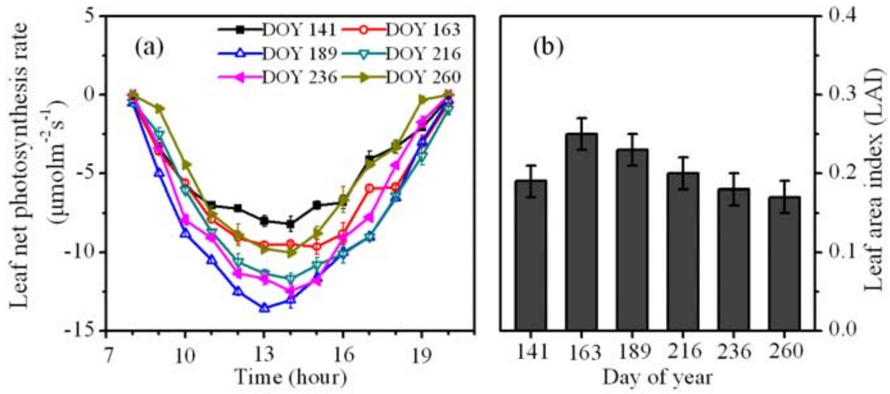

Fig. S1 Diurnal variations of leaf net photosynthesis rate of Tamarix ramosissima (a) and leaf area index (LAI) dynamic (b) during the growing season in 2009.

The authors validated their eddy fluxes against chamber measurements of soil respiration and NPP (line 25, page 10430). It is needed, in the Methods section, to mention how NPP was measured by the chamber method and how NPP measured in the chamber was scaled up to match the footprint area of the eddy-covariance instrument.

Response: As stated in AC6, NPP canopy was estimated by scaling up leaf net photosynthesis with LAI, which was monitored in the center of the footprint area. The method about chamber-based estimation of NPPcanopy has been modified. (P8. L12-21; P9. L1-21)

The authors used an exponential relationship between respiration and soil temperature in gap-filling (line 28, page 10424), whereas they used a Lloyd-Taylor function in extrapolating ecosystem respiration from nighttime to daytime (line 23, page 10425). Is there any explanation why use different models?

Response: Thanks you for pointing out this vague expression. The same Lloyd-Taylor function was used for gap-filling during the night and extrapolating daytime ecosystem respiration from nighttime measurements. It has been described more clearly in method part. (P7. L11-14)

Table 2 seems redundant to me as all related results appeared in the text (section 3.3).

Response: Thanks for point this. Table 2 has been removed.

The authors should avoid explaining or discussing their findings in the Results section. For example, *the sentence at line 4, page 10430 and that at line 16, page 10432.*

Response: Related text has been revised as proposed. (P17. L17-22)

Line 14, page 10421, change "With its characteristics of : : :" to "With characteristics such as : : :"

Line 27, page 10422, change "Here it is hypothesized that : : :" to "Here, we hypothesized that : : :".

Line 18, page 10427, should it be "packed with stratified (: : :) soil samples"?

Line 28, page 10430, should be "on six days".

Line 19, page 10432, delete "was".

Line 22, page 10432, the first sentence describes methods instead of results. A possible revision could be "The laboratory leaching experiment showed that : : :".

Response: Thanks for technical comments and related text have been revised as proposed. (P3. L4; P5. L1; P11. L16-18; P18. L1-2)

Line 18-21, page 10424, this sentence needs rewording. In addition, was the u filter applied only to nighttime data or to both day and night?*

Response: The u* filter was applied to both daytime and nighttime data, although for daytime, low friction velocity hardly occurs. This sentence has been rewritten. (P7. L11-14)

Line 2, page 10430, it is needed to clarify which test was used to yield $P > 0.05$. In addition, the value of the statistic should be provided.

Response: We used Pearson correlation analysis to test the linear relationship between annual NEE and precipitation. The Pearson's correlation coefficients and P values have been added. (P14. L14; P15. L7)

Line 2, page 10432, should it be "$P > 0.05$ for all pairs" ?

Response: Thank you for pointing out this error. It has been revised. (P17. L4)

Reviewer # 2

The authors use a clever title to draw attention to the controversy about carbon sequestration in deserts. Although long debated, the controversy was heightened by Richard Stone in his Science article widely publicized deserts as possible CO_2 *sinks by asking whether "researchers have found a missing loop in the carbon cycle" (Stone 2008). Studies by Wohlfahrt et al. (2008) in the Mojave Desert of southwestern USA and Xie et al. (2009) in the Gubantanggut Desert of northwestern China made that implication.*

It was pointed out by Schlesinger et al. (2009), however, that the numbers don't add up. To sequester that much carbon an unrealistic amount of biomass should have been produced. Likewise, if the carbon was sequestered as pedogenic $CaCO_3$ *an unrealistic amount of calcium would be needed from chemical weathering or atmospheric additions.*

The study by Ma et al., like the study by Xie et al., was conducted in northwest China at the Fukang Experimentation Station. They acknowledged that carbon did not go into biomass or pedogenic carbonate based on comparisons with data made when the station was established in 1989. Still, their eddy-covariance measurement from 2002 to 2012 showed net CO_2 *uptake at the study site. They checked the accuracy of their measurements with a closed-chamber method and both methods yielded similar readings.*

To account for where the carbon goes they proposed a "passive leaching" mechanism. Atmospheric CO_2 *is brought down by photosynthesis, respired into the soil profile where a portion is converted into dissolved inorganic carbon (DIC). The DIC then moves into the water table when it is high and carried away when the water table falls. As evidence to support their model, they have 30 years of data showing a water table that fluctuates between about 1 to 3 meters. They also conducted a lab experiment on a soil column simulating a fluctuating water table and used δ13C values to trace carbon from* CO_2 *to DIC.*

The paper makes a great contribution, despite the carbon isotope experiment being hard to follow. It is clear that field measurements of DIC in soil and groundwater are now needed to test their hypothesis. The major problem with applying the passive leaching model globally is that many, if not most, desert do not have shallow fluctuating water tables. Vadose zones in these deserts can be 10 to over 100 meters deep. Where would the carbon go in these deserts?

Response: We are very grateful for the referee's positive appraisal of our work. The laboratory soil column leaching experiment, presented here, was just a test of our hypothesis on "passive leaching": surface dissolve inorganic carbon (DIC) can be leached by fluctuations in groundwater. We have streamlined the text in the related parts of Materials and Method, and Results, in the hope that it is now easier to follow (P11. L16-18; P12. L21; P13. L1, 4-5; P17, L17-22; P18, L1-2).

In this study, the saline desert is typical of desert-oasis ecotones. With high groundwater level and strong groundwater fluctuation, the passive leaching of DIC is significant (25 g C m^{-2} y^{-1}). For the vast desert, of which the groundwater can be 10 to over 100 meters deep, the passive leaching should be very limited. Instead, they probably play a greater role in carbon storage of groundwater, which is transported horizontally from the passive leaching active areas. We fully aware that the passive leaching presented here may occur only within a limited geographic range. To clarify this, field measurements of DIC in soil and groundwater should be ways to advance this line of research. Discussion on this point has been added. (P22, L4-13).