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Interactive comment on "The amount and timing of precipitation control the magnitude, seasonality and sources (^{14}C) of ecosystem respiration in a polar semi-desert, NW Greenland" *by* M. Lupascu et al.

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Reviewer 2 The article by M. Lupascu et al. considers the control of the amount and timing of precipitation on the magnitude and seasonality of C-14 sources at a semi-desert site in NW Greenland. Although the premise of small precipitation events leading to the release of C-14 is an interesting conclusion, I think the article could do with a better statistical analysis, more figures showing the driving environmental parameters, and quite a lot less clutter as the paper tends to bog down in obvious remarks. For example, the author mentions in general that snowfall and rainfall drive soil water con-

tent, which is incredibly obvious. More such general statements abound in the paper, and should be removed to reduce the amount of reading needed to get to the point. The main point of this paper is the release of old carbon during small rainfall events. Focus on that, and remove all the trivial information and speculation. I also noticed that the discussion mentions many things as 'likely' without giving more evidence than a citation. Don't confuse likely with possible! I, therefore, suggest a major revision that cuts down the amount of unnecessary information in this paper, with a stronger focus on the core message of precipitation and old carbon release.

We would like to thank the reviewer for the thorough review and feedback. In this manuscript, we try to highlight the influence of changes in soil water on ecosystem respiration. We provide one of few datasets from a long-term field experiment that allows us to examine the interactions of warming and wetting on C cycling in the Arctic. The effect of discrete rain events on the 14C content of Reco is a major and (as far as we are aware) new finding of this study. In order to demonstrate this effect, however, we had to pool all available measurements from all five treatments and three years of study. We think that this result is very interesting and deserves future study, but should not be the centerpiece of this work.

Main remarks: page 2465, you measure Reco and soil concentrations in the middle of the day, when soil temperatures are arguably the highest. How can this represent daily mean Reco? R: Reviewer 2 makes a good point. Once a month we measured Reco at 6 am, 12 pm, 6 pm and 12 am for each plot to estimate daily Reco. Our regular morning measurement has a 0.9 correlation with our diurnal, averaged Reco measurement. Thus, we can be confidant that morning Reco is representative of the "daily" Reco flux. We added a short explanation in the method section.

July 2010-bare R2 0.90 July 2011-bare R2 0.70 July 2010-veg R2 0.90 July 2011-veg R2 0.90 Aug 2010-bare R2 0.91 Aug 2011-bare R2 0.93 Aug 2010-veg R2 0.88 Aug 2011-veg R2 0.91

page 2465, line 23-25: How many replicates of these stainless steel gas wells were installed per treatment? Is there a possibility that the variability within a plot is greater, or just as great, as within plots and treatments? R: As suggested by both reviewers, we have included sample size in our revised method section. Well replicates vary from n=1-2 per treatment. Due to cryoturbation, the spatial variability of the organic C content in high arctic soil is very high and our findings would have been improved by more replications. Nevertheless, as shown in Fig. 3, [CO2] concentrations are variable between different plots, but we observed significantly higher concentrations at all depth in the irrigation treatment compared to the control. In addition, the 14C content of CO2 at a given depth shows low interannual variability. Combining multiple years of observation implies that the 14C content is significantly different between treatments.

page 2470, line 5-6: why aren't these episodic cold snaps displayed in Figure 1? Or better still, add a separate figure that shows precipitation, soil water content, active layer depth and temperature, so the environmental drivers can be better understood by the reader. R: As suggested by both reviewers, we are now showing more of our data. We added a new figure (Fig. 1) showing air and soil temperature, precipitation events and soil moisture. In addition, we added Supplement Fig. 4. showing daily Reco for bare (a,b,c) and vegetated (d,e,f) areas for all treatments and sampling years along with air temperature. This figure allows the reader to see the impact of cold snaps on Reco. As explained to Reviewer 1 above (point 6.), we do not have continuous estimates of active layer depth.

page 2470, line 11: why is there an error estimate of _0 percent? This does not make sense and can't be true. Were there no replicates made? How significant is your comparison between treatments then? R: The reviewer raises a valid point. We have amended the number of digits shown. In this section, we compared the mean and standard deviation of Reco fluxes in all treatments (n=2 per treatment) over the three-year period (2010-2012). The increase in Reco due to warming $\times$ irrigation was of $\sim$44, 46 and 48% for 2010, 2011 and 2012, respectively. The associated SD is $\pm$2%

page 2470, line 12-13: When looking at table 3, I don't see significant differences between the different treatments. It's only obvious that they're all higher than the control, but otherwise there's too much variance. The percentage values being mentioned here do not correspond to the variation I see in the table. R: We would like to thank the review for catching this! We have revised the paragraph to reflect that all treatments increase cumulative summertime Reco relative to control, but there is no difference between the irrigation treatments and the 2°C warming treatment; the 4°C treatment shows the smallest effect. In our initial analysis, we calculated the percentage increase under treatment conditions compared to the control for each year and then we averaged the results for the three years. However, we failed to correctly propagate the uncertainty.

page 2474, line 22-24: isn't this just because the active layer is shallow, and profiles deeper in the ground are still frozen? The reader can't check this, because no active layer measurements are shown! Please provide them! R: As explained above (Reviewer 1, point 6), we do not have direct measurements of active layer depth for each plot. We have added the approximate DOY of the snowmelt to the discussion section, so aid the reader in understanding Fig. 3. during snowmelt soils thaw. We were able to progressively sample more and more wells over the course of a few days to about two weeks. In 2011 and 2012, we observed a peak in [CO2] that coincides with the snowmelt period and the peak also at greater depth with a time delay. (in 2010 we started measurements after snowmelt). In 2011, we were able to extract CO2 from some wells before the snowmelt, which were lower than during the snowmelt (DOY 168) (Fig. 3). Thus, we conclude that during the snowmelt, soils conditions rapidly improve to favor microbial decomposition of in situ C and possibly DOC to produce the highest [CO2] recorded during the entire summer.

page 2477, line 21-24: this means that you can't say that these water pulses are a control on old carbon release. What is described here, is that C-14 is released by these water pulses, but this may just be C-14 that was already built up. There is no proof that these water pulses increase e.g. the respiration of old carbon from the soil. Perhaps it's just a modulating effect. This should be much more stressed in this article. R: We fully agree with the reviewer that the increase in [CO2] after rain events can be attributed to increased in situ CO2 production or to a reduction in pore space. We discuss this in the manuscript: "Two processes may explain this observation…" We have added a Supplementary Figure 5 showing that pore space [CO2] in the deeper soil near the permafrost table increases in response to irrigation over a 24 hour period.

page 2478, line 3-5: closing a chamber for 24 hours is incredibly long! What is the effect of this on the C-14 that's being measured? Aren't you introducing artificial heating, which leads to more respiration, possible with a different C-14 signature? R: We agree with the reviewer that chambers affect the soil CO2 profile and possible warm the soil. However, this problem cannot be quantified. In order to accurately measure the 14C content of CO2 with accelerator mass spectrometry we have to collect $\sim$ 0.5-1 mg C. In addition, we need to build chamber [CO2] to above atmospheric [CO2] (otherwise we just measure the 14C content of atmospheric CO2). Scrubbing the chamber to remove atmospheric CO2 from the headspace imposes other changes to the CO2 profile, which can also not be quantified, and is impractical due to the high gravel and macropore content.

page 2478, line 13-14: likely because of water pooling on the permafrost table? Was the water table measured? Don't guess! Show! Otherwise, it's 'possible'. Not 'likely'. R: We have added an explanation to this paragraph. We directly observed the water table via our wells, which are open ended. After heavy rainy events, wells were full of water (normally at 90, 60 cm) not allowing us to measure CO2 for up to 5 days. If the wells are saturated in water, it is impossible to pull out more air than the air that is in the well tube. If the soil is partially filled with water, we will collect both soil air and water.

page 2479, line 1: this study does not show anything about the stability of old carbon! It just shows that old carbon is released in different quantities following rainfall. That's a variation, but no proof is presented that this leads to more release of old C. Additional rainfall events may release lower amounts of CO2, but the magnitude of Reco fluxes was not measured together with the C-14 measurements, so this is unknown. R: We clearly state that more, continuous measurements of emissions are needed to accurately calculate the loss of old C from this ecosystem. The goal of our work here was to demonstrate a mechanism of old C loss that has not been discussed as such before. We think it is very important to bring to the attention of the community that we might be missing a lot of old C emissions due to our sampling scheme and need to develop more continuous samplers or higher frequency, mobile analyzers, In related work discussed in the paper we show a mass balance that indicates that the 4°C warming and 4°C×W treatments are sources of old carbon, but the amounts release from the 4°C×W treatments are lower. Lupascu, M., Welker, J. M., Seibt, U., Maseyk, K., Xu, X., and Czimczik, C. I.: Arctic wetting reduces permafrost carbon feedbacks to climate warming, Nature Clim. Change, 4, 51–55, 2014.

page 2491, table 3: does the n.a mean that there were no replicates being done that summer? How can you compare in between treatments if the variation within a treatment isn't known? R: Unfortunately, we were not able to measure all the plots like in 2010 and 2011, with the exception of the water plot, for different reasons. However, all treatments in 2010, 2011 and the W treatment in 2012 have replicates showing the same variability and treatment effect on Reco. As one of the ANOVA analysis looks at the treatment effect-only (thus with all the years in the same data set) we feel scientifically confortable to state our treatment effect results (based on our 960 observations, for vegetated only).

page 2494, figure 2, middle panel: at some depths measurements are missing for some treatments. Nonetheless, the concentrations at depth are connected by a straight line, making them seem comparable to other depths, while they are not due to the missing data! R: The lines in fig.2 have been removed where applicable.

Some additional remarks: page 2461, line 3: ice-free Arctic: do you mean the high Arctic? And I assume just the terrestrial part? This is not clear. R: The word High

Arctic has been removed

page 2461, line 20: please cite the new IPCC report. R: amended

page 2463, you say the site is located near Thule Air Force Base. This could mean that a lot of disturbance has taken place in the vicinity. How certain are you that the site is undisturbed? If not, how does that affect your measurements? R: The site is 20 minutes drive from the base. It was chosen by J. Welker to be in an undisturbed area, but within the vicinity of a power line and dirt road. Only researchers reach that zone with no disturbance from other vehicles.

page 2464, line 4: why denionized water? Thule is a coastal site, and the precipitation is not going to resemble deionized water. Besides, nutrients such as DOC are not necessarily removed through simple deionization. This could still lead to a fertilization effect. R: Water used in the military base is collected from precipitation and snowmelt. Thus, our deionized water used in the irrigation experiment is very close to the water that plants would use in natural conditions.

page 2464, line 23-24: saying that a trend is significant when it exceeds 1 standard deviation is not statistics. One large outlier in your data could falsely represent a trend. Please use pearson-r correlations with associated p-values to indicate the significance of the trend. R: In the revised manuscript all tables include the errors associated with the trends and we modified the text and the table descriptions to clarify that all errors correspond to 1-sigma, or 68% confidence level. To obtain an error corresponding to 2-sigma, i.e. 95% confidence level, the reader should multiply by 2 the 1-sigma error. We also specified that we used a Bayesian statistical approach in our error analysis.

page 2465, line 22: I assume the data from the arrhenius relationship is only used to estimate seasonal budgets, and are not shown in Figure 1? R: We explain how Reco was calculated in the method section. We use the Arrhenius relationship following Lloyd & Taylor 1994.

page 2465, line 27: these wells remained in the ground. Were they not damaged or disformed due to freeze-thaw actions? R: The wells are made from stainless steel specifically chosen to withstand the site conditions.

page 2468, line 6-7: fair could be explained a lot better. Currently it's not clear what's meant by it. R: The sentence has been amended.

page 2468, line 13-14: why do you only look at trends including the last ten years? Why not a trend per decade? Or even better: plot the data in a graph, so the reader can actually see that there is a trend. At the moment, these trends can be completely dominated by a couple of years of high precipitation in recent years, which is not necessarily a guarantee that this is something ongoing and consistent. R: We included a figure for precipitation and one for July air temperature in the supplemental material. Regarding the choice of looking at the trend for overlapping periods, this approach is used by the IPCC AR4 to compare temperature trends over the past 25, 50 and 100 years and to show that global temperature trends have been increasing (Figure TS.6 http://www.ipcc.ch/publications_and_data/ar4/wg1/en/tssts-3-1-1.html).

page 2469, line 8-11: this is incredibly obvious! of course soil water content is driven by precipitation patterns and irrigation. Why mention this at all? One sentence saying that your irrigation didn't influence irrigation patterns is all that's of relevance in this paragraph. R: Although it might sounds obvious that precipitation patterns, irrigation and snowfall affect SWC, different studies did not show the exact same thing so we think it is fine to make a point here. As an example, in vegetated areas we found that greater winter snowfall resulted in higher summer SWC throughout the growing season. While some studies have reported similar results to ours [Chimner and Welker, 2005; Morgner et al., 2010; Rogers et al., 2011], others did not find any increase in SWC over the summer [Buckeridge et al., 2010]. This is not surprising as soil (porosity and water holding capacity) and vegetation type can play a crucial role in affecting SWC [De Michele et al., 2008]. Furthermore, depending on the photosynthesis rates, plants can use more or less water [Sullivan and Welker, 2007] thus affecting SWC.

page 2470, line 6-7: it's not surprising that SWC is negatively correlated with Reco. A higher soil water content reduces the amount of pore space filled with air, thus less oxic environments, and less respiration can occur. R: The word surprising has been removed.

page 2471, line 15: again, this is obvious. Of course vegetated areas will have younger C, as plants put fresh carbon into the soil. R: We feel this fact is not necessarily obvious to every member of this journal's audience. Therefore we have elected to keep it.

page 2473, line 10: not just irrigation, but warming itself also raises Reco. Actually, all treatments raise Reco by approximately the same level! Why is that? R: Our data shows that the system is both temperature and moisture limited. The interactions are most clearly seen in the 4°C warming treatment, which shows the smallest increase in Reco relative to control. The stronger warming treatment led to drought stress. This was alleviated in the 4C°×W treatment. Unfortunately, we are unable to further detangle which respiration source (plant or microbes) are responsible for the increase in Reco in the treatments.

page 2473, line 16: again, it would be good to show more environmental parameters, so the reader can see for themselves that temperatures decrease following rainfall. R: Environmental parameters are now shown in new figure 1.

page 2473, line 19: does your soil water content show that it was drought stress? Don't guess, show it! R: The paragraph has been rephrased

page 2474, line 1-6: this is again very obvious, as it has long been known that respiration is driven by temperature. This is general knowledge. R: This study looks at the effect of temperature, water and the combination of the two on ecosystem respiration and sources of carbon. Even if it might sound obvious that Reco correlates with temperature, we feel that is important to show that our data support previous work from other High Arctic sites.

page 2475, line 1-2: again, isn't this just to do with the development of an active layer? The soil has to thaw first before processes can start. R: See previous comment page 2474, line 22-24

page 2475, line 5: why would CO2 production be stimulated by small precipitation events? R: Carbon dioxide production could be stimulated by precipitation events, because these ecosystems (polar semi-deserts) are water limited. We discuss that it has been shown that microbial respiration in (semi-)arid systems is highly sensitive to changes in soil moisture.

page 2476, line 18: is this likely or are you guessing? R: We discuss that old radiocarbon values during the snowmelt are produced during the wintertime and thus released during the permafrost thawing as already shown in Schimel et al. 2006. In situ production is unlikely, because the soil temperatures are around freezing. However, more data is needed to understand C cycling during the shoulder seasons and wintertime.

page 2478, line 23-26: so in other words, the contribution of old C is low. R: Our results showed for the first time that old C release concurred with episodic small rain events. The cited paragraph actually infers that old C pulse releases might occur more often than what we have recorded. Due to our monthly sampling scheme, we likely missed a lot of the old C release events. To fully quantify this effect, we need to develop a more continuous sampler or mobile 14C analyzer.

page 2491, table 2: please show the temperature and precipitation also in a graph, so the trend can be visualised. R: Amended

page 2491, figure 1: like said before, it would help to have some environmental parameters plotted in the same graph to show the temporal variation of those in relation to the numbers presented here some spelling improvements: R: Amended

page 2460, line 16: 'extend' should be 'extent' R: Amended

page 2462, line 19: 'microorganism' should be 'microorganisms' R: Amended

2464, line 4: ’through the end’ should be ’through to the end’ R: Amended

page 2465, line 26: please change ”allowed and remained’ into ’allowed for it and these remained’ R: Amended

page 2466, line 14: ’that in’ should be ’that of’ R: Amended

page 2467, line 22: ’graphite is measured with accelerator’ should be ’graphite was measured with the use of accelerator’ R: Amended

page 2470, line 9-10: ‘relative to control’ should be ‘relative to the control’ R: Amended

page 2476, line 23: ‘periods is not’ should be ‘periods are not’ R: Amended

page 2477, line 4: ‘described described’ should be ‘described’ R: Amended

References

Buckeridge, K. M., and P. Grogan (2010), Deepened snow increases late thaw biogeochemical pulses in mesic low arctic tundra, Biogeochemistry, 101, 105-121, doi: 10.1007/s10533-010-9426-5 Chimner, R. A., and J. M. Welker (2005), Ecosystem respiration responses to experimental manipulations of winter and summer precipitation in a Mixedgrass Prairie, WY, USA, Biogeochemistry, 73(1), 257–270, doi:10.1007/s10533-004-1989-6. De Michele, C., R. Vezzoli, H. Pavlopoulos, and R. J. Scholes (2008), A minimal model of soil water–vegetation interactions forced by stochastic rainfall in water-limited ecosystems, Ecol. Model., 212(3-4), 397–407, doi:10.1016/j.ecolmodel.2007.10.035. Morgner, E., B. Elberling, D. Strebel, and E. J. Cooper (2010), The importance of winter in annual ecosystem respiration in the High Arctic: effects of snow depth in two vegetation types, Polar Res., 29(1), 58–74, doi:10.1111/j.1751-8369.2010.00151.x. Rogers, M. C., P. F. Sullivan, and J. M. Welker (2011), Evidence of nonlinearity in the response of net ecosystem CO2 exchange to increasing levels of winter snow depth in the High Arctic of Northwest Greenland, Arct. Antarct. Alp. Res., 43(1), 95–106, doi:10.1657/1938-4246-43.1.95. Sullivan, P. F., and J. M. Welker (2007), Variation in leaf physiology of Salix arctica within and across

ecosystems in the High Arctic: Test of a dual isotope conceptual model. Oecologia. 151: 372-386.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C2282/2014/bgd-11-C2282-2014-supplement.pdf

Interactive comment on Biogeosciences Discuss., 11, 2457, 2014.