We wish to thank the reviewers for their insightful comments and for the general appreciation of our study. We believe that these indications can significantly contribute to improve the manuscript and we hope that our replies will be satisfying. We reported the responses after each comment (in Italic).

Anonymous Referee #1

The introduction overall sets the scene for the research quite well. Yet, in its currents stage, I miss a little bit of a narrative. Often, facts are following each other without a clear connection of the sentences. Take for example the section in lines 20–28 on p.10274. The message is very complex and hidden in the summation of facts. In a very condensed space, plant–soil interactions and functional traits are introduced, as well as the concept of scale dependency. The paragraph ends with a statement on small–scale difference being more important then broad–scale differences, yet I wonder what that refers to: functional traits, the physical environment, the C sink capacity, or all.

The introduction part has been revised. We connected better the concepts explained in different paragraphs and tried to make the argumentation more fluent. We discussed the concept of plant traits in a new paragraph. The concept expressed in lines 20–28 on p.10274 has been elaborated and enhanced in order to improve clarity (see below).

Further, I think the citation to Reichstein needs some more context.

The citation has been contextualized by extending the concept in a new paragraph, as follows:
'In the recent years, research on interactions between biological systems and atmosphere, tied to water and carbon cycles, is moving from the categorization of plant functional types (PFTs) (Box, 1981; Box, 1996) toward a new concept of plant functional traits (Lavorel and Garnier, 2002; Suding et al., 2008; Violle et al., 2007; Wellstein, 2011). In fact, it was observed that classical category of PFTs, based on apparent phenological and physiognomic features that also characterize the different biomes, were of little help in the modelling of biological exchanges (Groenendijk et al., 2012; Yang et al., 2014) and that differences within PFTs were similarly large as those among PFTs. In the new category, defined by measurable properties of organisms that strongly influence their performance (McGill et al., 2006), structural and physiological key features have been considered (Kattge et al., 2011). Some characters, like vegetation height or leaves nitrogen content, have been found of special relevance for the explanation of maximal photosynthetic capacity and canopy conductance. Plant functional traits can exert effects on ecosystem properties (effect traits *sensu* Lavorel and Garnier, 2002; Suding et al. 2008) and they are expected to influence the interaction between the physical environment and the carbon sink capacity with scale dependent effects. Small-scale differences in plant traits are often larger than broader-scale differences (Reichstein et al., 2014), hence in the comparison of the different vegetation patches growing on deglaciated areas we can expect significant variability in ecosystem functioning related to plant form and function.'

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Enquist, B.J., Soudzilovskaia, N.A., Ackerly, D.D., Anand, M., Atkin, O., Bahn, M., Baker, T.R., Baldocchi, D., Bekker, R., Blanco, C.C., Blonder, B., Bond, W.J., Bradstock, R., Bunker, D.E., Casanoves, F., Cavender-Bares, J., Chambers, J.Q., Chapin, F.S., Chave, J., Coomes, D., Cornwell, W.K., Craine, J.M., Dobrin, B.H., Duarte, L., Durka, W., Elser, J., Esser, G., Estiarte, M., Fagan, W.F., Fang, J., Fernandez-Mendez, F., Fidelis, A., Finegan, B., Flores, O., Ford, H., Frank, D., Freschet, G.T., Fyllas, N.M., Gallagher, R.V., Green, W.A., Gutierrez, A.G., Hickler, T., Higgins, S.I., Hodgson, J.G., Jalili, A., Jansen, S., Joly, C.A., Kerkhoff, A.J., Kirkup, D., Kitajima, K., Kleyer, M., Klotz, S., Knops, J.M.H., Kramer, K., Kuhn, I., Kurokawa, H., Laughlin, D., Lee, T.D., Leishman, M., Lens, F., Lenz, T., Lewis, S.L., Lloyd, J., Llusia, J., Louault, F., Ma, S., Mahecha, M.D., Manning, P., Massad, T., Medlyn, B.E., Messier, J., Moles, A.T., Muller, S.C., Nadrowski, K., Naeem, S., Niinemets, U., Nollert, S., Nuske, A., Ogaya, R., Oleksyn, J., Onipchenko, V.G., Onoda, Y., Ordonez, J., Overbeck, G., Ozinga, W.A., Patino, S., Paula, S., Pausas, J.G., Penuelas, J., Phillips, O.L., Pillar, V., Poorter ,H., Poorter, L., Poschlod, P., Prinzing, A., Proulx, R., Rammig, A., Reinsch, S., Reu, B., Sack, L., Salgado-Negret, B., Sardans, J., Shiodera, S., Shipley, B., Siefert, A., Sosinski, E., Soussana, J.-F., Swaine, E., Swenson, N., Thompson, K., Thornton, P., Waldram, M., Weiher, E., White, M., White, S., Wright, S.J., Yguel, B., Zaehle, S., Zanne, A.E. and Wirth C.:TRY - a global database of plant traits, Global Change Biology, 17, 9: 2905-2935, doi: 316,10.1111/j.1365-2486.2011.02451.x, 2011.

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The manuscript is rather interesting, and I think it would make an interesting contribution, especially in the light of colonization of harsh environmental like glacial forefields. That said, I have some worries which deal with the concept of the CAM metabolism and the temporal separation of photosynthesis and CO_2 fluxes, which cascades to the conclusions (difference in light constraints between C3 and CAM plants). I have the feeling the authors are mixing fluxes and photosynthesis. In C3 metabolisms these are related, but in CAM they are temporally separated. Perhaps the authors can fix this ambiguity in their manuscript.

We wish to thank the reviewer for this observation. We agree that the peculiar metabolism of CAM species, which shows a temporal separation of CO_2 uptake from photosynthetic reactions, poses the

need for a better clarification of what we are really measuring and showing. With the chamber method used in our study, we measured directly the net ecosystem exchange (NEE), i.e. the net CO_2 exchange occurring between the soil-plant system and the atmosphere, which is the result of the CO_2 assimilated and emitted by the studied ecosystem. Our attempt was to partition NEE fluxes into these two downward (CO_2 uptake) and upward (CO_2 emission) components as shown in Figure 5 of the manuscript. If for C3 species (i.e. *Festuca halleri*) CO_2 uptake occurs essentially at the same time of the photosynthetic reaction, this is not the case for CAM species, where the CO_2 is taken up during the night when stomata are open, stored in the vacuole as Malic acid and then transported to the chloroplast during the day (when stomata are closed). In the chloroplast, the CO_2 derived from malate degradation is re-fixed by the Calvin cycle ending in starch or sucrose formation (Taiz and Zeiger, 2006).
We thus realized that if it may be correct to define "gross primary production (GPP)" the downward CO_2 uptake flux for *Festuca* plots, the same term is inaccurate for *Sempervivum* plots, dominated by a 'weak CAM' species where CO_2 uptake and primary production are not in phase. For this reason, we decided to change the term gross primary production (GPP) into gross ecosystem exchange (GEE); see for its definition and use Malone et al. (2013). In fact, our study was focused on the exchange, and not on the phases of molecular production of organic matter.
We also specified in the text that with the proposed flux partitioning method we calculated the total CO_2 uptake of the two studied plant communities, while we cannot say anything about the trend in time of re-fixation of internally released CO_2 derived from malate degradation.

Cited references:

Taiz, L. and Zeiger, E.: Plant Physiology, Sinauer Associated Inc. Sunderland, MA, USA, 2010.

Malone , S.L., Starr, G., Staudhammer, C.L. and Ryan, M.G.: Effects of simulated drought on the carbon balance of Everglades short-hydroperiod marsh, Global Change Biol., 19, 2511-2523, doi: 10.1111/gcb.12211, 2013.

Method and assumptions: The first and second paragraph describing the methodology of the NEE measurements seem a bit ambiguous. NEE was measured using a LiCor with eight chambers, equally divided over transparent and opaque chambers. A bit later it is described that the S and F plots are replicated five times, making ten plots in total. Apart from the fact that NEE dark and NEE light are measured at different times, which I can see is difficult to avoid, I also interpret that there is a temporal distribution in the measurements in NEE of the replicates within and/or between the plant types. Perhaps I misunderstood the set-up; did you use more gas analysers?

The setup of the experiments was different in the two years of the study. In both years, four opaque and four transparent chambers connected to a single gas analyser were used.
In 2012, we identified five plots characterized by the presence of *Festuca* (F1, F2, F3, F4, F5) and five characterized by the presence of *Sempervivum* (S1, S2, S3, S4, S5). The experimental set-up foresaw the full rotation of the chambers among plots. In each measurement period (three days), two clear and two dark chambers were placed on each vegetation types. Therefore, we collected data from 8 chambers for each period of measurement. After three days, the 8 chambers were moved on other collars till all the 10 plots were measured both with opaque and transparent chambers. The following figure, showing as an example the setup of the first three measurement periods, may help the understanding.

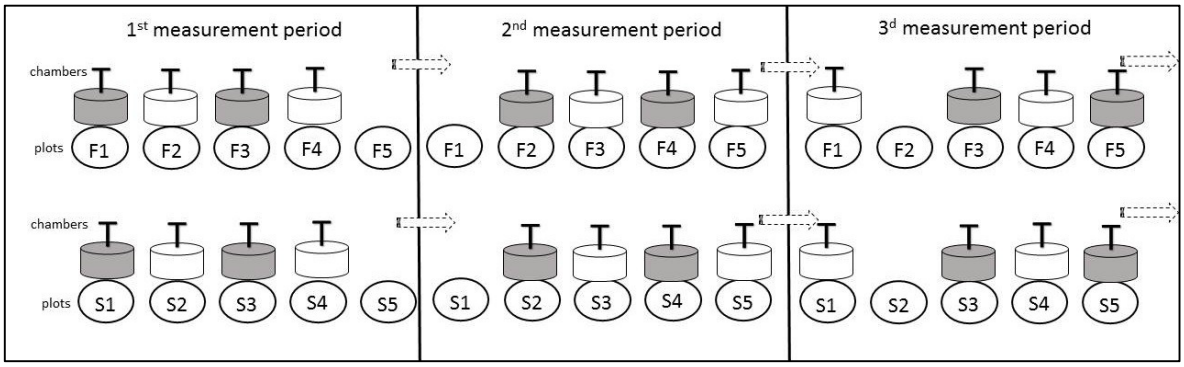

Figure 1. Example of the experimental setup in 2012. Only the set-up used during the first three measurement periods is shown. Each measurement period lasted three days.

In 2013, we analysed the same 10 plots but with a different set-up, where only some of the chambers were rotated among the plots. Two chambers, a transparent and an opaque one for each vegetation community, were kept on the same plots for the entire period (four months): one *Festuca* plot with an opaque chambers (Fo_{LT}); one *Festuca* plot with a transparent chamber (Ft_{LT}), one *Sempervivum* plot with an opaque chamber (So_{LT}), one *Sempervivum* plot with a transparent chamber (St_{LT}). Moreover, we used the other four chambers (two opaque and two transparent) connected to the same gas analyser and we called these chambers "short term".
Every week, we changed the position of the four short-term chambers on other collars by rotating one transparent and one opaque chamber per vegetation type among three different plots. The following figure, showing as an example the set-up of the first three measurement periods, may help the understanding:

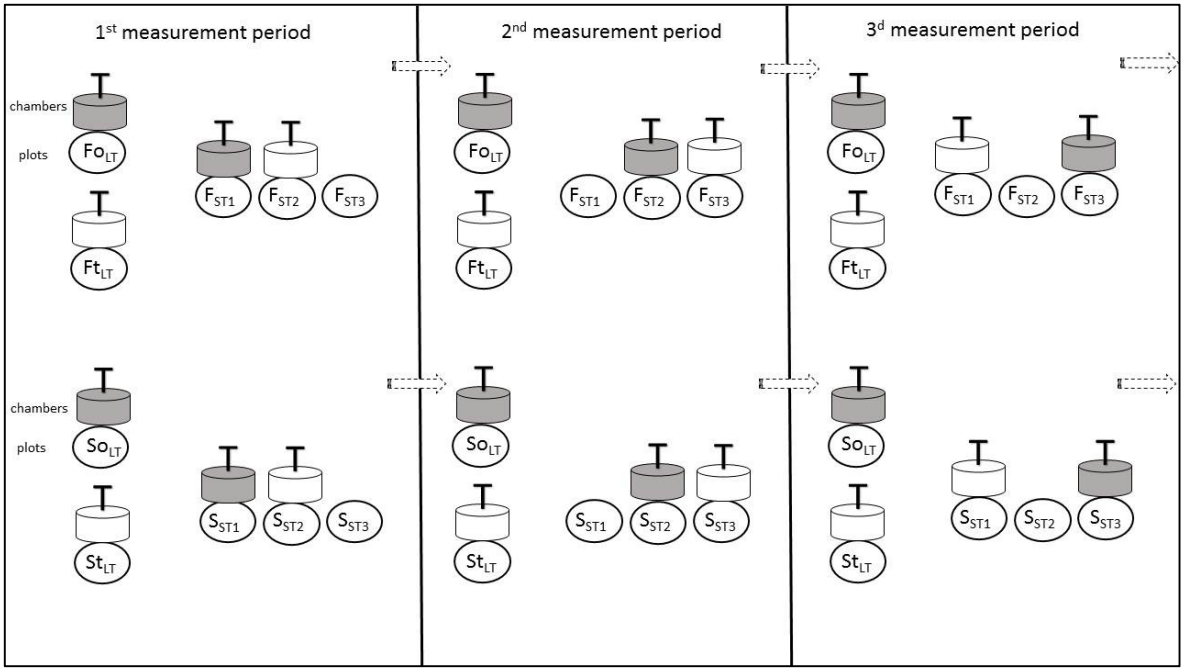

Figure 2. Example of the experimental set-up in 2013. Only the set-up used during the first three measurements periods is shown. Each measurement period lasted one week.

To gain clarity, in the method section we changed the description of the experimental set-up as follows: 10278 / 22-23 instead of: "These were called short-term plots (FtST, StST, FoST and SoST). "we wrote: "..These were called short-term plots (F_{ST1}, F_{ST2}, F_{ST3}, S_{ST1}, S_{ST2}, S_{ST3})."

In case not, who did you correct for potential changes in time (climate, PAR, etc.) between sets of measurements. More information is needed!

We thank the reviewer for giving us the opportunity to better explain our data collection and analysis. Meteorological data were taken every 10 s and averages were collected at 10 min intervals. Measurements of CO_2 fluxes were taken in sequence from 8 chambers. Each chamber measurement lasted around 2 minutes. The entire collection of data from 8 chambers lasted around 20 minutes. We therefore collected hourly two series of measurements from all the chambers.
During data elaboration, we synchronized the data from the measurements of the chambers and the meteorological variables at half hour time steps. We assumed that half hour averages of the meteorological conditions were representative of the 2 minutes period in which any single chamber measurement was taken. In addition, by comparing the fluxes, we analysed separately each plot in order to avoid confusion between temporal and spatial pattern. The statistical analysis was done for each single plot and the presented averages were performed accordingly.

On that note, I wonder how the 2012 was treated to serve to compare the daily courses of NEE is S and F plots, as stated in the first sentence of the 'data analysis' chapter.

The measurements in 2012 were carried out on five plots for each of the two vegetation communities to verify the consistency of their specific diurnal pattern as shown in Figure 1. That figure shows the measured fluxes on plot F2 and S2 (clear chambers) and S3 and F3 (dark chambers) collected from August 6th until August 9th 2012. We modified table 2 of the original manuscript to better characterize the fluxes measured in each plot. In addition to the mean cumulated daily value (g C $m^{-2}day^{-1}$), we reported four basic statistical parameters (average, standard deviation, minimum and maximum values) calculated on the raw data measured in each plot (in µmol CO_2 $m^{-2}s^{-1}$).

Table 1 in this text (corresponding to Table 2 in the manuscript). Characterization of NEE fluxes measured for each plot with dark and clear chambers in 2012. Average, standard deviation (St. Dev.), minimum (Min) and maximun (Max) values are obtained on the half hour flux data (µmol CO_2 $m^{-2}s^{-1}$). The cumulated value was obtained as the sum of the grams of C exchanged every 30 minutes averaged for the three days of measurements of each plot. Positive values of NEE indicate net C loss by the ecosystem. The day-to-day variability in the fluxes, reported in brackets, represents the average of range (max-min value) of every half-hour.

Vegetation community	Plot	NEE transparent chambers					NEE opaque chambers				
		Average (µmol CO2 $m^{-2}s^{-1}$)	St. Dev.	Min	Max	Cumulated (g C $m^{-2}day^{-1}$)	Average (µmol CO2 $m^{-2}s^{-1}$)	St. Dev.	Min	Max	Cumulated (g C $m^{-2}day^{-1}$)
Festuca	F1	-0.19	3.17	-8.23	4.03	-0.132 (0.028)	2.84	0.99	1.26	5.56	3.215 (0.006)
	F2	0.67	3.80	-8.80	4.36	-0.369 (0.015)	2.56	0.51	1.28	3.78	2.644 (0.006)
	F3	0.02	1.88	-3.41	2.49	0.044 (0.011)	2.29	0.73	1.07	4.28	2.530 (0.010)
	F4	0.61	1.71	-3.38	3.03	0.390 (0.020)	2.21	0.63	1.22	4.58	2.260 (0.012)
	F5	0.22	1.82	-3.85	2.59	0.294 (0.016)	1.94	0.50	0.94	3.33	2.000 (0.008)
Sempervivum	S1	0.67	0.69	-1.97	1.99	0.737 (0.007)	1.93	1.13	0.17	5.18	1.977 (0.010)
	S2	0.14	1.32	-3.29	1.90	0.160 (0.012)	1.56	1.19	0.05	4.51	1.873 (0.007)
	S3	0.64	0.96	-2.50	2.14	0.661 (0.010)	1.92	1.24	0.60	5.20	2.360 (0.009)
	S4	0.62	1.23	-2.93	2.50	0.648 (0.014)	2.44	1.08	1.07	5.88	2.510 (0.014)
	S5	0.66	0.75	-1.75	2.07	0.687 (0.010)	2.41	1.02	0.77	5.29	2.454 (0.020)

Flux calculations: linear regression was used to calculate fluxes. Did you check if the evolution of carbon dioxide in the chambers followed a linear pattern? See Kutzbach, L., Schneider, J., Sachs, T., Giebels, M., nen, H.N., Shurpali, N.J., Martikainen, P.J., Alm, J. & Wilmking, M. (2007) CO_2 flux determination by closed-chamber methods can be seriously biased by inappropriate application of linear regression. miniBiogeosciences, 4, 1005–1025.

This aspect was a matter of debate also within our group before submitting the paper, so we wish to thank the reviewer to give us the possibility to explain in more detail the reasons why we finally choose to keep the flux values obtained with the linear regression instead of those coming from an exponential fitting. Although not yet widely used in literature, we are aware that the exponential fitting of the CO_2 emitted/absorbed by the soil plant system during chamber measurement may perform better with respect to linear regression, as demonstrated by Kutzbach et al. (2007).
In our experiment, however, we had to face two different problems. At the one hand, it was needed to take into account the reduction of molecular diffusion taking place inside the chamber with increasing mole density values. On the other hand, in clear chambers we had to minimize the environmental alteration given by the presence of the chamber itself, with reduced wind speed, increased temperature and water vapour mole density inside the chamber. This would have biased our results considerably. This environmental perturbation was visually observed on-site, where water condensation in the internal surface of the clear chambers appeared approximately 80-100 seconds after chamber closure, which corresponded to a rapid change in the trend of the CO_2 concentration when approaching the end of the measurement (see Figure 3 after second 90). Minimization of environmental alteration was achieved by reducing the computational period, and re-computing the fluxes considering only the data collected in the time-window between 20 and 60 s after chamber closure (Figure 3). After having selected that time-window, we realized that the exponential fit was often causing a significant scatter in the calculated fluxes, especially for negative values of NEE measured with the transparent chambers (Figures 4a and 4b). A comparison between the fluxes obtained with linear and exponential fitting (shown in Figure 3) are reported in Table 2 of the current text. The Bartlett test for homogeneity of variance confirmed the existence of a significant difference in the variability of fluxes between the two regression methods, finally supporting our choice to rely on the more conservative linear regression.

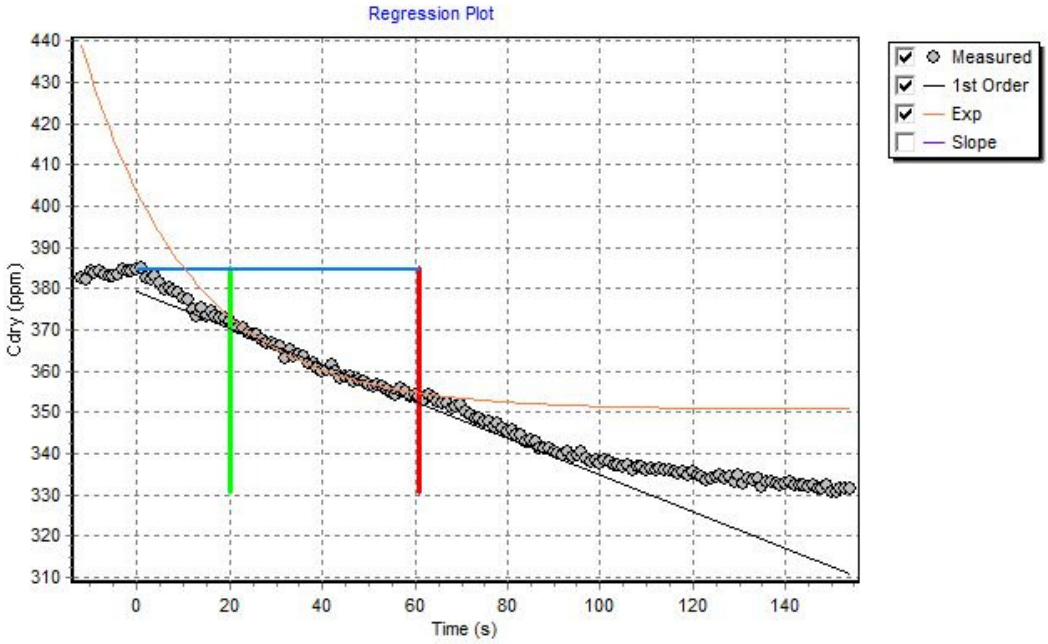

Figure 3. Example of raw CO_2 data (1 Hz) collected during one measurement with a clear chamber over a *Festuca* plot. X axis represents the time after chamber closure (s). Fluxes were recomputed considering only the data measured between 20 and 60 seconds after chamber closure (limits represented in the plot by the green and red vertical lines). In such a short time the exponential fitting produced an uncommon high value of CO_2 uptake (-9.33 µmol CO_2 m^2s^{-1}) compared with the linear fitting (-2.90 µmol CO_2 m^2s^{-1}) or compared with the exponential fitting if the regression time was enlarged to a 20-100 s time window (- 2.63 µmol CO_2 m^2s^{-1}).

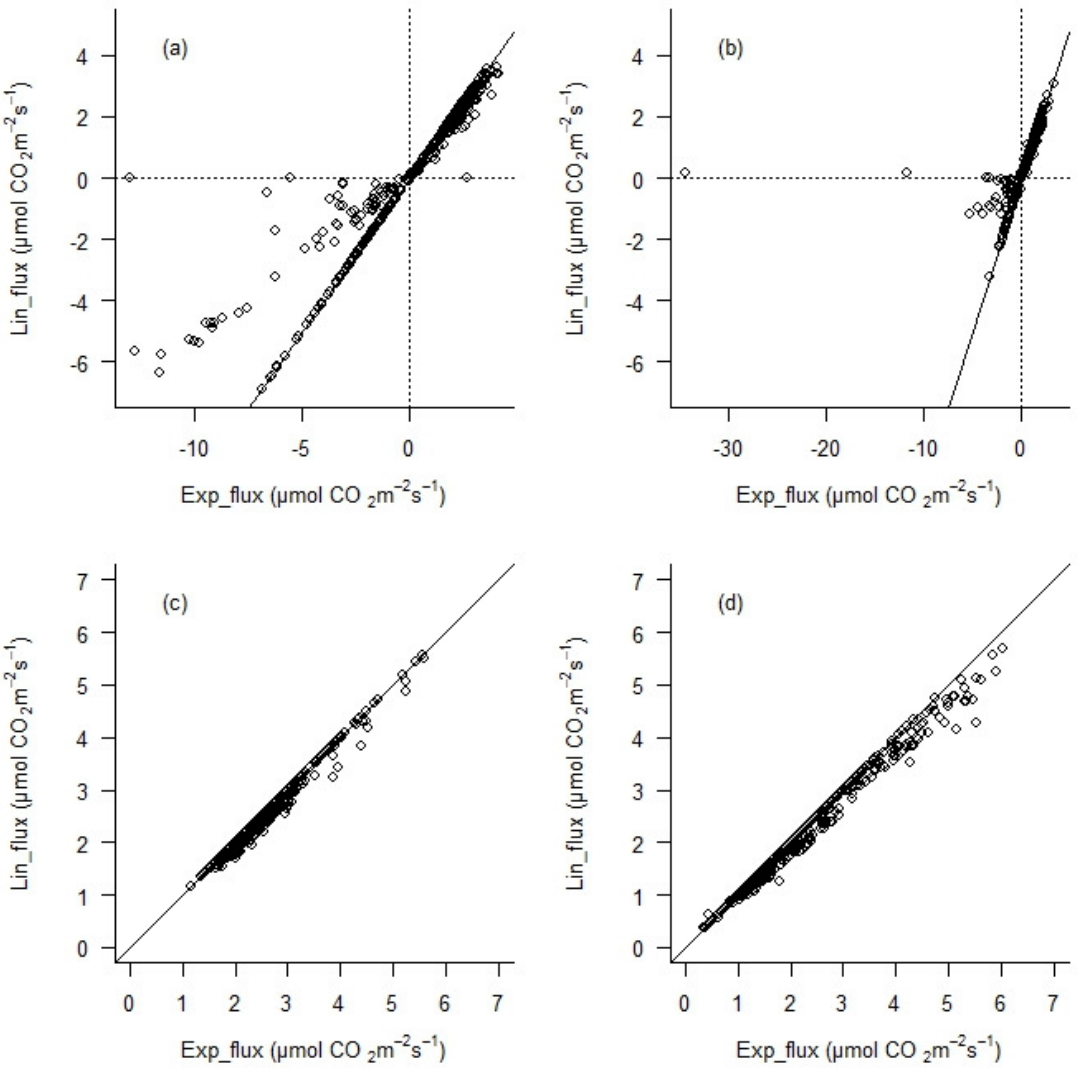

Figure 4. Scatterplots of the CO_2 fluxes obtained with an exponential or a linear regression fit. Data come from the five different plots for each vegetation communities. Ten days of data from *Festuca* plots (a and c) and *Sempervivum* plots (b and d), measured in July and August 2012 using clear (a and b) and opaque (c and d) chambers are shown. Total number of flux points per plot is 462.

Table 2. Comparison of fluxes shown in figure 4. Data are expressed in µmol CO_2 m^2s^{-1} and each sample size is 462. The null hypothesis of the Bartlett test is that variances are homogenous, which can then be accepted at 95% confidence interval when p-value > 0.05 (in our case only when fluxes are measured with opaque cambers).

Operation	Transparent chambers				Opaque chambers			
	Festuca		***Sempervivum***		***Festuca***		***Sempervivum***	
	Exp.	Lin.	Exp.	Lin.	Exp.	Lin.	Exp.	Lin.
Average	0.144	0.380	0.539	0.703	2.528	2.467	2.256	2.145
Standard deviation	2.955	2.243	2.106	0.958	0.751	0.743	1.205	1.120
Min value	-13.02	-6.88	-34.35	-3.25	1.17	1.17	0.33	0.36
Max value	4.09	3.62	3.27	3.06	5.60	5.55	6.03	5.68
Bartlett test of homogeneity of variance (p-value)	< 0.001		< 0.001		0.821		0.117	

Light response curves: I have the feeling we are facing a conceptual complication here (underpinned by the authors statement that CAM photosynthesis is performed at night [10281 / 6]. In CAM metabolism, essentially carbon dioxide fluxes and photosynthesis are temporally separated. CO2 uptake takes place in the night and is stored during as malate. During the day malate is decarboxylated after which it is subject to photosynthesis. NEE would as such also not depend that much on light, but on the vacuole storage capacity.

As suggested by the referee, we were not enough clear in presenting the light response curve chart, and we were not precise in the use of terminology. We revised the text and specified where we were referring to CO_2 exchange or photosynthesis. We presented the chart showing the light response curve of NEE measured by transparent chambers to underline the different patterns of CO_2 exchange in the two plant communities. In the *Festuca* plots, NEE measured by transparent chambers corresponded to the process of photosynthetic assimilation of C, being respiration little influenced by light. Hence, an evident correlation between CO_2 assimilation and light intensity (PPFD) was found. Differently, in the chart presenting the gas exchange of the *Sempervivum* plot, the process of CO_2 uptake was temporally separated from the light phase of photosynthesis, and part of the CO_2 fixation (but not all as in the 'full CAMs') took place at night. Therefore, we do not see any evident correlation between CO_2 assimilation and light.

Novelty of the study: Generally, I feel the questions asked are rather interesting. I wonder however how correct the statement is about the apparent absence of studies dealing with flux measurements from within-ecosystem plant communities (two studies immediately pop into my mind: Ward et al. (2015) Vegetation exerts a greater control on litter decomposition than climate warming in peatlands. Ecology, 96, 113–123. Ward et al. (2013) Warming effects on greenhouse gas fluxes in peatlands are modulated by vegetation composition. Ecology letters, 16, 1285–1293). Certainly the question regarding the differences between ecophysiological pathways, and if ecophysiological processes are subject to different environmental variables, is interesting. It however comes a bit out of the blue, and I feel in the introduction the authors should put more focus on the comparison of the C3 and CAM vegetation. How do these communities differ, and as such why is it important to know if they respond differently to environmental change?

We wish to thank the reviewer for the suggestion of these two interesting papers. We inserted the two papers as references in the introduction. Moreover, we inserted in the introduction a new paragraph where we briefly presented the main ecophysiological differences between C3 and CAM plants.
One of the main questions of our paper was about the difference in the assimilation and emission processes and final C sequestration of these two communities. With this study, we aimed to test if the two communities, characterized by different C fixation pathways, show a different C budget. For an accurate calculation of the C budget of an ecosystem, we wanted to find out the importance of considering the presence of different vegetation communities characterized by different plant traits. As

suggested by the reviewer, the importance of knowing if plant traits have an effect on the calculation of the C budget of an ecosystem was not well underlined. In the revised text, we stress this concept by giving more emphasis to the citation of Chapin et al. (2006) followed by the new paragraph explaining the importance of considering the different plant traits in the studies of C budget of ecosystems (see our second answer in this text).

10273 / 10 See Bardgett et al. (2007; Heterotrophic microbial communities use ancient carbon following glacial retreat. Biology Letters, 3, 487–490) for a mechanistic explanation on soil/substrate formation after glacial retreat.

We wish to thank the reviewer for providing also this interesting reference. In our studied environment, the two processes, biological colonization by mosses and vascular plants and microbial colonization (Ciccazzo et al., 2014) are taking place in parallel. We therefore added a sentence related to this suggestion in the first paragraph as following:
'In parallel, also a heterotrophic community establishes on barren areas (Bardgett et al., 2007), contributing to soil formation.'

Cited reference:

Ciccazzo, S., Esposito, A., Rolli, E., Zerbe, S., Daffonchio, D., and Brusetti, L.: Safe-Site Effects on Rhizosphere Bacterial Communities in a High-Altitude Alpine Environment, BioMed Research International, 06, doi: 10.1155/2014/480170, 2014.

10272 / 22–23 There are two things I think the authors need to be more careful with. First, the used reference discusses the use of Eddy covariance techniques, not how to asses carbon budgets. Second, an important part of the carbon budget, especially in these dynamic ecosystems, comes from DOC and DIC and should be taken into consideration. [this is basically addressed one sentence later].

The reviewer is correct. The definition of the net ecosystem production is not the main focus in the previously referenced paper (Baldocchi, 2003). We therefore replaced it with a reference to the already cited Chapin et al., 2006 and adding one to Luyssaert et al. (2009) where the different approaches and equations to obtain the carbon balance are considered.
We added to the text the consideration that part of the carbon, in the glacier foreland ecosystem, as it happens also in other grassland ecosystems, can be also significantly leached in the form of organic (DOC) and inorganic carbon (DIC) (Kindler et. al., 2011).

Cited references:

Kindler, R., Siemens, J., Kaiser, K., Walmsley, D.C., Bernhofer, C., Buchmann, N., Cellier, P., Eugster, W., Gleixner, G., Grunwald, T., Heim, A., Ibrom, A., Jones, S.K., Jones, M., Klumpp, K., Kutsch, W., Larsen, K.S., Lehuger, S., Loubet, B., Mckenzie, R., Moors, E., Osborne, B., Pilegaard, K., Rebmann, C., Saunders, M., Schmidt, M.W.I., Schrumpf, M., Seyfferth, J., Skiba, U., Soussana, J., Sutton, M.A., Tefs, C., Vowinckel, B., Zeeman, M.J. and Kaupenjohann, M.: Dissolved carbon leaching from soil is a crucial component of the net ecosystem carbon balance, Global Change Biology, 17, 21167-1185, doi:102,10.1111/j.1365-2486.2010.02282.x, 2011.

Luyssaert, S., Reichstein, M., Schulze, E.D., Janssens, I.A., Law, B.E., Papale, D., Dragoni, D., Goulden, M.L., Granier, A., Kutsch, W.L., Linder, S., Matteucci, G., Moors, E., Munger, J.W, Pilegaard, K., Saunders, M. and Falge, E.M.: Toward a consistency cross-check of eddy covariance flux–based and biometric estimates of ecosystem carbon balance, Global Biogeochemical Cy, 23, GB3009, doi:10.1029/2008GB003377, 2009.

10273 / 10–13 The way of referencing would suggest that the Laine paper deals with plants with different photosynthetic pathways (C3, C4, CAM). Their study has been performed in peatlands, with only C3 plants; indeed they used different plant growth forms.

The reference to Laine paper was changed as follows: '…plants belonging to different growth forms (Laine et al., 2012) and characterized by different photosynthetic pathways can differ for seasonal productivity, carbon allocation in their tissues, and therefore quality and quantity of the litter which affects heterotrophic respiration (Laganière et al., 2012).'

10276 / 23–26 I guess this is better: : : :a pioneer grassland community, dominated by: : : : : :, is present, and covers about 35% of the surface area.

The sentence was modified as suggested. Instead of: "On the moraine ridges of 1940, the total vegetation cover amounts for 35% of the soil and in the area is present a pioneer grassland community dominated by *Poa laxa* Haenke and *Gnaphalium supinum* L., *Cerastium cerastoides* (L.) Britton, and *Arenaria biflora* L." the new text version reports: 'On the moraine ridges of 1940, a pioneer grassland community, dominated by *Poa laxa* Haenke, *Gnaphalium supinum* L., *Cerastium cerastoides* (L.) Britton, and *Arenaria biflora* L., is present, and covers about 35% of the surface area.'

10279 / 11–12 what was the volume of the soil samples? It is a little strange to say that the soil samples were separated from the root; better: Course roots (> 2mm) were separated from the soil.

We precisely calculated the volume of the soil extract by each collars, by using and weighting plastic balls of known volumetric density. The volume collected varied between 1000 and 1500 mL. Afterwards, we calculated the roots content per volume unit of soil (cm^3) in order to compare the collars.
We modified the sentence as suggested.

10280 / 6 so, the time unit is dropped in your NECB calculation? Why do you not simply call it the ecosystem carbon content?

We modified the sentence as follows: instead of "NECB is expressed as kg C m^{-2}." we wrote: "The ecosystem carbon content is expressed in terms of kg C m^{-2} , which corresponds to the NECB calculated over 160 years".

10282 / 17–19 repetition of what is shown in Table 1. Should it not be noted that that the isotopic signature of the S biomass does not show full CAM metabolism? Many CAM plants can function in a C3 mode when water is available. Do you know how this is for Sempervivum? In Table 1 caption, what sense does it make to indicate what the significance code means when the numbers are presented in the table?

We removed from the text in page 10282, lines 17-19 as they were a repetition on what reported in Table 1. *Sempervivum montanum* is a CAM facultative and becomes less engaged in the CAM pathway in conditions of low temperature and no water stress (Wagner and Larcher, 1981). The isotopic signature of *Sempervivum montanum* can vary in the range of δ13 C=-18.6 in Tirol and -24.3 in the Swiss Alps, indicating either a full CAM metabolism or almost no CAM metabolism (Körner, 2003). Table 1 reported the isotopic signature found in the biomass of our *Sempervivum* samples (δ^{13} C=-21.18 ± 0.41). According to Körner (2003) this value indicates a weak CAM metabolism performed by *Sempervivum* in our study area. In the revised text version, we discussed this concept. As suggested, we also removed from the caption of Table 1 the explanation of significance codes.

10282 / 20–21 I do not understand what the authors mean with 'spatial variability was similar for all plots belonging to the same vegetation'? How was this tested? There seems to quite some variance between the plots, especially for Festuca! In Table 2, do positive NEE values indicate net loss of C (as in Fig. 2)?

We thank the referee for drawing attention to this point of the paper. Purpose of Table 2 was to present to the reader the studied plots, giving some information on the general characteristic of each one. Thanks to this comment, we realized that our goal was not achieved, and conversely we did a mistake in presenting the comparison of the cumulated values of NEE (g C m^{-2} d^{-1}) measured on each plot. By conducting the ANOVA test, we mixed up the spatial and temporal variability, an aspect that we

considered carefully both in the experimental set-up design and in general in the use of collected data. In fact, by comparing the cumulated values of NEE, we not only weighted up values collected from different plots but also values collected in different days, when variability in meteorological constraints could have affected the measurements. Therefore, we changed Table 2 accordingly to our scope: we removed the ANOVA test and we inserted other relevant parameters to describe our measured plots. The new Table 2 has been reported previously in this text. The Material and Methods section of the paper (10280 8 / 14) has been modified as follows: "The data obtained in 2012 with both transparent and opaque chambers were used to present the daily courses of NEE in *Festuca* and *Sempervivum* plots. To characterize the plots, we calculated for each sampled collars the average, standard deviation (St. Dev.), minimum (Min) and maximum (Max) values of fluxes with both transparent and opaque chambers. Values were calculated from the half hour flux data (µmol CO2 m-2s-1). The daily cumulated NEE (g C m-2d-1) was obtained as the sum of C exchanged every 30 minutes, averaged for the three days of measurements of each plot. The day-to-day variability in the fluxes is reported as the average of range (max-min value) of every half-hour." The text in the Results section (10282 20 / 22) was modified as follows: "Table 2 reports the fluxes parameters, measured with both transparent and opaque chambers in 2012, characteristic for each plots."

10283 / 14–15 vs. 22–23 How do these two sentences relate? First NEE dark fluxes show similar trend, then they are different! If you intend to say that the trends ae different between NEE dark and light measurements, to me that is a rather open door! Perhaps I miss the intention of the last paragraph.

The reviewer is right, these two paragraphs were not clear. We tried to explain better the concept we wanted to convey by modifying the text as follows:" Measurements conducted with the opaque chambers showed that *Festuca* displayed a peak of respiration during phase 3 of the day (Figure 2b). Similarly, also *Sempervivum* showed a peak of respiration in the same period of the day (Figure 2b), reaching higher values of emission in comparison to *Festuca* plots. During the night, *Sempervivum* showed lower emission levels than *Festuca* plots.
Summarizing, *Festuca* plots reached in phase 2 of the day the maximum assimilation value measured with the transparent chambers and in phase 3 the maximum CO_2 emission measured with the opaque chambers. In *Sempervivum* plots, we observed with the transparent chambers that there were two periods of high assimilation during the day: phase 2 and 4. Maximum respiration occurred, as for *Festuca*, in phase 3.'

10287 / 15–16 This is a one-sentence paragraph.

This was a typing error. We merged the sentence reported in line 10287 / 15-16 with the following paragraph.

Anonymous Referee #2

Varolo et al. tell an intriguing story of vascular plant life under harsh environmental conditions in a glacier forefield. The narrative is clear and the research objectives well justified by the bibliography. In the introduction, the authors lay out clearly the specific questions they address in their study and in the conclusion provide a clear account of the main findings. Special care, however, should be taken in the transitions between sentences and paragraphs where the fluidity of the text is often interrupted by rapidly switching arguments. Overall, despite the complex experimental design and the subject-specific terminology, the authors manage to convey a convincing story.

One aspect of the study that could be elaborated further is the comparison of carbon accumulation into the ecosystem by the contrasting vegetation types. The authors give only a brief tentative explanation as to why they find similar soil carbon stocks and isotopic signatures despite the clear differences in the magnitude and isotopic composition of photosynthates assimilation. The proposed mechanism of lateral transport (i.e. herbivory) resulting in equal carbon stocks despite the higher productivity of Festuca is sound. Nevertheless the strikingly similar isotopic signature of the soil organic matter under the two contrasting species could only imply that 13C enriched Sempervivum litter does not get incorporated into the soil (a process which should even further enrich the SOM due to fractionation)

and ends up being respired upon senescence. It was Körner in his Alpine plant life book stating that cushion and rosette growth forms run their private nutrient cycling by creating favourable microclimatic conditions for microbial turnover of organic matter. If this argument holds, perhaps the observed high Reco fluxes and low assimilation rates in Sempervivum could be attributed to an excessively high heterotrophic respiration, rather than an inefficient photosynthesis. I believe that the authors should at least reflect on such a scenario and perhaps propose tentative ways to test it employing isotopic CO2 analyses and microbiological assays.

We wish to thank the reviewer for the nice words expressed. We appreciated also the suggestion arising from the book by Körner (specifically at page 157). We added in the discussion section the suggested argumentation as following:
'… but it must be considered also the high degree of internal recycling shown by cushion plants, in terms of nutrients but possibly also of carbon, which makes this plant form almost independent from its surroundings (Körner, 2003), therefore minimizing the amount of carbon with low δ13 enrichment released into the soil'.
Concerning the suggestion about a possible alternative mechanism explaining the different patterns of carbon accumulation into the soil, we should consider that, based on measured net ecosystem exchange, it doesn't appear that respiration alone can explain the apparent inconsistence between observed NEP and carbon accumulation. As shown also in Figure 4, the observed carbon balance was always negative or close to zero during all the vegetation period, so possible inaccuracies in the proposed flux partitioning method wouldn't have changed the observed carbon balance. Nevertheless, as suggested by the reviewer, the isotopic signature analysis could help to shed light in the process. Therefore, in the discussion section, we added the following indication:
'To confirm the findings obtained through this partitioning methodology, a straightforward method could be the isotopic discrimination of the respired carbon dioxide, and possibly the Bayesian modeling as described in Ogle and Pendall (2015).'
Concerning the possible use of microbiological essays, we believe that they could be interesting as well, although not necessarily conclusive in quantitative terms. We know from previous studies that microbial population structure is frequently driven by heterotrophs composition (Berg & Smalla, 2009), but there are also contrasting findings in the linkage between plant species diversity and microbial diversity (Schlatter et al., 2015), which suggest care in the interpretation of rhizosphere-microbes interaction. In particular, although they could be not decisive in the assessment of ecosystem carbon balance partitioning, a large amount of information can be gained from microbiological studies, as the works from Ward et al. have showed (Ward et al., 2013, 2015). We therefore added in the text the suggestion of complementing gas exchange measurements with microbiological studies.

Cited references:

Berg, G. and Smalla, K.: Plant species and soil type cooperatively shape the structure and function of microbial communities in the rhizosphere, FEMS Microbiol Ecol, 68, 1–13, doi: 10.1111/j.1574-6941.2009.00654.x, 2009.

Schlatter, D.C., Bakker, M.G., Bradeen, J.M. and Kinkel, L.L.: Plant community richness and microbial interactions structure bacterial communities in soil, Ecology, 96 1, 134-142, 2015.

Anonymous Referee #3

The manuscript presents a very interesting study about the different physiology of two plant communities living in the same environment. The novelty of the study concerns both the species and the remote investigated ecosystem, that is a glacier forefield. The authors found an interesting within-ecosystem variability in CO2 fluxes related to the C uptake capacity and the specific metabolism of the two species. I overall think these results could provide an interesting contribute to the CO2 fluxes and the alpine ecology communities Nevertheless, I think that the manuscript still need some work to improve the clarity of the main message, the grammar and the text readability. The main scope, i.e.

the comparison of the two metabolisms is confused by too many details in both the introduction and materials and methods sections. In details, the introduction section lacks clarity, the reader can catch the story but the sentences and paragraphs are not well merged. The complex metabolism of the CAM species should be better introduced, to help the later reading of the results.

We thanks the referee for the good comments on the paper and for the suggestion on the introduction part. The need of improving the introduction, especially in the clarity of the message conveyed and in the connection of the different part, is very similar to what was suggested also by the 1st referee. Therefore, we revised the sentences structure and inserted a text explaining the different CO_2 fixation pathways of C3 and CAM species to better convey the message of the study.

Specific points
I think there is a bit of confusion about the role of source/sink strength of the two ecosystems: the role of sink or source of an ecosystem should be referred to a defined timespan. Here, NEE is measured only during the vegetative period, specifically four months. Lacking the off-season measurements the authors should be more specific about the terms sink and source and better discuss that it refers to the four months period, since for example on a annual basis also the grasses, which is a very weak sinkon the summer period, could act as a source. For example at 10272/l.20 "the grassland acted mainly as a carbon sink with a total cumulated value of -46.4 _ 35.5 gCm-2" on which time span? The authors should specify that this value refers to a 4 months period otherwise one can think that this is an yearly cumulative.

We certainly agree. We inserted the indication that the total cumulated values refer to a 4 months period in the text.

The abstract should be shortened, I suggest to introduce less details, the reading is not fluent and the last sentence (10273/l.1-4) could be removed.

We agree that the abstract was quite long. In addition to the last sentence, we removed also the half line sentence, possibly redundant, indicating the amount of carbon present in the two vegetation communities and in the soil. We believe that now the abstract is short enough and more fluent.

In the Methods section, even if simple in principle, the flux partitioning was not described.

In the Data analysis section of the original manuscript (10281/l6-24) we described the procedure used to assess ecosystem respiration (R_{eco}, Eq. (2)). We completed the flux partitioning description by adding a new equation showing how gross ecosystem exchange (GEE) is determined (Eq. (4)):

$$GEE = NEE - R_{eco}$$

Where NEE represents the net ecosystem exchange measured with transparent chambers.

Moreover, I think that the use of a negative GPP in the figure 5 is unusual and confusing, since the authors then used a positive GPP cumulative (page 10286). I suggest to use a positive GPP also for figures

We agree that a negative GPP can be confusing, although largely used (see Asaf et al., 2013). Instead of using positive GPP, however, also based on the comment of #Ref. 3, we preferred to use the term gross ecosystem exchange (GEE), which represents the gross amount of CO_2 entering the considered portion of ecosystem, and not necessarily the gross production. In fact, while these two terms can be almost interchangeable in C3 and C4 plants, in CAMs dark, light photosynthesis and the CO_2 exchange can be shifted in time, hence requiring a precise terminology. Following the micrometeorological approach used for NEE, GEE has to be shown as negative.

Cited reference:

Asaf, D., Rotenberg, E., Tatarinov, F., Dicken, U., Montzka, S.A. and Yakir, D.: Ecosystem photosynthesis inferred from measurements of carbonyl sulphide flux, Nature Geoscience, 6, 186–190, doi:10.1038/ngeo1730, 2013.

In the Results section the separation of results from the 2012 and 2013 experiments is not completely clear e.g 10284/l27

The text has been revised accordingly to this comment and, where not clearly specified, we inserted the reference to the data used to perform the specific analysis. Below some examples:
10280 25/ 26: "To verify the response of the two vegetation communities to the light regime, we applied a logistic sigmoid model (Moffat et al. 2010; Eugster et al. 2010) to NEEL data measured with the transparent chambers in 2012: .."
10282 8 /9: "Instead, by analysing CO_2 exchange data of the opaque chambers collected in both 2012 and 2013,.."
10284 1: "Using the data of 2012, we analysed the light sensitivity of NEE fluxes collected by the transparent chambers. The most evident difference of NEE.."
10286 17 / 18: "The cumulated value of NEE along the four months of study in 2013 (Figure 7) in the permanent plots."

Minor edits:
-10272/l.5-"In this study, using a comparative analysis of the C fluxes of two contrasting vegetation types, we intend to evaluate if the different physiologies of the main species have an effect on Ecosystem Respiration (Reco), Gross Primary Production (GPP), annual cumulated Net Ecosystem Exchange (NEE), and long-term carbon accumulation in soil." change with -"This study, uses a comparative analysis of the C fluxes of two contrasting vegetation types, to evaluate if the different physiologies of the mainspecies have an effect on Ecosystem Respiration (Reco), Gross Primary Production
(GPP), annual cumulated Net Ecosystem Exchange (NEE), and long-term carbon accumulation in soil."

We agree on the comment and we changed the text accordingly.

-10273/l.7 -"The Alps are particularly vulnerable to climate change and it has been estimated that since 1850, glaciers in the Alps have lost half of their total extent" remove "it has been estimated"

We agree on the comment and we changed the text accordingly.

-10273/l.9-13 some repetitions, these paragraphs should be shortned and more fluent

We modified the text as follows: "Shortly after ice melt, these large areas are colonized by plants and a new ecosystem begins to develop (Marcante et al., 2009). The primary succession starts with the establishment of a scattered pioneer flora, characterized by high levels of disturbances, and then develops, in combination with the formation of soil, progressing towards different vegetation communities. These communities of older succession stages are more stable and are typically present with increasing biodiversity and biomass (Matthews 1992)."

-10273/l.19-20 many passive forms can be converted in to active ones such as: "To date there have been few studies that analysed the carbon budget in the glacier foreland and there is a large uncertainty about the role... "change with "To date few studies analysed the carbon budget in the glaciers foreland and a large uncertainty exists about the role"

We revised the text and changed several passive voices into active ones, including the sentence suggested by the reviewer. See below some other changes done:
10272 / 10-11 "We measured the NEE of two plant communities present on a Little Ice Age moraine in the Matsch glacier forefield (Alps, Italy) over two growing seasons."
10272 / 15-16 "We collected soil samples from the same site to measure long-term C accumulation in the ecosystem."
10273 / 19-20 "To date few studies analysed the carbon budget in the glaciers foreland and a large uncertainty exists about the role.."
10275 / 19-20 "We carried out a comparative analysis of the ecophysiological responses to the environmental drivers of these two contrasting plants communities."

….

-10276/l.23-27 reformulate this sentence. Plus, the description of plant communities could be more clear and concise

The sentences were modified as follows: "On the moraine ridges of 1940, a pioneer grassland community, dominated by *Poa laxa* Haenke, *Gnaphalium supinum* L., *Cerastium cerastoides* (L.) Britton, and *Arenaria biflora* L., is present, and covers about 35% of the surface area. On the LIA moraine, the vegetation covers about 70% of the soil and is composed by different vegetation communities."

-10277/l.12-13 Also global radiation and wind speed are measured at both 0.1 and 2 m above the ground or the authors need to better specify the heights of the pyranometer and anemometer?

The reviewer is correct when asking this information. In fact, while radiation is not expected to vary significantly along the vertical in a few meters distance, a strong vertical gradient near the soil surface is expected in mountains, particularly in terms of wind speed, temperature and relative humidity, see for instance Montagnani et al. (2005). Therefore, we added the information about measurement height in the text, which was both at 0.1 m and 2 m for temperature and relative humidity, 0.1 m for radiation and 2 m for wind velocity.

Cited reference:
Montagnani, L., Maresi, G., Dorigatti, C., Bertagnolli, A., Eccel, E., Zorer,R. and Bertamini, M.: Winter depression and spring recovering of photosynthetic function of five coniferous species in the treeline zone of the Southern Alps (Trentino/Alto Adige), Studi Trent. Sci. Nat., Acta Biol., 81, Suppl. 1: 227-244, 2005.

-10277/l.12 I think the authors mean "LI-8150" the multiplexer for LI-8100 not Li 8100-105. Moreover, I suggest to cite the instruments as generally the companies do, like Li 8100 -> LI-8100

We agree on the comment and we changed the text accordingly.

-10278/l.10 in the previous paragraph the plots are defined as Festuca plot and Sempevivum plot, here change accordingly: "five Sempervivum plots..."

We agree on the comment and we changed the text accordingly.

-10278/l.26 I would place this paragraph at line 7 before the detailed description ofthe measurements –

We agree on the comment and we changed the text accordingly.

10279/l.2 remove soil... in this case the CO2 efflux is from the ecosystem not only the soil.

We agree on the comment and we changed the text accordingly.

-10280/l.11-12-13 Not clear

The paragraph has been modified according also to the comments of referee 1. The sentence at line 11-12-13 was modified as following: "The daily cumulated NEE (g C $m^{-2}d^{-1}$) was obtained as the sum of the grams of C exchanged every 30 minutes averaged for the three days of measurements of each plot. The day-to-day variability in the fluxes is reported as the average of range (max-min value) of every half-hour."

-10282/l.4-10 this part is a bit redundant

We removed some details on statistical computation.

-10282/l.13-15 "To verify the CAM behaviour in S. montanum, we investigated the carbon isotopic ratio (_ 13 C) .... "remove "To verify the CAM behaviour in S. montanum" and change with "We investigated the _13C" the _13C acronym was already introduced

We agree on the comment and we changed the text accordingly.

-10282/l.16 it is not clear different among what. Refomulate: "The _13C in the aboveground biomass was significantly different between the two main species (Table 1) (P_ 0.001) highlighting their different photosynthetic pathways"

We agree on the comment and we changed the text accordingly.

-10282/l.22 "the DAILY average NEE".

We agree on the comment and we changed the text accordingly.

-10284/l.1 "The most evident difference in response to PPFD of Festuca and Sempervivum" change with "The most evident difference in the light response curve between Festuca and Sempervivum"

We agree on the comment and we changed the text accordingly.

-10284/l.2 remove "As a result"

We agree on the comment and we changed the text accordingly.

-10284/l.15 difficult to catch the meaning of this sentence: "but eventually adapted to light until the closure of the opaque chambers"??

The reviewer is right, the sentence was unclear. We modified the text as following:
'The relationship between NEE obtained with the opaque chambers and air temperature for the two vegetation communities was calculated on the basis of 2012 data. Inside the chambers, the vegetation was always in dark conditions during the measurements, but acclimated to light until the closure of the chambers during the day. Measurements revealed that both vegetation types responded to temperature, but differed during the four Osmond's phases in the *Sempervivum* plots (Figure 4).'

-10285/l.4 I would change "response to temperature" with "temperature sensitivity"

We agree on the comment and we change the text accordingly.

-10287/l15 At the beginning of the discussion please recall some details e.g. "Robust differences in CO2 fluxes between two vegetation types, a C3 (Festuca) and a CAM (Sempervivum) species"

We inserted a sentence at the beginning of the paragraph: "The present study analysed the C fluxes of two vegetation communities characterized by a different carbon fixing pathways, a C3 community (*Festuca*) and a CAM one (*Sempervivum*)."

-10290/l14 "It has been shown that high GPP values are not always coupled with high rates of C accumulation in the ecosystem." in which study?

To clarify the concept, we added in this sentence the reference to the already mentioned Fontaine et al., 2004, and also a new reference to the more recent finding about the differential effect of above- and belowground carbon inputs. In the revised text version the sentence reads as follows: 'It has been shown that high GPP values are not always coupled with high rates of C accumulation in the ecosystem, in particular if the input comes from aboveground litter (Bowden et al., 2014; Fontaine et al., 2004).'

Cited reference:

Bowden, R.D., Deem, L., Plante, A.F., Peltre, C., Nadelhoffer, K. and Lajtha, K.: Litter input controls on soil carbon in a temperate deciduous forest, Soil Sci. Soc. Am. J. 78, 66-75, 2015.