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Interactive comment on "A downward CO_2 flux seems to have nowhere to go" *by* J. Ma et al.

Anonymous Referee #1

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General comments

By carrying out a series of field and laboratory experiments, the authors proposed a novel mechanism which may explain the observed CO2 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 CO2 through groundwater table fluctuations seem to explain the downward CO2 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.

Specific comments

According to the authors' conclusion, the observed downward CO2 fluxes were dissolved into the saline/alkaline soil and then taken away by the rises and falls of the groundwater 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 CO2, 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.

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 CO2 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?

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., CO2 assimilated by plants was all respired by autotrophic and heterotrophic respiration? Therefore, both the plant carbon pool and the soil carbon pool were unchanged.

The dissoluble organic carbon may also be leached from the soil. How to rule out this possibility in explaining the downward CO2 fluxes?

If atmospheric CO2 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 CO2 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).

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

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 ex-

trapolating ecosystem respiration from nighttime to daytime (line 23, page 10425). Is there any explanation why use different models?

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

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.

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.

Technical comments Line 3, page 10421, delete "projections for".

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

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

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.

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

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

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 . . .".

Interactive comment on Biogeosciences Discuss., 11, 10419, 2014.