In the reply, the referee's comments are in *italics*, our response is in normal text, and quotes from the manuscript are in blue.

Anonymous Referee #2

General comments

While the introduction has sufficient breadth, there are areas where it lacks a discussion of more recent work. For example, regarding the statement ". . .climate model simulations. . .indicate a poleward expansion of the Hadley circulation, though weaker than that observed", there numerous studies suggesting that this may not be the case. Choi et al. 2014 and Quan et al. 2014 both suggest that reanalysis trends in the HC edges may be overstated, especially compared to independent observations. And it appears that the model trends may not be so different from the reanalysis trends (Garfinkel et al. 2015, Davis and Birner 2017). The choice of metric also matters (Solomon et al. 2016). My understand is that it's actually unclear whether models, reanalyses, and observations disagree on the response of the Hadley circulation; but that itself is a valid motivation. The authors may also consider connecting their work to studies like Schmidt and Grise (2016), who have investigated some of the longitudinal characteristics of Hadley cell variability. Full references can be found at the end.

Reply: Thanks for these suggestions. We modify the introduction to include many suggestions by both referees:

Climate model simulations with increased greenhouse gas forcing also indicate a poleward expansion of the Hadley circulation, (Hu et al., 2013; Ma and Xie, 2013; Kang and Lu, 2012; Davis et al., 2016). Vallis et al. (2015) analysed the response of 40 CMIP5 climate models finding that there was only modest model agreement on changes. Robust results were slight expansion and weakening of the winter cell Hadley circulation in the Northern hemisphere. It is unclear how closely the model simulations match reality. Choi et al. (2014) and Quan et al. (2014) both suggest that reanalysis trends for the Hadley cell edges may be overstated, especially compared to independent observations, and model trends are in reasonable agreement with the reanalysis trends (Davis and Birner, 2017; Garfinkel et al., 2015), but choice of metric also matters (Solomon et al., 2016) when discussing trends.

Many authors have considered the impact of greenhouse gas forcing on the Hadley circulation, particular in respect of changes in the width of the tropical belt (e.g., (Frierson et al., 2007; Grise and Polvani, 2016; Johanson and Fu, 2009; Lu et al., 2007; Seidel et al., 2008), but far fewer have discussed changes in Hadley intensity (Seo et al., 2014; He and Soden, 2015). The importance of tropical belt widening is of course due to its impact on the hydrological system, especially the locations of the deserts (Lau and Kim, 2015; Seager et al., 2010), which are a critically important for the habitability of several well-populated areas.

I think the GISS-E2-R model should be excluded from the composite figures, while the composites with GISS should be shown in the supplementary information (opposite to what is currently done). The authors have made a good choice to show composites with *and without GISS, as its behavior deviates so much from the other models, but I think swapping these figures would better support their conclusions and interpretations while still maintaining full disclosure.*

Reply: We thought long about doing this. Although GISS-E2-R model anomalies in G1 and abrupt4xCO2 are different from other models they more or less plausibly describe Walker and Hadley circulation structure. We felt that it would be better to include the GISS model results in the ensembles we show in the main text simply because of the virtue of including more models. The GISS model does not unduly affect the ensemble means, and while in some cases it does affect the model spread, this is perhaps more representative of actual uncertainties when the models attempt to simulate the large forcings in G1 and abrupt4×CO_2.

It would be helpful if the authors were more explicit and definitive throughout the manuscript. For example, on lines 97-100, it would be helpful to readers if the sign of the changes were stated, e.g., ". . .report that decreases in Hadley cell intensity drive the reduction in tropical precipitation under solar geoengineering. . .". Or, on lines 144- 146, state whether the abrupt4xCO_2 experiment is close to RCP8.5 in terms of CO2 ppm or in terms of radiative forcing. What follows is a list of some but not all of these instances: -Line 74 -Line 80 -Lines 103-104 -Lines 223-224 -Lines 251-252 -Lines 293-294 -Lines 337-340 (are intensity changes signed, or in terms of absolute value?) -Lines 480-482

Reply: Thanks, we have made the changes suggested:

97-100: report that decreases in Hadley cell intensity drive the reduction in tropical precipitation under solar geoengineering

144-146 an atmospheric CO_2 concentration of nearly 1140 ppm, close to concentrations under "business as usual" scenarios such as RCP8.5

74 produce net drying due to the decreasing in vertical temperature gradient

80 The general pattern of temperature change under all abrupt4×CO_2 includes accentuated Arctic warming, and least warming in the tropics. G1 largely reverses these changes, but leaves some residual warming in the polar regions and under-cools the tropics relative to piControl. Geoengineering also reduces temperatures over land more than over oceans relative to abrupt4×CO_2, and hence reduces the temperature difference between land and oceans by about 1°C.

103-4 This tropical circulation pattern is intimately related to changes in the Walker circulation by their dependences on the Pacific Ocean zonal sea surface temperature gradient

223-4 The boundary at the edge of the tropics is also known to move latitudinally but the circulation cell rapidly becomes weaker beyond the zero crossing of the rotation sense.

251-2 This sentence is not relevant so we delete it.

293-4 The Southern cell shows a complex anomaly structure with positive anomaly between 45°S-65°S also in the Ferrel cell circulation that borders it at higher southern latitudes.

337-40 We have defined the Hadley circulation intensity in section 2.3 as we write in line 334-336

480-482 Beyond the Hadley cells there are modest, but statistically significant changes, particularly in the Southern hemisphere Ferrel circulations with poleward movement.

I have difficulty discerning information from the anomaly contour figures, like Fig. 2. Standard practice is to show control values overlaid as contours on the shading, so that shifts/expansions/contractions can be more easily discerned. This is especially critical for the Walker circulation - its mean structure and response have substantial spatial variability

Reply: Figure2, 5, 6 have been redrawn with piControl contours overlaid anomalies shading.

The questions posed at the end of the introduction are a great way to orient the reader. It may be worth specifically restating these questions in the discussion as a way of summarizing the results.

Reply: To answer the questions we posed in the introduction we reorganized our paper and add a summary section:

Our main purpose in this study has been to answer the following questions: Does the G1 scenario counteract position and intensity variations in the Walker and Hadley circulations caused by the greenhouse gas long wave forcing under abrupt4×CO_2? How does the tropical atmospheric circulation, including the Walker and Hadley circulations, respond to warm and cold phases of the El Niño Southern Oscillation (ENSO) in G1 and abrupt4×CO_2?

The Walker circulation in G1 displays insignificant increases in intensity and no shift in its western edge in the Pacific Ocean relative to piControl and hence does counteract the changes from greenhouse gas forcing. There is a potentially important change in position of the Walker circulation associated with the West African rainforest and East African grassland zones under G1, with potential for the encroachment of a drier climate into the Congo basin. In contrast, the Hadley circulation shows larger changes under G1 that are not simple reversals of those induced by greenhouse gas forcing on piControl climate. There is an asymmetric response between the hemispheres under both greenhouse gas and solar dimming that are correlated with direct forcings rather than adjustment of sea surface temperatures, and correlated with changes in meridional and land-ocean temperature gradients. These differences in response of the Hadley and Walker circulations are consistent with the zonally invariant forcing of both solar dimming and greenhouse gases and the meridionally varying solar dimming.

A clear Walker circulation westward movement during El Niño and an eastward movement during La Niña are shown nearly everywhere along the equator in abrupt4×CO_2. However the eastern and western boundaries of the Walker circulation shift westward during El Niño in G1 relative to piControl. The range and amplitudes of significant changes are smaller in G1 than in abrupt4×CO_2. The same is true in general for the Hadley cell. Under abrupt4×CO_2 the Northern Hadley cell significantly decreases in intensity under both la Niña and El Niño conditions while under G1 the decreases are smaller and limited to each cell's poleward boundaries.

Both models and the limited observational data available on the Hadley circulation indicate that it is not zonally symmetric: there are intense regions of circulation at the eastern sides of the oceanic basins (Karnauskas and Ummenhofer, 2014), while elsewhere circulation is reversed, and much of the natural variability of the circulation is related to ENSO (Amaya et al., 2017). This and the opposite correlations with surface temperatures in the Pacific and SPCZ with STRF under G1 (Fig. 12) suggests an interplay between Hadley and Walker circulations that could repay further consideration of model data at seasonal scales. The importance of the tropical ocean basins as genesis regions for intense storms also suggests that changed radiative forcing there under geoengineering could cause important differences in seasonal precipitation extremes, that may be hidden in monthly or annual datasets.

The figure production quality is high, but the image quality is low. Per ACP guidelines, PDF or EPS is preferred so that the figures are crisp when zoomed in (save with vector graphics enabled; in MATLAB, it's the "painters" renderer, not sure how this works in IDL or other languages). Otherwise, I think the DPI needs to be increased for the figures.

Reply: The figures are standard pdfs as made, and passed the quality control for ACP. The final version figures would be higher resolution we expect. But the figures zoom quite well to 300% or more in our viewer.

Specific comments

Line 65: Define "SRM" here rather than on line 73.

Reply: done

Line 99: What are the seasonal changes?

Reply: we rephrase: and that seasonal changes mean that the ITCZ has smaller amplitude northward shifts compared with no geoengineering.

Line 135: ENSO was already previously defined.

Reply: Deleted here

Lines 191-194: What is the order of the variability in the first 3 years - 1 sigma, 2 sigma? This may help convince readers it's nothing to worry about.

Reply: rephrased as: that have significantly ($p<0.05$) higher STRF in the first 10 years of the abrupt4×CO_2 simulation than in following decades. This is not due to a transient affecting the first few years, but rather to higher values around 3 years into the simulation, but this is not unusual for each model's multiannual and decadal variability.

Lines 227-233: I am somewhat unclear on the metrics for Hadley cell intensity. It seems like the authors are using the average of the 900-100 hPa stream function; are they *using a point maximum or an area average?*

Reply: We rephrase for clarity: Thus we define the Hadley circulation intensity for the Southern cell as the average meridional stream-function between 900-100hPa over the area between 40°S and 15°N in July, August and September (JAS), and the Northern cell as the absolute value of mean meridional stream-function between 15°S and 40°N in January, February and March (JFM).

Line 239, 283-285: "Intuitively": "effectively" or "naturally" might be more appropriate?

Reply: We prefer the version as it on line 239. We rephrase line 283 as This can naturally describe

Line 243: Mentioning a specific number is good, as is describing how it is derived.

Reply: This seems clear from the text the intensity measured by STRF is underestimated by 3% relative to ERA-Interim. This number is the relative change between piControl and ERA

$$\frac{STRF_{piCotrol} - STRF_{ERA-Interim}}{STRF_{ERA-Interim}}$$

Lines 263-266: The scatter here is very large among the models, even neglecting GISS, which I think is worth noting.

Reply: yes this is true, a similar point was raised by ref#1. Another method commonly used to assess model agreement is if some super-majority, e.g.75% of models agree on the sign of an anomaly. Using that measure implies that the change under abrupt4×CO_2 is not robust. We add the model agreement where relevant to the results we present, for example we change:

"However there is some scatter between models (Table 2)." To:

There is significant change in the ensemble mean position and strength under abrupt4×CO_2, but not G1 in Table 2. However, only 5 out of 8 models agree on the sign of the changes, so the inter-model differences are rather large in this case.

Lines 289-290: It might be more effective to say that there is enhanced overturning aloft and weakened overturning at lower levels; as written, it almost sounds like the anomalies don't conserve mass (reduced equatorward + enhanced poleward).

Reply: Agree. We change it as "Circulation anomalies under abrupt4×CO_2 (Fig. 5 (B)), show enhanced overturning aloft and weakened overturning at lower levels."

Line 309: How is the ITCZ metric defined?

Reply: This defined in the caption to fig. 7, cited on line 308**:** The ITCZ position is defined from the centroid of precipitation (Smyth et al., 2017).

Lines 321-325: I think this was essentially said previously on lines 288-294.

Reply: yes, we delete the repeated section and rewrite according to suggestions of ref#1.

The situation under abrupt4×CO_2 is more complex (Fig. 6 (E) and (F)). The expansion of the tropics has been noted both in greenhouse gas simulations and observationally (Davis et al., 2016; Hu et al., 2011), along with the larger southern expansion. The extratropical changes in the Ferrel circulation are also more pronounced in the Southern hemisphere.

Reduction in strength of the Northern hemisphere winter cell was also a robust result of climate models under RCP8.5, while, the Southern cell exhibited almost no change (Vallis et al., 2015). Our results in Fig. 8 show that the multi-model ensemble mean reveals a diminished Northern Hadley, intensity under G1 of -18×10^{8} kg s^{-1} and of -7×10^{8} kg s-1 for abrupt4×CO_2. The Southern Hadley intensity in JAS exhibits a fall of -16×10^{8} kg s^{-1} under G1 but an increase of 23×10^{8} kg s^{-1} under abrupt4×CO_2. Thus the Southern hemisphere results differ for abrupt4×CO_2 from those presented in Vallis et al. (2015).

Line 333: Could write simply "more pronounced in the Southern hemisphere", as the "than. . ." is implied.
Reply: Deleted, as shown above.

Line 341: I think this is too colloquial, maybe state the p-value or % significance it reaches - stating something like "99.9% significant" is more convincing than "hugely"
Reply: The exact significance level is very hard to define given the lack of knowledge of the probability distribution at the extreme tails. We change the text: The anomalies for most models are significant, and the ensemble means are 8 standard errors from zero and thus very highly significant.

Lines 399-400: What is meant by "monthly temperature"?
Reply: Monthly correlations between STRF and surface temperatures. These are not relevant to the rest of the manuscript and this sentence is deleted.

Line 409: Which experiments? Citation?
Reply: The citation is Van der Wiel et al., 2016, we were not clear how this was linked to the next sentence. We rephrase this part as: Experiments with an atmospheric circulation model (Van der Wiel et al., 2016) suggest that a key feature of the diagonal structure of the SPCZ is the zonal temperature gradient in the Pacific which allows warm moist air from the equator into the SPCZ region. This moisture then intensifies (diagonal) bands of convection carried by Rossby waves (Van der Wiel et al., 2016).

Lines 413-415: Suggest stating model names, or at least specifying "two models" instead of "three. . .except BNU"
Reply: Rewritten as "Two of the three models with positive correlation between STRF and SPCZ temperatures, CCSM4 and NorESM1-M, have increased STRF and ΔSST under G1"

Lines 420-421: But isn't it the case that there are still some weak, positive correlations in regions like the SPCZ?

Reply: Yes, there are some weak, positive correlations in some region under IPSL-CM5A-LR, MIROC-ESM and HadGEM2-ES. But here we are focusing the most obvious difference in these 3 model compared to others.

Section 6.2/Table 3: I suggest mentioning the highest and lowest model value for each category, or really anything to help illustrate the model spread. With an N of 4, the average doesn't mean as much, but I think this section is still worth including. For the critical relationships, it may help to show them in scatter plots. I'm curious what output fields are needed that are lacking in most of the models?

Reply: Yes we include the range from the 4 models in Table 3

Scenario	G1-piControl		abrupt4×CO_2-piControl	
	North	South	North	South
Temperature gradient	-2.6 (-3.5 — -1.1)	-1.2 (-1.7 — 0.1)	-4.4 (-6.1 — 0.7)	-4 (-6.1 — -0.3)
Static stability	-3.4 (-4.7 — -1.5)	-3.2 (-5.2 — -0.4)	21 (18 — 26)	23 (21 — 27)
Subtropical tropopause height	-0.1 (-2.1 — 1.8)	-0.5 (-1.4 — -0.1)	0.87 (1.2 — 6)	3 (-0.7 — 4)
Function 1	-3.35 (-9.8 — 4.4)	-1.05 (-7.5 — 1.2)	-29.8 (-30 — -17)	-25.5 (-32. — -19)
Function 2	-2.9 (-8.2 — 3.8)	-1.13 (-6.4 — 0.7)	-22.6 (-23 — -12)	-18.5 (-24 — -14)
Hadley intensity	-3.7 (-6.4 — -0.5)	-1.2 (-6 — 0.8)	-3.4 (-4.1 — -1)	4.3 (2.4 — 4.8)

Lines 513-514: Why is this expected? Does it follow from the vertical expansion, or from the Held and Soden static stability/Clausius-Clapeyron scaling?

Reply: The two scaling theories from Seo et al., (2014) listed in Table 3 indicate that the HC strength is proportional to the tropopause height and equator-to-higher-latitude potential temperature gradient. However, most climate models predict both increases in tropopause height and decreases in intensity under greenhouse gas forcing. Vallis et al (2015) summarize the argument as: "A general weakening of the tropical circulation might be expected from thermodynamic and energetic arguments involving water vapour concentration and precipitation (Boer, 1993; Held and Soden, 2006) and reviewed by Schneider et al. (2010). In brief, unless changes in relative humidity are very large, changes in the water vapour content of the atmosphere are mainly determined by changes in the saturation vapour pressure and hence by the Clausius–Clapeyron relation, and so increase by about 7% K^{-1}. However, maintaining a surface energy balance constrains the changes in evaporation and precipitation to be closer to 3% K^{-1}. Thus, the overall water vapour turnover rate will decrease as surface temperature increases, possibly leading to a weakening of the atmospheric circulation, and in particular the tropical circulation – at least to the degree that the circulation is controlled by such an effect. It is however by no means clear that the dynamics of the Hadley Cell is so controlled."

The situation is far from clear theoretically, and so we reflect this in modified text: We note that the robustly understood vertical expansion of the circulation as the tropopause rises under abrupt4×CO_2, has been associated with a decrease in the circulation intensity (Seo et al., 2014; He and Soden, 2015) in climate models forced by greenhouse gases, and as expected from considerations of Clausius-Clapeyron scaling if relative humidity is relatively constant, as summarized by Vallis et al. (2015). This is not the case for the scaling functions from Seo et al., (2014; Table3), where tropopause height change is proportional to intensity change. Nor it is consistent with increases simulated in the Southern Hadley cell intensity and simultaneous decreases in the Northern one relative to piControl, although both are stronger than under the G1 forcing.

Lines 520-524: Grise and Polvani (2016) would be a good reference for the dynamical response in abrupt4xCO2 outpacing the thermodynamic response. Doesn't this call into question the importance of static stability and meridional gradients in driving the changes in the circulation, if the circulation responds faster? Is it possible that the thermodynamic responses the authors examine might follow from some of the circulation changes, as these circulations transport heat?

Reply: Thank you very much. This issue has indeed concerned us as well. We were hesitant to put it into our manuscript, but now we have made the statement following your suggestion.

Grise and Polvani (2016) explored how the dynamic response of the atmosphere, including metrics such as Hadley cell edge, varied with model climate sensitivity, that is the mean temperature rise associated with doubled CO_2. They found significant correlation across a suite of CMIP5 models running the abrupt4×CO_2 were largely confined to the Southern hemisphere, and also that the pole-to-equator surface temperature gradient accounted for significant parts of the dynamic variability that was not dependent on the mean temperature. However, we find that the response times of the Hadley circulations to changes in radiative forcing are very fast, as shown by the lack of transients in the simulated time series. Sea surface temperatures, especially under the strong abrupt4×CO_2 forcing takes at least a decade and parts of the system, such as the deeper ocean, would require even longer to reach equilibrium. Under abrupt4×CO_2 the global land-ocean temperature difference is reduced by about 1.3°C relative to piControl, while G1 reduces the contrast by only 0.3°C. The Northern hemisphere continents have faster response times than the oceans and so we would expect the Southern hemisphere to be much further from an equilibrium response than the Northern. This is also reflected in the lack of an equivalent to the "Arctic amplification" seen in the Northern hemisphere under both observed and simulated forcing by greenhouse gases. The lack of anomalous Southern polar warming is linked to the much cooler surface temperatures in the Antarctic mitigating against both temperature feedbacks and the ice-albedo feedback mechanism (Pithan and Mauritsen, 2014). The speed of response of the circulation changes calls into question the importance of static stability and meridional gradients in driving the changes in the circulation, since the circulation responds faster. Bony et al. (2013) attributed rapid changes in circulation in quadrupled CO_2 as due to direct CO_2 forcing. Fast response could also be a result of cloud feedback, land-ocean temperature differences and perhaps humidity, which are also important for poleward energy transport in G1 (Russotto and Ackerman, 2018; Russotto and Ackerman, in review ACP). Low cloud fraction decrease under G1, warming the planet by reducing the reflection of solar shortwave radiation, but atmospheric humidity is reduced allowing heat to escape, and less energy is transported from tropics to poles.

Line 548: What is "Rx5day"? I would rewrite this more generally, and avoid acronyms unless they will be useful later

Reply: Yes agreed, rewritten as: shows that the annual wettest consecutive five days are drier

Lines 553-554: "Intense" - subsidence?

Reply: We rewrite this as: Both models and the limited observational data available on the Hadley circulation indicate that it is not zonally symmetric: there are intense regions of circulation at the eastern sides of the oceanic basins (Karnauskas and Ummenhofer, 2014), while elsewhere circulation is reversed, and much of the natural variability of the circulation is related to ENSO (Amaya et al., 2017).

Figure 1 caption: Suggest rewriting so description doesn't only apply to the third subplot, i.e., "Walker circulation in the ERA-Interim reanalysis (top), . . . , model ensemble mean under piControl (third row), . . ."

Reply: We will label the panels in the figure. And change the caption: Walker circulation in the ERA-Interim reanalysis (A), NCEP2 reanalysis (B), model ensemble mean under piControl (C) and difference between ERA-Interim and piControl (D). Color bar indicates the value of averaged zonal mass stream-function (10^{10} kg s^{-1}). Warm color (positive values) indicate a clockwise rotation and cold color (negative values) indicate an anticlockwise rotation.

Figures 7, 8, 11, 13: I think the authors have crafted the color scheme to avoid some of the major color-blindness combinations (i.e., no green/red), but a further helpful step is to not rely solely on color when trying to distinguish data points. I encourage the authors to use different symbols/shapes, in addition to their current color scheme, if they want to communicate values for each model individually rather than the behavior of the models as a whole

Reply: In Fig. 7 this could be done, but the other figures would not work well. In Fig. 8 we want the shape (circle) to represent the size of the model standard error, these are the same for each model, having different shapes might confuse that point. Actually in figures 11 and 13 we want to distinguish between the G1 and the 4×CO_2 results by using different shapes, adding more shapes would results in much confusion and difficulty in recognizing 8 or more different shapes. Hence for simplicity and readability overall we prefer to keep the figures with simple shapes.

Table 2: How is the percent change in position defined?

Reply: First we define the position in section 2.2 as : we use the western edge of Walker circulation to represent it position. The western edge is defined by the zero value of the vertically averaged ψ_z between 400 – 600 hPa in the western Pacific 120°E – 180°E, (Ma and Zhou, 2016). Then we use the following function to calculate the percentage change relative to piControl. x here refer to G1 or abrupt4xCO$_2$ experiment.

$$\frac{Position_x - Position_{piControl}}{Position_{picontrol}}$$

We think that the table header: The number in the brackets represent percentage change relative to piControl explains this calculation.

Table 3: The definitions should not be in the caption, but should instead be in the text.
Reply: We prefer not to since the definitions are simply extracted from Seo et al. (2014), and would present a fairly large block of distracting definitions and equations that breaks the overall point of the section. Putting the definitions in the Table means they are included within the paper, but do not distract the reader, or give the impression that we have deduced them ourselves. The placing of the text, in either footnotes as we have now done, or the header as originally, we prefer to leave to the ACP editorial team.

Additional modifications: We also revise the following sections.
(Line 59) Rewrite this sentence.
Model results suggest a significant eastward movement with weakening intensity under greenhouse gas forcing (Bayr et al., 2014), and He and Soden (2015) propose that the sea surface temperature warming plays a crucial role in both the eastward shift and the weakening of Walker circulation. They also note that this weakening may be reversed by rapid land warming.

(Line 207) Correct the writing mistake (5°S - 5°N, 160°W - 80°E) to (5°S - 5°N, 160°W - 80°W)

(Line 450) We change "respectively in south and north cell" to "respectively in Southern and Northern cells"

(Line 475) Delete "Davis et al., (2016) note an expansion in the Hadley cells in proportion to the temperature rises in the models under both G1 and abrupt4×CO$_2$."

(Line 543) Rewrite it as "The reduction in incoming shortwave radiation in G1 would intuitively mean reduced heating, sea surface temperatures and moisture flux in the ITCZ"

(Line 545) Delete "Reduced ocean heating would then tend to mean a smaller amplitude of seasonal movement of the ITCZ"

(Line 269-273) We redraw Fig.1 to show the difference of ERA-piControl rather than piControl-ERA and rewrite the relevant text "Fig. 1 (D) shows that the ERA-Interim circulation has an eastward displacement and the intensity measured by STRF is overestimated by 26% relative to ensemble piControl. There is a similar structure to the stream function differences between NCEP2 reanalysis and piControl, and the STRF is only overestimated by 3% relative to ensemble piControl."

(Line 316-317) We redraw the Fig.4 show the difference of ERA-piControl rather than piControl-ERA and rewrite the relevant text "The intensity anomalies relative to piControl from both the reanalysis data sets are less than 21% (Fig. 4)."