REVIEW 1 for:

Ch 2, a
Ch 2, b
Ch 2, c1a
Ch2, c1b

(I did not read Ch2 c3-4, d, e, or f)

COMMENT FROM REVIEWER:
I think the content of the sections I read is acceptable. The text and figures related to these sections are well-organized and clear, particularly the discussion regarding 2.c.1.b tropical intra-seasonal precipitation patterns (the subject I'm most familiar with). Some minor errors/issues:

* Line 27 (2.a): "ratios" should be "ratio"... the verb is "was"
RESPONSE: Accepted – change made in text.

* Line 82 (2.b.2): ";" should be ":"
RESPONSE: Accepted – change made in text.

* In 2.c.1.a, the phrase "drier-than-average" on lines 191, 200, and 204 should not be hyphenated... the phrase is not acting as an adjective or adverb
RESPONSE: Accepted – change made in text.

* Similarly, in 2.c.1.a the phrase "wetter-than-average" on lines 201 and 203 should not be hyphenated
RESPONSE: Accepted – change made in text.

REVIEW 2

Review of NOAA State of the Climate Report.

General Comments. The report itself is well written and contains adequate information, both quantitative and qualitative, on describing the state of the climate system in 2007. The report is organized well, but the transitions between sections are quite choppy and could be improved for better continuity. The figures are meaningful and convey the appropriate supporting evidence for most arguments made in the report.

Specific Comments.

(1) *Overview Section - Lines 25-26: "…(CO_2) continued to increase in 2007, rising to 382.7 ppb.*" "ppb" should be changed to "parts per million (ppm)." All remaining references to "parts per million" should consequently be changed to "ppm."

RESPONSE: Accepted – changed text from ppb to ppm, and checked all subsequent references in text for accuracy.

(2) *Sidebar Section* – In general, the conclusions drawn by the author from the AMIP and CMIP simulations are too far-reaching. While it is certainly valid to state that the CMIP simulations have a higher degree of continental warming, across the tropics, this warming is of the wrong sign anyways (see Fig. 2.7a). Here, I would argue the AMIP simulations come closer to reality (albeit still attributing positive anomalies across western South America). But my major criticism is with lines 162-164. The experiments run have not exclusively targeted GHG forcing as a *leading* cause (or "likely principal factor" as written on Line 166) but simply as an attributable component of the climate system. Moreover, because the SST dataset used in the AMIP simulations has a signal attributable to GHG forcing, the effective separation of "radiative forcing from GHG" and "SST forcing" is not well done. I highly suggest that the overall conclusions for this section be toned down – moreso to acknowledge GHG forcing as an *important* and certainly non-negligible component of the record warmth experienced over continental areas during 2007.

RESPONSE: Accepted – authors modified the text of the sidebar, with conclusions "toned down" based on the reviewer comments.

(3) *Line 154:* Insert "to" between "relative" and "the" in the parenthetical.

RESPONSE: Accepted – change made in text.

(4) *Line 167:* Change "global" to "global-mean".

RESPONSE: Accepted – change made in text.

(5) *Lines 236 – 238*. The statement "warming temperatures have increased the water-holding capacity of the atmosphere" is incorrect. The relationship described by the Clausius-Clapeyron equation relates the exponential increase of *saturation vapor pressure* with ambient temperature. That is, warmer air can hold more *water vapor*, not liquid water.
Also, there is no cited evidence or appropriate figure supporting a positive trend and/or significant increase in tropospheric water vapor concentrations over the last decade or two.

Trenberth et al. (2005) and references within contain a good discussion about water vapor measurements in the atmosphere and how they vary throughout the world. Note also that water vapor increases may or may not result in increases in precipitable water. The measurements of the latter are sparse and have no definitive correlation with water vapor trends.

RESPONSE: Rejected. However, the Trenberth et al. (2005) reference was added to clarify this point, since that was the source of the original statement in the text.

(6) *Lines 421 – 423*. Based on my understanding of the carbon cycle, carbon emissions into the atmospheric reservoir are "cycled out" after ~12 years. Most of the carbon burial, as noted, is into the oceanic reservoir, of which most eventually ends up in the deep ocean. However, the residence time of the deep ocean reservoir is only about 300-400 years. Hence, any carbon stored in that reservoir that does not deposit into sediments or organic matter on the ocean floor will return to the atmospheric reservoir "in a few hundred years", and thus the climate system would experience the same levels of CO_2 we have today plus whatever additional CO_2 is in the atmosphere at that time. The justification for only 15% of the total carbon emitted to reside in the atmosphere in a few hundred years is therefore unclear to me and should be clarified.

RESPONSE FROM AUTHORS: "The Carbon Cycle is a 2-way stream, CO_2 going into and coming out of the oceans. We are talking about a slow chemical equilibrium between the atmosphere and oceans, with different time constants involved. See for example E. Maier-Reimer and E. Hasselmann (1987), *Climate Dynamics*, **2**, 63-90 (reference was added to the text to help clarify this issue)."

(7) *Line 426:* The author attributes the recent increase in the rate of CO_2 increase in the atmosphere to a longer residence time of fossil fuel CO_2 emissions. There is no basis for this conclusion. As admitted earlier in the section, the terrestrial biosphere is "not well-understood." There could be a natural source of CO_2 that has amplified and increased the trend. Or, parts of the carbon cycle sources and sinks could have changed in the last 10 years.

RESPONSE FROM AUTHORS: Rejected. No, the statement is based on this: When full chemical equilibrium between the atmosphere and oceans has been reached (assuming no change in circulation and biology) there is a permanently airborne fraction of 15%. "Permanent" is used in the sense that geological erosion/sedimentation processes have been ignored. The latter have time constants of several thousands of years in the case of the carbon cycle.

(8) *Line 435:* The average uptake of North American ecosystems has a larger error bar than the measurement itself! Is this correct? If so, what is to be said about this measurement and how reliable is it?

RESPONSE FROM AUTHORS: Use a better estimate of uncertainty, as given in W. Peters' PNAS (2007) paper (W. Peters et al., An atmospheric perspective on North

American carbon dioxide exchange: CarbonTracker,// PNAS, 104, 18925-18930, 2007) where uptake is between -0.40 and -1.00 PgC/yr with a preferred value of -0.65.

On a more grammatical note, "N. American" should be changed to "North American."

RESPONSE: Accepted – change made in text.

(9) *Lines 696 – 698*: The author should include supporting evidence for the increase in the surface footprint of the Southern Hemisphere polar vortex. One way to do this is to note that the Antarctic Oscillation (AAO), or its zonally-symmetric counterpart the Southern Annular Mode (SAM) both showed strong *negative* anomalies during the austral fall and winter. Negative anomalies in the SAM are suggestive of coupling between the polar stratosphere and the troposphere and hence support the surface footprint seen. See Fig. S.1 for reference.

RESPONSE: Rejected – the author's agree that this is a valid point by the reviewer, but well beyond the scope of the report.

(10) *Major Modes of Northern Hemisphere Variability Section:* In this section I have several comments and criticisms:

a) *Line 731*: Both the AO and PNA patterns can be derived from the geopotential height fields (the AO is the leading mode of variability in wintertime 1000 hPa geopotential height anomalies / SLP anomalies, while the PNA pattern emerges as the second leading mode of variability in wintertime 500 hPa geopotential height anomalies). This distinction needs to be explicitly stated here, particularly when using "climate variability." Variability can be measured many ways in the climate system.

RESPONSE: Accepted – the author's added a statement to clarify/define that the AO is the 1st leading mode and the PNA is the 2nd mode in the height fields.

b) The landmark paper for the AO is Thompson and Wallace (1998). The reference given (Thompson and Wallace 2000) is acceptable, but that paper begins the shift of discussion from the AO to the recently more-accepted Northern Annular Mode (NAM) term. The former paper should appear in citations of the AO for historical accuracy.

RESPONSE: Accepted – change made in text, and the reference included.

c) *Line 732*: Insert the word "collectively" between "patterns" and "explain."

RESPONSE: Accepted – change made in text.

d) *Line 735*: Insert the phrase "opposite-signed anomalies" in place of "opposite anomalies."

RESPONSE: Accepted – change made in text.

e) Lines 736 – 738: Two different versions of the PNA are presented and interchanged incorrectly. The traditional definition of the PNA from Wallace and Gutzler (1981) involves the average of 500 hPa geopotential height anomalies throughout the Pacific-North America sector. Hence, as said later, this pattern is a "wave train" of height falls and rises across the Pacific-North America sector. Quadrelli and Wallace (2004) present a new take on the PNA by regressing 500 hPa geopotential height anomalies *onto* the second principal component of Northern Hemisphere wintertime SLP anomalies. This process is different than the single correlations done by Wallace and Gutzler (1981) or traditional EOF analysis on the 500 hPa geopotential height fields. (See Fig. S.2). As such, the statements made about the PNA in lines 736 – 738 are incorrect. In the SLP field, the "new PNA" pattern (i.e., the second PC of SLP anomalies) explains only variability in the strength of the Aleutian Low and does not feature any sort of "alternating centers of anomalous pressure that arc northeastward" (see Fig. 1 of Quadrelli and Wallace 2004). However, note that the 500 hPa geopotential height field *does* feature this alternating pattern. The authors have to decide how they wish to describe the PNA pattern and be consistent. I advocate using the traditional definition of the PNA (i.e., the second EOF of wintertime 500 hPa geopotential anomalies) and then describe its surface reflection as explaining primarily changes in the intensity of the Aleutian Low during the winter.

RESPONSE FROM AUTHORS: Reject. In this section, we do not feel compelled to bind ourselves to one definition of the PNA (i.e. 2nd EOF of wintertime SLP anomalies) for several reasons. In the literature there are many widely accepted definitions of the PNA index, each of which are highly correlated to each other, and not one can lay claim to being the most authoritative index. Indices are only so useful to describe the physical phenomenon they represent….in the case of the PNA, a large portion of variability is describing the fluctuations of the mid-latitude Asian jet, which in winter, is manifest primarily by fluctuations in the Aleutian Low and its downstream influences. Whether the PNA is defined by an REOF (Barnston and Livezey, 1987), the 2nd EOF (Quadrelli and Wallace, 2004), or a one-point correlation map (Wallace and Gutzler, 1981), all of these definitions are describing the same basic wintertime physical pattern. Therefore, we do not believe describing the pattern as it typically known (a 500mb wave train- lines 736 to 738) is contradictory to our statement that the PNA is describing a large part of SLP anomalies (or proves it is incorrect).

In addition, because we show results for the summertime, when the Aleutian Low is nonexistent, we prefer to not define the PNA based upon the wintertime pattern. During the summer, the PNA is physically different as we show in Figure 2.26a.

f) Line 743: The implication of the PNA strengthening during positive ENSO events does not conflict with the thoughts of it being an internal mode of variability for the North Pacific. By contrast, internal modes of variability can be excited or amplified by other internal modes of variability (e.g., ENSO). Why external forcing (i.e., orbital changes, changes in solar output, or anthropogenic greenhouse gas forcing) is an implication here is not clearly explained.

RESPONSE FROM AUTHORS: Accept. We were not clear, so we have clarified the sentence to state "internal atmospheric dynamics." We did not intend to imply "internal to the climate system" but rather "internal to the atmosphere."

g) Use "AO" instead of "Arctic Oscillation" in all instances after line 731.

RESPONSE: Accepted – changes made in text.

h) *Lines 773 – 775*: The correlation between cold air outbreaks and the phase of the AO is incorrectly stated. It should read "…with the positive phase associated with less frequent cold air outbreaks into the mid-latitudes and more cold air outbreaks for the negative phase (Thompson and Wallace 2001)." Note that the words "high" and "low" have been replaced with the correct "positive" and "negative" terminology. The former imply amplitude (of either sign) and does not accurately reflect the relationship being conveyed. With the corrections stated in this document made, I would feel comfortable accepting the report to be published.

RESPONSE: Accepted – changes made in text.

References Mentioned

Quadrelli, R., and J.M. Wallace, 2004: A simplified linear framework for interpreting patterns of Northern Hemisphere wintertime climate variability. *J. Climate*, **17**, 3728 – 3744.
Thompson, D.W.J., and J.M. Wallace, 1998: The Arctic Oscillation signature in the wintertime geopotential height and temperature fields. *Geophys. Res. Lett.*, **25**, 1297 – 1300.
-------, and -------, 2000: Annular modes in the extratropical circulation: Part I: Month-to-month variability. *J. Climate*, **13**, 1000 – 1016.
Trenberth, K.E., J. Fasullo, and L. Smith, 2005: Trends and variability in column-integrated atmospheric water vapor. *Clim. Dyn.*, **24**, 741 – 758.
Wallace, J.M., and D.S. Gutzler, 1981: Teleconnections in the geopotential height field during the Northern Hemisphere winter. *Mon. Wea. Rev.*, **109**, 784 – 809. -3 -2 -1 0 1 2 3

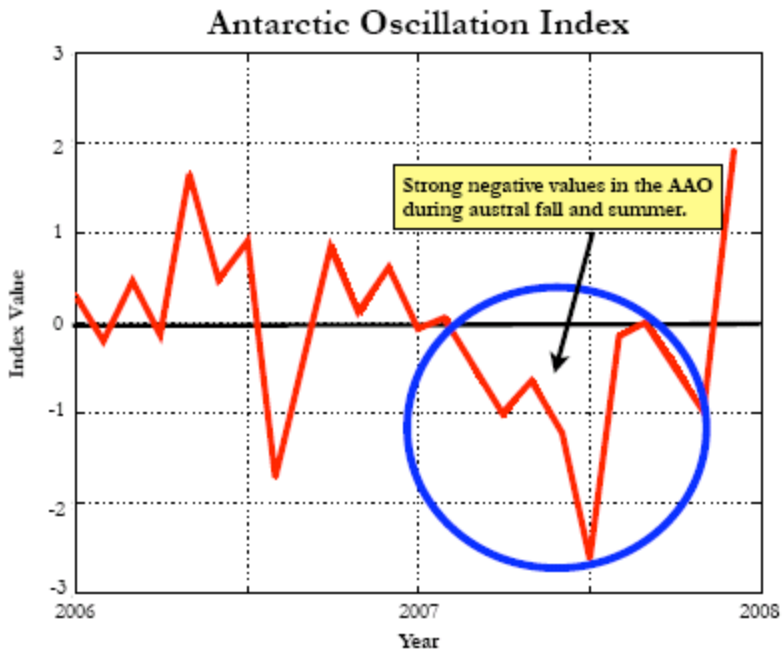

FIG. S.1. The Antarctic Oscillation Index (AAO; from CDC), monthly resolved, from 2006 - 2008. Positive (negative) values of the AAO indicates a strong (weak) stratospheric polar vortex and less (more) chance for coupling with the underlying troposphere. Horizontal black line denotes the zero line.

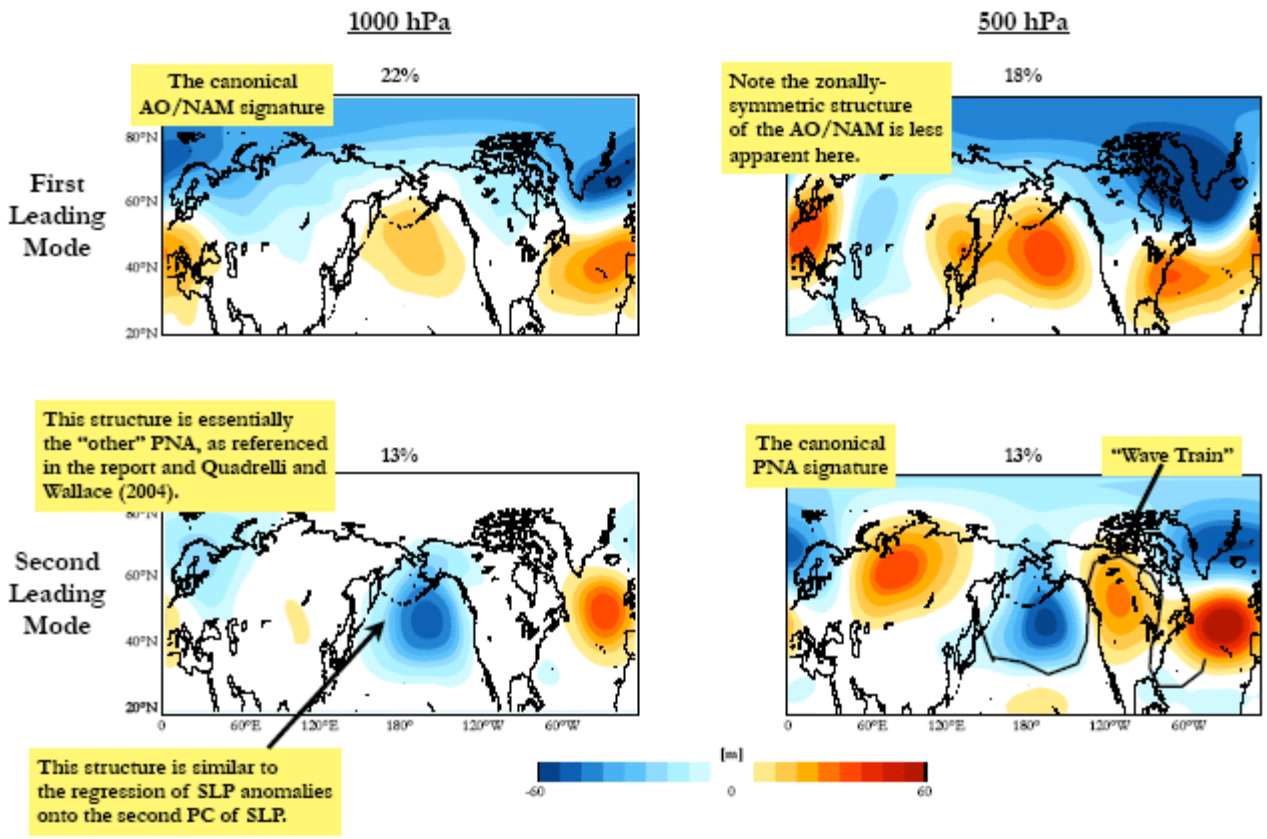

FIG. S.2. Regression of wintertime (December - March) geopotential height anomalies (m) from 20°N - 90°N at (left) 1000 hPa and (right) 500 hPa onto the (top) first and (bottom) second principal component time series of wintertime geopotential height anomalies at their respective level. The leading mode of variability in 1000 hPa geopotential height anomalies is the canonical AO/NAM, while the second leading mode of 500 hPa geopotential heights displays the canonical PNA pattern. At the surface, the PNA signature is reflected in the variability in the Aleutian Low and also the Azorean High. In the mid-troposphere, the zonall-symmetric signature of the AO is distorted because of increased wave activity at this level.

REVIEW 3

Review of "State of the Climate Report" Chapter Two: Global Processes

<u>Major comment</u>
I really liked the sections where the authors provided a brief comparison of the climatic conditions between 2006 and 2007. For the sake of consistency, I mentioned several places (see below) that would benefit from a similar comparison.

<u>2a. Overview</u>
Line 8-9: The authors might also note the annual departure of the low-to-mid tropospheric temperature.

RESPONSE: Accepted – changes made in text.

Line 12: Noting that 2006 was the 8th wettest year doesn't seem to fit with the paragraph.

RESPONSE: Accepted – the reference to 2006 was removed from the text.

Line 25: What was the globally averaged CO_2 concentration in 2006?

RESPONSE: Accepted – the 2006 concentration was added to the text.

Line 26: What is the concentration of CH4 and CO in 2007 and how do they compare to the concentration levels in 2006?

RESPONSE: Accepted – the 2007 concentrations were added to the text.

<u>2b. Global Surface Temperatures</u>
Line 93: The 0.2°C anomaly is based on which dataset? If this value is with respect to the median value in Figure 2.5, then the author might clearly state in the text how the value is derived.

RESPONSE: Accepted – the 0.2 C anomaly is an average over all the datasets quoted in the text, so we clarified this in the sentence.

Sidebar
The title of the sidebar is somewhat misleading because the impact of both SSTs and greenhouse gases on land surface temperatures are discussed. Might revise title to include both factors.

RESPONSE: Accepted – sidebar title was changed based on author input.

Line 132: The authors might clarify the kind of connection that is expected between SSTs and land surface temperatures?

RESPONSE: Accepted – changes made in text.

2c. Hydrologic Cycle

Over land

Line 188: What is the annual precipitation in 2006?

RESPONSE: Accepted – 2006 global precipitation anomaly added in text.

Would it be possible to add a figure showing the seasonal precipitation anomalies? It would nicely compliment the discussion of the anomalous rainfall patterns (Lines 191-225)?

RESPONSE: Rejected – the author's agree that it would be helpful to add the seasonal anomaly maps, but the global seasonal time series are already included in Fig. 2.9 (b-e).

Over Oceans

Line 255: How does the mean precipitation amount in 2007 compare to the amount in 2006?

RESPONSE: Accepted – changes made in text.

Line 276: Add the year to the citation.

RESPONSE: Accepted – changes made in text.

Why is the base period for precipitation 1979-1995, but the base period for land-surface temperatures is 1971-2000 and the based period for SSTs is 1961-1990?

RESPONSE: Rejected - the base period for the CAMS_OPI dataset starts in 1979, since this is a satellite derived precipitation index based on OLR measurements and that's when the global satellite dataset used for this index began. However, the land surface uses the same base period as the sea surface temperatures for determining anomalies, which covers the period 1961-1990. Note that all base periods are chosen by the authors and are influenced by the period of record and other measurement limitations inherent to the data utilized, so there is no standard base period mandated for inclusion in the report.

2.c.2 Continental Snow Cover Extent

Line 292: What was the snow extent range in 2006?

RESPONSE: Accepted – the snow extent in 2006 was 24.9 million sq km, which was 0.9 million sq km more than 2007. This was added to the text for clarification.

2.c.3 Global Cloudiness

Line 335-336: The authors might state whether the strong deviations are positive or negative.

RESPONSE: Accepted – a reference to Figs. 2.15 – 2.17 was added to the text.

Were the deviations in the Arctic and Amazon regions observed in previous years?

RESPONSE: Accepted – changes made in text for clarification.

2.d.6 Influence of Trace Gases….
Line 601: How well are these trace gases known back to 1750?

RESPONSE: Accepted – trace gases are known only coarsely from ice cores, and text was added to this effect for clarification.

Line 608: The authors might remove the parentheses around the phrase " based upon changes since 1750" because it seems to be important to the definition of AGGI.

RESPONSE: Accepted – change made in text.

Line 610-613: If possible, the authors might expand the discussion of the AGGI to include 2007.

RESPONSE: Accepted – additional text was inserted discussing the 2007 value.

2.d.7 Global Aerosols
Line 632: Why are the MODIS and AVHRR records considered reliable?

RESPONSE: Accepted – both datasets are well calibrated, and additional text added for clarification.

2.e Global Winds
Line 685: The authors might rewrite the statement to say that the wind anomalies are most pronounced over the Southern Ocean (rather than in the Southern Ocean).

RESPONSE: Accepted – change made in text.

Are the increased winds over the Southern Ocean associated with variability in the SAM/AAO?

RESPONSE: Accepted – change made in text.

2.f Major Mode of Northern Hemisphere Variability
The description of the teleconnections seems too detailed compared to the other discussions in the chapter.

The discussion focuses on the NH modes of atmospheric variability but was there any significant variability in the SH patterns of atmospheric circulation (e.g., SAM/AAO) that is relevant to the State of the Climate report?

RESPONSE: Accepted – change made in text.

REVIEW 4

Review of Global Processes Chapter for NOAA's annual State of the Climate Report

Comments (I did read the entire chapter)

Section (a), Line 26, "382.7 ppb"
CO_2 measurements should be in parts per million, not ppb (and ppm should be written as parts per million to define the abbreviation)

RESPONSE: Accepted – change made in text.

Line 28
I think it would be more clear to say " an increase of 1.4 ppb and 0.29 ppt *respectively* over 2006."

RESPONSE: Accepted – change made in text.

Line 35
It would be more physically accurate to say "Over the Southern Ocean, long bands of greater wind speeds" rather than "In the Southern Ocean…".

RESPONSE: Accepted – change made in text.

Section (b2), Line 82
Semicolon should be just a colon or even a comma

RESPONSE: Accepted – change made in text.

Line 98
May be more clear to say "the tropical LT values began the year *warmer than* the 1979-1998 average"

RESPONSE: Accepted – change made in text.

Section (SIDEBAR), Line 154
I believe the figure being referred to ("2007 land surface temperature anomalies averaged across all 48 model simulations") is not Figure 2.8 (bottom panel) but rather Figure 2.7 (bottom panel).

RESPONSE: Accepted – change made in text.

Line 155
Should be "a mean annual globally *averaged* land temperature departure"

RESPONSE: Accepted – change made in text.
Line 166

May sound better to say "greenhouse gas forcing was *likely the* principal factor"

RESPONSE: Accepted – change made in text.

Section (c1a), Line 193
JJA for some reason is not marked on Figure 2.9 (the 4th plot down)

RESPONSE: Accepted – change made in figure.

Line 195-195
It may just be hard to see, but I can't see any positive precipitation anomalies in 1996 or 2005 in the JJA plot as claimed.

RESPONSE: Rejected – the 1996 and 2005 anomalies were very small, but positive, as both were +0.1 mm during JJA.

Section (c1b), Line 257
May want to define acronyms ITCZ and SPCZ

RESPONSE: Accepted – change made in text.

Line 284
I found this phrasing confusing. Is this saying the MJO was stronger in 2007 compared to the past 4-5 years? Or is it saying that the past 4-5 years have had stronger intensity overall?

RESPONSE: Accepted – change made in text to clarify.

Section (c2), Line 311
Figure 2.14 has no labels (a), (b), etc.

RESPONSE: Accepted – change made in figure.

Section (d1), Lines 405-407
This implies fossil fuel combustion alone is responsible for the increase in CO_2, but really the increase in CO_2 is 80% fossil fuel combustion and 20% land-use change (deforestation, biomass burning).

RESPONSE: Rejected – however, a footnote was added clarifying this issue.

Line 411
Is there a reference for this 8 Pg value for fossil fuel emissions? This value doesn't match with IPCC AR4 Figure 7.3 (from Sarmiento and Gruber, 2006; fossil fuel emissions are closer to 6.4 Pg C per year, with an extra 1.6 Pg C from land-use change… maybe the sum of these is the 8 Pg value?)

RESPONSE FROM AUTHORS: Rejected – No. Our number is for fossil emissions alone, based on recent CDIAC updates (on the web) that include 2004. Since then, for 2005 and 2006, the BP global energy estimates for coal, oil, and natural gas have been used to extrapolate carbon emissions for those years. This gives an estimate for 2006 of 8.4 Pg.

Line 413-414
There may be more recent estimates than from Prentice et al. (2001), such as the Sarmiento and Gruber (2006) paper mentioned above, or Sabine et al. (2004).

RESPONSE: Rejected – Sabine estimates a cumulative ocean uptake of 118Pg+-19Pg through 1994, and annual uptake of 1.9 pg/yr for 1980-1999.

Line 414-416
Are these gross fluxes just the natural fluxes? Or natural and anthropogenic?

RESPONSE: Accepted – these are anthropogenic fluxes.

Line 421-423
What about the biosphere in this scenario?

RESPONSE: Rejected – the biosphere is a minor player at this time.

Section (d2), Line 461
May be more clear to say "atmospheric CH_4 has remained nearly constant *since the late 1990s*"

RESPONSE: Accepted – change made in text.

Section (d7), Line 617
No comma after "Aerosols in 2007"

RESPONSE: Accepted – change made in text.

Lines 618-622
This is a bit confusing. It starts off mentioning how "smoky" 2006 was, and then follows with an example, but the example says that there are "much clearer skies… in 2006". I think this is due to regional differences, but this could be made more clear.

RESPONSE: Accepted – change made in text to clarify.

Section (e1), Line 685
May change sentence to "Wind anomalies were most pronounced over the Southern Ocean throughout 2007".

RESPONSE: Accepted – change made in text.

Section (e1.1), Line 691
Since the ocean itself doesn't exhibit wind speeds, you may change this sentence to either "the region of the circumpolar Southern Ocean exhibited…" or else "the atmospheric circulation over the circumpolar Southern Ocean exhibited…"

RESPONSE: Accepted – change made in text to clarify.

Section (e1.1), General comment
May relate these wind changes to the Antarctic Oscillation (AAO)/Southern Annular Mode (SAM), either in this section or at the end of the chapter when discussing the NAM, e.g., whether the SAM was more positive/negative in 2007

RESPONSE: Accepted – change made in text to clarify.

Section (ef2), Lines 773-775
I believe this is reversed. It should be "the high index phase associated with *less* frequent cold air outbreaks into the mid-latitudes and *more* cold air outbreaks for the low index phase". In later paragraphs, the association to cold air outbreaks is correct, so I think it was just mistyped here. The phrasing may also be better if it says "the high index phase associated with less frequent cold air outbreaks into the midlatitudes and the low index phase associated with more cold air outbreaks."

RESPONSE: Accepted – change made in text for accuracy and to clarify.

Line 776
May mention that NAM is strongest in DJF and that's why the discussion is focused on these months (similar to how seasonality was described in more detail in the PNA section).

RESPONSE: Accepted – change made in text to clarify.