We would like to thank the reviewer for their time and effort in reviewing the manuscript. Their comments have improved the manuscript for which the authors are greatful. Below are our point-to-point responses to the comments. For clarity, we colored our responses in blue.

• I am somewhat familiar with LWA, but have always struggled to understand the mapping from equivalent latitude back to the conventional geographic latitude. A brief discussion of this issue would be beneficial, especially for the unaccustomed Reader.

We're not sure how to construct a map from equivalent latitude back to the geographic latitude. Perhaps a better way to think about it is that for every geographic latitude there is a contour of potential vorticity (or in our case geopotential height) for which that latitude is the equivalent latitude. As stated in the text the area within the equivalent latitude circle is equal to the area within the q=Q contour. So for any latitude we can find a contour for which the area within the contour is the same as the area within that latitude circle. This is also shown in Figure 1. (No change to text).

• The ozone dataset is normalized to have unit variance. Is the local variance introduced back into the analysis of the projection value of the ozone influence from AWA?

This is a good point. We did not normalize the time series but just deseasonalized it. We state now on Page 6: "The linear relationship between changes (in time) of deseasonalized ozone at a point (i0, j0) and changes in deseasonalized wave activity at another point (i, j), can be simply expressed as the slope of ozone with respect to wave activity (ppb/m2)(Si0,j0(i,j))."

• I understand the need to look at a broader time period to analyze the ozone-AWA relationship, but I do not follow the logic presented in equations 5 and 6. Can beta be interpreted as the importance of the pattern contribution to the seasonal value? And then alpha is a sort of baseline amount? It seems to me that beta will change with emissions and climate, and also be intricately linked to AWA. It is stated on Page 12 that beta doesn't change, but shouldn't it? If more days are under larger AWA or the magnitude changes, I would expect the contribution to seasonal averages to Change.

Yes, beta is the contribution of the pattern to the ozone concentration and alpha is the background value. To clarify this we have amended the text to read:

"Therefore, to correct for this we build a linear regression model where we relate O3i0,j0 (t) to pi0,j0 (t) through the linear regression coefficients alphai0,j0 and betai0,j0, where beta is a measure of the overall sensitivity of ozone to AWA and alpha gives the ozone background concentration:"

Alpha and Beta might change with changes in emissions or the emissions distribution, but here we are interested in the emissions independent change (e.g., the change due to changes in the circulation). We expect the sensitivity of future ozone to future wave activity (ppb/m2) (assuming no changes in emissions) to be the same as the present sensitivity. In other words, the same pattern of wave activity would be expected to change ozone to the same extent in the future as at present. To clarify this we have added: "The fact that Beta does not significantly change in the future is confirmed by an analysis of the GCM2100 simulation (see section 3.3.2)."

The background value of ozone (reflected in alpha) might change in the future due to changes in temperature, but changes in alpha are not represented in equation 6.

• Are the quantities in Figure 2 estimated from monthly mean geopotential heights or the average of daily AWA/LWA? Should there be a difference?

We use the average of daily AWA. We have clarified this in the caption. There should be a difference. Use of monthly mean geopotential heights would result in an AWA with smaller magnitude.

• Is AWA normalized to unit variance for the MCA analysis? It strikes me that the variables should have comparable variance in order to prevent one from dominating the results.

No. MCA does not require the scaling of the input. Normalization does not change the result.

• I have to ask about the reliability of the projections given an ensemble of three and relatively short analysis record. There is some discussion, but I think a bit more is warranted.

As the reviewer recognizes there is considerable variability in the ensemble means. In the present climate we examined a 20-year time period due to limitations in the data record. For consistency we also used a 20-year period averaging period in the ECMWF data and in the two constant climate simulations (GCM2000 and GCM2100). In the ensemble simulations we examined a 10-year period as these simulations are non-stationary and we wanted to examine differences representative of the 100 year timespan between 2100 and 2000. We point out the variability due to this short averaging period in reference to Fig. 8 (with implications for the variability in Fig. 9) and in the conclusion. We point out the importance of this variability in interpreting long-term trends. It is unclear to us how much more discussion of this is warranted in the paper.

• What impact does the spatial and temporal resolution of the geopotential height fields have on the estimate of AWA? I assume additional structure is available with higher spatial resolutions. The resolution used here in the reanalysis and climate model output is rather course. Would a higher spatial resolution product improve the relationships?

For Fig. 2, the reanalysis and the model used geopotential height at different resolutions to calculate AWA. The reanalysis has a spatial resolution of 1.125*1.125 and the model has the resolution as 2.5*1.9. The AWA patterns are similar at these different spatial resolutions so we doubt the higher spatial resolution would improve the estimate of AWA. Furthermore, one of the points of using an analysis based on AWA is it captures the larger spatial scale aspects of the circulation (aspects where we might expect to capture future changes) as opposed to the fine details. Thus we do not feel that higher resolution products would improve the relationships.

Specific Comments

• Page 2, Lines 7, 11, 12 - There are some citation formatting issues here.

Changed in text.

• Equation 2 - I am confused how both the cyclonic and anticyclonic LWA integrate inclusively to equivalent latitude.

The cyclonic part of the wave activity is that part south of the equivalent latitude (AC < 0) while the anticyclonic part is to the north (AA > 0). Note that the total LWA is defined by AC - AA (Equation 3) so these two parts don't cancel each other.

• Page 5, Line 23 - Stating "scenario" and "pathway" is redundant.

'Pathway' is removed in the text.

• Table 1 - The SST/Sea Ice and Meteorology columns are unnecessary if they're all the same value (online)

These columns are removed in the text.

• Page 5, Lines 32, 33, 34 - Subscript missing in CH4 and CO2.

Changed in text.

• Equation 5 - Is O3;i0;j0(t) the seasonal (JJA) average of MDA8?

It is the JJA average of deseasonalized MDA8.

• Page 8, Line 29 - A period has gone astray.

Changed in text.

• Page 9, Lines 1-7 - Does the interpretation of Shen et al. (2015) for their first two EOF modes match the physical explanation for the first two MCA patterns presented here?

The MCA patterns we find are consistent with those of Shen et al. (2015). Consequently, we assume the MCA patterns found in this study are consistent with the physical explanation in Shen et al. (2015)

• Page 9, Lines 1-3 - Is there any reason to prefer this method over the EOF analysis of 500hPa heights?

MCA is a more elegant way to find the primary modes of variability between two fields than using the two-step procedure of performing an EOF on one field and then correlating with another field. It also offers a more concise interpretation of how the variability of the fields are related.

• Page 9, Line 19 - “less also” should probably be just “less”

Changed in text.

• Page 9, Lines 18-19 - This is too be expected, right? The southeastern US flow from the Gulf is more mesoscale and likely poorly resolved transport in a global model at this resolution.

We would expect that a model at this resolution would be able to simulate a large scale feature such as the North Atlantic subtropical anticyclone. While we might expect a global model not to correctly simulate details of the Great Plain low level jet, it should be able to simulate the overall flow.