The response to the different comments from the reviewers is provided below**.**

Response to Reviewer 1

Comment1: P1651: Ln 10-29: The authors list a variety of previous models which have been developed and published in the literature. This information would be much easier to digest and compare in a tabulated format with the name of each model, type of parameterization, variable list then reference. To provide a better motivation for the development of a more advanced model as presented in this manuscript, the authors should outline inaccuracies and shortcomings of all the previous models which have been listed with associated uncertainty if available.

Response: *This work is focusing on developing an easy to use and simple regression model for estimating diffuse Photosynthetically Active Radiation (PAR) (400-700nm) using easy to measure environmental variables. In this study we use a much larger data set than any previous studies covering a much larger geographic area and ecosystem types. We have presented the short comings of the existing models and our goal for development of this model in the introduction section. We agree with the reviewer that presenting a detailed overview in the form of a table will be helpful for the readers, but we feel that such an intense treatment of literature is suited for a review type paper. The primary goal of our paper is model development and that is what we strive to achieve in this work.*

Comment2: P 1651, ln 26: As stated later on at the start of Section 3, the BRL model is similar to that presented here. This should be highlighted in the introduction as it appears on reading Section 3 that this work is in fact an extension of the BRL model using more variables.

Response: *The BRL model is developed for estimating shortwave diffuse radiation fraction and so in its original form cannot be used to model diffuse PAR. In this work we are adopting the particular form of the equation which can capture the particular shape of the relationship between PAR diffuse fraction and PAR clearness index. Since we have to make comparison with a PAR diffuse fraction model, we selected a cubic model (Jacovides et al., 2009) to make a meaningful comparison. A sentence will be added to introduction section to indicate this.*

Comment3: P 1652, ln 16: A figure needs to be included showing the location of the 9 Ameriflux sites so that the reader can gauge the quality of the sampling with respect to latitude and elevation. Six of these sites seem to be at 2 locations as given in Table 1. How are these sites different given their close proximity? Is there sufficient coverage to be sure that the parameterization gives good results across the US?

Diffuse PAR is a variable which is not measured in standard meteorological observatories and it is also not a standard variable measured in the AmeriFlux sites. Only very few sites in the network have these sensors and we used data from such sites. We included the multiple sites from the same location as they represented different management conditions and ecological disturbances (Flagstaff, NM sites) and different crop management practices (Agricultural sites in Mead, Nebraska). The latest data set was obtained from the Ameriflux website which includes 19 sites and is now used to develop the model. A map is also included which shows the location of these sites as Figure 1.

Comment4: P 1653, ln 4: Where does RE come from? Maybe from a geo-stationary or earth orbiting Satellite or a measured solar spectrum (e.g. Atlas-3)? How do you account for the fluctuation in RE due to e.g. the 11-year solar cycle or is this irrelevant for this work?

Response: *Extra-terrestrial irradiance (RE) is modeled as a simple equation (equation 1). This type of relationship was used in many previous studies as it is simple and requires only the geographic location and time of day. This relationship doesn't take into account the sun spot activity cycle. Most of the studies that would require diffuse PAR as a variable will be studying intra or inter annual variability and study periods seldom exceed decades. Hence sunspot activity will not be an important factor in this model development.*

Comment5 P1653, ln 6-9: This implies clear-sky only measurements are exploited which seems odd considering that the diffuse component increases with respect to cloud cover. Why is fractional cloud cover not a parameter which affects the parameterization?

Response: *The data set used in the study comes from the Ameriflux sites, which are primarily ecosystem carbon and energy flux measurement sites. These sites mostly have simple radiation sensors (Net radiation components and PAR) and only few sites feature sensors for measuring diffuse PAR. Fractional cloud cover data is not readily available as it will require data generated by a human observer, complex instrumentation or from a satellite derived product. The model is primarily intended for aiding researchers in understanding ecosystem response in terms of carbon and energy exchange in relation to the diffuse PAR fraction with data recorded at the site. Hence we choose the variables collected at the site as independent variable for the model. We fully agree with the reviewer that cloud fraction has an important role in determining the diffuse fraction, but we feel that inclusion of clearness index as a model driver, which is related to the cloud fraction will account for the effects of cloud fraction.*

Comment6: P1654, ln 6: Why throw out data points of RH 100% then?

Response: *Relative humidity of 100% indicates condensing conditions and this can cause dew/water droplets to form on the optics of the sensor. This can cause errors in the measurement of direct and diffuse radiation components from the BF3 sensor which is used in all the sites considered in this study. For the same reason we avoided all conditions when the rainfall exceeded 5 mm*.

Comment7: P1654, ln 16: Is there any wavelength dependence or plant type dependence of this albedo value and, if so, how is this treated considering that scattering (i.e.) the diffuse component is also dependent on wavelength.

Response: *Albedo generally refers to the ratio of outgoing to incoming radiation in the shortwave range (0.15-4 µm). Albedo of vegetated surface can vary from 0.05 to 0.25 and for agricultural sites there can be changes in albedo as the crops gets harvested exposing the bare soil. Changing seasons can also influence albedo as a snowpack has a much higher albedo (0.4 to 0.95) compared to a vegetated surface. Since albedo is an indicator of surface changes, we included this as a driver for the model. PAR reflectivity would be an apt variable to use instead of albedo, but since this variable is not routinely measured at the flux sites, we used albedo which is more commonly available.*

Comment8: P1655, ln 5: It would also be logical to compare against the BRL model to show that the authors have actually made improvements to the basic algorithm.

Response: *The BRL model is developed for estimating shortwave diffuse radiation fraction and so in its original form cannot be used to model diffuse PAR. In this work we are adopting the particular form of the equation which can capture the particular shape of the relationship between PAR diffuse fraction and PAR clearness index. Since we have to make comparison with a PAR diffuse fraction model, we selected a cubic model (Jacovides et al., 2009) to make a meaningful comparison.*

Comment9: Pg1656, ln 16-17: Is this due to the longer path length through the atmosphere increasing scattering? What is the ratio for diffuse/direct. This implies that the parameterization works well until the direct/diffuse falls below a certain threshold.

Response: The logistic model we develop under performs when we have higher solar elevation angles (noon time) and when the clearness index reaches a value around 0.67. This point represents a clear sky condition above which the diffuse PAR fraction stays constant with increasing total PAR. Other diffuse radiation models (piece wise regression) have used this transition point to (Leuning et al., 1995) prescribe different model coefficients. We are not completely sure about the reviewers question "what is the ratio for diffuse/direct" , but we could improve the discussion session, by including more detailed explanations as given in the first few sentences of this response.

Comment10: Pg 1657, ln 1-5: This implies there should be a different set of co-efficient for each season. Why was this not done when it could improve the parameterization?

Response: *Our main goal was to develop a very simple model to aid researchers in synthesizing the impact of diffuse PAR fraction on ecosystem carbon exchange. We agree with the reviewer that developing coefficients for each season will provide a much improved parameterization, but it will make the model difficult and cumbersome to use. The model in its presents form has 10 coefficients and seasonal fitting could increase them by four or five fold. However with the newer data set which has more number of points, we have included determined the seasonal coefficients and they are presented in table 4.*

Comment11: Figures 1a and b are not presented well as this is a scatter plot masking any type of relationship between the parameters. The authors need to bin the data with respect to relative humidity.# and then provide a mean plus standard deviation of the data point.

The figures are now redrawn according to the suggestions of the reviewer.

Comment12: Figure 2: Same comment applies as for figure 1.

Response: *The Figure 2 represents the actual fit of the model to the data. Binning both the measured and modeled data will produce two lines and this will not reveal how the logistic model captures the variability in the diffuse fraction compared to the cubic model. The panels a) and b) reveal how the*

second fit corrects some of the errors which could occur with a single logistic fit. Hence we believe that scatter plots are more appropriate for this figure.

Comment13: Figure 3: Suggests with the correct set of variables the negative bias could reach >25%? If true please expand the text related to Fig 3.

Response:
Inclusion of the new data set has changed some of the results and we have expanded the discussion section to include more details as shown below.

The largest differences are associated with clearness index values around 0.67, albedo values of 0.24, moderate relative humidity (between 50-60%) and solar elevation angles of 46˚ (Figure 3). The logistic model thus produces the largest errors under moderately clear sky conditions, during the late morning and afternoon periods and when the atmosphere has moderate humidity. The PAR clearness index values close to 0.67 represents a clear sky condition above which the diffuse PAR fraction stays constant with increasing total PAR. The inability of the model to accurately capture this behavior results in large errors around this clearness index threshold. Further higher PAR clearness index values indicate low diffuse PAR fraction levels, which along with the above mentioned PAR clearness index threshold can lead to uncertainties in the measurement of the diffuse PAR fraction by the sensor. Albedo value of 0.24 produced the large errors as this is in the range of most vegetated surfaces and hence other confounding factors contributes to model errors around this albedo range.

Response to Reviewer 2

Comment 1: P1649. Title: I would recommend to fully write out the acronym PAR in the title to make the study clear to newcomers and outsiders to the field.

Response:
The title is now modified to read "Development of a semi-parametric Photosynthetically Active Radiation partitioning model for the Contiguous United States"

Comment 2: P1652. Line 27: 'removed outliers'. In the remaining of the paragraph some criteria are given. Is this the what you meant with removing outliers, or is this in addition to that? If the former, then please make the clear in the text and if it is in addition, then please clarify what the criteria are to remove outliers.

Response: The removed outliers in this case refer to removal of data points in addition to the criteria mentioned in the text. This removal process was done after visual examination of data to remove points which are physically not possible. Such data points could occur due to electronic noise or instrument malfunction. Since we didn't do any de-spiking or statistical screening of data, we resorted to such a simple method. More over some of the agricultural sites included center pivot irrigation systems which can affect the sensor performance.

Comment 3: P1653. Line 10: I would like to see somewhere how many years of measurements are available for each station. Maybe this could be included in Table 1.

Response: This data is now included and the modified table is included in the manuscript. The sensor name column is also now removed.

Comment 4: P1653 Eq 1: R_E is not mentioned in the text.

Response: line 11 of page 1653 is modified now to include a definition of RE "Extraterrestrial PAR (R_{EP}) was calculated with solar elevation angle at a location according to"

Comment 5: P1653, line 22 and Eq. 2: In line 15 the sine of the solar elevation angle is set as sin . I would use the same variables here: thus not S (in the text, or is this actually the albedo?) or just (in the equation). Also in the equation, k_t appears twice.

Response:
The sine of the solar elevation angle is represented as sinβ, just in the equation. This same notation is followed in equation 3 and 1. Equation 2 has a different notation, which was the original form the authors used, this will be changed to maintain uniformity and this change will also be made to line 22 of the text on page 1653. The original model of Ridley et al., (2009) has the clearness index (k_t) appearing twice in the denominator.

Comment 6: P1653, line 3: just to be sure; k_t refers to the daily clearness index and k_tp to the PAR clearness index, correct?

Response: k_t refers to the daily clearness index as used in the model of Ridley et al., 2009 and k_{tp} in our model is a hourly PAR clearness index.

Comment 7: P1654, line 9 and line 18. The figures are discussed here, but I am not sure I can read the figures fully. There are so many points, that some points may be overlapped by others. So, the order of printing actually matters. I believe that the conclusions in this paragraph are correct, but I am not sure if I can easily read this from the figures.

Response: The figure 1 is now redrawn with 2panels and the two panels show the relationship between the diffuse fraction, relative humidity and albedo. The data is now presented after binning albedo and relative humidity into classes of equal width.

Comment8: P1655, line 20-23. In Fig 2 the measured and modeled values are depicted in one plot and it seems that the modeled values lie nicely in the center of the range of measured values. However in Fig. 3 the differences show only negative values. I am not sure I understand this. I would expect that fitting would result in some positive and some negative values.

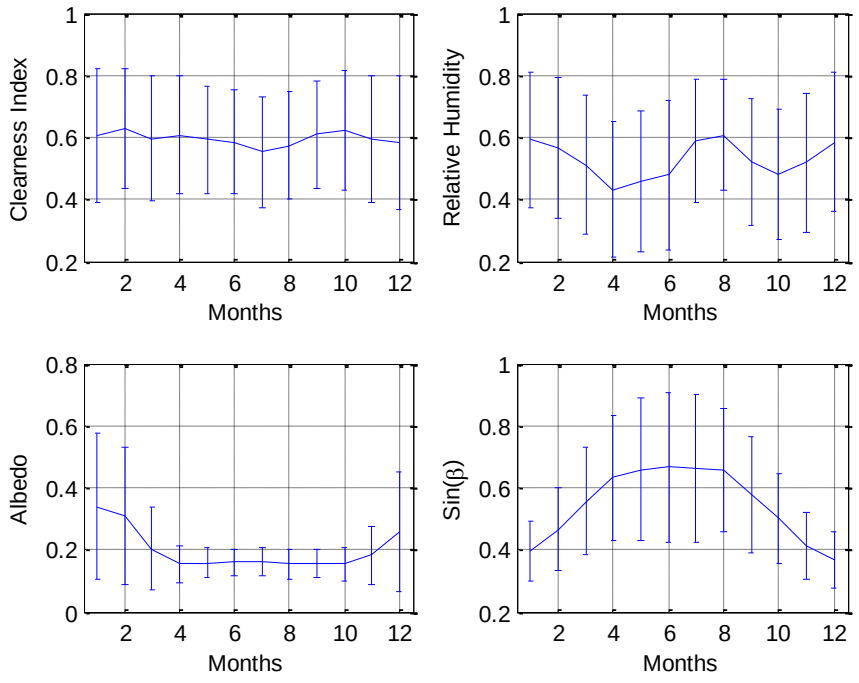
Response: Figure 2 is derived from the data set which is used to estimate the model coefficients (2/3rd of the dataset, whereas figure 4 which shows the fit of the model with coefficients derived from 2/3rd of data to the rest 1/3rd data. This difference in the data sets used for plotting the figures can explain the inconsistencies between figure 2 and figure 4.

Comment9: P1656 line 16. I enjoyed this interpretation of the findings, but I was wondering if there is a physical explanation why it is harder to model these conditions. If so, then please insert that here.

Response: Under clear sky conditions, diffuse fraction is low compared to the total incoming PAR and under such conditions uncertainties in estimation of diffuse fraction are high. Further conditions which limit instrument performance such as lower solar elevation angles and condensing conditions are not included in the study. This could be a reason why errors are more observed under clearer conditions than cloudy conditions. We will include the above paragraph in the manuscript (Discussion Section).

Comment10:P1657 line 3-5: Does one of the parameters show significant different values for Sep-Dec wrt. the rest of the year?

Response: Clearness index shows no seasonal trends, but relative humidity and albedo shows increases during the October to December months. Similar higher values are also observed during the months from January to March. The solar elevation angle shows the highest value during the summer months compared to the fall and winter months as expected.



Comment11: P1662, Table 1: Indicate the total amount of data (measurement points and/or range of years). You might want to consider to remove the sensor column.

Response: The table is now modified; please see response to comment 3

Comment12: P1663, Table 2: The two sets of parameters differ wildly from each other. I presume this is because of strong correlations between certain parameters. Because of these correlations, the 95% confidence interval is of limited value, as you may not freely change all parameters within the corresponding ranges. If the authors have some insight in these correlations, then it may be worthwhile to discuss this in the text.

Response: The parameters included in this model are sine of the solar elevation angle, albedo, and PAR clearness index. The correlation coefficients between these variables from our data set are given below and all correlations are significant at 95% confidence interval, other than the relationship between albedo and PAR clearness index.

Variable	K_{tp}	*RH*	*albedo*	*Sin(β)*
K_{tp}	1.000	-0.489*	0.004	0.192*
RH	-0.489*	1.000	0.289*	-0.210*
albedo	0.004	0.289*	1.000	-0.262*
Sin(β)	0.192*	-0.210*	-0.262*	1.000

A high negative correlation is observed between RH and K_{tp} as increased humidity is often associated with cloudy conditions. Lower correlation coefficients are observed for the relationship among other variables. Under clear sky conditions the diffuse PAR fraction is influenced by the model drivers in a different way than under partially cloudy or cloudy conditions. Hence the model coefficients show a bigger difference above and below a PAR clearness index of 0.68.

Comment13: P1665: Fig 1: I have the impression that I do not see all the points making it hard to interpret these figures. Could the data be presented in a different way, or with a reduced data set?

Response: The figures is now modified, please see response to comment7.

Comment14: P1666: Fig 2: Please explain the difference between panel a and b in the caption.

Response: The caption can be modified to "Model fit for the proposed multi-parameter logistic model (a and b) and cubic model (c). Panel a represents the initial fit to the logistic form and panel b indicates the modification to the initial logistic fit with a second logistic fit"

Comment15: Technical corrections
P1664: Caption: estiamte ➔ estimate

Response:

This error is corrected now.

List of Relevant changes to the manuscript

1) The model data set is now revised with a much larger and newer data set with a greater geographic coverage
2) The inclusion of the data set has changed the model coefficients and performance parameters
3) New figures have been included to explain seasonal variations and inter-site variations
4) A new table is included to show seasonal coefficients which are determined by classifying the data according to the seasons
5) A map is now included to show the geographic location of the sites
6) Inclusion of the data from sites in Alaska requires changing of the papers title to “Development of a semi-parametric PAR partitioning model for the United States”