Dear reviewer,

we appreciate your constructive comments that will help to improve our manuscript. Please find our response (blue) to your comments (black) below.

Reviewer 1:

General comments

This is a carefully done study about the production, oxidation and emission of CH_4 in Patagonian bog, the results are of considerable interest and the paper is well written. However, some points need clarifying and certain statements require further justification.

Specific comments

1. the authors should not ignore that acetogenesis might be important in anaerobic environments when H_2 partial pressures are high and temperatures are low. Acetogens can outcompete methanogens at low temperature, as many acetogens seem to have a higher growth rate at low temperature than most methanogens (Kotsyurbenko et al., 1996, 2001). If acetogenesis process is active in the bog, the δ3C value of actetate in the porewater will be largely decreased because of the substantial fractionation during acetate production from CO_2 and H_2. And resultantly, the 13C value of CH_4 will also be lower and resulted in larger apparent isotopic fractionation factor (ac) between CO_2 and CH_4. Therefore, it's difficult to determine the relative importance of acetoclastic versus hydrogenotrophic methanogenesis pathway without the 13C value of acetate in this study.

We agree with the reviewer, that the d13C-CH4 signal was surprisingly low and therefore needs an explanation. Indeed, a strong effect of only methanotrophy should result in a less negative signature of d13C in CH4. We attempt to explain the low d13C-CH4 signal by the occurrence of microsite that create a mixed isotopic signal from methane production and consumption. As the reviewer correctly points out, the occurrence of acetogenesis is a reasonable explanation for the surprisingly negative d13C-CH4 signatures. Reviewer 2 suggests occurrence of hydrogenotrophic methanogenesis as a possible explanation. We agree that both pathways could explain the pattern in the d13C-CH4 signal, although we propose in accordance with the reviewer that hydrogenotrophic methanogenesis was probably relatively more important below pools while acetoclastic methanogenesis in combination with acetogenesis seemed to contribute more below Astelia lawns. But as we did not quantify other parameters such as labile organic matter from roots, acetate concentrations or its carbon isotopic signatures, we cannot clearly determine the relative importance of both pathways for our study, as the reviewer correctly states.

In the revised discussion, we will emphasize that the depleted d13C-CH4 signal is an unexpected result that needs better explanation. We will better explain possible sources for depleted 13C-CH4 within the rhizosphere and discuss the possibility of the occurrence of acetogenesis. The difficulties to separate the isotopic effects arising from methanogenic pathways will be elaborated. We will try to balance the suggestions of both reviewers by discussing the arguments for both, hydrogenotrophic and acetoclastic methanogenesis and acetogenesis, carefully.

2. In the first page, line 26-28, it's stated that: "Below the rhizosphere. . .. . .CH4 was predominantly produced by hydrogenotrophic methanogenesis". In fact, data in Figure 4def showed that the hydrogenotrophic pathway had higher contribution to CH_4 in the pool, while the acetoclastic pathway must play relatively more important role for the CH_4 production below the rhizosphere of Astelia Lawn. This is consistent with the supply of labile organic carbon from the root exudates of Astelia. To sum up, I think it's difficult to conclude that CH_4 is mainly produced from the hydrogenotrophic pathway below the rhizosphere of Astelia.

We agree with the reviewer that hydrogenotrophic methanogenesis was probably relatively more important below pools while acetoclastic methanogenesis seemed to contribute more below Astelia lawns. This seemed to have been even true below the rhizosphere of Astelia lawns, but is a result hard to explain from the data obtained in our study. As the reviewer correctly points out, labile organic matter from roots could be a possible explanation. But as we did not quantify other parameters such as labile organic matter from roots, acetate concentrations or its carbon isotopic signatures, we cannot clearly determine the relative importance of both pathways for our study, as the reviewer correctly states in the general comment 1.

In the revised abstract, we will rephrase that sentence on page 1, line 26-28. It will be emphasized in the discussion that the depleted d13C-CH4 signal is an unexpected result which needs better explanation. We will better explain possible sources for depleted 13C-CH4 and discuss the possibility of the occurrence of acetogenesis. The difficulties to separate the isotopic effects arising from methanogenic pathways will be elaborated. We will try to balance the suggestions of both reviewers by discussing the arguments for both, hydrogenotrophic and acetoclastic methanogenesis and acetogenesis, carefully.

3. It's stated that mean root lifetimes of A. pumila has been estimated to be ~3-4 years. So, whether the production and oxidation of CH_4 will be strongly affected in case of the turnover of large amounts of roots?

It is an interesting point that turnover i.e. presence and activity of roots should largely determine the occurrence of microsites. One could expect that temporal and spatial expansion of microsites is not static but varies with root life time and turnover. As reviewer 2 points to a partly speculative discussion about microsites in the current version of the manuscript, we will briefly include this aspect into the revised discussion.

4. Please check Table 3, the data in the last three columns are in wrong places.

Yes, the reviewer is correct. We will correct Table 3 accordingly.