We thank the three reviewers for their careful and thoughtful reading of our manuscript. These comments have prompted us to clarify our thinking in several places, to correct errors, and to provide important but previously absent detail. The manuscript has improved as a result.

Our specific responses to the comments follow.

Reviewer #1 Specific Comments:

I suggest being more specific in the title and use the term tallgrass prairie instead of grassland

Title is modified as suggested.

p. 6869: What were the size (diameter) and insertion depth of the PVC collars?

The text is revised to indicate that the collars were 8 cm diameter, 1.7 cm height, inserted to approximately 1.2 cm.

p.6868: At what temperature was ACO2 measured? Were measurements made at a reference temperature for all treatments, or did the temperature vary among treatments? Could you find an indication for acclimation to warming? If ACO2 at one reference temperature was similar in heated and unheated plots, no acclimation occurred, whereas lower ACO2 in heated versus unheated plots (measured at one reference temperature) indicates thermal acclimation of ACO2. If thermal acclimation occurred, this would explain (in part) why the grasses revealed no warming response for ACO2.

ACO2 measurements were conducted throughout the growing season; as a result leaf temperature varied widely from 25 – 45 °C, with a mean of 36 °C. The measurements were not made at a set reference temperature. On average, the warming treatment did not significantly increase leaf temperature during either 2005 and 2006, although warming did increase leaf temps on three individual sample dates (out of eight during 2005-2006; Nippert et al. 2009 Acta Oecologia 35:400). The objective of these measurements was to document seasonal patterns and treatment responses in ACO2 rather than to study underlying mechanisms for seasonal and treatment responses, and we felt that this objective was best met by allowing things like leaf temp to vary, as they do naturally through the season. Therefore our data are not especially well suited for evaluating a possible acclimation effect. But we certainly agree with the reviewers point that photosynthetic acclimation could possibly explain a 'no-difference' response of ACO2 to warming. This question requires more space than can be accommodated in the present study, which aimed to document broad pattern rather than mechanistic detail. It would be very interesting material for a subsequent study.

p.6879, l. 6: authors write 'The result of increased water limitation may be a grassland that is more sensitive to interannual climate variation (Huxman et al., 2004).' Did you find any indications for this in your study? I think the significant year*pattern interaction effect on e.g. ACO2 may indeed confirm Huxman et al (2005) if this interaction effect reflects a larger effect on ACO2 to interannual variability for the altered versus ambient rainfall pattern.

This is a very important question, and one that we plan to address in future papers. We indeed expect the sensitivity of various ecosystem processes to increased rainfall variability to change with more or less annual rainfall. The significant year x pattern interactions certainly point in this direction.

p.6880, l. 9-19: How does acclimation of soil respiration to warming - a commonly observed phenomenon (see e.g., Luo et al., 2001; Hartley et al., 2007; Bradford et al., 2008) - fit into this story? The positive warming effect on soil respiration during winter seems to indicate no or only partial thermal acclimation. I realize that this may be too detailed to include into this paper, but maybe it's worth a sentence.

As with ACO2, this question deserves a more rigorous analysis and discussion than we can include here. But we agree with the reviewer's comment that if there was acclimation at all it may only be partial. We'd like to defer mentioning this in the present manuscript because the data analysis presented here really doesn't make it easy to say anything meaningful about acclimation.

p.6880-6881: The decrease in forb ANPP to warming is striking, in particular it can certainly not be related to soil moisture (which did was not altered by the warming and to which forb ANPP did not respond in the other treatments). Authors suggest that the potentially stronger warming effect in spring, when forbs are growing stronger, as compared to summer may be responsible for this. I have some doubts about this explanation. Were optimum temperatures actually exceeded to such degree that warming could be expected to decrease forb ANPP? Can a shift in root:shoot ratio in response to warming have induced the decrease in forb ANPP? And what about competition for resources with the C4 grasses? I think it's impossible to go into this with the current data set (which lacks any measurements of the roots and also ACO2 was not determined for forbs in the warming experiment), so I suggest not to speculate on this and admit that the question cannot be answered at this moment.

These are all important questions which indeed we're unable to answer presently. We have revised the relevant sentences in this section to suggest possible mechanisms by which the forb decrease may have occurred, but do not specify any one of them as the main underlying causal mechanism.

Reviewer #2 Specific Comments:

The study links biotic variables to three abiotic variables: the mean soil water content, its variability (through the coefficient of variation) and the temperature 5 cm in the soil. Although I realise that most roots in a grassland system are found in the upper 15 cm, I do wonder whether measuring the water content in deeper layers would not provide important information to better attribute changes in soil moisture to plant or ecosystem performance (cf. p 6879 li 1-8). Indeed, plants often have one or a few deeper roots that serve as lifelines when water in the top soil becomes limiting. I suggest that the authors address this briefly in the manuscript.

The reviewer raises an important concern. Deep soil moisture would be expected to increase in the altered rainfall treatment, with potentially important implications for species with deep roots to access this water. Unfortunately, in our experiment we do not presently have adequately calibrated deep soil moisture data collected with the necessary frequency to give reliable estimates of deep soil moisture means and variability. Moreover, studies at this site suggest that the functional importance of deep roots (> 2m) for the C4 grasses is marginal. The stable isotope signature of water in the grasses shows that they only use surface soil water regardless of landscape location, soil water availability, or time of year. (Nippert and Knapp 2007 Oecologia 153:261, Nippert and Knapp 2007 Oikos 116:1017). Additionally, a paper in review suggests that the hydraulic conductivity of deep root xylem (> 1m) is sufficiently low, and the total length and biomass of deep roots is very low, excluding the possibility that

deep water sources make a significant contribution to plant water balance for C4 grasses in these prairies. We originally touched on this point in p6878 l 2-4, and address this matter in more detail there.

I find the use of soil temperature to connect this to parameters such as canopy greenness, aboveground productivity and leaf photosynthesis somewhat problematic, as these are all affected much more by air than by temperatures. I would argue that only the soil CO2 efflux is governed mainly by soil temperature. Were air or canopy temperatures measured? Please provide these, and at least explain why you opted to use soil temperatures for correlation with aboveground parameters rather than air or canopy temperatures.

The reviewer raises a very valid point. Air temperature was measured in the center of each of four of the rainout shelters, (two ambient rainfall timing, two altered rainfall timing), with the goal of evaluating rainfall treatment effects on air temperature, but not the warming effect. Reliable canopy temperature data would be desirable, and we deployed infrared thermometers in warmed and unwarmed subplots in the same four rainout shelters where air temperature was measured. However, because of issues with sensor calibrations, technical problems causing gaps in the data, and inconsistences caused by varying wind-speed and canopy structure (leaf angle and density, etc), we found the canopy temperature data to be unreliable. The soil temperatures, in contrast, were complete and stable. Therefore in our judgment soil temperature gave a more useful measure of the impacts of the heating treatment, and they are the highest quality temperature data we had available for quantifying the effects of the warming treatment.

2. p6861 li 26-28: mesic and semiarid grasslands seem to differ in their responses to rainfall variability, but in the next lines, only responses from tallgrass prairie seem mentioned.

The paragraph referred to here is indeed focused on studies from tallgrass prairie, mostly our previous work. We changed the topic sentence for this paragraph to state clearly a tallgrass prairie focus.

3. p6867 li 2: how precisely was the percentage of green cover quantified from the images?

We added the following detail on the estimation of green cover using the First Growth camera: Percent green cover is estimated by a proprietary algorithm in the camera processor, resulting in an estimate of the ratio of green pixels to total pixels in the image. The camera was calibrated with a white card prior to each sampling, to control for variation among sample dates in illumination.

4. p6867 li 21: how many profiles were omitted?

Only 7 out of 300 profiles (~2%) were omitted using these criteria, now indicated in the text.

5. p6868 li 22: how many omitted, how many were left?

We omitted 244 out of almost 3500 observations (~7%), now indicated in the text.

6. p6869 li 11: more detail is needed on how exactly the measurements were made (e.g. duration) and the dimensions of the PVC collars

The PVC collars were 8 cm diameter and 1.7 cm tall. Each measurement was based on the increase in chamber CO_2 concentration over 1 – 3 minutes

7. p6871 li 7: 600 mm of rainfall was the maximum, but on p 6864 (li 15) it is mentioned

that rainfall during the growing season averages 635 mm. Please explain.

We reduced all experimental rainfall applications in the rainout shelters by 10%, to offset an average increase in soil moisture of ~10% in the rainout shelters compared to adjacent unsheltered plots (presumably as a result of reduced ET under the shelters). This did not affect comparisons of rainfall treatments among sheltered plots, since both ambient and altered timing treatments were treated the same way. In addition, small variations from year to year in the duration of experimental treatments and variability in the ambient seasonal distribution of rainfall account for variation in the actual treatment applications from the ambient average rainfall.

8. p6871 li 22: the 1.9% and 1.8% reductions, what was the baseline/average?

These reductions were in warmed subplots compared to unwarmed. This is clarified in the text.

9. p6878 li 19-20: what does this imply?

It implies that the mean and variability are related to each other. The next sentence goes on to contrast the present finding with previous ones. We have added a phrase to highlight that these two sentence are intended to draw a contrast between the two results.

10. p6879 li 23-25: this is fairly irrelevant as the reader does not know whether the two experiments were similar (soil type, infrared lamps used, power output, etc.)

Like our study, the cited study here was also conducted in tallgrass prairie. Moreover, our warming treatments were modeled after those used in this study, using identical infrared lamps. Wan et al. used a higher power output than our study, resulting in a somewhat stronger soil temperature response. Also, their study is conducted on a sandier soil than that of the Ramps. However even with these differences, this study is still one of the most comparable to ours, and we believe that the similarities outweigh the differences. However, the section reads better with this sentence omitted, and this study is cited later in the section in a more specific way.

11. p6882 li 1-8: I would argue that the rate of warming is also very important as is its variability (and note that your method stimulated soil warming more in some parts of the year than others)

We agree with the reviewer that the rate of warming (i.e., the rate of spring warm-up) and the temporal variability in spring temperatures could influence that rate at which this ecosystem becomes active in spring and switches to water limitation. Spring freezes are a good example of that kind of variation. Our conceptual model doesn't account for those sorts of events, and they are an interesting subject for further study.

12. Fig 2: how many different years are shown?

The data here are from 1998 to 2007, now noted in the figure legend.

13. Fig 3BCDE: I can spot only 3 treatments here on the graphs, I never see the ambient/unwarmed line

We thank the reviewer for spotting that error- all four treatments are now graphed, with a modification to clarify the closely-overlapping lines.

Reviewer #3 Specific Comments:

- Abstract: the abbreviation ANPP should be introduced with its first usage (l20)

Abbreviation added.

- P6861, l1: I would suggest being careful with this statement as marine ecosystems exchange even larger quantities of C between biosphere and atmosphere (see e.g. Sarmiento & Gruber 2006).

Rephrased to read 'account for large exchanges of carbon' rather than 'the largest'.

- P6862, l12: I suppose rather affect than 'reduce' (as increases of these fluxes in response to increased variability have been reported as well)? Overall, I have the impression that this sentences provides no valuable information to the subject and most processes in ecosystems are 'water sensitive'.

We agree, and have deleted this sentence.

- P6862, l16-19: The sentence appears confusing to me (widespread increase across biomes but increases and decreases etc. in grasslands), although I understand the point that the authors would like to make. Could the statement be improved for clarification?

We agree, and have rephrased the sentence to say 'Increased soil respiration is generally the most commonly found response to warming', and then contrast this with the finding of both increased and decreased soil respiration in grasslands.

- P6863, l6: What does the term 'tractable systems' imply here?

Added the term 'experimentally' to denote the sense in which grasslands are tractable.

- P6863, l7: 'sizeable portion' – what does that mean and could the authors provide specific numbers (see e.g. Gilmanov et al. 2010; Wang and Fang 2009)?

We have rephrased to indicate 'cover approximately 40% of the land surface'.

- P6863, l11: 'these systems' – grasslands?

Rephrased to specify 'grasslands'.

- P6863, l21: what does 'relative responsiveness' particularly refer to?

The term 'relative' here was unnecessary and has been deleted.

- P6863, l27ff: The hypotheses are formulated rather complicated and hard to understand by the reader (which makes it difficult to link the specific testing of the hypotheses later in the manuscript).

We agree the phrasing here was complicated, and we simplified the hypothesis statements by removing the phrases 'as expressed by variation in rainfall amount and mean soil moisture among years' and 'as reflected by variability in soil moisture within growing seasons,'. These associations are explained adequately later in the manuscript.

- P6864, l13: what's the annual mean temperature at the site?

The mean annual temperature at the sites is 13°C.

- P6866, l17: Confusing – TDR used and/or Tektronix cable tester (I am unaware of what it is)??

These sentences have been revised to clarify that the Tektronix cable tester was the device used to read the TDR probes.

- P6867, l20ff: What was the canopy height at all (mean and range)?

The maximum canopy height never exceeded 1m and averaged approximately 80 cm at the midseason measurements.

- P6868, l17: Measurements of CO2 fluxes?

Headed revised as suggested.

- P6868, l18: Please introduce abbreviation 'ACO2' with first usage.

This abbreviation was introduced and explained in a preceding paragraph which was inadvertently lost from the online version. This has been corrected in the revised version.

- P6868, l18: How frequent was the IRGA calibrated?

IRGAs were calibrated annually.

- P6868, l21ff: Is this important when considering the quality filtering applied?

It is common for the times of day to be specified for photosynthesis measurements, because of the sometime strong diurnal pattern in leaf photosynthesis. This shows that we kept our measurements within a similar time range on each sample outing.

- P6871, l15: Differences in general or between treatments?

We found significant differences among years in soil moisture- we are describing just the interannual variability in this sentence. Line 15 has been rephrased to clarify this.

- P6872, l16: The differences reported for Tsoil are surprisingly small, are these based on daily/weekly averages?

In line 16, the values referenced are not differences (e.g., between treatments), but ranges of variation over the course of a day in Tsoil, encompassing all treatments. Perhaps the reviewer is referring to the next paragraph, where relatively small warming treatment effects are summarized. These are correct, and they were annual averages of the 1 h soil temperature data.

- P6873, l14: The sub-heading does not fit to the content of the paragraph, which deals predominantly with canopy light levels

We added 'aboveground' to line 17 to reinforce that Xo is the measure of aboveground biomass at mid season. The use of light as a proxy for biomass at midseason is also explained in the methods (Section 2.3.2).

- P6873, l23: It might be useful to include the abbreviation in the heading (ANPP).

While the abbreviation 'ANPP' could certainly be included in the heading, it would seem that for consistency, all abbreviations should be included in all headings where there is an associated abbreviation. We appreciate the reviewer's intent to clarify, but since ANPP is a very common abbreviation in this subject, and since we have used the convention of introducing abbreviations at first mention in the text, we prefer to leave the headings as text, and trust the reader will make the association.

- P6875, l9f: What about exchanging the order of argument 2 and 3 to improve understanding?

We have exchanged the order of the clauses in this sentence as suggested.

- P6878, l7: Towards C4 plants (see e.g. Morgan et al. 2011, Nature)?

We appreciate the reviewer's point, and shifting toward C4 grasses might be one way that communities might change under more variable rainfall patterns. However, increased variability implies the potential for both drought and wet periods, and community change would likely be different in response to each. We find that a better closing sentence here would be to say that 'these differential response could provide a mechanism for increased temporal variability in community structure', to avoid the implication that we are predicting a directional change.

- P6878, l9: To what does 'ecosystem processes' refer to here?

The phrase 'ecosystem processes' as used elsewhere in the manuscript, refers collectively to several variables considered throughout this study- primarily ANPP, soil CO_2 efflux, leaf photosynthesis, along with a few others. In the usage in question here, we mean to make a broad statement about the effect of increased intra-annual rainfall variability on most of the metrics we considered. The section goes on to highlight the specifics.

- P6878, l10: What is the 'statistical structure of rainfall inputs'?

Here we were referring to the finding illustrated in Figure 1B where the probability distributions of events sizes and dry intervals were changed substantially by the altered timing treatment. We have inserted a callout to Figure 1B and instead use the phrase 'probability distributions' to clarify that we are referring to these distributions.

- P6882, l25: I am not aware of the respective journal guidelines, but wouldn't a subheading 'Conclusions' improve the clear structure of the manuscript?

We added a subheading as suggested.

- Fig. 3: Is the first graph (A) really needed and if yes, shouldn't it content wise be a separate graph? Furthermore, the error bars of 1 SE are barely visible and my personal impression is that SD might be more appropriate to report here to get insights on intra-annual variability.

Because a major emphasis in this paper is on interannual variability, we find it very important to provide a range of interannual variability in mean soil temperature, so the reader may compare the magnitude of that interannual variability to the magnitude of the warming treatment effect, and to the effect of the altered rainfall treatment. It is included in Figure 3 because that is where we present the major sources of variation in soil temperature. Because we use standard errors throughout the figures, we believe it would add confusion to make an exception here.

- Fig. 5: Axis caption appear rather small and hard to read here. Abbreviation ANPP needs to be explained in caption. Inconsistent panel numbering (vertical order priority compared to horizontal in Fig. 3).

We have rearranged the lettering in Figure 3B-E to be consistent with the other figures. We agree that the fonts were too small in Figure 5, and have increased the font size and added the explanation of ANPP.

- Fig. 6: It might be helpful for the reader when C3/C4 would be added in parenthesis behind the species.

C3/C4 designations were added as suggested.

- Fig. 7: The scaling of panel D (winter fluxes) substantially differs from the other panels and thus gives a false impression of rather high winter fluxes. I would suggest correcting the scaling or at least noting the different scaling in the caption.

We judged that using the same scale for winter and summer efflux means would render the significant winter differences very hard for readers to discern, and so we added a note to the caption to bring attention to the scale difference.

- Fig. 8: Abbreviation ANPP needs to be explained in caption. 'Regression statistics are shown in Table 1' (missing word?): If the regression statistics are reported separately in Table 1, are both (table and figure) and really needed or provide redundant information? Furthermore, see comment on panel numbering in Fig. 5.

We added the ANPP abbreviation, corrected the 'regression statistics' phrase, and the panel number is consistent with Figs 3 and 5.

- Fig. 9: ANPP – abbreviation? Midseason aboveground only or including belowground as well?

We added the abbreviation, and also indicated 'aboveground' for them id season biomass, because our midseason biomass estimate was based on canopy light extinction, as explained earlier.

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Technical Corrections:
C2850

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- P6860, l18: increased soil temperature in 5 cm depth?

Rephrased as suggested.

- P6868, l8: check spelling of 'weighted'

'Weighed', a verb, denotes the act of determining the weight of an object, which is the sense we intend here. 'Weighted', as suggested by the reviewer, is a transitive verb meaning to add weight to, make heavier, or to load down. This term would not be appropriate here.

- P6871, l11: larger/higher?

'Greater', an adjective, denotes larger than others of the same kind, which is the correct connotation here, also in the mathematical sense of one quantity being greater than (i.e., >) another. 'Higher' means projecting upward, tall, or elevated, a connotation of vertical distance. 'Larger' has a similar meaning to 'greater' in noting size or quantity.